



## The impact of death education

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of some death educators to clarify their educational objectives, to use instructional strategies suited to achieving desired goals, and, finally, to evaluate program impact using satisfactory experimental procedures. These concerns were raised in relation to the first outcome studies that appeared. However, the subsequent research literature has not been systematically reviewed and evaluated. The purpose of this review is to evaluate the published outcome research on death education using metaanalytic procedures.

Metaanalysis is particularly useful for measuring the magnitude of the impact from an intervention and for investigating how conceptual and methodological features affect outcomes. Therefore, we sought to assess how findings varied as a function of three important factors: (1) the area of outcome assessed; (2) the type of death education program conducted; and (3) the design quality of the study. In a final section, we offer recommendations to improve the quality of future research.

## Method

### *Studies Reviewed*

We began our review in 1975 when controlled research on death education first appeared. The two inclusionary criteria used to identify relevant studies were: (1) the study was published and appeared between January 1975 and May 1987, and (2) the protocol included a control group.

Three methods were used to locate relevant studies. First, a computer search was conducted using five data bases (ERIC, *Index Medicus*, *Index Medicine*, *Psychological Abstracts*, *Hospital Literature Index*, and *Psychology Alert*). Second, a manual search of the following five sources was conducted: *Cumulative Index to Nursing and Allied Health Literature*, *Hospital Literature Index*, *Index Medicus*, and *Psychological Abstracts*. As a final check, the reference lists of all identified studies were examined.

Forty-seven studies from 41 publications were located (6 articles described 2 separate programs). One additional article was considered (7) but rejected when it was found to contain the same data from a previously published study (8). Only 20 of the 41 publications

included were initially identified through the computer search, illustrating the need for a careful follow-up manual search.

### *Coding of Studies*

The outcome data from each study were converted into effect sizes (ESs) according to the following formula:

$$\frac{m_1 - m_2}{s_2}$$

where  $m_1$  is the mean of the death education group,  $m_2$  is the mean of the control group, and  $s_2$  is the standard deviation of the control group. Therefore, ESs were calculated in such a way that positive scores indicate that the death education group demonstrated more positive change than the controls; negative scores indicate the opposite outcome. When these data were unavailable, estimates of the ES were computed following the procedures described by Glass, McGaw, and Smith (9, chapter 5). In 6 studies, in which experimental and controls were not equivalent on some outcome measures at pretest, ESs were computed following the recommendation of Wortman and Bryant (10). That is, in these studies an ES was calculated at posttest, and another ES was calculated at pretest in the usual manner. The final ES was determined by subtracting the latter ES from the former. Wortman and Bryant indicated that such an adjusted ES is appropriate when groups differ at pretest to estimate the true effect of an intervention. If a finding for an outcome measure was described as nonsignificant and no other information was provided, the ES on that measure was set at zero. Although this approach provides a conservative estimate of ESs, this procedure has been used in several metaanalyses (see 11, 12). We also conducted alternate analyses excluding these zero ESs. The analyses reported here did not differ in significance when zero ESs were excluded. Finally, to account for small sample sizes, ESs were adjusted using

<sup>1</sup>For a few programs, data were collected a few weeks later rather than immediately after the program ended. These data were treated as postprogram effects, a procedure that seems reasonable given the results of a subsequent analysis that indicated there were no differences in ES for studies that collected data at both post and follow-up points (see later discussion).

the correction suggested by Hedges (13). The design quality of each study was assessed along a 5-point scale. One point was assigned for meeting each of the following 5 criteria: (1) random assignment to groups; (2) use of placebo or waiting-list controls (the latter would control for motivation or interest in death); (3) use of reliable outcome measures; (4) use of multiple reliable outcome measures; and (5) followup of program effects. Studies were credited on criterion 3 if there was information that the outcome measures possessed adequate levels of internal consistency or test-retest reliability ( $r_s > .60$ ).

Admittedly, there are no absolute standards with which to evaluate design quality. Other reviewers might choose to emphasize different study characteristics. Nevertheless, we believe that our criteria provide a reasonable assessment of the general quality of death education programs. Increasingly, metaanalysts are recommending that the experimental features of reviewed studies be assessed and then related to program outcomes (14).

Instead of coding each study on as many other variables as possible and analyzing all possible relationships, we restricted our further analysis to how outcome varied as a function of program type and area of outcome assessed. These variables are explained next.

## **Results**

### *General Program Characteristics*

Table 1 summarizes the characteristics of the 47 studies reviewed (8, 15-54) in terms of the participants, sample size, type and length of program, and outcomes assessed. Three additional treatment studies (55-57) are also included in this review, but these investigations are analyzed and discussed separately. Information is lacking on some programs because authors did not always provide the necessary details.

Programs were classified as primarily didactic or experiential. The former programs attempt change by increasing cognitive awareness and understanding of death-related issues. Lectures, media presentations, and large-group discussions are typically used for this purpose. Experiential programs may use some of the same instructional techniques to convey information about death, but their ulti-

**TABLE 1** Characteristics of Death Education Programs ( $N = 47$ )

Variable	<i>n</i>
Participants	
Junior high/high school students	8
College students	17
Health care workers	9
Nursing students	11
Adults/community volunteers	2
Sample size <sup>a</sup>	
Fewer than 50	15
50-100	14
More than 100	18
Outcomes domains assessed	
Cognitive	12
Affective	44
Behavioral	3
Personality characteristics	8
Type of program	
Didactic	15
Experiential	27
Could not be categorized	5
Program length <sup>b</sup>	
Less than 10 hours	18
10 to 30 hours	9
31 to 48 hours	15
Longer than 48 hours	1
Could not be determined	4
Program context	
Mini-course	15
Full college course	16
Workshop	10
Special training sessions	6

<sup>a</sup>Includes both experimental and control groups; overall, programs averaged 126 participants ( $SD = 181$ ; range = 20-1,093, median = 74).

<sup>b</sup>Estimated whenever possible on the basis of a 50-min class session and 16-week school semester.

mate goal is to help individuals examine and discuss their personal feelings and concerns about death. Experiential exercises (fantasy, role playing, and simulations) are introduced to induce exploration and sharing of personal feelings and experiences. Although there is some overlap in content and process between didactic and experien-

tial programs, the distinction is important. Several writers contend that knowledge alone is insufficient in changing death-related feelings and behaviors (see 50, 58). Technically, because some didactic elements are included in all experiential programs, the major distinction is between programs with and without experiential features. In the ensuing discussion, however, the didactic versus experiential terminology is used to refer to this distinction.

Outcome variables were categorized in four ways. A few investigators assessed changes in personality characteristics, but most measured death-related changes at the cognitive, affective, or behavioral level. Cognitive variables included attitudes or values regarding various death-related issues such as euthanasia, abortion, and suicide. Affective variables assessed fears and anxieties surrounding death. Behavioral outcomes focused on either changes in personal habits or life-style (e.g., quitting smoking, making out a will) or changes in job-related helping skills (for health care workers and nursing students).

As Table 1 indicates, participants in death education programs have included junior high/high school, college, and nursing students, health care workers, and adults from the community. Programs have consisted of specialized training sessions, workshops, complete college courses, and minicourses that involve educational units offered within the context of a semester-long class. We next describe a few representative studies to provide the reader with more specific details regarding death education efforts.

### *Representative Programs*

The program conducted by McDonald (28) illustrates a didactic approach. This death education college course emphasized a review of research findings in thanatology. Students met twice a week over a 15-week semester, completed readings, listened to lectures, and viewed various audiovisual materials. An experimenter-constructed questionnaire compared the postcourse attitudes of the 88 participating students to 80 control students who expressed a wish to enroll in the death course in a subsequent semester. Significant differences favoring the experimental group were obtained on items assessing perceived comfort in interactions with the dying and bereaved, but not on an item assessing feelings about personal death.

Experiential college courses have included many different activities. These include visits to mortuaries or cemeteries, witnessing in-class interviews with the terminally ill, viewing a variety of emotionally toned films and videos, role playing, and several personal awareness exercises such as death fantasies, completion of personal death certificates, and group discussions of death experiences and feelings. Typically, such exercises are introduced in relation to didactic presentations or readings on specific topics. Time is then allotted to discuss death-related feelings induced by these experiential activities.

Noland et al. (20) described the results of 2 successful experiential death education units offered at the high school level. An 8-session death instruction unit was offered as part of a freshmen health education class. In each study, self-report data were collected to assess changes in knowledge of thanatology, feelings about death and dying, and death-related behaviors (e.g., initiating conversations about death, visiting funeral homes). In Experiment 1, there were no significant experimental versus control group differences in knowledge following the death instruction, but significant differences favoring the experimental group did appear on death-related feelings and behaviors. In Experiment 2, significant between-group differences were found on all 3 measures favoring the experimental group.

One other program is noteworthy. Yeaworth et al. (52) described the most comprehensive death education effort to date. In this case, death education consisted of a carefully sequenced set of activities integrated into a 4-year nursing school curriculum. For instance, students took two separate death classes: one on death and dying and one on grief and loss. Moreover, throughout their training, clinical conferences and seminars were arranged when patients with whom the students were working died or approached death. Finally, a consultant was on call for students for individual contacts or counseling.

Yeaworth et al. (52) reported that senior nursing students who had the benefit of the death education sequence had significantly more positive death attitudes than first-year students. In several respects, the program described by Yeaworth et al. (52) is ideal because the death education is not a one-shot effort, but rather is something that is offered over time to students as they mature and gain experience with death and dying. In particular, the clinical or experiential aspects of training are offered as the need arises to help students deal



with problematic situations. Unfortunately, the evaluation of this unique death education program was not strong. Yeaworth et al. (52) constructed their own questionnaire to assess death attitudes, and their nonequivalent control group design further lessens the confidence that can be placed in their results. For example, the first-year control students differed in several ways from the senior experimental students apart from the death education received.

### *Treatment Studies*

Three treatment studies (55–57) directed at reducing death anxiety were located, and are useful as a comparison of usual death education efforts. In these treatment studies, nurses (57), nursing students (58), or college students (56) with high death anxiety were identified and were then offered an intervention designed to reduce these anxieties. Each study involved behavioral treatment consisting of 10 or fewer group sessions of systematic desensitization, relaxation, or implosive therapy. Each of these treatments has been relatively successful, and their results are discussed later.

### *Program Outcomes*

In 4 reports (40, 46, 48, 50), both didactic and experiential programs were evaluated. Because such programs represent distinct psychological constructs, separate ESs for didactic and experiential interventions were calculated for each of these 4 reports. Therefore, the total sample size for the initial analysis was based on 47 effect sizes.<sup>1</sup>

There are several possible ways to analyze the results of death education because authors used different types and numbers of outcome measures to assess change in different areas. We were guided by the general principle that it is important to keep ESs separate for different psychological constructs. Therefore, for each study, we calculated a separate ES for each outcome domain assessed. This procedure provides separate information for each outcome domain. However, because 14 of the 47 investigations assessed outcomes in more than 1 domain, the ESs across domains are not independent. When multiple outcome measures were used within the same domain, ESs were averaged to yield a single effect per domain.

Table 2 presents the mean ES, 95% confidence intervals, and

**TABLE 2** Means, Standard Deviations, Confidence Intervals, and Fail-Safe Sample Sizes of Effect Sizes in Terms of Types of Outcomes

Outcome domain	<i>M</i>	<i>SD</i>	Confidence interval <sup>a</sup>	Fail-safe <i>N</i>
Affective ( <i>N</i> = 44)	0.18	0.43	0.05–0.31	—
Cognitive ( <i>N</i> = 12)	0.67	0.60	0.33–1.01	28
Personality ( <i>N</i> = 8)	0.12	0.14	0.02–0.22	—
Behavior ( <i>N</i> = 3)	0.69	0.23	0.43–0.95	7

<sup>a</sup>If the confidence interval does not include zero, then the mean effect size differs significantly from zero at the .05 level.

associated fail-safe sample sizes for each outcome domain. If the confidence interval does not contain zero, then the mean ES is statistically significant ( $p < .05$ ). Results indicated that findings for all 4 outcome domains were significant. Twelve studies that assessed cognitive outcomes yielded a moderate to strong effect of 0.69. Although statistically significant, the findings for affective and personality outcomes were low (mean ES = 0.18 and 0.12, respectively). Only 3 studies assessed behavioral outcomes (mean ES = 0.69), and each relied upon self-report measures of change rather than direct behavioral observations.

Orwin (59) presented a formula by which it is possible to calculate a fail-safe sample size in relation to mean ES. That is, how many more studies would be needed to reduce the obtained ES to a certain level? Cohen (60) suggested that an ES of 0.20 represents a small or nonsignificant effect in the social sciences, 0.50 represents a moderate effect, and 0.80 represents a strong effect, and many metaanalysts have accepted Cohen's convention. Using Orwin's formula, we can determine the fail-safe sample size needed to reduce our obtained ESs to 0.20. The fail-safe sample sizes are 28 for cognitive and 7 for behavioral outcomes. Fail-safe sample sizes were not calculated for affective and personality outcomes because the obtained ESs were already below the 0.20 level. The implications of these fail-safe sample sizes are discussed later. Because a reasonably large sample size of

studies ( $N = 44$ ) was available only for affective outcomes, subsequent analyses were limited to this outcome domain.

### *Design Quality*

The relationship between design features and ES was investigated in two ways. First, the score reflecting each study's overall design quality was correlated with ES. The correlation was nonsignificant ( $r = .19$ ). To ensure that this correlation was not unduly affected by the Wortman and Bryant (10) adjustment for pretreatment nonequivalence, the correlation was recomputed without using the adjustment procedure. The correlation remained nonsignificant ( $r = .18$ ). Second, the potential impact of each design feature was considered in relation to outcome by entering each feature separately in a multiple regression analysis with ES as the criterion. None of the individual design features emerged as significant predictors in the regression equation for affective outcomes. In summary, analyses suggested that methodological features did not have a significant bearing on outcomes.

### *Experiential Versus Didactic Programs*

Five of the 47 studies could not be categorized as either didactic or experiential, and 2 additional studies did not assess affective outcomes. There was a significant difference in affective outcomes achieved by experiential and didactic programs,  $t(38) = 2.70$ ,  $p < .01$ . The mean score for the 14 didactic programs was below zero ( $-0.04$ ,  $SD = 0.44$ ), and half of these programs produced negative effects. There were only 3 negative effects for affective outcomes among the 26 experiential programs ( $M = 0.33$ ,  $SD = 0.41$ ). Overall, results indicated that whereas experiential programs produced a decrease in death fears and anxieties, didactic programs slightly increased participants' discomfort with death.

To ensure that these findings were a function of the didactic versus experiential character of death education programs, two additional, related variables were coded: program length and a four-fold categorization reflecting the context within which each program was offered (see Table 1). In the former case, program length in hours was recorded whenever this information was available or could be

estimated. In the latter case, programs were categorized as either specialized training sessions, workshops, full-semester courses, or minicourses. Neither correlational analysis (program length) nor analysis of variance (general program context) yielded a significant relationship to effect size. These analyses suggested that, as expected, the didactic versus experiential distinction among programs offered the best discriminations regarding program outcomes.

### *Effects at Followup*

There were 7 studies in which it was possible to compare ESs for affective outcomes obtained after the program and at followup. Six of these were experiential programs, and the mean follow-up period was 16 weeks.<sup>2</sup> The mean ES at followup was 0.37 ( $SD = 0.40$ ), which was exactly the same mean effect obtained at the end of the study for these 7 programs ( $SD = 0.42$ ). Unfortunately, too few studies conducted followup to assess empirically how characteristics of these investigations might have contributed to their outcomes.

## **Discussion**

Results of the present review illustrate the importance of considering types of outcomes and types of programs when assessing the impact of death education. For example, death education programs have been more successful in changing participants' attitudes on death-related issues than their personal feelings about death, as indicated by the average ES obtained on these dimensions (mean ESs of 0.67 and 0.18, respectively). Moreover, the type of program conducted makes a difference for affective outcomes. Not only do experiential programs achieve significantly better affective outcomes (mean ES = 0.33) than didactic programs (mean ES = -0.04), but also half of the latter programs have produced negative effects. That is, participants have left 50% of didactic programs feeling more uncomfortable and anxious about death than when they entered. Therefore, the data suggest that experiential programs should be favored over

<sup>2</sup>This average follow-up period does not include that for reference 49 in which it was indicated that followup ranged from "one month to five years."

didactic programs if the aim is to modify participants' personal feelings about death. Even so, the impact of experiential programs has been modest.

How generalizable are current findings? The fail-safe sample sizes calculated for cognitive and behavioral outcomes were 28 and 7, respectively. A fail-safe sample size calculated only for experiential programs assessing affective outcomes was 17. In concrete terms, 28, 7, and 17 additional studies with a mean ES of 0.00 based on cognitive, behavioral, or affective outcome measures, respectively, would be needed to reduce current findings to a small and nonsignificant effect.

How likely is the existence of such studies? It must be emphasized that this review was restricted to published studies. Usually, unpublished studies yield lower effects than published ones (61). Because the fail-safe sample sizes are quite small for each outcome domain, it is not unreasonable to believe that enough unpublished reports can be located to render current findings nonsignificant. As a result, the robustness of current findings is in question. Nevertheless, fail-safe sample sizes offer only projections. For example, with respect to cognitive outcomes, whether the results of a careful review of unpublished sources would change the significance of obtained findings is an empirical question.

In addition to doubts about the stability of current findings, there is concern surrounding the negative effects obtained in 10 investigations. The finding that some programs apparently affected participants negatively increases the importance of careful implementation and evaluation of future programs.

Numerous factors could have contributed to current results. In addition to the conceptual and methodological features that are discussed later, other potentially important factors include the skills of the death educator or trainer, the personal characteristics and life circumstances of program participants, and ineffective program implementation. Unfortunately, current programs have not been conducted and reported in such a way that the relative contributions of these factors can be evaluated empirically.

The experimental quality of current research is not high, averaging 1.40 on our 5-point design scale. Only 14 programs randomly assigned groups to conditions; only 9 used placebo or waiting-list controls; and only 7 conducted followup. Although 23 studies used

multiple outcome measures, only 12 of these relied exclusively on reliable measures to assess change. In addition, all studies relied exclusively on self-report measures to assess change. Finally, only 4 investigations assessed the possible influence of social desirability response parameters on questionnaire data, which should be an automatic procedure in death research. Attending to the basic experimental design features just enumerated would substantially improve the quality of future research.

Although all these issues are important, we are most concerned with the methods used by death educators to assess program impact. Essentially, investigators have relied exclusively on self-report measures, many of which possessed unknown psychometric properties. The overall finding that death education programs are only modestly effective must be tempered by the possibility that programs have not been adequately evaluated. The substantial measurement error that exists in current program evaluations might well have obscured beneficial changes that were actually obtained. We can offer five recommendations to improve the adequacy of future evaluations.

First, death educators must operationalize both their goals and their instructional techniques. Moreover, each of these dimensions of death education should become more theoretically based. Many published reports have described program goals implicitly if at all, and contain only vague descriptions of program contents. However, as Stillion (62) indicated, death education is a complex phenomenon. Differences in course goals and program participants may require different instructional strategies. Although the distinction made in this review between didactic and experiential programs was very broad, these programs were found to differ on affective outcomes. Greater specification of teaching objectives coupled with finer procedural discriminations among programs may prove quite useful in identifying interventions with different levels of success.

In addition, it is surprising to observe how little theory exists in death education. For example, it is rare to see detailed discussions of exactly why death education is expected to change attitudes or feelings, or why particular instructional strategies should exert their intended effects. The findings from this review indicating that very brief interventions lasting only a few hours can produce effects comparable to programs lasting for a full college semester suggest that

many portions of the latter programs are probably not very potent in changing attitudes and feelings.<sup>3</sup> If death educators design their interventions more explicitly on theoretical and conceptual grounds, then they would also be led to conduct more specific program evaluations in line with these theories and conceptualizations. The active components of interventions could then be identified.

Second, investigators must limit their choices of outcome measures to those with the soundest psychometric properties. In fact, the lack of information concerning the reliability and validity of death scales has probably been the greatest hindrance to past program evaluations. Death educators cannot accurately document the impact of their efforts if the variables they seek to modify are not measured very well, and there have been few data to guide them in the choice of appropriate outcome measures. Fortunately, progress in assessment is being made. Durlak and Kasimatis (63) recently evaluated the validity of some commonly used self-report death scales. They recommended the use of three measures based upon evidence concerning their convergent and discriminant validity. These measures are the Negative Evaluation of Death subscale of the Death Concern Scale (64), the Collett-Lester (65) Fear of Death of Others Scale, and the Nelson and Nelson (66) Avoidance of Death Scale. These brief, self-administered scales assess feelings about personal death, feelings about the death of significant others, and reactions to death-related stimuli, respectively. These instruments should be selected over others whose validity is unknown. Neimeyer's (67) excellent review and critical appraisal of the conceptual and methodological issues relative to assessing death anxiety also offer a wealth of information that has practical implications for improving research studies.

Third, researchers should consider the use of retrospective pretest analysis (68). In this technique, when respondents complete traditional posttests, they also provide pretests retrospectively. Data suggest that retrospective pretest analysis can provide accurate assessments of change over time and are ideally suited, and indeed are preferred, when one assumes that an intervention has

<sup>3</sup>At the same time, it must be recognized that full course offerings have additional academic goals, for example, increasing students' knowledge about thanatology and improving their writing or library research skills. We do not wish to dismiss either the legitimacy or importance of these additional goals, which are seldom the focus of outcome research.

changed respondents' standards of reference or criteria for evaluation (69). This assumption is germane to death education. Many death educators believe that successful programs provide participants with a different perspective or orientation on death-related issues. Retrospective pretest analysis could be used to test this assumption empirically, and perhaps provide a more accurate account of how individuals are affected by death education.

Fourth, because one of the main purposes of death education is to help individuals face and cope with death more effectively, behaviorally oriented techniques are needed to monitor progress toward these goals. Important behavioral changes might include: (1) ability to express personal feelings about death; (2) increased discussion of death issues with relatives and friends; (3) more contact with terminally ill persons; and (4) concrete actions taken to prepare for death that could involve organ donor status, preparing a will, and preplanning funeral arrangements. In their treatment study, White et al. (57) used a behaviorally oriented outcome measure by asking nurses to respond to representative communications from terminally ill patients (e.g., "I've gone through so much sometimes I think it would be better to just let me die"). Similarly, death education participants could be interviewed or asked to engage in various role-play exercises so that their behavior could be rated in terms of communication skills or degree of comfort in dealing with death-related situations. The clinical literature contains several examples of how skills and feelings can be assessed behaviorally (see 70, 71). The development of standardized behavioral assessments coupled with the use of valid self-report measures will provide a more comprehensive evaluation of the goals of death education than is currently available.

Fifth, and finally, there is evidence that participants' preprogram status should be considered and related to outcome. For example, surveys invariably disclose that individuals have different motivations for participating in death education (72, 73). Educators cannot assume that their instructional goals are shared equally by all participants. Furthermore, data from some programs indicate that mean preprogram levels of, for example, death anxiety are not very high (40, 46, 54). Such data could not be due only to inadequate measuring instruments, but also might accurately reflect that an unknown number of participants begin death education with low



levels of death fear or anxiety. One would not expect substantial positive changes to occur for such individuals on these variables. Therefore, researchers should categorize participants according to their preprogram status on outcome variables (e.g., low, medium, and high death anxiety), and assess program impact in relation to these categories. Such program evaluations would provide a more specific assessment of the impact of death education in relation to participants' motivations, needs, and characteristics.

The importance of relating participant characteristics to outcomes is emphasized when considering the three treatment studies also noted in this review (55–57). These treatment studies obtained a mean ES of 0.64 ( $SD = 0.27$ ) on affective outcomes, an effect almost twice that achieved by the experiential death education programs (0.33). The results from the treatment studies, however, were achieved with highly motivated individuals who initially possessed high levels of death anxiety or fear. Such findings can provide a useful perspective against which to evaluate death education efforts.

For example, most thanatologists would agree that some degree of fear or anxiety about death is normal. If this is so, and if participants begin death education programs with varying levels of death anxiety, then it seems unreasonable to expect death education efforts to produce extremely large effects for all participants. Rather, modest changes for the total group are to be expected, and some individuals may not change on certain dimensions. In other words, the phenomenon of interest determines the boundary levels of expected effects. In the case of interventions to reduce death anxiety via death education, modest effects may be the rule, and current programs may be as effective as they can be under the circumstance at least for some individuals. Once again, however, careful measurement is needed to document the validity of such assumptions.

In effect, research on death education should begin to parallel the developments that have occurred in the field of psychotherapy in which global questions about treatment effectiveness (“Is therapy effective?”) have given way to more finely grained analyses. Ultimately, in death education we need to know which participants change in which ways on which dimensions as a result of which instructional techniques. Providing answers to this question presents a great challenge for future investigators.

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