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COMMENTARY ON REPLICATIONS OF MARLATT'S TAXONOMY

Marlatt's classification of relapse precipitants: is the Emperor still wearing clothes?

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Abstract

The present paper provides comments on the series of papers dealing with the reliability and validity of Marlatt's taxonomy of relapse precipitants. The results of these papers suggest that the degree of reliability and predictive validity of the original relapse taxonomy, as operationalized and employed in the present studies, is lower than would be hoped for. Both methodological factors in the studies and limitations in the taxonomy are discussed. While the original taxonomic system has provided a useful heuristic model and a guide for clinical intervention, it is recommended that it be modified to improve its utility in research and practice. A number of specific recommendations are provided for modifying the system

Introduction

My task is to comment on a series of papers meant to bridge the gap between the origins of Marlatt's relapse classification system as originally developed (Cummings, Gordon & Marlatt, 1980; Marlatt & Gordon, 1985), the replication and validation of this system, and future directions in its development and enhancement. The present research, conducted in an effort to replicate and extend Marlatt's relapse classification system, is extremely important even if some of the results are less positive than hoped. The model of relapse and its prevention developed by Marlatt and colleagues has provided an important heuristic framework within which to describe, understand and, potentially, predict relapse. It has contributed significantly to clinical practice and has stimulated clinical research on relapse prevention strategies. However, despite its considerable influence on clinical practice within the addictions, many of the underlying assumptions of the model have not been adequately tested. One such assumption is that the relapse taxonomy as originally developed adequately and reliably captures and allows for the prioritization of those interpersonal and intrapersonal situations that precipitate relapse. The present set of studies has served to challenge this assumption.

Longabaugh and colleagues, in their paper entitled "The reliability of Marlatt's taxonomy for classifying relapses", have presented data

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that question the reliability of the relapse classification system. It is clear that this system, as originally developed, is complex, difficult to implement and has lower than hoped-for levels of inter-rater reliability, both within sites as well as across different sites. Given the efforts of the investigators to specify the classificatory rules and protocol through consultation with Marlatt and colleagues, as well as the degree of training provided coders at each of the three research sites it is likely, as Longabaugh noted, that the upper limits of achievable intersite reliability for the system may have been achieved. Not to make a silk purse out of a sow's ear, but things may not be quite as bad as first thought. The kappa coefficient values reported by Longabaugh and colleagues, while less than hoped for, fall predominantly into the "good agreement" category. Less surprising were the findings that the greater the specificity or elaboration of the coding system being evaluated, the lower the inter-rater agreement. These findings parallel those from other areas of investigation that suggest that reliability begins to decrease as a function of the degree of complexity of the judgements being made by raters. It appears that reliability of the ratings improves across time as the raters gain increased experience with the classification system. The examination of the sources of disagreement in the classification of relapse situations is particularly helpful in identifying those categories that have considerable overlap in assignment, thus contributing to the observed decreased reliability. It is also this analysis, along with other observations, that have led to recommendations for changes in the relapse coding system. It should be noted that despite reporting considerably higher inter-rater reliability than achieved by Longabaugh and colleagues, Hodgins and colleagues (1995) have recommended that similar modifications be made to the Marlatt coding system. I will return to these recommendations shortly.

Maisto and colleagues, in their paper entitled "Construct validation analyses on the Marlatt typology of relapse precipitants", have raised questions about the validity of the relapse classification system. The positive finding, that the frequency of relapse situations at baseline coded using Marlatt's classification rules was associated with that factor on Annis' Inventory of Drinking Situations (IDS) having the highest score, suggests the presence of concurrent validity. Sobell, Toneatto & Sobell (1994), however, have pointed out that while the IDS is based on Marlatt's relapse system, the IDS scales and categories are only *associated* with heavy drinking. Therefore, one cannot presume a causal link between the situations defined as "risky" on the IDS and subsequent alcohol use or the actual situation in which relapse might occur. Also, the IDS provides a picture of the person's heavy drinking-related situations over the past year and only identifies generic situations or general problem areas (Sobell *et al.*, 1994).

Maisto's results more seriously question the predictive, rather than the concurrent validity of Marlatt's coding system. This is also the case in the paper by Stout and colleagues, entitled "Predictive validity of Marlatt's relapse taxonomy versus a more general relapse code". They found that the classification of the pretreatment relapse episode using Marlatt's taxonomic system was not predictive of the classification of post-treatment relapses, time to first drink or first heavy drinking episode, or estimates of overall drinking-related outcomes. The use of a modified classification system, based either on the characteristics of the relapse situation or on cluster analytically derived empirical typologies of relapse situations, led to a somewhat better predictive utility, although there was still a relatively large amount of variance unaccounted for with this system as well. It will be interesting to see if subsequent analyses that incorporate the rated intensity of attributes present in the relapse settings and their temporal proximity to the relapse increase further the utility of this alternative approach to coding relapse episodes.

The paper by Zywiak and colleagues, entitled "Relapse research and the Reasons for Drinking Questionnaire: a factor analysis of Marlatt's relapse taxonomy", examined the usefulness of the Reasons for Drinking Questionnaire as a continuous measure serving as a surrogate for taxonomic classification. As exemplified by the results of both Maisto and Stout, a limitation of Marlatt's system is the apparent lack of predictive utility of the mutually exclusive categorical taxonomy. Zywiak found three factors, similar to those obtained by Cannon *et al.* (1990) and Isenhart (1991) on the Inventory of Drinking Situations and Litman (1986) on the Precipitants of Relapse Inventory, including negative emotions of both an interpersonal and intraper-

sonal nature, social pressure and positive emotions with others and urges to use and substance sues. All of Marlatt's 13 minimal level relapse categories loaded onto one of these three factors. Of note is that these three factors were predictive of subsequent relapse. In particular, the negative emotion factor, which accounted for the greatest amount of variance in the factor analysis, predicted the duration of a subsequent relapse and the BAC on the first drinking day. Scores on this factor were also highly related to a subsequent relapse in response to negative emotions. In addition, an initial post-treatment relapse associated with negative emotions was highly predictive of yet another relapse occurring. Clearly, a multi-dimensional approach such as that used by Zywiak and colleagues appears to have substantially increased predictive power over that of the mutually exclusive categorical taxonomy.

These three papers focusing on the validity of Marlatt's taxonomy raise a number of points for consideration. First, as has been pointed out, the upper limits of validity are bounded by the reliability of the classification system. Given the low inter-rater reliabilities noted by Longabaugh and colleagues, one might anticipate only low to moderate validity indices. Secondly, the search for validity is based on the coding of that relapse episode that occurred closest in time to the entrance into the treatment from which subjects were recruited. A major question and concern is the extent to which this single pretreatment relapse episode is representative of the individual's relapse "risk hierarchy". There are a number of factors that lead to this question. In talking with clients in treatment, it is clear that they often have had multiple relapses, many of which have different relapse precipitants and no clearly distinguishable pattern. Stout also interpreted the results of his cluster analyses as suggesting that there do not appear to be highly distinct groups of relapses that naturally fall together. Annis (1991), in the work with the IDS, found that individuals endorse a wide range of situations in which they have drunk heavily in the past. Their responses are aggregated to provide scores on scales derived from Marlatt's coding system. From the derived profile of scores, a hierarchy of *apparent* "risk" is derived. Where a clear differentiation of risk categories can be made, the potential for prediction appears to increase. According to Annis, the most difficult situation is the "undifferentiated profile" in which no one situation is more or less problematic than any other. Prediction would presumably be poor given comparable levels of risk and equal probabilities of precipitating relapse across multiple situational contexts. It is likely that an assessment of multiple relapse episodes in the past, rather than relying on only the most recent occurrence, would allow the derivation of similar risk profiles. It may be that the relapse category evidencing the greatest relative risk, rather than the classification of the most recent episode, might have greater predictive utility. The examination of multiple relapse episodes is consistent with the recommendation of Sobell *et al.* (1994) to ask clients to describe their three highest risk situations for alcohol use in the past year. Also, Litman (1986) found that the total number of potential relapse precipitants endorsed on the Relapse Precipitants Inventory (RPI), independent of their category, was predictive of relapse. This finding suggests that the frequency of exposure to high risk situations in a given time frame, regardless of their specific content, may be a critical factor in predicting the likelihood of a future relapse.

The latter hypothesis, which can be evaluated empirically, leads to a third point. In a provocative editorial in the journal *Addiction*, entitled "Is wearing clothes a high risk situation for relapse? The baserate problem in relapse research", Sutton (1993) raises a number of salient points about determining the apparent risk associated with various potential relapse situations. Sutton notes that finding relapse occurring in a given situation, such as negative mood states or in the presence of other drinkers, is often interpreted as implying that these are "high risk" situations. However, without taking into account the baserates of occurrence of these situations for the individual, a potentially erroneous inference might be made about their relative "risk". As an example, he notes that experiencing negative emotional states may be a fairly frequent experience for substance abusers. Relapses that occur in such settings may be more attributable to being in such negative mood states with a high degree of frequency rather than to their being "risky". Within this context, the proportion of relapses that occur in negative mood states depends both on the frequency of exposure to such states and the riskiness of such situations defined in terms of the conditional probability of relaps-

ing given that one is in that state. Sutton stresses the importance of collecting data on the frequency, pattern and duration of exposure to a variety of situations in which relapses have occurred. Additionally, it is important to assess situations to which the individual has been exposed but in which relapse has not occurred. From this assessment process, it is possible to derive a 2×2 matrix of low frequency/low risk, low frequency/high risk, high frequency/low risk and high frequency/high risk situations. It is not clear into which of these four cells the individual's pretreatment relapse situation might fall; nor is it clear where in this matrix the post-treatment relapse episode might be classified. However, based on this model, one might expect a relatively low level of predictive validity from pretreatment to post-treatment relapse categories.

Fourthly, as noted previously, relapse is likely to result from an interaction among the individual's mood state and/or social, interpersonal, and situational factors; the availability, effectiveness, accessibility, and deployability of emotional and/or cognitive coping strategies; and the perceived efficacy and confidence not to drink in situations appraised as "risky". Given the multiple and interactive nature of these elements, and given their likely fluctuation across more distal as well as proximal time frames prior to a relapse, the ability to accurately predict a given relapse category without relatively continuous assessment is an exceedingly difficult task. This latter point was noted by Hodgins *et al.* (1995). In a prospective assessment condition, subjects were called weekly for them to provide mood ratings. For subjects in this condition who subsequently relapsed, the average length of time from the most recent assessment prior to the relapse episode was 2.4 days. Hodgins *et al.* (1995) noted that even this relatively brief time frame may be too distant to adequately capture the rapidly fluctuating moods associated with relapse. Thus, to some extent, Maisto's findings about the lack of association between the classification of the pretreatment relapse episode and more distant levels of alcohol dependence or the diagnosis of either affective or anxiety disorders is less surprising. Also, in the absence of both more proximal measures of the situation and of other elements of the relapse model (e.g. coping skills, self-efficacy, etc.), it may be inappropriate to attempt to rely only on prior relapse episodes to predict subsequent relapses.

The fifth, and potentially the easiest solution, is to view Marlatt's relapse classification system as a descriptive rather than a predictive model. Despite the issues of reliability and predictive validity of the original relapse taxonomy raised in the present papers, there appears to be a convergence of data to suggest the importance of negative emotional states, social pressure, and other situations in the relapse process. This has been found fairly consistently in the present papers using the classification system. Similarly, the results of Zywiak and colleagues, as well as other factor analytic studies of instruments derived in part from Marlatt's relapse taxonomy, have consistently found relatively analogous scales that are endorsed with a high degree of frequency. From a clinical perspective, the utility of the original relapse classification system lies more in identifying possible points of intervention rather than in predicting subsequent relapse episodes. That is, the original taxonomic system as employed in the present studies provides information about one potential precipitant of relapse that should be addressed therapeutically. However, as the present results suggest, an exclusive focus on this one relapse category may insufficient to prevent future relapse since precipitants other than those associated with the most recent pretreatment episode may also contribute to subsequent post-treatment relapse. Thus, interventions should include the pretreatment relapse precipitants as one of a number of therapeutic targets that are hierarchically ordered as determined by a more extensive evaluation of multiple relapse episodes and dimensional ratings of their relative risk of relapse. This point is emphasized by Zywiak and colleagues.

The paper by Rubin and colleagues, "Gender Differences in Relapse Situations", represents an important attempt to explore the applicability and generalizability of Marlatt's relapse classification system to both men and women. The original system was developed on a sample of male alcoholics involved in inpatient treatment, so it is appropriate and important to ask if it applies to women in a comparable fashion. Rubin found no differences between men and women in the nature of relapse episodes; similarly no differences were found in the topography of the drinking that took place in the relapse episode. In looking at other measures of mood, women were found to express greater feelings of being afraid and dissatisfied with self than men. Women were also

more likely to report relapsing in the presence of female friends or boyfriends; men were more likely to relapse alone or with male friends. This latter pattern of findings is of interest. However, Rubin had predicted that women would be more likely to relapse while alone while men would be more likely to relapse in the company of others. How might the discrepancy between the predicted and these actual findings be explained? Also, Hodgins *et al.* (1995) found women to be more likely to report *interpersonal* factors, particularly conflict, as the major precipitant of relapse and were less likely to report *intrapersonal* determinants, namely negative emotional states, when compared to men. The lack of consistency between predicted and actual findings within the present study and the differences noted in relapse precipitants across gender by Hodgins *et al.* (1995) suggest the need for continued research in this area rather than Rubin's conclusion that Marlatt's relapse classification system is not responsive to the characteristics of women.

One finding of interest in Rubin's study that was not highlighted had to do with the mood states clients reported feeling after a relapse. These included a mix of both negative (e.g. guilt, dissatisfaction with self) and positive moods (e.g. at ease, calm). It may be of value to explore in future research the impact of this mixed emotional state on the transition from an initial lapse to a more serious relapse. Another construct within Marlatt's model of relapse that has received relatively limited attention is the Abstinence Violation Effect (AVE), which posits a high level of negative affect typically directed at oneself following relapse. Zywiak *et al.*'s results of negative emotions associated with an initial relapse predicting a subsequent one lend some support to the AVE. While this was also noted in Rubin's findings, the observed positive moods following relapse are not predicted within the AVE construct yet might have a synergistic effect, along with the negative moods, on continued drinking.

Marlatt's taxonomy and classification system was developed on the information available at the time concerning the process of relapse. Relapse had never before been examined in any systematic way; rather, it was typically viewed as a treatment "failure" in a binary outcome classification scheme. Marlatt's work has brought significant attention to relapse as a phenomenon of relevance and importance in theory and practice within the addictions. However, it appears necessary to incorporate information generated from the present set of studies to improve on and build from the foundation Marlatt has provided.

I believe that changes in the original relapse classification system are needed and fully support the majority of the recommendations made by Longabaugh and colleagues, and would offer a few additional ones for consideration. A number of these have been incorporated into the work of Stout and Zywiak. I will summarize these recommendations briefly and then make a few observations.

- *The interview used to probe for relapse precipitants should be more fully structured*. Increased structure of interview protocols is typically associated with increased levels of reliability. Also, rather than focusing only on the most proximal precipitant, predictive validity might be enhanced further (beyond that accruing from improved reliability) by probing more directly about a range of possible contributing factors within the relapse situation, as well as sampling a larger number of such situations rather than just one.
- *The relapse categories should be modified*. In particular, the current distinction between interpersonal and intrapersonal positive and negative emotional states should be dropped. Based on Longabaugh's results, this distinction contributes markedly to the unreliability and lack of agreement among raters concerning categorization of relapse episodes. This recommendation also appears to correspond to the phenomenology of the substance abuser. It is more likely to be the mood states as experienced in the moment of potential relapse, in combination with the individual's difficulty monitoring and managing them, rather than the interpersonal or intrapersonal source of these emotions, that may precipitate the relapse.
- *Urges, temptations, and craving should be given a more formal position in the relapse taxonomy*. Heather & Stallard (1989) argue that the current taxonomy does not adequately address the role of craving in the relapse process. This category is presently used most often as a "wastebasket" when a relapse cannot be classified elsewhere. Longabaugh notes that

the actual wording of the open-ended questions originally used by Marlatt to probe for relapse precipitants may also contribute to difficulties. In exploring both interpersonal and intrapersonal determinants, clients are asked to describe those events or feelings "that triggered off your need or desire to take the first drink". The way in which the question is asked may lead clients to presume that urges, temptations or craving are presupposed and, as such, they are less likely to suggest these as contributing factors in their responses. Any modifications in the interview protocol probing for relapse precipitants should also be sensitive to this possibility.

- *The explicit hierarchical rules for classification of relapse precipitants should be removed.* The current system may artifactually lead to a preponderance of episodes being classified as interpersonal negative emotional states and much less often as urges and temptations.
- *The requirement of selecting only one category to reflect a given relapse episode should be removed.* Instead, all categories having relevance to a given relapse episode should be considered. In addition to this, I would also add that the focus on the most proximal factor immediately prior to taking the drink needs to be reconsidered. As an example, an alcoholic following treatment is living alone, feels socially isolated, begins to feel depressed, chooses to go to the local bar to see some of his/her old drinking buddies, while there begins to experience a sense of temptation and craving in response to the many cues of prior drinking episodes, feels a sense of direct and indirect social pressure and says "yes" when offered a drink. At present, given the requirements of temporal proximity and choice of a single category, this situation would likely be classified as social pressure. Given the recommendations to eliminate a single category and not to be restricted by temporal proximity would allow one to consider multiple precipitants.

Each of the recommendations proposed by Longabaugh and colleagues will help to improve the reliability of relapse classification procedures and, in the process, hopefully contribute to improved validity. Many of these recommendations also appear to be much more consistent with a view of relapse as an end point of an ongoing process or sequence of events rather than as a discrete event. It is of interest that many of the recommended modifications to the relapse classification system also reflect methods already applied by clinicians in practice settings to help describe relapse episodes (e.g. Schoenfeld, Peters & Dolente, 1993; Sobell *et al.*, 1994). As an example, the Substance Abuse Relapse Assessment (SARA) developed by Schoenfeld *et al.* (1993), is a semi-structured interview protocol that incorporates many of the recommendations proposed by Longabaugh and colleagues. The interview includes a specific and separate focus on situations, thoughts, feelings, cues and urges as independent categories that are probed for occasions of both drinking or substance use. To provide additional structure to the assessment of emotions, clients are provided with a list of 28 positive and negative emotions, are asked to choose that feeling most prominent immediately before drinking, and to continue this until they have rank ordered the five most notable emotions experienced prior to use. In addition, appropriate to Sutton's suggestions, clients are asked how they dealt with these thoughts and feelings on days that they experienced them but did not drink. The SARA represents one example of the type of assessment approach that has both direct clinical applicability and the likelihood of addressing many of the concerns presented in the current series of papers. Clearly, there are likely to be many more examples available in the literature that can make similar contributions.

A final recommendation proposed by Longabaugh and colleagues, that a comprehensive theory of relapse be developed within which the relapse precipitant is one component may, however, be either unnecessary or premature at this time. Such a multi-component theory of relapse is required for us to ultimately understand, predict and prevent relapse (e.g. Donovan & Chaney, 1985). However, this is in fact what Marlatt's model of the relapse process has proposed (e.g. Marlatt & Gordon, 1985). The focus of the present series of papers has been on the classification of a relapse episode into one of a series of categories based on Marlatt's taxonomy. While there have been a number of serious questions raised about the reliability and validity of this coding system, Marlatt's multi-component model of the relapse process, in which relapse precipitants represent one component, has not been put to a test by the present studies. This

model requires that the relapse situation and its potential precipitants be evaluated within the context of the individual's commitment to abstinence; appraisal of the relative threat of the situation; the availability, effectiveness, accessibility and deployability of behavioral and/or emotional coping skills to deal with this situation; and the individual's sense of self-efficacy. It is of interest in this regard to note that Annis (1991) uses both the Inventory of Drinking Situations (IDS) and the Situational Confidence Questionnaire (SCQ) together; Litman (1986) uses the Relapse Precipitants Inventory (RPI) in conjunction with the Coping Behaviors Inventory (CBI); and DiClemente and colleagues (DiClemente *et al.*, 1994) have developed complementary temptation and confidence scales for their Alcohol Abstinence Self-Efficacy questionnaire. This use of combined measures, consistent with aspects of a multi-component model of relapse, suggests that even when using more structured measures to assess the relative risk of potential relapse situations, these authors and theorists feel it necessary to do so only in conjunction with measures of coping ability and/or self-efficacy that may moderate this relative risk. The need to develop a new multi-component model of relapse, while worthy of consideration, does not seem to flow logically from nor appear contingent upon the findings reported in the present papers concerning Marlatt's relapse taxonomy.

Many aspects of Marlatt's broader cognitive–behavioral model of relapse are yet to be tested adequately; I would hope that this will be done in the future. Borrowing from Maisto and colleagues' paper, "until additional ... research is accomplished, it seems that the Marlatt relapse precipitant taxonomy is best considered the extremely valuable clinical tool that its widespread treatment applications has shown it to be". It seems to me that our present evolution in understanding relapse and its precipitants falls midway along a continuum from "wearing clothes is a high risk situation for relapse" to "the Emperor wears no clothes".

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the actual wording of the open-ended questions originally used by Marlatt to probe for relapse precipitants may also contribute to difficulties. In exploring both interpersonal and intrapersonal determinants, clients are asked to describe those events or feelings "that triggered off your need or desire to take the first drink". The way in which the question is asked may lead clients to presume that urges, temptations or craving are presupposed and, as such, they are less likely to suggest these as contributing factors in their responses. Any modifications in the interview protocol probing for relapse precipitants should also be sensitive to this possibility.

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- *The requirement of selecting only one category to reflect a given relapse episode should be removed.* Instead, all categories having relevance to a given relapse episode should be considered. In addition to this, I would also add that the focus on the most proximal factor immediately prior to taking the drink needs to be reconsidered. As an example, an alcoholic following treatment is living alone, feels socially isolated, begins to feel depressed, chooses to go to the local bar to see some of his/her old drinking buddies, while there begins to experience a sense of temptation and craving in response to the many cues of prior drinking episodes, feels a sense of direct and indirect social pressure and says "yes" when offered a drink. At present, given the requirements of temporal proximity and choice of a single category, this situation would likely be classified as social pressure. Given the recommendations to eliminate a single category and not to be restricted by temporal proximity would allow one to consider multiple precipitants.

Each of the recommendations proposed by Longabaugh and colleagues will help to improve the reliability of relapse classification procedures and, in the process, hopefully contribute to improved validity. Many of these recommendations also appear to be much more consistent with a view of relapse as an end point of an ongoing process or sequence of events rather than as a discrete event. It is of interest that many of the recommended modifications to the relapse classification system also reflect methods already applied by clinicians in practice settings to help describe relapse episodes (e.g. Schoenfeld, Peters & Dolente, 1993; Sobell *et al.*, 1994). As an example, the Substance Abuse Relapse Assessment (SARA) developed by Schoenfeld *et al.* (1993), is a semi-structured interview protocol that incorporates many of the recommendations proposed by Longabaugh and colleagues. The interview includes a specific and separate focus on situations, thoughts, feelings, cues and urges as independent categories that are probed for occasions of both drinking or substance use. To provide additional structure to the assessment of emotions, clients are provided with a list of 28 positive and negative emotions, are asked to choose that feeling most prominent immediately before drinking, and to continue this until they have rank ordered the five most notable emotions experienced prior to use. In addition, appropriate to Sutton's suggestions, clients are asked how they dealt with these thoughts and feelings on days that they experienced them but did not drink. The SARA represents one example of the type of assessment approach that has both direct clinical applicability and the likelihood of addressing many of the concerns presented in the current series of papers. Clearly, there are likely to be many more examples available in the literature that can make similar contributions.

A final recommendation proposed by Longabaugh and colleagues, that a comprehensive theory of relapse be developed within which the relapse precipitant is one component may, however, be either unnecessary or premature at this time. Such a multi-component theory of relapse is required for us to ultimately understand, predict and prevent relapse (e.g. Donovan & Chaney, 1985). However, this is in fact what Marlatt's model of the relapse process has proposed (e.g. Marlatt & Gordon, 1985). The focus of the present series of papers has been on the classification of a relapse episode into one of a series of categories based on Marlatt's taxonomy. While there have been a number of serious questions raised about the reliability and validity of this coding system, Marlatt's multi-component model of the relapse process, in which relapse precipitants represent one component, has not been put to a test by the present studies. This