

Impact of Kendall, Hollon, Beck, Hammen, and Ingram (1987) on Treatment of the Continuity Issue in "Depression" Research¹

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The article, "Issues and Recommendations Regarding Use of the Beck Depression Inventory" (Kendall, Hollon, Beck, Hammen, & Ingram, 1987), has had a major impact on depression research. A majority of studies using only the BDI in nonclinical samples now refer to the construct measured as "dysphoria" rather than "depression." This word change, however, is not always accompanied by other changes in research design and interpretation that would seem warranted by the concerns that initially prompted the "dysphoria" recommendation, such as the nonspecificity of high BDI scores to major depression. Researchers typically continue to derive hypotheses from depression theory, use only the BDI to measure "dysphoria" rather than purer markers of negative affectivity, cite as a limitation of their findings the danger of assuming continuity between subclinical and clinical depression, and sometimes lapse into "depression" terminology. Alternative suggestions are made for considering how the particular goals of a study might lead to various ways of handling the continuity issue.

KEY WORDS: depression; dysphoria; BDI; continuity; terminology.

The article, "Issues and Recommendations Regarding Use of the Beck Depression Inventory" (Kendall, Hollon, Beck, Hammen, & Ingram, 1987), has had a substantial influence on depression research. One indicator of this impact is frequency of citation by articles in journals covered by the

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Social Sciences Citation Index (SSCI). Kendall et al. (1987) received 2 citations in such journals in 1987, 6 in 1988, 17 in 1989, 32 in 1990, and 32 in 1991. By comparison, average figures for articles in *Cognitive Therapy and Research* 1984 through 1988 were about one-fourth of a citation (yearly means from .15 to .33) in the year of publication (SSCI "Immediacy Index") and just over one (yearly means from .84 to 1.34) citation per year in the 2 years thereafter (SSCI "Impact Factor").

The high impact of Kendall et al. (1987) is not surprising. When five eminent researchers (including the senior author of the test being reviewed) with expertise in depression, research methodology, and assessment pull together several lines of research on a widely used measure [Beck Depression Inventory (BDI); Beck, Rush, Shaw, & Emery, 1979] and offer a compelling argument for addressing several problems arising in research with this measure, it stands to reason that other researchers would take notice.

However, the manner in which researchers have responded to one of the issues considered by Kendall et al. (1987), the question of how studies using only the BDI to identify depressed persons pertain to the diagnostic category of major depression, may not be optimal. In particular, researchers using only the BDI in undiagnosed samples have embraced Kendall et al.'s recommendation to call high-scoring subsamples in such studies *dysphoric* rather than *depressed*, but it is less clear that the design and interpretation of this research have taken fully into account the logical implications of the data that prompted the dysphoria recommendation. The word change (*depression* to *dysphoria*) is reasonable in itself, but in many instances it may not be a sufficient response to the methodological concerns raised by Kendall et al. (1987).

The Continuity Issue in BDI Research

To establish our thesis, it is necessary first to summarize briefly the definitional issues and empirical findings leading to concern about continuity in depression research. As noted by Kendall et al. (1987):

The professional use of the term *depression* has several levels of reference: symptom, syndrome, nosologic disorder (Beck, 1967; Lehmann, 1959). Depression can itself be a symptom—for example, being sad. As a syndrome, depression is a constellation of signs and symptoms that cluster together (e.g., sadness, negative self-concept, sleep and appetite disturbances). The syndrome of depression is itself a psychological dysfunction but can also be present, in secondary ways, in other diagnosed disorders. Finally, for depression to be a nosologic category careful diagnostic procedures are required during which other potential diagnostic categories are excluded. (p. 290)

The content of the BDI taps depression as a syndrome, but it is not a specific indicator of major depression as a nosologic entity. Many subjects scoring high on the BDI do not meet criteria for a diagnosis of major depression. Deardorff and Funabiki (1985), for example, found that just 6 of 30 subjects scoring 10 or above on the BDI at both an initial screening and retesting 1 to 4 weeks later met criteria for current major depressive disorder; 16 met criteria for no current diagnosis, 8 for other disorders. Moreover, depressive symptom measures such as the BDI show substantial positive correlations with measures of other negative affects, particularly anxiety (e.g., Clark & Watson, 1991; Dobson, 1985; Gotlib, 1984).

These findings highlight several possible pitfalls in interpreting high-BDI subsamples as depressed. Effects of anxiety might be misinterpreted as effects of depression (Kennedy & Craighead, 1988); effects of depression could be underestimated because of the inclusion of many subclinical subjects in the depressed group; factors that relate in a curvilinear fashion to depression level could be mistaken for positive correlates of depression (Ruehlman, West, & Pasahow, 1985); and factors that relate to major depression but do not covary with symptom levels could be mistakenly viewed as unrelated to depression (Coyne & Downey, 1991). To be sure, the construct validity of the current DSM-III-R (American Psychiatric Association, 1987) criteria for major depression (and other disorders; Carson, 1991) is itself questionable. Klein (1990), for instance, found that increasing the stringency of symptom criteria for a diagnosis of major depression generated a more homogeneous group of patients in terms of the prevalence of mood disorders among first-degree relatives. If this finding were replicated and extended to other validation markers such as treatment response and course, it would suggest that major depression criteria should be made more stringent.

Doubts about the current standard definition of major depression notwithstanding, it is clear that if research is intended to pertain to the phenomenon now identified as major depression, then use of the BDI in isolation is inadequate (Ingram, 1991). Concerns about nonspecificity of the BDI to major depression are not solely hypothetical. A review of social factors and psychopathology (Coyne & Downey, 1991) concluded that (a) only major life events are associated with major depression, while both major and minor life events relate to increased depressive symptoms, and (b) some chronic stressors such as poverty seem to be positively correlated with depressive symptoms but not with risk of major depression. Similarly, subclinical samples exceeded nondistressed controls in attributional complexity (Flett, Pliner, & Blankstein, 1989), while diagnosed depressives scored lower than did controls (Flett & Hewitt, 1990).

Thus, there are several reasons to be concerned about the continuity issue in research using the BDI, and many researchers have attended to

Kendall et al.'s (1987) discussion of it. In order to evaluate our impression that the manner in which researchers have responded to this discussion has not been optimal, we reviewed articles that cited Kendall et al. (1987).

Review of the Impact of the Dysphoria Recommendation by Kendall et al. (1987)

Information concerning how researchers have incorporated Kendall et al.'s (1987) points about continuity was gleaned from a review of 85 of the 89 papers listed in 1987–1991 SSCI as citing Kendall et al. (1987). The other 4 were unobtainable by us.

METHOD

The authors read the articles under review and independently rated them according to a written coding manual.³ Coding was completed sequentially. Interrater agreement was calculated, and consensus derived in instances of disagreement, for each round of coding before proceeding with the next.

Coding Round 1: Type of Article

In the first round, each article was categorized as one of the following types:

A. Theoretical or Review Paper. These articles either reported no empirical data or reported data based only on reviewing data from other studies (e.g., a meta-analysis).

B. Empirical Study with Depression Diagnosed. These articles reported empirical data. Assignment of subjects to a depressed subsample was based on whether they met standard diagnostic criteria (e.g., DSM-III-R major depression, research diagnostic criteria), typically on the basis of a structured interview.

C. Empirical Study with Depression Not Diagnosed; BDI Not Used. These articles reported empirical data and did not measure depression in terms of standard diagnostic criteria. Depression was typically indexed by self-report, but the Beck Depression Inventory was not used.

³The coding manual may be obtained from the first author.

D. Empirical Study with Depression Not Diagnosed; BDI Used. This category is the same as the prior one except that the Beck Depression Inventory was included in the study.

Coding Round 2: Compliance with Dysphoria Recommendation

Studies rated in Round 1 as belonging in category D (Empirical study, depression not diagnosed, BDI used) were next rated as follows:

Complied with Dysphoria Recommendation. These studies used, at least once, the term *dysphoria* to describe what was indicated by high scores on the Beck Depression Inventory (either above a cutoff point, or as a continuous variable when the BDI was correlated with another variable).

Did Not Comply with Dysphoria Recommendation. These studies either never used the term *dysphoria* or alluded to the Kendall et al. (1987) argument only to reject its applicability (as in "Some would argue that we should call our subjects 'dysphoric,' but we consider them depressed because . . .").

Coding Round 3: Rationale for, and Sufficiency of, the BDI, Discussion of Limitations, and Terminological Relapse

Studies judged in Round 2 to have complied with the dysphoria recommendation were rated with respect to four additional features. First, did the rationale for the study, and particularly for the relevance of using the BDI, stem primarily from *depression* theory or research (e.g., a test of Beck's or Lewinsohn's theories of depression) or from theory or research on *dysphoria, general distress, or negative affectivity*? The latter code was interpreted broadly to include such issues as Eysenck's (1990) views on neuroticism, Frank's (1973) "demoralization" construct, and essentially any other reference to a general distress factor as opposed to depression in particular.

Second, was the BDI the *only dysphoria measure* included, or one of *multiple dysphoria measures*? The BDI was considered the only dysphoria measure in the study even if measures of, for instance, trait anxiety that might be viewed as appropriate indices of negative affectivity were included, so long as the authors clearly interpreted these as representing a separate construct rather than as additional markers of dysphoria.

Third, did the authors cite as a *flaw* of the research that subjects were not diagnosed as depressed and therefore that the conclusions could not be generalized to depressive disorders? If so, was this apology *generic* (noting the general point that high-BDI scorers might not meet diagnostic cri-

teria) or *specific* (discussing the relevance of the continuity issue particularly to the constructs being studied in the present research)?

Finally, after introducing the point that the subjects would be referred to as “dysphoric” if they scored high on the BDI in the absence of diagnostic confirmation of depression, did the authors ever *lapse* into calling them “depressed?” This code was only assigned if the authors were referring clearly to their sample or their results, not just, for instance, the general area of research or clinical work on which the findings might bear.

RESULTS

Interrater agreement was high for all coding categories, ranging from 83% to 94%. Kappa was moderate or high in most categories (.61 to .87). It was low for two of the codes used in Round 3, (a) whether the study was based on depression theory (kappa = .34) and (b) whether the BDI was the only dysphoria measure (kappa = .46), in part because of the extreme base rates of these two variables.

On the first round of coding, for type of article, consensus judgments were as follows: 10 articles were theoretical or review papers, and 23 had measured depression according to diagnostic criteria. Four studies used self-report depressive symptom measures but not the BDI. The remaining 48 were directly relevant to the dysphoria recommendation (i.e., studies using the BDI but not diagnostic criteria to measure depression) and were rated in coding Round 2. By consensus, 26 of the 48 papers (54%) were rated in the second round of coding as having complied with the recommendation to use *dysphoria*.

Characteristics of Studies Using Dysphoria Terminology

The remaining coding categories were applied only to the 26 articles using dysphoria terminology. First, nearly all of the articles (24 of 26, 92%) rationalized their work on the basis of theory and prior research pertaining to depression. Second, researchers attaching the dysphoria label usually ($n = 24$, 92%) continued to employ the BDI as their only marker of this construct. Third, researchers using the BDI and calling the construct it indexes “dysphoria” often ($n = 18$, 69%) apologized for the purported limitation that their findings could not safely be generalized to clinical depression. These apologies were often ($n = 10$, 56%) generic rather than reflecting a particular review of the applicability of concerns about continuity to the investigation under discussion. Finally, the term *depressed* or

depressive sometimes ($n = 7, 27\%$) reemerged as a descriptor of the variable identified by elevated BDI scores in undiagnosed samples.

DISCUSSION

Over 90% of the articles referring to what the BDI measures in undiagnosed samples as dysphoria predicated the work on theory and research concerning depression. If the dysphoria argument were taken to its logical conclusion, we might anticipate instead that hypotheses would be based on concepts such as demoralization, negative affectivity (NA), or neuroticism (e.g., Frank, 1973; Watson & Clark, 1984). An example of such a derivation in the set of papers studied here is Jorgensen and Richards' (1989) research extending the link between NA and health complaints to a college sample.

Similarly, almost all the studies reviewed used the BDI as the only measure of dysphoria. If the research were oriented toward dysphoria or NA in the first place, alternative measures might have been chosen. Watson and Clark's (1984) review found the Taylor Manifest Anxiety Scale (Taylor, 1953), among others, to be a purer NA marker. The BDI is less pure as an indicator of NA because it also reflects low positive affectivity ("disengagement from positive experience"; Watson & Clark, 1984, p. 472).

About two-thirds of the studies using dysphoria terminology cited as a flaw of the research that the results might not generalize to major depression. This statement would be puzzling if the research had been thoroughly reconceptualized as pertaining to dysphoria (for which the sample was presumably appropriate) rather than depression. About one-half of these apologies regarding generalizability were generic. That is, many researchers are not saying something like "Marital dysfunction might not show up in a dysphoric student sample, but the construct we studied should relate to depression as a continuum because . . .," but instead were making blanket self-criticisms regarding external validity.

Finally, about one-fourth of the articles describing high-BDI scorers as dysphoric resumed at some point depicting them as depressed.

Taken together, these four points lead us to conclude that the dysphoria recommendation is adhered to but has not necessarily prompted researchers to reconsider how continuity issues should affect research design and interpretation.

The validity of this conclusion is of course constrained by the methods we used. Citation analysis is a commonly used approach but can be ambiguous. Authors can cite (or not cite) earlier work for a variety of reasons (Leydesdorff & Amsterdamska, 1990; Shadish, 1989). Investigators using the BDI but failing to cite the Kendall et al. (1987) paper, for instance,

could nevertheless have been influenced by reading it, and these influences would be missed by our survey. Similarly, our coding categories were evaluated for interrater reliability but are of unknown validity. It seems safest in interpreting our data to conclude that at least some authors (i.e., those using dysphoria terminology accompanied by a citation to Kendall et al.) are being directly influenced by Kendall et al.'s recommendations. The validity of our evaluations of how that influence has affected research can be judged by interested readers, for our "subjects" are published articles identifiable by checking the *Social Sciences Citation Index*.

Alternative Strategies

If it is agreed that presumption of continuity is inappropriate and that the widespread adoption of the dysphoria label has not reflected a full reconsideration of implications of the continuity issue, what would be better? The best way to address this question may be to eschew dichotomous thinking. That is, it is unnecessary to heed Coyne and Downey's (1991) call for a parting of the ways between psychopathologists and investigators of subclinical levels of depression, but it also will not do to figure that the continuity problem can be handled for any and all investigations via a single tactic. Instead, it seems important to consider the implications of the continuity issue for each study. In the remainder of this paper we attempt to describe how reconsidering the implications of the continuity issue might affect behavior.

Case 1: Variable X Expected to Correlate with (Specifically) Depressive Symptom Severity Across a Wide Range. If theory or past research leads to an expectation that variable X is associated with depression in a continuous fashion throughout the distribution of symptoms, then no special apology need be made for using the BDI with an entirely nonclinical sample. Ideally one would sample broadly from the range of symptom severity in order to document the predicted relation in the most convincing manner possible, but omission of the high end of the distribution would be no more serious a flaw than omission of any other part (e.g., BDI scores from 8 to 13).

Besides sampling broadly from the distribution of depressive symptoms, controlling for secondary affective confounds such as anxiety (Ingram, 1989) to evaluate the specificity of the hypothesized relation would aid interpretation of Case 1 research (see Garber & Hollon, 1991, for a detailed discussion of what can and cannot be inferred from evaluations of specificity).

Case 2: Variable X Expected to be a Precursor of Major Depression. If a study concerns a possible predisposing factor to major depression, and

if it can be assumed that subdiagnosable but elevated levels of depressive symptoms suggest proneness to depression (a viable supposition; Lewinsohn, Hoberman, & Rosenbaum, 1988), then mildly symptomatic people selected from a nonclinical sample are *ideal* subjects, preferable to those who already have major depression. Studying purported predisposing factors by contrasting those already showing the disorder with the unaffected creates the problem of confusing consequences of depression with antecedents (Barnett & Gotlib, 1988).

In view of the interpretive advantages of the mild depressive for these purposes, apology about uncertain generalizability of findings to major depression is particularly out of place in Case 2 studies. Studies relating diverse variables to other markers of proneness to psychopathology are informative in this respect. Investigators who examine the correlates of psychosis proneness (e.g., Silverstein, Raulin, Pristach, & Pomerantz, 1990) would not cite as a limitation that subjects did not meet criteria for schizophrenia. Indeed, their not currently meeting such diagnostic criteria is an essential part of the rationale for the research method.

Case 3: Variable X Expected to Relate to the Overlap of Depression, Anxiety, and Other Negative Emotions. As outlined in Ingram and Kendall's meta-construct model of psychopathology (Ingram, 1990; Ingram & Kendall, 1987), some correlates or causes of distress (e.g., excessive, inflexible self-focused attention; Ingram, 1990) may be expected to differentiate distress from nondistress but not to distinguish types of distress. If a study concerns such a construct, then again no particular apology for using a nonclinical sample would be necessary. As noted earlier, though, it might be sensible to explore alternatives to the BDI as markers of this general factor of negative emotionality (e.g., Watson, Clark, & Tellegen, 1988) and to consider theories explicitly pertaining to general NA as sources of hypotheses.

Case 4: Variable X Expected to be a Correlate of (Specifically) Major Depression. This scenario envisions a step function such that mild depressive symptoms are not correlated with X, but among major depressives X is increased or decreased. In reviewing research on Beck's cognitive theory of depression, Haaga, Dyck, and Ernst (1991) took the position that the theory's descriptive postulates about cognitive correlates of depression fit Case 4. That is, Beck (1971) suggested that subclinical affect states are not comparable, smaller versions of clinical disorders with regard to cognitive features because they are associated with accurate perceptions of events, whereas disorders display a mismatch between cognition and reality. Whether or not that particular interpretation of Beck's theory is valid, it remains imaginable that researchers would have the Case 4 possibility in mind when noting the limitation that their result might not generalize to

clinical depression. If results of such studies as Deardorff and Funabiki (1985) are heeded, though, this sort of caveat is insufficient. When there is reason to believe that (a) major depression is the theoretically relevant independent variable and (b) about four-fifths of the "depressed" sample do not have major depression, the interpretability of the results is severely compromised.

Even if such a high misclassification rate on the independent variable were tolerable, the problem is that this is not just error variance attenuating substantive relationships. Instead, the "depressed" sample are likely to differ from controls on variables other than major depression. Thus, effects of anxiety, NA, or the like will be mistaken for effects of major depression (cf. Kennedy & Craighead, 1988).

Conclusion

All four of the scenarios described above are potentially defensible. That is, showing that *some* correlates of diagnosable depression differ from the correlates of depressive symptom scores (Coyne & Downey, 1991) does not mean they all do. The point is that each scenario has its own implications for the interpretation of results based on undiagnosed subgroups scoring high on the BDI. In Case 1 this design is incomplete but not uniquely or fatally flawed. In Case 2 it is advantageous. In Case 3 it is acceptable but not optimal. In Case 4 it is fatally flawed.

In sum, a currently common way of responding to the continuity problem is to base a study on depression theory, note as a limitation uncertain generalizability to clinical depression, and identify the subjects as dysphoric rather than depressed. We have argued that this response is not ideal for any of the scenarios described above. In its place we do not offer a list of specific recommendations for how to handle continuity issues in conceptualizing and reporting research on depression. Instead we conclude that these issues require careful case-by-case consideration by investigators. As exemplified by our discussion of several hypothetical scenarios, different decisions will no doubt be reached by different investigators, depending on the nature of their research questions.

REFERENCES

- American Psychiatric Association. (1987). *Diagnostic and statistical manual of mental disorders* (Rev. 3rd ed.). Washington, DC: Author.
- Barnett, P. A., & Gotlib, I. H. (1988). Psychosocial functioning and depression: Distinguishing among antecedents, concomitants, and consequences. *Psychological Bulletin*, *104*, 97-126.

- Beck, A. T. (1967). *Depression*. Philadelphia: University of Pennsylvania Press.
- Beck, A. T. (1971). Cognition, affect, and psychopathology. *Archives of General Psychiatry*, 24, 495-500.
- Beck, A. T., Rush, A. J., Shaw, B. F., & Emery, G. (1979). *Cognitive therapy of depression*. New York: Guilford Press.
- Carson, R. C. (1991). Dilemmas in the pathway of the DSM-IV. *Journal of Abnormal Psychology*, 100, 302-307.
- Clark, L. A., & Watson, D. (1991). Tripartite model of anxiety and depression: Psychometric evidence and taxonomic implications. *Journal of Abnormal Psychology*, 100, 316-336.
- Coyne, J. C., & Downey, G. (1991). Social factors and psychopathology: Stress, social support, and coping processes. *Annual Review of Psychology*, 42, 401-425.
- Deardorff, W. W., & Funabiki, D. (1985). A diagnostic caution in screening for depressed college students. *Cognitive Therapy and Research*, 9, 277-284.
- Dobson, K. S. (1985). The relationship between anxiety and depression. *Clinical Psychology Review*, 5, 307-324.
- Eysenck, H. J. (1990). Biological dimensions of personality. In L. A. Peruin (Ed.), *Handbook of personality: Theory and research* (pp. 244-276). New York: Guildford Press.
- Flett, G. L., & Hewitt, P. L. (1990). Clinical depression and attributional complexity. *British Journal of Clinical Psychology*, 29, 339-340.
- Flett, G. L., Pliner, P., & Blankstein, K. R. (1989). Depression and components of attributional complexity. *Journal of Personality and Social Psychology*, 56, 757-764.
- Frank, J. D. (1973). *Persuasion and healing*. Baltimore, MD: Johns Hopkins University Press.
- Garber, J., & Hollon, S. D. (1991). What can specificity designs say about causality in psychopathology research? *Psychological Bulletin*, 110, 129-136.
- Gotlib, I. H. (1984). Depression and general psychopathology in university students. *Journal of Abnormal Psychology*, 93, 19-30.
- Haaga, D. A. F., Dyck, M. J., & Ernst, D. (1991). Empirical status of cognitive theory of depression. *Psychological Bulletin*, 110, 215-236.
- Ingram, R. E. (1989). Affective confounds in social-cognitive research. *Journal of Personality and Social Psychology*, 57, 715-722.
- Ingram, R. E. (1990). Self-focused attention in clinical disorders: Review and a conceptual model. *Psychological Bulletin*, 107, 156-176.
- Ingram, R. E. (1991). Tilting at windmills: A response to Pyszczynski, Greenberg, Hamilton, and Nix. *Psychological Bulletin*, 110, 544-550.
- Ingram, R. E., & Kendall, P. C. (1987). The cognitive side of anxiety. *Cognitive Therapy and Research*, 11, 523-536.
- Jorgensen, R. S., & Richards, C. S. (1989). Negative affect and the reporting of physical symptoms among college students. *Journal of Counseling Psychology*, 36, 501-504.
- Kendall, P. C., Hollon, S. D., Beck, A. T., Hammen, C. L., & Ingram, R. E. (1987). Issues and recommendations regarding use of the Beck Depression Inventory. *Cognitive Therapy and Research*, 11, 289-299.
- Kennedy, R. E., & Craighead, W. E. (1988). Differential effects of depression and anxiety on recall of feedback in a learning task. *Behavior Therapy*, 19, 437-454.
- Klein, D. N. (1990). Symptom criteria and family history in major depression. *American Journal of Psychiatry*, 147, 850-854.
- Lehmann, H. J. (1959). Psychiatric concepts of depression: Nomenclature and classification. *Canadian Psychiatric Association Journal Supplement*, 4, S1-S12.
- Lewinsohn, P. M., Hoberman, H. M., & Rosenbaum, M. (1988). A prospective study of risk factors for unipolar depression. *Journal of Abnormal Psychology*, 97, 251-264.
- Leydesdorff, L., & Amsterdamska, O. (1990). Dimensions of citation analysis. *Science, Technology, & Human Values*, 15, 305-335.
- Ruehlman, L. S., West, S. G., & Pasahow, R. J. (1985). Depression and evaluative schemata. *Journal of Personality*, 53, 46-92.
- Shadish, W. R., Jr. (1989). The perception and evaluation of quality in science. In B. Gholson, W. R. Shadish, Jr., R. A. Neimeyer, & A. C. Houts (Eds.), *Psychology of science:*

- Contributions to metascience* (pp. 383-426). Cambridge, England: Cambridge University Press.
- Silverstein, S. M., Raulin, M. L., Pristach, E. A., & Pomerantz, J. R. (1992). Perceptual organization and schizotypy. *Journal of Abnormal Psychology, 101*, 265-270.
- Taylor, J. A. (1953). A personality scale of manifest anxiety. *Journal of Abnormal and Social Psychology, 48*, 285-290.
- Watson, D., & Clark, L. A. (1984). Negative affectivity: The disposition to experience aversive emotional states. *Psychological Bulletin, 96*, 465-490.
- Watson, D., Clark, L. A., & Tellegen, A. (1988). Development and validation of brief measures of Positive and Negative Affect: The PANAS scales. *Journal of Personality and Social Psychology, 54*, 1063-1070.