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Research on Information-Processing Factors in Child and Adolescent Psychopathology: A Critical Commentary

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Provides a critical commentary on the state-of-the-art of research on information-processing (I-P) factors in clinical child and adolescent psychology. The articles in this special section amply demonstrate the value of the I-P paradigm as a heuristic framework for conceptualizing and studying the role(s) of cognitive factors in the etiology and maintenance of child and adolescent psychopathology. However, the current status of such research also reflects a number of limitations that warrant consideration if the potential value of the I-P paradigm is to be fully realized. Specifically, understanding the role(s) played by such factors is impeded by a variety of insufficiently addressed methodological and psychometric issues, as well as by insufficiently articulated theories regarding such factors. These issues are particularly challenging for child and adolescent psychopathology researchers because of the complexities added by development. The value of I-P theories of childhood and adolescent psychopathology will be considerably enhanced if these issues are more fully considered in future research.

The articles in this special section collectively illustrate many aspects of the state-of-the-art of research on information-processing (I-P) factors in clinical child and adolescent psychology. Moreover, they amply demonstrate the value of the I-P paradigm as a heuristic framework for conceptualizing and studying the role(s) of cognitive factors in the etiology and maintenance of child and adolescent psychopathology (see Bijttebier, Vasey, & Braet, this issue).

Several of the studies in this section provide initial demonstrations of the potential importance of I-P biases in several clinical problems not previously addressed in children and adolescents. Specifically, Loney, Frick, Clements, Ellis, and Kerlin (this issue) show that adolescents with antisocial behavior problems differ dramatically in how they process emotionally negative stimuli depending on their level of callous–unemotional traits. Callous–unemotional traits are an important hallmark of the construct of psychopathy, and the results reported by Loney et al. provide important evidence supporting the relevance of that construct to understanding antisocial behavior in adolescence. Similarly, Braet and Crombez (this issue) demonstrate for the first time that children who meet criteria for serious obesity show a cognitive interference effect that is specific to food cues. This finding suggests that hypervigilance for such cues may be an important process contributing to the etiology and maintenance of childhood obesity. Finally, the construct of overgeneral autobiographical memory is introduced for the first time to the child and adolescent literature by de Decker, Hermans, Raes, and Eelen (this issue). Consistent with adult findings, de Decker et al. report that overgeneral memory appears to be a specific correlate of a history of trauma. Given the prognostic value of this memory pattern suggested by adult studies (e.g., Brittlebank, Scott, Williams, & Ferrier, 1993), it is a welcome and potentially valuable addition to the child literature.

The study by Orobio de Castro, Slot, Bosch, Koops, and Veerman (this issue) focuses on what is probably the most widely studied I-P bias of all, the hostile interpretive bias associated with aggression (Crick & Dodge, 1994). However, Orobio de Castro provide new insight into the operation of this bias by using an experimental challenge to help determine if heightened negative affect intensifies the hostile interpretative bias in aggressive children. Past studies have left some ambiguity regarding this issue, which the Orobio de Castro et al. study helps to resolve. Thus, their study nicely il-

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lustrates how experimental manipulations can be used to increase understanding of I-P factors and their operation.

The study by Schippell, Vasey, Cravens-Brown, and Bretveld (this issue) illustrates two important aspects of I-P research on child and adolescent psychopathology. First, it provides further support for the view of Dodge and colleagues that reactive and proactive aggression have distinct social I-P correlates and thus also further supports the distinction between these two types of aggressive behavior (e.g., Crick & Dodge, 1996). Further, Schippell et al. tested and found support for a mediational model predicting that biased attention allocation for social rejection, ridicule, and failure cues is related to reactive aggression by virtue of its relation to a negative or hostile interpretation bias. Thus, their study has considerably more value than more typical studies that examine one I-P factor in isolation as it relates to one syndrome or disorder construct. This is also true of the study by Dalgleish et al. (this issue), which offers a rare simultaneous look at several aspects of cognitive processing in several clinical disorders. This provides an opportunity for Dalgleish et al. to evaluate to what extent different disorders are characterized by different I-P biases and deficits.

As a group, these articles clearly illustrate the potential for research on I-P factors to advance knowledge concerning clinical problems in children and adolescents. However, the current status of such research also reflects a number of limitations that warrant consideration if the potential value of the I-P paradigm is to be fully realized. Because the articles in the special section already make clear the limitations specific to each study, our intent in this commentary is not to rehash these points and limitations. Rather, our intent is to provide a constructive critique of the broader I-P literature in hopes of strengthening future research conducted in this area.

In many respects, research on I-P factors in childhood and adolescent psychopathology has closely mirrored research on I-P factors in adult psychopathology. Having had a head start of over a decade, adult psychopathology researchers (henceforth labeled "adult researchers" for brevity) have blazed many trails that child and adolescent psychopathology researchers (henceforth called "child researchers") have benefited by following. However, in the excitement of trail blazing, adult researchers have often taken paths that have bypassed a number of important issues and questions. Perhaps following their lead, child researchers have tended to do the same. Thus, both the adult and child research literatures concerning I-P factors in psychopathology are characterized by a number of unanswered questions and unaddressed issues that are likely to hamper progress in many ways. These problems are the focus of the first portion of this commentary. Additionally, researchers wishing to apply the I-P perspective to the study of child and adolescent psychopathology face unique challenges stemming from developmental differences among adults, children, and adolescents. These developmental differences mean that paths followed in studying I-P factors in adult samples may not necessarily be the most appropriate for studying I-P factors in younger samples. The challenges posed by development comprise the second focus of this commentary. Finally, we conclude with a brief discussion of potential applications of research on I-P factors and clinical implications for clinical child and adolescent psychology.

General Issues Confronting I-P Researchers

To help illustrate the issues that confront I-P researchers regardless of the age groups studied, we begin by presenting two hypothetical studies. Both studies used a version of the emotional Stroop task to test the hypothesis that children with clinical levels of anxiety will exhibit an attentional bias such that their attention is disproportionately drawn to or held by threat-relevant versus neutral information. Although these studies are fictional, they reflect well the current state of emotional Stroop research on childhood anxiety (see Vasey & MacLeod, 2001, for a review of such studies).

As shown in Table 1, Study 1 assessed relative color-naming interference for threat-relevant versus neutral words using a card-format Stroop in which color-naming latency was timed manually. In a card-format Stroop task, words are presented as part of a large array of other words of the same emotional valence, and each word's color is named in succession (e.g., many threat words are presented simultaneously in various colors). Thus, in such a task, one must ignore not only the content of each word being color-named but also the potentially distracting content (and colors) of the other words in the array. Threat-relevant words in this study referred to a broad variety of physical and social threats, whereas the neutral stimuli comprised a set of similarly semantically related words pertaining to furniture. Using this task, 11- to 14-year-olds diagnosed with generalized anxiety disorder were compared to healthy controls on one occasion, and the clinical group was reassessed subsequent to completing a 12-week cognitive–behavioral treatment program. Results indicated that, prior to treatment (i.e., Time 1), the clinical group showed significantly greater color-naming interference for threat words than neutral words. At Time 2, however, that difference was not statistically significant. Similar to findings obtained with the clinical group at Time 2, no significant difference in color-naming latency for threat versus neutral words was found among healthy controls at Time 1.

Table 1. *Two Hypothetical Studies Using Emotional Stroop Tasks*

Study	Groups	Age Range	Task Format	Stimulus Content	Timing	Results (Color Naming Latency)
1	GAD vs. controls	11 to 14 years	Card Stroop (blocked by content domain)	General threat words vs. furniture words	Manually by experimenter	Time 1: GAD ≠ Controls GAD: threat > neutral Controls: threat = neutral Time 2: GAD-only, post-treatment GAD: no significant difference between threat and neutral words
2	Spider fearful vs. non-spider fearful	6 to 9 years	Single word (random order)	Spider words vs. insect words	Automatic by computer voice key	No significant difference between spider-fearful and non-fearful groups

Note: GAD = generalized anxiety disorder.

Study 2 used a computer-administered version of the Stroop task that presented words individually and timed color-naming latency automatically using a voice key. Participants in this study were 6- to 9-year-olds classified as having a specific phobia of spiders or having no fear of spiders based on their responses on a self-report questionnaire about spider fears. Like the task in Study 1, this task compared color-naming latencies for threat and neutral words. However, in this case, threat words were relevant to spiders whereas neutral words referred to insects. Contrary to expectations, compared to children with no fear of spiders, children with spider phobia did not show greater color-naming interference for spider words than neutral words.

How should the findings of these two studies be interpreted? It is tempting to interpret the studies in terms of theories of attention and anxiety and conclude that Study 1 shows that clinically anxious children evidence an attentional bias for threat that disappears subsequent to treatment whereas Study 2 shows that this attentional bias is absent in younger, clinically anxious children. Before drawing such inferences, however, it is important to rule out a variety of alternative explanations of these findings. These alternative explanations can be divided into two categories: (a) methodological problems that are specific to a given study, which prevent that study from providing results that be interpreted with confidence, and (b) psychometric problems that would limit the interpretability of any study using a given measure. In the following, we discuss each of these types of problems and illustrate them in the context of our two fictional Stroop studies.

Study-Specific Methodological Problems

A wide range of flaws in the design and execution of a study can prevent it from providing a valid test of the theoretical propositions that are putatively its focus. However, two such problems of particular relevance to I-P research are (a) problems with the measures that are used and the ways in which they are applied in a study and (b) problems of low statistical power due to inadequate sample size. With regard to the former, in the following discussion we focus particularly on study-specific problems that prevent I-P measures from producing reliable or valid results. However, it is important also to note that a study can yield invalid results because of the manner in which other types of constructs are operationalized. For example, the failure of our fictional Study 2 to produce the predicted difference between anxious and nonanxious children could be due to the manner in which anxiety was operationalized in that study. Children who are high in spider fear as measured by a self-report questionnaire are likely to differ substantially from children diagnosed with generalized anxiety disorder using a structured diagnostic interview.

There are many potential problems that may occur in any given I-P study that may cause otherwise sound I-P measures to yield unreliable or invalid results. For example, computer hardware may malfunction and software may contain "bugs," and such problems may introduce either random error, thus reducing a measure's reliability, or systematic error, thus reducing its validity. Similarly, participants may not understand task instructions or may lack skills (e.g., adequate reading ability) required for a measure to produce reliable and valid results. Finally, because the reliability and validity of I-P measures typically rely on carefully controlled experimental settings, environments that may be adequate for the use of other types of measures (e.g., questionnaires) may contain features, such as noise or distracting visual features, that increase error variance and thus decrease a measure's reliability.

The second problem affecting many I-P studies is their tendency to lack sufficient statistical power due to small sample sizes (Kramer & Rosenthal, 1999; see also Jaccard & Guilamo-Ramos, 2002, for a detailed discussion of statistical power in clinical child and adolescent psychology research). The tendency for the typical I-P study to rely on a small sample is likely due

to the time-consuming nature of many I-P measures, which typically are not amenable to group-based administration. Thus, a common scenario in both the child and adult research literatures has been for an initial study of some I-P factor to report a significant effect despite the use of a small sample. This initial success leads other researchers to attempt replication, generally with similarly small samples, regardless of the fact that a substantially larger sample size may be required to ensure adequate statistical power because, despite its success, the original study was substantially underpowered (Kramer & Rosenthal, 1999).

Both types of problems are clearly relevant to interpreting our hypothetical Stroop studies. For example, the significant predicted effect found at Time 1 in Study 1 could reflect experimenter bias. Manually timing color-naming latency in the study would introduce such a possibility, if experimenters were not "blind" to participants' anxiety status in this study, as has sometimes been the case in the literature. Similarly, the negative results at Time 2 in Study 1 and in Study 2 may reflect a wide range of flaws. For example, Study 2 may have been underpowered due to small sample size, preventing a difference between groups from achieving significance. Alternatively, perhaps faulty equipment (e.g., a miscalibrated voice key) or a faulty computer program produced large standard deviations for the color-naming latencies in Study 2, thereby swamping any effect with noise. Because I-P measures typically involve complex equipment and computer programs, great care is required to ensure that results are not merely an artifact of technical problems. Thus, researchers lacking experience with I-P measures would be wise to consult with experienced colleagues to limit the potential for such problems.

Psychometric Problems

Assuming the types of methodological problems discussed in the preceding section can be ruled out as the source of a study's findings, a second and more challenging set of alternative explanations must be considered before interpreting those findings at the level of theoretical constructs (Cronbach & Meehl, 1955; Garber & Strassberg, 1991). In general terms, these explanations reflect faulty relations between I-P measures and the theoretical constructs the measures are intended to index. There are two sources of faulty relations between measures and constructs: (a) the measure is unreliable (i.e., it relates poorly to any construct) or (b) the measure is reliable but invalid (i.e., it relates to some construct, but not the construct of interest).

As discussed in the previous section, study-specific features may interfere with the reliability or validity of an otherwise sound measure. However, questions may also exist regarding a measure's psychometric properties that do not reflect the idiosyncratic problems in a specific study, but would rather apply to any study that used that measure, although even such general questions may be limited to certain contexts. For example, a measure that has consistently been shown to be valid when used with adults or adolescents may be consistently invalid when applied to children. This and other challenges posed by the developmental differences among children, adolescents, and adults are discussed at greater length in a later section of this commentary.

Reliability issues. It is remarkably rare for reliability estimates of I-P measures to be reported in research studies. Thus little is known about the reliability of most such measures. In essence, it is as if I-P researchers have been granted psychometric free rein that would probably never be extended to researchers using other measures, such as questionnaires. Commonly, child researchers adopt or adapt for use with children measures that have been successfully used with adults, despite the fact that psychometric properties of those measures, even when used with adults, are unknown. Although it is understandable that reliability concerns are sometimes bypassed during initial efforts to replicate adult findings in child samples, failure to resolve such questions eventually may lead to patterns of results that are difficult to interpret and that may in fact be due to nothing more than the use of unreliable measures.

Poor reliability is a viable explanation for findings in both of our hypothetical Stroop studies. Negative findings are the most likely outcome when using measures with low reliability. Because low reliability reduces effect size, it thus reduces statistical power (Nicewander & Price, 1983). Moreover, this problem is intensified when combined with the previously noted tendency for studies to be underpowered due to the use of small samples. Thus, the negative findings in Study 2 and in Study 1 at Time 2 may reflect Type II errors. Consequently, it may be the case that attentional bias for threat is not absent in young children and does not disappear when clinical anxiety is treated. Rather, the Stroop task may be so unreliable that effects are present on some testing occasions but not others. It should be noted that, although the significant difference found at Time 1 in Study 1 may be a Type I error, low reliability does not increase the chance of such an error (Zuckerman, Hodgins, Zuckerman, & Rosenthal, 1993).

Validity issues. Even if a measure has been shown empirically to be reliable, it may not validly tap the construct of interest but may instead tap some other construct. For example, it is unclear if Stroop task color-naming interference effects are attributable to emotional disorders or psychopathology or to more mundane factors such as familiarity or experience with the task's stimuli. Dalgleish (1995), for example,

showed that, although anxious adults evidenced a significant color-naming interference effect for threat words, a similarly strong effect was found among ornithologists for bird words. This suggests that what the task may be tapping is an attentional bias for personally significant stimuli—an altogether different interpretation of the findings—rather than for those that are threatening.

Despite the importance of the issue of validity, surprisingly few studies have evaluated the validity of I-P measures. For example, little evidence exists to support the convergent validity of I-P assessment methods that putatively provide measures of the same construct. A lack of convergent validity leads to serious potential confusion in the literature as researchers struggle to generate coherent explanations for puzzling patterns of findings that reflect nothing more than the invalidity of some measures. This problem is illustrated clearly by the finding of a near-zero correlation by Dalgleish et al. (this issue) between scores derived from an emotional Stroop task and a probe detection task (PDT). Remarkably, few studies in either the adult or child literatures have compared the results of these two paradigms despite the fact that both have been used as measures of the same construct (i.e., an attentional bias in favor of threat-relevant stimuli). However, consistent with the findings of Dalgleish et al., recent research with adults also suggests that these two measures are not isomorphic (Mogg et al., 2000).

The potential for problems of convergent validity is not limited to different putative measures of a construct, such as the Stroop and the PDT. Even when several studies putatively use the same measure, they often differ in the specific version used, with the various versions typically treated as if they are equivalent. Unfortunately, there is an absence of evidence supporting this assumption of equivalence across different versions of measures. The questionable nature of this assumption is illustrated by the different pattern of results seen for card-format and single-word versions of the emotional Stroop task (Braet & Crombez, this issue; Dalgleish, 1995; McNally, Amir, & Lipke, 1996; Vasey & MacLeod, 2001), a pattern that appears to indicate that these two formats are not interchangeable measures of the same construct. If we again consider our fictional Stroop examples, the differences in findings may be due to the different task formats used (massed words on cards vs. single words on computer). For instance, the potential for word content to interfere with color naming may accumulate from trial to trial. Consequently, Stroop interference may have emerged in Study 1 because all the threat words were massed together and may have been absent in Study 2 because the words were presented one at a time and in a random order so that threat words did not consecutively follow one another.

In discussing the validity of I-P measures, it is also important to consider issues of ecological validity. Although I-P measures bring considerable potential precision with regard to the factors under consideration, this precision may sometimes come at a cost to ecological validity. For example, studies using the PDT to measure attention allocation processes typically have used pairs of words, leaving unclear the extent to which such attentional biases operate when children process more complex information (for further discussion see Schippell, Vasey, Cravens-Brown, & Bretveld, this issue). Similarly, most research on the social I-P theory of aggression has relied on measures that permit considerable time for conscious-reflective processing, leaving unclear the extent to which biases and deficits observed under such conditions characterize the processing of information in actual social interactions, in which rapid and often automatic processing may be more typical (Crick & Dodge, 1994).

Related to this point, it may appear as if effects reported by I-P researchers are found only when very arcane assessment methods are used. Thus, it is often unclear how clinically important such effects are. However, the seemingly arcane nature of many I-P measures is not an argument against the value of results obtained from their use. Rather it is an argument for making clear the clinical relevance for the use of those measures. This fact is illustrated by the study reported by Loney et al. (this issue). Had Loney et al. reported only the results of their regression analyses, one might argue that the observed effects would have little relevance for understanding adolescents with antisocial behavior problems in the real world, in which it is impossible in a given case to control for one variable (e.g., impulsivity) while considering the effect of another (e.g., callous–unemotional traits). However, because Loney et al. were able to show that the distinct clusters of adolescents mapped onto the different patterns of responses to negative emotional stimuli suggested by their regression results supports the potential clinical relevance of their regression results.

Problem Sources and Possible Remedies

Why do study-specific methodological issues and more general psychometric questions plague research on I-P factors in psychopathology to such an extent? In part, such problems seem to reflect the operation of a process analogous to "case law" that supports the perpetuation of unvalidated and potentially unreliable measures. It is as though all reasonable doubt (e.g., about reliability and validity) about a measure or construct was settled when each was first introduced into the literature. Commonly, the first studies to develop or adapt a measure for use with children have not addressed psychometric concerns (e.g., Vasey, Daleiden, Williams, & Brown, 1995; Vasey, El-Hag, & Daleiden,

1996). In these early studies, the combination of a novel measure and a novel construct (e.g., the PDT as a measure of an attentional bias toward threat in the case of Vasey et al., 1995, 1996) and findings that are both statistically significant and interesting may sufficiently outweigh concerns of a psychometric nature (or indeed many other concerns) to warrant publication. However, in the absence of adequate psychometric foundations, the rapid adoption of such measures, sometimes by researchers who are unfamiliar with their finer points (see the following discussion), has significant potential to produce a confusing pattern of mixed findings. This problem is illustrated clearly by the difficulties inherent in interpreting the results of our fictional (and indeed published) emotional Stroop studies of children.

A related source of problems may be the tendency for much research on I-P factors in psychopathology to be driven by measurement paradigms rather than by theory (and we discuss the potential inadequacies of the extant theorizing later). Child and adult researchers alike seem to reside in a space defined by two dimensions, with the first describing their level of expertise regarding I-P measures and the second describing their level of expertise concerning the disorder or syndrome to which they apply those measures. Thus, some (hopefully few) researchers may be low on both dimensions, but, more typically, researchers have considerable expertise on one dimension but not on the other. Thus, some researchers may have sophisticated understanding of one or more I-P measures, but they may have substantially less expertise about the (often wide) range of disorders or syndromes to which they apply those measures. Other researchers may have extensive knowledge of a particular disorder or syndrome (e.g., childhood anxiety) but little experience with the I-P measure(s) that they use to study it. Of course, optimal progress in understanding the role of I-P factors in child and adolescent psychopathology requires expertise on both dimensions.

How can this array of concerns best be addressed? In the short term, one way to improve confidence in research findings may be to conduct meta-analyses to identify reliable effects. Such an approach could potentially reveal consistent patterns of effects despite the fact that such effects may sometimes fail to achieve significance in individual studies due to limitations in statistical power due to the use of small samples. The value of meta-analysis is illustrated by Orobio de Castro, Veerman, Koops, Bosch, and Manshouwer (2002), who used such an approach to show that a hostile or negative interpretation bias is a reliable correlate of aggression. However, in doing such meta-analyses, it is important to consider the possibility that a bias toward publishing positive findings may lead to inflated effect size estimates if only published studies are considered (Lipsey & Wilson, 2001). Thus, it is possible that an apparent pattern of positive findings is little more than the Type I error tip of the iceberg, with an unknown number of negative studies residing unseen below the surface of the literature that have important implications for our theorizing. However, as illustrated by Orobio de Castro et al. (2002), this possibility can be evaluated using Rosenthal's fail-safe *N* to estimate the number of negative studies necessary to offset the positive effect size derived from published studies alone.

Of course, not even meta-analysis will reveal meaningful effects if the measures used in studies are unreliable or invalid. Thus, long-term progress in the field depends on firming up its psychometric foundations by conducting the basic research necessary to support the reliability and validity of I-P measures. In the case of some measures, the necessary work includes evaluation of test–retest reliability and internal consistency, whereas in other cases what is needed is evidence of convergent and divergent validity.

One especially valuable way of improving future research would be to use multiple measures of the constructs of interest (e.g., see Mogg et al., 2000). This would be valuable for several reasons. First, in studies with large sample size, such an approach will permit the modeling of I-P constructs as latent variables using structural equation modeling techniques (for an example of this approach, see Dodge, Laird, Lochman, Zelli, & the Conduct Problems Prevention Research Group, 2002). Such an approach offers a variety of statistical advantages over analytic approaches that rely on manifest variables (such as regression analysis; see Muruyama, 1998). Second, even in studies using small samples, the use of multiple measures would permit the creation of composite scores that can be expected to have better reliability and validity than the scores comprising that composite (Rushton, Brainerd, & Pressley, 1983). Third, the use of multiple measures permits a "bootstrapping" approach in developing improved measures of the constructs in question (Cronbach & Meehl, 1955). Specifically, once one has identified a construct of interest, it is often necessary to begin with several fairly crude measures of that construct. However, by examining the relations among those measures and measures of other related constructs, it is often possible to improve their validity.

Moving From Data to Theory

Only when alternative interpretations have been ruled out, and probably only after putting in place some of the remedies suggested in the last section, should serious efforts be made to develop or revise theories about the role of I-P factors in psychopathology to fit with empirical findings. However, even when a rich, methodologically, and psychometrically sound database is available, movement from data to theory within the I-P paradigm is often hampered by insufficiently specified theoretical models. For example, we

may have a reasonably specified theory of attention and anxiety, but this theory might not speak to the fact that phobic anxiety and generalized anxiety may be very different constructs or that different aspects of attention (e.g., attentional capture vs. attention maintenance) may interact with anxiety in different ways.

Furthermore, most theories consider certain cognitive processes and syndromes or disorders in relative isolation from cognitive processes and other syndromes or disorders. If we consider theorizing as existing on a continuum from process- or psychopathology-specific micro-theories to macro-theories that represent generalized architectures of cognition-psychopathology relations, then much extant theorizing within the I-P paradigm as it pertains to developmental psychopathology is nearer the micro-theory end of the dimension (Barnard, May, Duke, & Duce, 2000). For example, Crick and Dodge (1994) noted the failure of most social I-P studies to specify or test the relations among biases and deficits at various stages of the processing sequence, leaving unclear whether the contribution of each is unique or is instead mediated by biases or deficits in other steps in the sequence. Indeed, rarely have studies assessed more than one aspect of information processing simultaneously and this is reflected in the underlying theories. Evaluating several aspects of the I-P sequence in a single study encourages consideration of the relations between I-P factors as well as their zero-order relations to psychopathology. Such studies permit the specification and testing of mediational models that have the potential to add considerable clarity to theories of I-P factors in psychopathology. This is illustrated in the special section by Schippell et al. (this issue), who demonstrate that the relation between reactive aggression and a pattern of reduced attention to social threat cues is mediated by a bias toward negative interpretations of ambiguous social situations.

Similarly, it is often unclear if I-P biases and deficits are uniquely related to specific disorders or syndromes or if they are instead common to a variety of problems. For example, as noted by Schippell et al. (this issue), with few exceptions, studies of various I-P biases and deficits in aggressive children have not controlled for symptoms of inattention and hyperactivity–impulsivity. Indeed, many of the studies in this section provide welcome tests of the divergent validity of various I-P measures, often with interesting results.

Moving From Theories to Real-World Implications

One criticism of I-P research in psychopathology is that even if it produces rich theoretical and empirical insights into how information is processed in children and adolescents with clinical problems, these processes may not be fundamental to psychopathology but rather may be mere epiphenomena of such clinical problems. For example, in our fictional Stroop Study 1, even when we are confident of the methodological and psychometric adequacy of the data, the implications could be that attentional bias for threat-relevant information is present when participants are anxious but absent when participants are not anxious. This does not really move the field very far in understanding *why* anxiety states are precipitated and *how* they are maintained. Similarly, as noted previously, this bias may not be specific to threat cues at all and may just reflect the fact that people selectively attend to things that have meaning for them at that moment.

An important challenge for adult and child researchers alike is to develop studies to address these kinds of issues. For example, one way to determine if an I-P factor is validly associated with a disorder or syndrome rather than with some other construct such as personal relevance or expertise would be to compare a symptomatic group and a nonsymptomatic group who have similar levels of "expertise" with the stimulus material. This approach is illustrated in a study by Freeman and Beck (2000), who used an emotional Stroop task to compare sexually abused adolescent girls with and without posttraumatic stress disorder (PTSD) as well as nonabused controls. Because sexual abuse-related words were personally relevant to both groups of sexually abused girls, whereas only one group met criteria for posttraumatic stress disorder, the unique relations of personal relevance and posttraumatic stress disorder to Stroop interference for sexual abuse-relevant words could be discriminated. Although this particular study failed to yield the expected pattern of results (i.e., neither group of abused girls differed from controls), had it done so, its design would have permitted clarification of the source of the effect.

Assuming that an I-P bias or deficit is specifically related to a syndrome/disorder rather than to some other construct, the nature of that relation must still be determined. For example, research must determine if a bias or deficit is present prior to the onset of the syndrome/disorder in question and if the bias or deficit can be modified such that symptoms of the syndrome/disorder also change. One underutilized approach to testing the former question is to study I-P variables among children and adolescents at high risk for a given disorder/syndrome, either due to their own history, their family history, or their genetic make-up. A study by Dodge, Bates, and Pettit (1990) illustrates this approach. Dodge et al. studied the effects of physical abuse in early childhood on later I-P biases and deficits, and their further relation to later aggressive behavior problems. Early physical abuse is a risk factor for the development of aggressive behavior problems, thus if social I-P biases and deficits contribute to the development of later aggression, the relation between abuse and later aggression should be mediated by social I-P

biases and deficits. Indeed, the results reported by Dodge et al. supported this hypothesis, suggesting that the development of social I-P biases and deficits is an important step in the development of aggressive behavior problems. Similar approaches have promise for understanding I-P factors in relation to other syndromes/disorders. For example, would children who are at risk for depression show different I-P patterns (e.g., in memory for negative information) in response to mood challenge (cf. the adult study by Gerner, Segal, Sagrati, & Kennedy, 2001).

The optimal approach to answering questions about the causal status of I-P factors would be to conduct experiments in which the I-P factor in question is systematically manipulated to determine the effects of such changes on symptoms of the disorder/syndrome in question. For example, in a study of adults, MacLeod, Rutherford, Campbell, Ebsworthy, and Holker (2002) demonstrated that vulnerability to negative affect in response to stress can be altered by modifying how participants allocate attention to threat cues. Specifically, MacLeod et al. showed that an experimental stressor produced significantly greater negative affect among participants trained to have an attentional bias toward threat cues than in those trained to ignore such stimuli. Although ethical concerns exist regarding attempts to experimentally establish an I-P bias or deficit that may potentially contribute to the development of psychopathology, few such concerns would seem to apply were efforts made instead to reduce or eliminate a bias or deficit in hopes of reducing or eliminating symptoms. For example, Hazen, Vasey, and Schmidt (2002) found that a computer-based attentional retraining program designed to reverse the attentional biases of adult chronic worriers was significantly more effective than a placebo condition, suggesting that the attentional bias is indeed a cause of anxiety rather than simply a concomitant of it. Extension of such experimental methods to children and adolescents is an important direction for future research. Similarly, a variety of predictable stressors in youth (e.g., immunizations or the transition from elementary to middle school) provide opportunities for naturalistic experiments that may provide valuable information regarding the role(s) played by various I-P factors in the development of psychopathology and in coping with such stressors in those children who negotiate them successfully.

In summary, unless steps such as those described previously are taken to demonstrate and, where necessary, improve the reliability and validity of I-P measures, progress in the I-P research domain will continue to be hampered by uncertainty regarding the meaning of research results. Although studies of the psychometric properties of various I-P measures when applied to children and adolescents may lack the glamour of demonstrating some new effect, they are nonetheless vitally important to maximizing progress in this research domain. However, the lack of adequately articulated micro- and macro-theories of the roles played by I-P factors in typical and atypical development is also a serious impediment to progress. Indeed, progress in each of these domains requires progress in the other.

Challenges Posed by Developmental Factors

Whereas the issues summarized in the previous section apply generally to I-P research regardless of the age of participants, in this section our focus is on the specific challenges faced by child researchers due to the fact that children and adolescents are (sometimes rapidly) developing organisms and psychopathology occurs in the context of their development. Of course, many of these challenges are not unique to I-P research, but are faced in other clinical child and adolescent research domains as well. However, such general challenges are nevertheless worth highlighting here as they represent critical points for future empirical inquiry by I-P researchers. Thus, below we discuss two broad categories of challenges posed by development. First, we consider methodological and psychometric challenges faced by researchers when devising or adapting I-P measures for use with young people. Second, we discuss the need for theories concerning I-P factors in child and adolescent psychopathology to more fully consider developmental factors and to be better integrated with extant theories of both typical and atypical development.

Methodological and Psychometric Challenges Posed by Development

Although the effects of age as a main effect or as a moderator of other effects are occasionally tested in child I-P studies (e.g., Dalgleish et al., this issue), this is not necessarily the best way to test for developmental differences for at least two reasons. First, because samples in I-P studies are often small, power to detect age effects, especially if age functions as a moderator (i.e., in interaction with another variable), is often insufficient to avoid inflated rates of Type II errors. Second, it is important to remember that age is only a (sometimes crude) proxy for developmental differences in specific domains, such as cognitive development or physical maturation (Rutter, 1989). Thus, although examination of age differences is a worthwhile starting point in testing for developmental differences, the effects of development are ultimately best tested through the specification of differences in specific skills or characteristics. For example, in some age ranges, pubertal status is only moderately correlated with age (e.g., see Angold & Rutter, 1992)

and in such cases, if only age differences were examined, important effects of pubertal development could be missed.

The often wide developmental differences between children, adolescents, and adults pose potentially serious threats to the reliability and validity of I-P measures. In turn, these threats lead to serious difficulties in drawing clear conclusions regarding the effects of development on the operation of I-P factors in the development of child and adolescent psychopathology. For example, a measure that has empirically been shown to be valid among adults or adolescents may consistently fail to produce expected results with younger children. This may *not* be because that construct is not relevant to children. Rather, it may be because the measure is a poor index of that construct because the measure is unreliable when used with younger children or because its validity rests on the presence of some skill or knowledge that is beyond the developmental capabilities of such children.

An I-P measure's unreliability or invalidity may reflect something as simple as the use of a stimulus set that is inappropriate for children (e.g., words outside the vocabularies of most young children) or more complex and subtle problems. For example, many studies using the emotional Stroop paradigm with children have failed to produce expected results (see Vasey & MacLeod, 2001). Rather than showing that anxious children show greater color-naming interference than controls for threat-relevant versus neutral words, results suggest that children, regardless of their anxiety status, show heightened interference for threat words. Although this pattern of results may indicate that anxious children are no more distracted by threat cues than nonanxious children (i.e., a theory level interpretation; see Kindt, Brosschot, & Everaerd, 1997), it may instead simply reflect problems inherent in the Stroop paradigm (see Vasey & MacLeod, 2001). Specifically, Stroop tasks place heavy demands on children's inhibitory capacity in that the distracting stimulus (i.e., word content) and the target stimulus (i.e., word color) are integrated features of a single stimulus. Thus, a child is called on to ignore one aspect of a stimulus while still attending to another aspect of that same stimulus. Indeed, when Kindt and Brosschot (1999) used stimuli in which the target and distracting features were less integrated (i.e., monochrome words superimposed on a colored circle), they found that spider-fearful children showed significantly greater color-naming interference for spider-relevant versus neutral words than did nonfearful controls. Thus, in our fictional Study 2, it may be that the lack of significant findings does not reflect the absence of an attentional bias for threat in the anxious participants, but rather reflects the fact that the type of emotional Stroop task used in that study was an insensitive measure of this construct in children ages 6 to 9 years old.

Reliability issues. There are many reasons why an I-P task may be less reliable when used with children or adolescents than when applied to adults. These include everything from task instructions that are insufficiently clear to permit them to be consistently followed by young children to the greater susceptibility of children to fatigue, necessitating more frequent or longer rest intervals for them versus adolescents or adults. Because there are so many reasons why the reliability of I-P measures is likely to decrease with the age of study participants, it seems wise to routinely evaluate this possibility in any study that includes an age range of more than 1 or 2 years. However, it is important to note that doing so involves examining within-participant variability as well as variability across each age group. It is possible for there to be substantially greater within-participant variation (e.g., among a participant's probe detection latencies across trials in the PDT) in some age groups without that variation being apparent in those participants' average performance.

Validity issues. Assuming that an I-P measure is similarly reliable for all developmental levels of interest, there remain many reasons why its validity may nonetheless vary substantially as a function of development. As noted previously, validity problems may reflect nothing more complex than the selection of a word set that is inappropriate for younger children. For example, the content of children's worries and fears varies substantially with development, with younger children being more concerned about physical threats, whereas adolescents are more concerned about social threats (Vasey, Crnic, & Carter, 1994). Thus, were a PDT to include mainly words related to negative social evaluation, it would not be surprising to find greater evidence of an attentional bias toward such threat words among anxious adolescents than among younger children—not because younger anxious children lack such a bias—but because the PDT was not a valid measure of that bias in such children. This is but one of many similar challenges to validity faced in developing or adapting I-P measures for use with children and adolescents. Further, it bears noting that this situation is even further complicated by the fact that it is not only I-P factors that may vary with development, but so too do the syndromes/disorders of interest.

Some aspects of information processing have yet to be examined in children, in most cases because of uncertainties about how best to adapt adult measures of such variables so that they are valid for use with children. For example, with few exceptions (e.g., Rabiner, Lenhart, & Lochman, 1990), there have been few efforts as yet to study automatic aspects of information processing despite the possibility that they may be of equal or greater importance as more conscious-reflective aspects of processing (Crick & Dodge, 1994). For example, Schippell et al. (this issue) suggest a hypothe-

sis that will require the use of backwards masking or similar methods to test it. The dearth of child research on such aspects of processing likely reflects the complexity of the methods necessary for their measurement, such as backwards masking techniques. Such techniques present stimuli for very brief intervals and replace them with a masking stimulus to preclude conscious awareness of them. With adults, presentation intervals as brief as 14 msec have been used effectively (e.g., Mogg, Bradley, Williams, & Mathews, 1993). However, it is unclear whether this same interval would be appropriate for children because of their lower reading ability. Thus, adapting such measures to children will require research to identify a presentation interval that is too brief to permit children to become consciously aware of stimuli, but long enough to permit those stimuli to produce the automatic effects of interest (assuming they exist in children). The fact that children vary widely within and across age in their reading ability further complicates matters and may mean that individually tailoring presentation intervals for each child will be required to produce an effective task.

In summary, the fact that child and adolescent psychopathology occurs in a developing organism means that simple downward extensions of adult theories and measurement paradigms to children may produce disappointing or surprising results. Indeed, when designing or choosing measures of I-P factors, child researchers must consider the possibility that the best measure may bear little resemblance to measures that have been effective with adults. In such a case, although measures may differ substantially when viewed at the molecular level, there may nonetheless be molar consistency in their meaning across age. In other words, there may be consistency at the level of the construct despite differences in the measures of that construct at different points in development.

Considering Development in Theories of I-P Factors in Child and Adolescent Psychopathology

In addition to considering the implications of development for the psychometric properties of I-P measures, it is important to consider the implications of development for theories of the role(s) played by such factors in child and adolescent psychopathology. Specifically, in keeping with a developmental psychopathology perspective (Cicchetti & Cohen, 1995; Masten & Braswell, 1991), development has implications not only for how to measure I-P factors, but for which I-P factors are likely to be important and for how they operate in children at various levels of development. Thus, for example, it is necessary to consider the possibility that different I-P factors may be important in childhood or adolescence than in adulthood. This is illustrated by the findings of Dalgleish et al. (this issue) regarding prospective cognition in child and adolescent posttraumatic stress disorder. Their results showed that in generally anxious children and adolescents, a potential future negative event was estimated as being more likely to happen to someone else than to the participant him or herself. This "other-reference bias" was stronger in the anxious youth than in the healthy controls. In contrast, in generally anxious adults, the opposite effect prevails, whereby future negative events are assigned a greater probability of happening to the participant than to another, again relevant to controls (Butler & Mathews, 1983). The adult finding has been interpreted as a straightforward instantiation of the "availability heuristic" (AH) (Tversky & Kahneman, 1974) along the following lines: (a) Anxious adults think more about negative things that might happen; (b) when asked about the likelihood that negative things will happen, the more you think about them, the higher your estimate (the AH). In younger populations, this straightforward explanation does not work. There are therefore several alternatives: (a) The AH does not operate in younger children; (b) the AH does operate, but this paradigm is not sensitive to it; (c) the AH does operate with respect to this paradigm, but its effects are masked by another process that is not evident in the adult results. Whichever of these explanations (if any) turns out to be the right one, it seems clear that the I-P processes operating in adults with respect to this task are different than those at work in younger samples.

Similarly, it is likely to be unwise to assume that all I-P factors operate in children as they do in adults. Indeed, they may operate differently even at different points in childhood and adolescence. Thus, a given factor may play a different role (or perhaps no role at all) in childhood than adulthood. For example, Neshat-Doost, Taghavi, Moradi, Yule, and Dalgleish (1998) showed that the strength of the relation between depression and memory bias for negative information increases with age among 10- to 17-year-olds. Assuming that this age effect is valid, the authors suggested that it can be understood in terms of the development of self-schema, which acts as a context for the encoding and elaboration of emotional material. Specifically, Neshat-Doost et al. argued that the self-schema becomes increasingly elaborated across this developmental period and thus potentially plays an increasing role in depression as well.

Finally, beyond the need to more fully consider development in I-P theories of childhood psychopathology, those theories would benefit from being more firmly placed within broader transactional theories of developmental psychopathology. The recent work of Dodge and colleagues (e.g., Dodge et al., 1990, 2002) is a noteworthy exception to this generalization, which illustrates the value of considering I-P factors within

the context of an integrative transactional theory of the development of aggressive behavior problems in children and adolescence.

In summary, the fact that children and adolescents are developing organisms poses some difficult challenges for researchers interested in I-P factors in child and adolescent psychopathology. These challenges have been given insufficient attention in the literature to date, and progress in this research domain will be seriously hampered if they are not dealt with more fully in the future.

Clinical Appslications

As noted by Bijttebier et al. (this issue), research on I-P factors in child and adolescent psychopathology may lead to a variety of clinical benefits. First, I-P measures may prove useful for assessing aspects of psychopathology that are not amenable to assessment in other ways. Second, I-P research may help to improve existing treatment approaches by elucidating the processes that account for their efficacy. Third, I-P research may reveal important new targets for treatment and suggest new approaches to treatment that are aimed directly at those targets. Finally, I-P research may lead to enhanced ability to identify children who are at risk to develop a syndrome or disorder and, further, may suggest novel approaches to reducing such risk.

Many aspects of the potential value of I-P measures for the assessment of various syndromes or disorders were discussed at length in the special section on performance-based assessments that appeared in the *Journal of Clinical Child Psychology* in 2000 (see especially Frick & Loney, 2000; Garber & Kaminski, 2000; Rapport, Chung, Shore, Denney, & Isaacs, 2000; Vasey & Lonigan, 2000). As the articles in that section consistently pointed out, there is little evidence as yet for the clinical utility of such measures. The major impediment to successfully using I-P measures in clinical assessment is likely to be the relatively high error variance in such measures relative to other measures (e.g., questionnaires). Thus, even in children and adolescents with no diagnosis, the variability in such measures is likely to be fairly high compared to the variance of other measures (e.g., neuropsychological tests). Such background noise is a serious impediment to applying such measures to individual cases.

However, even within the limitations imposed by high levels of variance, I-P measures can potentially help resolve thorny assessment problems. For example, Bramham and Dalgleish (2000) assessed a man suspected of running a hydroponic drug production factory. This highly technical method of farming would have required the ability to read and understand complex manuals and instructions. Indeed, such documents were found in the man's house. However, the man claimed that the documents did not belong to him and that he was unable to read them as he had never learned to read. To test this he was administered a standard reading test and a Stroop task in which the to-be-ignored words were related to hydroponic farming. The rationale was that, if he was faking his poor reading ability, he would do badly on the standard reading test as he could just pretend that he could not read the words. However, the Stroop task crucially relies on the fact that skilled readers will *automatically* read words even when trying to name their colors. Consequently, it is not possible to override Stroop interference effects just because one wants to look like a nonskilled reader. Indeed, the data revealed that the man's Stroop interference was the same as one would expect from a skilled reader, even though his standard reading test performance was very bad, thus providing evidence that he was faking his poor reading ability. This study therefore shows how an I-P task can deliver something in assessment that a standard measure would miss.

With respect to intervention and prevention efforts, to the extent that I-P theories posit causal roles for one or more I-P biases or deficits, they suggest the potential value of interventions targeted directly at correcting those biases and deficits. For example, Posner and Rothbart (2000) suggested that it may be possible to enhance attentional control through training with very young children at risk for developing attention deficit disorder. Similarly, if an attentional bias in favor of threat cues is a risk factor for the development of anxiety disorders or contributes to their maintenance, then training directed at eliminating that attentional bias may prove helpful for treating such disorders or indeed preventing their onset. Training targeted at automatic I-P biases may be an especially useful adjunct to cognitive–behavioral treatments because such therapies really struggle to impact automatic processes, and yet it may be these very processes that prove to be the vulnerable individual's undoing in terms of onset of episodes and so on. Consequently, training can offer something that talking therapies cannot, and it has the advantage of being available over the Internet or as software. Although time will tell if such targeted interventions can effectively and efficiently correct such I-P biases and deficits, preliminary evidence suggests they can. As discussed previously, preliminary evidence suggests that such training is effective in treating chronic worry and generalized anxiety disorder (Hazen et al., 2002). Based on such results, such attentional retraining interventions appear to have considerable promise and may warrant evaluation in child and adolescent samples.

Conclusion

In summary, I-P approaches to studying child and adolescent psychopathology have considerable value,

as illustrated by the studies in this special section. However, they have the potential for considerably more value if the limitations and challenges highlighted in this article are addressed.

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