
Temporal Dynamics Linking Aspects of Parent Monitoring with Early Adolescent Antisocial Behavior

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Abstract

This longitudinal study examined the covariation between parents' use of control and solicitation, youth willingness to self-disclose to parents, and youth antisocial behavior from ages 13 to 14. Structural equation analyses were conducted on a combined sample of Italian (N = 152) and French Canadian (N = 151) adolescents. Analyses tested for longitudinal cross-lagged effects while controlling for stability and all concurrent associations. Although bivariate correlations showed consistent associations among these constructs, both concurrently and over one year, SEM results revealed virtually no cross-lagged effects, after controlling for concurrent associations and stability. These findings suggest that the actual causal effects of parenting and youth behavior may best be conceptualized as occurring in the moment, rather than over extended periods of time. Results also showed that parental control and solicitation demonstrated very different associations with youth antisocial behavior, and should therefore be considered separately for research and prevention.

Keywords: parent monitoring; adolescence; antisocial behavior

During the past 50 years, a great deal of research has been conducted to shed light on the role of parenting practices in general, and supervision in particular, in the development of problem behavior during adolescence (e.g., Hawkins & Catalano, 1992; Loeber & Dishion, 1983; McCord, 1979; Wilson, 1980). In the early 1980s, Patterson coined the term *parental monitoring* (Patterson, 1982; Patterson & Stouthamer-Loeber, 1984) to describe clinical observations of how parents of adolescents with clinically elevated levels of problem behavior failed to track the whereabouts of their children, set rules and expectations regarding behavior with peers, and supervise or guide children in their presence. In the ensuing 15 years, research has documented the empirical covariation between 'parental monitoring', child problem behavior, and child health (Dishion & McMahon, 1998; Duncan, Duncan, Biglan, & Ary, 1998; Patterson & Stouthamer-Loeber; Pettit, Bates, Dodge, & Meece, 1999; Steinberg, Fletcher, &

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Darling, 1994), including early-onset substance abuse (Chilcoat & Anthony, 1996; Chilcoat, Dishion, & Anthony, 1995).

Recent research, however, has led to a conceptual discussion regarding the contribution of youth disclosure in the monitoring process and the validity of previously used measures of parental monitoring (Kerr & Stattin, 2000; Stattin & Kerr, 2000). Specifically, these authors have argued that the covariation between parent monitoring and youth problem behavior is confounded by youth disclosure. It was thus suggested that parents' efforts to monitor and supervise their youth might actually have limited beneficial effects. This important hypothesis, to date, has not been adequately tested with longitudinal data. In the present study, a longitudinal design is used to test for concurrent and cross-lagged effects between the constructs of parental control, solicitation, and youth disclosure and antisocial behavior.

Separating Parental Control, Solicitation, and Youth Disclosure

Although many researchers of adolescent development agree that parental monitoring reflects a 'relationship property' rather than a parent-driven process (Crouter, MacDermid, McHale, & Perry-Jenkins, 1990; Dishion & McMahon, 1998), the weighting of diverse aspects of parental monitoring, as potential causal factors, varies considerably. For example, whereas much of the past theory and research has emphasized the importance of active parental monitoring, Stattin and Kerr (2000) and Kerr and Stattin (2000) concluded that active parenting has limited benefit for reducing adolescent antisocial behavior (see also Crouter & Head, 2002, for a discussion of related empirical work). This conclusion was based on evidence showing that only youth disclosure showed a reliable relation with adolescent norm-breaking behavior, when considered together with parental solicitation, parental control, youth hanging out, and three interaction terms involving hanging out. The conclusion, then, was that parent effects were small and non-significant compared with the contribution of the child's tendency to reveal important aspects of their behavior to caregivers.

This provocative conclusion challenged many socialization researchers who have generally assumed parent effects, and have neglected systematic tests of contrasting models of child effects. These data also suggest that interventions that promote parental monitoring are likely to be ineffective. Considering the theoretical and practical implications of these conclusions, it is very important to fully test them with longitudinal data.

A number of studies have followed up on the initial work of Stattin and Kerr (2000) in an attempt to test for unique effects of active parenting (control and solicitation) and youth disclosure. For example, using cross-sectional data, Soenens, Vansteenkiste, Luyckx, and Goosens (2006) tested the plausibility of direct effects of parenting (responsiveness, behavioral control, and psychological control) and indirect effects of parenting (passing through youth self-disclosure and parental knowledge) on youth delinquency, substance use, and affiliation with delinquent and substance-using peers. Results suggested that parental monitoring behaviors may have indirect effects on youth outcomes, passing through youth self-disclosure and parental knowledge. However, the lack of longitudinal data limits the possibility of inferring causal direction.

In another study following up on Stattin and Kerr's original work (2000), Fletcher, Steinberg, and Williams-Wheeler (2004) tested for effects of parental monitoring (similar to Stattin and Kerr's measure of solicitation), parent warmth, and parent control on adolescent substance use and delinquency, using longitudinal data across a

one-year period. Results showed consistent indirect effects of parenting behaviors (monitoring, warmth, control) passing through parent knowledge, as well as a concurrent direct effect of parental monitoring (solicitation) on substance use, and a significant direct effect of control on concurrent substance use and on Time 2 (T2) substance use and delinquency. However, in addition to not including youth disclosure, not all measures were considered at both time points, so full control of stability and concurrent associations could not be applied. As Anderson and Gerbing (1988) pointed out some years ago, the systematic testing of longitudinal causal effects requires the inclusion of all possible variables, and the inclusion of relevant stability and cross-lagged effects. In this example, it would have been important to model Time 1 (T1) and T2 monitoring as well as T1 and T2 youth problem behavior, to determine if parental monitoring accounted for unique variance in youth problem behavior at T2, controlling for youth problem behavior at T1, and all concurrent associations. Moreover, by including all cross-lag effects, such an approach would also allow the *simultaneous* tests of both parenting and youth effects.

Finally, in a third study following up on Stattin and Kerr's (2000) original work, Kerr and Stattin (2003) also conducted longitudinal analyses with results suggesting that parental monitoring has no or very weak effects on youth behavior, and that parenting behaviors appear to be driven by youth behavior (Kerr & Stattin). However, in this study, the authors surprisingly combined the solicitation and the control measures, despite their own arguments in the past to separate these parenting behaviors (Stattin & Kerr, 2000). Moreover, although a measure of disclosure was included, it was combined with several other measures (off-task behavior, dishonest charm) to create a variable called 'youth behavior in the family'. Combining measures and scales that represent very different behaviors may result in a poorly defined and poorly measured construct, attenuating relations with other variables. Moreover, this study also did not include all measures at both time points; so again, full control of stability and concurrent associations could not be applied.

In summary, the work of Stattin and Kerr (2000) has clearly shown that past research has not distinguished between the specific constructs of parental control, parental solicitation, and child disclosure, and therefore, conclusions are limited by this lack of construct specification. The added specificity to the parental monitoring construct and the addition of the disclosure construct are important contributions and have stimulated discussion and new research efforts to better understand the relations among these constructs.

Reciprocal Effects

In addition to distinguishing parent monitoring strategies (control and solicitation) from youth disclosure, Stattin and Kerr's (2000) work has also helped to focus attention on the child's role in determining parent-child interactions and parental monitoring (see Kerr & Stattin, 2003; Patterson & Fisher, 2002, for theoretical discussions of this topic). As a result, a number of recent studies have examined the question of reciprocal effects between youth and their parents. For example, Brody (2003) showed that, whereas parental monitoring (mother reported knowledge of youth behavior) longitudinally predicted reductions in aggressive and delinquent behavior, difficult youth temperament longitudinally predicted decreased maternal psychological functioning, which then predicted worsening parent-child relationship quality, which in turn diminished parental monitoring/knowledge. Also using a

longitudinal design, Laird and colleagues found that youth antisocial behavior predicted a deterioration in parent–child relationships, which in turn resulted in less parental ‘monitoring knowledge’ (knowledge of youth behavior; Laird, Pettit, Dodge, & Bates, 2003), and that youth delinquency and parental monitoring knowledge showed reciprocal cross-lagged effects over four annual assessments (Laird, Pettit, Bates & Dodge, 2003).

Finally, in an 8–10-year longitudinal study, Dishion, Nelson, and Bullock (2004) found that, over time, the parents of the early-starting antisocial boys disengaged from family management and monitoring (interviewer impressions of parental monitoring, relationship quality, and positive parenting behaviors toward child), and that the negative effects of having deviant peers was most pronounced for boys whose parents had decreased their family management practices.

Although these studies do not consider the same aspects of monitoring and disclosure that were defined by Stattin and Kerr (2000), together they do suggest that parenting activities and youth antisocial behavior are tightly linked, with reciprocal effects unfolding over time. This further emphasizes the need to consider all relevant concurrent and cross-lag effects when considering the developmental consequences of parenting, youth disclosure, and youth antisocial behavior.

Cross-lagged Effects vs. Synchronous Effects

Although temporal proximity of effects is not often specified in theories of parent and youth effects, the distinction between concurrent and cross-lag effects is theoretically and analytically very important. At an analytic level, the exclusion of such consideration may result in the mis-specification of a model. For example, one may test for temporally distal effects (cross-lagged over a long period of time) when the actual effects may be temporally proximal (T1 parenting influences T1 youth behavior). Conclusions from these analyses will not properly describe the actual relations among the variables. At a theoretical level, lack of specification may result in a theory that is overly general and vague, and that cannot be adequately tested. For example, if we hypothesize that parent monitoring today will have an effect on behavior a year from now, we must specify a mechanism for this effect, linking T1 parenting with T2 youth behavior (e.g., mediation through some third variable). Failure to specify such a link implies that parental monitoring has a ‘ghost-like action on distant behavior’ (taken from quantum mechanic’s ‘ghost-like action-at-a-distance’, see Mattuck, 1982). The idea of a ‘ghost-like action on distant behavior’ suggests that one variable has an effect across time on a second variable, without specifying a mechanism that links these variables across time (e.g., a third variable). Without this specification the theory is incomplete, and cross-lag effects may be the theoretical equivalent of ‘ghost-like action on distant behavior’.

In the context of parenting and youth behavior, we propose that the most parsimonious hypothesis is that parent behaviors first have an effect on contemporaneous youth behavior (e.g., antisocial behavior) or context (e.g., deviant peer affiliation), which is then stable through time. The same can be said for youth effects on parenting. According to this hypothesis, parent and youth effects are temporally proximal, and T1 to T2 effects will be the result of stability. This hypothesis specifies that when controlling for all concurrent effects and stability, previously significant cross-lag effects will drop to non-significant. Thus, this hypothesis follows the same logic as a mediational hypothesis by specifying that the effect of the causal variable on the outcome will be present

when excluding the meditational variable, but will not be present (or will be significantly reduced) when the meditational variable is included.

The lack of consideration given to the temporal proximity of effects is seen in the lack of attention given to testing fully cross-lagged models, and represents an important limitation in past research on these constructs. Although some research has examined longitudinal relations, both within an SEM framework and within a standard multiple regression framework, a full cross-lag analysis has never been conducted considering these specific constructs. By full cross-lag we mean that T1 and T2 measures of all relevant constructs are included, and that all relevant concurrent and cross-lag associations are included in the analyses. One study that did examine parental control and support, and which used structural equation modeling to control for stability and T2 concurrent associations, found reciprocal cross-lagged effects when considering youth substance use, but not externalizing symptoms (Stice & Barrera, 1995). However, the measure of parental control was very different from that proposed by Stattin and Kerr (2000). For example, one sample item was 'Dad frequently changed the rules I was supposed to follow'. This item clearly taps a different parenting strategy than is measured by Stattin and Kerr's measure (see Appendix). Moreover, although the authors conducted a cross-lag analysis, controlling for stability and T2 correlations, the T1 correlations were not included. Therefore, the conclusions from this study do not provide a test of the current conceptualization of parental monitoring, and do not fully control for concurrent correlations.

We are aware of only one longitudinal study (fourth to sixth grade) considering parenting practices and youth antisocial behavior that has applied fully controlled cross-lag analyses, and that has provided a clear comparison of concurrent vs. cross-lag effects (Vuchinich, Bank, & Patterson, 1992; these authors also included peer relations in their longitudinal analyses). In these fully controlled analyses, no cross-lagged associations were found between parent discipline (six-item observer rating of discipline and 'nattering') and youth antisocial behavior, even though bivariate correlations consistently demonstrated associations at T1, T2, and across time. The conclusion of these authors was that causal association among these constructs is best conceptualized as concurrent reciprocal effects. This earlier study, however, did not consider the recent distinctions between parental solicitation, control, and youth disclosure. Therefore, these results cannot be generalized to these more recent conceptualizations of parenting and youth disclosure.

The Present Study

In the present study, we considered longitudinal data to simultaneously test for concurrent and longitudinal cross-lag effects of parental control and solicitation, and youth disclosure and antisocial behavior, over a one-year period. The conceptual definition and measurement of parenting and child disclosure closely follow Stattin and Kerr's (2000) measurement (Kerr & Stattin, 2000), with only minor changes in the wording of some questions (see Appendix).

Two samples from two different countries (the Veneto region in northern Italy and the province of Quebec, Canada) were used in this study. Conducting a cross-national study provides a greater degree of confidence in the results, and allows for a greater degree of generalizability. Although cross-national differences between Canada and Italy have been shown to exist with regard to family relations (Claes, Lacourse, Bouchard, & Luckow, 2001), we did not anticipate that the functions of parenting

behaviors on youth behavior, or the effects of youth behavior on parenting, would differ across these contexts. Therefore, in the present study, although we test for differences across samples, we do not make specific hypotheses regarding differences in the relations among these variables. Instead, we test a general set of models including data from these two samples.

Method

Data collection for the two samples occurred during the same period of the year (although the Canadian data had been collected one year earlier), and we used the same procedures for the two samples. Because the same procedures and measures were used for both the Italian sample and the French Canadian sample, in the following section we first present a general description of the procedures and measures, then provide relevant information specific to each sample (e.g., descriptive statistics). It should be noted that the present study is part of a larger project examining diverse aspects of adolescent social development.

Participants

For both the Italian and French Canadian samples, letters were sent to the parents of all eighth-grade students in the participating middle schools. These materials explained the nature of the study and invited parents to sign a letter of informed consent if they agreed that they and their child would participate.

For the Italian sample, two middle schools participated, with a total of 244 eighth-grade students (i.e., potential participants). Signed consent letters were returned for 152 (62 percent of all potential participants) students. Of these participants, 69 (45 percent) were female and 83 (55 percent) were male. Their average age was 14 years. Almost all participants ($N = 146$; 96 percent) indicated that they were born in Italy and that their native language was Italian.

For the French Canadian sample, two middle schools participated, with a total of 260 eighth-grade students (i.e., potential participants). Signed consent letters were returned for 151 (58 percent of all potential participants) students. Of these participants, 90 (60 percent) were female and 61 (40 percent) were male. Their average age was 14.55 years. Most participants ($N = 123$; 82 percent) indicated that they were born in Canada. This sample, however, demonstrated more variability with regard to native language, with 96 (64 percent) reporting that French was their native language, 16 (11 percent) Spanish, 9 (6 percent) Creole, 4 (3 percent) Vietnamese, 4 (3 percent) English, 4 (3 percent) Portuguese, 2 (1 percent) Chinese, 1 Italian, and 15 (10 percent) some other language.

These data show that the Italian sample was very homogeneous with regard to nationality and native language, and that the French Canadian sample was more diverse. Moreover, although the mean ages of these samples seem very similar, the French Canadian sample demonstrated more variability on this measure also: 90 percent of the Italian sample were 14 years old, almost 10 percent were 15 years old, and 1 participant was 16 years old, whereas for the Canadian sample, 58 percent were 14 years old, 31 percent were 15 years old, 8 percent were 16 years old, and nearly 3 percent were 17 years old.

Demographic variables also suggest that the Canadian sample was more diverse and possibly a higher risk sample. For example, 86 percent of the Italian parents

reported owning their own home, and 12 percent reported renting, whereas 27 percent of the Canadian parents reported owning their own home and 71 percent reported renting. None of the Italian parents reported being homeless or living in a shelter, whereas 5 percent of the Canadian parents reported being homeless or living in a shelter. None of the Italian parents reported being unemployed, whereas 9 percent of the Canadian parents reported being unemployed. With regard to marital status of the responding parent, 89 percent of the Italian sample was married, none were single, and 9 percent were divorced or separated, whereas 36 percent of the Canadian sample was married, 23 percent were single, and 25 percent were divorced or separated. Finally, 86 percent of the Italian sample was in custody of both parents, whereas 43 percent of the Canadian sample was in the custody of both parents. The Canadian sample was from a large metropolitan city (>2 000 000 residents), whereas the Italian sample was from a moderate-sized industrial city (circa 200 000 residents).

At Wave 1, 12 parents from the Italian sample and 11 parents from the Canadian sample did not return the parent report questionnaire. Thus, at Wave 1, parent data were available for 140 families in both the Italian and Canadian samples (92 percent of participating families). At Wave 2, data were available for 142 of the Italian youth (93 percent), 124 of the Italian parents (82 percent), 144 of the Canadian youth (95 percent), and 143 of the Canadian parents (95 percent). Thus, retention was very high for both samples. Descriptive statistics cited in the Method section are based on all available data at each wave and for each measure.

Measures

Parental Monitoring and Child Disclosure. To measure parental monitoring behaviors and child disclosure, we used the measures developed by Stattin & Kerr (2000; Kerr and Stattin, 2000); This measure includes three subscales: control, solicitation, and disclosure (see Appendix). The same items were used for both child and parent reports. A five-point response scale was used for all items. Although the wording of the five responses was adapted for each item, the overall ratings were the following: 1 (*almost never*), 2 (*rarely*), 3 (*sometimes*), 4 (*often*), 5 (*very often*). All three subscales, across both child and parent reports, across both assessment waves, and across both samples, demonstrated adequate internal reliability, with Cronbach's alphas ranging from $\alpha = .60$ to $\alpha = .85$.

As noted in the introduction, minor changes were made to some questions. This was done in order to improve accuracy of the questions with respect to who is initiating the behavior and what behavior is being initiated. For example, one solicitation item originally asked 'How often do you usually want to tell your parents about school?' and was changed to 'How often do you tell your parents how you are doing in school, without them asking?' The change was made to eliminate 'want to tell' and focus on actually telling, which more specifically taps the construct of disclosure. For the control scale, because most questions in the original version focused on activities in the evening, we dropped one item regarding evening activities and replaced it with a question regarding finishing homework before going out with friends (see Appendix).

To test for mean level differences in the parental monitoring constructs across the Italian and Canadian samples, we conducted four one-way multivariate analyses of variance (MANOVAs)—one for the child report measures and one for the parent report

measures, and separately for the year 1 and year 2 data. In the first MANOVA, testing for differences across countries in the child report measures at year 1, a significant multivariate effect was found for country, Wilks's lambda = .79; $F(3, 298) = 26.77$, $p < .001$. Follow-up univariate analyses showed that the youth in the Italian sample reported higher levels of solicitation ($M = 3.00$, $SD = .87$) and control ($M = 3.70$, $SD = .82$) than did the Canadian youth ($M = 2.50$, $SD = .86$; $M = 3.13$, $SD = .82$, respectively). Youth from the Canadian sample reported marginally higher levels of disclosure ($M = 3.33$, $SD = .88$) than did the Italian sample ($M = 3.15$, $SD = .88$, $p < .07$). In the second MANOVA, testing for differences across countries in the parent report measures at year 1, a significant multivariate effect was again found for country, Wilks's lambda = .84; $F(3, 275) = 17.20$, $p < .001$. Follow-up univariate analyses showed that the parents in the Italian sample reported higher levels of disclosure ($M = 4.04$, $SD = .69$), solicitation ($M = 3.67$, $SD = .68$), and control ($M = 4.44$, $SD = .50$) than did the Canadian parents ($M = 3.83$, $SD = .82$; $M = 3.11$, $SD = .81$; and $M = 4.12$, $SD = .64$, respectively).

When these analyses were repeated for the second year of data, the same pattern of results emerged, with two exceptions. First, youth-reported disclosure dropped from marginally significant to not significant. Second, parent-reported disclosure dropped from significant to marginally significant ($p = .07$). All other significance tests, both multivariate and univariate, and directions of mean differences, led to the same conclusions at year 2 as had been observed at year 1.

To test for mean level differences in the parental monitoring constructs across males and females, we again conducted four one-way MANOVAs (similar to those reported above). Of these four analyses, only the MANOVA testing parent reports at T2 was significant $F(3, 261) = 3.14$, $p < .05$. The remaining three MANOVA's were statistically not significant (all F s < 1.55). A follow-up analysis of the only significant MANOVA revealed that of the three parent reported measures at T2, only solicitation demonstrated a significant gender effect ($F(1, 263) = 5.10$, $p < .05$: the parents of boys reported higher levels of solicitation $M = 3.39$, than the parents of girls $M = 3.17$). Overall, these results suggest that males and females generally did not differ on these measures.

Youth Self-report of Antisocial Behavior. To measure self-reported antisocial behavior, we used a modified version of a previously developed youth antisocial behavior scale (Metzler, Biglan, Ary, & Li, 1998). This modified version contained 12 items tapping a variety of antisocial behaviors. Example items included 'skipped school without an excuse', 'stole or tried to steal things worth \$5 or more', 'purposefully damaged or tried to damage property'. Responses were given on a 6-point scale ranging from 1 (*never*) to 6 (*more than 20 times*). This scale demonstrated adequate internal consistency for both the Italian sample ($\alpha = .76$ at year 1, $\alpha = .85$ at year 2) and for the Canadian sample ($\alpha = .78$ at year 1, $\alpha = .74$ at year 2). There was no significant difference between mean levels of self-reported antisocial behavior across the two samples at either year 1 ($t = .59$, $df = 295.36$, NS; $M = 1.46$, $SD = .44$, for the Italian sample, and $M = 1.49$, $SD = .50$, for the Canadian sample) or year 2 ($t = .143$, $df = 274.83$, NS; $M = 1.44$, $SD = .48$ for the Italian sample, and $M = 1.36$, $SD = .40$ for the Canadian sample).

Youth Self-report of Substance Use. To measure youth substance use, we developed a self-report scale asking how often in the past month the youth had smoked cigarettes,

drunk beer, drunk wine, drunk wine coolers (for the Italian sample we used the term *spritz*, which is similar to a wine cooler), drunk hard alcohol, and smoked marijuana. Responses were given on a 14-point scale ranging from '0' to '41 or more times' in the past month. This scale demonstrated adequate internal consistency for both the Italian sample (standardized $\alpha = .75$ at year 1, standardized $\alpha = .81$ at year 2) and for the Canadian sample (standardized $\alpha = .85$ at year 1, standardized $\alpha = .74$ at year 2). Because of differences in the frequency distributions across substance types, the item scores were standardized before combining them into a scale score. This was done within each sample, and therefore, both samples had a mean level of substance use equal to zero, and no group comparisons could be tested. However, it should be noted that there were fairly large differences across the groups in terms of the percentage of youth who reported having *ever* used each substance. Specifically, for the Canadian sample, 49 percent had smoked cigarettes, 66 percent had drunk beer, 72 percent had drunk wine, 56 percent had drunk wine coolers, 46 percent had drunk hard alcohol, and 39 percent had smoked marijuana, whereas for the Italian sample, 32 percent had smoked cigarettes, 54 percent had drunk beer, 59 percent had drunk wine, 35 percent had drunk wine coolers (*spritz*), 30 percent had drunk hard alcohol, and 3 percent had smoked marijuana. Thus, in all cases, a larger percentage of the Canadian youth reported having ever used the specific substance, with differences ranging from 12 to 36 percentage points. This same pattern was also observed at year 2, with a high percentage of Canadian youth using all types of substances, compared with the Italian sample.

Parent Reports of Youth Antisocial Behavior. Parent reports of child antisocial behavior were based on a modified version of the problem checklist (Dishion & Kavanagh, 2003). This version consisted of 15 items regarding diverse types of antisocial behavior. Example items included 'arguing with or talking back to an adult', 'taking things that didn't belong to her/him', 'purposefully destroying property', 'smoking cigarettes', and 'drinking alcohol'. Responses were given on a 5-point scale ranging from 1 (*never or almost never*) to 5 (*always or almost always*). This scale demonstrated adequate internal consistency for both the Italian sample ($\alpha = .76$ at year 1, $\alpha = .81$ at year 2) and for the Canadian sample ($\alpha = .79$ at year 1, $\alpha = .81$ at year 2). At year 1 there was a significant difference between mean levels of parent-reported antisocial behavior across the two samples ($t = 3.81$, $df = 275.70$, $p < .001$), with the parents in the Canadian sample reporting higher levels of youth antisocial behavior ($M = 1.92$, $SD = .43$) than did the parents of the Italian sample ($M = 1.74$, $SD = .39$). However, at year 2 there was no difference ($t = .04$, $df = 258.52$, NS; $M = 1.77$, $SD = .45$ for the Italian sample, and $M = 1.78$, $SD = .44$ for the Canadian sample).

Translation of Measures

All measures were separately translated from English to Italian and French by individuals who were fluent in English and whose first language was Italian or French. Following this initial translation, a back translation was conducted by a second person for each language. When differences were identified, they were discussed until agreement was established. Next, a person whose first language was French, and who was also fluent in Italian, then compared the Italian and French translations. Again, when questions about match were raised, these were discussed with the other individuals involved in the translation, until agreement was achieved.

Results

Combining Groups

In the present analyses we combine the Italian and Canadian samples. One motivation for doing so was the high number of estimated parameters relative to the number of subjects in each national sample. Moreover, although the covariance matrices were not equivalent across samples ($\chi^2 = 53.35$, $df = 36$, $p = .03$), a comparison of the structural coefficients showed no group differences ($\chi^2 = 8.03$, $df = 4$, $p = .09$). Finally, using the Fisher *r*-to-*Z* transformation, and the Benjamini and Hochberg (1995) adjusted *p* values (for multiple comparisons), only 1 of the 28 correlations was found to differ across groups (the correlation between disclosure at T1 and disclosure at T2; $r_{\text{italian}} = .58$ and $r_{\text{canadian}} = .79$). Therefore, given the lack of differences in structural coefficients and in the correlations, and because the limited number of participants in each sample, these samples were combined for the following analyses.

We also compared all correlations across males and females, within each country, using the Fisher *r*-to-*Z* transformation, and the Benjamini and Hochberg (1995) adjusted *p* values. For the Italian sample, of the 28 correlation pairs, no differences were found. For the Canadian sample, only one significant difference was found (the correlation between control at T1 and control at T2; $r_{\text{males}} = .42$ and $r_{\text{females}} = .74$). Although significantly different, both correlations are positive and strong, and generally lead to the same conclusion. Because of a lack of differences in these correlations (1 significant difference in 56 comparisons), males and females were combined for the following analyses.

It is also important to note our rationale for combining parent and youth reports, instead of conducting separate analyses on these data. This was done because past research, using the same measures of parenting and disclosure, has provided strong evidence that the overall pattern of associations with parental knowledge and youth behavior were extremely similar across youth report and parent report, and provided no evidence of important differences across these respondents (Stattin & Kerr, 2000). Similarly, using very different measures of parenting, Laird, Pettit, Bates, et al. (2003) presented cross-lag analyses showing that parent reports and youth reports resulted in essentially the same results and conclusions. Therefore, because past research does not indicate that these measures should be analyzed separately, in order to minimize the number of redundant analyses, and the possibility of finding chance differences, youth and parent reports were combined.

Missing Data

Finally, the structural equation models presented below were conducted without imputing missing data. It should be noted, however, that the final model (1c) was also conducted after imputing missing data. Imputation was conducted using the estimation procedures provided by LISREL 8 (Jöreskog & Sörbom, 1996), and using all existing T1 variables to estimate the missing T2 variables. Eighteen more participants were included in this analysis following imputation (following imputation $N = 302$, missing data for 1 participant could not be imputed). The results and significance tests were near identical to those presented below, and led to the same conclusions (the largest difference in the standardized path coefficients across analyses with and without missing data imputation was $<.03$).

Structural Models

Structural equation modeling (SEM; LISREL 8, Jöreskog & Sörbom, 1996) was used to test for longitudinal cross-lagged effects while controlling for concurrent correlations and construct stability. Specifically, three structural equation models were conducted. In the first model (Figure 1a), we examine only the cross-lag effects, excluding the stability and concurrent associations. This model simply tests whether each cross-lag effect is significant after controlling for the other cross-lag effects, and how much of the variance in each construct could potentially be explained by these cross-lag effects. Changes in fit indices comparing this model with the others will not be considered. In the second model (Figure 1b) we include all concurrent relations and all stability coefficients, but exclude the cross-lag effects. This model is nested in the full model (Figure 1c), which includes all concurrent associations, all stability coefficients, and the same cross-lagged associations that are included in the first model. Because model b is nested within model c, and because the difference across these models is the exclusion of the cross-lagged effects in model b, comparing the fit indices across these two models will allow us to test whether the inclusion of these cross-lagged effects results in a significant improvement in the model.

In these analyses, the following two cross-lagged associations were excluded: the paths going from solicitation to control, and from control to solicitation. These paths were excluded (1) to save degrees of freedom; (2) because, although it was hypothesized that these parenting behaviors would be concurrently correlated, there is no reason to hypothesize that they would be longitudinally predictive of one another; and (3) because the main focus of the present study was on the concurrent and longitudinal associations between youth behavior and parenting behaviors.

Our initial strategy for analyzing these data was to create latent constructs for control, solicitation, and disclosure. However, when these latent constructs were used for the longitudinal analyses, results indicated that the residuals for the parent report measures were highly correlated, and that in order to obtain a good-fitting model, we needed to free these correlations across constructs within assessment wave, across assessment wave within construct, as well as across constructs across assessment waves. Therefore, in these analyses, mean scores for each construct were created by combining the parent and youth reports. This was done for the control, solicitation, disclosure, and antisocial behavior constructs. The correlations between the parent and child reports of control, solicitation, and disclosure ranged from $r = .28$ to $r = .42$ at year 1, and from $r = .32$ to $r = .44$ at year 2, and the correlations among the three measures of antisocial behavior (parent report, child report, and self-reported substance use) ranged from $r = .24$ to $r = .47$ at year 1, and from $r = .34$ to $r = .60$ at year 2. If youth report data were available, but not parent report data, the score used was simply the youth report, and if parent report data were available, but not youth report data, the score used was simply the parent report. These scores were standardized separately for the Italian and the Canadian samples before calculating the correlation matrix used in the SEM analyses. The final sample used in this analysis was 284 (19 participants were missing both parent and youth reports at year 2). These mean scores were treated as latent constructs and their residuals were fixed to 15 percent. Mean scores were treated as latent constructs so that the year 1 concurrent correlations could be appropriately controlled for, which is not possible when using manifest variables (e.g., when using manifest variables the inclusion or exclusion of T1 concurrent correlations will have no effect on the rest of

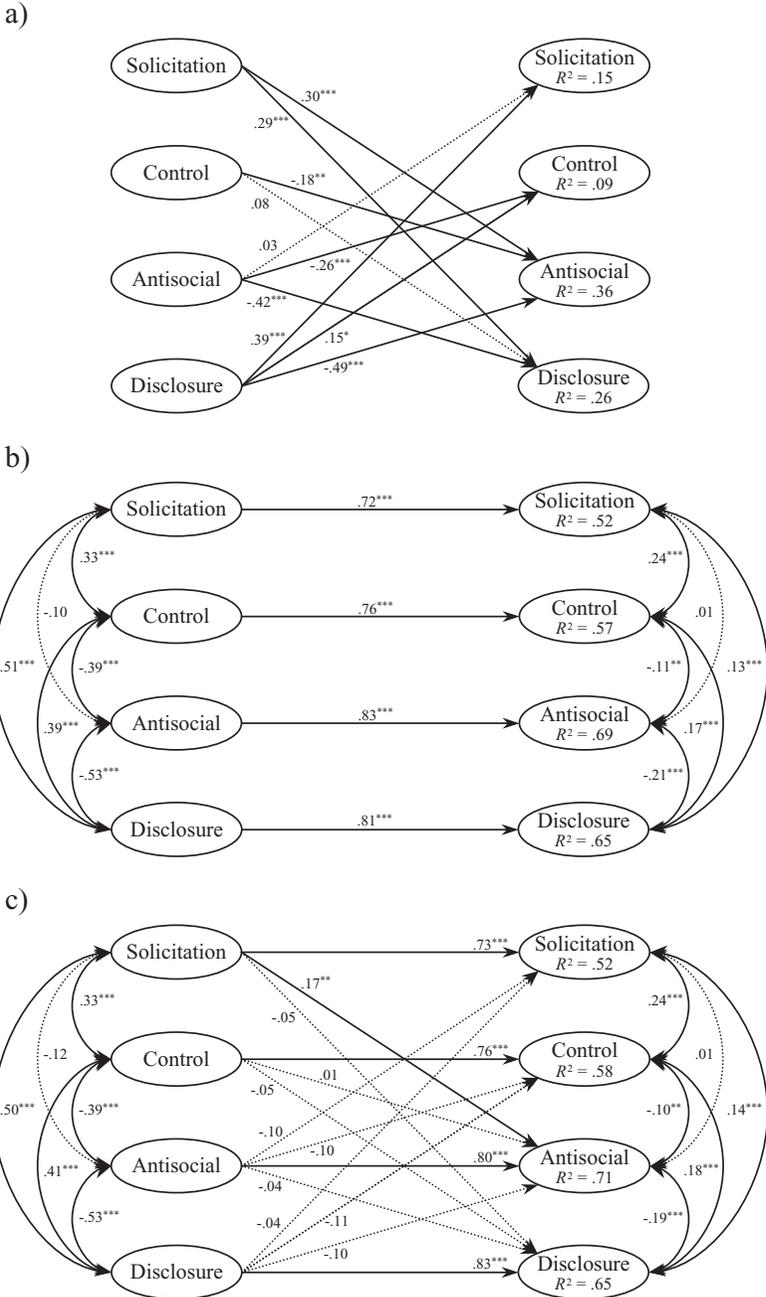


Figure 1. Results of structural equation model testing for longitudinal cross-lag effects while controlling for concurrent relations. * $p < .05$, ** $p < .01$, *** $p < .001$, solid lines indicate statistically significant relations.

the model, because those correlations are not actually estimated and cannot account for any variance in the measures). Residuals were fixed at 15 percent by considering the set of reliability indicators that were available (alphas, stability coefficients). For example, the stability coefficients can be considered as a minimum lower end estimate of reliability (all of which were high, as were the alpha coefficients). Based on these indices we considered 15 percent to be a safe and reasonable estimate.

The bivariate correlations used in this analysis are presented in Table 1. Two important conclusions can be drawn from these correlations. First, the pattern of correlations among these constructs is very similar at both year 1 and year 2. Second, the pattern of longitudinal correlations are very similar to the pattern observed at each time period separately (e.g., $r_{T1Control\ T1Antisocial} = -.33$, $r_{T2Control\ T2Antisocial} = -.30$, $r_{T1Control\ T2Antisocial} = -.24$, and $r_{T1Antisocial\ T2Control} = -.28$).

The results from the SEM models are presented in Figure 1. The first model did not fit the data well $\chi^2 = 535.74$, $df = 18$, $p < .0001$; root mean square residual (RMSEA) = .32; comparative fit index (CFI) = .30; goodness of fit index (GFI) = .68. However, as can be observed in the standardized path coefficients, all but two are significant, even after controlling for the other cross-lag effects.

The second model did fit the data well $\chi^2 = 13.18$, $df = 12$, $p = .36$; RMSEA = .02; CFI = .99; GFI = .99. The overall pattern of results shows that the year 1 concurrent associations and stability coefficients were similar to those observed with the bivariate correlations. However, the year 2 concurrent associations were much weaker than the bivariate correlations, indicating that much of the year 2 correlations are attributable to earlier associations and stability in these constructs.

The full model also fit the data well, $\chi^2 = .65$, $df = 2$, $p = .72$; RMSEA = .00; CFI = .99; GFI = .99. The correlations among the year 1 and 2 constructs are very similar to those observed in the second model. Of the 10 cross-lag effects, only 1 was significant and of low magnitude (the path from year 1 solicitation to year 2 antisocial was $\beta = .17$, $p < .001$); all other cross-lag effects were close to zero and not significant.

The comparison of the second and third models, using a change in χ^2 , showed that the final model did not fit the data significantly better than the second model ($\Delta\chi^2 = 12.53$, $\Delta df = 10$, $p = .25$). Thus, the most parsimonious model is the model excluding the cross-lag effects, although the one significant cross-lag effect (year 1 solicitation to year 2 antisocial behavior) should be considered.

Overall, the pattern of results suggests that the longitudinal relations among these constructs are almost entirely accounted for by the year 1 correlations and by the stability in these constructs. Moreover, the correlations among the year 2 constructs are partially, but not entirely, accounted for by year 1 associations and stability.

Discussion

This study was conducted to test for both concurrent and longitudinal relations between parental monitoring strategies and youth disclosure and antisocial behavior. Results provide insight for understanding both the cross-sectional and longitudinal relations among these constructs.

First, our results suggest that the possible causal relations among these constructs are likely to be synchronous. For example, an association between parenting and youth antisocial behavior is likely to depend on its cumulative concurrent effects at earlier time points, and a concurrent effect at the moment of assessment. Thus, today's

Table 1. Correlations Used in the Longitudinal Structural Equation Model (N = 284)

	Control T1	Solicitation T1	Disclosure T1	Antisocial T1	Control T2	Solicitation T2	Disclosure T2	Antisocial T2
1	1							
2	.28***	1						
3	.35***	.43***	1					
4	-.33***	-.10	-.45***	1				
5	.65***	.16**	.21***	-.28***	1			
6	.25***	.60***	.32***	-.13*	.35***	1		
7	.25***	.30***	.68***	-.38***	.30***	.34***	1	
8	-.24***	.03	-.37***	.71***	-.30***	-.02	-.48***	1

* $p < .05$, ** $p < .01$, *** $p < .001$.

parenting may have an effect on child behavior next year because it had an effect on behavior today. Similarly, today's child behavior may affect later parenting because it had an effect on parenting today. Past research also has provided support for the importance of concurrent effects relative to predictive longitudinal effects (Duncan et al., 1998; Patterson, 1993).

Thus, disentangling the solution of 'which variable causes which variable', as it occurs in daily life without intervention, may not be possible with traditional correlational cross-lag analyses. That is to say, if the effects are synchronous, those effects will be carried in the concurrent associations, and will not be detectable in the cross-lag paths, resulting in null cross-lagged effects. As suggested by Patterson and Fisher (2002), 'Each parenting variable reflects unknown joint contributions of parent and child to the compliance training process (p. 75).' These contributions appear to have temporally proximal effects, which then remain stable over time.

As suggested in the introduction, the distinction between concurrent and cross-lagged effects is essential for both theory and proper data analysis. The specification of cross-lagged effects, without a mechanism that can link the T1 variable with the T2 outcome, is a theoretical equivalent to what we have called a 'ghost-like action on distant behavior'. In the present analyses, the links between the T1 variables and the T2 variables are the concurrent associations and the stability in those same variables. Although robust bivariate and multivariate associations were found among these variables across time (e.g., T1 control and T2 antisocial behavior; see model 1a for multivariate cross-lag effects without controlling for stability and concurrent effects), these associations dropped to near zero after controlling for concurrent associations and stability. As stated above, our conclusion is that the effects between these variables are synchronous, which then remain stable over time.

The difference in findings between the control and solicitation constructs provides insight for prevention and intervention programs. It appears that solicitation provides no benefit, and as suggested by Stattin and Kerr (2000), may even be deleterious. High levels of control, on the other hand, was concurrently associated with lower levels of youth antisocial behavior. Thus, parental control, as indicated by having clear expectations and rules about youth activities, may be the best target for prevention and intervention. Experimental studies, specifically focusing on parental control (as measured in this study), will be needed to verify this approach. For example, random assignment prevention trials could compare parent-training programs that either include parental control as a target behavior, or some other parenting strategy such as solicitation.

Solicitation was the only variable to show a cross-lagged effect on youth antisocial behavior, and the direction of that effect suggests that high solicitation is associated with increases in later antisocial behavior. One possible interpretation for this effect is that solicitation decreases the quality of the parent-child relationship (mediating variable), thus putting the youth at risk for escalation in antisocial behavior. In this case, the deterioration in the parent-child relationship would provide the link between the T1 parenting and T2 youth behavior, thus avoiding an explanation based on 'ghost-like action on distant behavior'. To test this hypothesis it would be important to measure parent-child relationship quality, as well as other possible mediators, such as the youth distancing themselves from their parents. Moreover, this effect may depend on the parents' style when soliciting and whether the parents' solicitation is overly invasive or motivated by the child's behavior. Therefore, it could be important to measure the style and reason for solicitation. The complete absence of concurrent associations between

solicitation and youth behavior fails to shed light on the underlying mechanism of this effect. Based on the present findings it would be overly speculative to turn this finding into a concrete recommendation.

It is important to note that, finding parental control to be non-predictive of later antisocial behavior, after controlling for prior correlations and stability, does not imply that targeting these parenting strategies would be inefficacious for improving child outcomes. As discussed earlier, the actual effects may be concurrent, and thus not observable with cross-lagged analyses after fully controlling for concurrent correlations and stability. Moreover, there is evidence that, although many parents give up on family management practices (Dishion et al., 2004), this trend can be changed, as revealed by randomized intervention studies that target the monitoring process (Dishion, Nelson, & Kavanagh, 2003; Stanton et al., 2004; see also Eddy & Chamberlain, 2000, for similar results with chronic offending youth in *Treatment Foster Care*). These researchers showed that random assignment to a brief intervention that focused on parental monitoring reduced parental disengagement, which in turn mediated changes in youth substance use. Given the adolescents' lack of involvement in the intervention, this study suggested that parents can positively influence youth development by changing their behavior in a way that 'looks like' improved parental monitoring, as defined by Dishion and McMahon (1998).

Two comments regarding the measures of parental monitoring should be made. First, we suggest that the term *control* may be an unfortunate label for the questions used for that construct, and perhaps leads to a negative bias regarding the importance of this set of parenting behaviors. We propose that a more fitting name for the control construct would be *rules and expectations* because it is more consistent with the common set of items used across these studies. This is consistent also with the conclusion offered by Hayes, Hudson, and Matthews (2004) that 'adequate rules appear to form the foundation for better supervision and less conflict, and hence lower levels of problem behaviors' (p. 587). Adapting the name for this construct to more accurately reflect the content of the items will also avoid confusion of this measure with other constructs such a psychological control.

Second, our data clearly show that the control and solicitation constructs should be considered separately. As discussed above, these measures showed very different patterns of relations with youth antisocial behavior: Control demonstrated a pattern of robust negative bivariate associations with youth antisocial behavior but no cross-lagged effect, whereas solicitation showed only a weak positive cross-lag effect and no concurrent associations. Thus, it is important not to combine these measures into one construct. Doing so may result in a weak measure that shows no relations with important outcome variables.

Comment should also be made regarding the high level of stability of all measures. It could be argued that cross-lag effects become statistically difficult to find when stability is very high. However, it should be noted that 29 to 48 percent of the variance in these constructs was not explained, suggesting that there remains a large portion of the variance not attributable to the present set of variables—including stability. Thus, it is not likely that the lack of cross-lagged effects is an artifact of having already explained too much of the variance. Moreover, to the extent that these high levels of stability accurately reflect reality, our conclusion that associations among these variables are primarily determined by concurrent events and behavioral stability (of parents and youth) is further supported.

Finally, three important limitations of the present study should be noted. Although this study considered longitudinal data, the time period was fairly short (one year) and fairly late in development (middle adolescence). Different effects may be found if considering different or longer developmental periods. Considering a different set of parenting practices, Pardini, Fite, and Burke (2008) found that the bidirectional effects between youth behavior and parenting practices changed from age 6 to age 16, for some measures. For example, the bidirectional effects of parent monitoring and youth behavior were found to strengthen from childhood to early adolescence, although by age 15 parent monitoring no longer predicted later youth behavior, whereas youth behavior continued to predict later parenting. These results are similar to results presented by Laird, Pettit, Bates, et al. (2003) who found that the reciprocal effects of youth delinquency and parental knowledge showed an increase in effect size from grade 9 to grade 12, when considering parent-reported delinquent behavior. Thus, further research is needed to better understand how the reciprocal effects of specific parenting practices and child behaviors change throughout childhood and adolescence.

A second limitation regards the generalizability of the present results. Although we believe that the inclusion of two very diverse samples maximizes the external validity/generalizability of these results, it is very possible that these findings may not apply to very high-risk contexts. Therefore, caution should be taken when applying these results to such contexts.

The third limitation regards the number of participants in the present study. Although the number of participants of the combined sample was sufficient to test our hypotheses, it would have been interesting to consider contextual variables, such as single parenting and neighborhood effects, as possible moderators of the pattern of findings. To do this, however, would require a much larger sample.

In conclusion, because the actual effects of parents on youth, and youth on parents, are likely to be reciprocal, evolving over time, and temporally proximal, correlational cross-lag studies are likely to be inadequate for fully understanding the causal relations among these variables. We should be careful, therefore, when using such research designs to establish the null hypothesis that parenting behaviors have no effect on youth during adolescence. Parenting and youth effects may be present in the form of concurrent effects. Our study provides compelling evidence that longitudinal correlations among parenting and youth behavior are almost fully accounted for by considering concurrent relations and construct stability. Future research must more carefully consider the possibility that parent and youth effects may best be considered in terms of temporally proximal effects. Future research and theory must also consider the importance of random assignment to intervention/prevention trials as a way of testing whether parents can be instrumental in guiding their youth through adolescent development, and in helping their youth avoid negative developmental trajectories.

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AppendixItems Used in the Parental Monitoring–Child Disclosure Questionnaire (*Child Report Version*)

Subscale	Item
Disclosure	
1	How often do you tell your parents how you are doing in school, without them asking?
2	Do you keep secrets from your parents about what you do during your free time?
3	Do you hide a lot from your parents about what you do during nights and weekends?
4	How often do you spontaneously tell your parents, without them asking you, what you do when you go out during the evening?
5	How often do you spontaneously tell your parents, without them asking you, about your friends and what you do together?
Solicitation	
6	In the past month, how often have your parents talked with the parents of your friends?
7	How often do your parents ask your friends what they like to do or what they think about different things?
8	During the past month, how often have your parents started a conversation with you about what you do during your free time?
9	How often do your parents ask you about things that happened during school?
10	How often do your parents ask you to talk about your friends and what you do together?
Control	
11	Do you need to have your parents' permission to stay out late on a weekday evening?
12	Do you need to ask your parents before you decide what you and your friends will do on a Saturday evening?
13	Do your parents always require that you finish your homework before going out with your friends?
14	Do you need your parents' permission before you spend money?
15	Do you need your parents' permission before going to a friend's house?