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Keijsers, L.

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Parental monitoring and adolescent problem behaviors: How much do we really know?

Loes Keijsers[1,2]

Abstract

This article aims to provide a critical analysis of how much we know about the effectiveness of parental monitoring in preventing adolescent delinquency. First, it describes the historical developments in parental monitoring research. Second, it explains why it is uncertain whether causal inferences can be drawn from contemporary research findings on the link of parenting and adolescent problem behaviors. Third, it is empirically demonstrated, using Random-Intercept Cross-Lagged Models, how distinguishing between-person and within-person associations may alter or strengthen conclusions regarding the links of parental monitoring and adolescent disclosure with adolescent delinquency. Previously detected correlations between parental monitoring and adolescent delinquency were not present at the within-family level. However, there were significant associations between within-person fluctuations in disclosure and delinquency. Together, these models provide stronger evidence for a potential causal link between disclosure and delinquency, but also suggest that previously detected linkages of parental monitoring and delinquency can be explained by stable between-person differences rather than causal processes operating within families.

Keywords

Parental monitoring, adolescent disclosure, parental control, parental solicitation, longitudinal, cross-lagged model, within-person

Over the course of adolescence, developmental changes occur in children's lives and their relationships with their parents. Adolescents are increasingly allowed to spend their leisure time with friends (Larson, Richards, Moneta, Holmbeck, & Duckett, 1996). In the hours that are spent with friends, there is not always a parent or other adult present to supervise their activities. Therefore, with increasing adolescent age, parents are less able to keep a watchful eye on their teenager's behaviors and activities (Keijsers, Frijns, Branje, & Meeus, 2009). An increase in unsupervised and unstructured leisure time may provide opportunities to experiment with shoplifting, vandalism, or other types of minor delinquencies (e.g., Moffitt, 1993; Mahoney & Stattin, 2000).

At this point, it is still quite unclear what parents can do to prevent adolescent engagement in norm-breaking and delinquency. One parenting practice that has been proposed to prevent delinquency is parental monitoring. Parental monitoring is defined as a set of parenting behaviors aimed at paying attention to and tracking the child's whereabouts, activities, and adaptations (Dishion & McMahon, 1998). The idea behind it is that parents who are sufficiently aware of what is going on in the lives of their children and what may be going wrong can undertake appropriate action. They can support and comfort their child when needed, and help the child make more responsible decisions in the future when transgressing societal norms or the law. Following these lines of reasoning, adequate parental monitoring could potentially be crucial to help a child to safely and successfully navigate through the adolescent years. But empirical research indicates that it is not that easy. In fact, whether or not parental monitoring is an effective deterrent of problem behaviors has been at the core of a scientific debate.

So, how much do we know about parental monitoring? In a first part, I will review the progression in parental monitoring research over the last 60 years and also indicate what the open questions are at this point. In a second part, I will discuss why there is a potential misfit between the questions parents have about how they should monitor their child's behaviors, and the phenomena that are quite commonly studied. Going one step further, in a third part, I will empirically demonstrate that studying processes in parental monitoring research, using longitudinal data and cross-lagged models, may not adequately capture causal processes operating within families. Ultimately, I will indicate some of the methodological challenges ahead.

Parental Monitoring Research

Early Views

The scientific interest in parental monitoring as a potentially protective factor against adolescent delinquency stems back to the nineteen fifties. In the work of Glueck and Glueck (1950, p. 130), mothers of delinquent youth, in comparison to mothers of non-delinquent youth, were more likely to inadequately supervise their children (i.e., being in the auditory and visual field of a child). Inspired by these findings, in the 1970s and 1980s, measurements

[1] Faculty of Social Sciences, Utrecht University, The Netherlands
[2] Faculty of Life Science, Humboldt University Berlin, Germany

Corresponding author:
Loes Keijsers, Faculty of Social Sciences, Department Youth & Family, Utrecht University, PO Box 80140, Utrecht, 3508 TC, The Netherlands.
Email: l.keijsers@uu.nl

of parental monitoring were incorporated in large surveys. At the time, the construct was operationalized as a multi-informant concept encompassing awareness of parents, parent–child communication, and interviewers' impressions of monitoring. These studies confirmed that youth with lower levels of parental monitoring were more delinquent. In fact, three large studies (Loeber & Dishion, 1983; Loeber & Stouthamer-Loeber, 1986; Patterson & Stouthamer-Loeber, 1984) showed that family management practices, and parental monitoring in particular, were among the best statistical predictors of adolescent delinquency. For instance, parental monitoring was a stronger predictor of delinquency than were structural family factors, such as single-parent status (meta-analysis: Loeber & Stouthamer-Loeber, 1986). Adequate parental monitoring of friendships, whereabouts and leisure time of adolescents was identified as a key risk factor for juvenile offending. Accordingly, parental monitoring was incorporated as a core component in many theories on the development of crime (e.g., Gottfredson & Hirschi, 1990; Patterson & Reid, 1982) and interventions for antisocial conduct and delinquency were increasingly aimed at improving parents' monitoring skills (Bank, Patterson, & Reid, 1987).

Over the course of the 1990s, research not only confirmed that poor parental monitoring is linked to adolescent delinquency, but also revealed similar links with broader indices for problem behaviors, including substance use and smoking, deviant and drug-using friends, risky sexual behavior, lower school achievements, and depression (reviews: Dishion & McMahon, 1998; Racz & McMahon, 2011). Importantly, the operationalization of parental monitoring in these more recent survey studies was less optimal, often relying on uni-informant measures of parental knowledge, including questions such as: "How much do your parents really know what you do with your free time?" Collectively, these studies showed that children who reported that their parents were not well aware of their leisure time activities, friendships, and whereabouts were statistically more likely to display all kinds of problems. Given these findings, by the late 1990s it seemed quite clear that parental monitoring was linked to fewer problem behaviors. Although in later years the shortcomings of these studies became more evident, at that point in time it was suggested that parents should be stimulated and instructed to actively monitor their adolescent's daily activities.

The Parental Monitoring Debate

This conclusion was challenged as it became evident that research on parental monitoring had been plagued by a conceptual confusion. In three influential empirical studies (Kerr & Stattin, 2000; Kerr, Stattin, & Trost, 1999; Stattin & Kerr, 2000), it was argued that parental monitoring behaviors should not be operationalized with measures tapping solely or predominantly into parental knowledge. Instead, Stattin and Kerr demonstrated that parental knowledge may come not only from parental active monitoring attempts, but also from a child's disclosure of information to their parents. Instead of only relying on measures for parental knowledge, they split the construct into two. First, they measured parental solicitation – a parenting practice in which parents ask children and children's friends for information. Second, they assessed parental control – a parenting practice that encompasses imposing rules and restrictions on adolescents' behaviors and associates, thereby limiting the amount of freedom children have to do things without telling parents (Stattin & Kerr, 2000). In addition to tapping the parental monitoring behaviors, solicitation and control, Stattin and Kerr measured adolescent disclosure of information. Interestingly, adolescents' disclosure, and not the actual parental monitoring behaviors, was found to be the primary source of parents' knowledge of adolescent whereabouts, friends, and activities.

Perhaps more importantly, adolescent disclosure, and not parental monitoring, was the strongest correlate of adolescent problem behaviors. In other words, children who indicated that they openly disclose information about their leisure activities to their parents, scored lower on problem behaviors compared to children who kept many secrets or were less willing to disclose information. Fewer and weaker linkages were found between the actual parental monitoring practices and adolescent problem behaviors.

Linking this back to the previous conceptualization of parental monitoring as a protective factor against adolescent problem behaviors, conclusions from many earlier studies on parental monitoring seemed to be based on the inferential leap that parental knowledge must have come from parental active efforts to track and control. The work of Stattin and Kerr suggested that this assumption was mostly incorrect. As such, they challenged the idea of parental monitoring as a protective parenting factor, and shifted attention to adolescents' agency in disclosing information to their parents as an important correlate of adolescent problem behaviors.

Bidirectional View

In subsequent years, in line with other developments in the parenting literature (Lollis & Kuczynski, 1997), parental monitoring research progressed toward conceptualizing families as transactional systems. In transactional systems, not only parents do affect their child, but also children's behaviors may change the parenting practices (e.g., Kerr, Stattin, & Pakalniskiene, 2008). Although transactional systems imply more than just reciprocity between active agents in a relationship, it is mainly the reciprocity or bidirectionality of parent–child effects that has influenced the monitoring debate. Due to the cross-sectional nature of Stattin and Kerr's first set of studies (Kerr & Stattin, 2000; Stattin & Kerr, 2000), however, the potential reciprocity of parent and child influences could not be determined. Therefore, answers were missing to questions such as: Does parental monitoring affect later problem behaviors? And, do parents adjust their monitoring when they find out about their child's misbehavior?

Driven by these questions regarding the potential reciprocity of these effects, from 2003 onwards longitudinal research on parental monitoring was conducted (review: Racz & McMahon, 2011). Longitudinal research allows researchers to test questions regarding the temporal ordering of events in human development. In particular, cross-lagged models are often considered quite suitable to determine "what comes first" (Hamaker, Kuiper, & Grasman, 2015) and are therefore quite often applied in longitudinal research on parental monitoring (e.g., Keijsers, Branje, Van der Valk, & Meeus, 2010; Kiesner, Dishion, Poulin, & Pastore, 2009).

Although findings differed somewhat between studies, adolescent disclosure predicted lower problem behaviors over time, but also vice versa, adolescent problem behaviors were predictive of fewer disclosures toward parents (e.g., Keijsers, et al., 2010; Kerr, Stattin, & Burk, 2010). Regarding parental monitoring, findings were less conclusive and at times even contradicting. While some studies did not find longitudinal linkages between monitoring practices and later problem behaviors (e.g., Keijsers, et al., 2010;

Stavrinides, Georgiou, & Demetriou, 2010), others found that parental monitoring predicted higher levels of problem behaviors (e.g., Kerr, et al., 2010; Kiesner, et al., 2009), and yet others found that monitoring predicted lower problem behaviors, at least in some circumstances (Kakihara & Tilton-Weaver, 2009; Laird, Marrero, & Sentse, 2010). Additionally, some studies indicated that adolescent problem behaviors predicted lower levels of parental control over time (e.g., Kerr & Stattin, 2003; Willoughby, & Hamza, 2010).

Conclusion

Recent research highlights the need to consider the linkages of parental monitoring and adolescent disclosure with adolescent problem behaviors as a child-driven process, that is, potentially bidirectional in nature. Whereas adolescent disclosure is consistently found to predict fewer adolescent problem behaviors at the population level, the linkages of parental monitoring behaviors with later problem behaviors may be less straightforward than previously assumed and effects from delinquency to decreases in later monitoring are sometimes reported. Future research is needed to clarify whether, when, and for whom parental monitoring may be an effective deterrent of problems behaviors.

Translating Findings to Practice: A Methodological Concern

Ultimately, it would be very valuable if our research could inform parents about what they should do. In other words: Will adequately monitoring a child's leisure time prevent problem behaviors? Many researchers have attempted in the discussion of their findings to interpret their findings in terms of processes within families, or have translated their findings to a concrete advice. In this second part, I will address a potential problem underlying this interpretation of research findings. More specifically, I will assess the extent to which state-of-the-art longitudinal evidence is informative regarding causal processes within families that link monitoring or adolescent disclosure to adolescent problem behaviors.

Driven by questions regarding bidirectional causality between parents and children, several longitudinal studies are conducted on parental monitoring (review: Racz & McMahon, 2011). The data are often analyzed using state-of-the-art statistical models to test for the existence of a correlation or longitudinal effect between parental monitoring and adolescent disclosure on the one hand, and adolescent problem behaviors on the other hand. With some exceptions (e.g., multilevel models: Kuhn, Phan, & Laird, 2014; Smetana, Villalobos, Rogge, & Tasopoulos-Chan, 2010), the majority of the applied statistical models capture co-variation in rank order positions of individuals, that is, the position of an individual relative to the other participants in the study both at a given time point and over time as a proxy for understanding causal processes (e.g., Keijsers et al., 2010; Kiesner et al., 2009). However, correlations only capture between-person differences (or interindividual differences) and most of the applied longitudinal models do not disaggregate between-person changes from within-person fluctuations over time. Below, I highlight why this may potentially be problematic.

Interactions between children and parents take place in families. Therefore, causal links between parent–child communication and adolescent problem behaviors also take place within families (e.g., the parent–child interaction in family A affects the child's problem behaviors in family A; the parent–child interaction in family B affects the child's problem behaviors in family B). Causal linkages do not operate at the between-person level (e.g., the average parent–child interaction across families A, B, and C does not affect problem behaviors of the children in families A, B, and C).

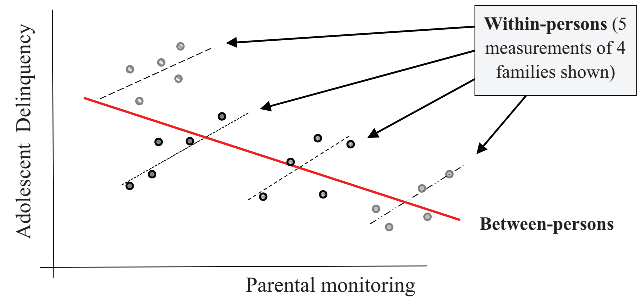

Figure 1. Example of Simpson's Paradox. Studying effects between persons (e.g., assessing many families) versus within persons (e.g., assessing one family multiple times) may yield contradicting conclusions.

In the interpretation of between-person effects, findings can only have practical implications for real parents, under one specific condition: What is found at the between-family level (e.g., by studying covariance at this level), should be a good proxy for the causal processes operating within families that link poor monitoring to adolescent problem behaviors. This assumption may or may not be met in reality.

To illustrate what, in theory, may go wrong when between-person findings are interpreted as within-person causal processes, Figure 1 provides an example of Simpson's paradox (cf. Kievit, Frankenhuis, Waldorp, & Borsboom, 2013). Simpson's paradox is a specific ecological fallacy that may be best known as the problem that causal inferences drawn at the population level may not apply to subgroups (e.g., that correlations are negative when tested for boys and for girls separately, but positive at the aggregate level). However, the paradox may arise with any inference that is drawn across different levels of observation, including from populations to subgroups, subgroups to individuals, but also from populations to intra-individual changes over time (Kievit et al., 2013). In this article, I am focusing on the last example, in which the correlation at the between-person or population level may be reversed to the correlation at the within-person or repeated-measurement level.

In the example in Figure 1, a negative correlation is found at the between-person level: Overall, adolescents who are more often involved in delinquency have parents who are less intensively engaged in monitoring. Yet, within each of the families, a positive link is present: the more parents are involved in monitoring, the higher the adolescent scores on problem behaviors. In this hypothetical situation, studying the data globally at the population level (e.g., using correlations or regression models and focusing on the interindividual or between-person level) versus assessing processes within families (i.e., studying the intra-individual or within-person level) would have resulted in opposing conclusions. Therefore, if practical implications for parents regarding what may happen in their family would be based on the findings of this hypothetical survey study, and on the between-person covariance matrix derived from that, they could potentially be misleading for practice.

There are conceptual reasons to believe that this type of caution is warranted when drawing inferences in studies on parental monitoring, and this has to do with heterogeneity between families and the nature of developmental changes over time. During adolescence, a family can be conceptualized as a dynamic system in flux

(Branje, Keijsers, Van Doorn, & Meeus, 2011; Collins & Steinberg, 2006). For instance, parents gradually have to reduce control and power over the child in order to allow adolescent autonomy and independence to flourish (Branje et al., 2011; Keijsers & Poulin, 2013). Moreover, families differ from each other, also in the extent to which parents can have positive effects on child adaptation. In other words, families are by no means all the same or stable units over time. Under circumstances when individuals differ from each other and processes develop with time, a discrepancy between what is statistically found at the between-person level and processes at the within-person level is highly likely in a dynamic system (Molenaar & Campbell, 2009).

Translating this to our field of study, it may be that processes detected in correlational studies (i.e., examining how families differ from each other) do not represent what happens within families (Molenaar & Campbell, 2009; Voelkle, Brose, Schmiedek, & Lindenberger, 2014). If this is the case, studying how individuals or families differ from each other, or not differentiating within-person from between-person variation, may pose a threat to the accuracy of the inferences we, or our readers, draw regarding causal processes within families that link adolescent disclosure and parental monitoring to adolescent delinquency.

This idea that the main effects at the population level may not apply to an individual family has been recognized by several researchers in developmental psychology, who propose different helpful solutions to better address this issue in research (Bergman, & El-Khouri, 2003; Kuczynski, & Mol, in press). For instance, Bergman and El-Khouri suggested that one way of addressing this issue is to take a person-oriented instead of a variable-centered approach (e.g., studying processes in subtypes of families). A person-oriented approach (e.g., latent class models) would help to unravel heterogeneity in between-person effects. Likewise, some studies on monitoring have found differential processes for subgroup of families, for instance by including interaction terms with latent classes of families (e.g., Keijsers et al., 2009). However, although this approach helps to identify heterogeneity at the between-person level, and addresses the problem associated with drawing inferences from populations to subgroups, it would not unravel the within-person development (Bergman & El-Khouri, 2003, p. 28). In fact, Simpson's paradox (Figure 1) may also apply to each of the latent subgroups: the statistical effects found at the subgroup level may not be representative for the processes that operate within families that belong to the specific subgroup. So, although other authors and I highlight a similar concern that many of the current methodologies do not capture what is going on in real families, our approaches to solve the issue diverge. In this article, I focus on the ecological fallacy associated with interpreting population parameters as within-person or intra-individual effects.

A first step in better understanding whether the population parameters (or between-person differences) are different from the within-person or intra-individual effects is to use a longitudinal research design. Moreover, in analyzing the data, it would require to explicitly model both the processes at the between-person (e.g., the stable differences between persons) and the processes at the within-person level (e.g., how fluctuations in parenting and delinquency are correlated within families). More sophisticated longitudinal statistical models, such as cross-lagged panel models, quite often aggregate both sources of variance, and are therefore are not explicit regarding whether the variance is explained at the between- or within-person level. Therefore, it is unknown whether the findings represent between-person differences or within-person effects.

A recent simulation study was conducted to test whether cross-lagged models adequately estimate parameters when processes at the between-person level and the within-person level are opposing (Hamaker et al., 2015). In a first simulation, Hamaker generated data in which the correlation at the within-person level was positive and the correlation at the between-person level was negative. There were no within-person cross-lagged effects in these generated data, however. Despite this, 90% of the cross-lagged panel models indicated one or more significant cross-lagged effects. In other generated datasets, there were positive correlations and cross-lagged effects at the within-person level, and there was again a negative correlation at the between-person level. These cross-lagged effects, however, did not show up as significant effects in approximately 95% of the cross-lagged panel models, despite the fact that they were present in reality. In fact, in a final simulation, datasets were generated in which the between-person correlation was again negative and one of the positive cross-paths was relatively small. When these data were used for cross-lagged panel models, more than 80% of the models detected a negative cross-path, which was positive in reality. In sum, this study shows that cross-lagged panel models may reveal significant effects when they are not present at the within-person level, fail to detect them when they do exist, or even indicate a negative effect when in reality the within-person effect is positive. In other words, these findings suggest that cross-lagged panel models, in potential, may paint a misleading picture regarding processes that operate within families. For instance, cross-lagged models may indicate positive linkages between parenting and adolescent well-being, when in fact, negative causal linkages are operating within families. Such findings, in turn, may lead to erroneous inferences regarding the underlying causal process.

Conclusion

In sum, ultimately if we want to understand how poor parental monitoring and the emergence of adolescent problem behaviors are causally linked within a family, it would require an understanding of co-variance within families. However, focusing on covariance at the between families level (i.e., studying differences between families) may not be informative regarding this structure within families (i.e., how families fluctuate over time and how these changes are related to problem behaviors). Longitudinal research combined with cross-lagged models is not explicit whether it addresses questions regarding how people or families differ from each other or how fluctuations within a family go hand in hand with fluctuations in problem behaviors. Therefore, at this point, I am questioning whether commonly applied methods, such as cross-lagged panel models, can provide information about the theoretical processes at the within family level. A simulation study suggests that, at best, cross-lagged models may not be 100% informative regarding what is going on in families. At worst, they may yield opposing conclusions about processes in families. Therefore, in attempting to answer the question what parents should do, and how parenting works within a family, it might be problematic if this answer is based on co-variances, even when the estimated effects are obtained with a cross-lagged panel model.

Empirical Test of Methodological Concern

This concern outlined above is thus far hypothetical, and may or may not apply to longitudinal parental monitoring research in practice. Therefore, in this third part, I examined whether cross-lagged associations between parental monitoring/adolescent disclosure and

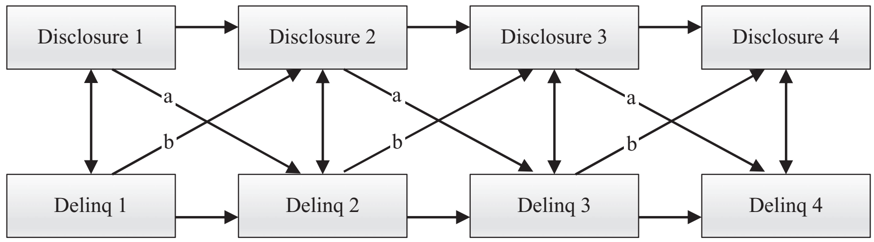

Figure 2. Standard Four-wave Cross-Lagged Panel Model, Linking Adolescent-reported Disclosure with Self-reported Delinquency (Delinq). Labels on cross-lagged effects indicate time constraints.

adolescent delinquency capture processes within families and/or rather reflect stable differences between-persons. I tested this by partitioning both sources of co-variation. If in reality adolescent disclosure and parental monitoring are causally linked to adolescent problem behaviors, I would expect within-family associations like they are previously reported in other studies (review: Racz & McMahon, 2011). That is, I hypothesized that even when stable between person-differences are controlled for, over time within-person fluctuations in adolescent delinquency would be moderately strongly and negatively linked to fluctuations in adolescent disclosure, and weakly negatively related to parental solicitation and control. If indeed, at the within-family level, fluctuations in delinquency are found to be related to fluctuations in parental monitoring and adolescent disclosure, it would strengthen the idea that perhaps these factors are causally linked to delinquency and a slightly more confident translation from research findings to practical advice can be made. However, if within-family effects are not found, it raises additional questions regarding the usefulness of cross-lagged panel models for studying the causal link of parental monitoring/adolescent disclosure and adolescent delinquency.

I have re-analyzed previously published (Keijsers et al., 2009) four-wave adolescent-reported data on 309 Dutch families (age 13 at T1, 149 boys) on adolescent delinquency, adolescent disclosure, parental solicitation, and parental control. Permission was obtained to reanalyze the data. The sample description of the CONAMORE study, measurements (including reliability and information about factor-structures), procedure, and descriptive statistics can be found in this publication. Across variables, on average 99% of the data were present. The maximum percentage of missing values per variable was 3.6%. The missing data pattern was completely at random according to Little's MCAR test ($\chi^2 = 87.095$, $df = 80$, $p = .275$).

Mean scale scores were used. Other studies have shown that the factor-structure of the parental control and solicitation scale (Hawk, Hale, Raaijmakers, & Meeus, 2008) and an adjusted three-item disclosure scale (Frijns, Keijsers, Branje, & Meeus, 2010) are time-invariant in this dataset. Although the reliability and the factor structure of the delinquency scale are adequate (Baerveldt, Van Rossem, & Vermande, 2003), there is no information regarding whether this factor structure would also time-invariant.

For the estimation of the models, I used M*plus* 7.3, and a Full Information Maximum Likelihood estimation (Enders & Bandalos, 2001), combined with a Robust estimator. All input and output files of the models in this manuscript will be provided upon request.

Standard Cross-Lagged Models

First, I specified three "standard" cross-lagged panel models. They were composed of four annual measurements of self-reported delinquency and four annual adolescent reports of either parental solicitation, parental control, or adolescent disclosure. Figure 2 illustrates the model for adolescent disclosure. As in a conventional cross-lagged panel model, I added T1 correlations, one-year stability effects, "correlated changes" (correlated error terms at measurement wave 2, 3, and 4), and cross-lagged effects from monitoring to delinquency and vice versa across one-year intervals. Wald test with four degrees of freedom indicated that the lagged effects were time invariant (adolescent disclosure: Wald = 0.617, $p = .961$; parental solicitation: Wald = 5.809, $p = .214$, parental control: Wald = 2.283, $p = .684$) and these parameters could therefore be constrained over time, reducing the model complexity (Hamaker et al., 2015). The model fits were borderline acceptable to good (disclosure: $\chi2$ (16) = 53.027, CFI = .952, TLI = .919, RMSEA =.087; solicitation: $\chi2$ (16) = 77.920, CFI = .906, TLI = .841, RMSEA =.112, control: $\chi2$ (16) = 45.561, CFI = .957, TLI = .927, RMSEA =.077). The results of this model are presented in Table 1, under the heading *standard cross-lagged panel model*. In the text, I highlight the standardized effects (β) to facilitate the interpretation of Table 1.

The direction and magnitude of effects replicate previous findings on the same dataset using the adolescent-reported data (two waves of data: Keijsers et al., 2010). Also, results are in line with other studies using similar models (e.g., Kerr et al., 2010; Willoughby & Hamza, 2010). A first model shows that adolescents who report higher levels of disclosure relative to the rest of the sample, report lower levels of delinquency. The effect size was modest (T1 correlation: −.338). Moreover, small negative cross-lagged effects are found in both directions between disclosure and delinquency ($\beta = -.068$ to $-.096$), and correlated change is found at two of the three measurement waves ($\beta = -.118$ / $-.214$). In a second model, perceived parental control and self-reported delinquency are negatively correlated at T1 ($\beta = -.107$). Parental control was not predictive of later delinquency, nor vice versa. One small negative correlated change was found between control and delinquency ($\beta = -.138$) (Table 2). Finally, in a third model, adolescent perceptions of parental solicitation was negatively correlated with self-reported minor delinquency at T1, again with a relatively small effect size ($\beta = -.143$) (Table 3). No other significant effects were found in this model.

Random Intercept Cross-Lagged Models

In a second model, I intended to split the variance between between-person stable traits and within-person fluctuations. As an initial test, I examined the extent to which there is sufficient variance at the within-person (i.e., fluctuations of the same person over time) and between-person level (i.e., differences between persons

Table 1. Parameter Estimates Obtained in Cross-Lagged Panel Models and Random Intercept Cross-Lagged Panel Models Linking Adolescent Disclosure and Adolescent Delinquency.

	Standard Cross-Lagged Panel Model					Random Intercept Cross-lagged Panel Model				
Parameters	B	SE	p_B	CI_B	β	B	SE	p_B	CI_B	β
T1 correlation[a]	−.567	.125	.000	−.811 to −.323	−.338	−.201	.099	.042	−.394 to −.008	−.239
Disclosure→ Delinquency[a]	−.595	.164	.000	−.918 to −.273	−.068 to −.075	−.449	.466	.335	−1.363 to .464	−.059 to −.065
Delinquency→ Disclosure[a]	−.012	.004	.004	−.021 to −.004	−.092 to −.096	−.010	.015	.521	−.039 to .020	−.053 to −.063
Correlated change T2[a]	−.248	.075	.001	−.395 to −.101	−.214	−.182	.157	.246	−.490 to .126	−.217
Correlated change T3[a]	−.128	.071	.070	−.266 to .011	−.128	−.107	.075	.158	−.254 to .041	−.124
Correlated change T4[a]	−.152	.064	.018	−.277 to −.026	−.118	−.142	.068	.036	−.275 to −.010	−.122
Between-person correlation						−.439	.141	.002	−.715 to −.163	−.483

Note. Cross-Lagged Panel Model (Figure 2), Random Intercept Cross-lagged Panel Model (Figure 3). CI = 95% Confidence Interval. Standardized effects (β) are indicators of effect size. $N = 309$, measurements = 4.
[a] The substantial interpretation of correlations, cross-lagged effects, and correlated change is different in both models. In a standard cross-lagged panel model, parameters reflect how an individual's relative position compared to the rest of the samples are correlated or can be predicted, which aggregates within- and between-person sources of variation. In a Random Intercept Cross-Lagged Panel Model, parameters reflect how within-person variations relative to their own scores are correlated or can be predicted.

Table 2. Parameter Estimates Obtained in Cross-Lagged Panel Models and Random Intercept Cross-Lagged Panel Models Linking Parental Control and Adolescent Delinquency.

	Standard Cross-Lagged Panel Model					Random Intercept Cross-Lagged Panel Model				
Parameters	B	SE	p_B	CI_B	β	B	SE	p_B	CI_B	β
T1 correlation[a]	−.249	.113	.028	−.471 to −.027	−.107	.132	.163	.416	−.186 to .451	.100
Control→ Delinquency[a]	−.059	.115	.608	−.285 to .167	−.013	.320	.321	.318	−.308 to .948	.066 to .071
Delinquency→ Control[a]	−.012	.006	.058	−.025 to .000	−.046 to −.051	.017	.024	.465	−.029 to .064	.062 to .073
Correlated change T2[a]	−.083	.095	.385	−.270 to .104	−.049	.058	.173	.333	−.282 to .397	.043
Correlated change T3[a]	−.211	.093	.023	−.393 to −.028	−.138	−.102	.100	.309	−.298 to .094	−.076
Correlated change T4[a]	.133	.105	.206	−.073 to .339	.075	.185	.112	.098	−.034 to .403	.110
Between-person correlation						−.433	.161	.007	−.749 to −.117	−.404

Note. Cross-lagged panel model (Figure 2), Random Intercept Cross-Lagged Panel Model (Figure 3). CI = 95% Confidence Interval. Standardized effects (β) are indicators of effect size. $N = 309$, measurements = 4.
[a] The substantial interpretation of correlations, cross-lagged effects and correlated change is different in both models. In a standard cross-lagged panel model, parameters reflect how an individual's relative position compared to the rest of the samples are correlated or can be predicted, which aggregates within- and between-person sources of variation. In a Random Intercept Cross-Lagged Panel Model, parameters reflect how within-person variations relative to their own scores are correlated or can be predicted.

Table 3. Parameter Estimates Obtained in Cross-Lagged Panel Models and Random Intercept Cross-Lagged Panel Models Linking parental Solicitation and Adolescent Delinquency.

	Standard Cross-Lagged Panel Model					Random Intercept Cross-Lagged Panel Model				
Parameters	B	SE	p_B	CI_B	β	B	SE	p_B	CI_B	β
T1 correlation[a]	−.281	.098	.004	−.472 to −.089	−.143	−.085	.088	.337	−.285 to .088	−.075
Solicitation→ Delinquency[a]	−.140	.133	.291	−.401 to .120	−.022 to −.026	.300	.271	.268	−.231 to .831	.042 to .057
Delinquency→ Solicitation[a]	−.007	.005	.104	−.017 to .002	−.042 to −.045	.008	.013	.545	−.018 to .034	.046 to .050
Correlated change T2[a]	−.015	.079	.848	−.169 to .139	−.012	.103	.092	.260	−.076 to .283	−.119
Correlated change T3[a]	−.140	.080	.079	−.297 to .016	−.115	−.062	.079	.429	−.216 to .092	−.065
Correlated change T4[a]	−.059	.080	.459	−.215 to .097	−.042	.020	.071	.775	−.119 to .160	.017
Between-person correlation						−.232	.096	.016	−.420 to −.044	−.260

Note. Cross-lagged panel model (Figure 2), Random Intercept Cross-Lagged Panel Model (Figure 3). CI = 95% Confidence Interval. Standardized effects (β) are indicators of effect size. $N = 309$, measurements = 4.
[a] The substantial interpretation of correlations, cross-lagged effects and correlated change is different in both models. In a standard cross-lagged panel model, parameters reflect how an individual's relative position compared to the rest of the samples are correlated or can be predicted, which aggregates within- and between-person sources of variation. In a Random Intercept Cross-Lagged Panel Model, parameters reflect how within-person variations relative to their own scores are correlated or can be predicted.

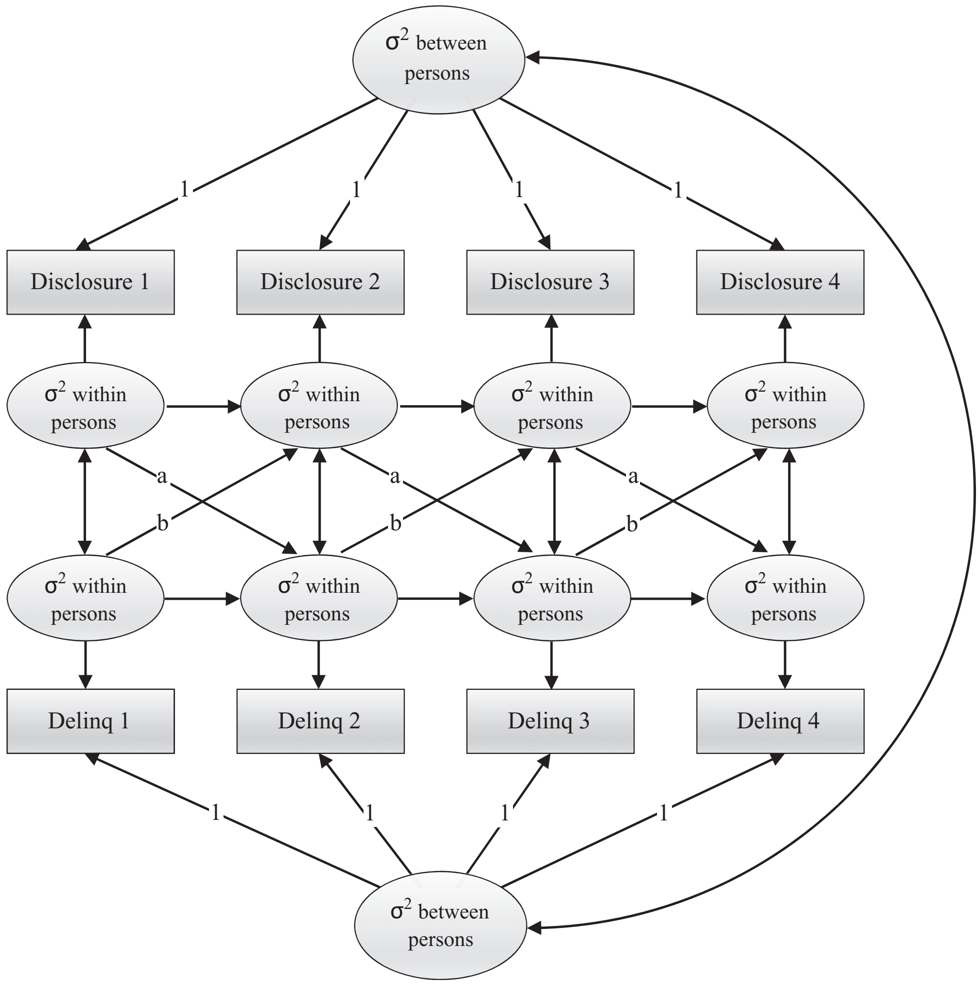

Figure 3. Four-wave Random Intercept Cross-Lagged Panel Model (Hamaker et al., 2015), Linking Adolescent-reported Disclosure with Self-reported Delinquency (Delinq), Differentiating Within- and Person-person Variance.

in stable traits). Therefore, I calculated the intra-class correlations (ICC; as is common in multi-level modeling). For adolescent self-reported disclosure, the ICC was .543. This indicates that 54.3% of the variance in the four measures of disclosure is explained by differences between adolescents, and the remainder (45.7%) is explained by fluctuations within a person. Likewise, the ICCs for perceived parental solicitation and control were .466 and .447, respectively. For adolescent self-reported delinquency, the ICC was .617. Hence, for each of the variables in this study, a substantial part of the variance is due to stable differences between adolescents (ranging between 44.7% and 61.7%). However, this also means that 38.3% to 55.3% of the variance is due to fluctuations over time in adolescent-reports of disclosure, parental monitoring, and minor delinquency. In the next cross-lagged model, I disentangled these two types of variance.

I extended the cross-lagged models to test whether the statistical effects reflect within-person linkages or time-invariant trait-like differences between persons (see Figure 3). Following the procedures describes by Hamaker and colleagues (2015), I regressed each observed score on its own latent factor (each loading constrained at 1). This resulted in eight latent factors. The parameters that would normally be included in a cross-lagged panel model, that is, stability paths, cross-lagged effects, T1 correlation, and correlated changes, were specified between these latent constructs rather than between the observed scores. Subsequently, I added two overarching random intercept factors to capture the trait-like differences between persons in both variables (similar to a random intercept in a growth curve model). The observed scores were the indicators of these factors, and all factor loadings were constrained at 1. Finally, the variances of the observed scores were constrained to be zero, therefore all variation in the observed measures was completely captured by within-person and between-person latent factor structure. The mean structure of the observed variables was unconstrained in this model because of known developmental changes in these constructs (e.g., Keijsers et al., 2009; Keijsers & Poulin, 2013). The inclusion of this random intercept improved the model fit compared to the standard cross-lagged panel model, suggesting it provides a better representation of the data (disclosure: $\chi 2$ (13) = 30.908, CFI = .978, TLI = .952, RMSEA = .067; solicitation: $\chi 2$ (13) = 25.909, CFI = .981, TLI = .958, RMSEA =.057, control: $\chi 2$ (13) = 31.987, CFI = . 972, TLI = .941, RMSEA = .069).

The specific feature of this Random Intercept Cross-lagged Panel Model (RI-CLPM; Hamaker et al., 2015) is that it partitions the variance of the observed scores into variance between persons (i.e., stable time invariant traits) and variance within persons (i.e., fluctuations over time). Therefore, the substantial interpretation of the stability, correlations, correlated change, and cross-lagged effects changes compared the first series of "standard" cross-lagged models where both sources of variance are not differentiated. Each individual has an expected score based on the sample mean levels across time and the individual stable trait factor. The variance at the within-person level captures fluctuations over time, or more technically described, deviations of individuals from their own expected scores.

Regarding the specific parameters that link the two measures at the within-person level, much like in a multi-level model, a T1 correlation reflects whether individuals' deviations from their own expected scores in one variable are linked to deviations from their own scores in another variable. The stability paths in the structural part of the model can be interpreted as the extent to which within-person deviations in the level of disclosure or delinquency can be predicted by the individual's prior deviation from the own scores. The cross-lagged parameters reflect whether within-person change in one variable is predicted by deviation from the own expected scores on the other variable assessed one year earlier. The "correlated change" reflects whether within-person changes in one variable are linked to within-person changes in another variable. Hence, all of these parameters reflect intra-individual processes, and capture how fluctuations over time of a person in one variable are linked to fluctuations over time of the same person in another variable. In contrast, the correlation between the overarching latent factors reflects how individuals differ from each other, or, in other words, how stable between-person differences in one construct are linked to stable between-person differences in another construct.

In a first Random Intercept Cross-lagged Panel Model, the link of adolescent-reported disclosure with self-reported delinquency was examined. At the between-person level, there was a strong correlation between stable traits of disclosure and delinquency ($\beta = -.483$). This indicates that adolescents who reported higher levels of disclosure across the four measurement waves reported less delinquency across the four years. After controlling for these traits, some evidence was found for small to modest within-person associations. There was a correlation at the first measurement wave ($\beta = -.239$) and a significant correlated change at T4 ($\beta = -.122$). There were no cross-paths at the within-person level. Hence, for adolescent disclosure, in addition to between-person correlations, within-person deviations in disclosure were negatively linked to within-person deviations in delinquency (but no cross-lagged effects were found). Comparing the standardized between-person association with the T1 within-person associations in this model indicates that the between-person correlation is approximately two times stronger.

Second, a model was estimated for adolescent perceptions of parental control. A strong negative between-person correlation in how much control and delinquency adolescents report was found ($\beta = -.404$). Hence, at the between-person level, adolescents who reported lower levels of delinquency, also reported lower levels of parental control. However, at the within-person level no evidence was found for a within-family process. T1 associations, correlated change, and cross-paths were not significant. The standardized between-person correlation was four times stronger than the T1 within-person correlation.

Regarding parental solicitation, the only significant parameter in this Random Intercept Cross-lagged Panel Model was a moderately strong correlation at the between-person level ($\beta = -.260$). In other words, adolescents who reported more delinquency than their peers, report lower levels of parental solicitation than their peers. No associations are found at the within-person level. The standardized between-person correlation was 3.5 times stronger than the T1 within-person correlation.

Limitations and Conclusion

Random Intercept Cross-Lagged Models, in which covariance was partitioned into the between-person and within-person level, provide support for within-person linkages of adolescent disclosure with delinquency. Earlier studies have described this link at the between-person level (e.g., correlations) or did not differentiate the stable differences between persons from the processes within persons (e.g., using cross-lagged models). A simulation study (Hamaker et al., 2015) indicates that estimates from standard cross-lagged models may have a limited validity when it comes to understanding the within-person effects. As such, this specific finding provides stronger evidence for the idea that delinquency and disclosure might be causally linked, compared to earlier studies. In contrast, the links of parental control and solicitation with delinquency were only present at the between-person level. These monitoring practices were not correlated to delinquency within families. Findings in standard cross-lagged models on parental monitoring, where variance between-persons and within-persons is not split, may therefore have mainly illustrated stable between-person differences in monitoring and in delinquency, rather than the actual within-family processes occurring in real families.

One important limitation of this study lies in the use of adolescent perceptions of the process within families and their own norm-breaking. Although it may be a reasonable reliable and valid source of information regarding their delinquent activities (Jolliffe et al., 2003), family relationship may strongly depend on the eye of the beholder and this similarly applies to parental monitoring and adolescent disclosure (e.g., De Los Reyes, Goodman, Kliewer, & Reid-Quinones; Laird & Weems, 2011). Additionally Random Intercept Cross-Lagged Panel Models are not suitable for disentangling heterogeneity in processes at the within-family level. The within-family effects can be conceptualized as the average within-person effects across the sample (not to be confused with the between-person effects, see Figure 1).

Despite the limitations, findings confirm that adolescent disclosure is linked to self-reported delinquency at the within-person level. However, findings also suggest that standard cross-lagged models on parental monitoring provide results that cannot be replicated at the within-person level. Therefore, if our research is aimed at understanding dynamics of people rather than populations, it may be crucial to examine associations at the level where causality takes place: the within-person level, in order to prevent false positive results. Improving models to distinguish within family processes from stable traits may provide a first step on a possible route, toward a better understanding of whether, how, and for whom parental monitoring and adolescent disclosure are causally linked to adolescent delinquency.

Conclusion and Future Directions

Over the last 60 years, big leaps were made in our understanding of how parental monitoring may prevent adolescent delinquency. Stattin and Kerr raised awareness that children in adolescence are active agents in managing information from parents. Also, they indicated that it is mainly the adolescent's willingness to disclose that is an important correlate of adolescent problem behaviors. We now also acknowledge the bidirectional nature of these processes: it is increasingly acknowledged that adolescent disclosure and parental monitoring may not only affect later problem behaviors, but also themselves be influenced by the existence of these problems. It seems that the amount of adolescent disclosure is of primary importance when distinguishing delinquent from non-delinquent youth. However, much also remains unknown about the causal processes that link parent–child communication to adolescent delinquency and substance use.

At this point, it is quite uncertain whether we can confidently draw causal inferences regarding the link of parenting and adolescent problem behaviors from research findings. Although causality takes place at the within-family level, longitudinal research designs are often not explicit regarding whether differences between persons or fluctuations over time within persons are explained. Focusing on one specific common type of research, longitudinal cross-lagged models, I have argued and empirically demonstrated that examining how adolescents differ from each other may yield a different pattern of results compared to studying the same questions at the within-person level. The correlation of parental solicitation and control with delinquency that is often detected correlationally (e.g., Stattin & Kerr, 2000) or longitudinally (e.g., Willoughby & Hamza, 2010) seems to reflect differences between-persons, rather than linkages within-persons. Theoretically these findings provide additional support for the notion of Stattin and Kerr (2000) that there are few or no linkages between actual parental monitoring practices and adolescent delinquency. Only the correlation between disclosure and lower levels of delinquency was found both at the between-person and at the within-person level. Because these findings show that the link of disclosure and delinquency cannot be completely explained by differences between persons, it supports the hypothesis that fluctuations in disclosure are linked to lower levels of delinquency within real families.

Finally, I would like to highlight some challenges ahead, perhaps not only for the monitoring literature, but also for parenting research in a broader context and potentially other fields of study that use cross-lagged models. Many research questions are aimed at understanding causal linkages, for instance between parenting and child well-being or adaptation. These causal processes take place within persons or within families. In future research, I think it is important to be more explicit about the exact nature of the phenomena that are studied (i.e., whether between-person differences or within-person processes are studied), and to address the limitations of our findings in terms of generalizability to the within-person level more explicitly. To indicate the extent to which findings may or may not describe within-family processes may avoid that readers will draw incorrect causal inferences from research findings.

In order to make a better distinction, a first methodological challenge would be to differentiate the covariance at the within-person level from the covariance at the between-person level (e.g., by using multi-level models: Kuhn et al., 2014; Smetana et al., 2010) in order to better understand the causal linkages within families. This may be of importance, because there seems to be meaningful variation at both levels. For instance, in this dataset, approximately half of the variance in the monitoring scales was due to stable differences between persons, and approximately half due to fluctuations of individuals over time. Extending models to distinguish within-family variance from stable traits, as in the models presented in this article (Hamaker et al., 2015), may be one step toward being more explicit, and it is relatively easily to implement (syntax files will be provided upon request). Focusing not only on how families differ from each other, but also on how adolescents and parents fluctuate across time may offer a first glimpse into the within-family processes.

A second methodological improvement for future research would be to be more explicit regarding the role of time. This review suggests that Random Intercept Cross-Lagged Panel Models with one-year intervals are less optimal in detecting over time causal effects that operate within families, even when controlling for stable between-person differences. In fact, in contrast to the findings in standard cross-lagged models, no cross-lagged effects were found at the within-person level in a Random Intercept Cross-Lagged Panel Model. This may not be quite surprising, given that causality tends to operate at much smaller time intervals (Branje et al., 2011; Granic & Patterson, 2006), but it does indicate the time frame in which causality takes place and how causality may develop with time is yet to be explored (e.g., Voelkle, Oud, Davidov, & Schmidt, 2012).

Combining the two methodological challenges by capturing the micro-dynamics operating within families (see, Voelkle et al., 2014) could be an interesting next step toward better understanding how causal processes evolve in the everyday lives of teenagers and parents, and how this may ultimately explain why adolescents differ from each other in their level of delinquency. This understanding would be crucial to more confidently translate our findings into a practical advice.

It may be that extending our research paradigm will indicate that the same theoretical concepts known to explain why adolescent differ from each other also explain behavior of individuals. This would be reassuring, because it would mean we can be slightly more confident in providing practical advice to parents based on previous empirical research. However, the findings presented here suggest that perhaps cross-lagged models on monitoring have predominantly captured differences between persons rather than processes within families. Therefore, there may also a possibility that such a new research paradigm may alter our conclusions regarding the effectiveness of parental monitoring and adolescent disclosure in preventing adolescent problem behaviors. Possibly, it will also illustrate that we have to search in another direction, and seek for other parenting practices that help to prevent adolescent problem behaviors.

Author Note

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