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INTIMATE PARTNER VIOLENCE RISK AMONG VICTIMS OF YOUTH VIOLENCE: ARE EARLY UNIONS BAD, BENEFICIAL, OR BENIGN?*

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Abstract

Youth violent victimization (YVV) is a risk factor for precocious exits from adolescence via early coresidential union formation. It remains unclear, however, whether these early unions 1) are associated with intimate partner violence (IPV) victimization, 2) interrupt victim continuity or victim–offender overlap through protective and prosocial bonds, or 3) are inconsequential. By using data from the National Longitudinal Study of Adolescent to Adult Health (N = 11,928; 18–34 years of age), we examine competing hypotheses for the effect of early union timing among victims of youth violence (n = 2,479)—differentiating across victimization only, perpetration only, and mutually combative relationships and considering variation by gender. The results from multinomial logistic regression models indicate that YVV increases the risk of IPV victimization in first unions, regardless of union timing; the null effect of timing indicates that delaying union formation would not reduce youth victims' increased risk of continued victimization. Gender-stratified analyses reveal that earlier unions can protect women against IPV perpetration, but this is partly the result of an increased risk of IPV victimization. The findings suggest that YVV has significant transformative consequences, leading to subsequent victimization by coresidential partners, and this association might be exacerbated among female victims who form early unions. We conclude by discussing directions for future research.

Keywords

youth violent victimization; intimate partner violence; union timing

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Violent victimization is concentrated disproportionately among youth (Snyder and Sickmund, 2006); for example, youth 12–17 years of age experience twice the risk of being a victim of robbery or aggravated assault and three times the risk of being a victim of simple assault compared with adults (Truman, Langton, and Planty, 2013). Youth violent victimization (YVV) increases the risk of subsequent victimization (Lauritsen and Davis Quinet, 1995) and is associated with role transitions that mark a precocious exit from adolescence and premature entry into adulthood (Hagan and Foster, 2001; Haynie et al., 2009). Such precocious role exits have largely been considered hallmarks of continued disadvantage (e.g., high-school dropout, teen pregnancy, and running away); yet a recent study by Kuhl, Warner, and Wilczak (2012) found that YVV also leads to early intimate union formation. Whether such early unions are indicative of continued disadvantage remains unclear. Although some studies have shown that marriage is a prosocial transition (Sampson and Laub, 1990), other studies have shown that *early* union formation is linked to disadvantages such as unemployment, financial difficulties, relationship conflict, and divorce (Booth and Edwards, 1985; DeMaris et al., 2003).

Given life-course continuity in victimization and the risk of early union formation, which sets up further disadvantages, the need to understand whether early union formation among victims of youth violence represents a context perpetuating subsequent violence or a positive turning point in the lives of victims is critical. To address this need, we use data from the National Longitudinal Study of Adolescent to Adult Health (Add Health) to examine the effect of YVV on intimate partner violence (IPV) in coresidential first unions with attention to union timing.

We motivate the focus of our article as follows. First, we discuss the life-course consequences of YVV with respect to precocious exits from adolescence, focusing on early entry into coresidential unions. Second, we derive three competing hypotheses for the consequences of early union formation among victims of YVV—that is, early coresidential union formation could increase, decrease, or be inconsequential for the risk of subsequent IPV in early adult unions. Third, given gender differences in YVV (Snyder and Sickmund, 2006) and the ongoing debate over gender differences in the context and prevalence of IPV (Archer, 2000; Johnson and Ferraro, 2000), we consider whether gender differences exist in the effect of YVV and coresidential union timing on subsequent IPV risk.

BACKGROUND

Youth violent victimization is a potentially developmentally disruptive force during adolescence (Hagan and Foster, 2001). Prior research has identified YVV as a risk factor for suicidal thoughts and actions (Cleary, 2000), depressive symptoms (Latzman and Swisher, 2005), anger and aggression (Turner, Finkelhor, and Ormrod, 2006), and substance abuse (DeMaris and Kaukinen, 2005). Victims of violence are also at risk of experiencing subsequent victimization (Lauritsen and Davis Quinet, 1995; Schreck, Stewart, and Osgood, 2008). Furthermore, victims are at risk of becoming offenders (Lauritsen, Sampson, and Laub, 1991; Menard, 2002), a link that has been established in research on victim–offender overlap (Schreck, Stewart, and Osgood, 2008), which has highlighted the role of certain

lifestyle risks (Wittebrood and Nieuwbeerta, 2000) in maintaining patterns of violence over time.

In addition to undermining psychological health and perpetuating subsequent victimization and offending, YVV disrupts the age-graded, normative timing of the transition to adulthood. Youth victimization is associated with high-school dropout, teen pregnancy, running away from home, and having contact with the criminal justice system (Hagan and Foster, 2001; Haynie et al., 2009). Recognizing the possible negative consequences of YVV for the developmental experiences that characterize adolescence, Macmillan (2001) urged scholars to explore the effect of youth victimization on a key task of adolescence and young adulthood: the formation of intimate relationships (see also Meier and Allen, 2009). However, despite the links between YVV and other precocious exits, few scholars have assessed whether (and how) YVV might influence the formation and quality of young adult romantic or intimate unions. One exception is a study by Kuhl, Warner, and Wilczak (2012), which found that victims of youth violence experienced subsequently higher rates of first coresidential union formation, especially marriage, in the transition to adulthood. That is, victims entered into coresidential unions earlier than their nonvictimized peers; however, the consequences of this earlier union timing among victims of youth violence remain unclear.

Life-course criminology has primarily characterized coresidential union formation—principally marriage—as a *prosocial* role transition related to desistance (e.g., Sampson and Laub, 1990): the “good marriage effect” (but see Giordano, Cernkovich, and Rudolph, 2002). Yet despite this beneficial effect for marriage, at least with respect to criminal desistance, several limitations remain. Notably, research has not yet explored the effect of coresidential union formation on other outcomes of criminological interest (e.g., victimization) or thoroughly assessed the role of union formation outside its effect among offenders. Of particular relevance for advancing scholarship on “precocious exits,” considerations of timing are altogether absent from this research—despite an established link between union timing and a host of disadvantages. Prior research, particularly in family sociology, has shown that early coresidential union formation is associated with a wide range of negative outcomes (see Amato and Booth, 1997; Wickrama, Wickrama, and Baltimore, 2010), and such unions could be characterized as “early or precocious exits.”

Given the unique contribution of YVV to the timing of coresidential unions, as evidenced by Kuhl, Warner, and Wilczak (2012), the logical follow-up question is whether these unions are *good* or *bad*. Does YVV also shape the “character and content” (Macmillan, 2001) of these precocious unions, as one might anticipate? An important indicator of such character and content likely relevant for victims of youth violence—given patterns of both victim continuity and victim–offender overlap—is IPV. Because 1) victims of youth violence are at an increased risk of continued exposure to violence (as victims or offenders), 2) victims of youth violence are entering into coresidential unions early, and 3) early coresidential unions often are characterized by stressors that promote violence (DeMaris et al., 2003; Fox et al., 2002), early unions formed by victims of youth violence could be a context for continued exposure to violence.

VIOLENT VICTIMIZATION: CONTINUITY, OVERLAP, AND LIFE-COURSE CONSEQUENCES

Victimization experiences perpetuate violence across time and relationships in a social learning process often called the “cycle of violence” (Doumas, Margolin, and John, 1994; Widom, 1989). The cycle of violence is most often used to describe the fact that childhood and adolescent *family* violence is associated with subsequent adult perpetration of familial violence, including parent–child violence and IPV (Simons et al., 1995; White and Widom, 2003; Whitfield et al., 2003). Moreover, IPV victimization and perpetration in past relationships are associated with victimization and perpetration in subsequent relationships (Coker et al., 2000; Whitaker, Le, and Niolon, 2010). Recent work (e.g., Cui et al., 2013) has found evidence of victim–offender overlap in intimate relationships, such that victims of partner violence are at risk for continued victimization by an intimate (continuity) *and* perpetration against an intimate (overlap).

Because of the focus within the cycle of violence literature on the family context, the expansive body of research on IPV (in adolescence and young adulthood) has been fairly disconnected from broader scholarship on YVV. This gap is noteworthy considering that most youth victimization occurs *outside* the home (Finkelhor et al., 2009; Lauritsen, 2003). Although it has been attributed to several theories (Schreck, Stewart, and Osgood, 2008), most relevant to our discussion of nonfamilial violence is that the consistent finding that violence begets violence—in terms of both victim continuity and victim–offender overlap—has drawn on the population heterogeneity versus state dependence debate (Lauritsen, 2003; Nagin and Paternoster, 2000; Ousey, Wilcox, and Brummel, 2008).

Although originally addressing continuity in offending, Ousey, Wilcox, and Brummel (2008) extended the population heterogeneity versus state dependence debate to continuity in victimization. According to a population heterogeneity argument, victimization continuity over time is attributable to stable individual characteristics that increase risk. For instance, impulsivity or other personality characteristics make youth vulnerable to early victimization and to subsequent victimization in adulthood. Alternatively, the state dependence argument posits that prior victimization leads to psychological or behavioral transformations that reduce or increase subsequent risk. Prior victims might avoid high-risk situations to guard against repeat victimization, or their earlier victimization experience could result in victim labeling, which increases their visibility to subsequent offenders and results in subsequent victimization (Wittebrood and Nieuwbeerta, 2000). The state dependence argument is closely related to scholarship on victim–offender overlap: Those who engage in offending behavior are at subsequent risk of victimization in part because they are surrounded by other offenders. Yet despite a strong overlap between victims and offenders, Shreck, Stewart, and Osgood (2008) found numerous examples in which one role (e.g., victim) predominated over the other.

Focusing on IPV victimization in particular,Halpern et al. (2009) found evidence to support both population heterogeneity (e.g., family structure) and state dependence (e.g., childhood sexual abuse) explanations for victimization continuity. These findings suggested that experiencing victimization within a familial context where violence is more normative might increase ones’ risk of persistent IPV victimization. Although studies have long examined the

links between childhood family violence and later IPV, more recent work has given attention to whether youth who experience victimization *outside* the family are also at subsequent risk for IPV in adulthood—that is, victim continuity and overlap across contexts (Malik, Sorenson, and Aneshensel, 1997; Ousey, Wilcox, and Brummel, 2008). Although patterns differ across samples, a few recent studies showed that YVV is associated with increased IPV victimization and perpetration (Fang and Corso, 2007; Menard, 2002; Murphy, 2011; Renner and Whitney, 2012).

Although this recent work on YVV as a risk factor for IPV has extended the idea of continuity and overlap across contexts, these studies have remained disconnected from the "precocious exits" literature and, thus, have neglected the role of the timing. According to the life-course perspective, the timing of an event could be more consequential than its occurrence (Elder, Johnson, and Crosnoe, 2003). As such, issues of timing are vital for exploring the consequences of both YVV and coresidential union formation. Because the timing of transitions is an important aspect of the precocious exits literature, it seems remiss that research has not examined the association between YVV and the "character and content" of future unions with specific attention to the timing of those unions.

EARLY UNION TIMING AMONG VICTIMS OF YOUTH VIOLENCE: BAD, BENEFICIAL, OR BENIGN?

Scholarship in criminology has documented the "good marriage" effect for desistance, but an important qualification is that "any" marriage is not always better—timing in the life course matters. Victims of youth violence might be propelled into precocious coresidential unions (Kuhl, Warner, and Wilczak, 2012), but we do not yet know whether these unions mitigate the risks of subsequent victimization or whether they are a context for further victimization and/or perpetration. Next, we discuss three competing expectations for the ways in which precocious coresidential union formation could condition the effect of YVV on IPV.

Early Unions Are Bad—Ample evidence shows the negative consequences of early marriages: They are less stable than later marriages, of lower quality, and at a greater risk for numerous stressors such as early pregnancy, unemployment or economic instability, financial difficulties, and role conflict (e.g., Amato, 1996; Amato et al., 2007; Booth and Edwards, 1985). Getting married at a young age could expose individuals to stressors they are not yet mature enough to handle because "premature engagement in adult activities and responsibilities during adolescence interferes with the acquisition of psychosocial skills necessary for success in these adult roles" (Newcomb, 1996: 478).

The absence of emotional maturity could be particularly problematic among victims of youth violence given the link between exposure to violence and psychoemotional maladjustment (O'Donnell, Schwab-Stone, and Ruchkin, 2006). Early victimization experiences lead persons to "become vigilant for threat and mistrusting of others" and to be "more likely to elicit conflict and rejection and less likely to garner warmth and support" (Miller, Chen, and Parker, 2011: 965). Indeed, YVV engenders an aggressive social-cognitive interactional style that makes victims more likely to interpret ambiguous

interpersonal situations as threatening and hostile and, thus, to respond in a similar manner (O'Donnell, Schwab-Stone, and Ruchkin, 2006). The behavioral and psychosocial consequences of YVV, coupled with the adverse outcomes of precocious coresidential unions, might thus place youth victims in a state of "double jeopardy" in terms of their risk for IPV. Prior work has not examined thoroughly the effect of coresidential union timing on relationship aggression (Craig and Foster, 2013; Sampson and Laub, 1990) even though the stressors inherent in early unions could promote such behavior (DeMaris et al., 2003; Flake, 2005).

Early Unions Are Beneficial—Although most research on early coresidential unions has highlighted a host of negative consequences, a smaller body of work has illustrated that early unions—particularly marriages—do not necessarily lead to negative outcomes for everyone, especially individuals from disadvantaged backgrounds. Early coresidential unions can be beneficial if they offset the otherwise negative consequences of prior disadvantages or offer positive change. For example, Booth, Rustenbach, and McHale (2008) found that respondents who made early family transitions were rarely different in terms of depressive symptoms than those not making early transitions despite coming from disadvantaged families. They concluded that for some people, such early relationship transition decisions can be "rational and sound" (Booth, Rustenbach, and McHale, 2008: 12). This finding is consistent with other work on the role of parenthood among disadvantaged populations (Edin and Kefalas, 2011) and recent evidence of the "good marriage effect" for desistance among a sample of early marrying juvenile delinquents (Bosick, 2012).

The formation of a coresidential union could be an attractive means of coping with the challenges to the self-concept brought on by early traumas particularly because victims who perceive high levels of emotional support report lower levels of depression and anxiety than victims who have low levels of support (Ruback and Thompson, 2001). Coresidential partners, especially marital partners, are culturally expected to be one's greatest source of intimacy and social support (Waite and Gallagher, 2000). The formation of a coresidential union might be important for restoring trust in others as violent victimization erodes one's sense of trust, undermining victims' belief in the world as meaningful, benign, trustworthy, and predictable (Ruback and Thompson, 2001). Therefore, early coresidential unions could represent a positive life-course transition for victims of youth violence, a turning point that could actually curb the negative effect of YVV on subsequent IPV.

Early Unions Are Benign—Finally, it is possible that coresidential union timing could have no effect on IPV risk among victims of violence—neither exacerbating nor ameliorating the tendency toward victim continuity or victim–offender overlap. That is, consistent with the state dependence perspective described previously (Nagin and Paternoster, 2000), YVV might have such a profound, transformative, long-term effect on victims' life circumstances that it structures victimization (or perpetration) in coresidential unions regardless of when those unions are formed. In this case, the consequences of YVV are so prominent and far reaching, so disruptive to ones' developmental trajectory, that the timing of coresidential union formation is irrelevant.

Gendered Context of IPV—Our consideration of the life-course consequences of YVV also must consider the gendered context in which union timing and IPV experiences occur. Although the violent victimization rate among adolescent males is twice that among adolescent females (Snyder and Sickmund, 2006), gender differences in the prevalence of IPV are less clear. Indeed, there is a longstanding debate over gender differences in the prevalence of IPV victimization and perpetration; data from community-based samples often show gender symmetry, whereas hospital- or shelter-based data show higher rates of IPV perpetration among men and more victimization among women (e.g., see Archer, 2000; Johnson and Ferraro, 2000; Langhinrichsen-Rohling, 2010).

Nevertheless, if union formation is a means of coping with the trauma of violent victimization (as noted previously), then it is plausible that the interaction between union timing and YVV would be gendered particularly given females' lower risk of violent victimization but earlier entry into coresidential unions in general (Manning, Brown, and Payne, 2014). Gender differences in the experience and expression of negative emotionality (Hagan and Foster, 2003), for example, would lead us to expect higher risks of perpetration among male victims of violence and higher risks of victimization among female victims of violence. Yet, a study by Fang and Corso (Fang and Corso, 2007) found that child physical abuse significantly increases the risk of IPV perpetration for females but not for males—perhaps because female victims of violence become hypersensitive to potential threats given their prior experiences. In either case, we would expect such differences to be even larger in early unions because of a lack of emotional maturity (Miller, Chen, and Parker, 2011; O'Donnell, Schwab-Stone, and Ruchkin, 2006). Thus, although it is unclear what form of IPV should be more prevalent, it does seem that female victims of youth violence could be especially at risk for IPV. As such, not only do we explore the ways in which union timing could condition the effect of YVV on IPV, but also whether there are variations in these associations by gender.

CURRENT STUDY

As discussed, scholarship on nonfamilial YVV and young adult IPV remains fairly disparate, notwithstanding a large (and growing) body of research on violent victimization—including victim continuity and victim–offender overlap—more broadly. Recent work (Kuhl, Warner, and Wilczak, 2012) has established that YVV is a precursor to early or precocious coresidential union formation, and extensive family sociology scholarship has documented that early unions, in general, are associated with numerous negative life-course outcomes. Despite the persistent effects of early life-course victimization on later violence and victimization, it remains unknown whether (or how) precocious union timing will alter this continuity (and/or overlap). Thus, in an effort to connect research on partner violence and on the consequences of nonfamilial youth victimization with family research on early unions, the current study tests three competing hypotheses for the effect of precocious union formation on IPV in first coresidential unions among victims of youth violence:

> *Hypothesis 1*: Early unions are bad—Precocious coresidential union formation exacerbates the effect of YVV on IPV in first coresidential unions.

Hypothesis 2: Early unions are beneficial—Precocious union formation protects against the negative consequences of YVV for IPV risk in first coresidential unions.

Hypothesis 3: Early unions are benign—Precocious union formation is inconsequential for the effect of YVV on IPV in first coresidential unions.

Consistent with past research in criminology and family sociology, we expect the main effects of both YVV and early union formation to be positively and significantly associated with IPV. However, support for (or against) the three proposed hypotheses will be evidenced by the significance and direction of the interaction between YVV and union timing. A statistically significant and positive coefficient would provide support for hypothesis 1, whereas a significant and negative coefficient would provide support for hypothesis 2; a statistically nonsignificant effect would provide support for hypothesis 3.

As the focus of the current analysis is to assess the hypothesized effect of early union timing on IPV among victims of youth violence, we are mindful of the fact that both IPV and YVV are significantly gendered processes that are likely to place females at particular risk of victimization given that they form unions at earlier ages in general. Accordingly, we test a fourth hypothesis:

Hypothesis 4: The effect of union timing on IPV differs by gender—that is, any effect of the interaction between YVV and union timing on IPV (early unions as bad, beneficial, or benign) will be more pronounced among females.

Our examination of the effect of YVV on IPV in first coresidential unions advances prior research in four key ways. First, we incorporate the role of union timing, which could have implications (as noted previously) for the risk of IPV among victims of youth violence. Second, we focus on first coresidential unions as a potentially precocious exit from adolescence—most research on IPV uses data from samples of current couples (Hardie and Lucas, 2010; Murphy, 2011) or collapses across all relationships (Cui et al., 2013; Fang and Corso, 2007; Halpern et al., 2009; Renner and Whitney, 2012). Third, we assess the possibility of victim continuity *and* overlap by distinguishing among IPV perpetration, IPV victimization, and mutually combative IPV. Fourth, we add to the ongoing debate over gender differences in the context and prevalence of IPV (Archer, 2000; Johnson and Ferraro, 2000) by gender stratifying our test of the competing hypotheses for the effect of union timing on IPV. As we show next, doing so reveals important, yet nuanced, gender differences in the effects of YVV and union timing on IPV perpetration that are obscured in pooled analyses.

DATA AND METHOD

DATA

We used data from the nationally representative Add Health study (for sampling details, see Harris, 2005). At wave I (1994–1995), a random subsample of adolescents in Grades 7 to 12 (11–21 years of age) in sampled schools completed an in-home questionnaire (N = 18,924 with valid weights). A subset of wave I respondents was reinterviewed in 1996 (wave II). The full wave I sample was contacted for reinterviews in 2001–2002 (wave III) and 2007–2008 (wave IV) when respondents were 18–26 and 24–32 years old, respectively. Add

Health is advantageous for the current analysis because of its measurement of YVV and information on the timing and quality of coresidential unions.

ANALYTIC SAMPLE

We focus on the effect of YVV (reported at wave I) on experiences of IPV in first coresidential unions (reported at waves III and IV). We limited our analyses to unions that became coresidential after 16 years of age as unions reported to have been initiated before this age are either highly selective and problematic (Wolfinger, 2003) or, more likely, reflect a data entry error (only .5 percent of respondents reported coresidential unions prior to 16 years of age). Preliminary analyses indicated that victims of youth violence were not excluded systematically by this sample specification.

At wave III, respondents inventoried all past and current romantic relationships and detailed information was collected for 90.6 percent of the relationships reported (n = 38,375 relationships). Of these relationships, 8,362 were coresidential (cohabiting and/or marriage). Coresidential start date information was missing from 515 of these relationships. For sample selection purposes, we used respondents' age when their romantic or sexual relationship started (available for 425 of these relationships) to identify the first occurring union (then later multiply imputed the missing coresidential start dates).[1] From the 8,210 relationships with coresidential start information, we retained the first relationship (n = 6,467) with valid data on IPV within that relationship (n = 6,228).[2] Given continuity in IPV across relationships (Cui et al., 2013; Halpern et al., 2009), for respondents missing data on IPV in their first union, we retained the next occurring coresidential relationship with valid IPV data (n = 203) for a combined wave III sample of 6,431 respondents reporting on their first coresidential union. We controlled for this selection decision by including a dummy variable for *not first union*.

At wave IV, an inventory of relationships was recorded but detailed data were collected on only the current or most recent relationship. Of the 15,216 respondents who had valid relationship details, 10,882 identified their current or most recent relationship as coresidential (marriage or cohabitation), and 6,301 respondents had not already reported a coresidential union at wave III. After combining the 6,431 cases from wave III and 6,301 cases from wave IV, and excluding relationships that became coresidential prior to 16 years of age (n = 61) and that lacked valid sample weights (n = 743), our final analytic sample included 11,928 respondents reporting on their first coresidential union.[3]

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[1]Of the 515 relationships missing coresidential start dates, we had information on the age the relationship started for 387 relationships. Overall, 190 relationships were missing all timing information, but we were able to retain 38 additional relationships because they were the only coresidential relationship reported. The remaining 152 relationships were excluded because there was no way to distinguish timing among multiple coresidential relationships.

[2]Two-hundred thirty-nine first coresidential relationships had to be excluded because that relationship was part of the Add Health "Morris subsample" and respondents were, by design, not asked detailed questions about that relationship—although detailed information could have been collected on other relationships that were not part of that subsample.

[3]A limitation of the wave IV data is that relationship details were collected only for the current or most recent relationship; therefore, the available detail might not correspond to respondents' *chronologically* first coresidential union. In fact, 13.1 percent of the 6,301 respondents at wave IV reported a prior coresidential union that was not their current or most recent. Because these respondents might have selected themselves out of previous coresidential unions where IPV occurred (Kenney and McLanahan 2006), our test of the effect of YVV could be conservative. To address this selection issue, our analyses control for whether data were from respondents' chronologically first coresidential union or the first coresidential union with valid relationship details (203 cases at wave III and 824 at wave IV).

MEASURES

Dependent Variable—IPV was assessed via eight questions about past year frequency of victimization by and perpetration against the romantic partner (e.g., "How often have you slapped/kicked/hit partner?" and "How often has partner threatened you with violence, pushed or shoved you, or thrown something at you that could hurt?") (Conflict Tactics Scales, Straus et al., 1996). Given the low frequencies, we dichotomized each item (1 = ever experienced) and created a multinomial indicator of IPV combining information on victimization and perpetration. Respondents reporting perpetration without victimization were coded as *perpetrator only*; respondents who were victimized but who did not perpetrate violence against that partner were coded as *victimization only*; respondents reporting both victimization and perpetration were coded as *mutually combative. Nonviolent* unions served as the reference group.

Independent Variables

Youth Violent Victimization: Our focal independent variable was assessed by wave I reports of how often in the past 12 months 1) "someone had pulled a knife or gun" on them, 2) "someone cut or stabbed" them, 3) "someone shot" them, or 4) they "were jumped." Original response options were "never," "once," and "more than once." Given the low prevalence of each item, we created a dummy indicator for any experience of *youth violent victimization* (Fang and Corso, 2007).

First Union Timing: We categorized the first union timing based on the age the union became coresidential (described previously). The average age of first union formation in our sample was 22 years 7 months (M = 23 years) with women a year younger ($\bar{x}$ = 22.4 years, M = 22 years) than men ($\bar{x}$ = 23.4 years, M = 23 years) when they first formed a union (t = 12.12, $p < .001$). We designated *early unions* (=1; otherwise = 0) as those that began at ≤20 years of age for women and ≤21 years of age for men, which is approximately 2 years younger than the median age of cohabiting union formation (Manning, Brown, and Payne, 2014) and more than a standard deviation younger than the average age by which people expect one should marry (Settersten and Hagestad, 1996). Approximately one third of unions were thus classified as "early" (32.4 percent).[4]

Control Variables: To guard against spurious interpretations, we controlled for several sets of factors that prior work indicated could be correlated with both YVV and IPV. These measures are divided among *demographic characteristics, violent and deviant behavior, other precocious exits, psychosocial disposition, childhood abuse*, and *union type formed* (married or cohabiting). Preliminary analyses indicated the effects of YVV on IPV did not differ by union type (married vs. cohabiting); this finding is consistent with recent work showing that earlier union timing—which is the focus of our analysis—is largely responsible for some of the negative outcomes associated with cohabitation (Kuperberg, 2014). However, given that cohabitation has generally been associated with IPV risk (Brown

[4]In preliminary analyses, we tested an interval-level specification of union timing and alternative age cutoffs to designate early unions. These results confirmed that, although later union formation is typically associated with lower risks of IPV, the effect of union timing is not linear, and first unions formed before 20 or 21 years of age have substantially higher rates of IPV than those first formed at later ages.

and Booth, 1996; Brown and Bulanda, 2008; Stets, 1991), we included union type as a control in our analyses. In addition, we included three *sample controls* (union reported at wave IV, not first union, and union dissolved) to correct for measurement inconsistencies described previously. We present the full list of controls and describe their measurement and coding in table 1.

ANALYTIC STRATEGY

Our analysis proceeded in three steps using multinomial logistic regression. First, we explored whether YVV was associated with victim continuity (IPV victimization only), victim–offender overlap (IPV perpetration only), or both (mutually combative IPV) in first coresidential unions. Next, we tested our three competing hypotheses for the effect of union timing on the risk of IPV among victims of youth violence by incorporating the interaction between YVV and early union timing. Finally, we reestimated our focal models stratified by gender and tested whether the effects for men significantly differed from those for women. We used the Stata *ice* procedure (Royston, 2005; StataCorp, College Station, TX) to create ten complete data sets and then estimated across the combined data sets, while accounting for the variance across them, using the MI procedures in Stata 12.[5] All multivariate analyses were weighted and adjusted for the complex survey design.

RESULTS

DESCRIPTIVE RESULTS

Table 2 presents descriptive statistics for the unweighted total sample, as well as separately by YVV status. In our sample of first coresidential unions, although most indicated that their union was nonviolent (62.9 percent), a large number of respondents reported experiencing some type of IPV. Specifically, approximately 19 percent of the sample reported that their relationships were mutually combative, 11 percent reported intimate partner victimization only, and 6.7 percent reported intimate partner perpetration only. Slightly fewer than one third of respondents (29.3 percent) entered their unions early, with women starting coresidential unions at significantly younger ages than men ($t = -17.93$, $p < .001$; not shown). Slightly more than 31 percent of women entered their first coresidential union at 20 years of age or younger, whereas 29.0 percent of men entered first unions at 21 years of age or younger. Approximately one fifth (20.9 percent) of respondents who entered a coresidential union reported that they had experienced YVV in the 12 months prior to their first interview.

Not surprisingly, victims of youth violence significantly differed from nonvictims on many indicators. Youth violence victims were significantly more likely to have entered a first union at a young age (33.8 percent vs. 28.1 percent, $p < .001$) and were significantly disadvantaged compared with nonvictims with respect to their demographic, violent and delinquent behavior, precocious exit, dispositional, and child abuse profiles. Youth victims were slightly older; were more likely to be male, Black, or Hispanic; and were more likely

[5]Missing data were greatest for indicators of the age the coresidential union started (3.5 percent; as described previously), reports of family of origin socioeconomic status (5.7 percent), and parental reports of whether the respondent had a temper (14.1 percent; largely because of missing parent interviews). For all other indicators, fewer than 2 percent of cases were missing.

to come from lower socioeconomic status (SES) families. Youth victims displayed significantly greater general violent perpetration, nonviolent delinquency, problem drinking, and drug use during adolescence, and they were more likely to have had a teen pregnancy and to have been arrested (prior to their baseline interview). The psychosocial disposition of victims also was significantly poorer than that of nonvictims, with more depressive symptoms and lower instrumental problem solving skills. Youth victims also were more likely to be viewed as having a "bad" temper by a parent, and they rated their pubertal development ahead of their peers. Although victims of youth violence reported higher levels of childhood physical abuse by a parent or caregiver than nonvictims, reports of childhood sexual abuse did not significantly differ. The unions in our analytic sample also differed by YVV status; victims' unions were more likely to form first as cohabitations (87.5 percent vs. 80.6 percent) and more likely to have dissolved before the interview.

With respect to our dependent variable, the differences in IPV between victims and nonvictims of youth violence are striking. Although most first unions were nonviolent regardless of YVV status, IPV was generally more frequent in the first unions of youth victims. In fact, victims of youth violence were significantly more likely to report that they were victimized in their first unions—with higher prevalences of victimization (14.5 percent vs. 10.2 percent, $p < .001$) and mutually combative (23.4 percent vs. 18.3 percent, $p < .001$) experiences. Correspondingly, victims of youth violence were significantly *less* likely to be the sole perpetrators of violence in their unions (5.7 percent vs. 7.0 percent, $p < .05$). Thus, at least at the bivariate level, some evidence shows that victimization follows those who have experienced YVV into their first coresidential unions (victim continuity), but the extent of victim–offender overlap remains unclear. We now turn to the multivariate results to determine whether these associations hold once we account for the different profiles of victims of youth violence.

MULTIVARIATE RESULTS

We first assessed the extent to which victims of youth violence experience continuity or overlap in terms of intimate partner violence in first coresidential unions overall, ignoring the effects of union timing that we hypothesize could affect IPV risk, to establish a basis for comparison. Table 3 presents two multinomial logistic regression models. Model 1 shows the baseline effect of YVV on having a subsequent first union characterized by the various configurations of IPV, compared with having a nonviolent first union. This initial model adjusts only for union type and the three sample-selection indicators. Model 2 shows the effect of YVV on first union IPV after controlling for the full set of indicators. In preliminary analyses, we entered the blocks of control variables in a stepwise manner. The overall pattern of effects across these models was similar to those in the full model, and therefore, we do not present intermediate models here.

Net of sample controls, we find that victims of youth violence were significantly more likely to report some types of IPV in their first unions than those who did not experience YVV (model 1). The relative risk of experiencing IPV victimization in first unions, compared with being in a nonviolent union, was approximately 70 percent [(exp (.529) – 1) × 100] higher for those who experienced YVV than for those who did not. Victims of youth violence also

had a 28 percent greater risk of being in first unions that were mutually combative compared with being in nonviolent unions. YVV was not significantly associated with IPV perpetration.

After incorporating our full set of controls (model 2), YVV remained a significant risk factor for IPV victimization (as the state dependence explanation would suggest) with the risk of IPV victimization in first unions 31 percent higher among victims of youth violence. Cross-comparisons (rotating the reference category) also indicate that YVV was associated with a significantly greater risk of having a first union marked by IPV victimization than either IPV perpetration or being mutually combative (b = .344, standard error [SE] = .156, $p < .05$; b = .380, SE = .119, $p < .01$, respectively [not shown]). In the fully adjusted model, however, YVV no longer significantly increased the risk of being in a mutually combative first union relative to a nonviolent first union. The increased risk of being in a mutually combative first union was explained by the higher levels of general violent and delinquent behavior exhibited by victims of youth violence than non-victims of youth violence. The constellation of general violent and delinquent behavior was such that the inclusion of any one predictor —youth violent perpetration, nonviolent delinquency, problem drinking, or drug use (all of which were significantly associated with mutually combative first unions)—reduced the effect of YVV to statistical nonsignificance. This finding is consistent with the population heterogeneity explanation, as discussed previously.

Although the effects of our control variables differed somewhat across types of IPV, the general pattern of effects largely conformed to prior research. Respondents who were younger at the first interview, were non-White, and had experienced childhood abuse all faced higher risks of IPV in first coresidential unions than their older, White, non-abused counterparts. Women were more likely to report that their unions involved IPV perpetration or were mutually combative and were less likely to report IPV victimization than men—a pattern that has been found in prior studies of IPV with these data (Whitaker et al., 2007). First coresidential union IPV experiences generally were not predicted by the other substantive covariates, with only a few exceptions. Respondents reporting a teen pregnancy had an increased risk of mutually combative IPV, as did those who had been arrested previously—although this latter effect only approached statistical significance. Instrumental problem solving in adolescence was associated with a low risk of IPV perpetration in first unions. Adolescents who reported their pubertal development was ahead of their peers were more likely to report subsequently that they entered first unions characterized by IPV victimization. Adolescents who reported more depressive symptoms than their peers had an increased risk of subsequently forming mutually combative first unions.

First Union Timing—Having established that YVV increases the risk of IPV victimization in first coresidential unions—that victims of violence in adolescence experience continuity in victimization by being at an increased risk of IPV victimization in young adulthood—we now consider our three competing hypotheses for the effect of the age at which the union became coresidential. Victims of youth violence enter unions at earlier ages than nonvictims (table 2; see also Kuhl, Warner, and Wilczak, 2012). We added a measure of early union formation to the full model and interacted this with YVV to determine whether early union formation is problematic, beneficial, or inconsequential

altogether. We present these results in table 4; we omit the effects of control variables in the interest of space.

As presented in model 1, early first coresidential union formation was associated with a 36 percent higher risk of being mutually combative [(exp (.310) – 1) × 100] than later first union formation. Early first union formation did not present significantly different risks of either IPV perpetration or victimization alone. The results in model 1 also indicate that early union timing does not mediate the association between YVV and first union IPV as the estimated effects of YVV are largely unchanged from those presented in model 2 of table 3.

Specifying an interaction between YVV and early union timing reveals, however, that early union formation is linked to IPV differently for victims and nonvictims of youth violence (table 4, model 2). Forming an early coresidential union is associated with increased risk of mutually combative IPV for those who did *not* report experiencing YVV ($b = .406$, $p < .001$). However, victims of youth violence *do not* experience increased risks of IPV (perpetration, victimization, or mutual violence) when they formed unions early. Indeed, the negative coefficients for the interaction term suggest that adolescent victims of violence have *slightly lower* risks of being in mutually combative first coresidential unions than the risks faced by nonvictims of youth violence who enter first unions at later ages. This finding is consistent with the expectation of hypothesis 2: “Early Unions are Beneficial.” Supplemental analyses show that early union formation increases the risk of mutually combative IPV in first unions only among individuals who did *not* experience YVV ($b = .450$, SE = .157, $p < .000$ [not shown]). The effect of YVV on subsequent IPV victimization and perpetration in first unions did not differ by union timing, as predicted by hypothesis 3: “Early Unions are Benign.” The pattern of coefficients indicates that victims of youth violence do not face additional IPV risks if they form unions at younger ages.

As stated in hypothesis 4, we expected the effect of YVV and early union timing to be contingent on gender with the effects more pronounced among females. To examine this possibility, we stratified our focal model (model 2, table 4) by gender and tested for statistically significant differences in the estimated effects for men and women. These results presented in table 5 indicate that pooled analyses obscured nuanced gender differences. To facilitate interpretation of these effects, table 5 displays two sets of results: models using nonviolent union as the reference (Panel A) and those using mutually combative union as the reference (Panel B). Again, we do not present the estimated effects of our controls.

As the results from table 4 suggest, YVV is significantly associated with an increased risk of victimization only IPV, relative to being in a nonviolent union, independent of union timing. This effect is largely similar for men and women (table 5, Panel A). Although the estimated effect of YVV on IPV only achieves statistical significance in the model for men, the coefficients for men and women are not significantly different. Victim continuity between YVV and IPV in first unions thus seems to be a relatively robust consequence of YVV.

However, as model 2 (table 5) indicates, there were several unique findings for females. Female victims of youth violence and nonvictimized females who formed early coresidential

unions are at a moderately increased risk of perpetrating IPV in those unions (b = .480, p < .10; b = .314, p < .05, respectively). Early union formation is associated with a statistically significant increased risks of IPV perpetration for females, but among female victims of youth violence, early union formation is associated with an *offsetting reduction* in the risk of being in perpetration only (b= −1.010, p < .01) and mutually violent (b = −.816, p < .01) first unions relative to being in nonviolent first unions. Male victims of youth violence do not experience any reduction in these risks with early union formation. In fact, the estimated coefficients, although not statistically significant, are in the opposite direction, suggesting that the lower risk of being in a mutually combative first union among victims of youth violence identified in model 2 of table 4 is being driven solely by the experiences of women. The results in panel A thus suggest that for female victims of youth violence, early union formation is associated with reductions in exposure to both perpetration only and mutual violence (in support of hypothesis 2).

However, the findings from Panel B (in which the risks of IPV are in reference to being in a mutually combative union) partially contradict the conclusion that early union formation is beneficial for female victims of youth violence. Here, the positive and significant interaction between YVV and early union formation among females indicates that early unions are in part associated with decreased risks of being in mutually combative unions because female victims of youth violence who enter coresidential unions early face higher risks of experiencing IPV *victimization*. Female victims of youth violence have relative risks of nonviolent unions that are 2.26 times greater than being in a mutually combative union. Similarly, female victims of youth violence have relative risks of victimization-only unions that are 2.72 times greater than being in a mutually combative union. Importantly, the difference between the risks of being in a nonviolent first union, or a first union characterized by victimization only, does not significantly differ for women who experienced YVV (see also table 5, panel A). We did not detect this effect when we rotated the reference groups in the pooled model because the nonsignificant interaction between YVV and early union formation for IPV victimization for males is negative and, thus, counterbalanced the positive interaction for females.

By considering these complex patterns as a whole, we can observe that early union formation among female victims of youth violence reduces the risk of IPV perpetration and mutual violence because it equally *increases* the risk of being in nonviolent or victimization-only unions. Thus, early union formation among female victims of youth violence seems to be beneficial for some (who form nonviolent unions, in support of hypothesis 2) but bad for others because they are equally likely to form unions where they are subsequently victimized (in support of hypothesis 1 and consistent with the overall findings of victim continuity). The suggestion that for at least some women who experienced YVV early union timing reduces the risk of IPV because such early unions are nonviolent is consistent with other studies showing benefits to early unions among vulnerable groups (Booth, Rustenbach, and McHale, 2008).

Overall, our findings yield the most support for hypothesis 3—precocious coresidential union formation neither exacerbates nor ameliorates the effect of YVV on subsequent IPV. YVV is significantly associated with subsequent IPV independent of union timing and net of

a host of individual controls, providing robust evidence for victim continuity between adolescence and young adulthood across domains. However, consistent with the supposition of hypothesis 4, we also found key differences by gender in the effect of union timing for victims of youth violence. Among males, early union timing seems to be benign, which is consistent with hypothesis 3 and the findings from the pooled analyses; given that nearly 70 percent of persons who experienced YVV were male, this similarity is not surprising. Among females, however, we find evidence suggesting that early unions could be bad for some by exacerbating the effect of YVV on IPV victimization (hypothesis 1) but beneficial for others by reducing the risk of intimate partner violence (hypothesis 2). What allows some women who were victimized as youths to form beneficial unions, whereas others form unions in which they are subsequently victimized, however, is unknown—a point to which we return in our discussion.

SUMMARY AND DISCUSSION

Motivated by recent efforts to converge two largely disconnected bodies of research—work investigating the consequences of nonfamilial YVV and work examining the causes and correlates of IPV in young adulthood—the current study bridges these two lines of work with family research on early coresidential unions, infused with insights from life-course theory. Recent work (Kuhl, Warner, and Wilczak, 2012) demonstrated that violent victimization in adolescence affected the timing of coresidential relationship formation, propelling youth into early cohabiting and marital unions. However, we lacked any indication of the "character and content" (Macmillan, 2001) of these early coresidential relationships.

By using longitudinal data from Add Health, we sought to understand whether early union formation among victims of youth violence represents a context perpetuating further violence and victimization or a turning point in the lives of victims. We used multinomial logistic regression to examine three competing hypotheses as to whether the effect of early union timing among victims of youth violence increased, mitigated, or was inconsequential for the risk of subsequent intimate partner violence—differentiating between victimization only, perpetration only, and mutually combative relationships. Given differences in rates of victimization and union formation, we were attuned to the potential for these associations to differ by gender, and we hypothesized that any union timing effects would be more consequential for women.

Initial analyses found that victims of youth violence were significantly more likely than nonvictims to experience victimization or mutual violence in first unions. After controlling for individual demographics, behavioral and dispositional characteristics, other precocious exits, and childhood abuse, the effect of YVV on the risk of being in a first union characterized by victimization was robust. This continuity in victimization is consistent with the assertions of a state dependence explanation (Nagin and Paternoster, 2000; Ousey, Wilcox, and Brummel, 2008). The experience of victimization in adolescence has a profound, transformative effect on victims, changing them in such a way as to make them consistently vulnerable to subsequent victimization. That is, the stability of victimization does not seem to be a result of individual differences in victim proneness (as a population

heterogeneity explanation would expect). The effect of YVV on mutually combative IPV, in contrast, was accounted for by individual characteristics, particularly adolescent problem behavior (violent perpetration and delinquency, problem drinking, and drug use), which is consistent with a population heterogeneity approach.

With respect to union timing, although we observed a direct effect of union timing on IPV—early first coresidential unions increased the risk of being in a mutually combative union—our analyses suggested that early union timing did not interact with YVV status to influence risk of IPV. However, this benign effect of union timing was only the case for IPV victimization. Early union timing of victims of youth violence seemed to be beneficial for reducing the risk of mutual partner violence, however. Only among *nonvictims* of youth violence is there an increased risk of mutually combative IPV when they enter into unions prematurely. Importantly, we uncovered a complex gender pattern. For women, early union timing among victims of youth violence mitigated the risks of being in first unions characterized by IPV perpetration and mutual violence. However, the reduced risk of being in a mutually combative first union for female victims of youth violence did not represent a wholly ameliorative or beneficial effect. Female victims of YVV were equally likely to enter a nonviolent union *or* a union in which they were further victimized.

The overall pattern of results thus leads us to two conclusions. The first conclusion is that there is a robust effect of YVV on IPV victimization in first coresidential unions independent of union timing. Consistent with a state dependence approach to continuity in victimization (Halpern et al., 2009; Ousey, Wilcox, and Brummel, 2008), being a victim of nonfamilial violence in adolescence has a significant transformative—perhaps even scarring—long-term effect, and it leads to subsequent victimization by coresidential partners, regardless of the age at which those unions are formed.

The effect of YVV on IPV victimization thus looks not to be a “types of people” effect—as it is robust to all controls—but a damaging effect that persists into adult first coresidential unions, whereby (and for reasons not identified here) victims of youth violence continue to be victimized. That the effect of YVV on IPV victimization persists net of controls for psychosocial and dispositional qualities (including instrumental problem solving) suggests that this persistence in victimization is not simply a result of learned helplessness. Although YVV is associated with precocious union formation (Kuhl, Warner, and Wilczak, 2012), the victimization experience creates a population of youth who are at risk for subsequent IPV victimization regardless of union timing—the precocious nature of former victims’ unions does not exacerbate these risks.

The null finding with regard to union timing is important because it means that even if these youth victims delayed coresidential union formation, their unions would remain more likely to be violent. Future research is needed to understand the persistent long-term link between YVV and subsequent IPV victimization; furthermore, violence intervention programs should be mindful of these long-term consequences and recognize that experiences of YVV have consequences beyond immediate involvement in risky behaviors. Given that the finding of victim continuity is robust to many controls, it could be that the characteristics of coresidential partners are key. Perhaps youth victims are forming coresidential unions

(precocious or not) with older partners who use that status differential to wield more power in the relationship (Roberts, Auinger, and Klein, 2006; Teitelman et al., 2008).

The second conclusion is that the effect of YVV on being in a mutually combative union principally reflects victim–offender overlap, but for female victims of youth violence, the timing of union formation reduces the risks of perpetration (either alone or in a mutually combative manner). That the higher risks of being in a mutually combative union generally reflect individual factors, particularly involvement in adolescent risk behaviors, indicates that youth victimization is associated with a set of risky behaviors, which are themselves risk factors for later IPV perpetration, consistent with population heterogeneity explanations. Violent, delinquent, and/or substance-using adolescents are victimized, and this "risk package" carries into young adulthood and shapes the risk of IPV in intimate unions.

However, at least for female victims of youth violence, there does seem to be a reduced likelihood that they will perpetrate violence (either alone or in a mutually combative manner) if they form coresidential unions early. While part, although not all, of the lower risk of being in a mutually combative union is offset by an increased risk of experiencing only victimization, these differences do suggest that for at least some subset of female victims of youth violence, early union formation is a beneficial or a *prosocial* role transition (Booth, Rustenbach, and McHale, 2008; Sampson and Laub, 1990).

It is unclear why early union formation heightens the risk of victimization for some women who experience YVV but increases the risk of being in a nonviolent union for others. It could be that the differences in YVV experiences—such as the severity of victimization, whether there were repeated episodes, or the availability of psychological services after victimization—differentiate the consequences of early union formation for women. Given the measures available, unfortunately we cannot explore this possibility in Add Health. Much like with the finding of victim continuity discussed previously, it also could be that the characteristics of partners with whom female victims of youth violence form precocious unions are pivotal for reducing risk of IPV overall or for leading to more victim continuity. Perhaps female victims of youth violence who form early unions with other victims are at risk of continued victimization, whereas those who form unions with nonvictims are protected. Unfortunately, data on partner characteristics are limited and do not include information on victimization experiences. Future research should explore these possibilities.

Our findings contribute to increasing efforts toward theoretical and empirical integration of research on youth violence and young adult IPV, and they apply general life-course principles (particularly, recent attention to "precocious role exits") to examine the developmental period from adolescence into young adulthood. We add new insight into both the consequences of YVV and the correlates of young adult intimate partner victimization and perpetration. Despite these additions, it is important to acknowledge some key limitations of the current study. First, Add Health is a school-based study, and as such, our population of interest—youth victims of violence—could be more likely than nonvictims to be truant or to have dropped out altogether; thus, these individuals might be underrepresented in the current sample. A second limitation concerns the discrepancy between waves III and IV with respect to the relationship information collected as

respondents at wave IV were asked detailed questions of only their current or most recent relationship. We made efforts to account for this in our models but recognize that respondents would likely select themselves out of violent relationships. The current or most recent relationships reported at wave IV could be a selective set of "better" unions (possibly resulting in conservative estimates in our analyses).

Notwithstanding these limitations, the current study makes several noteworthy contributions to scholarship on adolescence, violence and victimization, and coresidential relationship formation and stability. We extend victimization research by expanding the focus beyond childhood and familial victimization, which is important given that most youth victimization occurs outside the home (Finkelhor et al., 2009; Lauritsen, 2003). Although much research on IPV in young adulthood has focused on dating relationships, collapsed all intimate relationships into one amorphous category, or failed to make any distinctions about relationship type at all, we focused on first coresidential unions. Union formation is a significant marker of the transition to adulthood and aligns our findings with the precocious role exits literature. We examined both intimate partner violent victimization and perpetration with attention to issues of directionality, and our pattern of results supported the need for these distinct classifications. Furthermore, we separately examined men and women to show that YVV had gendered consequences for some types of IPV experiences in precocious first unions.

Our findings are generally consistent with several issues raised within the current body of scholarship on IPV, namely, the gender symmetry of violence and the prevalence of mutually combative unions found in survey data—what Johnson (2008) called situational couple violence. At the same time, however, the pattern of associations between YVV and the different classifications of IPV reveal a complicated, dynamic picture of partner violence and perhaps the need for further exploration of subtypes or possible dyadic typologies, as well as the need to be "gender inclusive" in developing measures to assess different types of violence in intimate relationships (Langhinrichsen-Rohling, 2010). These additional considerations are beyond the scope of this investigation, yet they are important concerns for future research. Finally, longer term follow-up of these relationships is necessary to examine patterns of relationship duration or the cessation and escalation of partner violence.

We have addressed a significant gap in the literature by focusing on the violent nature of the first coresidential unions formed by victims of youth violence, but much work still needs to be done. The mechanisms linking youth violent victimization to subsequent victimization in first coresidential unions remain elusive. Moreover, IPV is just one facet of the "character and content" of these relationships (Macmillan, 2001). As such, scholars should continue efforts to examine additional features of adult coresidential unions, in addition to more detailed information on youth victimization experiences. Considerations of relationship satisfaction, support, and intimacy are key to understanding long-term patterns of risk among prior victims of violence and can offer additional insight to life-course criminology.

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Table 1

Additional Control Variables [a]

Construct	Indicators and Response Options
Demographic Characteristics	
Gender	Dummy variable for female (0/1)
Race/ethnicity	Dummy variables for (a) non-Hispanic White, (b) Black, (c) Hispanic, and (d) Other (includes Asian and American Indian) races (0/1)
Immigrant status	Dummy variable for respondent not born in the United States (0/1)
Family socioeconomic status	Combined scale of parent's education and parent's occupational level (0–9; Bearman and Moody, 2004)
Family structure	Dummy variables for lived with biological parents (0/1)
Violent and Delinquent Behavior	
Violent perpetration	Four-item count of any past year perpetration (e.g., "been in a serious fight;" range: 0–4)
Nonviolent delinquency	Ten-item mean rating scale of frequency of past year perpetration (e.g., vandalism, theft; range: 0 = never to 3 = 5 or more times)
Problem drinking	Dummy variable for any trouble with parents, friends, family, or work because of drinking (0/1)
Illegal drug use	Dummy variable for any past month marijuana, cocaine, or other drug use (0/1)
Other Precocious Exits [b]	
Teenage pregnancy	Dummy variable for respondents who had been or had gotten a partner pregnant by wave I (0/1)
Arrest	Dummy variable for respondents having been arrested by wave I age (0/1); measured at wave III
Psychosocial Disposition [c]	
Temper	Dummy variable for parents' assessment that respondent had a "bad" temper (0/1)
Depressive symptoms	Nineteen-item summated scale of CES-D items (e.g., past week frequency respondent was "bothered by things" or "could not shake the blues;" range: 0 = never to 3 = most of the time; $\alpha = .87$)
Instrumental problem solving	Four-item mean rating scale assessing self-efficacy (e.g., "When you have a problem to solve, one of the first things you do is get as many facts about the problem as possible"; range: 0 = "strongly disagree" to 4 = "strongly agree;" $\alpha = .74$)
Relative pubertal development	Self-rated physical development compared to same-aged peers (range: −2 = "I look younger than most" to 2 = "I look older than most")
Childhood Abuse	
Physical abuse	Dummy variable for any parent/caregiver physical abuse experienced before sixth grade/12 years of age (0/1); measured at wave III
Sexual abuse	Dummy variable for any parent or caregiver sexual abuse (including forced touching) experienced before sixth grade/12 years of age (0/1); measured at wave III
Union Type	
Cohabitation	Dummy variable for coresidential unions formed as a cohabitation (0/1); measured at waves III and IV
Sample Selection [d]	
Union reported at wave IV	Dummy variable for the interview wave the first coresidential union was reported (0/1); measured at wave IV
Not first union	Dummy indicator that the union included in the analyses was not chronologically first (0/1); measured at waves III and IV
Union dissolved	Dummy indicator that the union was no longer intact (0/1); measured at waves III and IV

[a] Unless otherwise noted, all indicators are measured at wave I.

[b] Preliminary analyses tested whether the respondent had ever dropped out of high school or run away from home in the past year (wave I), but neither was significantly associated with IPV net of other variables and thus were excluded for parsimony.

[c] Preliminary analyses tested measures of self-esteem, intelligence, parent-child relations (i.e., closeness or supervision), religious importance, and expectations of marriage by 25 years of age; these measures were not significantly associated with IPV net of other variables and thus were excluded for parsimony.

[d] These factors could have affected which coresidential unions were eligible for inclusion in our analytic sample. To the extent that IPV is negatively associated with the age at first union formation (and thus nonviolent unions are more likely to be reported at wave IV) and positively associated with greater instability, failure to control for these three factors could bias our sample toward nonviolent relationships.

Table 2

Unweighted Sample Descriptive Statistics by Youth Violent Victimization: Means/Proportions (Standard Deviations) and *t*-Tests[a,b]

			Youth Violent Victimization				
	Full Sample		Nonvictims		Victims		
Variable	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**	***t* Test[c]**
Youth Violent Victimization	.209	—	—	—	—	—	
Intimate Partner Violence							
Nonviolent [d]	.629	—	.646	—	.564	—	***
Perpetration only	.067	—	.070	—	.057	—	*
Victimization only	.111	—	.102	—	.145	—	***
Mutually combative	.193	—	.183	—	.234	—	***
First Union Timing							
Age at first union	22.830	(3.132)	22.878	(3.119)	22.654	(3.165)	**
Early union[e]	.293	—	.281	—	.338	—	***
Demographic Characteristics							
Age at first interview[f]	16.251	(1.673)	16.207	(1.683)	16.418	(1.626)	***
Female	.549	—	.606		.333		***
Race/Ethnicity							
White [d]	0..578	—	.605	—	.480	—	***
Black	.189	—	.177	—	.235	—	***
Hispanic	.161	—	.147	—	.212	—	***
Other race	.071	—	.070	—	.072	—	
Immigrant	.076	—	.077	—	.072	—	
Family SES	4.470	(2.642)	4.559	(2.651)	4.130	(2.577)	***
Live with biological parents	.514	—	.535	—	.438	—	***
Violent and Delinquent Behavior							
Violent perpetration	.764	(1.091)	.495	(.840)	1.710	(1.279)	***
Nonviolent delinquency	.239	(.353)	.183	(.278)	.453	(.497)	***
Problem drinking	.241	—	.202	—	.392	—	***
Drug use	.174	—	.132	—	.313	—	***

	Youth Violent Victimization						
	Full Sample		Nonvictims		Victims		
Variable	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**	***t* Test[c]**
Other Precocious Exits							
Teenage pregnancy	.093	—	.074	—	.165	—	***
Arrest	.022	—	.013	—	.057	—	***
Disposition							
Bad temper	.317	—	.289	—	.422	—	***
Depressive symptoms	11.440	(7.634)	10.851	(7.341)	13.592	(8.222)	***
Instrumental problem solving	2.812	(.622)	2.827	(.608)	2.758	(.671)	***
Relative pubertal development	.237	(1.117)	.210	(1.097)	.345	(1.191)	***
Childhood Abuse							
Childhood physical abuse	.215	—	.197	—	.284	—	***
Childhood sexual abuse	.058	—	.056	—	.067	—	*
Union Type Formed							
Married[d]	.180	—	.194	—	.125	—	***
Cohabiting	.820	—	.806	—	.875	—	***
Sample Controls							
Union reported at wave IV	.495	—	.507	—	.451	—	***
Not first union	.081	—	.075	—	.103	—	***
Union dissolved	.214	—	.204	—	.250	—	***
N g	11,928		9,385		2,479		

ABBREVIATION: SD = standard deviation.

[a] Unweighted estimates (standard deviations) are presented; standard deviations are omitted for dummy variables.

[b] Means for dummy variables can be interpreted as the proportion of the sample coded 1 on that indicator.

[c] Statistically significant differences between nonvictims and victims of youth violence are indicated.

[d] Serves as the reference group.

[e] Early unions were those that started at 20 years of age or younger for women and at 21 years of age or younger for men. See text for details.

[f] Centered at 16 years of age in multivariate analyses.

[g] Sample sizes for YVV subsamples do not total 11,928 because of missing data.

* $p < .05$;

** $p < .01$;

*** $p < .001$.

Table 3

Effect of Youth Violent Victimization on Intimate Partner Violence in First Coresidential Unions, Survey Adjusted Multinomial Regression Estimates (N = 11,928)[a,b]

	Model 1[c]			Model 2[c]		
	Perpetration Only	Victimization Only	Mutually Combative	Perpetration Only	Victimization Only	Mutually Combative
	b	*b*	*B*	*b*	*b*	*b*
Variable	**(SE)**	**(SE)**	**(SE)**	**(SE)**	**(SE)**	**(SE)**
Intercept	−2.371***	−2.496***	−1.504***	−2.600***	−2.470***	−2.096***
	(.157)	(.117)	(.123)	(.354)	(.285)	(.224)
Focal Independent Variable						
Youth Violent Victimization	−.192	.529***	.252**	−.076	.268**	−.112
	(.147)	(.086)	(.089)	(.161)	(.101)	(.119)
Demographic Characteristics						
Age at first interview[d]				−.075*	−.043	−.105***
				(.030)	(.027)	(.024)
Female				1.004***	−.650***	.299***
				(.140)	(.092)	(.076)
Race/Ethnicity[e]						
Black				.548***	.422***	.888***
				(.152)	(.110)	(.110)
Hispanic				.278	.135	.312**
				(.181)	(.157)	(.104)
Other race				.436†	−.139	.394*
				(.236)	(.193)	(.184)
Immigrant				.362†	.257	.124
				(.210)	(.172)	(.156)
Family SES				−.022	−.013	−.020
				(.022)	(.018)	(.015)
Live with biological parents				.013	−.039	−.013

	Model 1[C]			Model 2[C]		
	Perpetration Only	Victimization Only	Mutually Combative	Perpetration Only	Victimization Only	Mutually Combative
	b	*b*	*B*	*b*	*b*	*b*
Variable	**(SE)**	**(SE)**	**(SE)**	**(SE)**	**(SE)**	**(SE)**
				(.106)	(.085)	(.088)
Violent and Delinquent Behavior						
Violent Perpetration				.003	−.021	.080*
				(.060)	(.040)	(.033)
Nonviolent Delinquency				.125	.188	.220*
				(.175)	(.152)	(.102)
Problem Drinking				.169	.117	.181*
				(.145)	(.109)	(.079)
Drug Use				.015	−.110	.305***
				(.179)	(.124)	(.082)
Other Precocious Exits						
Teenage pregnancy				.027	.107	.346**
				(.193)	(.157)	(.115)
Arrest				.389	−.440	.371†
				(.324)	(.291)	(.202)
Disposition						
Bad temper				.118	−.023	.085
				(.116)	(.082)	(.079)
Depressive symptoms				−.012†	.010	.019***
				(.007)	(.007)	(.005)
Instrumental problem solving				−.218*	.061	−.065
				(.088)	(.065)	(.056)
Relative pubertal development				.072	.124**	−.036
				(.058)	(.038)	(.036)
Childhood Abuse						
Childhood physical abuse				.261†	.241*	.538***

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	Model 1[C]			Model 2[C]		
	Perpetration Only	Victimization Only	Mutually Combative	Perpetration Only	Victimization Only	Mutually Combative
	b	*b*	*B*	*b*	*b*	*b*
Variable	**(SE)**	**(SE)**	**(SE)**	**(SE)**	**(SE)**	**(SE)**
				(.137)	(.117)	(.079)
Childhood sexual abuse				.416*	.031	.345*
				(.196)	(.213)	(.132)
Union Type Formed						
Cohabitation	.495**	.281*	.451***	.442**	.201†	.266*
	(.156)	(.112)	(.117)	(.152)	(.118)	(.116)
Sample Controls						
Union reported at wave IV	−.717***	.366***	−.662***	−.595***	.358***	−.491***
	(.116)	(.093)	(.073)	(.123)	(.098)	(.084)
Not first union	.312†	−.129	.236†	.294	−.173	.146
	(.175)	(.163)	(.122)	(.180)	(.169)	(.128)
Union dissolved	.018	.808***	.484***	.052	.767***	.436***
	(.113)	(.097)	(.078)	(.113)	(.097)	(.081)

[a] Weighted logit coefficients (standard errors) are presented.

[b] Standard errors are calculated using the Taylor series method to account for stratification and clustering.

[c] Nonviolent serves as the reference group.

[d] Centered at 16 years of age.

[e] White serves at the reference group.

† $p < .10$;

* $p < .05$;

** $p < .01$;

*** $p < .001$.

Table 4

Effect of Youth Violent Victimization on Intimate Partner Violence in First Coresidential Unions by Age Union Started, Survey Adjusted Multinomial Regression Estimates (N = 11,928)[a,b]

	Model 1[c,d]			Model 2[c,d]		
	Perpetration Only	Victimization Only	Mutually Combative	Perpetration Only	Victimization Only	Mutually Combative
	b	*b*	*b*	*b*	*b*	*b*
Variable	**(SE)**	**(SE)**	**(SE)**	**(SE)**	**(SE)**	**(SE)**
Intercept	−2.700***	−2.559***	−2.304***	−2.740***	−2.574***	−2.349***
	(.376)	(.287)	(.230)	(.378)	(.289)	(.230)
Focal Independent Variable						
Youth Violent Victimization	−.082	.264**	−.124	.077	.327**	.044
	(.161)	(.101)	(.119)	(.202)	(.110)	(.136)
First Union Characteristics						
Early union	.148	.143	.310***	.225	.187	.406***
	(.130)	(.123)	(.083)	(.137)	(.131)	(.097)
Youth victimization × early union				−.396	−.188	−.411*
				(.274)	(.186)	(.193)

[a] Weighted logit coefficients (standard errors) are presented.

[b] Standard errors are calculated using the Taylor series method to account for stratification and clustering.

[c] Nonviolent serves as the reference group.

[d] Models also include the full set of demographic, behavioral, precocious exit, dispositional, child abuse, and sample controls. See text for details.

* $p < .05$;

** $p < .01$;

*** $p < .001$.

Table 5

Effect of Youth Violent Victimization on Intimate Partner Violence in First Coresidential Unions by Age Union Started and Gender[a,b]

	Males[c,d]				Females[c,d]			
	Nonviolent	Perpetration Only	Victimization Only	Mutually Combative	Nonviolent	Perpetration Only	Victimization Only	Mutually Combative
	b	*b*	*b*	*b*	*B*	*b*	*b*	*b*
Variable	**(SE)**	**(SE)**	**(SE)**	**(SE)**	**(SE)**	**(SE)**	**(SE)**	**(SE)**
Panel A								
Intercept	—	−2.581**	−2.664***	−2.598***	—	−1.843***	−3.212***	−1.921***
		(.778)	(.408)	(.330)		(.379)	(.411)	(.295)
Focal Independent Variable								
Youth Violent Victimization	—	−.592*e*	.361*	−.099	—	.480†*e*	.152	.215
		(.359)	(.143)	(.179)		(.255)	(.243)	(.172)
First Union Characteristics								
Early union	—	−.049	.206	.248†	—	.314*	.182	.492***
		(.326)	(.193)	(.148)		(.152)	(.190)	(.110)
Youth victimization × early union	—	.674*e*	−.250	.024*e*	—	−1.010***e*	.186	−.816***e*
		(.523)	(.239)	(.245)		(.351)	(.415)	(.257)
Panel B								
Intercept	2.598***	.018	.327*	—	1.921***	−.116	.706***	—
	(.330)	(.783)	(.154)		(.295)	(.184)	(.181)	
Focal Independent Variable								
Youth Violent Victimization	.099	−.493*e*	.461**	—	−.215	.265*e*	−.063	—
	(.179)	(.382)	(.174)		(.172)	(.254)	(.278)	
First Union Characteristics								
Early union	−.248†	−.297	−.042	—	−.492***	−.178	−.310	—
	(.148)	(.365)	(.171)		(.110)	(.167)	(.196)	
Youth victimization × early union	−.024*e*	.650	−.374*e*	—	.816***e*	−.194	1.002**e*	—
	(.245)	(.553)	(.276)		(.257)	(.360)	(.419)	

NOTE: Survey adjusted multinomial regression estimates (*N* = 11,928).

[a] Weighted logit coefficients (standard errors) are presented.

[b] Standard errors are calculated using the Taylor series method to account for stratification and clustering.

[c] Nonviolent serves as the reference group in Panel A; Mutually Combative serves as the reference group in Panel B.

[d] Models also include the full set of demographic, behavioral, precocious exit, dispositional, child abuse, and sample controls. See text for details.

[e] Coefficients are significantly different between males and females.

† $p < .10$;

* $p < .05$;

** $p < .01$;

*** $p < .001$.