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# When Involvement Travels Further Than Supervision: Family Cohesion, Income, and Arrest in Early Adulthood

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**Abstract:** Arrest in the transition to adulthood can redirect schooling, work, and civic standing, yet family-process research often treats supervision and involvement as a single social-control bundle (Brame et al., 2012; Kirk & Sampson, 2013). Using public-use Add Health data, this article compares parental monitoring with shared activities with parents in a Wave 1-Wave 2-Wave 4 single-mediator model and tests whether family cohesion carries either association across family income quintiles. The complete-case sample includes 2,118 respondents. Shared activities predicted later family cohesion, but the monitoring measure, which captures parental decision control over routine domains, did not. Family cohesion was inversely associated with Wave 4 arrest in the lowest income quintile, and the indirect association from shared activities through family cohesion was significant at low income but not at high income. Monitoring showed no comparable indirect pattern. Routine involvement and parental decision control are not interchangeable in the family-arrest relationship, and the association between cohesive family ties and early-adult arrest is concentrated where family resources are most limited.

**Keywords:** social control theory, family cohesion, shared activities, parental monitoring, arrest

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**Data Availability Statement** This study uses the public-use National Longitudinal Study of Adolescent to Adult Health (Add Health) data distributed through ICPSR. Access and reuse are governed by the Add Health and ICPSR terms of use.

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***Conflict of Interest Statement:*** The authors declare no conflict of interest.

***Ethics / IRB:*** This study used publicly available de-identified secondary data from the National Longitudinal Study of Adolescent to Adult Health (Add Health). Ethics approval was therefore not required for the present

secondary analysis under the applicable institutional and data-use conditions. Please revise this statement if the target journal or institution requires a different formulation.

## Introduction

National estimates indicate that at least 30.2% of Americans are arrested by age 23, and demographic estimates by that age reach 49% for Black males and 38% for White males (Brame et al., 2012; Brame et al., 2014). These contacts occur during the transition to adulthood, when schooling, work, residence, and family roles remain unsettled and later well-being is increasingly tied to how adults are sorted across educational, relational, and status arrangements (Arnett, 2000; Ju et al., 2026; Sampson & Laub, 1993). Justice contact in this period can disrupt schooling, weaken employment prospects, depress wages, and harden later stratification (Kirk & Sampson, 2013; Pager, 2003; Western, 2002; Western & Pettit, 2010). In educational settings already shaped by intense competition over selection, evaluation, and advancement, official contact may also alter how young adults move through opportunity contests that sort later trajectories (Chen et al., 2024; Chen et al., 2025). The burden is not confined to the arrested individual. It also falls on households, labor markets, and public institutions that absorb lost earnings, weaker labor-force attachment, and wider inequality (Wakefield & Uggen, 2010; Western, 2006).

Most research on parenting and adolescent risk speaks instead to delinquency, substance use, or broad problem behavior. Those outcomes do not settle whether the same family processes travel to arrest. Official contact depends on conduct, surveillance, discretion, and visibility to authorities (Brame et al., 2014; Kirk & Sampson, 2013; Pager, 2007). A family correlate of adolescent misconduct is therefore not automatically a correlate of early-adult arrest. The criminological question is narrower: which family inputs remain linked to this institutional outcome, through what mechanism, and under what social conditions?

Family social control has long been central to explanations of why some young people avoid later offending and justice contact whereas others do not (Hirschi, 1969; Sampson & Laub, 1993). Yet empirical work frequently groups supervision, involvement, attachment, support, and communication into broad bundles. When several family processes are folded into one parenting construct, it becomes difficult to determine whether a study is testing social control theory, a diffuse family-climate thesis, or a mixture of both. The problem is sharper here because parental rule setting over routine domains is not the same thing as recurring shared activity with parents, and neither construct is identical to a later sense of family cohesion.

Family influence also operates within unequal economic settings. The same household tie may have different consequences across family-income positions, especially when households face unequal access to material support, organizational buffers, and institutional protection (Conger & Donnellan, 2007; McLoyd, 1998; Sampson & Laub, 1994). Using a Wave 1-Wave 2-Wave 4 Add Health design, the analysis compares parental monitoring with shared activities with parents, estimates whether either association travels through Wave 2 family cohesion, and tests whether the cohesion-to-arrest association changes across Wave 1 family income. The design therefore asks whether these two adolescent family inputs reach the same early-adult institutional outcome through the same pathway, or separate once the mechanism and the economic boundary condition are specified.

## Literature Review

### *Theoretical framework*

Hirschi's (1969) social control theory treats attachment, involvement, commitment, and belief as distinct components of the bond to conformity. Later life-course work likewise placed family ties near the center of explanations of delinquency and offending across adolescence and adulthood (Laub & Sampson, 2003; Sampson & Laub, 1993). Two elements of Hirschi's framework are especially useful here. The theory does not assume that all family processes are interchangeable, and it points toward attachment and involvement as related but analytically separable routes through which family life may shape later conduct.

Empirical work often moves too quickly from broad family-process language to omnibus measures. Meta-analytic and observational research shows that parenting and family support are related to delinquency and allied risk behaviors, but the strength and meaning of those associations vary across specific constructs and instruments (Demuth & Brown, 2004; Hoeve et al., 2009; Wells & Rankin, 1988). Recent review work has also shown how theory, measurement, and causal evidence can drift apart when adjacent constructs are absorbed into a single empirical label (Liu et al., 2026). A design that compares two specific parental inputs inside the same model therefore speaks more directly to theory than a design that treats family process as a single domain.

Theoretical payoffs are clearest when the compared inputs are conceptually adjacent but not identical. Monitoring and shared activities both belong to family social control in a broad sense, yet they imply different mechanisms and different expectations about what should happen next. The comparison is most informative when the two inputs are close enough to invite conflation but different enough to justify separate interpretation.

### *Parental monitoring, shared activities, and arrest*

The monitoring literature has repeatedly warned against treating parental rule setting, parental knowledge, parental solicitation, and adolescent disclosure as though they were the same process (Dishion & McMahon, 1998; Kerr et al., 2012; Stattin & Kerr, 2000). The measure used here captures who decides curfew, friends, bedtime, clothing, and related routine domains. It is best read as parental decision control, not as a general measure of parental knowledge. Shared activities with parents capture something different: recurring co-presence in shopping, meals, school projects, religious attendance, movies, and similar routine settings.

The contrast becomes sharper when the outcome is arrest instead of broad self-reported delinquency. Arrest reflects conduct, but it also reflects surveillance, official discretion, and the point at which behavior becomes legible to legal authorities (Brame et al., 2014; Kirk & Sampson, 2013; Pager, 2007). A family input that is associated with adolescent misconduct is not automatically associated with early-adult arrest. Theoretical clarity therefore requires a direct comparison between supervision and involvement.

Research on family meals, family time, and parental involvement suggests that shared routines are often associated with stronger family relationships and lower levels of several adolescent risk behaviors, even though those associations remain entangled with selection and broader household conditions (Fiese & Schwartz, 2008; M usick & Meier, 2012). The evidence does not place decision control at the center of this pathway. It suggests that routine involvement may be closer than parental authority to the attachment side of social control. **H1:** shared activities with parents will show the stronger inverse association with early-adult arrest, whereas parental monitoring will show a weaker or null association once both family inputs appear in the same specification.

### *Family cohesion as a mediating mechanism*

Family cohesion offers a plausible mechanism through which to evaluate that distinction. If shared activities help reproduce attention, familiarity, enjoyment, and mutual regard inside the household, they should be more closely tied to a later family climate marked by understanding and connectedness. That mechanism fits the attachment side of social control theory and the Add Health tradition of treating parent-family connectedness as a protective social bond (Cernkovich & Giordano, 1987; Resnick et al., 1997).

The same mediated pathway is theoretically less certain for parental decision control. Rule setting may still be associated with later arrest, but its association need not travel through the same attachment process. For that reason, family cohesion is the only mediator in this study. The model does not attempt to represent every family mechanism that could bear on arrest. It tests whether a later cohesive family climate is one route through which the two focal family inputs differ. **H2:** the indirect association through Wave 2 family cohesion will be clearer for shared activities than for parental monitoring.

### *Family income as a boundary condition*

Income enters the model in a narrower way than a full inequality account would imply. The theoretical claim is not that income should moderate every segment of the model. The claim is that the association between family cohesion and later arrest may be stronger where family resources are tighter. Family stress research has long shown that economic strain shapes both family process and developmental consequences (Conger et al., 1994; Conger & Donnellan, 2007). Earlier criminological work similarly argued that poverty changes the context in which family control and attachment operate (Sampson & Laub, 1994).

This is not a family-deficit argument. The argument does not rest on any claim that lower-income families are less cohesive. The narrower claim is that the same degree of family cohesion may be more consequential where there are fewer institutional and material buffers against later justice contact (McLoyd, 1998; Western & Pettit, 2010). For that reason, income is modeled here as a boundary condition on the family cohesion-to-arrest segment. **H3:** family income will moderate the family cohesion-to-arrest association, and the resulting indirect association will be strongest toward the lower end of the income distribution.

## Data and Methods

### *Data*

The study uses the public-use National Longitudinal Study of Adolescent to Adult Health (Add Health), a nationally based longitudinal study that began with adolescents in grades 7-12 and followed them into adulthood (Add Health, 2020; Harris & Udry, 2026; Resnick et al., 1997). The wave structure follows the temporal sequence implied by the argument. Wave 1 supplies the two family-process inputs, the moderator, and the controls; Wave 2 supplies the mediator; and Wave 4 supplies the arrest outcome.

The analytic sample is a complete-case sample with valid information on every variable used in the main specification. That sample contains 2,118 respondents. Using a fixed complete-case sample keeps the longitudinal case base identical across the mediator and outcome equations and avoids shifting denominators across indirect-effect calculations. The tradeoff is straightforward: the estimates describe the subset of respondents with complete information on the study variables, not the entire public-use Add Health cohort, and any systematic attrition or

item nonresponse that remains after covariate adjustment may shape the estimates (Add Health, 2020; Harris & Udry, 2026). The implications of complete-case selection are addressed again in the Discussion and supplement.

### *Measures*

**Parental monitoring (Wave 1).** The monitoring measure is a seven-item index based on who usually decides weekend curfew, friends, clothes, amount of television, television content, weekday bedtime, and food choice. Substantively, the index captures parental decision control over adolescent routine. That is a defensible part of the broader monitoring tradition, but it is narrower than measures centered on parental knowledge or adolescent disclosure (Dishion & McMahon, 1998; Stattin & Kerr, 2000). Higher values indicate stronger parental control over these routine domains.

**Shared activities with parents (Wave 1).** The second focal predictor is an index of routine involvement with resident parents. It includes shopping, sport, religious attendance, movies, school projects, and frequent shared dinners. This measure sits within the family-time and parental-involvement literatures, which have linked regular joint activity with stronger family relationships and lower levels of several adolescent risk behaviors, while also noting ongoing disputes over causality and confounding (Fiese & Schwartz, 2008; Musick & Meier, 2012). Higher values indicate more frequent shared activity.

**Family cohesion (Wave 2).** The mediator is a three-item sum indicating that family members understand the respondent, have fun together, and pay attention to the respondent. The study treats this measure as an indicator of family cohesion and attachment, close to the parent-family connectedness tradition in Add Health and to the attachment side of social control theory (Resnick et al., 1997; Jiang et al., 2024). Higher values indicate stronger cohesion.

**Family income (Wave 1).** Family income is measured as a quintile variable. Conditional effects are evaluated at quintiles 1 and 5 to represent the lower and higher ends of the observed income distribution. This operationalization treats economic location as a conditioning context for family process, not as a background nuisance (Conger & Donnellan, 2007; McLoyd, 1998).

**Arrest (Wave 4).** The outcome is a binary indicator of whether the respondent had ever been arrested by Wave 4. The article treats arrest as a consequential, though still limited, marker of justice contact: it captures a transition in legal-system involvement but does not distinguish charge severity, conviction, or incarceration (Brame et al., 2012; Kirk & Sampson, 2013).

### *Controls*

The main specification adjusts for Wave 1 age, sex, race, baseline violence, baseline substance deviance, baseline delinquency, baseline theft, and baseline family cohesion. The control set follows a pre-treatment confounding rationale. In verbal DAG terms, these covariates are treated as plausible common causes of adolescent family process, later family cohesion, and early-adult arrest, so the aim is to block backdoor paths from the Wave 1 family inputs and the Wave 2 mediator to the Wave 4 outcome while avoiding adjustment for obvious post-treatment variables (Greenland et al., 1999; Morgan & Winship, 2015; Pearl, 2009; Tennant et al., 2020; Vander Weele, 2019). Wave 1 family cohesion is retained as an adjustment for prior family climate. This choice is substantively defensible because a household that is already cohesive at baseline may foster more later involvement and less later arrest. It also has a limitation because the baseline control is conceptually close to the

Wave 2 mediator and may absorb part of the very process under study. The estimates should therefore be read as net of a prior-climate adjustment, not as estimates free of overcontrol risk.

***Analytic strategy***

The statistical model is a single-mediator design in which family income moderates only the association between Wave 2 family cohesion and Wave 4 arrest. The mediator equation regresses Wave 2 family cohesion on parental monitoring, shared activities with parents, and the control set. The outcome equation regresses Wave 4 arrest on both Wave 1 family inputs, Wave 2 family cohesion, family income, the cohesion-by-income interaction, and the same controls. Indirect effects are then computed separately for each Wave 1 family input from this same frozen pair of equations.

The decomposition follows the counterfactual mediation tradition and evaluates indirect paths without requiring a significant total effect as a precondition (Imai et al., 2010; Jiang et al., 2021; StataCorp, 2025; Valeri & VanderWeele, 2013; VanderWeele, 2015; Zhao et al., 2010). Recent applied work has used comparable mediation and serial-mediation designs to study how family support, communication, and related psychosocial processes link antecedents to later outcomes (Liu et al., 2023; Ye et al., 2024; M. Li et al., 2026; Tuo et al., 2026). The article retains restrained causal language because the data are observational. A causal interpretation would require strong assumptions, including no unmeasured confounding of the exposure-mediator, exposure-outcome, and mediator-outcome relations after conditioning on the covariates, and no mediator-outcome confounder affected by the exposure (Imai et al., 2010; Pearl, 2009; Valeri & VanderWeele, 2013; VanderWeele, 2015). The available data do not permit those assumptions to be verified. The indirect effects are therefore best read as model-based decompositions from a longitudinal specification, not as identified causal mechanisms.

The main analysis uses linear-probability path equations with heteroskedasticity-consistent covariance matrix estimator number 3 (HC3) standard errors and 5,000 percentile-bootstrap replications for the conditional indirect effects (Hayes & Cai, 2007; Long & Ervin, 2000; MacKinnon & White, 1985). HC3 is used because the linear-probability equations combine a binary outcome with unequal cell sizes across the income distribution, making finite-sample sensitivity to heteroskedasticity a live concern. The linear-probability form also keeps the moderated indirect decomposition readable in probability terms. For publication reporting, the article follows the `M001` effect-size style and presents both standardized coefficients (`beta`) and percentage coefficients (`bp`). The `bp` estimates come from the min-max normalized version of the same frozen sample and frozen equations, which keeps the reporting scale aligned across paths while preserving the underlying model structure (Zhao et al., 2024; Zhao et al., 2025). The supplement reports a logit sensitivity check to show whether the core pattern survives a nonlinear outcome model and to guard against the possibility that the linear link alone is driving the substantive reading of the results (Morgan & Winship, 2015; Musick & Meier, 2012). By analogy, work in other fields that combines 16S ribosomal DNA profiling with shotgun metagenomic sequencing also shows how complementary data layers can sharpen construct resolution when a single measurement stream is too coarse for mechanism claims (Gao et al., 2024).

Conditional estimates are reported at income quintiles 1 and 5. Those values mark the lower and upper ends of the observed income distribution and keep the moderated path easy to interpret without implying that the full income gradient is exhausted by a simple low-high contrast. The article uses those endpoints as substantively legible reference points. It does not claim that only the tails of the distribution are relevant.

## Results

### *3.1 Sample characteristics*

**Table 1.** Descriptive Statistics of the Analytic Sample by Family Income Quintile

| Characteristic | Overall | Income quintile 1 | Income quintile 5 | p value |
|---|---|---|---|---|
| **Sample size** | | | | |
| No. of participants | 2118 | 207 | 530 | |
| **Demographic characteristics** | | | | |
| Age (W1), mean (SD) | 14.956 (1.562) | 15.290 (1.614) | 14.983 (1.467) | 0.018 |
| Male sex, No. (%) | 979 (46.223%) | 99 (47.826%) | 248 (46.792%) | 0.865 |
| Race/ethnicity, No. (%) | | | | <0.001 |
| Hispanic | 198 (9.348%) | 42 (20.290%) | 21 (3.962%) | |
| Black/African American | 305 (14.400%) | 43 (20.773%) | 78 (14.717%) | |
| Asian/Pacific Islander | 68 (3.211%) | 8 (3.865%) | 13 (2.453%) | |
| American Indian/ Native American | 48 (2.266%) | 7 (3.382%) | 13 (2.453%) | |
| Other race/ethnicity | 18 (0.850%) | 2 (0.966%) | 4 (0.755%) | |
| White | 1481 (69.924%) | 105 (50.725%) | 401 (75.660%) | |
| **Primary study variables** | | | | |
| Family income quintile (W1), mean (SD) | 3.407 (1.284) | 1.000 (0.000) | 5.000 (0.000) | <0.001 |
| Parental monitoring (W1), mean (SD) | 4.944 (1.529) | 4.763 (1.663) | 5.017 (1.495) | 0.056 |
| Shared activities with parents (W1), mean (SD) | 3.787 (2.184) | 2.961 (2.115) | 3.992 (2.232) | <0.001 |
| Family cohesion baseline (W1), mean (SD) | 11.490 (2.389) | 11.502 (2.614) | 11.496 (2.358) | 0.976 |
| Family cohesion (W2), mean (SD) | 11.374 (2.478) | 11.072 (2.801) | 11.381 (2.330) | 0.160 |

| Characteristic | Overall | Income quintile 1 | Income quintile 5 | p value |
|---|---|---|---|---|
| Any arrest by Wave 4, No. (%) | 551 (26.015%) | 75 (36.232%) | 119 (22.453%) | <0.001 |
| **Baseline behavioral covariates** | | | | |
| Violence index baseline (W1), mean (SD) | 0.139 (0.431) | 0.232 (0.595) | 0.104 (0.387) | 0.004 |
| Substance deviance baseline (W1), mean (SD) | 0.123 (0.417) | 0.101 (0.387) | 0.132 (0.449) | 0.357 |
| Delinquency index baseline (W1), mean (SD) | 0.567 (0.616) | 0.502 (0.614) | 0.628 (0.611) | 0.013 |
| Theft index baseline (W1), mean (SD) | 0.261 (0.515) | 0.271 (0.525) | 0.266 (0.510) | 0.916 |

Note. Continuous variables are reported as mean (SD). Categorical variables are reported as No. (%). p values compare income quintile 1 and income quintile 5 using Welch two-sample t tests for continuous variables and Pearson chi-square tests for categorical variables.

### *3.2 Zero-order correlations of key study variables*

**Table 2.** Zero-order Pearson Correlations of Key Study Variables

| No. | Variable | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | Age (W1) | | | | | | | | | | |
| 2 | Male sex | 0.066** | | | | | | | | | |
| 3 | Family income quintile (W1) | -0.044* | 0.011 | | | | | | | | |
| 4 | Parental monitoring (W1) | 0.302*** | -0.036 | 0.058** | | | | | | | |
| 5 | Shared activities with parents (W1) | -0.260*** | 0.006 | 0.136*** | -0.113*** | | | | | | |
| 6 | Family cohesion baseline (W1) | -0.144*** | 0.034 | 0.010 | -0.021 | 0.340*** | | | | | |

| No. | Variable | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 7 | Family cohesion (W2) | -0.050* | 0.026 | 0.017 | 0.008 | 0.246*** | 0.593*** | | | | |
| 8 | Violence index baseline (W1) | 0.047* | 0.083*** | -0.074*** | 0.026 | -0.073*** | -0.107*** | -0.088*** | | | |
| 9 | Substance deviance baseline (W1) | 0.124*** | 0.078*** | 0.024 | 0.072*** | -0.136*** | -0.162*** | -0.104*** | 0.290*** | | |
| 10 | Delinquency index baseline (W1) | -0.002 | 0.016 | 0.065** | 0.048* | -0.064** | -0.245*** | -0.183*** | 0.238*** | 0.241*** | |
| 11 | Theft index baseline (W1) | 0.027 | 0.076*** | 0.011 | 0.012 | -0.114*** | -0.223*** | -0.159*** | 0.272*** | 0.325*** | 0. |
| 12 | Any arrest by Wave 4 | 0.029 | 0.253*** | -0.068** | -0.004 | -0.125*** | -0.105*** | -0.086*** | 0.160*** | 0.232*** | 0. |

Note. Cells report Pearson correlations. * p < .05; ** p < .01; *** p < .001.

### *3.3 Direct, mediated, and moderated associations*

Consistent with H1, shared activities showed the clearer protective pattern in the frozen equations. In Equation I of Table 3, shared activities predicted higher Wave 2 family cohesion (beta = 0.062, bp = 0.065, p = .001), whereas parental monitoring did not (beta = 0.017, bp = 0.016, p = .399). In Equation II, shared activities retained an inverse association with arrest (beta = -0.082, bp = -0.180, p < .001), whereas parental monitoring did not (beta = -0.007, bp = -0.013, p = .760). The family input built around routine involvement therefore traveled further than the measure of parental decision control. These results partially support H1.

The mediator and moderator terms also followed the expected pattern. Family cohesion was inversely associated with arrest in Equation II (beta = -0.147, bp = -0.229, p = .015). The cohesion-by-income interaction was positive and statistically significant (beta = 0.266, bp = 0.329, p = .015), indicating that the protective cohesion-arrest association weakened as family income rose. The moderated segment therefore appeared where the design placed it: on the family cohesion -> arrest path.

**Table 3.** Regression Analyses of Mediation and Moderation Effects

| Characteristic | Equation I: Family cohesion (W2) | | | Equation II: Arrest by Wave 4 | | |
|---|---|---|---|---|---|---|
| | bp (SE) | beta (SE) | p | bp (SE) | beta (SE) | p |
| **Focal predictors** | | | | | | |

| Characteristic | Equation I: Family cohesion (W2) | | | Equation II: Arrest by Wave 4 | | |
|---|---|---|---|---|---|---|
| | bp (SE) | beta (SE) | p | bp (SE) | beta (SE) | p |
| Parental monitoring (W1) | 0.016 (0.019) | 0.017 (0.020) | 0.399 | -0.013 (0.044) | -0.007 (0.022) | 0.760 |
| Shared activities with parents (W1) | 0.065** (0.020) | 0.062** (0.019) | 0.001 | -0.180*** (0.050) | -0.082*** (0.023) | <0.001 |
| **Mediator and moderator terms** | | | | | | |
| Family cohesion (W2) | | | | -0.229* (0.098) | -0.147* (0.060) | 0.015 |
| Family income quintile (W1) | | | | -0.321** (0.097) | -0.295** (0.095) | 0.002 |
| Family cohesion (W2) x family income quintile (W1) | | | | 0.329* (0.135) | 0.266* (0.109) | 0.015 |
| **Demographic covariates** | | | | | | |
| Age (W1) | 0.050* (0.023) | 0.042* (0.019) | 0.030 | -0.083 (0.055) | -0.033 (0.022) | 0.134 |
| Male sex | 0.004 (0.007) | 0.009 (0.018) | 0.617 | 0.211*** (0.019) | 0.240*** (0.021) | <0.001 |
| Black/African American (vs Hispanic) | -0.004 (0.017) | -0.007 (0.028) | 0.801 | 0.056 (0.038) | 0.045 (0.031) | 0.147 |
| Asian/Pacific Islander (vs Hispanic) | -0.056* (0.026) | -0.048* (0.022) | 0.028 | -0.093 (0.056) | -0.037 (0.022) | 0.098 |
| American Indian/Native American (vs Hispanic) | -0.007 (0.026) | -0.005 (0.019) | 0.796 | 0.094 (0.067) | 0.032 (0.023) | 0.163 |
| Other race/ ethnicity (vs Hispanic) | 0.013 (0.046) | 0.006 (0.020) | 0.773 | -0.081 (0.088) | -0.017 (0.018) | 0.357 |
| White (vs Hispanic) | -0.015 (0.014) | -0.032 (0.030) | 0.284 | 0.032 (0.031) | 0.033 (0.033) | 0.310 |
| **Baseline covariates** | | | | | | |
| Violence index baseline (W1) | -0.030 (0.032) | -0.021 (0.022) | 0.344 | 0.161* (0.075) | 0.053* (0.024) | 0.031 |

| Characteristic | Equation I: Family cohesion (W2) | | | Equation II: Arrest by Wave 4 | | |
|---|---|---|---|---|---|---|
| | bp (SE) | beta (SE) | p | bp (SE) | beta (SE) | p |
| Substance deviance baseline (W1) | 0.007 (0.020) | 0.007 (0.020) | 0.737 | 0.325*** (0.051) | 0.155*** (0.024) | <0.001 |
| Delinquency index baseline (W1) | -0.020 (0.014) | -0.030 (0.021) | 0.144 | 0.063 (0.033) | 0.044 (0.023) | 0.051 |
| Theft index baseline (W1) | -0.012 (0.015) | -0.015 (0.019) | 0.424 | 0.125** (0.043) | 0.073** (0.025) | 0.004 |
| Family cohesion baseline (W1) | 0.584*** (0.023) | 0.563*** (0.022) | <0.001 | -0.055 (0.060) | -0.025 (0.027) | 0.354 |
| Model N | 2118 | | | 2118 | | |
| R^2 | 0.360 | | | 0.147 | | |

Note. bp (SE) = percentage coefficient and its HC3 robust standard error from the min-max normalized version of the frozen sample and frozen equations. beta (SE) = standardized coefficient and its standard error derived from the frozen raw-sample equation. p = two-sided p value. * p < .05; ** p < .01; *** p < .001. Race coefficients use Hispanic respondents as the reference category.

### *3.4 Indirect effects*

Table 4 reports the indirect effects with explicit path labels. For parental monitoring -> family cohesion -> arrest, the conditional indirect effect was not statistically significant at income quintile 1 (bp = -0.004, 95% CI [-0.015, 0.005]), income quintile 5 (bp = 0.002, 95% CI [-0.003, 0.008]), or in the high-minus-low contrast (bp = 0.005, 95% CI [-0.007, 0.021]). The corresponding index of moderated mediation was also not statistically significant. Under this specification, parental monitoring did not generate a mediated arrest pathway through family cohesion.

Shared activities showed a different pattern. For shared activities -> family cohesion -> arrest, the conditional indirect effect was negative and statistically significant at income quintile 1 (bp = -0.015, 95% CI [-0.032, -0.002]). At income quintile 5, the indirect effect was not statistically significant (bp = 0.006, 95% CI [-0.003, 0.019]). The high-minus-low contrast was itself statistically significant (bp = 0.021, 95% CI [0.003, 0.046]), and the index of moderated mediation matched that same positive contrast on the normalized income scale. The indirect pathway was therefore concentrated at the lower end of the income distribution. These results partially support H2 and support H3.

**Table 4.** Indirect Effects

| Path | Condition | bp | BootSE | BootLLCI | BootULCI |
|---|---|---|---|---|---|
| Parental monitoring -> Family cohesion -> Arrest | Income quintile 1 | -0.004 | 0.005 | -0.015 | 0.005 |

| Path | Condition | bp | BootSE | BootLLCI | BootULCI |
|---|---|---|---|---|---|
| Parental monitoring -> Family cohesion -> Arrest | Income quintile 5 | 0.002 | 0.003 | -0.003 | 0.008 |
| Parental monitoring -> Family cohesion -> Arrest | High minus low | 0.005 | 0.007 | -0.007 | 0.021 |
| Parental monitoring -> Family cohesion -> Arrest | Index of moderated mediation | 0.005 | 0.007 | -0.007 | 0.021 |
| Shared activities -> Family cohesion -> Arrest | Income quintile 1 | -0.015 | 0.008 | -0.032 | -0.002 |
| Shared activities -> Family cohesion -> Arrest | Income quintile 5 | 0.006 | 0.006 | -0.003 | 0.019 |
| Shared activities -> Family cohesion -> Arrest | High minus low | 0.021 | 0.011 | 0.003 | 0.046 |
| Shared activities -> Family cohesion -> Arrest | Index of moderated mediation | 0.021 | 0.011 | 0.003 | 0.046 |

Note. Indirect effects are reported as bp estimates with percentile-bootstrap uncertainty based on 5,000 replications.

### *3.5 Sensitivity analysis*

The nonlinear sensitivity analysis does not change the substantive reading of the main results. As shown in Supplementary Table S1, shared activities retained a negative association with arrest (OR = 0.901, 95% CI [0.851, 0.953]), whereas parental monitoring did not (OR = 0.986, 95% CI [0.915, 1.062]). The family cohesion-by-income interaction remained positive (OR = 1.037, 95% CI [1.004, 1.071]). The sensitivity check therefore preserves the direction and asymmetry reported in the main linear-probability specification.

## Discussion

### *Primary findings*

This study asked whether supervision and involvement are linked to early-adult arrest through the same family mechanism. The estimates suggest they are not. In this specification, shared activities with parents showed an indirect association with later arrest through family cohesion, whereas parental monitoring did not. The two

predictors are often discussed as neighboring pieces of a broader family-process package, but the present results do not support treating them as interchangeable. Routine involvement with parents and parental decision authority over everyday domains do not appear to enter the arrest process through the same route.

The institutional character of the outcome sharpens that distinction. Arrest is not simply another self-reported problem behavior. It reflects both conduct and the settings in which conduct becomes visible to authorities (Brame et al., 2014; Kirk & Sampson, 2013; Pager, 2007). The conditional pattern is equally pronounced. Family cohesion was inversely associated with arrest only toward the lower end of the family-income distribution. Under this specification, the indirect pattern is concentrated where the household context offers fewer material and organizational buffers against later justice contact.

***Theoretical and practical implications***

The theoretical contribution is narrow. Social control theory identifies attachment and involvement as distinct components of the bond to conformity, yet empirical work often collapses them into omnibus family-process language (Hirschi, 1969; Hoeve et al., 2009; Sampson & Laub, 1993). Recent criminological work has likewise continued to separate family attachment, parental knowledge, and neighboring social bonds instead of treating them as a single undifferentiated family climate (Jiang et al., 2024; Wu et al., 2020). The present estimates are more consistent with that disaggregated view. Shared activities are linked to a later family climate characterized by mutual understanding, attention, and enjoyment. The monitoring measure used here does not show the same pattern. The findings should not be read as a general test of social control theory. They indicate that a familiar empirical shorthand, treating supervision, involvement, and family attachment as readily substitutable, can hide substantive differences when the outcome is early-adult arrest.

The practical implication is also specific. In this model, the clearer indirect pattern involves routine involvement and later cohesion, not parental decision authority alone. That result aligns with work showing that cohesive ties and ordinary family routines are associated with lower risk, while also fitting criminological arguments that bonds operate differently across social contexts (Jiang et al., 2024; Markowitz et al., 2001; Resnick et al., 1997; Sampson & Laub, 1994). It also fits evidence outside criminology linking family emotional support to motivational engagement in educational settings, where ordinary support processes help sustain persistence under structural constraint (Chen et al., 2022). The estimates do not warrant a direct policy prescription. They do suggest that family-based approaches concerned with later justice contact should pay close attention to recurring parent-adolescent involvement and household cohesion, especially in settings marked by tighter material constraint.

One plausible explanation for the stronger shared-activities pattern lies in what the two measures capture. Shared activities record recurring occasions of co-presence in which parents and adolescents spend time together, build familiarity, and reproduce a sense of mutual regard. That is close to the attachment climate represented by the family cohesion measure, and it fits prior work on family meals, family time, and parental involvement as routine settings in which family ties are enacted in ordinary life (Fiese & Schwartz, 2008; Hoffmann & Warnick, 2013; Musick & Meier, 2012). The monitoring index captures parental decision control over routine domains. It says something about authority, but it does not directly measure parental knowledge, adolescent disclosure, or parental solicitation of information. The monitoring literature has repeatedly shown that these are distinct constructs conceptually and empirically (Dishion & McMahon, 1998; Kerr et al., 2012; Stattin & Kerr, 2000). The weaker monitoring pattern in this study should therefore be read in relation to this operationalization. It does not show that monitoring is generally irrelevant.

### *Limitations*

Several limitations qualify the contribution. First, the study uses a complete-case public-use Add Health sample. That decision keeps the longitudinal case base fixed across the decomposition, but it also means that attrition and item nonresponse may shape the estimates in ways the observed controls cannot remove. Related work on structured criminal-court data likewise shows how data accuracy and sample-selection decisions can shape downstream inference before any statistical model is estimated (Liu et al., 2026). Second, the arrest outcome is self-reported and records whether any arrest occurred by Wave 4; it does not distinguish frequency, seriousness, charge disposition, conviction, or incarceration. Third, the monitoring measure captures parental decision authority over routine domains and may not travel well to literatures that define monitoring in terms of parental knowledge or adolescent disclosure. Fourth, Wave 1 family cohesion is included as a prior family-climate control, a choice that helps separate the mediator from a wholly stable family environment but also raises the possibility of overcontrol because the baseline control is conceptually close to the Wave 2 mediator.

The mediation claims also require restraint. The indirect estimates are interpreted within a causal-mediation framework, but the data remain observational. A stronger causal reading would require assumptions about unmeasured confounding of the exposure-mediator, exposure-outcome, and mediator-outcome relations, as well as the absence of mediator-outcome confounders affected by the exposure, that cannot be guaranteed here. The linear-probability specification keeps the conditional indirect effects readable on a probability scale, but the binary outcome also makes nonlinear sensitivity checks necessary. The logit appendix supports the main directional pattern, though it does not resolve the broader identification limits. That caution is warranted in part because statistical significance and a clean narrative can travel further in the prose than the underlying design will bear if theory claims outrun the evidence (Li et al., 2025). Parallel debates over recidivism-prediction tasks make a related point: predictive performance, task definition, and claims about responsibility can exceed what a design can justify as theory or explanation (Liu & Li, 2024). A stronger case would require replication with richer monitoring measures, more detailed justice-contact outcomes, and designs that can better distinguish family selection from family influence.

## Conclusion

Separating supervision from involvement and treating family cohesion as a specific mechanism reveals the clearer indirect pattern through shared activity. In this study, that pattern appears in lower-income families and does not appear in the same form at the top of the income distribution. The broader implication is straightforward: criminological accounts of family social control gain precision when they distinguish routine involvement from parental decision authority and ask where cohesive family ties are most closely associated with later justice contact.

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