Learning the Binding Nature of the Law: Police Violence, Criminal Offending and Adolescent Legal Socialization

Technical Appendix

1 Estimating the effect of exposure to police behavior on legitimacy

To test hypotheses 1.1 through 1.3, we rely on matching methods for longitudinal data using a difference-in-differences estimator (Imai et al., 2023). This is a flexible, non-parametric approach that leverages panel data (i.e., four waves of data) to estimate the impact of a binary treatment that can switch status over time (i.e., whether respondents have witnessed some type of police behavior in the past year) on a continuous outcome (i.e., scores reflecting legitimacy judgements). This matching framework relaxes some of the stronger assumptions involved in the two-way fixed effects regression estimator.[1] Crucially, it allows us to properly define and estimate our causal quantity of interest. In this case, the causal estimand consists of the average treatment effect of change in treatment status among observations that did change their treatment status and were exposed to treatment between any two periods $t-1$ and t,[2] up to F time periods after the exposure,

$$ATT_{(F)} = \mathbb{E}\left\{ Y_{i,t+F}^{X_{it=1,i,t-1=0}} - Y_{i,t+F}^{X_{it=0,i,t-1=0}} \;\middle|\; X_{it}=1, X_{i,t-1}=0 \right\}, \tag{1}$$

where Y_{it} refers to the observed outcome for unit i at period t and X_{it} refers to the observed treatment status for unit i at period t. Observations are considered treated when they changed their treatment status from $X_{(i,t-1)} = 0$ to $X_{it} = 1$ between any two periods $t-1$ and t. The superscript $(X_{it} = 1, X_{i,t-1} = 0)$ corresponds to the potential outcome of exposure to treatment between any two periods

[1] With panel data, a continuous outcome, and a binary treatment that can switch status over time, the standard approach would consist of using a two-way fixed effects (TWFE) regression estimator of the multi-period difference-in-differences. In certain circumstances, the TWFE regression estimator can yield the average treatment effect among treatment units (ATT) (Cunningham, 2021). However, this estimator can be unreliable in some scenarios. First, Imai and Kim (2020) demonstrated that including both unit and period fixed effects simultaneously is not a non-parametric, design-based strategy to estimate the ATT—the TWFE estimator yields unbiased estimates only under the standard linear regression modeling assumptions. Second, the TWFE estimator assumes no treatment effect heterogeneity over time (Goodman-Bacon, 2018; Callaway and Sant'Anna, 2020). Considering parallel trends, linearity, and effect homogeneity combined, the TWFE estimator's assumptions are unrealistically strong.

[2] In recent years, several novel multi-period difference-in-differences estimators have been developed, each suggesting different solutions to overcome some of the limitations of the TWFE regression estimator—e.g., linearity (Imai and Kim, 2020) and heterogeneity (Goodman-Bacon, 2018) assumptions. For example, Callaway and Sant'Anna (2020) developed a doubly robust estimator for scenarios that follow a staggered treatment implementation, and Sun and Abraham (2021) developed an estimator that uses the never-treated units as the main control group. However, as highlighted by Roth et al. (2023), the choice of estimator should be made entirely based on the estimand of interest. Rather than using several different estimators, one should select the estimator that properly identifies the estimand. In this case, considering multiple periods, variation in treatment timing, and a treatment status that can switch on and off, the causal quantity of interest refers to comparisons between units that have and have not changed their treatment status (as per Eq. 1), and Imai et al.'s (2023) matching framework consists of an unbiased estimator under a conditional parallel trends assumption.

$t-1$ and t, whereas the superscript $(X_{it} = 0, X_{i,t-1} = 0)$ corresponds to the potential outcome of no change in treatment status. The causal estimand is then defined as a comparison between treated and non-treated observations with carryover effects allowed up to F pre-specified temporal leads (Imai et al., 2023).

In this application, considering four waves of data, we are interested in both short-term, contemporaneous effects (e.g., $F = 0$), that capture effects of exposure to police behavior at the same period when the change in treatment status occurred, and long-term, cumulative effects (e.g., $F = 2$), which capture effects up to two years after the change in treatment status. While specifying a longer-term F would be of substantive interest too, with four waves of data that would be impractical. Given that we also need to condition on treatment history (and pre-treatment covariates, as per a conditional parallel trends assumption), F =2 is the largest number of temporal leads we can include.

Treatment dynamics

This causal quantity allows for future treatment reversal (i.e., a treated observation can go back to the control group in the future) and for some units not being used at all in the comparison. With two treatment groups that can switch on and off over time and four waves of data, there are 16 possible combinations of treatment dynamics, from never treated units (i.e., $X_{i,t=1} = 0, X_{i,t=2} = 0, X_{i,t=3} = 0, X_{i,t=4} = 0$) to always treated units (i.e., $X_{i,t=1} = 1, X_{i,t=2} = 1, X_{i,t=3} = 1, X_{i,t=4} = 1$). Units can play different roles in the definition of causal quantity at different periods depending on their change in treatment status. For example, always treated units are removed from the analysis, and treated units that go back to the control condition are not considered at that period, though they might be again in the future. Table 1 summarizes the treatment dynamics and how different units contribute to the definition of treatment effects at different periods.

Estimation strategy

Under a conditional parallel trends assumption (e.g., conditioning on treatment history, lagged outcome, and time-varying covariates), Imai et al.'s (2023) matching framework for panel data identifies the $ATT_{(F)}$. This estimation strategy has three steps. First, treated and non-treated observations are matched based on an identical treatment history (e.g., lagged by one period). Second, this matched set is further refined by including other potential confounders using the Mahalanobis distance measure. In this case, we match observations based on legitimacy beliefs in the previous period, exposure to neighborhood violence and disorder (including gun shots, gun carrying, robberies, and drug trafficking), personal victimization (including assault by an adult and exposure to fights), family victimization (including relatives or friends being robbed, murdered, or imprisoned), offending behavior among peers (including friends or siblings engaging in theft, disorder, drug use, or violent assault), bullying behavior, bullying victimization, gender, race, and type of school. Finally, the difference-in-differences estimator is applied to estimate the $ATT_{(F)}$.

Importantly, the $ATT_{(F)}$ is only identifiable under a conditional parallel trends assumption. While we do leverage unique longitudinal data and include a comprehensive set of time-varying control variables to refine the matched sets, it is still possible that there are other time-varying confounders un-

Table 1: Treatment dynamics with four periods

Dynamic patterns of treatment allocation	**Role in the definition of ATT$_{(F)}$**		
Treatment status at T1, T2, T3, T4	**T1 → T2**	**T2 → T3**	**T3 → T4**
Group: 0 – 0 – 0 – 0	Control	Control	Control
Group: 0 – 0 – 0 – 1	Control	Control	Treatment
Group: 0 – 0 – 1 – 0	Control	Treatment	Removed*
Group: 0 – 0 – 1 – 1	Control	Treatment	Removed*
Group: 0 – 1 – 0 – 0	Treatment	Removed*	Control*
Group: 0 – 1 – 0 – 1	Treatment	Removed*	Treatment
Group: 0 – 1 – 1 – 0	Treatment	Removed*	Removed*
Group: 0 – 1 – 1 – 1	Treatment	Removed*	Removed*
Group: 1 – 0 – 0 – 0	Removed	Control	Control
Group: 1 – 0 – 0 – 1	Removed	Control	Treatment
Group: 1 – 0 – 1 – 0	Removed	Treatment	Removed*
Group: 1 – 0 – 1 – 1	Removed	Treatment	Removed*
Group: 1 – 1 – 0 – 0	Removed	Removed	Control
Group: 1 – 1 – 0 – 1	Removed	Removed	Treatment
Group: 1 – 1 – 1 – 0	Removed	Removed	Removed
Group: 1 – 1 – 1 – 1	Removed	Removed	Removed

* When considering long-term (e.g., $F = 2$) treatment effects, effects can last up to $F = 2$ leads. These units are not removed from estimation, and repeated, cumulative treatment effects are taken into account.

adjusted for, which would imply that our models do not identify the $ATT_{(F)}$. We acknowledge this limitation. We are drawing on the causal inference framework as a tool, given that our research questions are clearly causal. Rather than steering away from this discussion, we believe the honest approach is to explicitly state all identification assumptions, get as close as possible to an unbiased estimate, and transparently acknowledge all limitations involved in this study.

We estimate the effects of exposure to three types of police behavior (i.e., police stops, arrests, and assault) on changes in beliefs about the legitimacy of the law. For each of the three treatment variables, we specify two models: $F = 0$ to estimate short-term, contemporaneous effects; and $F = 2$ to estimate long-term, cumulative effects. Matched sets are created considering five permitted closest control units. Standard errors were obtained with 1000 bootstrap iterations to handle statistical uncertainty.

2 Estimating the effects of exposure to police behavior and legitimacy beliefs on criminal offending

Second, we move to the treatment-outcome and the mediator-outcome links and test hypotheses 2.1 through 2.3 and 3. To estimate the effects of exposures to different types of police behavior (i.e., police stops, arrests, and assault) and beliefs about the legitimacy of the law on offending behavior, we rely on a different estimation strategy. Ideally, given that the nature of the research question is also causal, we would adopt a similar strategy as we did in the previous section. However, given that we now have two main explanatory variables (each of the three types of police behavior and legitimacy scores) and one

of them is a continuous variable, Imai et al.'s (2023) matching method for longitudinal data was not appropriate. There have been some important developments in the field of multi-period difference-in-differences with variation in treatment timing and a continuous treatment variable (see Callaway et al., 2024), but interpretation of parameters remains highly challenging. Theoretically, while the causal quantity of interest would be like the one defined in Eq. 1, in reality defining the causal estimand is challenging because defining treated and non-treated observations in a continuous setting typically involves unrealistic parametric assumptions.

In addition, modeling self-reported offending behavior is a challenge on its own. Rather than assessing the effects of exposures to police behavior and legitimacy beliefs on each of the four items measuring recent different offending behaviors (theft, disorder, drug use, and violent behavior) separately, we draw on Raudenbush et al.'s (2003) multivariate, multilevel Rasch model and estimate models based on an item-response framework (see also Sampson et al., 2005). This model captures the likelihood that a given respondent, at a given point in time, will commit a specific offending behavior, taking into account that some offenses are rarer than others and that adolescents change their propensity to offend as they get older. Using longitudinal data, Sampson et al. (2005) expanded this framework and fit multivariate, multilevel Rasch models using pooled data with random coefficients for age and age squared—essentially estimating a quadratic growth curve model on propensity to violent offense.

Statistically, this model consists of a multilevel logistic regression in which item responses are nested within individuals. Our estimation strategy builds on (Sampson et al., 2005) and also involves estimating a multilevel logistic regression model in which item responses are nested within individuals using pooled data and a quadratic growth specification. We combine information on 11,904 item responses to the four survey questions generated by 800 adolescents who were interviewed in at least one of the four waves of data between 2016 and 2019. We also expand that model and rely upon the 'within-between' (hybrid) framework that combines the desirable aspects of both fixed effects and random effects econometric models (Mundlak, 1978). Hybrid models can be estimated by decomposing each time-varying predictor into a within-person component and a between-person component, and then estimating a random effects model with both components; the within-person component (i.e., the observation's deviation from that unit-specific mean) is equivalent to a fixed effects estimator, whereas the between-person component models unit-specific means of each variable (i.e., equivalent to time-invariant covariates) (Allison, 2009).

Formally, let t denote the wave of data collection ($t = \{0, 1, 2, 3\}$, where 0 represents the first period), let j denote the survey item of interest ($j = \{0, 1, 2, 3\}$, with one item representing the reference group), and let i denote the survey respondent ($i = \{1, 2, \ldots, 800\}$). $Y_{itj} = 1$ therefore indicates that respondent i self-reported committing offense j at period t, whereas $Y_{itj} = 0$ indicates otherwise. The multilevel within-between model is then defined by

$$log\left(\frac{Prob(Y_{itj}=1)}{1-Prob(Y_{itj}=1)}\right) = \beta_0 + \boldsymbol{\beta_1}(\boldsymbol{X_{it}} - \boldsymbol{\bar{X}_i}) + \boldsymbol{\beta_2}\boldsymbol{Z_i} + \boldsymbol{\beta_3}\boldsymbol{\bar{X}_i} + \gamma_i t_{it} + \theta t_{it}^2 + \mu_i + \alpha_j + \epsilon_{it}, \quad (2)$$

where $\boldsymbol{X_{it}} - \boldsymbol{\bar{X}_i}$ is a row vector of demeaned time-varying predictors (e.g., including exposure to various policing practices, legitimacy beliefs, and time-varying control variables), $\boldsymbol{Z_i}$ is a row vector of time-constant predictors (e.g., race, gender, and school type), $\boldsymbol{\bar{X}_i}$ is a row vector of averaged time-varying

predictors (e.g., individual average over time of exposure to various policing practices, legitimacy beliefs, and time-varying control variables), now treated as time-constant predictors, and t_{it} and t_{it}^2 are age and age squared (measured based on wave of data collection, i.e., $t = \{0, 1, 2, 3\}$), respectively. μ_i and γ_i are individual-level random effects that represent growth parameters, α_j is a fixed effect for each survey item j (e.g., equivalent to difficulty parameters), and ϵ_{it} is a disturbance term. $\boldsymbol{\beta_1}$ is a row vector of coefficients focused on within-individual change over time, whereas $\boldsymbol{\beta_2}$ and $\boldsymbol{\beta_3}$ are row vectors of coefficients reflecting between-individual differences.

3 Expanded results

Covariate balance

Table 2: Covariate balance: contemporaneous effects of exposure to police stops

Control Variables	**Standardized Difference between treated and non-treated observations**
Seen or heard of people selling drugs on the streets	0.010
Seen or heard of people being robbed	0.158
Seen or heard of people other than police officers carrying guns	0.026
Listened to gunshots	0.146
Gender: male	-0.124
Race: White	-0.204
Type of school: private	-0.007
Personal victimization: assaulted by an adult	0.110
Family victimization: relatives or friends were robbed	0.110
Family victimization: relatives or friends were murdered	0.031
Family victimization: relatives or friends were imprisoned	0.173
Offending behavior among peers: thefts	0.294
Offending behavior among peers: disorder	0.063
Offending behavior among peers: drugs	0.152
Offending behavior among peers: violent assault	0.081
Bullying behavior	0.017
Bullying victimization	0.188

Note: Positive values indicate higher values in the treated group; negative values indicate higher values in the non-treated group.

Table 3: Covariate balance: cumulative effects of exposure to police stops

Control Variables	**Standardized Difference between treated and non-treated observations**
Seen or heard of people selling drugs on the streets	-0.342
Seen or heard of people being robbed	0.316
Seen or heard of people other than police officers carrying guns	-0.061
Listened to gunshots	0.138
Gender: male	-0.064
Race: White	-0.511
Type of school: private	-0.105
Personal victimization: assaulted by an adult	0.286
Family victimization: relatives or friends were robbed	-0.130
Family victimization: relatives or friends were murdered	0.079
Family victimization: relatives or friends were imprisoned	0.293
Offending behavior among peers: thefts	0.316
Offending behavior among peers: disorder	-0.014
Offending behavior among peers: violent assault	0.075
Bullying behavior	-0.093
Bullying victimization	0.321

Table 4: Covariate balance: contemporaneous effects of exposure to police arrests

Control Variables	**Standardized Difference between treated and non-treated observations**
Seen or heard of people selling drugs on the streets	0.026
Seen or heard of people being robbed	0.028
Seen or heard of people other than police officers carrying guns	-0.008
Listened to gunshots	0.072
Gender: male	-0.001
Race: White	0.001
Type of school: private	0.001
Personal victimization: assaulted by an adult	0.123
Family victimization: relatives or friends were robbed	0.177
Family victimization: relatives or friends were murdered	0.122
Family victimization: relatives or friends were imprisoned	-0.001
Offending behavior among peers: thefts	0.070
Offending behavior among peers: disorder	0.141
Offending behavior among peers: drugs	0.410
Offending behavior among peers: violent assault	0.174
Bullying behavior	0.154
Bullying victimization	0.287

Table 5: Covariate balance: cumulative effects of exposure to police arrests

Control Variables	**Standardized Difference between treated and non-treated observations**
Seen or heard of people selling drugs on the streets	-0.106
Seen or heard of people being robbed	-0.269
Seen or heard of people other than police officers carrying guns	-0.109
Listened to gunshots	0.106
Gender: male	0.088
Race: White	0.048
Type of school: private	0.079
Personal victimization: assaulted by an adult	-0.014
Family victimization: relatives or friends were robbed	0.117
Family victimization: relatives or friends were murdered	0.145
Family victimization: relatives or friends were imprisoned	-0.115
Offending behavior among peers: thefts	0.059
Offending behavior among peers: disorder	0.072
Offending behavior among peers: drugs	0.143
Offending behavior among peers: violent assault	0.063
Bullying behavior	0.114
Bullying victimization	0.325

Table 6: Covariate balance: contemporaneous effects of exposure to police violence

Control Variables	**Standardized Difference between treated and non-treated observations**
Seen or heard of people selling drugs on the streets	0.127
Seen or heard of people being robbed	-0.051
Seen or heard of people other than police officers carrying guns	0.068
Listened to gunshots	0.021
Gender: male	0.008
Race: White	-0.090
Type of school: private	0.147
Personal victimization: assaulted by an adult	-0.023
Family victimization: relatives or friends were robbed	0.123
Family victimization: relatives or friends were murdered	-0.043
Family victimization: relatives or friends were imprisoned	0.169
Offending behavior among peers: thefts	0.022
Offending behavior among peers: disorder	0.144
Offending behavior among peers: drugs	0.119
Offending behavior among peers: violent assault	0.053
Bullying behavior	0.176
Bullying victimization	0.293

Table 7: Covariate balance: cumulative effects of exposure to police violence

Control Variables	**Standardized Difference between treated and non-treated observations**
Seen or heard of people selling drugs on the streets	-0.251
Seen or heard of people being robbed	-0.203
Seen or heard of people other than police officers carrying guns	-0.033
Listened to gunshots	0.198
Gender: male	0.007
Race: White	-0.172
Type of school: private	-0.121
Personal victimization: assaulted by an adult	-0.062
Family victimization: relatives or friends were robbed	0.198
Family victimization: relatives or friends were murdered	-0.153
Family victimization: relatives or friends were imprisoned	0.229
Offending behavior among peers: thefts	0.082
Offending behavior among peers: disorder	0.033
Offending behavior among peers: drugs	0.096
Offending behavior among peers: violent assault	0.033
Bullying behavior	0.088
Bullying victimization	0.143

Table 8: Longitudinal, multivariate, hybrid Rasch models predicting propensity to criminal offense

Variable	**Model 1**	**Model 2**	**Model 3**	**Model 4**
Within Effects				
Police stops	0.183	0.131	0.096	0.058
	[-0.387, 0.753]	[-0.444, 0.707]	[-0.477, 0.669]	[-0.519, 0.636]
Police arrests	–	0.261	0.237	0.238
	–	[-0.100, 0.623]	[-0.127, 0.600]	[-0.125, 0.602]
Police violence	–	–	0.446*	0.401·
	–	–	[0.032, 0.859]	[-0.014, 0.816]
Legitimacy of the law	–	–	–	-0.322*
	–	–	–	[-0.585, -0.059]
Exposure to violence: drug trafficking	0.075	0.062	0.060	0.050
	[-0.268, 0.418]	[-0.282, 0.406]	[-0.284, 0.403]	[-0.296, 0.395]
Exposure to violence: robberies	-0.158	-0.176	-0.193	-0.213
	[-0.482, 0.166]	[-0.500, 0.149]	[-0.519, 0.133]	[-0.541, 0.115]
Exposure to violence: gun carrying	0.070	0.052	-0.028	-0.005
	[-0.335, 0.475]	[-0.355, 0.459]	[-0.437, 0.380]	[-0.414, 0.404]
Exposure to violence: gunshots	0.015	0.007	0.013	0.019
	[-0.333, 0.364]	[-0.344, 0.358]	[-0.338, 0.363]	[-0.332, 0.370]
Victimization: assaulted	0.086	0.079	0.089	0.087
	[-0.272, 0.444]	[-0.279, 0.438]	[-0.268, 0.447]	[-0.271, 0.445]
Victimization: exposure to fights	0.179	0.209	0.169	0.160
	[-0.362, 0.719]	[-0.334, 0.751]	[-0.377, 0.715]	[-0.386, 0.705]
Family victimization: robberies	0.098	0.109	0.094	0.077
	[-0.251, 0.448]	[-0.241, 0.458]	[-0.257, 0.444]	[-0.272, 0.427]
Family victimization: murder	0.192	0.185	0.145	0.178
	[-0.353, 0.736]	[-0.360, 0.730]	[-0.401, 0.690]	[-0.365, 0.722]
Family victimization: imprisonment	0.258	0.227	0.198	0.234
	[-0.167, 0.682]	[-0.202, 0.655]	[-0.229, 0.625]	[-0.194, 0.661]
Peer offending: theft	0.580**	0.561**	0.549**	0.523*
	[0.165, 0.995]	[0.145, 0.977]	[0.134, 0.964]	[0.106, 0.940]
Peer offending: disorder	0.667***	0.672***	0.687***	0.695***
	[0.307, 1.027]	[0.311, 1.033]	[0.325, 1.049]	[0.334, 1.056]
Peer offending: violent assault	0.218	0.223	0.184	0.158
	[-0.145, 0.582]	[-0.141, 0.588]	[-0.182, 0.550]	[-0.209, 0.525]
Peer offending: drug use	0.498*	0.466*	0.472*	0.426·
	[0.065, 0.931]	[0.029, 0.902]	[0.036, 0.908]	[-0.013, 0.866]
Bullying behavior	0.635***	0.635***	0.640***	0.636***
	[0.446, 0.825]	[0.445, 0.825]	[0.452, 0.829]	[0.447, 0.825]
Bullying victimization	0.108	0.105	0.100	0.080
	[-0.043, 0.260]	[-0.048, 0.257]	[-0.052, 0.251]	[-0.072, 0.232]
Between Effects				
Police stops	-0.051	0.047	-0.015	0.049
	[-0.888, 0.786]	[-0.832, 0.926]	[-0.887, 0.856]	[-0.824, 0.921]
Police arrests	–	-0.197	-0.368	-0.394

	–	[-0.773, 0.379]	[-0.961, 0.224]	[-0.982, 0.194]
Police violence	–	–	0.902**	0.757*
	–	–	[0.254, 1.550]	[0.100, 1.414]
Legitimacy of the law	–	–	–	-0.462*
	–	–	–	[-0.878, -0.046]
Exposure to violence: drug trafficking	0.901*	0.881*	0.819*	0.754*
	[0.155, 1.647]	[0.136, 1.625]	[0.076, 1.561]	[0.012, 1.495]
Exposure to violence: robberies	0.079	0.092	0.040	0.122
	[-0.631, 0.789]	[-0.619, 0.803]	[-0.670, 0.749]	[-0.586, 0.830]
Exposure to violence: gun carrying	0.452	0.485	0.444	0.515
	[-0.398, 1.301]	[-0.369, 1.340]	[-0.406, 1.293]	[-0.331, 1.361]
Exposure to violence: gunshots	0.101	0.122	0.015	0.014
	[-0.650, 0.853]	[-0.629, 0.873]	[-0.736, 0.766]	[-0.733, 0.761]
Victimization: assaulted	0.303	0.338	0.352	0.370
	[-0.261, 0.866]	[-0.226, 0.902]	[-0.210, 0.915]	[-0.190, 0.929]
Victimization: exposure to fights	0.070	0.090	-0.082	0.042
	[-0.911, 1.052]	[-0.887, 1.068]	[-1.065, 0.901]	[-0.932, 1.015]
Family victimization: robberies	-0.170	-0.164	-0.171	-0.162
	[-0.755, 0.415]	[-0.750, 0.422]	[-0.755, 0.413]	[-0.743, 0.419]
Family victimization: murder	0.038	0.117	-0.018	-0.078
	[-0.975, 1.051]	[-0.896, 1.130]	[-1.037, 1.001]	[-1.093, 0.938]
Family victimization: imprisonment	0.690*	0.698*	0.570·	0.600·
	[0.056, 1.325]	[0.062, 1.335]	[-0.071, 1.210]	[-0.039, 1.239]
Peer offending: theft	0.295	0.283	0.317	0.349
	[-0.535, 1.124]	[-0.548, 1.113]	[-0.505, 1.139]	[-0.465, 1.164]
Peer offending: disorder	0.629·	0.591·	0.558·	0.471
	[-0.036, 1.294]	[-0.072, 1.254]	[-0.102, 1.217]	[-0.190, 1.132]
Peer offending: violent assault	1.513***	1.502***	1.538***	1.449***
	[0.800, 2.226]	[0.791, 2.214]	[0.828, 2.248]	[0.742, 2.156]
Peer offending: drug use	0.605	0.642	0.536	0.505
	[-0.162, 1.372]	[-0.123, 1.408]	[-0.227, 1.299]	[-0.254, 1.264]
Bullying behavior	1.150***	1.162***	1.115***	1.043***
	[0.897, 1.403]	[0.908, 1.416]	[0.862, 1.368]	[0.786, 1.301]
Bullying victimization	-0.042	-0.039	-0.033	-0.010
	[-0.255, 0.171]	[-0.252, 0.174]	[-0.244, 0.178]	[-0.221, 0.201]
Gender: Male	0.636***	0.629***	0.575**	0.621***
	[0.287, 0.984]	[0.280, 0.978]	[0.225, 0.925]	[0.270, 0.972]
Race: White	-0.172	-0.174	-0.152	-0.183
	[-0.529, 0.185]	[-0.532, 0.184]	[-0.508, 0.205]	[-0.539, 0.173]
Type of school: private	0.407*	0.397*	0.408*	0.420*
	[0.020, 0.793]	[0.011, 0.783]	[0.023, 0.794]	[0.037, 0.803]
Growth parameters (average)				
Intercept	-7.275***	-7.295***	-7.060***	-7.104***
	[-8.239, -6.312]	[-8.263, -6.326]	[-8.031, -6.088]	[-8.075, -6.132]
Age	-0.215	-0.240	-0.266	-0.211
	[-0.689, 0.259]	[-0.714, 0.233]	[-0.741, 0.208]	[-0.687, 0.265]
Age squared	0.067	0.073	0.075	0.051
	[-0.073, 0.207]	[-0.067, 0.213]	[-0.065, 0.214]	[-0.091, 0.192]
Survey item fixed effects (difficulty parameters)				

Survey item: theft	-1.031***	-1.031***	-1.046***	-1.048***
	[-1.425, -0.638]	[-1.425, -0.637]	[-1.441, -0.650]	[-1.444, -0.652]
Survey item: violent assault	0.736***	0.732***	0.736***	0.739***
	[0.443, 1.028]	[0.439, 1.025]	[0.443, 1.029]	[0.445, 1.032]
Survey item: drug use	-0.955***	-0.962***	-0.958***	-0.964***
	[-1.34; -0.569]	[-1.35; -0.575]	[-1.34; -0.571]	[-1.35; -0.576]
Model fit				
N (observations)	11904	11904	11904	11904
N (respondents)	792	792	792	792
Standard deviation (Intercept)	0.637	0.595	0.619	0.615
Standard deviation (Age)	0.220	0.239	0.210	0.202
Intraclass correlation	0.11	0.10	0.10	0.10
Pseudo R2 (total)	0.55	0.55	0.55	0.55
BIC	2618.81	2634.94	2641.10	2649.15

Note: Coefficients are presented with 95% confidence intervals in brackets. *p<0.05, **p<0.01.

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