The rational adolescent:

Discipline policies, lawsuits, and skill acquisition

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Abstract
The paper estimates the response of student truancy and long-run labor market outcomes to discipline policies in middle and secondary school. Simultaneous determination of student behaviors and school policies motivates an instrumental variables strategy. Because judicial climate influences administrators' fear of discipline-related lawsuits, measures of judicial-legal climate at the state level court are used as instruments for local discipline policies. Results indicate that the state-level judicial/legal climate does appear to influence administrators' discipline policies; that students appear to be truant less often when discipline is stricter; and that school and long-run labor participation outcomes appear higher for students from schools with stricter discipline policies. (JEL classification J22, J24).

1. Introduction

How does school discipline influence education outcomes? Discipline in schools has been a policy concern for many years, but received particular attention in The National Education Goals. These posit a link between disciplined learning environments and student learning, however, evidence of such a link has tended to rely on anecdotal evidence or small-scale studies in which long-run outcomes associated with student learning (such as finishing high school) have not been observed. Moreover, a fundamental difficulty has been that school policies and student behavior choices are simultaneously determined. Students alter behavior in response to discipline policies, but schools also alter discipline policies in response to student behaviors. Thus, any observed correlation between discipline policies and student outcomes may be explained by either channel.

This paper attempts to fill a gap in the literature by examining the relationship between discipline policies and student outcomes, including truancy, high school completion, and labor market participation after high school. In addition, the analysis addresses the fundamental simultaneity problem by using appellate court climate and per capita density of public interest lawyers as instruments for discipline policies. I will argue that the judicial-legal climate in a state influences school policies but (conditional on an extensive set of control variables) does not influence student behavior except through these policies. Given these two conditions, it is possible to identify the effect of discipline policies on drop-out rates, truancy, and other student outcomes. Main empirical findings include the following: 1) State-level judicial/legal climate appears to influence administrators; 2) Students appear to cut class less often when discipline policies are

stricter; and 3) High school graduation and labor participation outcomes appear higher for students who attended schools with stricter discipline policies.

The remainder of the paper is organized as follows: Section 2 describes previous research; section 3 lays out the empirical strategy; section 4 analyzes the results of the empirical analysis; section 5 concludes.

2. Previous research

2.1 Discipline and student outcomes

In theory, stricter discipline policies could raise or lower student learning. If discipline policies alter rewards and penalties in the short run, then they may alter behavior choices, and through them, influence long-run education outcomes. There exists some evidence of a strong response by student to short-run education incentives, such as monetary pay for academic achievement (Angrist and Lavy, 2003), but whether the results generalize to the short-run incentives associated with discipline policies is unclear. There is also concern that external incentives could erode internal incentives and reduce effort and performance in the long run, even if behavior and/or effort improve in the short run.[1] Ultimately, then, discerning the effect of discipline policies on student outcomes is an empirical matter, and one that requires an analysis of long-run outcomes.

The National Educations Goals, created in 1994, included the establishment by the year 2000 of "safe, disciplined, and alcohol-and-drug-free schools." Barton Coley, and Welingsky (1998) note that though The National Education Goals posit a link between disciplined learning environments and student learning, evidence supporting such a link has been for the most part based on case studies or small-scale evaluations of

schools or districts. In their survey of the literature on discipline policies and student outcomes, the authors highlight the Safe School Study of 1978 as the exception, a large-scale study of the issue. The study found that discipline policies perceived as fair by students and teachers were associated with lower levels of victimization, but the analysis did not include academic achievement or truancy as outcome variables. Using NELS88, Barton et al. (1998) find stricter discipline policies in tenth grade to be associated with lower rates of delinquency in twelfth grade. The present analysis differs from previous research both in the long-run outcome variables examined (high school graduation and labor market participation) and in the instrumental variables strategy used to address the simultaneity problem.[2]

2.2 Lawsuits and discipline policies

Interaction between schools and the judiciary suggests a set of plausible instruments for discipline policies. The threat of lawsuits is a major concern for the administrators who create and enforce school behavior policies—a threat that varies in seriousness from state to state, depending on the climate of the appellate courts that set judicial precedents. According to the Baltimore Sun (March 23, 2003), teachers have been leaving the Baltimore County school system "because they grew frustrated with superiors who wouldn't discipline students who cursed in class, cheated on research projects and hit classmates." The administrators would not back up teachers because of a fear of lawsuits. Similar accounts have been published in the Los Angeles Times, the St. Petersburg Times (of Florida), and the Associated Press. Newsweek ran a cover story on the topic in December 2003 contending that "legal fear" among education professionals is

a national phenomenon. Alan Bersin, superintendent of San Diego City Schools, quoted in Newsweek, calls the risk of being sued "'the anaconda in the chandelier'—it hangs overhead, threatening to strike at any time."

Beyond anecdotes in the popular press, there exists systematic evidence that schools and students have been influenced by case law. Arum, Beattie, Pitt, Thompson, and Way, (2003) examine 6,277 court cases that reached state and federal appellate courts between 1946 and 1992, identifying 1,204 cases that contest a school's right to discipline and control students. They argue that the involvement of courts in overturning school disciplinary policies altered the motivations of administrators and the expectations of students and families. The authors construct an index of court climate based on the "student-friendliness" of state and federal appellate courts. The student-friendliness measure is the relative frequency with which the court ruled in favor of students in discipline-related lawsuits. They go on to identify variation in court climate over time and—more important for the present purpose—across regions and jurisdictions. Student-friendliness of appellate courts, they find, correlates with decreases in strictness of discipline. (They use several measures of school discipline, including the existence of corporal punishment, and teacher and student perceptions of strictness). Evidence indicates that schools in more student-friendly judicial environments have discipline policies that are less strict. This paper will argue that the variation in discipline policies due to judicial environment is exogenous to student behavior and outcomes (conditioned on demographic controls). I will use student-friendliness of court climate—the relative frequency with which state and regional appellate courts sided with students in discipline lawsuits between 1960 and 1992—to instrument for discipline policies.

The history of student rights litigation and case law motivates a second instrument. Large increases in school discipline cases in appellate courts took place after 1969 (from about 8 cases a year in 1960-1968 to an average of 76 cases a year between 1969 and 1975). Arum et al. identify and document carefully what appears to have been a major cause of the increase: Public interest law firms began to use school discipline lawsuits as a means of education reform. Without support from non-profit lawyers, it had been difficult for students or their families to bring lawsuits and to persist until the lawsuits reached the appellate level. Student lawsuits appear to have been made possible, in large part, by third parties engaged in reform advocacy.[3] This paper takes no stand on the merit of the reforms. In addition to whatever may have been accomplished with respect to the protection of a student's rights, the reforms appear to have had a number of unintended consequences. These relate to the present inquiry. If public interest law firms supported and sustained student lawsuits, then schools in states with a high density of public interest lawyers may have had greater reason to fear litigation. The density of public interest lawyers at the state level will be used as a second instrument.

3. Empirical strategy

The Add Health survey, conducted by the Carolina Population Center from 1994 to 2002, contains data on adolescents in 132 schools across the country, grades 7-12. The in-school portion of the Wave 1 survey, conducted in 1994-1995, contains cross-section data on about 90,000 adolescents. The students filled out the main questionnaires. These will be the source of the truancy measure. In addition, school administrators filled out questionnaires describing characteristics of the schools in the sample. These provide the

source for the behavior policy measure. A subset of the initial sample, about 20,000 subjects, was selected for the in-home portion of Wave I in 1994-1995. Because parents were also interviewed in this smaller sample (and because students were interviewed more extensively) a large set of family and neighborhood controls is available. The in-home subjects were interviewed again in 1995-1996 (Wave II), and a final time in 2001-2002 (Wave III). The Wave III sample, 7 years after the initial surveys, will be the source of long-run employment and education outcomes. When appropriate weights and cluster coefficients are used, regressions on data from each of the surveys, or from merged samples, yield results representative of the U.S. population.[4]

The measure of truancy is the frequency with which the student skipped class without an excuse. Seven discrete responses were options on the In-school questionnaire survey: never, once or twice a year, once a month or less, 2 or 3 days a month, once or twice a week, 3 to 5 days a week, nearly every day. The school administrator answered questions about what the usual punishment would be for a variety of disruptive behaviors: cheating, bringing alcohol or drugs to school, disruption, abusing teachers, fighting. Punishments varied in strictness from verbal reprimands to permanent expulsion. There were 24 dimensions of behavior in the questions of Add Health school administrator survey. In the questionnaire, allowed answers were restricted to the 5 discrete options displayed in Table 1A. The Discipline Policy Index (DPI), which I construct for each school, is the average of these 24 responses.[5] The Discipline Policy Index is conceived as a proxy for a range of unobserved administrator policies and attitudes. Table 1, in panels B and C, shows descriptive statistics for the DPI and its component policies.

[Table 1 here]

The model underlying the ordered probit for truancy is:

$$T_{ij}^* = \beta_1 x_{ij} + \beta_2 s_j \beta_3 d_j + \varepsilon_{ij} \qquad (1)$$

Here, T_{ij}^* is the latent variable from which the truancy choice is derived (where truancy is measured by the 7 discrete levels of cutting class described in the preceding), outcomes x_{ij} are student characteristics, s_j are school characteristics, d_j is the Discipline Index, and ε_{ij}, the standard probit error term, is such that $(\varepsilon_{ij} \mid x_{ij}, s_j, d_j) \sim N(0, 1)$. The regressor of interest then is β_3.

From a policy perspective, a second and perhaps more meaningful goal would be to determine whether future economic outcomes are influenced by discipline policy during middle or secondary school:

$$y_{ij}^* = a_1 x_{ij} + a_2 s_j + a_3 d_j + \varepsilon_{ij}, \qquad (2)$$

Here, the explanatory variables are the same as before, and y_{ij}^* is the latent variable for an observed binary outcome in the Wave III survey in 2001-2002, 7 years after the first interview. The specific long-run outcomes I will study are high school graduation and labor market participation.

It will not be assumed that long-run incentives have no influence on adolescent choice. Truancy and long-run skill-acquisition outcomes may depend on the wage premium for skilled labor, as predicted by standard economic models (e.g., Ben-Porath, 1967). One concern is that behavior policies in a school could be correlated with the skilled wage premium in the geographical area. DPI would then be a proxy for the return to education. To account for this possibility, and to allow for comparisons between the

effects of short-run factors and long-run factors, the regressions will include state-level skilled wage premia as control variables.

Validity of the instruments is a primary concern. Are court attitudes and the number of non-profit lawyers at the state-level uncorrelated with local factors that influence student behavior outcomes (after conditioning on demographic variables)? It could be that unobserved characteristics of the local population influence court climate at the state level and student outcomes, introducing bias in the IV estimates. Arum et al. argue that courts have been relatively autonomous from locally defined political cultures, serving as "one of the institutional mechanisms whereby larger political and social pressures" filtered down to local schools and were brought to bear on administrators and students. Indeed, strategies that work through the judiciary offered a distinct advantage for reformers precisely because local preferences could be superseded. The history of racial integration of schools in the south, for example, suggests that courts can and do institute reforms that differ from local preferences.

Figure 1 displays levels of court climate and per capita public interest lawyers, by state, on a map of the U. S. It is possible that judicial-legal climate proxies for other state-level characteristics that influence skill acquisition. Because there are only 35 states in the sample, including a large set of state-level control variables in the main regressions is problematic. Supporting regressions, available upon request, show that states with **higher** per capita income have more student friendly courts and higher density of public interest lawyers, on average. Resulting bias then appears to go in a direction that makes the magnitudes of the estimates in section 4 lower bounds: Students in less-strict schools (to the extent that these are predicted by judicial-legal climate) have **lower** skill-related

outcomes in spite of residing in relatively more advantaged states. Arguably, then, the most obvious concerns about the validity of the instruments would imply, if anything, that the IV strategy underestimates the magnitude of the effect of discipline policies on student outcomes. It remains possible, of course, that state-level judicial-legal climate shaped student culture in some other way. Some of these possibilities will be discussed in Section 4.

[Figure 1 here]

An IV ordered probit was used to estimate the two-stage analog of specification (1), and an IV probit, for the two-stage version of specification (2) with dichotomous outcomes. In the Add Health Survey, observations are weighted by probability and errors should be clustered at the school level to allow for arbitrary within-school correlation of unobserved student characteristics. Estimates were corrected to account for these design elements. State-level instruments are used in two-stage probit and two-stage ordered probit estimates. Consequently, errors were clustered at the state level in these regressions.

4 Results

4.1 Truancy

As a first pass at the data, Table 2 shows estimated coefficients from the ordered probit and IV ordered probit regressions based on specification (1). In the full sample, the Discipline Policy index is negatively correlated with truancy, significant at the 5% level. Students who skip class less often are associated with stricter schools. The regressions include controls for skilled wage premia. The 2000 census was used to construct the

state-level controls “H.S. (Mincer)” and “B.A. (Mincer).” These are Mincer coefficients on the dummy variables for high school graduation and 4-year college graduation, respectively, in a regression of log wages on these two dummy variables, age, age squared, race, sex, and marital status. The H.S. control captures the percentage increase in wages associated with completing high school in a given state (relative to the wage for not completing high school), and the B.A. control captures the additional percentage increase in wages associated with acquiring a 4-year degree. Signs and magnitudes of the wage premia coefficients will be discussed in more detail in the next set of regressions.

[Table 2 here]

Coefficients on the other covariates have the expected signs. Older students cut class more often. Students from more educated families skip class less often, schools with larger class sizes and inexperienced teachers are associated with higher probability of truancy, the percentage of teachers with master’s degrees does not influence truancy significantly, and students skip class more often when they feel unsafe. Columns 3-6 break down the regressions by gender. For males, the coefficients on DPI are larger in absolute value than for females. This finding recurs for the dependent variables reported in subsequent sections, across specifications and sample sizes. Subsequent regressions will be restricted to males, as their responses are more clearly distinguishable from the null.

The Add Health Wave I In-home sample contains a detailed set of family and neighborhood variables. Table 3 shows the descriptive statistics of variables that will be

used to capture characteristics that could be a source of correlation between the DPI and the unobserved error term. Variables describing income distribution, population, crime, marital status, family structure, and other relevant neighborhood characteristics will be included in the next set of regressions. Some of the variables are available at the census tract level, others at the county level only. In addition, the student's score on the Add Health Vocabulary Test and a more detailed set of family descriptive variables (also shown in Table 3) will be added to the list of right-hand variables. The descriptive variables come at a cost. They are available only for the In-home survey, so sample size is sacrificed.

[Table 3 here]

[Table 4 here]

Column 1 of Table 4 shows the baseline regression from Table 2. Column 2 shows the same regression calculated from the smaller In-home sample, without any additional controls. Column 3 estimates the model with added family-descriptive variables. Column 4 shows the results when the full set of neighborhood-descriptive variables is included, as well. Adding covariates appears to reduce the magnitude of the coefficient on DPI, though the estimate remains significant at the 5% level.

In columns 5 through 8, court climate and public-interest lawyers instrument for DPI in IV ordered probit analogs to columns 1 through 4. The DPI coefficients in the IV regressions do not move toward zero as controls are added. Interestingly, the IV estimates of the DPI coefficient are larger in absolute value than the estimates in the reduced form regressions. DPI may be a noisy measure of school policy. If the instruments are correlated with the informative portion of DPI variance in the sample, but not with the

noise, then the estimates in the reduced form will be biased toward zero. In such a case, IV estimates may be larger in absolute value—as here. Also, if administrators in schools with high truancy respond by tightening discipline, then the reduced form estimates will be biased upward toward zero—and again, one might expect to see IV estimates more negative than reduced form estimates.

Marginal effects of changes in DPI on truancy have been calculated for each category of truancy choice. Weighted averages of the marginal effects (for all individuals in the sample) are displayed in Table 4. For all specifications, a 1 point increase in DPI shifts probability mass to choice 0—not skipping class at all—and away from all other categories. The probability of every non-zero truancy choice goes down, whether students have been skipping class often, or whether they have been doing so infrequently. To interpret the marginal effects, one could imagine a uniform change in all 24 behavior policies. For example, if punishments for each of the 24 infractions were to be raised in severity by one category (minor punishment to in school suspension, in school suspension to out of school suspension, and so on), this would be associated (in column 8) with a 16.5 percentage point increase in the probability that a student would not skip class, and respective decreases of 6.1, 2.8, 2.9, 1.7, .7, and 2.3 percentage points in the probabilities of skipping class once or twice a year, once a month or less, 2 or 3 days a month, once or twice a week, 3 to 5 days a week, and nearly every day. Alternatively, one could imagine an exogenous change of one-standard deviation in the strictness of the school's behavior policy. The standard deviation of DPI is .31. The marginal effects in column 8, then, indicate that an increase in DPI of one standard deviation is associated

with an increase of 5.1 percentage points in the probability of not skipping class at all, and decreases of 1.9, .9, .9, .5, .2 and .7 percentage points in the higher truancy categories.

Coefficients on skilled wage premia, by contrast, vary in sign and are not statistically significant. Even if one were to take the point estimates at face value, the marginal effects seem small. In the preferred specification (column 8), a change in the high school wage premium of 100 percentage points is associated with a decrease of 6.4 percentage points in the probability of not skipping class. It would take an enormous change in the high school wage premium—about 80 percentage points—to duplicate the effect of a one-standard deviation change in DPI on the probability of not skipping class.

The first stages of the IV regressions show that the instruments have highly significant t-stats (and F-stats) in the expected direction for all specifications: Student-friendly courts and a high density of public-interest lawyers are associated with schools whose discipline practices are relatively less strict. Scatterplots in Figures 2A and 2B show a visible negative correlation between DPI and each of the instruments in the raw data. The excellent fit of the first-stage regressions may be of interest in itself. Have institutional changes arising from student rights litigation led to a weakening of school discipline? The first stage regressions support, and add to, the body of evidence presented by Arum et al. Their data allowed for a time series analysis that is not possible here; however, their inquiry focused largely on corporal punishment and perceptions of strictness. Here, the DPI derives from 24 discipline practices, and may be more nuanced than a measure based on corporal punishment alone. Moreover, if one worries about the subjective nature of perceptions of strictness, then evidence about specific practices, rather than perceptions, may offer advantages.

[Figure 2 here]

Consistent with Barton et al, findings indicate that for the range of discipline policies in place at sampled schools, strictness of discipline is negatively associated with truancy. The negative correlation is robust to the inclusion of family and neighborhood controls that characterize income distribution, public assistance, crime, population, race, family structure, and a number of other socioeconomic traits. When instruments are used, the relationship persists and the absolute value of the coefficient rises. Long-run incentives, as measured by state-level wage premia for skilled labor, do not appear to drive the correlation or to explain effort choices perceptibly.

4.2 Long-run outcomes.

The Wave III survey contains data on outcomes 7 years after the initial adolescent surveys. Specification (2) offers a second framework for drawing inferences about the relationship between discipline policies and effort choices. If discipline policies influence behaviors, and through them, the acquisition of skills, one would expect to observe a correlation between strictness of discipline and future education-related outcomes.

In Table 5, high school graduation and labor force participation outcomes for Wave III males 7 years after the original Wave I survey (when agents were in grades 7 through 12) are regressed on DPI and the full set of control variables. The respondents' ages in Wave III are 20-25 years. High school graduation appears positively associated with DPI in both specifications, and significant in the IV specification. In Columns 3 and 4, agents are categorized as "Employed" if they are working at least 20 hours per week or still attending school.[6] The coefficients are positive and significant in these regressions,

as well. Attending a school with stricter discipline is associated with a higher probability of having graduated from high school and a higher probability of being employed (or in school), 7 years later. If disruptive behavior occurs more often in schools serving unobservably bad neighborhoods, and if administrators respond to increased disruption by increasing penalties for disruption, then the OLS estimates are biased downward. Consistent with such an interpretation, IV probit estimates in column 2 and 4 exceed their reduced form counterparts.

[Table 5here]

The wage premium for high school educated workers does appear to have some predictive power for high school graduation, and the coefficient on wage premium for college educated workers is significant in the employment regressions. DPI does not, however, appear to be a proxy for state-specific return to skill. It could be that agents are mobile between states at low cost. Census regions might capture the relevant wage premia more accurately, as mobility between large regions could be more costly. Wage premia were calculated for the 9 Census regions and used instead of state-level wage premia in these and the previous regressions. Results were qualitatively similar.[7]

Standard deviation in DPI is .31. Thus, thus if penalties for 8 of the 24 infractions were to be raised in severity by one category (minor punishment to in school suspension, in school suspension to out of school suspension, and so on), this would constitute an increase in DPI of about one standard deviation. Assuming the IV estimates identify causal effects, increasing DPI by one standard deviation would increase high school graduation by 4 percentage points and employment seven years later by 3.7 percentage points. Given a graduation proportion of .79 with standard deviation .41, the effect size

on high school graduation is about .10 standard deviations. If policy driven, the effects above would seem large enough to be of interest to parents, policy-makers, and the median voter.

It could be that when courts offer more protections to students accused of misbehavior, they tend to do the same for criminals. States with stricter schools would feature harsher penalties for criminals and less restrictive forms of law enforcement. Education choices and labor participation in these states could be influenced by state-level determinants of crime prevention. State-level measures of court climate would then be correlated with the residuals in the IV regressions. To account for this possibility, I included the state-level crime rate from the FBI's Uniform Crime Report as a right-hand variable. It could also be that state-level minimum wage laws are correlated with the legal climate in the state and long-run skill-related outcomes. I added state-level minimum wage as a regressor to account for this possibility. Neither addition altered the results significantly. Results displayed in Table 5 were calculated with both crime and minimum wage covariates included.

5. Summary and conclusion

The paper estimated the response of student truancy, high school completion and labor market participation outcomes to discipline policies in middle and secondary school. Results included several main findings: Schools appear to be influenced by the state-level legal-judicial climate with respect to student rights; students appear to engage in less truancy when discipline policies are stricter; and high school graduation and labor

participation outcomes appear higher for students who attended schools with stricter discipline policies.

A number of factors ignored in this analysis merit further investigation. Though the findings suggest a link between discipline policies and student outcomes, they do not clearly or conclusively illuminate the mechanism. The lack of a strong response to variation in long-run wage incentives combined with some evidence of response to short-run incentives suggests that students may be myopic, but additional evidence is needed. Secondly, reductions in adolescent behavior problems generate positive externalities in the education production process. One reason long-run outcomes might respond to small changes in discipline policy is that effects may be amplified by spillovers associated with a social multiplier. It would be informative to model strategic interactions by students, and to test the predictions empirically using social network data in the Add Health database. Thirdly, because "zero-tolerance" policies had not been implemented at the time of the Wave I surveys, the analysis above does not speak in a direct way to the zero-tolerance debate.[8] Extensions related to this policy may merit further investigation. Lastly, findings here are relevant to research on Catholic schools and their apparent effectiveness in disadvantaged communities. Though the number of Catholic schools in the Add Health dataset is too small for such an analysis, findings suggest that discipline policies may have explanatory power in this setting. These are subjects for future research.

Notes

[1] For a discussion of external versus internal incentives, see Kremer, Miguel, Thornton (2003).

[2] Barton et al. (1998) examine delinquency outcomes two years removed from the discipline policy, but the analysis may still suffer simultaneity bias if choices, habits, skills acquired, or behaviors that led to later delinquency were contemporaneous with the discipline policy.

[3] For example, Goss vs. Lopez, decided by the U.S. Supreme Court in 1975, was brought by OEO Legal Services and the Center for Law and Education at Harvard. It included "friends of the court" briefs from the ACLU, the NAACP, and the Children's Defense Fund. (See Arum et al.)

[4] See Chantala and Tabor, 1999.

[5] A simple average is an arbitrary weighting of DPI component measures. Factor analysis yields an alternative weighting. The sign on DPI coefficients estimated in Section IV is robust to an index that uses weightings derived through common factor analysis, but the estimates are less precise. Possibly, some components of DPI have unique information, uncorrelated with other components of the index, that should not be discarded.

[6] In most cases, students still in "school" were in college.

[7] The 2000 Census was used to compute wage premia. In standard models, agents forecast the return to skill and make investment choices based on their estimates, so the 2000 Census would be appropriate (as Wave 3 surveys were conducted in 2001.) Wage premia from the 1990 census were used as an additional check in the above regressions, with similar results.

[8] These policies started with the Gun-Free Schools Act of 1994. States were required to have in effect by October, 1995 a law mandating expulsion from school for gun possession.

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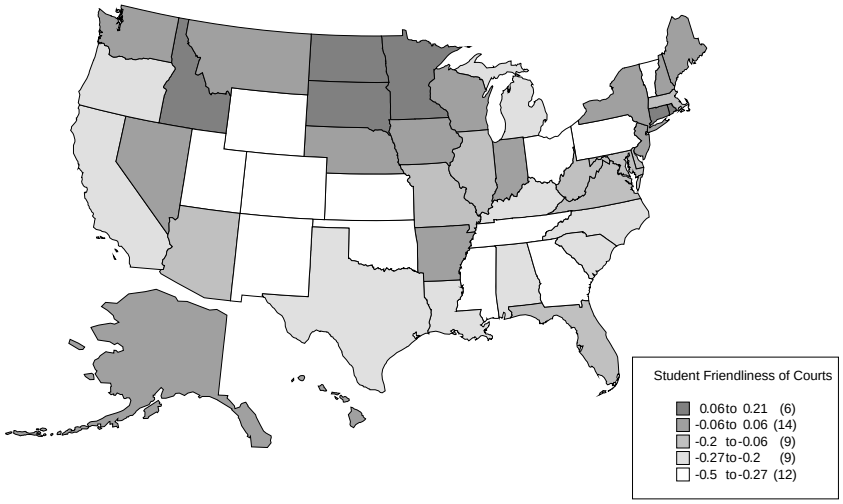
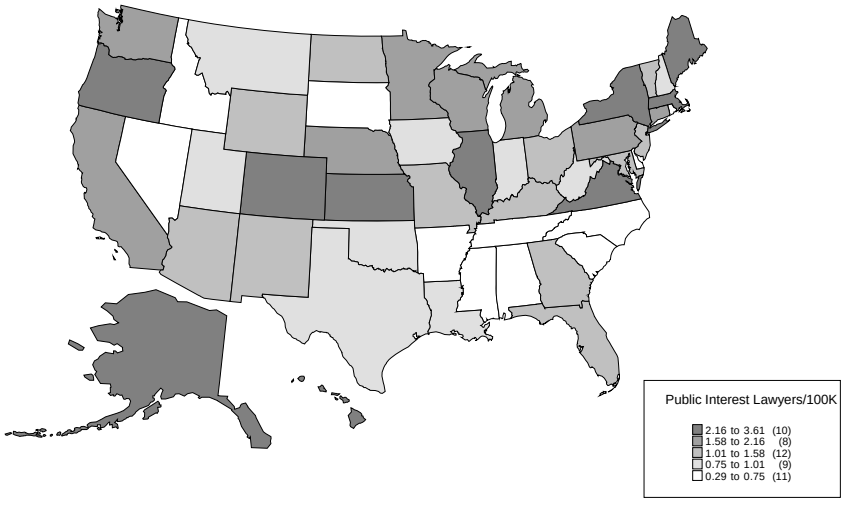
Figure 1

A. Court climate*

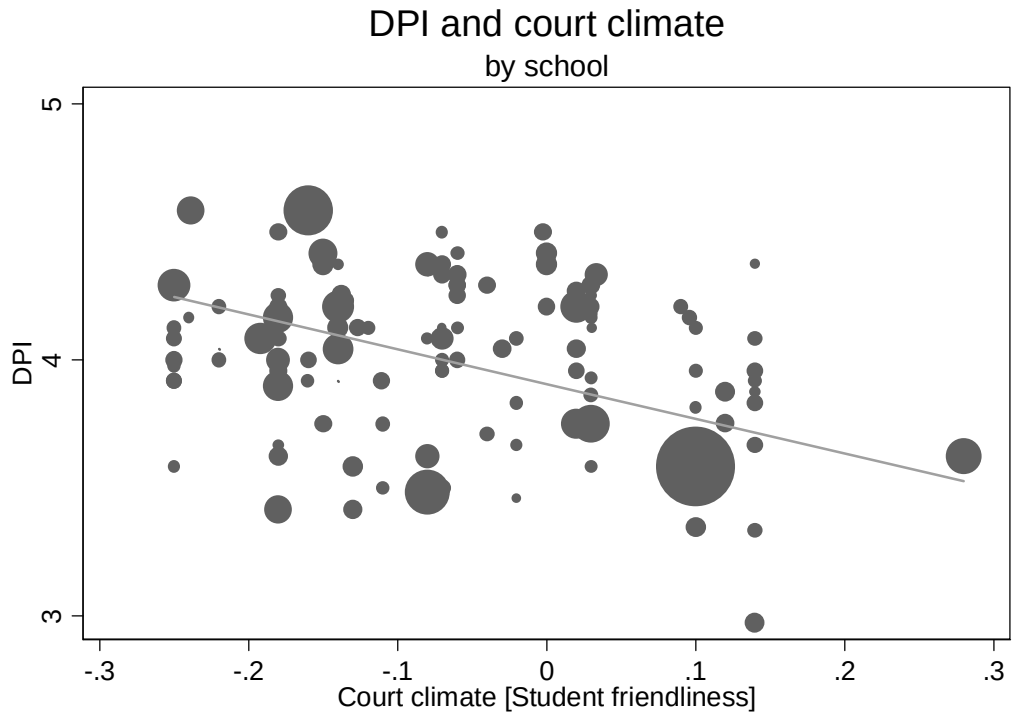


B. Lawyers



***More positive numbers for Court climate index imply greater student friendliness.**

Figure 2
A.


DPI and court climate
by school
DPI
5
4
3
-.3
-.2
-.1
0
.1
.2
.3
Court climate [Student friendliness]

B.

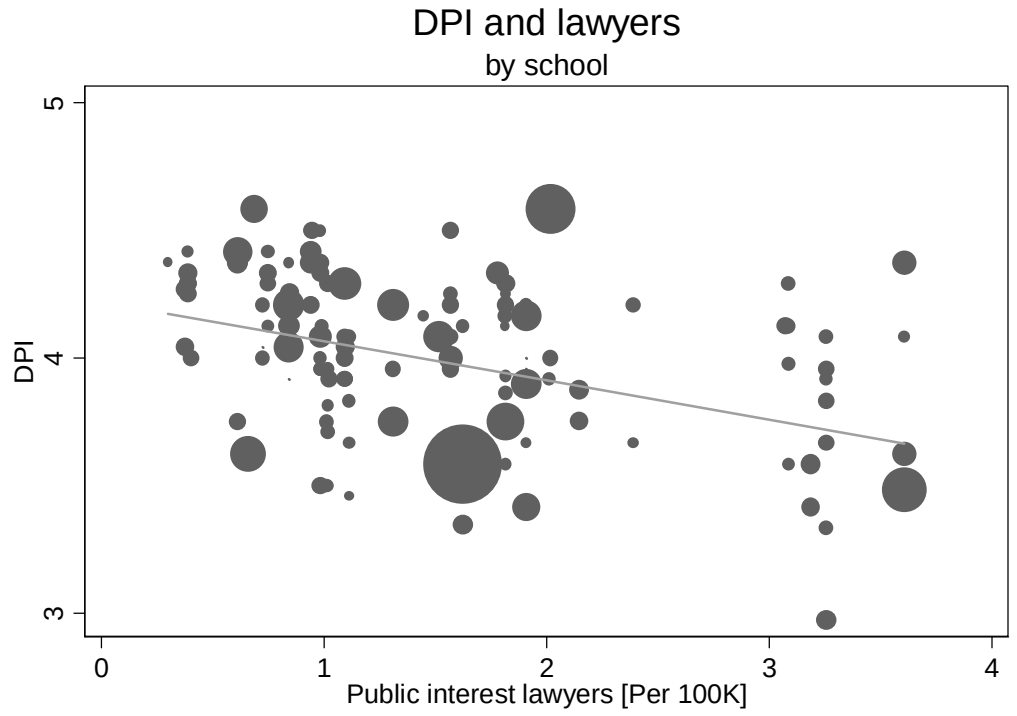

DPI and lawyers
by school
DPI
5
4
3
0
1
2
3
4
Public interest lawyers [Per 100K]

Table 1
Construction and descriptive statistics for discipline policy index

A. Allowed discrete responses to the question: What is the typical punishment imposed for [specific behavior infraction]?

0	No policy
1	Verbal warning
2	Minor action
3	In-school suspension
4	Out-of-school suspension
5	Expulsion

B. Discipline policy index

Mean : 4.00 StDv: 0.31 Min: 2.97 Max 4.63

C. Component policies

UUDescription	Mean	StDv	Min	Max
Penalty for cheating, 1st occ.	1.93	0.87	0	4
Penalty for cheating, 2nd occ.	2.64	1.03	0	5
Penalty for fighting, 1st occ.	3.58	0.71	1	5
Penalty for fighting, 2nd occ.	3.98	0.56	2	5
Penalty for injuring student, 1st occ.	3.63	0.75	1	5
Penalty for injuring student, 2nd occ.	4.20	0.65	2	5
Penalty for possess alcohol, 1st occ.	3.97	0.59	2	5
Penalty for possess alcohol, 2nd occ.	4.39	0.62	2	5
Penalty for possess drug, 1st occ.	4.24	0.56	2	5
Penalty for possess drug, 2nd occ.	4.69	0.48	3	5
Penalty for possess weapon, 1st occ.	4.71	0.47	3	5
Penalty for possess weapon, 2nd occ.	4.91	0.28	4	5
Penalty for drink alcohol, 1st occ.	4.06	0.56	2	5
Penalty for drink alcohol, 2nd occ.	4.52	0.57	2	5
Penalty for use drug, 1st occ.	4.29	0.53	3	5
Penalty for use drug, 2nd occ.	4.70	0.47	3	5
Penalty for smoking, 1st occ.	3.09	0.89	1	5
Penalty for smoking, 2nd occ.	3.73	0.71	1	5
Penalty for verbal abuse teacher, 1st	3.21	0.92	0	5
Penalty for verbal abuse teacher, 2nd	3.97	0.82	0	5
Penalty for injure teacher, 1st occ.	4.65	0.52	3	5
Penalty for injure teacher, 2nd occ.	4.88	0.34	3	5
Penalty for steal sch. prop., 1st occ.	3.68	0.71	0	5
Penalty for steal sch. prop., 2nd occ.	4.33	0.69	0	5

Table 2
Dependent variable: Days of class skipped w/o excuse

	All (O-Prob) 1	All (IV O-Prob) 2	Boys (O-Prob) 3	Boys (IV O-Prob) 4	Girls (O-Prob) 5	Girls (IV O-Prob) 6
DPI	-.212**	-.48	-.268***	-.555*	-.145	-.348
	(.0841)	(.298)	(.083)	(.299)	(.0965)	(.282)
H.S. (Mincer)	.777	.748	.342	.325	1.28**	1.24***
	(.504)	(.466)	(.526)	(.515)	(.563)	(.446)
B.A. (Mincer)	-1.78**	-1.65*	-1.37	-1.25	-2.22**	-2.13***
	(.823)	(.858)	(.834)	(.941)	(.912)	(.816)
Age	.17***	.17***	.185***	.185***	.122***	.121***
	(.0138)	(.0156)	(.0164)	(.0182)	(.0193)	(.0211)
Mother's education	-.0329***	-.0332***	-.0274***	-.0275***	-.0415***	-.042***
	(.00415)	(.0044)	(.00467)	(.00543)	(.00703)	(.00592)
Mother employed	.0639***	.0605**	.0223	.0184	.102***	.0998***
	(.0231)	(.0246)	(.0292)	(.0333)	(.0269)	(.0311)
Class size	.016***	.0158***	.012**	.0118**	.0201***	.0199***
	(.00527)	(.00539)	(.00508)	(.00499)	(.0061)	(.00651)
% New teachers	.00253**	.00293	.00195**	.00241	.00332**	.0036*
	(.00105)	(.00197)	(.000931)	(.00197)	(.00127)	(.00196)
% Teachers M.A.	.000754	.000604	.000213	.000126	.00139	.00122
	(.00116)	(.00150)	(.00102)	(.00142)	(.00143)	(.00162)
% PTA participation	-.00101	-.000664	-.00043	.000000	-.00184	-.00163
	(.00105)	(.00121)	(.001)	(.00126)	(.0013)	(.00136)
% Students@grade level	.0059**	.00703**	.00738***	.00852***	.00404*	.00499
	(.00247)	(.00324)	(.0027)	(.00327)	(.00244)	(.00332)
% Students below grade	.00439	.00576	.00718*	.00853	.000898	.00212
	(.00385)	(.00536)	(.00415)	(.00547)	(.00372)	(.00543)
"Unsafe" neighborhood	.0774***	.0756***	.0871***	.0854***	.0738***	.0724***
	(.00819)	(.00808)	(.0105)	(.0115)	(.013)	(.0118)
"Unsafe" school	.104***	.107***	.111***	.113***	.0959***	.0984***
	(.00947)	(.00937)	(.0119)	(.013)	(.0119)	(.0121)
Obs	76363	76363	37767	37767	38596	38596

Based on AddHealth In-school Wave I, 1994-1995. Grade and race dummies, dummy variables for drug and alcohol programs, state-level crime and minimum wage variables, and dummy variables for missing data are included as covariates in these and subsequent regressions

* significant at 10% level
** significant at 5 % level
*** significant at 1% level

Table 3
Individual, family, and neighborhood contextual variables

In-school (Baseline controls)*:	Mean	Std. Dv.
Age	14.86	1.78
Race (black)	.20	.40
Race (asian)	.05	.22
Race (amer ind)	.06	.24
Race (other)	.08	.27
Mother's education	13.83	2.41
Mother works	.82	.39
Ave class size	25.48	5.04
New teachers (%)	9.91	14.73
Teachers with M.A. or higher (%)	50.93	24.97
Families in PTA(%)	23.45	22.25
Students testing @ grade level(%)	58.63	23.88
% testing >one grade below	20.71	14.85
Student feels safe in neighborhood	2.01	1.07
Student feels safe at school	2.26	1.10
Family Variables:		
Parent married	.73	.44
Receives public assistance	.09	.29
Household income (1000s)	45.74	45.17
Receives AFDC	.07	.26
Receives food stamps	.12	.33
Add Health vocabulary test	101.32	14.81
Census tract variables		
Black(%)	.14	.25
Asian(%)	.03	.08
Other(%)	.04	.09
Aged 5-17 non english speaking(%)	.02	.04
Fem HH w/chld, no husb pres (%)	.07	.05
Marr-cpl fam w/chld(%)	.29	.09
Children not living w/both pars (%)	.25	.17
HHs w/ income < $15,000(%)	.26	.15
Median family income(1000s)	33.95	13.39
Age 25+ w/ college degree(%)	.23	.13
Unemployment rate	.08	.05
County level variables		
Total serious crimes/100,000	5666.82	2657.54
Juvenile serious crimes/100,000	345.50	168.44
State level controls		
Wage Premium: HS Grad	.23	.06
Wage Premium: B.A.	.44	.04
Minimum wage	4.28	.15
Crime rate	5267.38	1146.97

*Controls also include grade dummies and indicator variables for schools with drug and alcohol counseling/abuse/awareness programs.

Table 4

Dependent variable: Days of class skipped w/o excuse[a,b]

	Ordered probit				IV ordered probit			
	Large	In-home Sample			Large	In-home		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DPI	-.268***	-.235***	-.221***	-.184**	-.555*	-.496*	-.504*	-.55*
	(.0830)	(.0807)	(.0813)	(.0877)	(.299)	(.293)	(.280)	(.329)
Mfx (0)	.0839	.0717	.0668	.0553	`	.1516	.1523	.1654
Mfx (1)	-0.02817	-.0266	-.0248	-.0205	-0.0583	-.0563	-.0565	-.0614
Mfx (2)	-0.01314	-.0122	-.0114	-.0094	-0.02724	-.0259	-.026	-.0282
Mfx (3)	-0.01558	-.0127	-.0119	-.0098	-0.03231	-.027	-.0271	-.0294
Mfx (4)	-0.00958	-.0073	-.0068	-.0057	-0.01986	-.0155	-.0156	-.0169
Mfx (5)	-0.0045	-.0029	-.0027	-.0022	-0.00932	-.006	-.006	-.0066
Mfx (6)	-0.01298	-.01	-.0093	-.0077	-0.02688	-.021	-.0211	-.0229
H.S. (Mincer)	.342	-.105	-.329	-.163	.325	-.176	-.40	-.253
	(.526)	(.594)	(.603)	(.674)	(.515)	(.626)	(.660)	(.668)
Mfx (0)	-.1071	.0322	.0993	.0489	-.1019	.0538	.1208	.076
Mfx (1)	.0359	-.012	-.0369	-.0182	.0342	-.02	-.0448	-.0282
Mfx (2)	.0168	-.0055	-.0169	-.0083	.016	-.0092	-.0206	-.0129
Mfx (3)	.0199	-.0057	-.0177	-.0087	.0189	-.0096	-.0215	-.0135
Mfx (4)	.0122	-.0033	-.0101	-.005	.0116	-.0055	-.0123	-.0078
Mfx (5)	.0057	-.0013	-.0039	-.002	.0055	-.0021	-.0048	-.003
Mfx (6)	.0166	-.0045	-.0138	-.0068	.0157	-.0075	-.0167	-.0105
B.A. (Mincer)	-1.37	-.947	-.926	-1.11	-1.25	-.732	-.691	-.638
	(.834)	(.808)	(.821)	(.854)	(.941)	(1.19)	(1.14)	(1.26)
Mfx (0)	.4303	.289	.2796	.3335	.3927	.2236	.2087	.1918
Mfx (1)	-.1444	-.1073	-.1038	-.1238	-.1316	-.0829	-.0774	-.0712
Mfx (2)	-.0674	-.0492	-.0477	-.0567	-.0615	-.0381	-.0356	-.0327
Mfx (3)	-.0799	-.0512	-.0497	-.0592	-.073	-.0397	-.0372	-.0341
Mfx (4)	-.0491	-.0294	-.0285	-.0341	-.0448	-.0228	-.0213	-.0196
Mfx (5)	-.0231	-.0115	-.0111	-.0133	-.021	-.0089	-.0083	-.0076
Mfx (6)	-.0665	-.0403	-.0389	-.0464	-.0607	-.031	-.0289	-.0266
Family controls	No	No	Yes	Yes	No	No	Yes	Yes
Neighborhood controls	No	No	No	Yes	No	No	No	Yes
Obs	37767	6302	6302	6302	37767	6302	6302	6302
					First stage:(Dep. variable : DPI)			
Court climate		NA			-.416**	-.505**	-.503**	-.575**
					(.180)	(.197)	(.193)	(.183)
Lawyers		NA			-.105***	-.086***	-.089***	-.089***
					(.030)	(.026)	(.024)	(.027)
F-test: Court cl. & lawyers		NA			12.48	12.22	13.72	14.59
Prob >F					.000	.000	.000	.000

[a]Sample of males in Wave I In-School and In-Home surveys. Standard errors in parentheses.

* significant at 10% level ** significant at 5 % level *** significant at 1% level

[b]Mfx(0), Mfx(1), Mfx(2), Mfx(3), Mfx(4), Mfx(5), Mfx(6) refer to the population mean of the marginal effect for the choice to skip class (respectively):never, once or twice a year, once a month, 2 or 3 days a month, once or twice a week, 3 to 5 days a week, and nearly every day.

Table 5

Long-run skill-related outcomes[a,b,c]

	Dependent variable:			
	H.s. grad (Probit)	H.s. grad (IV-prob)	Employed (Probit)	Employed (IV-prob)
	1	2	3	4
DPI	.136	.536*	.243**	.562***
	(.103)	(.303)	(.109)	(.203)
Mfx	[.033]	[.13]	[.054]	[.12]
H.S. (Mincer)	1.06	1.18*	-1.17	-1.04
	(.825)	(.662)	(.78)	(.751)
Mfx	[.25]	[.28]	[-.26]	[-.23]
B.A. (Mincer)	.333	-.0228	2.04*	1.71*
	(1.04)	(.958)	(1.07)	(.932)
Mfx	[.080]	[-.005]	[.45]	[.28]
Obs	6510	6510	6510	6510

[a] Wave III males.

[b] "Employed" is defined as working at least 20 hrs/wk or still attending school.

[c] Regressions also include full set of covariates from Table 3.

* significant at 10% level

** significant at 5 % level

*** significant at 1% level