Spring 1-1-2015

Empirical Studies in Family and Education Policy

Zachary Feldman
University of Colorado Boulder, feldmanz@colorado.edu

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by

Zachary Feldman

B.A., Pomona College, 2002

M.S., University of Arkansas, 2005

M.A., University of Colorado, 2011

A thesis submitted to the

Faculty of the Graduate School of the

University of Colorado in partial fulfillment

of the requirements for the degree of

Doctor of Philosophy

Department of Economics

2015

This thesis entitled:
Empirical Studies in Family and Education Policy
written by Zachary Feldman
has been approved for the Department of Economics

Professor Terra McKinnish

Professor Francisca Antman

Date

The final copy of this thesis has been examined by the signatories, and we find that both the content and the form meet acceptable presentation standards of scholarly work in the above mentioned discipline.

Feldman, Zachary (Ph.D., Economics)

Empirical Studies in Family and Education Policy

Thesis directed by Professor Terra McKinnish

Abstract

The first chapter evaluates the effectiveness of state arrest legislation to deter domestic violence. Mandatory arrest laws, recommended arrest laws, protective order laws, and primary aggressor laws are evaluated using homicide and suicide rates by state and year. This paper corrects the law classification and law enactment date specification errors in the current literature and allows for a broader look at domestic violence by using suicide rates in addition to the previously used homicide rates. I find that mandatory arrest laws, recommended arrest laws, and primary aggressor laws have no effect on homicide rates and that protective order laws show significant lowering of homicide rates, though only in one of two age groups. Using suicide rates, while recommended arrest laws increase suicide among women by an estimated 8-20%, the more common mandatory arrest laws have no significant effect. Additionally, there is limited evidence for a protective effect of primary aggressor laws and no significant effect of protective order law.

The second chapter examines the effect of new student inflow into Arkansas following hurricanes Katrina and Rita. Over sixty thousand people fled Hurricanes Katrina and Rita in 2005 and came to Arkansas. School aged children were quickly registered and enrolled in local schools. Using this inflow of displaced students, I examine the effect of the inflow of these displaced students on incumbent students in Arkansas. Arkansas was the only state bordering Louisiana not affected by either Hurricane Katrina (Mississippi) or Hurricane Rita (Texas) allowing for an evaluation of incumbent students unaffected by the hurricanes. Additionally, unlike previous research which fits one model for all years post Katrina (giving an average over the two years post Katrina), I fit a separate model for one and two years post Katrina allowing me to test for short term disruption. I find a decrease in attendance one year post Katrina. The effect is largest in K-6th grades among

white students and among male students. The effect is found state wide as well as in the subset of counties along major highways. The drop in attendance is short lived with no significant effect two years post Katrina.

The third chapter examines the effect of new student inflow following court mandated school consolidation. In this paper I examine the effect of peer group composition of student outcomes. I exploit the court mandated consolidation of Arkansas school districts with fewer than 350 students. The influx of new students changes the peer group composition of incumbent students and has two important characteristics. First, the new students and incumbent students are very similar demographically. The consolidated schools are in nearby and similar districts. Second, other than the change in school, new students have undergone a relatively minor disruption to their lives. They are living in the same home with the same family, friends, and neighbors. These two attributes allow for an examination of the effects of the introduction new student on incumbent students outcomes that is more closely tied to new student achievement. The incumbent students will not be reacting to demographic changes in the classroom and the behavior and achievements of the new students will not be influenced by having moved, possibly fled, their previous homes which could have been in another state or another country. The court ordered consolidation thus gives an exogenous shift in students large in scale with many students changing schools yet benign in implementation with the lives of the new students otherwise unchanged.

I assess the effect of peer group composition on incumbent student with a broad set of outcomes including attendance, mathematics and English language proficiency, and disciplinary infractions.

I regress individual level outcomes on the percent of new students in a school/grade from consolidation, the average prior year achievement of consolidated students, and the interaction between the two. I find that in the first year after consolidation, among high school students, the effect of average prior attendance increases with increasing new student inflow and that the effect of new student inflow increases with increasing prior average attendance. For male students in the first year after consolidation and for all students two years after consolidation, I find a similar significant effect on math scores where the interaction term is significant and positive.

Dedication

To Jill Gersh, who said she wouldn't marry me if I didn't finish my dissertation.

Acknowledgements

Many thanks to my advisor, Terra McKinnish. I would also like to thank Francisca Antman, Brian Cadena, Jeffrey Zax, and Stefanie Mollborn who comprised the remainder of my dissertation committee and whose feedback was invaluable in the process of improving my dissertation.

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Chapter 1

Assessing the Effectiveness of State Domestic Violence Legislation

1.1 Introduction

Starting in 1977 and continuing through 2000, 42 states plus the Distict of Columbia passed legislation designed to deter domestic violence. Mandatory arrest laws require the arrest of at least one person when police are called to an incidence of intimate partner violence (IPV). Recommended arrest laws recommend but do not require the arrest of at least one person when police are called to an incidence of IPV. Mandatory and recommended arrest laws are meant to prevent police officers refusing to arrest a violent partner after reported IPV and police officers or the violent partner from threatening or otherwise coercing the victim of IPV into denying the abuse. Mandatory and recommended arrest laws take the decision to arrest out of the hands of both the arresting officers and the victim. In addition to mandatory and recommended arrest laws, states passed laws (1) requiring police to arrest individuals who violate a protective order and (2) instructing police to only arrest one person, the primary aggressor, even if both parties used physical violence. To date, very little research has been undertaken to evaluate the effectiveness of these laws.[1]

Because the introduction of these laws change the circumstances under which arrests take place, many measures of IPV, such as law enforcement arrest records, are mechanically tied to arrest laws through the arrest rate. Homicide rate has been used to measure the effect of mandatory and recommended arrest laws on IPV (Iyengar, 2009), but this measure only captures part of what

[1] Iyengar (2009) examines mandatory and recommended arrest laws using homicide data, but uses inaccurate law classifications and inaccurate dates of enactment.

makes up domestic violence. Other forms of domestic violence may not respond to legislation in the same way as homicide. Additionally, a victim leaving an abuser is a risk factor for homicide (Campbell et al., 2003). Homicide would thus capture an increase in victims leaving abusers as an increase in abuse, making homicide poorly representative of all IPV abuse. Survey data could also be used, but are not available over enough years for enough states.

An indirect or proxy measure of IPV is the next best alternative. Suicide rate has been used as a measure of extreme distress to assess the impact of unilateral divorce laws (Stevenson and Wolfers, 2006) but has not been applied to state domestic violence arrest laws. The use of suicide rate to measure the effect of unilateral divorce by Stevenson and Wolfers shows that suicide rate varies across states, changes across time, and responds to law changes. In addition to homicide rate, I use suicide rate as a proxy for extreme distress to test the effectiveness of arrest legislation in lowering the incidence of IPV.

The effect of arrest legislation on IPV is identified using fixed effects regressions with law changes specified by dummy variables for pre and post law change. Regressions are fit separately for men and women and for different age groups with log of suicide rate and log of homicide rate as the dependent variables. Additionally, the regressions are fit on the subsample of states that enacted legislation.

As a validity test, I fit the binary law change regressions as above with the law change date shifted back 6 years for all states that passed laws. If the results are driven by the enactment of the legislation the indicators for years before law enactment should not be significant.

I construct a panel data set which includes suicide and homicide rate by state, year, sex, and age group (25-44 and 20-54) for the years 1976-2007.[2] The panel includes mandatory and recommended arrest, protective order, and primary aggressor law enactment dates and state and time varying controls.

I find no effect of mandatory arrest laws, recommended arrest laws, or primary aggressor laws on homicide and protective order laws show a significant protective effect among female victims

[2] Suicide rates are from 1979-2007.

20-54 (average decrease in homicide rate of 15-17 percent). When regressing suicide among women on IPV legislation I find that recommended arrest laws increase suicide by 8-20% but that the more commonly enacted mandatory arrest legislation has no significant effect on suicide. Additionally, there is limited evidence for a protective effect of primary aggressor laws and no significant effect of protective order law.

Using suicide and homicide rate I test for a change in IPV from introduction of arrest legislation using a more robust measure of distress than previously used and thus more clearly evaluate the effectiveness of the legislation to combat abuse. Additionally, I use a more accurate coding of recommended and mandatory arrest law classifications and dates of enactment than has previously been used in the literature and include protective order and primary aggressor legislation.

Section 1.2 gives a brief review of the existing literature. Section 3.3 is a description of the data. Section 1.5 defines the regression specifications. Sections 2.6 and 2.6 are results and conclusions.

1.2 Existing Research

1.2.1 Suicide and IPV

Many studies both in the United States and intentionally have shown an increased likelihood of suicide among victims of IPV. Pico-Alfonso et al. (2006) find a higher incidence of suicide among female victims of physical and psychological abuse. An analysis of data from the WHO multi-country study on womens health and domestic violence against women found intimate partner violence to be a strong predictor or suicideal thoughts and suicide attempts among women (Ellsberg et al. (2008) and Devries et al. (2011)). In a survey of IPV prevalence, Seedat et al. (2005) found 23 percent of abused women reported a suicide attempt compared to 3 percent of nonabused women. A review and meta-analysis of existing studies by Devries et al. (2013) found positive relationships between IPV and suicide in all three of the relevant studies reviewed.

1.2.2 Homicide Rate as Measure of IPV

Iyengar (2009) uses intimate partner homicide rate across states and years to study the impact of mandatory and recommended arrest laws on IPV. Mandatory and recommended arrest laws could have a potentially ambiguous effect on homicide rates. Certainty of arrest could be a deterrent to future abuse and lower homicide rates. However, if the abused individual becomes less likely to report abuse because of guilt about arrest or fear of future reprisals, the lowered reporting of abuse could lead to a decrease in police action and an increase in homicides. Additionally, presence of reprisals (additional retaliatory violence against the victim who reported the initial abuse in response to the arrest) can both lower reporting of abuse through fear on the part of the abused and increase violence since the reprisals themselves are additional IPV.

For the identification strategy to be causal it needs to be the case that the enactment of mandatory and recommended arrest laws was not driven by increased IPV. Iyengar argues that the enactment of these laws was driven by a move to criminalize IPV by the legal and medical community and increased exposure to lawsuits after a 1984 federal court decision established the right to police protection from domestic violence.

Iyengar fits a diff-in-diff regression with state and year fixed effects comparing mandatory arrest states, recommended arrest states, and those that did not enact new laws. Iyengar uses a the law change as a binary outcome, before and after the enactment of legislation. Iyengar finds an increase in intimate partner homicides among women after introduction of mandatory arrest laws. No change in rates is found for men or for other forms of homicide and no change is found for introduction of recommended arrest laws. As mentioned above, intimate partner homicide only measures one form of IPV. Other forms of IPV may respond differently to the enactment of mandatory and recommended arrest laws. Moreover, Iyengar uses inaccurate law codings in classification to arrest legislation group, year of enactment, and relevant statute.

1.2.3 Suicide and its use as a measure of IPV

Stevenson and Wolfers (2006) use suicide rate among married women to examine the impact of unilateral divorce legislation. By removing the hurdles to divorce, fewer women would find strategic suicide to be necessary. Two paths are proposed. First, women in abusive marriages will now be able to get a divorce, ending the abusive marriage and the IPV. However, Stevenson and Wolfers show that the increase in divorce rates was too transitory and small to account for the large drop in suicides. The second mechanism is through increased bargaining power within the marriage for the abused individual. Increased bargaining power provided by unilateral divorce would make strategic suicide unnecessary and lower suicide rates.

Stevenson and Wolfers fit a fixed effects regression with state and year fixed effects and year of unilateral divorce law enactment entered as dummies since enactment. Stevenson and Wolfers also add to the regression controls for female employment rates, welfare generosity, business cycle shifts, availability of abortion, and racial and age composition of each state. Stevenson and Wolfers find that suicides rates among women drastically and significantly decrease after the introduction of unilateral divorce laws but no changes are found among men.

There are two important aspects of the use of suicide data as response measure. First, it changes a great deal across time and changes differentially across states. Second, Stevenson and Wolfers show that a law change can lead to a large change in this measure. Both will be necessary to use suicide as a measure of the impact of mandatory and recommended arrest laws on IPV.

1.3 Mechanism

Mandatory and recommended arrest legislation can effect IPV through several potential mechanism. Increased likelihood of arrest may act as a deterrent against IPV, lowering rates of IPV. Arrest of a partner may induce the victim of IPV to leave a relationship, lowering the number of abusive relationships, and thus lowering the rates of IPV. Victims' fears of reprisals may lower the incidence of reporting, increasing rates of IPV. Additionally, reprisals in and of themselves are IPV and would increase the rates of IPV. Ambiguity thus exists in the direction of the effect of arrest legislation on IPV. Increased arrests could deter IPV, but if police arriving at a household reduces IPV even if no arrest is made, the introduction of these laws could actually increase IPV if abused individuals are less likely to report abuse and thus the abuse receives less police attention.

1.4 Data

1.4.1 Suicide Counts

Suicide counts by state, year, sex, and age group are obtained from the Center for Disease Control where suicide is indicated as defined by International Statistical Classification of Diseases and Related Health Problems 9 (ICD 9) and International Statistical Classification of Diseases and Related Health Problems (ICD 10) codes for years 1979 to 2007. Suicide counts from the CDC are available subdivided by sex and age, however, choice of ages is limited by two conflicting concerns. To understand which age groups are most affected by changes to arrest law legislation, small age cohorts are ideal. However, because the CDC suppresses counts of suicide below 10 per year, cutting age into smaller age groups leads to increased suppression of counts and thus fewer state/years included. With these restrictions in mind I use two age groups: first, individuals 20-54 at time of death in order to have the fewest suppressed observations and second, individuals 25-44 at time of death to see how the law affects a smaller and younger cohort. Because suicide is more prevalent among white individuals, I run the regressions with all suicides included and with only white individuals (there are too few suicides among black individuals to run the regressions for black individuals only).

Suicide rate is calculated as deaths per 100,000 from CDC provided data (both counts and state populations). Log of suicide rate is used in the regressions because the baseline suicide rate varies drastically between men and women. The log specification allows the parameter estimates on the dummies to be approximately interpreted as percent changes. Suicide count is for all individuals in the specified age/sex group and is not limited to those in a relationship. This means that the estimated effect of the arrest legislation is spread over a larger group of individuals than the law immediately targets. This is done both because data is not available for individuals only in relationships and because the estimated effect will include drops in suicide from individuals leaving abusive relationships.

1.4.2 Homicide Counts

Homicide counts by state, year, sex, and age group are obtained from Federal Bureau of Investigation Uniform Crime Reporting Program yearly Supplementary Homicide Reports. The homicide reporting data lists individual homicides and includes demographic data on the victim and offender. Intimate partner is defined as either husband, wife, common-law husband, common-law wife, ex-husband, or ex-wife. To allow for comparison with the suicide analysis, I limit the sample by sex, race, and age group.

1.4.3 Arrest Law Coding

Iyengar constructs a list of states classified into each law group (mandatory and recommended arrest), year of enactment of relevant law change, and applicable statute number. Westlaw database is cited as the source of these data. However, there are inaccuracies in the classification, year of relevant law change, and statute number.

Instead of using Iyengar's coding, I use the group classifications and statute numbers from two sources. First, I use a list of law classifications and relevant statutes from the unpublished final report to the The U.S. Department of Justice from a federally funded grant to study dual arrest (Hirschel et al., 2007). Second, I use a list of law classifications and relevant statutes from the American Bar Assocaition (ABA, 2007). Both sources are nearly identical. Protective Order and Primary Aggressor law coding is based on the report by Hirschel et al. (2007).

Because dates of the applicable statute revisions are not listed in the above sources and the Westlaw and Lexis-Nexis databases are not exhaustive in the text of revisions, I use the HeinOnline database to examine state legislative documents by year. For every state categorized as having enacted an arrest law I define the date of enactment in one of two ways. I examine revisions chronologically until I find the revision that makes the applicable change.[3] If the insertions and deletions are not listed, I examine revisions chronologically until I find a revision year document that contains the law change text such that the preceding revision document did not include the law change text.[4]

The lists of states with mandatory arrest, recommended arrest, protective order, and primary aggressor legislation are in table 1.1. Figure 1.1 shows the timing of the law enactments. The 45 degree line shows which states passed multiple laws in the same year. Table 1.2 shows the counts of states that passed different combinations of the laws.

Table 1.1: Domestic Violence Arrest Legislation Dates

State	Recommended Arrest	Mandatory Arrest	Protective Order	Primary Aggressor
Alabama				2000
Alaska		1996	1996	1996
Arizona		1991		
Arkansas	1991			
California	1995		1993	1995
Colorado		1994	1994	1994
Connecticut		1986		
Delaware			1993	
District of Columbia		1991		
Florida	1997			1997
Georgia				1991
Hawaii				
Idaho				

[3] Some state revision document, for example, denotes additions and subtractions with <<+ new text +>> and <<- old text ->>. When this is the case, the revision year document with the law change text defines the year of law change for my coding.

[4] For recommended arrest laws, the current statute will have wording that describes under what circumstances an officer may make warrantless arrest and will contain “preferred” when describing the act of arresting the individual suspected of domestic violence. For mandatory arrest laws, the current statute will contain “shall” or “must” when describing the act of arresting the individual suspected of domestic violence.

Illinois				
Indiana				
Iowa		1987	1988	1990
Kansas		1991	1992	
Kentucky			1992	
Louisiana		1985	1987	
Maine		1980	1980	
Maryland			1995	1996
Massachusetts	1991		1987	
Michigan				
Minnesota			1983	
Mississippi		1995	1995	
Missouri			1989	1989
Montana	1991			1997
Nebraska			1989	
Nevada		1985		1989
New Hampshire			1994	1989
New Jersey		1991	1991	1991
New Mexico			1987	
New York		1994	1994	1997
North Carolina			1999	
North Dakota	1995		1995	
Ohio		1994		1994
Oklahoma				
Oregon		1981	1977	1991
Pennsylvania			1990	
Rhode Island		1988	1988	1988
South Carolina		1995	1995	1995
South Dakota		1989	1989	1989
Tennessee	1995		1987	1995
Texas			1991	
Utah		1995	1995	1995
Vermont				
Virginia		1997	1996	1996
Washington		1984	1984	1985
West Virginia			1994	
Wisconsin		1988	1983	1987

Wyoming

Figure 1.1: Distribution of Laws

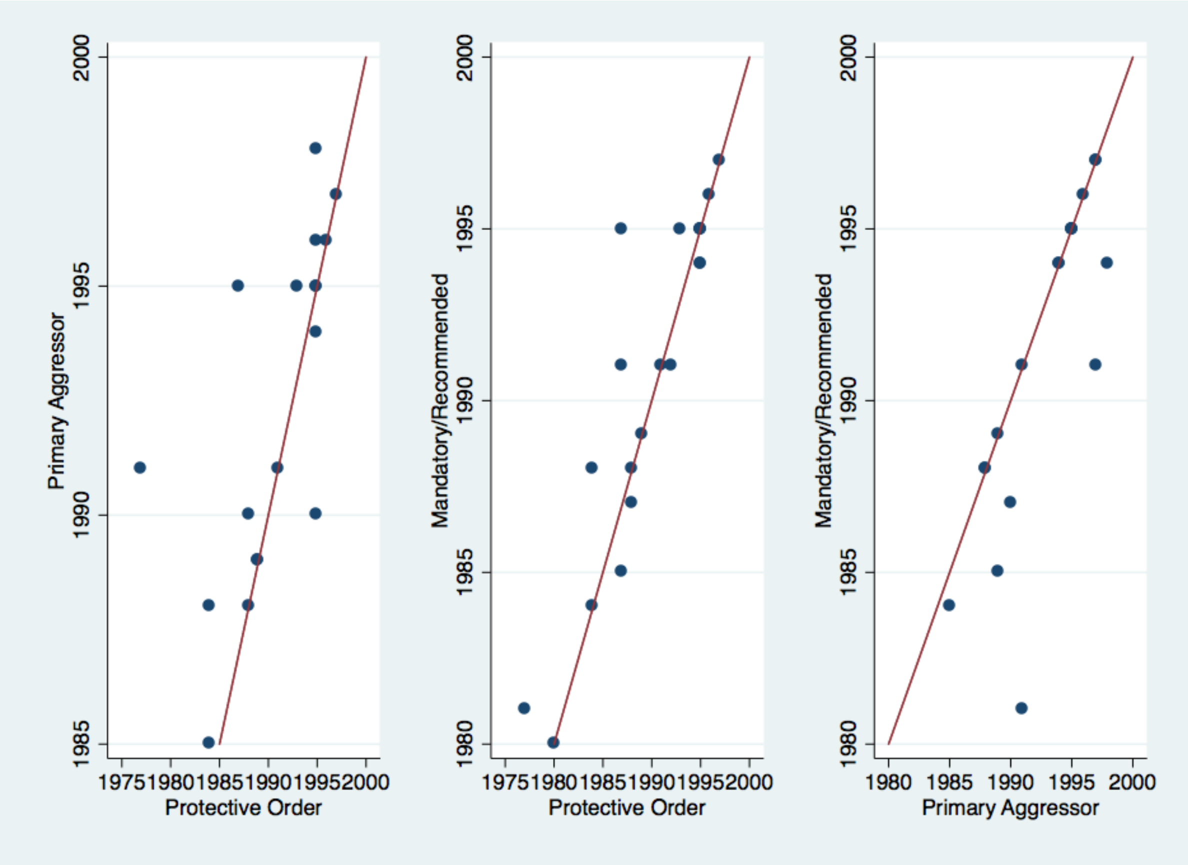


Table 1.2: Counts of States Passing Law Combinations

		Protective Order		Primary Aggressor	
	All	No	Yes	No	Yes
All	51	18	33	27	24
Mandatory Arrest					
No	29	13	16	20	9
Yes	22	5	17	7	15
Recommended Arrest					
No	44	15	29	24	20
Yes	7	3	4	3	4
Primary Aggressor					
No	27	12	15		
Yes	24	6	18		

1.4.4 Controls and Sample Description

Unemployment rate for each state in each year is obtained from the Bureau of Labor Statistics. Personal income per capita for each state in each year is obtained from the Bureau of Economic Analysis.

Summary statistics for women aged 20-54 in 1980 for states which passed discretionary arrest laws, mandatory arrest laws, and recommended arrest laws are in table 1.3. States that passed mandatory arrest laws are generally smaller and wealthier and states that passed recommended arrest laws are generally larger and less wealthy than states that did have discretionary arrest laws.

Table 1.3: Summary statistics in 1980 for women aged 20-54.

	N	mean	sd	min	median	max
Discretionary Arrest States						
Homicides	22	20.57	22.93	1.	15	96
Suicides	22	73.18	67.56	11.	65	274
Population	22	1047549	959145	111131	886307	3415386
Income	22	9530	1181	7825	9420	11668
Unemployment	22	6.88	2.04	3.87	6.88	12.12
Deaths per 100,000	22	7.63	2.25	4.04	7.53	13.50
Mandatory Arrest States						
Homicides	22	14.95	12.90	2	12	47
Suicides	22	64.73	65.44	8	47	283
Population	22	899795	959960	103483	645035	4298101
Income	22	10123	1794	7005	10053	14975
Unemployment	22	6.87	1.22	4.51	6.97	9.41
Deaths per 100,000	22	8.14	4.07	2.63	7.57	23.74
Recommended Arrest States						
Homicides	7	32	38.01	7	14	97
Suicides	7	155.86	212.03	7	79	596
Population	7	1624241	2001621	144152	1111478	5856155
Income	7	9300	1595	7521	9038	11928
Unemployment	7	6.42	0.93	5.01	6.28	7.58
Deaths per 100,000	7	8.25	2.51	4.86	8.55	11.95

Homicides, suicides, population, and deaths per 100,000 are specific to women aged 20-54, income and unemployment are aggregated across all individuals at the state/year level.

1.5 Model Specification

1.5.1 Binary Law Change Fixed Effect Specification

I use OLS with standard errors clustered at the state level to estimate parameters in the following equation.

$$\begin{aligned} log(Y_{s,t}) &= \beta_0 + \beta_1 Mandatory_{s,t} + \beta_2 Recommended_{s,t} \\ &\quad + \beta_3 ProtectieOrder_{s,t} + \beta_5 PrimaryAggressor_{s,t} \\ &\quad + \Psi State_s + \Gamma Year_t + Controls_{s,t} + \phi_s \times t + \varepsilon_{s,t} \end{aligned} \tag{1.1}$$

$ProtectiveOrder_{s,t}$ and $PrimaryAggressor_{s,t}$ are dummy variables for current protective order law and primary aggressor law in state s at time t. $Y_{s,t}$ is the outcome variable (suicide rate or homicide rate) in state s in year t. $Mandatory_{s,t}$, $Recommended_{s,t}$, $ProtectiveOrder_{s,t}$, and $PrimaryAggressor_{s,t}$ are dummy variables for current mandatory arrest law, recommended arrest law, protective order law, and primary aggressor law in state s at time t. $State_s$ and $Year_t$ are fixed effects indexed by state and year respectively. $Controls_{s,t}$ includes personal income per capita, unemployment rate in state s at time t, and a dummy indicating if state s in year t had passed unilateral divorce legislation.[5]

The coefficient on the law indicator is the average change in log(SuicideRate) or log(HommicideRate) after enactment of the law change. This specification assumes that the effect of the law is homogeneous across time after enactment.

I estimate equation (1.1) separately for suicide rate and homicide rate, men and women, all races and white only, and deaths at age 25-44 and 20-54 giving a total of 16 regressions. Because suicide is more prevalent among white individuals than black, I compare suicide and homicide rates for all races and for white separately.

Additionally, the above 16 regressions include state specific linear time trends (ϕ_s). For the state specific linear time trend, year is added to the regression with each state having its own

[5] Unilateral divorce legislation is included because Stevenson and Wolfers (2006) find a significant effect of unilateral divorce on IPV.

parameter (added as a continuous variable as opposed to the dummies in the year fixed effects which are still included). These will capture any linear trend in the outcome differentially across states; these state specific time trends will control for any omitted variables that are causing a continued linear in time change in the suicide rate. Since the introduction of the law is binary it is not expected to cause a linear in time change in the suicide rate over the entire range of observations.

I also fit the previous regressions on the subsample of states that passed a domestic violence prevention law to identify the parameters only on timing of enactment.

1.5.2 Falsification

As a falsification test, I estimate equation (1.1) with the year dummies shifted back 6 years (in calculating the dummies I subtract 6 from the law enactment date). The parameters on the dummies for the years since enactment that fall before the true enactment date should be small and not significantly different than zero if the results are driven by arrest legislation.

1.6 Results

Results from regressions fitting equation 1.1 with intimate partner homicides are shown in table 1.4. The only significant effect we see is for protective order laws among female victims 20-54 (while not significant, the estimate for female victims 25-44 is of a similar magnitude). The estimate of -0.148 indicates an average decline in intimate partner homicide rate of 14.8 percent among female victims aged 20-54.

Table 1.4: Intimate Partner Homicide Victims Ages 25-44 and 20-54

dependent variable: log(deaths per 100,000)	25-44		20-54	
	Female	Male	Female	Male
All Races				
Mandatory Arrest	0.132	-0.036	0.117	0.019
	(0.127)	(0.125)	(0.109)	(0.137)
Recommended Arrest	0.083	0.011	0.227	0.123
	(0.165)	(0.099)	(0.117)	(0.141)
Protective Order	-0.141	0.069	-0.148*	0.066
	(0.097)	(0.097)	(0.070)	(0.090)
Primary Aggressor	-0.112	-0.073	-0.034	-0.052
	(0.097)	(0.088)	(0.095)	(0.096)
Constant	0.662	-0.299	-0.213	0.636
	(1.037)	(1.131)	(0.947)	(1.215)
Observations	951	951	1072	1072
White Only				
Mandatory Arrest	0.003	-0.055	-0.129	0.011
	(0.137)	(0.089)	(0.111)	(0.093)
Recommended Arrest	-0.061	-0.113	0.099	-0.007
	(0.109)	(0.110)	(0.111)	(0.131)
Protective Order	-0.169	-0.092	-0.167*	0.003
	(0.113)	(0.090)	(0.070)	(0.108)
Primary Aggressor	-0.022	0.024	0.149	-0.058
	(0.098)	(0.082)	(0.105)	(0.083)
Constant	0.620	1.032	-0.163	0.414
	(1.275)	(1.113)	(1.019)	(1.396)
Observations	818	818	960	960

std. err. clustered at state level
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

All regressions include state and and year fixed effects, unemployment rate by state and year, state specific linear time trend, and personal income per capita by state and year.

Results from regressions fitting equation 1.1 with suicide rate are shown in table 1.5. The effect of recommended arrest laws on suicide rate is significant and positive for all regressions for women. Additionally, the effect size is larger among the 25-44 age group than the 20-54 age group. For all races the estimated average increase in suicide rate is 15.8 percent among women 24-44 and 8.3 percent among women 20-54. When looking at white only, the estimated average increase in suicide rate is 20.3 percent among women 24-44 and 9.0 percent among women 20-54. Additionally, there is a significant decline in the suicide rate among white women 24-44 with an estimated average decline in the suicide rate of 8.4 percent after the introduction of primary aggressor laws (though not signifant when including all races or the broader age group, the magnitude is similar with looking at all races).[6] ,[7]

[6] Removing Protective Order laws Primary Aggressor laws from the regressions does not materially change the estimates for Mandatory and Recommended arrest laws.

[7] Fitting the above regressions only with those states that passed laws does not change the estimates. Though if the regressions are limited to only those states that passed Mandatory and Recommended arrest laws the results are only significant for among white females, though the estimates change very little.

Table 1.5: Suicide All Races Ages 25-44 and 20-54

dependent variable: log(deaths per 100,000)				
	25-44		20-54	
	Female	Male	Female	Male
All Races				
Mandatory Arrest	-0.010	0.000	-0.009	-0.007
	(0.042)	(0.030)	(0.033)	(0.020)
Recommended Arrest	0.158*	-0.005	0.083*	-0.013
	(0.066)	(0.037)	(0.036)	(0.027)
Protective Order	0.023	-0.016	0.035	0.006
	(0.037)	(0.021)	(0.034)	(0.015)
Primary Aggressor	-0.075	0.005	-0.039	-0.008
	(0.041)	(0.024)	(0.035)	(0.015)
Constant	1.518***	2.957***	1.689***	2.958***
	(0.182)	(0.122)	(0.118)	(0.081)
Observations	1322	1322	1408	1408
White Only				
Mandatory Arrest	-0.022	-0.010	-0.008	-0.005
	(0.050)	(0.030)	(0.037)	(0.020)
Recommended Arrest	0.203***	-0.010	0.090*	-0.014
	(0.049)	(0.027)	(0.035)	(0.022)
Protective Order	0.030	-0.015	0.042	0.006
	(0.040)	(0.022)	(0.035)	(0.016)
Primary Aggressor	-0.084*	0.014	-0.040	-0.006
	(0.039)	(0.025)	(0.035)	(0.017)
Constant	1.774***	3.125***	1.903***	3.147***
	(0.174)	(0.137)	(0.116)	(0.091)
Observations	1295	1295	1381	1381
	std. err. clustered at state level			
	* p<0.05, ** p<0.01, *** p<0.001			

All regressions include state and and year fixed effects, unemployment rate by state and year, state specific linear time trend, and personal income per capita by state and year.

The falsification test in which law enactment dates are shifted back 6 years show no consistent effect of the IPV legislation (table 2.7).

Table 1.6: Falsification: Suicide All Races Ages 25-44 and 20-54

dependent variable: log(deaths per 100,000)				
	25-44		20-54	
	Female	Male	Female	Male
All Races				
Mandatory Arrest	-0.012	0.017	-0.021	0.028
	(0.056)	(0.033)	(0.043)	(0.026)
Recommended Arrest	0.025	-0.012	0.041	0.001
	(0.069)	(0.041)	(0.043)	(0.033)
Protective Order	-0.005	0.021	0.008	0.016
	(0.033)	(0.025)	(0.034)	(0.021)
Primary Aggressor	0.000	-0.004	-0.034	-0.003
	(0.040)	(0.029)	(0.028)	(0.021)
Constant	1.559***	2.942***	1.715***	2.960***
	(0.193)	(0.119)	(0.120)	(0.082)
Observations	1322	1322	1408	1408
White Only				
Mandatory Arrest	-0.028	0.064	-0.019	0.045
	(0.053)	(0.058)	(0.043)	(0.031)
Recommended Arrest	0.073	0.004	0.056	0.025
	(0.056)	(0.041)	(0.047)	(0.038)
Protective Order	0.004	0.006	0.013	0.024
	(0.031)	(0.026)	(0.037)	(0.021)
Primary Aggressor	-0.018	-0.022	-0.031	-0.010
	(0.042)	(0.032)	(0.028)	(0.023)
Constant	1.829***	3.093***	1.936***	3.148***
	(0.192)	(0.128)	(0.119)	(0.091)
Observations	1295	1295	1381	1381
	std. err. clustered at state level			
	* p<0.05, ** p<0.01, *** p<0.001			

All regressions include state and and year fixed effects, unemployment rate by state and year, state specific linear time trend, and personal income per capita by state and year.

1.7 Discussion

The above results indicate that (1) the conclusions of the existing literature that mandatory arrest laws lead to increased IPV as measured by homicide rates are incorrect and (2) that using suicide data in addition to homicide data gives a fuller description of the effects of IPV legislation.

The existing literature shows a significant increase in IPV after the enactment of mandatory arrest laws as measured by intimate partner homicides. With the corrected law coding and enactment dates there is no significant effect of either mandatory or recommended arrest laws.[8] However, protective order laws showed a significant negative effect for female victims 20-54.

When using suicide counts to measure IPV, mandatory arrest continues to have no significant effect and in addition recommended arrest legislation has a significant positive effect leading to an increase in IPV. With many more states having passed mandatory arrest legislation than recommended arrest legislation (22 when counting District of Columbia compared to 7) of particular importance is that while the above analysis shows no protective effect of mandatory arrest legislation, when using both intimate partner homicide and suicide as measures of IPV there is no evidence that the much more prevalent mandatory arrest laws laws have increased IPV.[9]

When using suicide data as a proxy measure for extreme distress, recommended arrest legislation is shown to increase IPV while mandatory arrest legislation has no significant effect. This difference in result is not seen when using intimate partner homicide data. Thus suicide as a measure of IPV is measuring different aspects of IPV than homicide.

Additionally, the difference in effect found for mandatory arrest and recommended arrest show that the arrest legislation is not a proxy for some form of "intent to do good". If passing arrest legislation was an indicator of a broader statewide push for stronger protection against IPV both laws would be expected to show the same effect.

[8] While the construction of the panel dataset used above is similar to that used by Iyengar (2009), differences in data sources means that the results in the current literature could not be exactly replicated and thus I cannot rule out other sources of the difference in results

[9] Because only seven states passed recommended arrest laws, it is possible that the results are being driven primarily by one state. To test for this I fit the regressions using suicide rates seven times, each time with one of the states which passed recommended arrest legislation removed. While significance was lost in some of the regressions, the sign of the effect of recommended arrest legislation stayed negative and of consistent magnitude.

If the falsification test in table 2.7 showed similar results to the regressions in table 1.5, it would have been an indication that the states that passed domestic violence legislation were already seeing changes in suicide rates.

1.8 Conclusion

In this paper I evaluate the effectiveness of state domestic violence legislation. Prior research (1) only looked at mandatory and recommended arrest laws, (2) only used homicide rates as an outcomes measure, and (3) used incorrect law classifications and dates.

I find some evidence that protective order legislation is effective when evaluting the laws using homicide rates and suicide rates and no effect of primary aggressor legislation. Unlike the current literature, I find that recommended arrest laws, and not the much more commonly passed mandatory arrest laws, have a deleterious effect. I find no significant effect of either recommended or mandatory arrest laws when using homicide rates, however I find a significant increase in suicide rates following the passage of recommended arrest laws. While there is little evidence that state legislation to limit IPV has been effective, it is the legislation type passed by the fewest number of states that increases suicide rates. This shows that while the passage of these laws was not generally beneficial, the negative effects are much less widespread than previously thought.

Chapter 2

Peer Effects from Externally Displaced Hurricane Evacuees

2.1 Introduction

The movement of students from school to school can come from many sources. Migration within a school district, a state, or the United States changes the composition of classrooms as students leave one school and attend another. If the migration is driven by broad economic or demographic shifts, entire school districts and states can be affected. The introduction of charter schools gives students an opportunity to leave an existing school, while school consolidation results in the opposite with students leaving a closing school. Immigration also brings foreign born students into US classrooms for the first time, which in addition to the demographic changes in the classroom can necessitate changes in curriculum and training for teachers. Examining the effects of student movement allows policy makers, school administration, and teachers to prepare for the flow of students into and out of a district.

In order to identify a causal connection between a student's peer group and a student's own performance, variation in peer group composition is needed. This variation must fulfill two requirements: first, it's distribution must be exogenous to incumbent student performance and peer group composition and second, the variation must not affect aspects of the student's environment other than peer group composition. To ensure the first requirement is satisfied, I exploit the inflow of evacuees from Hurricanes Katrina and Rita as a change in peer group composition in Arkansas schools that is exogenous to incumbent and entering student quality. I then test for changes in student environment to satisfy the second requirement.

Hurricane Katrina made landfall on August 29th, 2005 in New Orleans, LA and proceeded through Mississippi. Millions of people in Louisiana, Mississippi, and Alabama were displaced from their residences. Bureau of Labor Statistics estimates indicate 1.5 million people over the age of 16 years old were displaced (Groen and Polivka, 2008b). Of the evacuees from Katrina, many sought refuge in neighboring states. 50,000 Katrina evacuees came to Arkansas on their own while an additional 10,000 were brought by bus to Fort Chaffee, a National Guard training center east of Fort Smith, AR (SGFF, 2006).

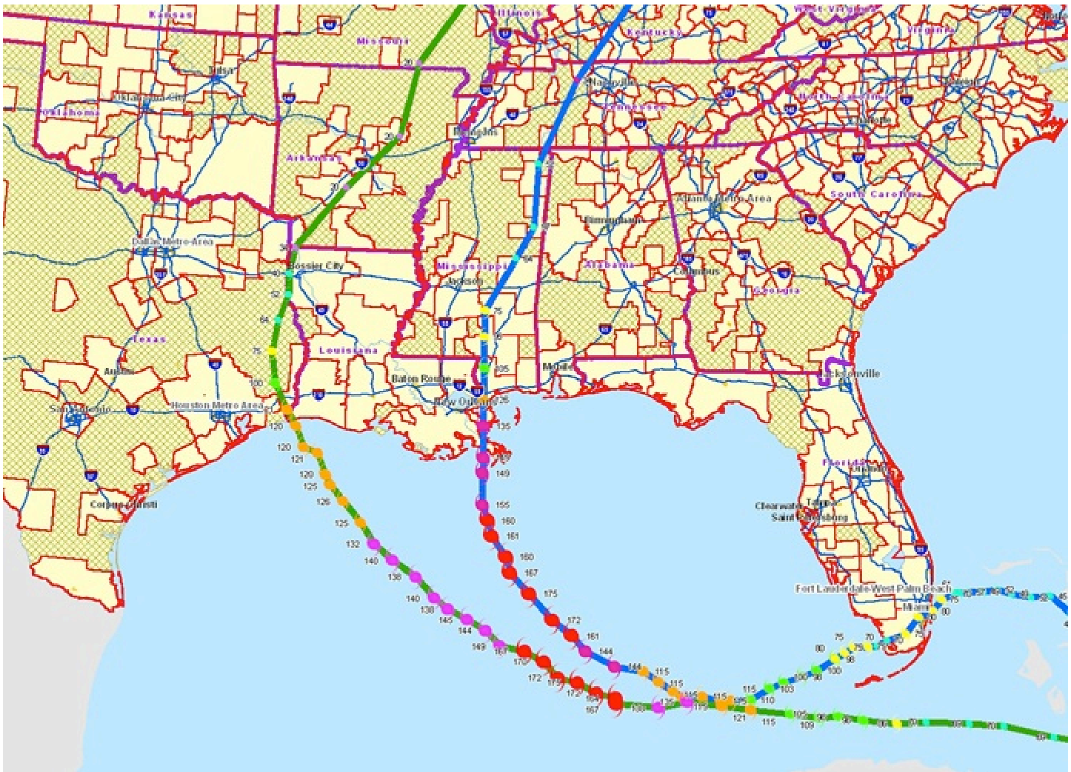
I use the inflow of hurricane evacuees to Arkansas as an exogenous introduction of new students to examine peer effects on incumbent students in Arkansas's K-12 school system. Arkansas is a well suited location to look for effects on education resulting from Katrina evacuees because Arkansas was the only state bordering Louisiana not affected by either Hurricane Katrina (Mississippi) or Hurricane Rita (Texas)[see figure 2.1].[1] Because Hurricanes Katrina and Rita made landfall in New Orleans and East Texas, respectively, whether looking in Louisiana or Houston, some portion of the evacuees will be internally displaced within the original state of residence. Therefore the incumbent students in Arkansas were not directly affected by the hurricanes. Externally displaced students may change the the composition of incumbent peer groups to a greater degree than internally displaced students.[2] ,[3] Unlike existing research, analysis of incumbent students in Arkansas can be performed with a treatment effect from peer group changes driven by an exclusively externally displaced evacuee population.

[1] Three weeks after Hurricane Karina, Hurricane Rita made landfall east of Houston on September 24th.

[2] I use *externally displaced* to indicate students who relocated after Katrina to a state other than their original states of residence and *internally displaced* to indicate students who relocated after Katrina within their original states of residence.

[3] State resources used to accommodate internally displaced students may also be zero-sum with resources being diverted from hurricane affected areas to schools taking in evacuee pupils.

Figure 2.1: Katrina and Rita Path



The treatment variable used in the analysis is the inflow of hurricane evacuees, but calculated as the increase above normal levels of out of state inflow. So, the treatment variable is the change from the normal level of *new out of state* students following hurricanes Katrina and Rita. The treatment variable is calculated at the school/grade level and as will be seen below allows for the inclusion of school and grade fixed effects in the analysis.

The outcomes variables used in the analysis are of two types: behavioral outcomes and scholastic outcomes. The behavioral outcomes are school attendance and disciplinary infractions of incumbent students. The scholastic outcomes are mathematics and English language proficiency tests. The scholastic outcomes allow for testing of final education proficiency while the behavioral outcomes allow for testing of intermediate class room outcome. Additionally, I examine the inflow of students on district level financial outcomes, though treatment at the district level precludes the use of fixed effect. I look at changes in total revenue, total expenditures, total revenue per pupil, and total expenditures per pupil. I find that the effects on incumbent students are generally limited to attendance and only in the first year post Katrina with the effect no longer significant two years after.

2.2 Related Literature

Research examining peer effects in grades K-12 have shown mixed results. Several papers show no or very small effects when looking at test scores: Angrist and Lang (2004), Hanushek et al. (2003), and Vigdor and Nechyba (2006). Several papers also show large beneficial effects when using test scores: Hoxby (2000), Hoxby and Weingarth (2005), Lavy and Schlosser (2007), and Lavy et al. (2012). Peer behavior has also been shown to lead to worse outcomes, with students with poor behaving peers having worse own behavior and achievement (Carrell and Hoekstra (2010), Carrell et al. (2008), Figlio (2007), Gould et al. (2009), Kling et al. (2007), and Lavy and Schlosser (2007)).

Vigdor (2008), Groen and Polivka (2008a), and Belasen and Polachek (2008) examine the economic effects of Hurricane Katrina on New Orleans and the labor markets of Katrina evacuees.

Paxson and Rouse (2008) examine why Katrina evacuees chose to return to New Orleans.

There are two papers directly addressing peer effect in K-12 education in the wake of hurricanes Katrina and Rita: Sacerdote (2012) and Imberman et al. (2012). Sacerdote (2012) looks at education outcomes of evacuees internally displaced in Louisiana. The author finds a drop in test scores in the year immediately following Katrina but the decline is overshadowed by gains in years two and three after Katrina. The subsequent gains are partially attributed to the closing of schools in New Orleans which were some of the worst performing in the state.

Imberman et al. (2012), the paper most similar to mine, examines peer effects from hurricane evacuees in Louisiana and Houston. The authors find that incumbent test scores are not affected by an increased proportion of hurricane evacuees in a grade. They also find that incumbent attendance falls as a result of increased hurricane evacuees, but only at the middle school and high school level among African American students.

This paper expands beyond the work of Imberman et al. (2012) in two ways. First, by using data from Arkansas the peer effect estimates are based on exclusively out of state evacuees. Second, the effects are estimated separately for the first and second year after hurricanes Katrina and Rita allowing me to rule out an initial drop in scores nullified by a reversion back in the second year after the storms. Imberman et al. (2012) estimate only the average effect across two years post Katrina.

2.3 Effect Pathways

The inflow of new students into Arkansas following hurricanes Katrina and Rita could affect the incumbent students through many pathways. Some of these pathways such as school funding and overcrowding are driven by a stretching of resources. If the addition of new students was not met with increased funding or the addition of extra classrooms, the resources available to incumbent students would fall. The analysis below examines the financial outcomes and enrollment at the district level to explore possible resource constraints following the introduction of new students. These effects, while important, are not direct peer effects.

The specific characteristics of the new students can have a direct peer effect on the incumbent students. The new students can have differences in health, behavior, economic disadvantage, and academic achievement. The introduction of the new students changes the composition of the classroom. The change in classroom composition can have unexpected consequences. For example, Lavy et al. (2012) find that the increase in low achievers change teacher practices which hurts inter-student and student-teacher relationships and increases behavior problems in the classroom. Thus, change in classroom composition of one attribute may not lead to a change in the same outcome measure. There can also be heterogeneous effects across the range of achievement. Lavy et al. (2009) find peer quality has negative effects at the bottom of the ability distribution, but that average ability and high ability peers have little effect. Gibbons and Telhaj (2008) find that low achieving students are disadvantaged by higher achieving peers while middle and upper achieving students benefit from higher achieving peers.

Additionally, the actions of parents of new students can effect such as when students are held out of school can effect the classroom experience of incumbent students.

2.4 Data

2.4.1 Sources

The Arkansas Department of Education Statewide Information System (SIS) provides enrollment data with enrollment date and drop date. Additionally, the SIS also provides quarterly attendance as days present and absent, discipline incidents, standardized test scores including English language and mathematics proficiency, and demographics for all K-12 students in Arkansas matched yearly from the 2003-2004 school year through the 2007-2008 school years. To examine peer effects from Katrina and Rita evacuees I use data from the 2003-2004 school year through the 2006-2007 school year. Student data is matched by student ID number from year to year.

The National Center for Education Statistics (NCES) provides additional data at the grade, school, district, and state level. I use the following yearly district financial data from NCES:

total district revenue, total district expenditures, total district revenue per pupil, and total district expenditures per pupil.[4]

2.4.2 Treatment Definition

The treatment variable that represents peer group composition changes is the increase in new out of state students in the 2005-2006 school year (first year post Katrina) above the usual level. Using new out-of-state students as a percent of incumbents for each year, I calculate the increase in the 2005-2006 school year above the average of the 2003-2004 and 2004-2005 school years.

$$\begin{aligned} EvacueeFraction &= \frac{NewOutofState}{Incumbent}_{2005-2006} \\ &- \frac{1}{2}\left[\frac{NewOutofState}{Incumbent}_{2003-2004} + \frac{NewOutofState}{Incumbent}_{2004-2005}\right] \end{aligned} \quad (2.1)$$

If either $\frac{NewOutofState}{Incumbent}_{2003-2004}$ or $\frac{NewOutofState}{Incumbent}_{2004-2005}$ is missing, I use the single year value to represent the normal pre-Katrina inflow. Using equation 2.1, I construct $EvacueeFractionSchool_k$ as the increase above average calculated at the school level (increase for school k) and $EvacueeFractionGrade_{jk}$ as the increase above normal calculated at the grade level (increase for grade j at school k).

In examining peer effects on incumbent students from Katrina and Rita evacuees, the treatment (inflow of students into Arkansas schools) must be large enough and varied enough to allow for identification.[5] Figure 2.2 shows the distribution of new out of state students as a percent of incumbents at the school level over five school years from 2003-2004 through 2007-2008. The mass of the distribution is shifted up in the first year post Katrina (2005-2006) with the median for the 2005-2006 school year near the third quartile of either of the previous two years' distributions. Figure 2.3 shows a similar distribution for the subset of counties along interstates and major highways entering the state and figure 2.4 shows the distribution of new out of state students in

[4] Much of the data from the SIS contains duplicates, the majority of which are exact duplicates of all data though some have differing school and attendance values. In the cases where the duplicates are not exact I use the yearly observation with the largest number of days accounted for (absent plus present).

[5] Bureau of Labor Statistics estimates show that a much smaller portion of evacuees stayed permanently in Arkansas than stayed permanently in Texas (Groen and Polivka, 2008b).

Pulaski county. As expected, the percentage of new out of state students is higher in Pulaski county since it is the most urban county in Arkansas and is at the intersection of the main highways and interstates that enter Arkansas from Louisiana and Texas. Figure 2.5 shows the spatial distribution of schools with the largest percent of new out of state students.

Figure 2.2: School Level Variation

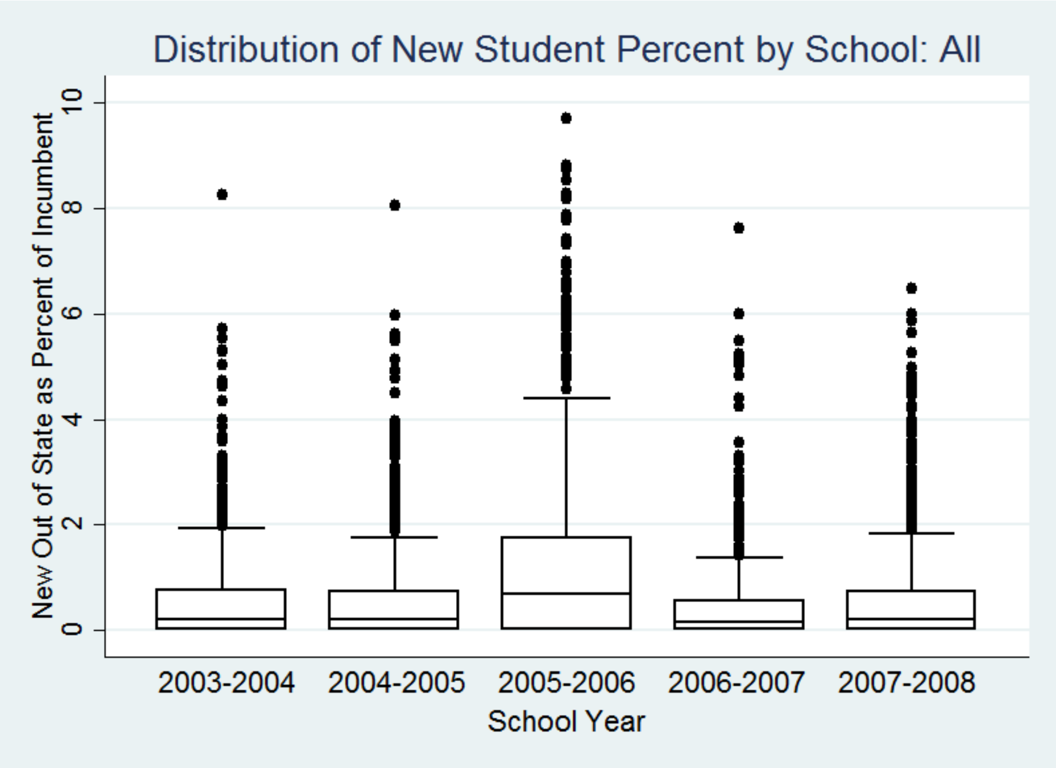


Figure 2.3: Highway District: School Level Variation

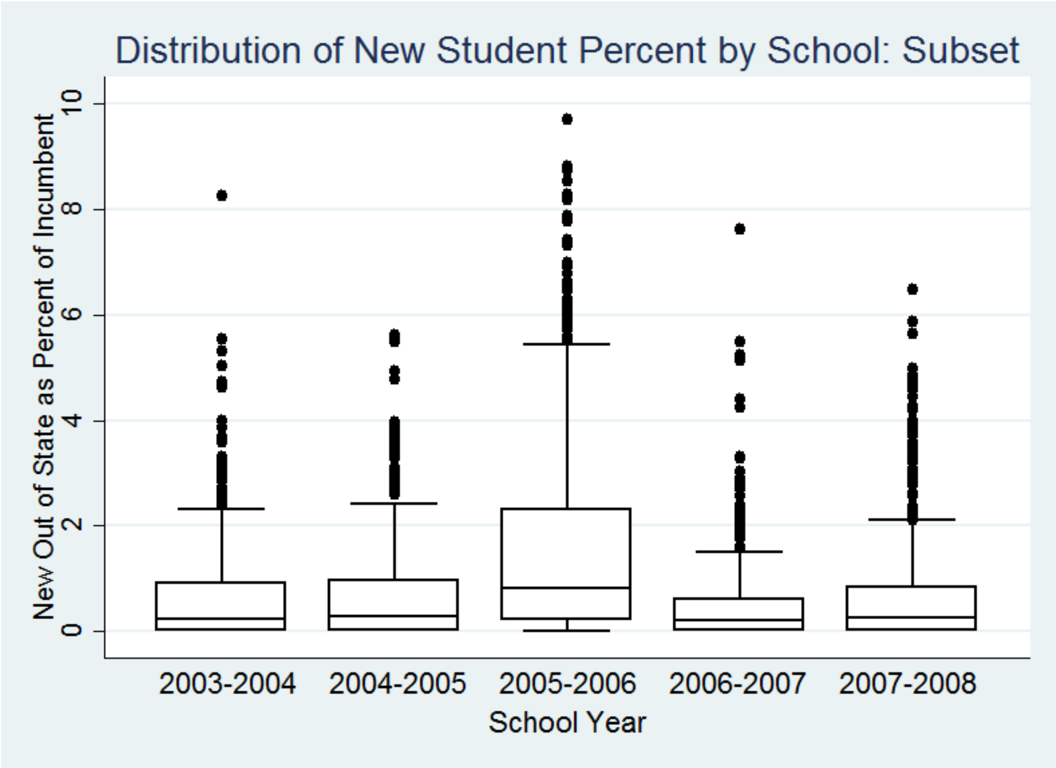


Figure 2.4: Pulaski County: School Level Variation

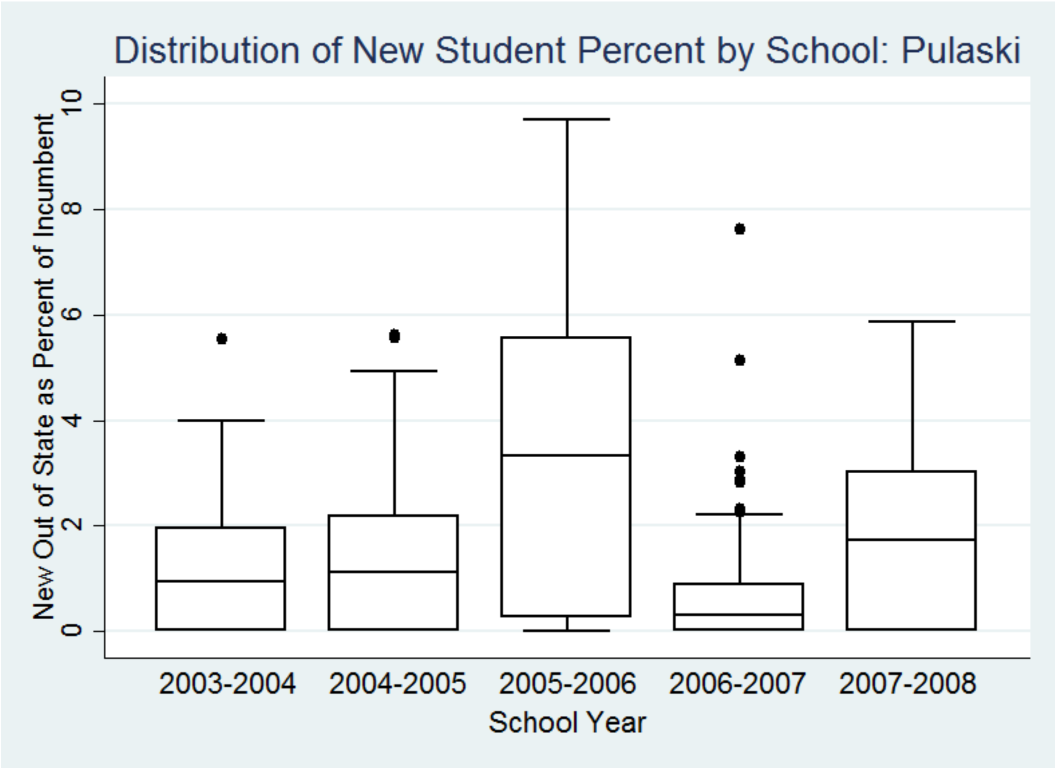
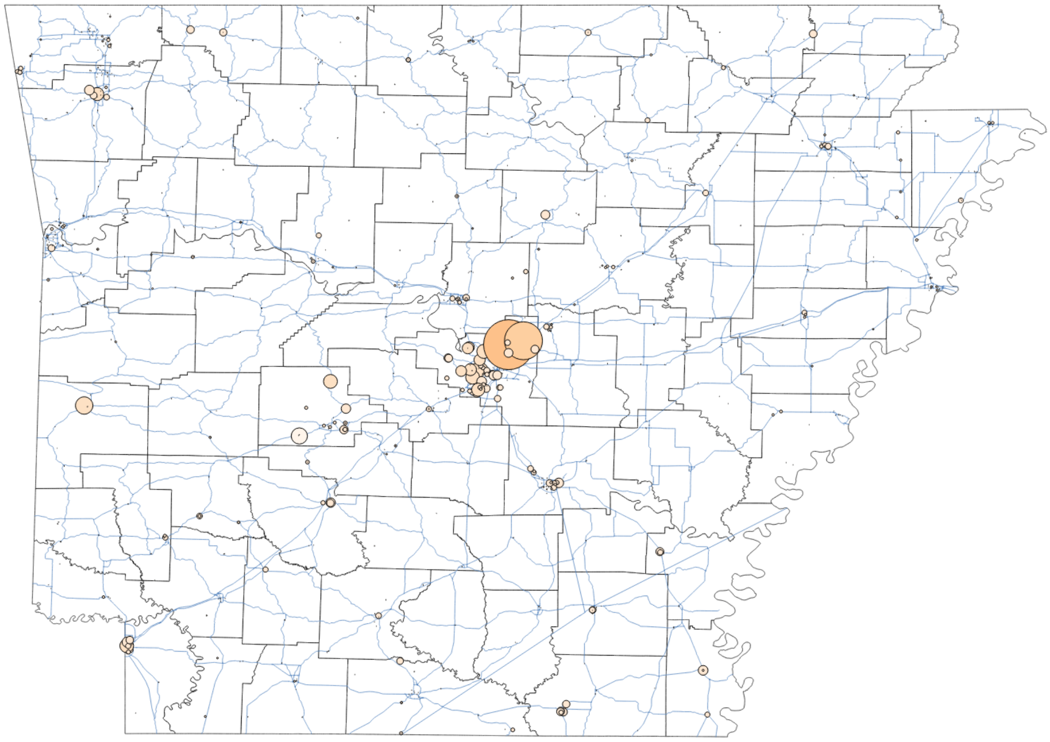


Figure 2.5: Location of Schools with Large Inflow of New Students



2.4.3 Outcome Variables

Two levels of analysis are used. First, the analysis estimates the response of district-level financial variables to district-level changes in the presence of new out of state students following Katrina. Second, the analysis estimates the response of individual-level incumbent student outcomes to school-grade level changes in the presence of new out of state students.

2.4.3.aDistrict Level School expenditure and revenue from NCES is used at the district level. Total expenditure, total revenue, total expenditure per pupil, and total revenue per pupil are used from the 05/06 and 06/07 school years. **2.4.3.bSchool/Grade Level** Yearly attendance

is calculated as average number of days present per quarter with quarters having fewer than 10 days present set to blank. By dropping quarters with fewer than 10 attendance I ensure the attendance measure is capturing observations from the school the student is currently attending and not dropout or transfer. Adjusting the cutoff of 10 days leads to very little change in the effect estimates.

English language and mathematics proficiency is provided by The Arkansas Comprehensive Testing, Assessment, and Accountability Program (ACTAAP) tests given to students in grades 3-8. The ACTAAP includes criterion-referenced tests for English language and mathematics. Tests from the 2003-2004 school year through the 2006-2007 school year are used.

Discipline infractions are recorded at the incident level for each student. Because a lack of infractions is not provided, I assign students a 0 for number of infractions if they are enrolled and present for the indicated school year.

2.4.4 Analysis Sample

The entire state of Arkansas did not receive uniform inflows of new students after Katrina. Particularly rural areas and those far from major highways received fewer new students. To limit the sample to those areas receiving new students I limit the sample to those counties along interstates and major highways entering the state from Texas and Louisiana.[6] Table 3.1 shows that the mean of the treatment variable in the Highway Counties is more than double the mean for the Non-Highway Counties. Additionally, Pulaski county (which contains Little Rock) saw far larger inflows of students than the rest of the state. The two analysis samples used are those counties along major highways and only Pulaski county.

[6] Alternatively, the limited sample could be defined as those counties with county mean(or median) inflow above the median of counties. Using other sample definitions does not appreciably change the results.

Table 2.1: Sample Description

	All Counties	Highway Counties	Non-Highway Counties	Pulaski County
Percent of Students	100.0	68.6	31.4	13.6
Average Number of Students				
Per District [n]	1,295.4	1,691.1	856.8	4,399.4
Per School [n]	314.9	334.6	278.9	296.1
Mean Percentage Points Above Normal Proportion of New Out of State Students				
2006 [%]	0.81	0.97	0.46	2.61
Black [n (%)]	55,562 (24.5)	35,648 (22.9)	19,913 (28.0)	18,437 (59.9)
Hispanic [n (%)]	12,938 (5.7)	11,349 (7.29)	1,589 (2.23)	1,266 (4.1)
White [n (%)]	153,743 (67.8)	104,563 (67.2)	49,180 (69.16)	10,618 (34.5)
Other [n (%)]	4,456 (2.0)	4,024 (1.8)	2,021 (2.8)	475 (1.5)
Female [%]	49.0	49.1	48.9	49.8
K-6 [%]	55.4	55.6	54.9	59.1
7-11 [%]	44.6	44.4	45.1	40.9
Free/Reduced Lunch [%]	53.3	49.2	62.2	56.7
Special Education [%]	13.1	12.7	13.9	18.8
Migrant [%]	1.1	0.9	1.48	0.0
English Sec. Lang. [%]	3.9	5.3	0.75	3.2
Schools [n]	720	465	255	104
Districts [n]	175	92	83	7
Counties [n]	59	24	35	1
Year of Most Recent Attendance Data				
2005 [%]	98.0	98.1	97.8	98.6
District Stats				
Total Revenue ’05 [$]	17,589,552	24,746,568	10,577,121	143,346,256
Total Expenditures ’05 [$]	18,113,530	26,108,886	10,279,697	136,811,744
Total Rev Per Pupil ’05 [$]	8,523	8,429	8,615	9,376
Total Exp. Per Pupil ’05 [$]	8,575	8,773	8,381	8,719

The analysis described below relies on within grade and within school variation and thus does not need counties with very low inflow of students. Since the treatment is not at the county or district level, I do not need treated and untreated counties for comparison and can thus cut the untreated counties from the analysis.

2.4.4.aHighway Counties Subset Below are the counties included in the Highway Counties sample with major highway access. I define a county as having major highway access if an interstate or other large highway which originates on the southern border of Arkansas goes through the county or borders the county. As of the 2010 census, the Highway Counties of counties below contained approximately 60 percent of total population of Arkansas (1.76 million out of 2.92 million). Median household income from 2012 census estimates is $34,977 for the entire state and $37,561 for the subset of states with major highway access.

- I-49: Benton, Washington
- I-49 and I-40: Crawford
- I-40: Sebastian, Franklin, Logan, Johnson, Yell, Pope, Perry, Conway, Faulkner, Pulaski, Lonoke
- I-30: Saline, Garland, Hot Springs, Clark, Nevada, Pike, Hempstead
- I-530/Hwys. 425 and 65: Jefferson, Lincoln, Drew, Desha, Chicot, Ashley

The subset of states with highway access and the entire state have similar percent of students by race though the districts are larger in the Highway Counties (Table 3.1).

2.4.4.bPulaski County

Pulaski county contains approximately 13 percent of the total population of Arkansas. Pulaski county contains Little Rock (the capital and largest city in Arkansas) and North Little Rock. Pulaski county, with Median household income of $46,102 is much wealthier than most counties in the state.

2.4.5 Comparison

As table 3.1 shows, the entire state and the Highway Counties are very similar. Particularly the racial breakdown and average district size is very similar for both samples. For Pulaski county, the proportion of black students is much higher and the average district size is much larger. While the analysis was performed on all three samples, only the results from the Highway Counties are shown since this subset is both similar to the state as a whole and is most likely to have received meaningful numbers of new out of state students from Katrina.[7] ,[8]

To ensure that the same population is being used to evaluate changes in outcomes in both the year following hurricane refugee inflow and two years following hurricane refugee inflow, I limit the sample to those students with data available in both the 2005-2006 and the 2006-2007 school year and either the 2002-2003 or the 2003-2004 school years. This means that the highest grade-level included are students that are in 11th grade in the 2005-2006 school year. Additionally, using the same population across all years restricts English proficiency and math test scores used in the analysis to grades 4-7. It is possible that this requirement could produce an unrepresentative sample since some districts may have less robust data collection or some students may have dropped out between the 2005-2006 and 2006-2007 school years, however, removing the requirement that student data be available in the 2006-2007 year does not appreciably change the estimates from the 2005-2006 outcomes.

2.5 Identification

The same analysis is performed both at the district level, with district level outcomes and district level treatment, and at the school/grade level, with individual level outcomes and school/grade level treatment.

[7] Results are generally similar for the entire state and the Highway Counties.

[8] Following a ruling by the Arkansas Supreme Court in 2003 and subsequent state legislation, school districts with fewer than 350 students were forced to consolidate starting in the 2005-2006 school. I exclude from the analysis all districts consolidated and all districts which received the students from those districts. However, including these districts did not appreciably change the analysis results.

2.5.1 District Level

To test for student inflow effects on classroom environment at the district level, I regress district financial outcomes and district enrollment on $EvacueeFractionDistrict_k$ using the following regression:

$$Z_{kt} = \alpha + \beta EvacueeFractionDistrict_k + \delta_1 Z_{k2004} + \epsilon_k, \tag{2.2}$$

where Z_{kt} is the financial outcome of district k in school year t, Z_{k2004} is the one year lag of the outcome value. The above regression is fit separately for t=(2005-2006) and t=(2006-2007). Equation 3.1 is fit with total revenue, total expenditure, total revenue per pupil, total expenditure per pupil, and total district enrollment. In equation 3.1, β is the change in per student funding at the district level from an increase in new out of state inflow.

2.5.2 School/Grade Level

Following the specification used by Imberman et al. (2012), individual outcomes are regressed on $EvacueeFractionGrade_{jk}$ to test for peer effects from the change in peer group composition using the following regression on individual student data.

$$\begin{aligned} Z_{ijkt} &= \alpha + \beta EvacueeFractionGrade_{jk} + \delta_1 Z_{ijk2004} \times I_{ijk2004} \\ &\quad + \delta_2 Z_{ijk2003} \times (1 - I_{ijk2004}) + \Omega X_{ijk} + \Pi Grade_j + \Gamma School_k + \epsilon_{ijk}, \end{aligned} \tag{2.3}$$

where Z_{ijkt} is the outcome of interest for incumbent student i enrolled in grade j at school k in school year t, $Z_{ijk2004}$ and $Z_{ijk2003}$ are the one and two year lags of the outcome value, X_{ijk} are indicator dummies for individual characteristic variables such as sex, race, and special education status, and $Grade_j$ and $School_k$ are grade and school fixed effects. The above regression is fit separately for t=(2005-2006) and t=(2006-2007). Individual level outcomes include math and lit test scores, attendance, and discipline. In equation 3.2, β is the peer effect on individual students of a change in peer group composition from an increase in out of state inflow.

All regressions include the one year lagged value of the outcome variable (2004-2005 school year), however if the one year lagged value is missing I instead include the two year lagged value (2003-2004 school year). I interact the one and two year lagged values with an indicator variable that equals 1 if the most recent lagged outcome is in the 2004-2005 school year and equals 0 if the most recent lagged outcome is in the 2003-2004 school year. This gives different coefficients in the regression for the one and two year lags allowing for increased growth since the two year lag than the one year lag. The above indicator takes three forms depending on the level of the regression: I_{k2004} tells which lagged outcome is available for school k, I_{jk2004} tells which lagged outcome is available for grade j at school k, and $I_{ijk2004}$ tells which lagged outcome is available for incumbent student i in grade j at school k.

For the β in the above regressions to be causal, $EvacueeFractionSchool_k$ and $EvacueeFractionGrade_{jk}$ must be uncorrelated with unobserved school and grade characteristics after controlling for lagged outcomes, observed school/grade/individual characteristics, and school and grade fixed effects. With the inclusion of fixed effects and lagged outcomes, the increase in out of state students needs to be uncorrelated with *changes* in school quality and student performance that are unrelated to the student inflow. The identification, thus, does not require that the increase in out of state students be uncorrelated with school characteristics and baseline student outcomes.

In addition to fitting the above regressions separately for one year and two years post Katrina, I also fit the regressions separately by sex and by race and limit the sample to K-6th grade and 7th through 11th grades to see which groups in which grades are most affected.[9] ,[10] I also limit the sample to schools in Pulaski County as well as a Highway Counties.

To examine possible mechanisms, I regress several intermediate outcomes on the treatment variable. I test for an effect of treatment on logged enrollment, fraction minority, fraction in special education classes, and fraction receiving free or reduced lunch at the school/grade level. These

[9] As noted above, since I require data for both the 2005-2006 and 2006-2007 school years, the highest grade included in the analysis are students in 11th grade in the 2005-2006 school year.

[10] Splitting the grades into K-5 for elementary, 6-8 for middle school, and 9-11 for high school shows the similar results with middle and high school having similar effect estimates.

regressions use school/grade level data unlike the above outcomes which use student level data.

2.5.3 Falsification

As a falsification test I fit the above school/grade level regressions with same treatment variable as above, but with the outcome variables for the years prior to hurricanes Katrina and Rita. Because only two prior years of data is available, only the outcome variable from the year prior, not the most recent of the two prior years is used. If the effect of new student inflow prior to hurricanes Katrina and Rita is similar to that found after, this would be an indication that the schools/grades which received increased inflow were already experiencing differential trends prior to Katrina and Rita.

$$\begin{aligned} Z_{ijkt} &= \alpha + \beta EvacueeFractionGrade_{jk} \\ &\quad + \delta_2 Z_{ijk2003} + \Omega X_{ijk} + \Pi Grade_j + \Gamma School_k + \epsilon_{ijk}, \end{aligned} \tag{2.4}$$

As can be seen in equation 2.4, only two years of data are used in the estimation instead of the three years of data in the main specification. Using only 2002-2003 and 2003-2004 school year data in the falsification test limits the available grades with English proficiency and math test scores to grades 5 and 7 only instead of grades 4-7 used in the main specification.

2.6 Regression Results

2.6.1 District Level

At the district level, the effect on total revenue, total expenditure, total revenue per pupil, total expenditure per pupil, and total district enrollment are evaluated using equation 3.1. Coefficients from regressions of district financial data in the 2005-2006 school year (first year post Katrina) on treatment (inflow of new out of state students) are shown in table 2.2. There are no significant effects on district funding or district enrollment from the influx of new out of state students. Selection could be a problem in this regression if incoming students selected into districts that were seeing or were expected to see increases in revenue or expenditures, however if new students were selecting into districts with increasing revenue the effect on total revenue and total expenditures would be biased up. The bias would thus be more worrying if effect on total revenue and total expenditures were found to be significant and positive.

Table 2.2: Deviation from Normal Prop. New Out of State Students

dependent variable: District Level Financials					
	Tot Rev	Tot Exp	Tot Rev PP	Tot Exp PP	Count New and Incumbent Students
	2005-2006				
Diff. From Normal	-5,292,354	-653,628	8,922	14,603	-5149.9
	(12,591,410)	(37,331,851)	(6,615)	(10,538)	(5471.4)
N	97	97	97	97	99
	2006-2007				
Diff. From Normal	-42,281,027	27,382,513	5,607	15,333	445.2
	(26,193,518)	(54,710,779)	(13,035)	(9,221)	(1018.0)
N	94	94	94	94	99

robust std. err. are used
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

All regressions include controls for most recent pre-Katrina financial data.

2.6.2 School/Grade Level

Using equation 3.2, coefficients from regressions of incumbent student average quarterly attendance in the 2005-2006 school year on treatment (inflow of new out of state students) are shown in table 2.3. The regressions are fit with the Highway Counties and with Pulaski county alone. Pulaski county is the central Arkansas county that contains Little Rock and thus saw the greatest inflow of evacuees. In column (1) for both the Highway Counties and for Pulaski county only, the inflow of new students led to a significant decrease in attendance in the year following hurricanes Katrina and Rita. In the year following the inflow a 10 percentage point increase in inflow from new out of state students led to an average decrease in attendance of 0.76 days over the school year for the Highway Counties and a decrease in attendance of 1.40 days over the school year for Pulaski County. However, in both populations the effect is short lived with no significant impact on attendance two years following the inflow.

Table 2.3: Treatment: Deviation from Normal Prop. New Out of State Students

Column	(1)	(3)	(3)	(4)
	Dependent Variable			
	Attendance	Infractions	English	Math
Highway Counties				
2005-2006	-1.939*	-0.260	0.101	-0.245
	(0.906)	(0.286)	(0.439)	(0.502)
2006-2007	-0.138	0.002	0.742*	0.093
	(-0.866)	(0.433)	(0.342)	(0.459)
Pulaski County				
2005-2006	-3.498*	0.030	-0.561	-0.838
	(1.578)	(0.358)	(0.448)	(0.621)
2006-2007	-0.246	0.280	0.076	0.705
	(-1.196)	(0.494)	(0.389)	(0.655)

std. err. clustered at the school level
* p<0.05, ** p<0.01, *** p<0.001

All regressions include school and grade fixed effects, controls for most recent pre-Katrina outcome data, and student demographic controls.

There are no significant effects on student infractions from the inflow of new out of state students (column (2)). Using test scores, both English proficiency and math show no significant effects of student inflow in the year following Katrina and Rita (columns (3) and (4)). Two years following Katrina and Rita there is a significant increase in English test scores among incumbent students in the Highway Counties from the inflow of new out of state students. A 10 percentage point increase in the inflow of new out of state students led to an average increase in test score of 0.0742 points. To understand this increase, 0.0742 points was approximately the difference between the 50th and 53rd percentile. While no other test scores showed a significant effect from inflow of new out of state students, both English scores in Pulaski county and math scores in both populations show the same direction in estimates as attendance with negative estimates in the year following Katrina and Rita and no effect or an increase in test scores two years after Katrina and Rita.

The effect of inflow of new students on attendance is not consistent across grade levels. In table 2.4 column (1) the significant effect of student inflow on attendance is only found for grades K-6 and not for higher grades.[11] For students in grades 7-11, the effect is small and not significant, but for those in K-6 the estimate is -2.687. On average this is a drop in attendance of 1.07 days over the school year. Similarly, among male students the effect is much larger than among women (columns (2) and (3)). The effect is larger and significant for male students when running the regression with all grades and with only K-6; the estimated effects for male students for all grades and for K-6 are -2.543 (1.02 days average decrease in yearly attendance) and -3.154 (1.26 days average decrease in yearly attendance). While the effect magnitude is similar for white students only and black students only, the effect is only significant for white students (columns (4) and (5)). In all cases whether limiting the regression by sex or race, when looking at all grades the effect estimates show a decrease in attendance in the first year after Katrina and Rita with minimal effect two years following the new student inflow. The same is seen when looking at only K-6th grades

[11] As noted above, splitting the grades into K-5 for elementary, 6-8 for middle school, and 9-11 for high school shows the similar results with middle and high school both having effect estimates near zero.

while 7-11th grades show no consistent decline in the year following or two years following Katrina and Rita.

Table 2.4: Treatment: Deviation from Normal Prop. New Out of State Students

Column	(1)	(2)	(3)	(4)	(5)
	Dependent Variable Highway Counties Attendance All	Male Only	Female Only	White Only	Black Only
2005-2006	-1.939*	-2.543*	-1.232	-1.767*	-2.00
	(0.906)	(1.114)	(1.189)	(0.883)	(1.639)
2006-2007	-0.138	-0.044	-0.269	-0.544	2.244
	(-0.866)	(1.146)	(1.069)	(1.162)	(2.082)
2005-2006: K-6th	-2.687*	-3.154*	-2.208	-2.635**	-2.221
	(1.045)	(1.343)	(1.138)	(0.994)	(1.774)
2006-2007: K-6th	-0.142	-0.202	-0.145	0.669	0.238
	(-1.536)	(1.116)	(1.034)	(1.079)	(1.366)
2005-2006: 7th-11th	-0.068	-0.930	1.12	-0.259	-0.253
	(1.935)	(2.289)	(2.496)	(2.289)	(4.020)
2006-2007: 7th-11th	0.061	0.851	-1.456	-2.907	4.433
	(3.397)	(4.315)	(3.346)	(4.433)	(6.283)

std. err. clustered at the school level

* $p<0.05$, ** $p<0.01$, *** $p<0.001$

All regressions include school and grade fixed effects, controls for most recent pre-Katrina outcome data, and student demographic controls.

Fitting the regressions for English and math scores separately by sex (table 2.4), the estimated effect of new student inflow on test scores is more negative for female students only than for male students only. In Pulaski county, as above, the estimated effect of new student inflow is significant and negative the year following Katrina and Rita, but not significant two years following. Given the effect size of -1.109 for English test scores, a 10 percentage point increase in new student inflow would be a decrease in test score of 0.1109 which is approximately the difference between the 50th percentile and the 55th percentile. Given the effect size of -1.334 for math test scores, a 10 percentage point increase in new student inflow would be a decrease in test scores of 0.1334 which is approximately the difference between the 50th percentile and the 57th percentile.

Table 2.5: Treatment: Deviation from Normal Prop. New Out of State Students

Column	(1)	(2)	(3)	(4)
	Dependent Variable			
	Highway Counties		Pulaski County	
	Male Only	Female Only	Male Only	Female Only
	English			
2005-2006	0.492	-0.314	0.0087	-1.109*
	(0.494)	(0.500)	(0.553)	(0.534)
2006-2007	0.885*	0.572	0.176	0.0485
	(0.374)	(0.43)	(0.498)	(0.523)
	Math			
2005-2006	0.124	-0.714	-0.421	-1.334*
	(0585)	(0.539)	(0.677)	(0.668)
2006-2007	0.218	-0.158	-0.663	-0.780
	(0.503)	(0.499)	(0.783)	(0.689)

std. err. clustered at the school level
* p<0.05, ** p<0.01, *** p<0.001

All regressions include school and grade fixed effects, controls for most recent pre-Katrina outcome data, and student demographic controls.

Table 3.6 shows the effect estimates of new inflow on the intermediate outcomes of logged enrollment, fraction minority, fraction in special education classes, and fraction receiving free or reduced lunch at the school/grade level. Further examination is needed to determine if these intermediate effects provide evidence for a potential mechanism.

Table 2.6: Treatment: Deviation from Normal Prop. New Out of State Students

Column	(1)	(3)	(3)	(4)
	Dependent Variable Fraction Minority	Fraction Special Education	Fraction Free/Reduced Lunch	Enrollment (Log)
Subset of Counties				
2005-2006	0.020	-0.004	-0.027	0.014
	(0.041)	(0.020)	(0.034)	(0.070)
Pulaski County				
2005-2006	-0.037	-0.020	-0.068***	0.122
	(0.022)	(0.016)	(0.014)	(0.089)

std. err. clustered at the school level
* p<0.05, ** p<0.01, *** p<0.001

All regressions include school and grade fixed effects.

2.6.3 Falsification

Table 2.7 shows the results of the falsification test. The only significant result is an positive effect on attendance, however this is the opposite sign from that found in the above analysis. If the schools/grades which received more new students were already experiencing an increase in attendance, this would bias the results above towards zero. Alternatively, there could be regression to the mean effect in which the schools/grades which saw large inflows of students experienced drops in attendance post Katrina/Rita because these schools/grades were coming down from the gains in attendance prior to Katrina/Rita.

Table 2.7: Falsification: Treatment: Deviation from Normal Prop. New Out of State Students

Column	(1)	(3)	(3)	(4)
	Dependent Variable			
	Attendance	Infractions	English	Math
Subset of Counties				
2005-2006	1.138***	-0.182	-0.454	0.889
	(0.22)	(0.19)	(1.65)	(2.50)
Pulaski County				
2005-2006	1.284***	-0.175	-3.005	0.0858
	(0.29)	(0.19)	(2.27)	(1.43)

std. err. clustered at the school level
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

All regressions include school and grade fixed effects, controls for most recent pre-Katrina outcome data, and student demographic controls.

2.7 Discussion

The effect of Katrina/Rita inflow on incumbent students is generally limited to attendance outcomes. The effect is also short lived with a significant effect only in the first year post Katrina. Additionally, the effect is strongest among elementary aged white students and elementary aged male students.

The effects on Katrina/Rita inflow on incumbent attendance described above are smaller in magnitude than those found in Imberman et al. (2012) for middle/high school students but larger in magnitude than those found in elementary school students. The only significant effect of evacuee inflow in Imberman et al. (2012) related to attendance is a decline among black students in middle school and high school in Houston. While the magnitudes are similar, the effected group changes. Instead of black middle/high school students showing a significant drop in attendance I find the largest drop in attendance in elementary school and among male students and white students.

There is little evidence of Katrina/Rita inflow on test scores or disciplinary infractions (excepting female students in the first year post Katrina which is not significant two years post Katrina). The short lived effect on attendance and lack of effect on other outcomes indicate that the new students displaced by hurricanes Katrina and Rita had a minor and transitory effect on incumbent students. The new students may have had a disrupting effect on classrooms as new students which led to a drop in attendance, but was too short and benign to effect other outcomes.

The lack of significant effect of Katrina/Rita inflow on district financial outcomes indicates that the effects on attendance are not caused by resource constraints from increased attendance. The effect on inflow on district level enrollment is also not significant and thus would likely not cause an overall strain on resources at the district level. [12]

As noted above, the drop in attendance post Katrina/Rita could be driven by a regression to the mean effect where the schools/grades which saw gains in attendance prior to Katrina/Rita saw drops in attendance post Katrina/Rita. The falsification also allow for the possibility that the

[12] However, as noted above, the regressions at the district level are less robust to selection bias than the regression on school/grade level outcomes which contain grade and school fixed effects.

schools/grades which received the most students were experiencing attendance gains and if those attendance gains continued post Katrina/Rita, the lack of significant effect of student inflow two years post Katrina/Rita could be caused by the downward bias of the prior trend.

2.8 Conclusion

Using the inflow of evacuees following hurricanes Katrina and Rita, I examine the effect of new student inflow on incumbent student outcomes. Unlike previous analysis of peer effects from Katrina and Rita evacuees, using data from Arkansas allows for the analysis to be done in a state with no internally displaced students.

I find that the effects of new student inflow are primarily limited to attendance with no consistent effect of on test scores or disciplinary infractions. This indicates that the inflow of students, while disruptive, did not lead to changes in scholastic outcomes. The effect on attendance is also short lived with no significant effect on attendance two years following the inflow of new students.

From an education policy or school administrative perspective, the evidence from Arkansas post Katrina and Rita indicates that any disruption and adverse outcomes from the migration of students will be short lived and any measures taken to smooth the transition must be taken as soon after new student inflow as possible in order to be useful.

Chapter 3

Peer Effects from School Consolidation

3.1 Introduction

In this paper I examine the effect of peer group composition of student outcomes. I exploit the court mandated consolidation of Arkansas school districts with fewer than 350 students. The influx of new students changes the peer group composition of incumbent students and has two important characteristics. First, the new students and incumbent students are very similar demographically. The consolidated schools are in nearby and similar districts. Second, other than the change in school, new students have undergone a relatively minor disruption to their lives. They are living in the same home with the same family, friends, and neighbors. These two attributes allow for an examination of the effects of the introduction new student on incumbent students outcomes that is more closely tied to new student achievement. The incumbent students will not be reacting to demographic changes in the classroom and the behavior and achievements of the new students will not be influenced by having moved, possibly fled, their previous homes which could have been in another state or another country. The court ordered consolidation thus gives an exogenous shift in students large in scale with many students changing schools yet benign in implementation with the lives of the new students otherwise unchanged.

I assess the effect of peer group composition on incumbent student with a broad set of outcomes including attendance, mathematics and English language proficiency, and disciplinary infractions.

I regress individual level outcomes on the percent of new students in a school/grade from consolidation, the average prior year achievement of consolidated students, and the interaction between the two. I find that in the first year after consolidation, among high school students, the effect of average prior attendance increases with increasing new student inflow and that the effect of new student inflow increases with increasing prior average attendance. For male students in the first year after consolidation and for all students two years after consolidation, I find a similar significant effect on math scores where the interaction term is significant and positive.

3.2 Related Literature

Three papers are most directly related to mine. First, Imberman et al. (2012), examines peer effects from hurricane evacuees in Louisiana and Houston. Second, Hoxby and Weingarth (2005) examines peer effects using students reassigned to schools in Wake County. Students were initially reassigned to balance schools' racial composition and later to balance schools' income composition. Third, Gould et al. (2009) examines peer effects using the the mass migration to Israel in the 1990s.

This paper expands beyond the existing literature in two ways. First, I am examining an exogenous movement of students that are very similar demographically to the incumbent students. Second, unlike the student movement in Imberman et al. (2012) and Gould et al. (2009), I am using student movement that arises from a minimally disruptive shock. These two attributes mean that any peer effects identified are less likely to be driven by reactions to classroom demographic changes and that the new students are more likely to react and behave similarly to incumbent students.

3.3 Data

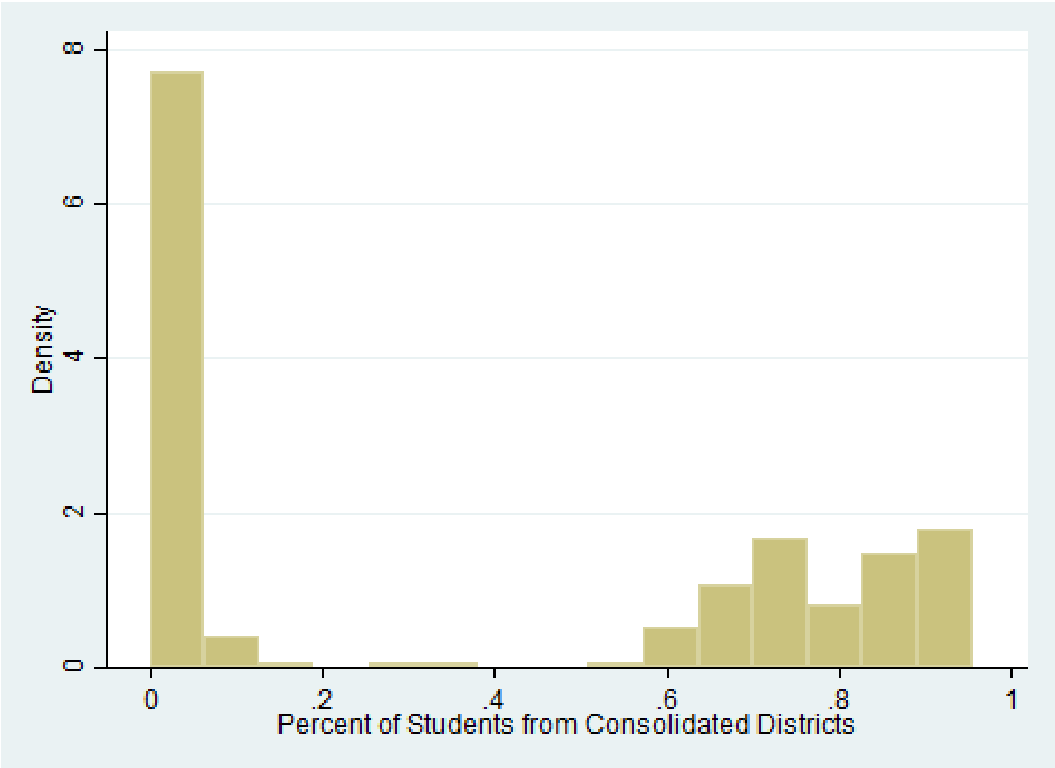
3.3.1 Arkansas Consolidation

Following a decade of litigation, the Arkansas Supreme Court ruled the state's school funding system was unconstitutional. Arkansas Act 60 was passed in early 2004 and required districts with average daily attendance lower than 350 students for two consecutive years to consolidate. The majority of district consolidations occurred immediately after passage of Act 60.[1]

[1] Mills and McGee (2013)

Consolidated districts are those with fewer than 350 students that closed because of passage of Act 60. Receiving districts, which contain the incumbent students used in the analysis, are those districts which received the students from the consolidated districts. Consolidated districts chose which district to send their students to, however the analysis below is based on variation across grades and schools and does not require the choice of districts to be random. Figure 3.1 shows the distribution of new students into the receiving district schools.

Figure 3.1: Distribution of New Students into Receiving Districts



3.3.2 Sources

The Arkansas Department of Education Statewide Information System (SIS) provides enrollment data with enrollment date and drop date. Additionally, the SIS also provides quarterly attendance as days present and absent, discipline incidents, standardized test scores including English language and mathematics proficiency, and demographics for all K-12 students in Arkansas matched yearly from the 2003-2004 school year through the 2007-2008 school years. To examine peer effects from consolidation I use data from the 2003-2004 school year through the 2006-2007 school year. Student data is matched by student ID number from year to year.

The National Center for Education Statistics (NCES) provides additional data at the grade, school, district, and state level. I use the following yearly district financial data from NCES: total district revenue, total district expenditures, total district revenue per pupil, and total district expenditures per pupil.[2]

The treatment variable that represents peer group composition changes is the fraction of students in a school/grade that switched school districts because of consolidation starting in the 2004-2005 school year.

The sample is limited to those school districts which received new students because of consolidation. The sample is also limited to incumbent students, defined as students in the same district in both the 2003-3004 and 2004-2005 school years. Table 3.1 compares the population of incumbent students with the population of new students from consolidation. The new students are generally from poorer districts with a larger percentage of students receiving free or reduced lunch and with more schools being title 1.

[2] Much of the data from the SIS contains duplicates, the majority of which are exact duplicates of all data though some have differing school and attendance values. In the cases where the duplicates are not exact I use the yearly observation with the largest number of days accounted for (absent plus present).

Table 3.1: Sample Description

	Incumbent Students	Consolidated Students
Total	15,299	7,936
Limited English Proficiency	1.21%	0.63%
Migrant	1.10%	1.21%
Special Education	12.1%	15.1%
Gifted and Talented	8.42%	10.1%
Title 1	31.1%	60.7%
Free or Reduced Lunch	50.0%	64.5%
Male [n (%)]	7,827 (52.3)	4,066 (51.3)
Female [n (%)]	7,134 (47.7)	3,866 (48.7)
White [n (%)]	11,862 (77.5)	6,306 (79.5)
Black [n (%)]	3,006 (19.7)	1,334 (16.8)
Hispanic [n (%)]	302 (2.0)	125 (1.6)
Other [n (%)]	129 (0.8)	171 (2.15)
Math Score 2004 [mean (sd)]	0.074 (0.95)	0.020 (0.96)
English Score 2004 [mean (sd)]	0.051 (0.98)	-0.040 (0.95)
Average Qtr. Attendance 2004 [mean (sd)]	42.2 (2.3)	42.3 (2.3)
Grades K-5 [n (%)]	6,412 (42.9)	3,929 (49.5)
Grades 6-8 [n (%)]	4,827 (32.3)	2,433 (30.7)
Grades 9-11 [n (%)]	3,722 (24.9)	1,570 (19.8)

3.3.3 Outcome Variables

Two levels of analysis are used. First, the analysis estimates the response of district-level financial variables to district-level changes in the presence of new out of state students following Katrina. Second, the analysis estimates the response of individual-level incumbent student outcomes to school-grade level changes in the presence of new out of state students.

3.3.3.aDistrict Level School expenditure and revenue from NCES is used at the district level. Total expenditure, total revenue, total expenditure per pupil, total revenue per pupil, and total district enrollment are used from the 05/06 and 06/07 school years. **3.3.3.bSchool/Grade Level**

Yearly attendance is calculated as average number of days present per quarter with quarters having fewer than 10 days present set to blank. By dropping quarters with fewer than 10 attendance I ensure the attendance measure is capturing observations from the school the student is currently attending and not dropout or transfer. Adjusting the cutoff of 10 days leads to very little change in the effect estimates.

English language and mathematics proficiency is provided by The Arkansas Comprehensive Testing, Assessment, and Accountability Program (ACTAAP) tests given to students in grades 3-8. The ACTAAP includes criterion-referenced tests for English language and mathematics. Tests from the 2003-2004 school year through the 2006-2007 school year are used.

Discipline infractions are recorded at the incident level for each student. Because a lack of infractions is not provided, I assign students a 0 for number of infractions if they are enrolled and present for the indicated school year.

3.4 Identification

A similar analysis is performed both at the district level, with district level outcomes and district level treatment, and at the school/grade level, with individual level outcomes and school/grade level treatment.

3.4.1 District Level

To test for student inflow effects on classroom environment at the district level, I regress district financial outcomes and enrollment on $NewStudentFraction_k$ using the following regression:

$$Z_{kt} = \alpha + \beta NewStudentFraction_k + \delta_1 Z_{k2004} + \epsilon_k, \tag{3.1}$$

where Z_{kt} is the outcome of district k in school year t, Z_{k2004} is the one year lag of the outcome value. The above regression is fit separately for t=(2004-2005) and t=(2005-2006). Equation 3.1 is fit with total revenue, total expenditure, total revenue per pupil, total expenditure per pupil, and total district enrollment. In equation 3.1, β is the change in per student funding at the district level from an increase in new students from consolidation.

3.4.2 School/Grade Level

Following the specification used by Imberman et al. (2012), individual outcomes are regressed on $NewStudentFraction_{jk}$, $NewStudentAvgZ_{jk2003}$, and the interaction between $NewStudentFraction_{jk}$ and $NewStudentAvgZ_{jk2003}$ to test for peer effects from the change in peer group composition using the following regression on individual student data. $NewStudentFraction_{jk}$ is the percentage of the grade j at school k which are new from consolidation and $NewStudentAvgZ_{jk2003}$ is the average prior year outcome of the new students in grade j at school k.

$$\begin{aligned} Z_{ijkt} &= \alpha + \beta NewStudentFraction_{jk} + \psi NewStudentAvgZ_{jk2003} \\ &\quad + \phi NewStudentFraction_{jk} \times NewStudentAvgZ_{jk2003} \\ &\quad + \delta_1 Z_{ijk2003} + \Omega X_{ijk} + \Pi Grade_j + \Gamma School_k + \epsilon_{ijk}, \end{aligned} \tag{3.2}$$

where Z_{ijkt} is the outcome of interest for incumbent student i enrolled in grade j at school k in school year t, $Z_{ijk2003}$ is the one year lag of the outcome value, X_{ijk} are indicator dummies for individual characteristic variables such as sex, race, percent change in grade size from previous year, and special education status, and $Grade_j$ and $School_k$ are grade and school fixed effects. The above regression is fit separately for t=(2004-2005) and t=(2005-2006). Individual level outcomes include math and lit test scores, attendance, and discipline. In equation 3.2, β is the peer effect on individual students of a change in peer group composition from an increase in out of state inflow. All regressions include the one year lagged value of the outcome variable (2004-2005 school year).

In addition to fitting the above regressions separately for one year and two years post consolidation, I also fit the regressions separately by sex and limit the sample to K-5th grades, 6-8th grades, and 9th through 11th grades to see which groups in which grades are most affected.[3]

In order to determine what mechanisms may be driving the effects on incumbent students, I regress several intermediate outcomes on the treatment variable using school/grade level observations and the same school/grade level treatment as above. I look at fraction minority, fraction receiving free or reduced lunch, fraction in special education and logged total enrollment.

3.5 Results

Using equation 3.1, district financials and enrollment are regressed on percent of the district which are new students from consolidation and shown in table 3.2. As expected with a large inflow of new students, total enrollment increases drastically. A one percent increase in new students from consolidation actually leads to a greater than one percent increase in enrollment indicating that the districts which see the largest inflow from consolidation are already growing. Total revenue and total expenditure both increase significantly with inflow. A one percent increase in inflow leads to a larger than one percent increase in total revenue and total expenditure. On a per pupil basis, revenue and expenditure are increasing, with a one percent increase in inflow leading to a roughly 0.3 percent increase in per pupil revenue and per pupil expenditure.

[3] I do not separate by race because the districts are predominantly white.

Table 3.2: Treatment: Proportion of Consolidated Students in District [Logged Outcomes]

	Total Revenue	Total Expenditure	Total Revenue PP	Total Expenditure PP	Enrollment
2004-2005	1.026***	1.232***	0.298**	0.304	1.341***
	(0.138)	(0.299)	(0.095)	(0.172)	(0.111)
2005-2006	1.338***	1.111**	0.250**	0.292*	1.351***
	(0.277)	(0.394)	(0.080)	(0.132)	(0.182)
N	37	37	37	37	37

robust std. err.

* $p<0.05$, ** $p<0.01$, *** $p<0.001$

Equation 3.2 is used to regress individual level outcomes on the new student percentage, new student average prior year achievement, and the interaction of the two. Table 3.3 shows the results with attendance as the outcome variable. For all students, male only, and female only, there is no significant effect of new students. For 9-11th grade students, effect of new student prior year attendance is significant and neg and the interaction term is positive and significant. The effect of new student prior achievement varies with percent of new students. Holding percent of students from consolidation constant at 10 percent, a one day increase in average prior year attendance of consolidated students leads to an increase in incumbent student attendance of 0.24 days. Holding percent of students from consolidation constant at 20 percent, a one day increase in average prior year attendance of consolidated students leads to an increase in incumbent student attendance of 0.54 days. Holding average prior year attendance of consolidated students constant at 39 days, a 10 percent increase in inflow leads to a decrease in incumbent attendance of 1.3 days per school year. Holding average prior year attendance of consolidated students constant at 41 days, a 10 percent increase in inflow leads to a increase in incumbent attendance of 1.1 days per school year.

Table 3.3: Outcome: Attendance

	All	K-5	6-8	9-11	Male	Female
2004-2005						
Percent Inflow From Consolidation	-12.71	22.67	219.2	-120.4*	-1.605	-12.03
	(28.55)	(29.01)	(133.7)	(54.59)	(46.64)	(41.76)
Avg. Attendance of Consolidated Students in Grade/School	-0.005	0.003	0.183	-0.057**	-0.017	0.013
	(0.010)	(0.015)	(0.118)	(0.015)	(0.018)	(0.018)
Percent Inflow x Avg. Att. of Consol. Stu.	0.360	-0.489	-5.141	3.003*	0.101	0.334
	(0.672)	(0.682)	(3.157)	(1.262)	(1.109)	(0.980)
Observations	15,523	6,703	5,061	3,756	8,125	7,398
2005-2006						
Percent Inflow From Consolidation	-48.62	17.00	310.5	-115.07	-2.457	-80.86
	(52.67)	(26.79)	(186.7)	(108.6)	(45.63)	(81.01)
Avg. Attendance of Consolidated Students in Grade/School	1.217	0.022	0.084	-0.033	0.0004	0.003
	(0.017)	(0.013)	(0.130)	(0.025)	(0.015)	(0.030)
Percent Inflow x Avg. Att. of Consol. Stu.	1.217	-0.322	-7.083	2.874	0.130	1.992
	(1.232)	(0.613)	(4.390)	(2.529)	(1.062)	(1.898)
Observations	15,523	6,703	5,061	3,756	8,125	7,398

std. err. clustered at the school level
* p<0.05, ** p<0.01, *** p<0.001

All regressions include school and grade fixed effects, controls for most recent year's attendance data, and incumbent student demographic controls.

Table 3.4 shows the coefficient estimates of English test scores regressed on percent inflow, prior average English score of new students, and the interaction. There is little evidence that new student inflow had an effect on incumbent English test scores.

Table 3.4: Outcome: English Score

	All	Male	Female
2004-2005			
Percent Inflow			
From Consolidation	2.096	3.028	2.053
	(1.428)	(-0.024)	(1.693)
Avg. English Score of Consolidated			
Students in Grade/School	0.068	-0.024	0.177
	(0.088)	(0.106)	(0.108)
Percent Inflow x			
Avg. Lit. of Consol. Stu.	0.252	-0.968	0.427
	(0.532)	(2.604)	(0.428)
Observations	5,784	2,951	2,833
2005-2006			
Percent Inflow			
From Consolidation	-1.840	-1.099	-2.674
	(1.874)	(2.519)	(1.576)
Avg. English Score of Consolidated			
Students in Grade/School	0.030	0.024	0.111
	(0.127)	(0.163)	(0.109)
Percent Inflow x			
Avg. Lit. of Consol. Stu.	-0.471	-5.574*	-0.510
	(0.531)	(2.627)	(0.391)
Observations	5,784	2,951	2,833

std. err. clustered at the school level

* $p<0.05$, ** $p<0.01$, *** $p<0.001$

All regressions include school and grade fixed effects, controls for most recent year's attendance data, and incumbent student demographic controls.

Table 3.5 shows the coefficient estimates for Math test scores regressed on percent inflow, prior average English score of new students, and the interaction. Holding percent of students from consolidation constant at 10 percent, a 0.2 point (approximate difference between 50th and 60th percentile) increase in average prior year math score of consolidated students leads to an decrease in incumbent student math test score of 0.18 points. Holding percent of students from consolidation constant at 50 percent, a 0.2 point increase in average prior year math score of consolidated students leads to an increase in incumbent student math score of 0.07 points. Holding average prior year math scores of consolidated students constant at -0.5 points, a 10 percent increase in inflow leads to a decrease in incumbent math score of 0.27 points. Holding average prior year math scores of consolidated students constant at 0.5 points, a 10 percent increase in inflow leads to an increase in incumbent math score of 0.04 points. Two years after consolidation the interaction term is significant for all students with similar estimates for male and female students. Similar to male students in the first year, the effect of prior year average consolidated student test score is more positive as inflow increases and the effect of inflow becomes more positive as prior year average consolidated student test score rises.

Table 3.5: Outcome: Math Score

	All	Male	Female
2004-2005			
Percent Inflow From Consolidation	-0.405	-1.155	-0.605
	(1.373)	(1.093)	(1.705)
Avg. Math Score of Consolidated Students in Grade/School	-0.092	-0.120*	-0.108
	(0.061)	(0.051)	(0.084)
Percent Inflow x Avg. Math of Consol. Stu.	1.031	3.052***	1.132
	(0.985)	(0.828)	(1.266)
Observations	5,714	2,908	2,806
2005-2006			
Percent Inflow From Consolidation	2.732	1.858	2.953
	(1.518)	(1.505)	(1.982)
Avg. Math Score of Consolidated Students in Grade/School	-0.360***	-0.472***	-0.291**
	(0.083)	(0.054)	(0.104)
Percent Inflow x Avg. Math of Consol. Stu.	2.834**	3.106**	2.850
	(0.918)	(1.028)	(1.442)
Observations	5,714	2,908	2,806

std. err. clustered at the school level
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

All regressions include school and grade fixed effects, controls for most recent year's attendance data, and incumbent student demographic controls.

Table 3.6 gives the results of regressing the intermediate outcomes at the school/grade level on the inflow of consolidated students. There is no significant effect on fraction minority, fraction free or reduced lunch, or fraction in special education. There is a significant increase in enrollment which is to be expected with new inflow.

Table 3.6: Treatment: Proportion of Consolidated Students in District [Logged Outcomes]

	Fraction Minority	Fraction Free or Reduced Lunch	Fraction Special Education	Enrollment
2004-2005	0.237	0.425	0.119	0.902*
	(0.253)	(0.245)	(0.189)	(0.424)
N	536	536	536	536

robust std. err.
* p<0.05, ** p<0.01, *** p<0.001

3.6 Discussion

Analysis of financial data indicates that the districts receiving new students are not overly burdened. The increased funding more than makes up for the increase in students. Though it is possible that finding classroom space, available teachers, and other resources in the short term could still lead to crowding and overuse of available resources.

In both attendance outcomes among high school students and math score outcomes, as new student inflow from consolidation increases, the effect of prior year consolidated student achievement increases. Similarly, as prior year consolidated student achievement increases, the effect of new student inflow increases. The effect on attendance is short lived, being significant only in the first year after consolidation. The effect on math scores increases in the second year after consolidation and is significant among all students instead of only male students.

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