UNDERSTANDING EDUCATION: THREE ESSAYS ANALYZING UNINTENDED OUTCOMES OF SCHOOL POLICIES

By

JEREMY CLAYTON LUALLEN

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By

Jeremy Clayton Luallen

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Major Department: Economics

The goal of my study is to examine specific school policies to determine if unintentional consequences result from these policies. Specifically, I focus on two main issues as they relate to student and teachers outcomes. I begin by looking at the effect of incapacitating juveniles in school as a force influencing juvenile crime. I exploit teacher strikes as a measure of unexpected student absence from school to measure the effect of school in preventing juvenile crime. My data set consists of information on every juvenile arrest made in Washington State over a 22-year period. I show that previous estimates of the effect of school incapacitation are systematically underestimated, that criminal activity increases as students continue to remain out of school. I also show that these increases in crime reflect an increase in overall crime, not a displacement. Lastly, I show that repeat juvenile offenders are more likely to have committed their first crime on a strike day, relative to a normal school day.

Chapters 2 and 3 of the study focus on the role of teacher networks in influencing teacher mobility. Specifically, my study develops a model of teacher networks that describes how teachers assemble networks through professional development activities (PDAs) and how these networks provide an effective sorting mechanism for public school teachers. I empirically test the existence of teacher networks with 2 distinct datasets. The dataset in Chapter 2 is comprised of various reports covering all 67 Florida school districts. Besides examining how professional development affects teacher movement, I am able to exploit the macro nature of the data to compare district characteristics (such as differences in compensation levels and school district density) to examine how these factors also influence teacher mobility. The dataset in Chapter 3 uses survey data from the "Schools and Staffing Survey" and includes over 17,000 teachers. The high-powered nature of this dataset allows me to identify specific details, such as teacher salary incentives, individual network strength and union membership. Ultimately I conclude that teacher networks are an integral part of a teacher's transfer decision and have a sizable impact on intra-district teacher mobility.

CHAPTER 1
SCHOOL'S OUT…FOREVER: A STUDY OF JUVENILE CRIME, AT-RISK YOUTHS AND TEACHER STRIKES

1.1 Introduction

Although we have taken important strides in understanding the economics of crime, there remains a great deal which we have yet to fully understand. The focus of this chapter is juvenile crime in particular. Because juvenile crime can be especially hard to study, due to general limitations on access to data, economists still have a lot to explore on this front.

Over the past 20 years, economists and social scientists have attacked the problem of crime in four distinct ways. Specifically they have analyzed the potential effects of deterrence, retribution, rehabilitation and incapacitation as forces for reducing crime (Ehrlich 1981).[1] Deterrence, a widely explored topic, stresses the importance of imposing penalties as an effective means of preventing crime, because the perceived costs to criminals of criminal activity increases (Mocan and Rees 1999, Levitt 1998, Freeman 1996, Ehrlich and Gibbons 1977, Lochner 2003). Retribution addresses the actual punishment of criminals. It suggests that a criminal experiences an increased cost to his crime when a punishment is imposed on him and/or other criminals (Levitt 1998). Rehabilitation stresses the importance of reforming criminals, through treatment and rehabilitative programs, in preventing future crime (Cuellar, Markowitz and Libby 2003). Finally, incapacitation deals with the notion that the physical lock-up and detaining of

[1] I am not arguing that these four ideas are mutually exclusive ways of preventing or reducing crime.

criminals may be an effective means of preventing crime (Freeman 1996, Lochner 2004). This paper will closely examine the role of incapacitation in preventing juvenile crime.

Specifically I deal with the effect of incapacitating juveniles in school on juvenile crime. Brian Jacob and Lars Lefgren (2003) were the first to address this topic in their paper, "Are Idle Hands the Devil's Workshop? Incapacitation, Concentration and Juvenile Crime". They examine the effect of school attendance on juvenile crime by using teacher in-service days, days where teaching professionals are required to attend work when children are not required to be at school, as a source of variation in student attendance. While their use of in-service days is very clever, I utilize teacher strikes as a source of variation of student school attendance to gain additional insights into the relationship between juvenile crime and school incapacitation.

One primary advantage to using teacher strikes over in-service days as a source of variation in school attendance is that strikes often occur in blocks larger than one individual day. This allows me to observe the effect of prolonged absence from school on juvenile crime. If juvenile criminal habits do change as absence from school increases, then a one day absence cannot reflect the average effect of school incapacitation.

Another advantage of utilizing teacher strikes is that they are relatively unpredictable. Since in-service days are planned at the beginning of the regular school year, parents have adequate time to make arrangements for their children during these off-days. In contrast, school closures caused by teacher strikes are often reported in local newspapers only a couple of days before they are likely to take place, and even then, there is no guarantee that these reported strikes will actually occur. Additionally, a

parent's ability to plan for their children's activities during strike days is complicated by the fact that details revealed in newspapers and other information sources may prove to be inaccurate or unjustified.[2] The late-breaking and incomplete nature of information leaves parents little time to plan their children's activities during the days of a teacher strike. Thus teacher strike days are likely to result in more unsupervised students, and more juvenile crime.

The main drawback to this use of strike as variation is that teacher strikes are often very sparse, and mostly occur in a only a few school districts. In order to overcome this difficulty, I rely on data from Washington State. Washington provides an ideal environment for this type of study because it has an extensive strike history as well as a detailed juvenile arrest data set that dates back as far as 1980.[3]

1.2 The Identification Strategy

As stated earlier, the objective of this analysis is to measure the impact of school incapacitation on juvenile crime rates by using teacher strike days as variation in student absence from school on an ordinary school day. Arguably, teacher strikes are a source of variation that is exogenous to variations in juvenile crime. Issues that lead teachers to strike include pay, class size and teacher planning time, none of which are likely to influence daily variations in juvenile crime.[4]

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[2] For instance, a school district may report that it is sure teachers will return to work when a court injunction is issued, and subsequently the teachers may defy the injunction.

[3] Unlike most states in the U.S., teacher strike activity in Washington remains very much active to this day. The most recent strike event took place in 2003 in Marysville school district. This strike lasted nearly two months (approximately 50 total days). A summary table describing teacher strikes in Washington can be seen in Table A-1.

[4] In fact, areas where juvenile crime may be an increasing problem are arguably less-likely to see a teacher strike, because greater crime has been shown to be positively correlated with higher teacher pay (Grogger 1997).

In order to draw direct comparisons between crime on strike vs. nonstrike days, I must be able to control for days in which schools are closed for other reasons. This would include days that coincide with spring, summer and winter vacations, weekends, teacher in-service and half-days, national holidays, etc. However since the data cover a large span of time (22 years), and since each Washington school district determines their own school calendar, it is impossible to know where all of these nontypical school days occur.[5] Further, a prolonged teacher strike sometimes results in a reworking of the existing school calendar (largely unobservable) so that a school district can meet the requirement set by the state that the school year last for 180 days.[6] Since I cannot control for these school holidays with complete accuracy, I must include only those days which I am strongly confident students are regularly scheduled to be in school.

To minimize any possibility that school holidays could bias the results, I take extra measures when eliminating questionable days. I begin by eliminating weekends and national holidays.[7] If a national holiday falls on a Saturday or Sunday, I eliminate the preceding Friday or subsequent Monday, respectively. The first two Fridays of October are dropped because they are both traditional days for the state mandated teacher in-service day. The entire months of June and July were also excluded, and only the last three school days in August are allowed in the sample. Finally I eliminate the first week

[5]Each school district may vary on the scheduling of breaks, half-days, inservice days, etc. To the best of my knowledge, there is no state or federal agency that has archived these calendars over the 21 years. Since the advent of increased technological integration with school systems, some districts are beginning to electronically archive these calendars, however these calendars generally only go back a couple of years. The Washington State Superintendent of Public Instruction was able supply me with sufficient calendars for most districts, for each year from Aug. 2000 to June 2004.

[6] The shortening of spring and winter breaks is common, as well as the lengthening of the school year into summer vacation.

[7] Thanksgiving falls on Thursday, however both Thursday and Friday are dropped from the sample because students are given both of those days off from school.

in January, the last two full weeks (at least) of March, the first two weeks of April (at least 8 school days), and the last two full weeks (at least) of December. Based on these criteria, I can be reasonably certain that each observation in the analysis is a typical school day.[8] Days which are included in the sample but are not ordinary school days will serve to downwardly bias the results, because more juvenile crime should be naturally occurring on those days. This is only true because this measurement of strikes as a treatment is limited to the days where students have missed an expected day of school.[9]

1.2.1 Zip-Code Matching

My data set consists of individual-arrest data, where each juvenile is matched to his home address zip code. Since zip codes are set by the Address Information division of the U.S. Post Office, they look very different from other boundaries like county borders, congressional districts, and most importantly school districts. In order to match each juvenile (and his/her arrest) to his/her appropriate school district, I must address two problems. The first problem is that zip codes often spill over multiple school districts. Therefore it is often uncertain which school district a juvenile is in, given their home zip code. The second problem is that zip codes are redefined over time. First I will focus on how zip codes change, and how this problem is resolved so that identification strategy is preserved.

As cities and towns develop both inside and outside city limits, the Post Office creates new zip codes and new zip-code offices to handle increasing mail traffic. When

[8] This is not a perfect matching process because half days and minor differences between districts are impossible to pinpoint.

[9] The timing of strikes is very particular and seldom overlaps with nonschool days, except weekends.

these new zip codes are created, they are drawn strictly as subsets of existing zip codes.[10] The creation of these new zip codes makes zip-code fixed effects useless from one year to the next. In addition, it is also very difficult to pinpoint when new zip codes are created. However it is possible to see which zip codes were created and what preexisting zip code they were originally a part of. By using zip-code maps of Washington State from 1984 and 2000, I am able to map each subset zip code back into its original (1984) zip code.[11] Essentially I treat zip codes in Washington as if they are never partitioned, beginning in 1984. This method leaves me with 510 zip codes in total and a stable zip code definition over a 22 year time frame.

The second problem is matching these zip codes to their corresponding school district. Zip-code overlap with school districts make this matching process difficult, however the approach I take is sound, and fairly straight forward. I began by totaling the number of schools in each zip code, noting which district they serve.[12] I then divided the total number of schools in that zip code for a given district by the total number of schools in that zip code, regardless of their district. Essentially what I am left with is a probability, that I will call p^*, which reflects the probability that a child living in a particular zip code is also part of a particular school district. This p^* measure allows me to assign a proper strike treatment to zip codes.

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[10] So far I have not been able to find an instance in Washington State since 1984 where a zip code was created from two or more existing zip codes

[11] Four maps of Washington State zip codes were provided by the Western Economics Research Co., Inc. courtesy of the Suzzallo Library at the Washington Library. They encompass they following years: 1984, 1989, 1992 and 1994. Zip code maps of Washington State for the year 2000 are made available by the Census.

[12] I did this using a master list of schools (as of the 2003-04 school year) that includes each school's address, grade level served, school code and district name. This list was provided by the Office of Superintendent of Public Instruction in Washington State. It is also publicly available.

There are several features of zip codes that simplify this matching process. One helpful factor is that many zip codes do not overlap school district boundaries at all. This leaves out any guess work. Another helpful point is that zip codes and school districts often share a significant number of common boundaries. These shared boundaries occur where divisions seem logical, such as along a county boundary, or a major river or highway. These convenient features lessen the confusion of the matching process. It is also worth noting that not all zip codes contain schools.[13] In my sample there were four zip codes without any schools; two were in the heart of Seattle, and two were located in rural areas. Conveniently, none of these four zip codes overlapped school district boundaries, so assigning them to the appropriate school district was simple.

The last concern I am left with deals with how *school districts* change over time, if at all. If school district boundaries are changing frequently and dramatically over time, then the matching process breaks down. However, what I find is that school district boundaries are stable over time. In order for a district to change its boundaries, it must engage in costly and time-consuming legal processes, however that does not imply that such adjustments never occur.[14] In fact, the two most frequent changes to school district boundaries seem to be district consolidation and district dissolution.[15] Neither changes turn out not to be problematic. These changes only occur a total of four times over the

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[13] The zip codes I use in the sample are zip codes with residences. Many zip codes like business zips, P.O. Box zips, and other nonresidential zips, do not contain schools. These kinds of zips are not included in the data sample.

[14] For an outline of regulations and requirements surrounding district boundary changes, please refer to the publication, "Changing School District Boundaries: A Lay Person's Guide" published by the Washington State Board of Education in conjunction with the Office of Superintendent of Public Instruction

[15] Consolidation is when two or more districts form a new superdistrict. Dissolution is when a one or more districts are absorbed into existing districts.

course of the 22 years and involve districts which do not experience any teacher strikes. I treat these integrated districts as if they had never been separate.

The school district zip code matching process I invoke does not provide perfectly accurate matches in all cases, but I can be certain of how any mismatching will bias the results. If I say there was a strike in an unaffected zip code, the effect of the strike will be biased toward zero because juvenile crime should be unchanged. If I say that there was not a strike in an affected group, then the strike effect will fail to pick up any increase in juvenile crime rates. Any mismatching that arises from the imperfect matching process will downwardly bias the results.

1.2.2 The Basic Model

In this basic model I am attempting to explain changes in juvenile crime for ordinary school days as a function of teacher strikes. I can start by expressing a simple regression model in the following form:

$$\text{Juvenile School Day Crime} = \alpha + \beta(p^*)(\text{Teacher Strike})$$

Again, all of the information from the crime data is reported at the zip-code level, however the teacher strikes occur at the school-district level. To adjust I introduce p^* where p^* represents the probability that the student population of a zip code is treated by the teacher strike. Rather than have fixed effects to take into account differences across zip codes, I want to initially include specific zip-code characteristics to make sure the data are well-behaved. I consider income levels, welfare status, parental education, juvenile work status, juvenile gender, single parent households and community characteristics in this model. Also I need time-fixed effects to control for temporal changes over 22 years. I therefore include year, month and day fixed effects in the model.

The regression model now takes the form:

$$\text{Juvenile School Day Crime}_{myd} = \alpha + \beta_1(p^*)(\text{Teacher Strike})_{myd} + \ldots$$
$$+\ldots\beta_2(\text{Median Income})_{myd} + \beta_3(\text{Welfare})_{myd} + \beta_4(\text{Parent Education})_{myd}$$
$$+\ldots\beta_5(\text{Student Employment})_{myd} + \beta_6(\text{Juvenile Gender})_{myd} + \ldots$$
$$+\ldots\beta_7(\text{Urban})_{myd} + \beta_8(\text{Single Parent})_{myd}\ \delta_1(\text{Year}) + \delta_2(\text{Month}) + \delta_3(\text{Day})$$

Once I am satisfied that the data exhibit all of the normal properties one would expect from specifying a model of juvenile crime, I can then shift the analysis to include zip code fixed effects take into account differences which I cannot control for. When I move to this specification, the final regression model looks like the following:

$$\text{Juvenile School Day Crime}_{mydz} = \alpha + \beta_1(p^*)(\text{Teacher Strike})_{mydz} + \ldots$$
$$+\ldots\ \delta_1(\text{Year}) + \delta_2(\text{Month}) + \delta_3(\text{Day}) + \delta_4(\text{Zip})$$

1.3 Data

My juvenile-arrest data comes from "Washington Juvenile Court Case Records" made available by the National Juvenile Court Data Archive. It provides juvenile arrest data over 22 years, spanning from 1980 to 2001. This data set is both lengthy as well as highly detailed at the individual level. It provides the home address zip codes of the offenders, the nature of the crimes committed, the dates of the offenses (as well as the arrests), as well as many other important characteristics surrounding the reported arrests. In total when I include only Washington juveniles who were arrested for crimes that occurred during the ordinary school days preselected for my study, I have 401,864 arrest cases starting in 1980.

In addition to the juvenile arrest data for Washington State, I use data provided by the "Census 2000 Summary File 3" and "1990 Summary Tape File 3" to derive annual zip-code characteristics. These Census files give zip-code level information on population and school enrollment numbers, as well adult educational attainment, the number of households who are welfare recipients, type of community, median household

income and student employment. Because these characteristics are only available for these two periods (1990 and 2000) I trend them over the 22 years using an exponential function rather than a linear function to describe the path of the trend.[16]

Finally, teacher strike information came from the report "Public Employee Strikes in Washington" published in 2003 by the Public Employees Relations Commission in Washington State. It covers all Washington public employee strikes since 1967[17]; however since this publication lacks some important details about these strikes, it has been supplemented with specific information from news articles provided by the Associated Press and the Seattle Times to strengthen the accuracy of the treatment effect. These articles are useful in pinpointing specific details surrounding each strike such as whether the school remained opened with emergency substitutes, and what information was distributed to parents in districts where strikes occurred.

1.3.1 Methodology

Choosing a proper methodology for this analysis requires some extra care. Because the analysis is done at the daily level, a huge percentage of observations for any given day will be zero. The distribution of the dependent variable is skewed towards zero, making an OLS regression analysis inappropriate. This could be solved by using a poisson regression analysis, however the fit for a poisson regression for this dataset is poor.[18] Also the poisson analysis does not account for strangely behaved standard errors

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[16] I also trended these variables linearly, however when I do this I end up with some numbers that are not feasible do to the imprecise nature of this process. When I impose minimum and maximum constraints on these variables, I find that these results are insensitive to whether these variables are defined linearly or exponentially.

[17] This report began with the enactment of Public Employees Collective Bargaining Act in 1967.

[18] A preliminary test of the data shows that the dependent variable generally has a standard deviation roughly 3 times larger than the mean. In addition, I observe large values of the Chi squared terms in the

and overdispersion. In this case, overdispersion may take the form of a single juvenile committing several crimes on a single strike day, or a gang of juveniles committing what could be considered one crime. Ultimately the chosen method of regression analysis is a negative binomial regression analysis. This deals with problems of overdispersion that a poisson does not correct for.[19] The dependent variable is aggregate crime per day (crime count) in a zip code, and the exposure is total student population in said zip code.

1.3.2 Defining the Parameters

As described earlier, I begin by controlling for differences among zip codes directly rather than using zip-code fixed effects. Each variable is derived from data taken from the 1990 and 2000 Census. Since I have only two observations for each variable, I interpolate annual values using an exponential path function. I started by solving for the growth rate ($G_{i,\ zip}$) of each variable i_{zip}:

$$G_{i,\ zip} = ((Census_{i,\ zip\ 2000} / Census_{i,\ zip\ 1990})^{(1/10)}) - 1$$

Then I interpolated each year's observation according to the equation:

$$\text{Variable } i_{zip} \text{ in year } t = Census_{i,\ zip\ 1990} * (1 + G_{i,\ zip})^{(n)}, \text{ where } n = t - 1990$$

Overall this analysis uses 7 variables to capture differences in communities. Income is measured using Median Income, which comes directly from the Census data. The Welfare variable is a percentage of households in a zip code that currently receive public assistance income. Of course I expect that local poverty is a serious factor in juvenile crime, so both of these variables seem to be relevant (Mocan and Rees 1999). Juveniles from poor neighborhoods are more likely to be less educated, and therefore more likely to

poisson "goodness-of-fit" tests. Both of these characteristics suggest that the poisson model in not the correct model for these regressions.

[19] The overdispersion parameter alpha is shown to be significantly different from zero. Thus I can conclude that the negative binomial regression significantly different from the poisson regression.

be criminally active overall (Freeman 1992). Since juveniles from poor households have a smaller opportunity cost of committing crime, I expect Median Income to be negatively correlated with juvenile crime, and naturally the converse should be true for Welfare.

The Poor Parental Education variable is defined as the percent of adults (25 years+) who have not completed the equivalent of a high school education. My prediction here is that as adult dropout rates increase, juvenile crime increases. Because poor adult/parental education equates to less adult human capital, parents are likely to have lower demand for child quality (Becker and Tomes 1976, 1986). Therefore juveniles in these neighborhoods should also have a lower opportunity cost of crime. In addition, one can argue that poor adult/parental education may also imply that parents have fewer resources to provide monitoring for juveniles, less overall time to provide supervision of their own children (parents may be working long hours or more than one job), or possibly uninformed/undesirable parenting techniques.

Our Student Employment variable describes the percent of 16-19 year olds who are both employed and also enrolled in school. The prediction of this variable is not clear. On one hand, students who are employed in after-school jobs may develop a sense of responsibility from working a job, or that they may fear losing future wages should they be caught committing a crime. This suggests that Student Employment should negatively influence juvenile crime. On the other hand, juveniles who work may have greater access to reliable transportation, or less restrictive parents, both of which could contribute to a greater propensity to commit crime. In this case, Student Employment would positively influence juvenile crime.

The Juvenile Male variable measures the percent of the juvenile population in a zip code who are male. Since juvenile males traditionally make up the majority of juvenile crime, juvenile crime for a zip code should increase as a juvenile population there becomes proportionately more male.[20]

The Single parent variable is the percent of single parent households in that zip code. One expects that this variable should be positively correlated with juvenile crime so that more single parent households lead to more potentially unsupervised juveniles, and potentially greater juvenile crime.

The Urban variable is the percent of the total zip code population who live in an urban community. This should pick up basic differences among community types, however I also interact this variable with the strike measure to see if strikes have differential impacts in different types of communities. For several reasons, I expect crime in Urban areas to differ from that of Rural or Suburban areas. Urban areas feature many characteristics, such as increased criminal opportunity, higher pecuniary benefit, and increased criminal anonymity, which makes criminal activity in these areas more desirable (Glaeser and Sacerdote 1999). As such this Urban variable should be positively correlated with teacher strikes. Lastly, the Strike variable is a dummy variable, with a value of 1 describing a teacher strike event, and zero otherwise.

1.4 Regression Analysis

The initial results of the negative binomial regression analysis seem to confirm previous findings about the nature of school incapacitation. Column 1 in Table 1-1

[20] From 1993 to 2002, males made up about 71-75% of total juvenile arrests. For more information about juvenile crime by gender, please reference the *Crime in the U.S.* annual report published by the Federal Bureau of Investigation.

shows that the presence of teacher strikes seems to have a positive and significant effect on juvenile criminal behavior. But before I begin to quantify the effect of school incapacitation on juvenile crime, I want to make sure that the data are behaving properly.

The signs of the covariates in Column 1 of Table 1-1 seem to support the predictions made on the effects of income, welfare, parent's education and urban status with respect to juvenile crime. First, we see that median income and welfare are negatively and positively significant, respectively. This implies that children in lower income households and welfare receiving households are more likely to engage in juvenile crime. These results support the predictions associated with poorer living conditions. In addition, lower parental education leads to higher crime as well. Again, for several reasons this result makes sense. The Urban variable is positive and significant, showing that urban communities experience more overall juvenile crime. Our Student Employment variable is positive and statistically significant. This may imply after-school jobs provide students with greater resources which they may use to commit crime. Further, it may reflect the fact that as more students get after-school jobs, they are spending less time increasing their human capital (through study, after-school activities, etc.)

One problem is that the effect of the Juvenile Male variable is negative and significant, which contradicts the predicted effect. It says that across juvenile populations, those that are comprised of relatively more females experience more juvenile crime. One possible explanation for this result is that it may be reflecting problems in variable measurement for small communities. For rural communities there are two basic problems. The first is that with a small population of juveniles, the

variation of the Juvenile Male variable increases significantly.[21] The second is that this unusual variation makes interpolating this variable equally unreliable. Since a significant portion of zip codes have very small populations of juveniles, it is possible that the negative result I observe is being driven by rural zip codes that have atypical juvenile gender characteristics.[22]

To check the sensitivity of the initial results I begin by eliminating months in which strikes did not occur from the sample: January, March, and May (Specification II).[23] I then went a step further and eliminated every month other than September, October and April from the sample (Specification III). September and October are preserved because they are the most common months for strikes, and April was included because in 1991 there was a "state-wide" teacher strike that involved at least 41 school districts. The results of Specifications II & III are reported in Columns 2 and 3 of Table 1-1 respectively. These results are not significantly different from Specification I.

Recalling the zip code matching process described earlier in the paper, I am able to accurately match back to 1984 zip codes. However the crime data dates as far back as 1980. To deal with potential zip code mismatches before 1984, I drop the first four years of the sample (Specification IV).[24] This fourth specification shows the strike variable

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[21] For zip codes with juvenile populations less than 500, the standard deviation of the Juvenile Male variable is approximately 3 times larger than the standard deviation of those zip codes with 500 or more juveniles.

[22] In fact when I look at only urban zip codes, I see that this variable seems to correct itself, which gives credence to this argument. Further, Table A-2 shows that when I drop the smallest zip codes (population less than 500 juveniles) I see that the negative significance of the coefficient is destroyed. Thus it seems plausible that poor interpolation of this variable for sparsely populated areas has tainted the coefficients.

[23] Over the 22 year period, there was one school district that orchestrated a strike in January, but it only lasted for one day.

[24] In doing this I eliminate 9 strike events, 3 of which lasted 9 or more days.

remains significant as the sample is modified (Column 4 of Table 1-1). These results seem robust to many different representations of the data set.

It is also important to verify that these results are not being driven by zip code characteristics being specified inaccurately. The most immediate check of this is to use zip code fixed effects in lieu of explicit variables to capture the difference. Column 5 shows the addition of zip code fixed effects does not significantly alter the results.

The coefficient of the strike variable Column 5 of Table 1-1 shows that strikes have a positive effect on juvenile crime that is statistically significant. To quantify the impact of this increase, I take the partial derivative of the dependant variable with respect to the strike variable (∂Totalcrime/∂Strike). When I divide this marginal effect by the average of the dependent variable, I am left with a change that reflects a proportion of the mean of the dependent variable. In the full data set, the marginal effect of the strike variable suggests that total juvenile crime increases by 56.71% on days when strikes occur. This is more than a modest change in total crime.

1.4.1 Community Differences

Since the full data set includes all communities, some of which may look and behave very differently from one another, I want to test whether juvenile crime in different communities is differentially affected on strike days. To capture community differences, I run separate regressions for each community type, where I restrict the sample to only those communities in which 51% or more of the population in a zip code live in one of three types of communities: urban, suburban or rural.[25] The results of these

[25] The reader should be aware that this specification implies that zip codes which are considered one-third urban, one-third suburban, and one-third rural, will therefore be excluded from this analysis. However because of the way in which the Census defines these communities, these kinds of zip codes make up a very small portion of the total sample.

regressions can be viewed in the Columns of Tables 1-2, 1-3 and 1-4. These results show that unexpected school absences significantly influence juvenile crime only in urban communities.

This result is not at all unreasonable, given there are many differences among these types of communities that no doubt influence criminal behavior. It seems understandable that rural communities experience no change in juvenile crime when a strike occurs. Rural communities may provide less opportunity for crime, as well as a small town atmosphere that makes anonymity difficult. The lack of a significant effect of strikes in suburban communities is less believable since they arguably provide greater opportunity and anonymity, however there may yet be other, (possibly unobservable), characteristics about suburban characteristics that naturally deter juvenile crime on these strike days. For instance, suburban communities may be populated by more involved parents, may have more effective emergency resources, etc. The strike effects in urban communities however are positive and significant. In urban communities juvenile crime increases 19.68% on strike days.[26]

Again I run the same specification tests to test the robustness of these results. The signs of the covariates in every urban subsample are the same as in Table 1-1, except for Juvenile Male. The Juvenile Male covariate does take on a positive and significant coefficient in the urban subsample, however in the suburban and rural subsamples it remains negative and significant.

[26] The huge decrease in the magnitude of the strike coefficient from the full sample to urban subsample indicates that small populations in rural communities are exaggerating the effects of the strikes in the full sample.

1.4.2 Differences in Offense Types

Since school incapacitation may be differentially affecting the types of crimes that juveniles are committing, I partition the dependent variable measure of total crime into 5 specific crime types. These types of crimes include: drug and alcohol related crimes, mischievous crimes, property crimes, violent crimes, and finally weapons and endangerment crimes. I still consider all three community classifications when doing this analysis because I may yet find significant results in types of crimes for rural and/or suburban communities.[27] Table 1-5 shows the results of this extended analysis.

Rural and suburban communities continue to experience no change in juvenile crime resulting from strike days regardless of the type of crime that is being committed. Urban communities though do experience an array of changes in juvenile criminal behavior. Both mischievous crimes and property crimes seem to increase in the presence of a teacher strike. I estimate that mischievous crime increases by 48% on average for days when strikes occur.[28] Property crime increases by an average of 28.81%. This change in property crime is nearly double previous estimates in the literature. Violent crime decreases by approximately 31.53%.[29] Drug and alcohol crimes, and weapons and endangerment crimes are unaffected by school incapacitation.

These changes in criminal activity seem to make sense. It seems logical that mischievous crime would be most affected by school incapacitation. After all, these are

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[27] This could arise if increases in certain crimes were offsetting decreases in other types of crimes.

[28] It is difficult to compare my measure of mischievous crime to Jacob and Lefgren's measure of minor offenses. They define minor offenses as "NIBRS Group B offenses", however my measure of mischievous crime is a select subset of these crimes.

[29] Jacob and Lefgren estimate that school attendance decreases property crime by approximately 14%. They also estimate that violent crime increases on school days by about 28%. Therefore, my estimate of 31.53% for violent crime is approximately 10% larger than previous estimates.

the types of crimes that often result from boredom rather than calculated criminal thought. On the other hand, the decrease in violent crime that occurs is harder to explain. I use, as Jacob and Lefgren do, social interaction theory to explain this result. Jacob and Lefgren argue that juvenile violence against other juveniles arises, at least in part, from disputes formed in and around the classroom. When these juveniles are not forced to be at school, there is a decrease in the amount of overall violent crime that reflects a decrease in juvenile violence against other juveniles. This explanation seems credible.[30] Given the limits of the data set, I cannot test this theory further.[31]

1.4.3 Types of Offenders

In addition to the topics already spoken to, the detailed level of the data also allows me to also explore the nature of the criminal, beyond superficial characteristics. By observing criminal record, I attempt to answer whether these additional crimes are caused by normal delinquents facing greater criminal opportunity, or by juveniles who do not normally engage in criminal activity. I again partition the dependent variable into two measures: 1) total crime by repeat offenders and 2) total crime by one-time offenders. I define repeat offenders not just as those juveniles who have committed previous crimes, but also those juveniles who will commit future crimes. Thus a juvenile who commits multiple crimes is considered to be a repeat offender juvenile even on their first crime. The results of these regressions can be seen in Table 1-6.

Additional overall crime is induced for both offender types, however the average increase in crime for one-time offenders as a percent of the mean crime level for that

[30] In 1998 and 1997, 62% of victims of juvenile violence were 17 or younger.

[31] I have no information concerning the victims of the crimes in my data set.

group is much higher than for the repeat offenders. On strike days there is a 33.45% increase in average total crime for one-time offenders, and a 13.89% increase in average total crime for repeat offenders. Essentially, one-time offenders as a group seem to be much more affected by the lack of school incapacitation then the repeat offenders. This evidence suggests that the increased crime on strike days is less about criminal motive, and is more a result of boredom.

If these two groups are motivated differently to commit crimes, these effects may not be expressing differences across groups in the same types of crimes. As a society we may be more concerned if the lack of school incapacitation were inducing more property crime by one-time offenders rather than repeat offenders (where the crime may have eventually happened anyway). Therefore I am interested in examining whether these two groups differ in offense types on these strike days. To do this I create a dependent variable that equals total crime conditional on offender type and offense type. The results are expressed in Table 1-6. The decrease in violent crime comes mainly from the repeat offenders. Violent crimes committed by repeat offenders drop by 36.72% on strike days. In addition, both groups are contributing to the overall increases in mischievous crimes and property crimes. Property crimes by one-time offenders and repeat offenders increase by 56.75% and 25.40% respectively. Likewise mischievous crimes increase by 83.11% for one-time offenders and by 30.82% for repeat offenders.

1.4.4 Strikes Days as Gateway Crimes

Since the increase in crimes committed on strike days is substantially larger for first-time offenders, it may also be true that these juveniles may carry this behavior forward after their first arrest. To see if this is the case, I examine whether or not a

current day repeat offender was significantly more likely to haven gotten his/her start (commit his/her very first crime) on a strike day versus a normal school day. To do this, I generate a daily crime total that expresses the total of "first time" crimes by subsequent repeat offenders. The results of this regression can be also viewed in Table 1-6. A future repeat offender is found to be significantly more likely have committed his first crime on a strike day rather than an ordinary school day. This suggests that not only is school incapacitation preventing the creation of crimes, but it may also be preventing the creation of criminals.

1.4.5 Displacement of Crime

Thus far it seems the evidence suggests juvenile crime increases when students are not incapacitated in school. However what is not clear is whether these changes describe an overall increase in the amount of juvenile crime, or a displacement of crime from one day to another. Jacob and Lefgren try to speak to this question of temporal displacement in their paper, however their use of in-service days again limits the power of their analysis. Jacob and Lefgren find that there is no temporal displacement of crime to in-service days, however because in-service days are completely known, this result may simply be showing that juvenile criminals will plan to commit more crime when facing more criminal opportunity. What we are really interested in is whether a juvenile will temporally displace a "planned" crime given an unexpected opportunity to do so, or whether an unexpected opportunity to commit crime results in more "unplanned" crime. Again teacher strikes provide a more ideal measure of unexpected criminal opportunity, so I revisit this question of temporal displacement.

To test whether crime is being displaced, I begin by aggregating Total Crime to a weekly measurement rather than daily[32]. If juvenile crime is simply being displaced from weekend crimes to weekday crimes, then I should not observe an effect for the strike variable. To capture the weekly aggregated effect of strikes, I proportion the fraction of the week where strikes occurred as a new measure of the strike treatment. Despite this aggregation, I find that strikes are still a significant factor in contributing to additional juvenile crime (Table 1-7).

Table 1-7 shows a positive and significant strike coefficient on Total Crime in the full data set. This is a strong indication that overall juvenile crime is increasing, rather than being displaced across time. However, if displacement of crime varies across crime type, or criminal type, we cannot conclude that no crimes are being displaced. To see if the type of crime or the characteristics of the perpetrator influences whether displacement of crime occurs, I partition weekly total crime by crime type and offender as before. The results of these regressions are also reported in Table 1-7. These strike coefficients on Table 1-7 show that every specification of crime type and criminal type results in an increase of overall juvenile crime, and not temporal displacement of crime. I confirm the lack of displacement in total crime by aggregating further to a monthly crime total.

Weekly and Monthly aggregations of juvenile crime provide a good test of displacement of crime. I perform another test by lagging the strike effect, in order to pick up acute changes in crime shortly after the strike ends. If fewer crimes are committed on days following the strike, then the coefficient of the lagged strike variable should be

[32] This weekly measurement includes weekends and holidays, since strike day crime could displaced from usual weekend crime. In addition, week fixed effects are used in place of day fixed effects.

negative and significant.[33] I run seven separate regressions, shown in Table 1-8, labeled "1 day" through "7 days." For a strike that ends on day *t*, the first treatment of the lagged strike variable under "1 day" begins on day ($t + 1$), under "2 days" begins on day ($t + 2$), and so on. Table 1-8 shows that there is no significant effect from the lagged strike treatment. These results provide further evidence that juvenile crime occurring on strike days is additional crime, rather than displaced crime.

1.4.6 Duration Effects on Criminal Activity

One last advantage I gain from using teacher strikes as an instrument, in conjunction with the strength of the daily arrest data, is that I can track how juvenile crime changes over time with prolonged absence. If it were true that an increase in juvenile crime is at least partially caused by boredom and inactivity on the part of juveniles, then I should expect that the longer juveniles remain "inactive", the more crime is likely to result. Arguably there is just as much uncertainty over when teacher strikes will end as when they will begin. As such, trends in juvenile crimes up until the start of the strike and over the period of the strike should not be biased by student or parent foresight. In addition, the longer a strike continues, the less able a parent or guardian is to provide adequate supervision to students. For example, a parent may be able to take one or two days away from work to supervise their child, but the cost of taking 10 consecutive days off work is substantially larger.

To examine if juvenile crime is changing with the length of a teacher strike I partition the strike variable into 4 groups of new independent variables. Three of these variables are clustered groups of 3 consecutive strike days, and the last variable pertains

[33] This sample period also includes weekends. I also revert back to day fixed effects rather than week FE.

to a strike in its 10th day or higher.[34] These results can be viewed in Table 1-9. Table 1-9 shows that for the first 3 days, there is no significant change in overall juvenile crime.[35] After the 5th day or so, the strike coefficient becomes positive and significant, reflecting an increase in overall crime. Further, after the 5th or so strike day there is a gradual increase in the level of juvenile crime as strike length continues. In unreported regressions I find that as time passes, property and mischievous crime trends upward, while the decrease in violent crime remains relatively constant across days. The positive coefficient shows that these increases in property and mischievous crime overpower the decreases in violent crime. Column 2 of Table 1-9 repeats the regression in Column 1, only with the strike variable clustered at the two-day level rather than at the three-day level. Clearly the evidence suggests that the longer juveniles remain absent from school, the more overall daily juvenile crime will be observed.[36]

1.5 Robustness Checks

The results of the analysis up to this point seem to point to a very clear story about the relationship between juvenile crime and school incapacitation. In this section I perform some robustness checks to ensure that these results are not spurious. I begin by

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[34] I do this because if I were to partition the strike variable for each separate day (1st day, 2nd day, etc.), then the number of treatments would be very small for the later strike days and the coefficients would be unreliable.

[35] When I break up the dependent variable (total crime) by crime type as before, violent, property and mischievous crime types are individually significant. However in the first 3 days, the opposite signs and magnitudes of the coefficients wash out aggregate effects in total crime. These results are consistent with Jacob and Lefgren's findings (2003).

[36] It is worth keeping in mind that these strike day counts are also limited to school days. That is to say that when a strike has reached its 10th day, a student has missed exactly 2 full weeks of school. It is also very likely that these criminal behaviors will change after a strike has gone on for an extensive amount of time. At that point, juvenile crime may level-off or even decrease substantially. Of course given the sparse and limited nature of extremely lengthy teacher strikes, an analysis of that nature is inherently difficult not to mention highly unreliable.

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testing whether the strike variable is correlated with some unobservable characteristic, and that perhaps the results are picking up some alternate relationship. For instance, if a strike event is in fact very foreseeable, then one might speculate that the increase in juvenile crime predicted by the strike variable is really picking up an increased police presence on days that strike occur. That is to say that the results are not reflecting a true increase in juvenile crime, but that there are simply more police on the streets capturing a larger portion of the same total amount of juvenile crime.

If this were true, then one would expect that the number of days between an offense and an arrest would be shorter on strike days. The graphs on Figure 1-1 show that this is not the case. When I consider only zip codes where strikes did occur, I see that the percent of quick arrests[37] is significantly higher on regular school days than on strike days. I explain this by suggesting that on regular school days, police have a better idea who the likely criminals are (repeat offenders), as opposed to strike days where offenders are more likely to be (previously unidentified) first time, or one-time offenders.

1.5.1 Importance of Singular Strikes

Given the uniqueness of strikes, I must confirm that these results are not being solely driven by a small subsets of strikes. To do this I systematically eliminate each individual year from my sample starting with 1984. Table 1-10 shows that strikes are a significant determinant of juvenile crime in every year with the possible exception of 1985 ($z = 1.70$). When I drop 1985 from the sample I find a much weaker significance than when I eliminate any other year. In September of 1985, the Seattle school district engaged in a teacher strike that lasted 20 school days. Given that the evidence has

[37] I define these as arrests where the associated offense occurred less than 48 hours before the arrest.

suggested that juvenile crime in urban districts is most affected by teacher strikes, it is possible that these results are being solely driven by the circumstances surrounding this one district.

To test this condition I restrict the sample twice, once to look at the sample without the Seattle school district, and then again to look at only the Seattle school district. If Seattle is driving all of the results, then the strike variable should be insignificant when I exclude Seattle, and extremely significant when I exclude all other districts except for Seattle. Table 1-10 shows that strikes still have a positive and significant effect when I drop Seattle from the analysis. In addition the strike variable in the Seattle subsample is marginally significant (z = 1.82). Therefore I can be confident that while this one event obviously contributes to the result, (as I would expect the second longest strike in the most urban school district in Washington State to do), it is not the sole force driving the results.[38]

1.5.2 Reproducibility of Results

Another robustness check I perform is to make sure that the results cannot be easily reproduced with random generations of the strike variable. To check if this is the case, I use a uniformly generated random variable on the interval from 0 to 1 to re-specify a random dummy strike variable. Since teachers are on strike 2,351 days out of the 1,703,910 day sample, strike days occur 0.13797 % of the time. I assign a strike value of 1 to the random variable, when the value of the random observation generated is less than or equal to 0.0013797. If the randomly generated observation is greater than 0.0013797, then it is reassigned a value of zero. The new random strike variable should approximate

[38] The Seattle strike of 1985 is actually tied with the Mukilteo strike of 1990 (at 20 days) for the second longest strike in Washington State history from 1980-2001.

the number of strikes in the original sample. The results in Table 1-11 show the mean of the random strike coefficient over 25 trial regressions, as well as the average number of random strike days generated. Of the 25 trials, only 2 random strike variable coefficients were significant. One of the statistically significant coefficients was positive, while the other was negative.

1.5.3 Further Tests

One final test of the data is to make sure that my finding of new criminal participation on strike days (when the sample is broken up into one-time and repeat offenders) is not a byproduct of mislabeling offenders. Since the crime data ends in 2001, it could be that in the years leading up to 2001 many future career criminals are being mislabeled as one-time offenders due to the fact that future arrests past 2001 are unknown. To test this possibility, I drop the last 4 years of the data sample and repeat the regressions using offender type as the dependent variable. Table 1-12 shows that this mislabeling does not affect the results. The effects of a strike on repeat offenders and one-time offenders are nearly identical for either specification.

1.6 Conclusion

The results presented in this chapter provide a clearer picture concerning the relationship between schooling and urban juvenile crime. My main findings are the following: 1) The effects of a lack of school incapacitation are larger than those found in Jacob and Lefgren's study. The evidence suggests that property crime rises by as much as 29%, almost twice as much as what is predicted in the existing literature. Likewise, violent juvenile crime decreases by as little as 31.53%, and as much as 36.72%. These estimates of increased violent crime represent a 10%-25% increase in previous estimates.

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2) I confirm that these changes in daily crime reflect changes in total crime, and not a displacement of crime from one day to the next, regardless of crime type and criminal type. 3) Different types of juveniles are affected differently by a lack school incapacitation. A significant proportion of the decrease in the level of violent crime can be attributed to repeat juvenile offenders, while one-time offenders contribute more to the increases in property crime and crimes of mischief. 4). A failure to incapacitate juveniles will result in significantly more crime by those juveniles who might not have otherwise engaged in criminal acts or who rarely engage in such acts. In fact, juveniles who become repeat offenders are more likely to have gotten their criminal start when incapacitation was expected but not implemented, as opposed to ordinary school days.

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Incapacitation seems to have the greatest implications for new and seemingly preventable urban crime. I find that on strike days, new criminals are engaging in new criminal acts. Incapacitating these juveniles seems to be effective at preventing at-risk urban youths from engaging in new and risky behavior. I do not find that school incapacitation has any significant effect in suburban and rural communities. It may be true that the characteristics that differentiate these community types from urban communities, as well as each other, inherently reduce juvenile criminal behaviors, however such characteristics may be largely unobservable to us.

If the evidence presented in this paper is accurate, then what we are learning is that how we manage children's time outside of school is very important. If parents are strained to provide adequate supervision for their children, juveniles who we might consider to be at-risk may be the ones who are affected most, though at-risk juveniles are not the only ones affected. Families and businesses also bear the burden of increased

juvenile delinquency. These results have profound implications for many urban school policies and programs. How school districts budget their students' days off is no longer a trivial matter. For example, school districts with long breaks (like in a traditional school calendar) may have very different juvenile criminal behaviors than a school district with frequent but shorter breaks (like in a year-round school calendar). Differences in how school calendars overlap within and among districts should have an effect on the nature of juvenile crime. The length of the school day or school year in a district, school district policies regarding student attendance requirements, and even the nature of after school programs within a district are all policy issues that determine more than just a child's educational outcome. It is my goal to continue to explore this data set and scrutinize these kinds of school policies, so that I can better understand the impact that these kinds of policies may have.

Table 1-1: Negative Binomial Regression onto Total Crime with Full Data Set

Variables	(I)	(II)	(III)	(IV)	(V)
Strike	**0.195****	**0.187****	**0.210****	**0.260****	**0.220****
	(4.06)**	**(3.86)****	**(4.16)****	**(5.06)****	**(4.38)****
Median Income	-0.000058**	-0.000059**	-0.000059**	-0.000055**	—
	(102.47)**	(83.82)**	(60.76)**	(52.80)**	—
Welfare	0.475**	0.452**	0.520**	0.821**	—
	(7.71)**	(5.96)**	(4.97)**	(6.28)**	—
Urban	0.132**	0.112**	0.097**	0.055**	—
	(26.96)**	(18.45)**	(11.54)**	(6.14)**	—
Poor Parental Educ.	0.329**	0.346**	0.398**	0.482**	—
	(10.40)**	(8.88)**	(7.45)**	(6.67)**	—
Juvenile Maleness	-0.330**	-0.341**	-0.355**	-0.396**	—
	(28.06)**	(26.24)**	(22.32)**	(22.72)**	—
Student Employment	0.291**	0.297**	0.290**	0.296**	—
	(17.49)**	(14.43)**	(10.23)**	(9.16)**	—
Single Parent House	0.500**	0.495**	0.466**	0.985**	—
	(9.52)**	(7.64)**	(5.20)**	(9.67)**	—
Alpha	0.989**	1.009**	0.997**	0.956**	0.759**
	(156.56)**	(126.04)**	(91.12)**	(87.74)**	(79.06)**
Number of obs.	1,703,910	1,128,630	583,440	479,400	479,400
Time Fixed Effects	Y	Y	Y	Y	Y
Zip Fixed Effects	N	N	N	N	Y
R-Squared	0.0367	0.0364	0.0367	0.0279	0.0559

In Columns I – V, the sample is re-specified as follows (Type I through Type V):
(I) – Sample includes all ordinary school days from 1980-2001
(II) – January, March and May are excluded from the sample 1980-2001
(III) – Includes only April, September and October from 1980 - 2001
(IV) – Only April, September and October from 1984 – 2001
(V) – Only April, September and October from 1984 – 2001, with zip code fixed effects
Z-statistics are given in the parentheses for every table. * Indicates significance at the 5% level, ** is significant at the 1% level or smaller. To interpret these regressions, I have to calculate the marginal effect of these variables. The marginal effect of the strike variable is 0.149, which represents a 56.71% increase in average total juvenile crime on strike days.

Table 1-2: Urban Subsample Negative Binomial Regression onto Total Crime

Variables	(I)	(II)	(III)	(IV)	(V)
Strike	**0.151****	**0.129***	**0.178****	**0.211****	**0.214****
	(3.07)**	**(2.60)***	**(3.50)****	**(4.11)****	**(4.26)****
Median Income	-0.000061**	-0.000062**	-0.000062**	-0.000057**	—
	(83.07)**	(67.39)**	(48.42)**	(42.17)**	—
Welfare	1.394**	1.369**	1.418**	1.703**	—
	(16.40)**	(13.01)**	(9.79)**	(9.46)**	—
Poor Parental Educ.	0.398**	0.403**	0.449**	0.462**	—
	(9.49)**	(7.79)**	(6.31)**	(4.65)**	—
Juvenile Maleness	0.602**	0.577**	0.482**	0.397**	—
	(11.13)**	(8.60)**	(5.16)**	(3.62)**	—
Student Employment	0.214**	0.224**	0.214**	0.157**	—
	(9.10)**	(7.67)**	(5.34)**	(3.47)**	—
Single Parent House	0.739**	0.790**	0.945**	1.276**	—
	(8.28)**	(7.12)**	(6.24)**	(7.55)**	—
Alpha	0.676**	0.685**	0.666**	0.638**	0.494**
	(114.06)**	(91.00)**	(65.18)**	(62.65)**	(54.87)**
Number of obs.	454,953	301,312	155,601	129,593	129,593
Time Fixed Effects	Y	Y	Y	Y	Y
Zip Fixed Effects	N	N	N	N	Y
R-Squared	0.0484	0.0484	0.0487	0.0400	0.0635

Table 1-2 limits the sample to include only zip codes where 51% or more of the population lives in an area considered by the Census to be Urban

In Columns I – V, the sample is re-specified as follows (Type I through Type V):

(I) – Sample includes all ordinary school days from 1980-2001

(II) – January, March and May are excluded from the sample 1980-2001

(III) – Includes only April, September and October from 1980 - 2001

(IV) – Only April, September and October from 1984 – 2001

(V) – Only April, September and October from 1984 – 2001, with zip code fixed effects

Z-statistics are given in the parentheses for every table. * Indicates significance at the 5% level, ** is significant at the 1% level or smaller. To interpret these regressions, I have to calculate the marginal effect of these variables. The marginal effect of the strike variable is 0.121, which represents a 19.68% increase in average total juvenile crime on strike days.

Table 1-3: Suburban Subsample Negative Binomial Regression onto Total Crime

Variables	(I)	(II)	(III)	(IV)	(V)
Strike	-0.027	-0.064	-0.159	-0.162	-0.449
	(0.12)	(0.29)	(0.61)	(0.62)	(1.74)
Median Income	-0.000015**	-0.000016**	-0.000013**	-0.000012**	—
	(8.01)**	(6.54)**	(4.74)**	(5.66)**	—
Welfare	-1.929**	-2.146**	-1.483**	-2.005**	—
	(9.21)**	(8.31)**	(4.20)**	(4.43)**	—
Poor Parental Educ.	1.639**	1.856**	1.683**	2.393**	—
	(13.33)**	(12.29)**	(8.09)**	(8.55)**	—
Juvenile Maleness	-1.432**	-1.973**	-2.151**	-2.899**	—
	(5.33)**	(5.94)**	(4.74)**	(5.66)**	—
Student Employment	-0.121*	-0.198**	-0.172	-0.064	—
	(2.23)*	(2.96)**	(1.87)	(0.59)	—
Single Parent House	2.285**	2.369**	0.217**	3.062**	—
	(14.13)**	(11.97)**	(7.83)**	(8.82)**	—
Alpha	1.326**	1.303**	1.240**	1.203**	0.969**
	(59.08)**	(54.84)**	(39.01)**	(37.78)**	(34.42)**
Number of obs.	134,100	88,841	45,794	37,992	37,992
Time Fixed Effects	Y	Y	Y	Y	Y
Zip Fixed Effects	N	N	N	N	Y
R-Squared	0.0294	0.0300	0.0302	0.0227	0.051

Table 1-3 limits the sample to include only zip codes where 51% or more of the population lives in an area considered by the Census to be Suburban

In Columns I – V, the sample is re-specified as follows (Type I through Type V):

(I) – Sample includes all ordinary school days from 1980-2001

(II) – January, March and May are excluded from the sample 1980-2001

(III) – Includes only April, September and October from 1980 - 2001

(IV) – Only April, September and October from 1984 – 2001

(V) – Only April, September and October from 1984 – 2001, with zip code fixed effects

Z-statistics are given in the parentheses for every table. * Indicates significance at the 5% level, ** is significant at the 1% level or smaller

Table 1-4: Rural Subsample Negative Binomial Regression onto Total Crime

Variables	(I)	(II)	(III)	(IV)	(V)
Strike	0.017	0.027	-0.343	-0.285	-0.392
	(0.09)	(0.14)	(1.45)	(1.20)	(1.65)
Median Income	-0.000034**	-0.000036**	-0.000036**	-0.000033**	—
	(26.68)**	(22.50)**	(16.62)**	(13.59)**	—
Welfare	-0.077	-0.063	-0.108	0.117	—
	(0.68)	(0.45)	(0.56)	(0.48)	—
Poor Parental Educ.	-0.282**	-0.332**	-0.218	-0.200	—
	(3.81)**	(3.63)**	(1.77)	(1.32)	—
Juvenile Maleness	-0.357**	-0.362**	-0.370**	-0.421**	—
	(37.69)**	(32.83)**	(25.81)**	(25.64)**	—
Student Employment	0.142**	0.156**	0.180**	0.267**	—
	(4.34)**	(3.86)**	(3.25)**	(4.15)**	—
Single Parent House	0.379**	0.401**	0.317*	0.864**	—
	(4.41)**	(3.81)**	(2.15)*	(5.21)**	—
Alpha	2.679**	2.767**	2.810**	2.712**	2.197**
	(80.71)**	(66.36)**	(49.08)**	(46.97)**	(44.48)**
Number of obs.	1,101,359	729,469	377,244	304,491	304,491
Time Fixed Effects	Y	Y	Y	Y	Y
Zip Fixed Effects	N	N	N	N	Y
R-Squared	0.0313	0.0308	0.0316	0.0218	0.0551

Table 1-4 limits the sample to include only zip codes where 51% or more of the population lives in an area considered by the Census to be Rural

In Columns I – V, the sample is re-specified as follows (Type I through Type V):

(I) – Sample includes all ordinary school days from 1980-2001

(II) – January, March and May are excluded from the sample 1980-2001

(III) – Includes only April, September and October from 1980 - 2001

(IV) – Only April, September and October from 1984 – 2001

(V) – Only April, September and October from 1984 – 2001, with zip code fixed effects

Z-statistics are given in the parentheses for every table. * Indicates significance at the 5% level, ** is significant at the 1% level or smaller

Table 1-5: Effects of Strike Days by Crime Type and by Community Type

Dependant Variable	Strike Coefficient for Urban Sub-Sample	Strike Coefficient for Suburban Sub-Sample	Strike Coefficient for Rural Sub-Sample
Drugs and Alcohol	0.200	-1.188	-0.113
	(1.36)	(1.48)	(0.20)
Mischievous	**0.536****	-0.813	-0.241
	(3.88)**	(0.73)	(0.34)
Property	**0.257****	-0.128	-0.276
	(3.79)**	(0.34)	(0.80)
Violent	**-0.364***	-1.103	-0.266
	(2.50)*	(1.44)	(0.52)
Weapon and Endangerment	-0.014	-53.096	—
	(0.05)	(0.02)	—
Number of obs.	129,593	37,992	304,491
Time Fixed Effects	Y	Y	Y
Zip Fixed Effects	Y	Y	Y
Ave. R-Squared	0.0506	0.0423	0.0579

Each row represents a new regression with the listed crime type as the dependant variable. The reported number is the strike coefficient for the regression with the corresponding dependant variable. Each column is a different subsample group: urban, suburban and rural. All 14 regressions are defined as a Type V regression, including data from only April, September and October from 1984 – 2001, and zip code fixed effects. The Pseudo R-squared reported at the bottom of each column is the average R-squared over the 5 regressions. Z-statistics are given in the parentheses for every table. * Indicates significance at the 5% level, ** is significant at the 1% level or smaller. The marginal effects of these variables show that violent crime decreases by 31.53% on strike days. Property crime and mischievous crime increase by 28.81% and 48% respectively. Again, these effects reflect changes in juvenile crime as a proportion of the mean. The coefficients of the marginal effects are (–0.032), (0.072) and (0.025) for violent, property and mischievous crime respectively.

Table 1-6: Effects of Strike Days by Crime Type and by Offender Type

Dependant Variable	Strike Coefficient for One-Time Offenders	Strike Coefficient for Repeat Offenders
Total Crime	**0.382****	**0.156****
	(4.05)**	**(2.77)****
Drugs and Alcohol	0.337	0.153
	(1.18)	(0.93)
Mischievous	**1.024****	**0.597****
	(3.66)**	**(3.99)****
Property	**0.534****	**0.220****
	(4.44)**	**(2.81)****
Violent	-0.567	**-0.408****
	(1.48)	**(2.59)****
Weapon and Endangerment	-1.330	-0.009
	(1.20)	(0.03)
Gateway Crime	—	**0.286***
	—	**(2.00)***
Number of obs.	129,593	129,593
Time Fixed Effects	Y	Y
Zip Fixed Effects	Y	Y

Table 1-7 includes only urban zip codes. All 13 regressions are defined as a Type V regression, including data from only April, September and October from 1984 – 2001, and zip code fixed effects. Z-statistics are given in the parentheses for every table. * Indicates significance at the 5% level, ** is significant at the 1% level or smaller. The marginal effects of these variables show that for repeat offenders, violent crime decreases by 36.72%, property crime increases by 25.4%, and mischievous crime increases by 30.82% on strike days. The coefficients of these marginal effects are (-0.032), (0.046) and (0.012). For one-time offenders property crime increases by 56.75% and mischievous crime increases by 83.11% on strike days. The coefficients of these marginal effects are (0.030) and (0.008). Again, these effects reflect changes in juvenile crime as a proportion of the mean.

Table 1-7: Temporal Displacement of Crime by Crime Type and by Offender Type

Weekly Aggregated Dependant Variable	Strike Coefficient for Full Data Set	Strike Coefficient for Urban Subsample	Strike Coefficient for One-Time Offenders	Strike Coefficient for Repeat Offenders
Total Crime	**0.235****	**0.243****	**0.418****	**0.176****
	(4.50)**	**(4.67)****	**(4.51)****	**(3.11)****
Mischievous	—	**0.570****	**0.820****	**0.501****
	—	**(4.39)****	**(3.09)****	**(3.55)****
Property	—	**0.312****	**0.492****	**0.225****
	—	**(4.73)****	**(4.27)****	**(3.06)****
Violent	—	**-0.300***	-0.223	**-0.317***
	—	**(2.32)***	(0.73)	**(2.27)***
Monthly Aggregated Dependant Variable				
Total Crime	**0.534****	**0.485****	—	—
	(3.86)**	**(3.73)****	—	—
Number of obs.	440,640	117,491	117,491	117,491
Time Fixed Effects	Y	Y	Y	Y
Zip Fixed Effects	Y	Y	Y	Y

Each row represents a new regression with the listed crime type aggregated at the weekly level as the dependant variable. The reported number is the strike coefficient for the regression with the corresponding dependant variable. Each column is a different sample group. Column 1 is the full sample. The remaining three Columns are from the urban sample. The top 10 regressions are defined as a Type I regressions including zip code fixed effects. The bottom 2 regression coefficients represent the strike effect on Monthly Total Crime. Z-statistics are given in the parentheses for every table. * Indicates significance at the 5% level, ** is significant at the 1% level or smaller

Table 1-8: Temporal Displacement of Crime through Lagged Strike Variable

Lagged Strike Variable	Strike Coefficient For Urban Sub-Sample
1 Day	-0.033
	(0.62)
2 Days	-0.028
	(0.54)
3 Days	-0.030
	(0.57)
4 Days	-0.046
	(0.87)
5 Days	-0.041
	(0.78)
6 Days	-0.093
	(1.75)
7 Days	-0.070
	(1.33)
Number of obs.	293,112
Time Fixed Effects	Y
Zip Fixed Effects	Y
Ave. R-Squared	0.0597

Each row represents a new regression with strike variable lagged the by the corresponding row. Each regression is defined as a Type IV regression including zip code fixed effects of the urban subsample. The notable exception is that weekends are included to allow for the correct number of strike treatments. Therefore, days where lagged strike treatments overlap breaks, holidays and other nontypical school days are included in the sample. Z-statistics are given in the parentheses for every table. * Indicates significance at the 5% level, ** is significant at the 1% level or smaller

Table 1-9: Changes in Total Juvenile Crime, by Days Elapsed since the Start of a Strike

Partitioned Strike Variable	Strike Coefficient	Partitioned Strike Variable	Strike Coefficient
Days 1 - 3	-0.005 (0.06)	Days 1 - 2	0.058 (0.58)
Days 4 - 6	**0.185** **(1.91)**	Days 3 - 4	0.006 (0.07)
Days 7 - 9	**0.261*** **(2.54)***	Days 5 - 6	**0.262*** **(2.37)***
Day 10+	**0.313**** **(2.93)****	Days 7 - 8	**0.284**** **(2.62)****
—	— —	Days 9 - 10	0.228 (0.98)
—	— —	Day 11+	**0.305**** **(2.67)****
Alpha	**0.533**** **(101.05)****	Alpha	**0.533**** **(101.04)****
Number of obs.	454,953	Number of obs.	454,953
Time Fixed Effects	Y	Time Fixed Effects	Y
Zip Fixed Effects	Y	Zip Fixed Effects	Y
R-Squared	0.0713	R-Squared	0.0713

Table 1-8 includes only urban zip codes. These two regression are defined as a Type I regressions, using data including all ordinary school days from 1980-2001, and zip code fixed effects. Both regressions use Total Crime as their dependant variable. Z-statistics are given in the parentheses for every table. * Indicates significance at the 5% level, ** is significant at the 1% level or smaller

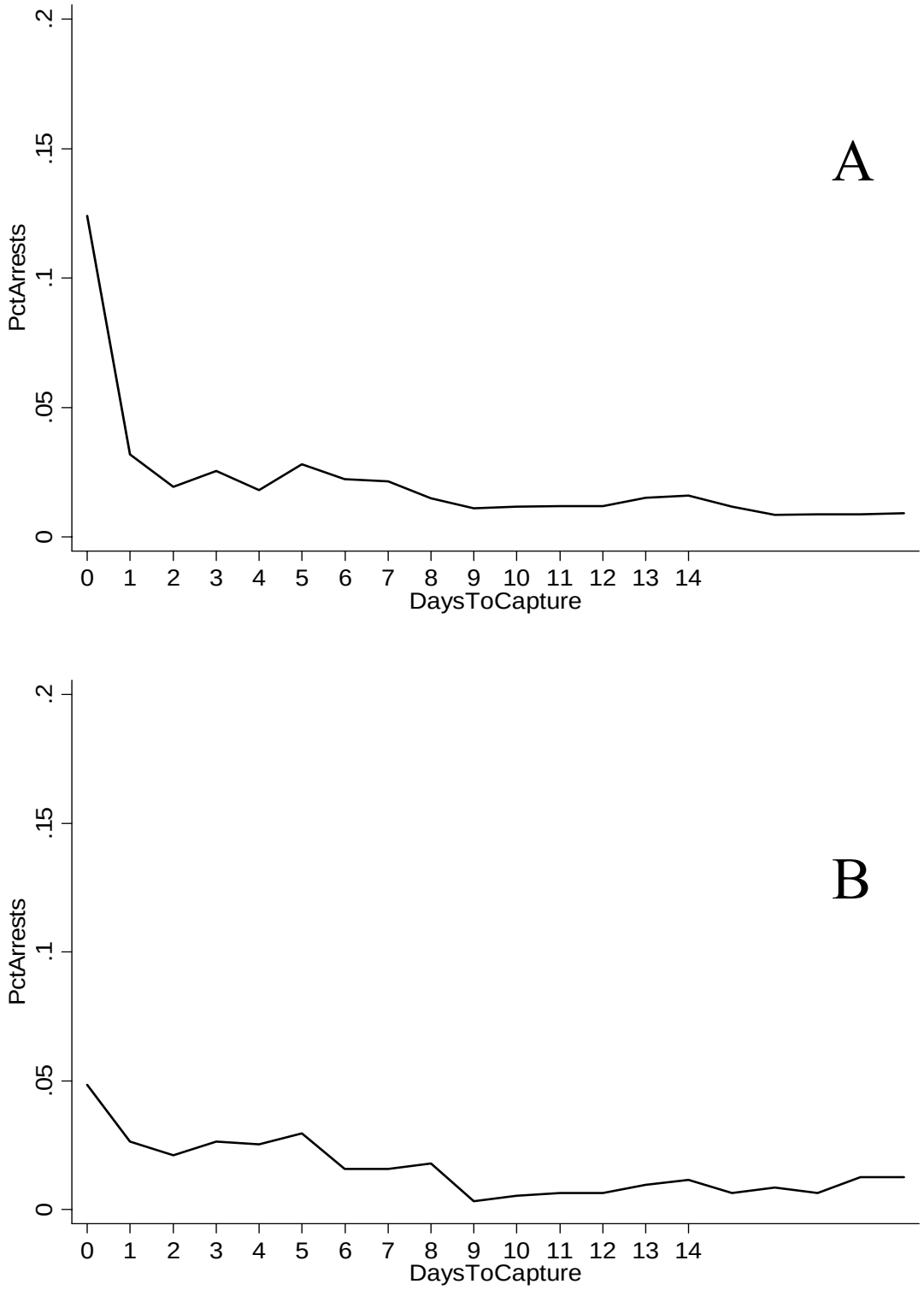

Figure 1-1: Average Percent of Total Arrests as Function of Days between Offense and Capture A) NonStrike Days for Zip Codes Where Strikes Occur, B) Strike Days for Zip Codes Where Strikes Occur

Table 1-10: Effects of Strike Days with Specific Years Dropped from the Sample

Year Dropped	Strike Coefficient	Year Dropped	Strike Coefficient
1984	**0.164****	1993	**0.166****
	(3.39)**		**(3.41)****
1985	0.093	1994	**0.163****
	(1.70)		**(3.28)****
1986	**0.166****	1995	**0.166****
	(3.43)**		**(3.40)****
1987	**0.160****	1996	**0.157****
	(3.16)**		**(3.23)****
1988	**0.163****	1997	**0.153****
	(3.37)**		**(3.16)****
1989	**0.148****	1998	**0.156****
	(3.02)**		**(3.23)****
1990	**0.189****	1999	**0.157****
	(3.77)**		**(3.26)****
1991	**0.229****	2000	**0.144****
	(3.18)**		**(2.99)****
1992	**0.157****	2001	**0.150****
	(3.24)**		**(3.11)****
Without Seattle	**0.133***	Seattle Only	0.153
	(2.04)*		(1.82)
Ave.Number of obs.	358274		358274
Time Fixed Effects	Y		Y
Zip Fixed Effects	Y		Y
Ave. R-Squared	0.0603		0.0603

The top half of this table is a regression of Strikes on Total Crime with each year systematically dropped from the sample. Each regression includes only urban zip codes. All 18 regressions are defined as a Type I regressions, using data including all ordinary school days from 1984-2001, and zip code fixed effects. The bottom half of the table reports the effect of strike days, for the same Type I sample, with Seattle dropped from the sample, and with Seattle making up the entire sample. The bottom of the table shows the average number of observations for the top 18 regressions, as well as the average R-squared term. Z-statistics are given in the parentheses for every table. * Indicates significance at the 5% level,** is significant at the 1% level or smaller.

Table 1-11: Average Effect of Randomly Generated Strike Days on Total Crime

Dependant Variable: Total Crime	Random Generations of Strike Days
Ave. Random Strike Coefficient	0.002 (0.05)
Average Number of Strike Days	2,355
Number of Trials	25
Number of obs.	1,703,910
Time Fixed Effects	Y
Zip Fixed Effects	N
Ave. R-Squared	0.0380

Table 1-12: Effects of Strike Days by Offender Type with the Last 4 Years of the Sample Dropped

Dependant Variable	Strike Coefficient for One-Time Offenders	Strike Coefficient for Repeat Offenders
Total Crime	**0.382**** **(4.05)****	**0.156**** **(2.77)****
Total Crime without 1998-2001	**0.383**** **(4.08)****	**0.128*** **(2.31)***
Number of obs.	129,593	129,593
Time Fixed Effects	Y	Y
Zip Fixed Effects	Y	Y

Table 1-11 is the average strike coefficient over 25 trials using a randomly generated strike variable. It includes the full data set. Table 1-12 is a Type V regression using just the urban subsample, and includes zip code fixed effects. Z-statistics are given in the parentheses for every table. * Indicates significance at the 5% level, ** is significant at the 1% level or smaller

CHAPTER 2
THE ROLE OF TEACHER NETWORKING IN TEACHER TRANSFER DECISIONS AND TEACHER MOBILITY

2.1 Introduction

In recent years, there has been growing research to support the notion that teacher movement is an important topic, and not one to be overlooked. Thus far, research on teacher sorting behavior has shown that student characteristics seem to influence a teacher's transfer decision (Hanushek, Kain, Rivkin 2001). More specifically, teachers sort away from relatively low-income, low-achieving schools. As a result, less experienced teachers are more typically found in poor, nonwhite, low-performing schools, particularly those in urban areas (Lankford, Loeb, Wyckoff 2002). This is most important because teacher experience seems to be directly linked to student achievement (Hanushek, Kain, Rivkin 1998, 1999)[1].

In addition to student characteristics, other characteristics (such as salary differences) have been studied in an effort to better understand the factors that influence teacher movement. Research aimed at identifying specific characteristics and conditions that promote teacher mobility have given more insight into the questions of when and why teachers move, however researchers still do not have the complete picture on how teachers move and why.

[1] Hanuchek, Kain and Rivkin (1998) show that variations in teacher quality account for 7.5% of the total variation in student achievement. In addition, teacher quality differences tend to outweigh school quality differences considerably.

The focus of this paper is to better understand the nature of teacher transfers, with special attention given to teacher intradistrict transfers from school to school. Specifically this paper will identify the use of Master Inservice workshops aimed at professional development as a powerful means of building teacher networks. Ultimately I test the hypothesis that Inservice workshops give teachers greater opportunities to network with teachers from other schools in their district, and therefore result in a higher rate of teacher transfer. The evidence will show that a one standard deviation increase in workshop attendance contributes to a 12.5% increase in teacher transfer rates per school. Thus teacher networks are a crucial element in influencing teacher transfer rates.

Section 2.2 of this paper will provide a context for the basic assumptions outlying the arguments set forth, Section 2.3 will specify the model therein, Section 2.4 reports the preliminary results, Section 2.5 will check the validity of the results, and Section 2.6 will make the concluding remarks.

2.2 Discussion of Networking

2.2.1 How Teachers Network

The idea that professionals develop networks is not a new concept in the social sciences. The term "networking" spans across many professions and takes many different forms. Like many professionals, teachers also engage in networking for various reasons I will address later in this section. First I will address the issue of how teachers build networks. Clearly some teacher networking takes place in private. Teachers may spend recreational time together getting to know one another, or they may talk over lunch. However these examples of the networking process, though valid, are not feasibly quantified or qualified. In order to quantify teacher networks, I rely on teacher Inservice activity to determine if teacher networks aid in mobility. Specifically I focus on the

number of hours teachers spend in Inservice activity as my measure of network size/strength. I argue that Master Inservice workshops, a part of the teacher re-certification process, provide an effective forum for networking. To understand why this is the case, it is necessary to first review how the re-certification process works.

Every teacher in the state of Florida is required to have an up-to-date license that enables him or her to teach in Florida public schools. This license is earned by every teacher at the beginning of his or her career and must be re-certified every five years. Teachers earn their re-certification through any combination of three things: they may take state-issued Florida Subject Area Exam in appropriate fields to prove their level of competency, they may enroll in and complete required coursework (6 credits minimum) at an approved university or college, and/or they may choose to complete up to 120 hours of Master Inservice courses/workshops offered by the school district.

This state mandated re-certification process provides an ideal environment for testing the effects of teacher networking. Teachers who take the Florida Subject Area Exam in their appropriate fields or who decide to take classes at a College or University have little to no interaction their colleagues in their school districts. However, the Master Inservice option is quite different from other re-certification options. First, Inservice courses/workshops are almost exclusively made up of other teachers, all within the same district and from differing schools.[2] Thus teachers interact with others who share a common profession, and often common interests. Secondly, the environment of most workshops is designed in such a way that encourages interaction among participants.

[2] It is possible that a marginal number of administrators, county officials, etc. attend these teacher components for other reasons (professional or personal). They are not included in the Inservice data in this paper.

Workshop assemblies are long in duration (often several hours per session) and can span weeks or months. In addition, individuals are often called upon to share experiences with all the participants, or present material regarding how the workshop has influenced their classroom activity. Thirdly, Inservice is an attendance based program, as opposed to the graded college coursework or graded test format. One could argue that the relatively stress-free nature of the Inservice allows teachers to spend more time getting to know each other, free from the distraction of grade requirements.

2.2.2 How Teachers Use Their Networks

There are many reasons why any professional or businessperson would network with others in their field of work. They yearn to share ideas, understand their competition, and exchange information that can benefit their personal interests, sometimes in a mutual way. Why teachers network with each other is not so different. They exchange classroom experiences and teaching methods, information on workplace environments, as well as build personal bonds that enhance their feelings towards the profession, or even their personal lives. As a result they could possibly use their networks to plan intradistrict school events, find work in the summer, or even to socialize.

However, there are specific applications of strong teacher networks that would enhance mobility within a district. First, teachers use their networks to gain information about the various student and administrative characteristics of schools in their district. Greater access to this information lowers the cost of job searching and enhances mobility. Secondly, teachers use their networks to reveal their teacher quality in a less costly way for the purpose of being hired at other schools within their own district. If teachers do use their networks to sort into preferred schools in a more efficient, less costly way, then

there are obvious implications for student outcomes, since others have successfully linked student outcomes to teacher mobility.

2.3 The Basic Model

A teacher's desire to transfer to school *n* (new school) is based on the utility they receive at school *n* minus the utility they receive at school *o* (old school). The utility at the old school is given by:

$$V_o(s_o, q_o, a_o, d_o, o)$$

where *o* indexes the old school, s_o is the salary of the old school , q_o is a measure of student/classroom quality, a_o (administration quality), d_o (distance from school to home), and o (opportunity cost of teaching). Similarly the utility at the new school is given by:

$$V_n(s_n, q_n, a_n, d_n, o)$$

where *n* indexes the new school, s_n is the salary of the new school , q_n is a measure of student/classroom quality, a_n (administration quality), d_n (distance from school to home), and o (opportunity cost of teaching). Teachers incur explicit costs to movement. These costs arise from the search and application process. More specifically, teachers may have to submit paperwork, spend some time interviewing, or generally gather information about the school they are transferring to. Thus I write this teacher decision as:

Transfer when: $V_n - V_o - C(i_n, r_n, n_j^*) \geq 0$

Do not transfer when: $V_n - V_o - C(i_n, r_n, n_j^*) < 0$

In this case $C(\cdot)$ represents the explicit cost borne by the teacher, where i_n is the amount of relevant school information a teacher desires to gather, r_n represents district and school requirements for transfers (paperwork, interviews, etc.), and n_j^* representing the network

size of the jth individual. I further note the relationship of each parameter to this explicit cost function.

It is reasonable to assume that the less a teacher knows about a prospective school, the more time he/she spends gathering relevant information. Therefore we expect an increase in necessary or relevant information (i_n) to translate to a direct increase in $C(\cdot)$, such that $\partial C/\partial i_n > 0$. In addition an increase in district and/or school requirements for transfer will increase time and resources spent towards transfer, such that $\partial C/\partial r_n > 0$. When looking at network size it becomes apparent that a larger network size will lend itself to better flow of school information, possibly fewer or shorter interviews, and more inside information that helps one to eliminate unnecessary measures. Therefore it seems logical that a larger network will decrease external cost, such that $\partial C/\partial n_j^* \leq 0$. Lastly we must qualify the network to external cost relationship as not being strict. This allows for the possibility that for school *n*, one's network is totally ineffective. That is to say that one's network is not the relevant network, i.e., a network in another state. From these equations it is clear why teachers may be more likely to transfer as their relevant network size increases

2.4 Data and Empirical Study

My data for my study comes from a variety of sources. I utilize individual school data taken from the "Florida School Indicators Report" (FSIR) and the "School Advisory Council Report" (SACR). Countywide data was also supplied in various forms from the Florida State Department of Education (FLDOE). These forms include the "FLDOE State Survey #2", "Teacher Exit Interview Information", an "Inservice Hours Report", "The Profiles of Florida District Schools (Students and Staff Data)", and "Teacher Salary,

Experience and Degree Level". The sample includes 67 observations, one for each Florida school district.

Because several variables introduced in the following section are highly collinear to district size, such variables have been defined on a per school basis.[3]

2.4.1 Defining the Parameters

The dependent variable for my study (**Transfers)** is the number of transfers within a district per school. In order to approximate the closeness of different school job options, or capture the density of school job choice in a district, I use the total area, expressed in sq. miles, divided by the number of schools in the district (**School Distance**). Since a denser school district gives a mobile teacher more viable, less costly job options, I expect this variable to be positively correlated with the total number of transfers per school.

Student/classroom quality is measured by taking the standard deviation of the percentage of free lunch eligible students among all schools in a given district. This measure (**School Difference**) should capture the variation of the percentage of the student body that is of low socio-economic status (SES) across schools. In essence, this variable measures how schools in a district differ along family backgrounds. Since greater variation in work environments should give teachers a greater incentive to sort, this variable should be positively correlated with transfers per school.

Wage differences may also play an important role in teacher transfer decisions. There are no wage differences among public schools in a given district, but wage differences among districts still exist and are important to account for. As such, **Salary**

[3]**Transfers**, **Network**, **Dislike Supervisor**, **Principal Moves**, **School Changes**.

Difference is defined as the average salary of the highest paying district directly adjacent to the current district minus the average salary of the district in which a teacher is currently employed. If a teacher considers salary difference in a move between districts, it is plausible that they may consider the first-best alternative salary difference above all else. If higher salaries provide incentives for teachers to leave their district rather than move within district, then there should be less movement between schools within district. Therefore this variable should be negatively correlated with within-district movement.

In addition to the interdistrict salary difference, a wage concern for teachers should also be the wage forgone from remaining a teacher. To capture the opportunity wage forgone, I construct a variable (**Wage Ratio**) using a county-level wage index. This wage index reflects the average wages across a predetermined set of occupations for each county. By dividing average salary for a district by its county wage index, I measure how well teachers in a county are paid relative to other occupations. The larger the **Wage Ratio**, the less appealing other jobs are. As a result this variable should be positively correlated with teacher transfers.

My measure of network size (**Network**) equals total hours spent in Inservice per school.[4] Since more hours spent in Inservice should lead to larger and more effective networks, this variable should be positively correlated with teacher transfers per school.

It is also true that the **Network** variable is positively correlated with larger school size. This is a potential problem since school size, measured in average number of teacher faculty per school, is positively correlated with district size itself. That is to say

[4]Component hours geared towards non-instructional staff (i.e., food service employees, transportation staff, administrative staff, substitute teachers, etc.) are not included in this measurement. Since each district adheres to a statewide numbering standard for component identification, I can be sure that I only include components which were teacher-oriented.

that as school districts grow, the size of the schools as measured in total faculty also grows. Since teachers in larger districts (with larger schools) have more positions to choose from, they should have greater transfer opportunities. To prevent this relationship from biasing any estimates of the **Network** variable, I create a variable to control for the effect of larger school size. This variable, **School Size,** measures the average teaching staff per school in the a district.

New public schools opening and old public schools closing also present teachers with new choices in making transfer decisions. New schools that open in a district may provide up-to-date facilities and technology in the classroom, making teaching there more desirable. School closures force teachers to reorganize into other schools or leave teaching entirely. The variable (**School Changes**) captures these elements of school turnover. This variable is measured by assigning a value of positive one for every school gained and/or lost from the 2000-01 school year to the 2001-02 school year, and then summing the totals for each county and dividing by the number of schools. Since school districts where schools open and/or close should result in more teacher transfers, this variable should be positively correlated with transfers.

There is another variable emphasized in this model that is based on administrative movement, specifically movement between schools by existing principals. When principals transfer from school to school within a district, positive teacher/principal relationships may induce principals to recruit teachers to work at a new assignment. In essence principals may bring their favorite teachers with them to another school under the expectation/promise of a positive work environment. This variable (**Principal Moves**) is measured as the total number of principals who moved from one school to another

school, divided by the number of schools. If principals take teachers with them when they move between schools, then principal transfers should be positively correlated with teacher transfers.

Another variable that should affect teacher movement within a district is the number of relevant alternatives. It is arguably difficult for teachers to move between high schools and elementary/middle schools. Obviously high school teachers who work in districts where there is only one high school cannot move to another high school in their district. As a result, one would expect that school districts with only one high school will have an inherently lower rate of teacher transfer compared to similar districts with multiple high schools. To account for this disparity I include a dummy variable (**One HS**) that receives a value of 1 when a school district has only one high school (zero otherwise). This variable should be negatively correlated with teacher transfers.

The last two variables I include in the regression help to describe the state of a district's preferential treatment towards transferring existing teachers to fill new vacancies when they open, rather than hiring new teachers. Some districts require that open positions are filled with teachers already employed before a new teacher may be hired; while some other districts give no special consideration to existing teachers over new applicants. Since more relaxed or favorable policies towards teacher transfers may enhance teacher mobility, I introduce two dummy variables that describe the priority given to teacher transfers. The first variable (**Transfer PC**) describes a district that affords partial consideration to existing teachers. This variable takes on a value of 1 when a district gives preferential treatment, but not exclusive privilege, to its teacher transfers (zero otherwise). The second dummy variable (**Transfer CC**) describes a

district's policy to necessarily fill a new position with a teacher transfer request before considering a new applicant. This variable takes on a value of 1 when a district offers complete consideration to existing teachers before all others (zero otherwise). In both cases, these variables should be positively correlated with transfers per school to reflect the enhanced opportunity for mobility given by preference.

2.4.2 Empirical Testing

The analysis begins with a simple OLS regression onto the dependent variable **Transfers**. The results are expressed in Table 2-1, listed by coefficient with the relevant t-statistic listed in parenthesis.

Columns 1 and 2 express the OLS regression with our 10 major variables of interest. Some of these variables that do not have the predicted sign, in this case **School Distance**, **Transfer PC** and **Transfer CC**, and are not significantly different from zero. Of the variables that are significant, (**Network**, **School Difference**, **School Changes** and **School Size**), each has the predicted sign.

The positive and significant coefficient of the **Network** variable in Column 2 seems to suggest that networks exist, and that in fact they do serve to enhance teacher intradistrict mobility. As a teacher networks grows, teacher mobility rises. This provides evidence that teachers are using their networks to gain information and enhance the transfer process. To determine the magnitude of the impact of networking on transfer rates, I look at the percent change in the average number of teacher transfers per school when the amount of networking increases by one standard deviation. I find that an increase in the number of inservice hours per school by approximately 1000 hours increases the teacher transfer rate per school by approximately 11.49%. These results can be viewed in Table 2-2.

The results in Table 2-1 also suggest that student characteristics, school size, and schools opening and closing all influence teacher movement. Teachers seem to sort more frequently in districts where variation in student quality across schools is high. The variable **School Difference** has a significant impact in teacher movement and shows that teachers are more likely to move around, or sort, when there are sizeable differences among student populations at different schools. According to the regression, a one standard deviation increase in the measure of school heterogeneity implies a nearly 15.25% increase in teacher transfers per school

Teachers also seem to be more mobile in districts where schools are larger. The **School Size** variable is positive and significant, suggesting teachers face greater opportunity to sort when there are more total positions in a school. The coefficient of the **School Size** variable implies that a one standard deviation increase in the average size of a school leads to a roughly 17.85% increase in transfer rates per school.

Besides student and school characteristics, teachers also have greater mobility in districts where schools are opening and closing with greater frequency. The **School Changes** variable suggests that as schools open and close in a district, teacher mobility rises. One would expect that as schools open in a district, teachers seek new facilities, new classrooms, etc. Further, as schools close teachers are forced to shuffle and sort to other schools so that they may continue teaching in their district. The evidence suggests that when 5 schools are added in a district with 100 schools, teacher transfers increase by nearly 30% for every school in the district.

Column 2 runs the same regression using robust standard errors. Using these robust standard errors helps to ease worries about heteroskedasticity in the regression. If

it were true that smaller districts have smaller measurement error than larger districts (which seems plausible), then robust standard errors help to control for this problem. We see roughly the same results as the regression in Column 1. **School Size** and **School Difference** have the predicted sign and are significant at the 1% level. **School Changes** also has the predicted sign and is significant at the 0.05% level.

Column 3 reports the same regression as in Column 1 except that one additional variable is included. This variable (**Dislike Supervisor**) is a rough measure of teacher dissatisfaction with administrative oversight in one's own school. It is calculated for each district as the number of teachers per school who cited "dissatisfaction with supervisor" as a reason for voluntarily terminating their employment on the Teacher Exit Interview from the 2000-2001 school year.[5] If teacher discontent with administration leads to greater mobility, then this variable should be positively correlated with teacher transfers.

In Column 3, the variable **Dislike Supervisor** has the predicted sign but is not significant. It suggests that job environments and teacher attitudes are not necessarily an important part of the movement decision. This result seems to defy intuition, however in the next section I will show later that this result is marginal, and that **Dislike Supervisor** is sensitive to model specification. Column 4 reports the same regression as in Column 3 using robust standard errors. It is also worth noting that the **Dislike Supervisor** variable is not an ideal measure of overall teacher attitudes towards administration because it does not include teachers who continued to teach in their own districts and only reflects a self-

[5] This survey includes teachers who continued to teach in other districts or other states, teachers who stayed employed in education departments in and outside of their district, teachers who left the education profession altogether, those who retired, and teachers who left for private schools in and out of their district.

selecting subgroup of teachers. It does however have some benefits. First, since there is no descriptive teacher evaluation of administration formally collected by the FLDOE or school districts, nor is there useable county data of the same nature, this variable offers at least some approximation of what that data might yield. Secondly, because the teachers in these interviews have already committed themselves to alternative employment, the responses given in these interviews are assured to be candid and honest. It is unlikely that a teacher respondent fears having their identity becoming known to others in these interviews.

Overall, these results seem at least superficially consistent with Hanushek, Kain and Rivkin findings (1999, 2001) that student characteristics influence mobility, and that salary changes at best have only a modest impact. However none of these variables seem to have a powerful an impact as the **School Changes** variable. There seems to be a large disparity between the predictive power of this variable, and other variables. To ensure that our results are valid, and to understand why each variable induces the kind of change it does, I check the robustness of the data.

2.5 Robustness Checks

2.5.1 Re-examining the Data

With respect to the **School Changes** variable, one observation in the dataset experienced an abnormally high rate of school change between the years of 2000-2001 and 2001-2002. This county in particular closed two school facilities to open one, much larger school, that would incorporate all of the existing student population of the other schools, however the small size of this district seems to exacerbate the measurement of the **School Changes** variable. It is possible that this one observation alone is driving the results of the **School Changes** variable. If this observation is more of an outlier, then it

may be overstating the overall importance of the variable it influences. An OLS regression analysis without this outlier is reported in Table 2-3.

Once the sample is modified to eliminate the possible outlier, the magnitude of the coefficient of the **School Changes** variable is reduced by almost half, though the variable remains significant. This does not necessarily mean that this variable is not important, however it does show that perhaps this outlier should be dropped to maintain the integrity of these results. These new results suggest that a one standard deviation increase in school openings/closings leads to a 14.55% increase in teacher transfers per school. This change is considerably lower than the previous estimate.

All other variables in the regression have the predicted sign and all of the formerly significant variables are still significant. **Network** and **School Size** are significant at the 1% level, while **School Difference** becomes marginally significant at the 10% level. Column 2 reports the same regression with robust standard errors. Column 3 includes the **Dislike Supervisor** variable, which is significant at the 10% level. This result suggests that there may be some value to job environment and administrative quality that teachers consider in their movement decisions. Finally Column 4 presents the same regression as in Column 3, including robust standard errors. I again assess the impact of each variable independently, by determining the effect of an increase of one standard deviation of each variable. These results can be viewed in Table 2-4. With the exception of **School Changes**, there are no substantial differences in the magnitudes of the coefficients of the other variables.

The key variable of interest, **Network**, is positive and significant in each regression specification on Table 2-3, suggesting that networks are an important factor contributing

to teacher movement. However, to confirm that the results are credible, I further test the robustness of these results. Specifically I address whether the variable itself is endogenous to the system. In the following section I test whether the measurement of networking is correlated with some unobserved teacher characteristic(s).

2.5.2 Unobserved Correlation

If my measure of teacher networks is correlated with some unobserved teacher characteristic(s), then it may be true that there is no real networking taking place. Instead, teachers who complete high levels of Inservice may simply be more likely to transfer for other reasons. If that were true then one should expect that teachers who complete Inservice would also be just as likely to move in ways other than just intradistrict transfers. To test whether the **Network** variable is reflecting true networking or is just reflecting self-selection of highly mobile teachers into Inservice, I utilize new dependent variables that depict other types of teacher movement.

Private Move equals the number of teachers who claim a move into private schools per school. The second variable, **District Move**, measures the number of teachers moving into other Florida districts per school. The last variable is teacher movement to other states to teach. This variable, **State Move**, equals the number of teachers moving to other states per school in the district. Regression results with these new dependent variables are reported in Table 2-5 using robust standard errors.

Columns 1 and 2 report the regressions with **Private Move** as the new dependent variable, and using robust standard errors. Columns 3 and 4 do the same with **District Move** as the new dependent variable, and Columns 5 and 6 have **State Move** as the new dependent variable. In each of these specifications, network size does not seem to be a significant factor influencing teacher movement. Since Inservice is not significant to any

of these different kinds of movement, it seems plausible that teachers who tend to move around more generally, are not self-select into attending a greater amount of Inservice. Thus I can be more confident that the positive effect of networking is not simply a reflection of the preferences of more mobile teachers.

2.6 Conclusion

The evidence set forth in this paper supports the argument that Master Inservice components provide an effective environment for teachers to network with each other. These networks seem to provide a basis for teachers to gather important information about schools and allow teachers access to transfer options that might have previously been closed to them. Overall, after controlling for determinants of mobility and eliminating biasing outlier effects, I show that an increase of one standard deviation in Inservice hours causes roughly a 12.74% increase in teacher transfers per school. The results suggest that teachers exploit these networks to sort into schools that they find desirable. If teachers can more effectively sort away from less desirable schools, such as low-income or failing schools, into more desirable schools, then it is easy to conceive that worse schools will be worse off as teacher networks improve.

Of course the goal of the Inservice plans set forth by school districts is to improve teacher quality. However, the unintended consequences of such staff development policies that encourage, or at least facilitate, teacher networking seems to exist as well. Further, the consequence of this particular policy seems to be quite substantial in promoting a potentially harmful action such as teacher transfer. This result may have considerable meaning for the future of public education policy. State officials must begin to weigh the possible negative effects of increased teacher networking with the potential

(and realized) benefits of Inservice, so that such policy analysis can be conducted in the future with a complete understanding of social welfare consequence.

Table 2-1: Ordinary Least Squares Regression onto teacher transfers using full sample with and without robust standard errors

Variable	Column 1	Column 2	Column 3	Column 4
Dependent	Transfers	Transfers	Transfers	Transfers
Network	0.0003261 (1.56)	**0.0003261** **(2.00)****	0.0003474 (1.67)	**0.0003474** **(2.19)****
School Distance	0.0065532 (1.12)	0.0065532 (1.16)	0.0078015 (1.33)	0.0078015 (1.33)
Principal Moves	0.3216886 (0.12)	0.3216886 (0.12)	0.9270341 (0.34)	0.9270341 (0.33)
School Changes	**17.86031** **(4.88)****	**17.86031** **(3.42)****	**17.55322** **(4.84)****	**17.55322** **(3.20)****
School Difference	**0.0584747** **(2.10)****	**0.0584747** **(2.34)****	**0.0557304** **(2.01)****	**0.0557304** **(2.16)****
Salary Difference	-0.0000891 (1.61)	-0.0000891 (1.50)	-0.0000766 (1.38)	-0.0000766 (1.21)
Wage Ratio	-0.0018573 (0.27)	-0.0018573 (0.22)	0.0001112 (0.02)	0.0001112 (0.02)
School Size	**0.0501213** **(2.46)****	**0.0501213** **(2.80)****	**0.0448708** **(2.19)****	**0.0448708** **(2.96)****
Dislike Supervisor	— —	— —	4.17856 (1.43)	4.17856 (1.17)
One HS	-0.7262709 (1.32)	-0.7262709 (1.27)	-0.7582897 (1.39)	-0.7582897 (1.31)
Transfer Policy PC	-0.2608151 (0.65)	-0.2608151 (0.64)	-0.2026472 (0.51)	-0.2026472 (0.48)
Transfer Policy CC	-0.2381146 (0.57)	-0.2381146 (0.67)	-0.1001645 (0.24)	-0.1001645 (0.25)
Constant	0.2509715 (0.09)	0.2509715 (0.08)	-0.5541842 (0.20)	-0.5541842 (0.19)
R-Squared	0.6154	0.6154	0.6295	0.6295
Number of Obs.	67	67	67	67
Robust SE	N	Y	N	Y

* denotes significance at the 10% level
**denotes significance at the 5% level

Table 2-2: Percent change teacher transfers per school for a one standard deviation increase in each significant variable with full sample

Variable	Mean	Standard Deviation	Range of % Change
School Changes	0.027	0.05	30.06 to 30.58 %
School Size	33.418	11.61	17.85 to 19.94 %
School Difference	15.945	7.99	15.25 to 16.00 %
Network	1758.317	1047.62	11.49 to 12.46 %

Table 2-3: Ordinary Least Squares Regression onto teacher transfers using sample with biasing outlier omitted

Variable	Column 1	Column 2	Column 3	Column 4
Dependent	Transfers	Transfers	Transfers	Transfers
Network	**0.0003563** **(1.79)***	**0.0003563** **(2.22)****	**0.0003822** **(1.95)***	**0.0003822** **(2.46)****
School Distance	0.0010389 (0.18)	0.0010389 (0.20)	0.0021892 (0.37)	0.0021892 (0.41)
Principal Moves	0.3430332 (0.13)	0.3430332 (0.12)	1.147493 (0.46)	1.147493 (0.36)
School Changes	**9.11579** **(1.90)***	**9.11579** **(1.91)***	**8.322631** **(1.76)***	**8.322631** **(1.73)***
School Difference	0.0433008 (1.60)	**0.0433008** **(1.74)***	0.0393933 (1.48)	0.0393933 (1.55)
Salary Difference	-0.0000671 (1.26)	-0.0000671 (1.06)	-0.0000516 (0.97)	-0.0000516 (0.79)
Wage Ratio	-0.0000751 (0.01)	-0.0000751 (0.01)	0.0020135 (0.30)	0.0020135 (0.30)
School Size	**0.0502127** **(2.60)****	**0.0502127** **(2.89)*****	**0.0442077** **(2.29)****	**0.0442077** **(2.95)*****
Dislike Supervisor	— —	— —	**4.78275** **(1.74)***	4.78275 (1.52)
One HS	-0.4594238 (0.86)	-0.4594238 (0.94)	-0.4825945 (0.92)	-0.4825945 (0.96)
Transfer Policy PC	-0.2247365 (0.59)	-0.2247365 (0.55)	-0.1563356 (0.41)	-0.1563356 (0.37)
Transfer Policy CC	-0.3207089 (0.81)	-0.3207089 (0.89)	-0.1669839 (0.42)	-0.1669839 (0.42)
Constant	0.0560173 (0.02)	0.0560173 (0.02)	-0.8754046 (0.33)	-0.8754046 (0.33)
R-Squared	0.6033	0.6033	0.6247	0.6247
Number of Obs.	66	66	66	66
Robust SE	N	Y	N	Y

Table 2-4: Outlier excluded percent change teacher transfers per school for a one standard deviation increase in each significant variable

Variable	Mean	Standard Deviation	Range of % Change
School Size	33.696	11.48	17.40 to 19.73 %
Network	1777.904	1043.22	12.73 to 13.65 %
School Difference	15.881	8.03	10.84 to 11.91 %
School Changes	0.023	0.04	0.37 to 11.36 %
Dislike Supervisor	0.027	0.05	8.78 %

Table 2-5: Ordinary Least Squares Regression of other types of teacher movements with outlier excluded

Variable	Column 1	Column 2	Column 3	Column 4	Column 5	Column 6
Dependent	Private Move	Private Move	District Move	District Move	State Move	State Move
Network	0.0000101 (1.21)	0.0000104 (1.22)	0.0000342 (0.49)	0.0000305 (0.43)	0.0000014 (0.06)	0.0000034 (0.01)
School Distance	-0.000214 (1.09)	-0.000196 (1.02)	0.0024853 (0.95)	0.0021902 (0.85)	-0.000891 (1.36)	-0.000813 (1.22)
Principal Moves	-0.001503 (0.02)	0.0073589 (0.07)	**-1.829443** **(2.09)****	**-1.930217** **(2.01)****	-0.190106 (0.53)	-0.142873 (0.39)
School Changes	0.125987 (1.11)	0.1214916 (1.07)	4.121108 (1.44)	4.236288 (1.46)	0.6497598 (0.90)	0.5957743 (0.78)
School Difference	-0.001018 (0.73)	-0.001058 (0.74)	0.0119132 (0.99)	0.0124806 (0.99)	0.0015131 (0.44)	0.0012472 (0.35)
Salary Difference	-0.000001 (0.43)	-0.000001 (0.31)	-0.000001 (0.06)	-0.000003 (0.18)	-0.000002 (0.37)	-0.000001 (0.20)
Wage Ratio	0.0002086 (0.96)	0.0002341 (1.02)	**-0.006608** **(2.47)****	**-0.006912** **(2.35)****	**-0.002273** **(2.41)****	**-0.002131** **(2.18)****
School Size	-0.000328 (0.42)	-0.000405 (0.51)	0.0019503 (0.32)	0.0028223 (0.47)	0.0017899 (0.76)	0.0013812 (0.58)
Dislike Supervisor	— —	0.0611693 (0.66)	— —	-0.694535 (0.70)	— —	0.3255332 (1.10)
One HS	-0.014332 (1.04)	-0.014801 (1.08)	0.2299846 (0.87)	0.2333494 (0.88)	0.0774991 (0.95)	0.0759221 (0.91)
Transfer Policy PC	0.0199595 (1.06)	0.0197389 (1.05)	-0.111869 (0.73)	-0.121802 (0.81)	-0.007048 (0.17)	-0.002392 (0.06)
Transfer Policy CC	0.0022509 (0.12)	0.0042704 (0.21)	-0.174603 (1.20)	-0.196926 (1.29)	-0.050714 (1.04)	-0.040251 (0.78)
Constant	-0.041984 (0.50)	-0.053770 (0.59)	**2.621647** **(2.52)****	**2.756905** **(2.38)****	**1.011499** **(2.84)****	**0.9481022** **(2.54)****
R-Squared	0.1931	0.1977	0.3416	0.3461	0.2398	0.2511
Number of Obs.	66	66	66	66	66	66
Robust SE	Y	Y	Y	Y	Y	Y
Outlier	N	N	N	N	N	N

CHAPTER 3
WITH A LITTLE HELP FROM MY FRIENDS: EVIDENCE OF TEACHER NETWORKS USING MICRO DATA

3.1 Introduction

In the last several years, the issue of teacher mobility has come to the foreground of discussions about the public education. Parents and families who value teacher quality have come to realize that teacher mobility is a topic that has potentially far reaching consequences, not just for education labor markets, but also in terms of levels of education and student achievement. Schools that struggle with teacher retention often find job vacancies difficult to fill, and may compensate by hiring less than fully qualified teachers, expanding class sizes, canceling course offerings and assigning teachers from other subject areas (NCES 1997). Of course these kinds of actions may have adverse effects on student learning and achievement. As a result, economists and policy makers have done more to explore the issues of teacher attrition and movement within school districts. The goals of such recent studies have been to shed light on the questions of which teachers are the most mobile, how do these teachers move, when do these movements take place, and what are the potential implications for students.

The most recent studies focused on teacher transfer behavior and sorting within education have provided evidence that supports the notion that student characteristics and school quality are important factors in a teacher's transfer decision (Hanushek, Kain, and Rivkin 2001). This also seems consistent with teacher self-reported information. In the 2000-2001 Teacher Follow-up Survey, administered by the National Center of Education

Statistics, 32% of teachers reported poor workplace conditions as a primary reason for the movement from one school to another. Research shows that teachers sort away from low-income, low-achieving schools. Consequently, less experienced teachers are more typically found in poor, nonwhite, low-performing schools, particularly those in urban areas (Lankford, Loeb, and Wyckoff 2002).

For policymakers, this can be a troubling result. If more experienced teachers tend to sort away from poorly performing schools, then it is the students at these schools who stand to lose the most, because experienced teachers have been found to be more effective than novice teachers in terms of higher student achievement (Hanushek, Kain, and Rivkin 1998, 1999).[1] In addition, urban schools and highly urban school districts face high rates of teacher turnover (Imazeki 2003). If teachers are sorting away from these areas, then these urban districts may have the most difficult time filling vacancies with replacement staff.

Aside from student characteristics, economists have also studied the potential effects of salary changes on teacher mobility. So far researchers have found that salary levels have only a modest impact in terms of student achievement and almost no impact on teacher mobility (Hanushek, Kain, and Rivkin 1999). However, others have also found that salary can influence a teacher's movement choice in ways other than strict transfer, such as length of stay in teaching (Murnane and Olsen 1990). Although much has been learned about teacher mobility, researchers still do not have the complete picture on how teachers move and why.

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[1] Hanushek, Kain and Rivkin (1998) show that variations in teacher quality account for 7.5% of the total variation in student achievement. In addition, teacher quality differences tend to outweigh school quality differences considerably.

The focus of this paper is to better understand the nature of teacher transfers from school to school by introducing the idea of teacher networks as an effective sorting mechanism for public school teachers. To study this issue I rely on information provided by the 1999-2000 Schools and Staffing Survey (SASS) put out by the National Center for Education Statistics. The individually detailed and specific nature of the dataset not only allows me to consider how individual networks affect mobility but also allows me to control for a well-defined set of variables, giving my study a more complete and detailed look at networks. This paper will begin discussion on the notion of teacher networks, and will address how these networks influence teacher movement and to what extent. Specifically this paper describes how professional development activities (PDAs) provide opportunity for teachers to network with other teachers. PDAs will be shown to have a sizable impact on intradistrict teacher mobility.

3.2 The Identification Strategy

Before I describe how I identify teacher networks, let me first discuss what teacher networks are, how they are useful, and how teachers build these networks.

3.2.1 What Are Networks And How Are They Useful

The term network in this paper is defined as group or "family" of co-workers or colleagues within one's field of work. Specifically, in this paper a network refers to a teacher's associations with other teachers who work within their own district. Although the context of a network here is limited to the field of education, people from all professions form networks for various reasons.

There are many reasons why any professional or businessperson would network with others in their field of work. They yearn to share ideas, understand their competition, and exchange information that can benefit their personal interests,

sometimes in a mutual way. Why teachers network with each other is not so different. They exchange classroom experiences and teaching methods as well as information on workplace environments, as well as build personal bonds that enhance their feelings towards the profession, or even their personal lives. As a result, teachers may be able to use their networks to plan intradistrict school events more efficiently, find work in the summer time, or sometimes just to socialize with others in their field.

However, there are two key uses of teacher networks that are the focus of this paper. First I argue that teachers use their networks to gain greater information about school characteristics. Because there are many characteristics about schools that may be difficult to observe (existing job vacancies, quality of administrative support, departmental environments, etc.), networks may allow teachers to evaluate other schools in their district more efficiently. Greater information about schools should allow teachers to sort into their preferred school more easily.

Secondly, I argue that teachers use their networks to reveal their quality as a job candidate in a less costly way. Because it can be costly (or sometimes impossible) to reveal one's aptitude for a job vacancy, teachers may call on their networks to provide important or relevant information to potential employers. For example, a teacher may ask another teacher to put in a favorable word to a hiring principal. In this way, networks provide a valuable advantage to those seeking employment, while at the same time being a more credible or reliable source of information for potential employers. If teachers do use their networks to foster more efficient, less costly sorting behavior, then policies that encourage network building may have unintended outcomes for teacher mobility and possibly student achievement.

3.2.2 How Do Teachers Build Networks?

Clearly some teacher networking takes place in private forums. Teachers may spend recreational time together getting to know one another, or they may talk over lunch. However these examples of the networking process are not feasibly quantified. To find a more measurable environment where networking takes place, I focus on professional development activities. Professional Development Activities (PDAs) consist of an array of different activities geared towards maintaining and enhancing teacher competency and knowledge of various issues (including such issues as the use of technology in the classroom, student assessment and state education standards, teaching methods, etc.).

PDAs can include many different types of activities such as workshops, mentoring and/or peer observation, collaborative research, University coursework, etc. Table 3-1 reports a summary of self-reported professional development activity from the 1999-2000 Schools and Staffing Survey.[2] Though each of these activities may be available to most teachers in the sample, attending workshops/conferences/training clearly seems to be the most popular form of PDA in the sample, with nearly 94% of teachers reporting they have engaged in this form of PDA in the past year (prior to the 1999-2000 school year).

PDAs provide an ideal environment for teachers to network with one another. Teachers who participate in workshops or conferences have greater opportunity to meet teachers from other schools within their district and enhance their networks. In addition most PDA is geared to a specific employment group within the school system. This

[2] The 1999-2000 Schools and Staffing Survey (SASS) is a survey of involving approximately 56,000 public school teachers from all over the U.S., and is produced by the National Center of Education Statistics. It is the main data source for this chapter and is described further in Section 3.3.

means that administrative personnel do not usually engage in the same PDA as teachers. Likewise teachers do not usually engage in the same PDA as other support staff (such as bus drivers, cafeteria workers, etc.). Thus I can be somewhat confident that if teachers are building networks through PDA, they are generally relevant networks.

Besides simply meeting other teachers through PDA, these activities often provide an environment conducive to promoting teacher interaction. Workshops especially are designed in such a way that encourages interaction among participants. Workshop assemblies are usually long in duration (often several hours per session) and can span weeks or months. In addition, individuals are often called upon to share experiences with all the participants, or present material regarding how the workshop has influenced their classroom activity.

3.2.3 Measuring Teacher Networks

In order to measure the size of a teacher's network, I utilize national survey data that provides information on professional development activity. The 1999-2000 Schools and Staffing Survey (SASS), the survey from which the overall dataset is derived, asks respondents to report the number of hours they spent in each of six major categories[3] of PDA within the last school year (1998-99). Rather than report the specific number of hours, respondents are asked to select a (predetermined) range of hours that includes the actual number of hours they have spent in each category of PDA. Using the midpoint of the reported range as my best estimate of reported hours, I create a continuous variable of

[3] These six categories are: PDAs with focus on 1) in-depth study of main assignment, 2) content and performance standards of main assignment, 3) methods of teaching, 4) uses of computers for instruction, 5) student assessment/methods of testing, and 6) student management in the classroom.

PDA Hours (*Hours)*, by summing the reported hours across all six PDA categories for each individual.[4]

In general the distribution of PDA Hours across my sample is skewed to the left with approximately 3.1 % of all teachers reporting that they have not engaged in any form of PDA within the past year. Both the level of skewness of the sample (approximately1.30) and percent of teachers reporting no PDA within the past year are roughly equivalent across movement categories as well.[5] In addition, the distribution of PDA Hours across these movement categories is also roughly identical. Of those teachers who did not move, 24.8 % (or 332) are in the upper quartile of the entire distribution. Likewise 25.2 % of teachers (or 88) who transferred within district were in the upper quartile of the entire distribution of PDA Hours.[6] It is also worth noting that in each individual PDA category, only a small fraction of teachers are top-coded (reporting the maximum number of hours allowed in the survey).[7] Although for each PDA category the percent of teachers who are top-coded ranges from 16.4 % to 2.1 %, these differences do not vary significantly across movement types. Overall the data seems to indicate that

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[4] It is important to note that the highest range of hours that teachers could report is "33 or more". Where this happens, I use 60 hours as the maximum value in this range. 60 seems to be a reasonable upper limit of hours, however I have also tested maximum values at 48 and 64. These changes do not significantly impact the results.

[5] The PDA distribution of teachers who move intradistrict is slightly more skewed (1.37) than the same distribution for those who did not move (1.25). Additionally, the kurtosis of the PDA distribution for all movement categories is nearly equivalent (4.34 for non-movers, 4.64 for intradistrict movers, and 4.61 for those who leave their district).

[6] Of those teachers who moved out of their district, 21.7 % (or 98) are in the upper quartile of the entire distribution. This includes both interdistrict and interstate movement.

[7] There is only one teacher in my sample who is top-coded for every category of PDA (and has thus taken the maximum amount of PDA measured in the survey)

along the PDA measurement, teachers in various movement categories do not look different enough from one another to warrant any concern these groups are incomparable.

3.2.4 The Basic Model

In this basic model I am attempting to explain how a teacher's decision to transfer to another school (in year *t*) within their own district is affected by their network. My measure of a teacher network here is the number of hours a teacher spent in professional development activities in the previous year (in year $t - 1$). I can start by expressing a simple regression model in the following form:

$$\text{Transfer}_{(t)} = \alpha + \beta(\text{PDA Hours}_{(t-1)}) + \delta X$$

Here the dependent variable is a dummy that expresses a teacher's transfer within his own district. The dependent variable (Transfer) is assigned a value of 1 for a within district move, and 0 otherwise. In addition to the amount of networking a teacher does, the basic characteristics of the most mobile teachers are somewhat different from those who are less mobile. To capture these differences I include variable X, a vector of individual and school characteristics that influence a transfer decision. Table 3-2 reports some descriptive statistics about the characteristics of teachers who do and do not move around in various ways. The most notable difference is that teachers who move within district tend on average to be about 3-6 years less experienced (and younger) then those who do not move. Since inexperienced teachers are generally not as far along the career path as more experienced teachers, this difference seems intuitive. In addition, I must also consider marital status and job status in the model. Those teachers who are not married may find a move to a new school to be more costly since spousal relocation can complicate matters. Those teachers who are employed part-time by their district should

have a greater overall benefit to searching for job vacancies since they stand to benefit from becoming a full-time employee.

Aside from the natural differences that exist among teachers, a teacher's transfer decision should also depend on the working conditions he/she faced the previous year. Specifically, a teacher's decision to transfer may be influenced by student quality/characteristics, administrative quality/characteristics, or compensation levels. As a measure of student quality (*L*), I construct a measure:

%Free Lunch Eligible = (%free-lunch eligible$_{school,(t-1)}$ - %free-lunch eligible$_{district,(t-1)}$)

This measure is the difference between the percent of the student population who are free-lunch eligible at a teacher's school and the average percent of the student population who are free-lunch eligible at the district level. It is designed to capture the socio-economic status (SES) of the student population at a teacher's initial school relative to the SES of the district as a whole. Given that teachers desire to sort away from low SES schools, this variable should have a positive impact on transfers.

To measure administrative quality (*Adm*), I utilize the variable:

Poor Administration = a dummy variable, receives a value of 1 is a teacher reports that administrative support is poor within the past year ($t-1$)

This variable captures a teacher's perception of how supportive/encouraging the administration at their school is towards its staff. The predicted sign of this variable should be positive, since in schools where administrative quality is perceived to be poor, teacher mobility should be higher.[8] Some additional measures of the working conditions in my regressions specification include:

[8] I have also measured poor administrative quality using the average teacher response for each school. My results do not change with this different measure.

Student Threat = a dummy variable, equals 1 if a teacher reports being threatened by a student within the past year ($t - 1$) (*Thr*).

Again because receiving a student threat indicates poor working conditions, the sign of the coefficient should be positive to express teachers' preferences to sort away from undesirable schools.[9]

Bonus Pay = a dummy variable, equals 1 if a teacher reports receiving bonus pay, separate from salary and extracurricular pay within the past year (*BP*).

Bonus pay should give teachers an incentive to stay at their existing school, and should therefore be negatively correlated with transfer rates.

I also use the self-reported data to construct a measure of job satisfaction (*Job*) that reflects a teacher's general attitude toward their workplace:

Job Dissatisfaction = a dummy variable, equals 1 if a teacher reports being strongly dissatisfied with being a teacher at their school in year ($t - 1$) (*Job*)

This variable is intended to capture not just the quality of the students, but also a teacher's perception about the quality of their co-workers, their administrative superiors, school resource availability, and general issues surrounding their employment. Since greater general job dissatisfaction should lead to greater mobility, I expect the sign of this variable to be positive.

While it is true that salary differences between schools seem to affect teacher transfer decisions, this analysis is limited to those teachers whose movement is within their district. Given that salaries schedules are generally set at the district level and do

[9] I have also measured student threat using the average teacher response for each school. My results do not change with this different measure.

not vary across schools in the same district, base salary differences between schools are negligible[10]. As a result, I do not include base salary as a variable in my analysis.[11]

I also control for whether a teacher belongs to a union in this model for two main reasons. The first is that union membership may provide existing teachers with preferential treatment in filling a job vacancy. Another reason is that districts may find unionized teachers more difficult to fire. As a result, problem teachers may be shuffled around more often. These two explanations of how unions affect transfer rates are very different, however in both scenarios union membership should positively affect the probability of transferring.

Finally I add state fixed-effects into the regression model to control for basic institutional and cultural differences across states, and a dummy = 1 for missing data in the % Free-Lunch (*L)* variable.[12] It would be ideal to use school district fixed-effects, however, doing this results in a dramatic reduction of my sample size.[13] Once this is done, the regression takes the following form:

$$\begin{aligned}\text{Transfer}_{(t)} = \alpha &+ \beta_1\text{Hours}_{(t-1)} + \beta_2\text{L}_{(t-1)} + \beta_3\text{Thr}_{(t-1)} + \beta_4\text{BP}_{(t-1)} + \beta_5\text{PT}_{(t-1)} + \beta_6\text{Exp}_{(t-1)} \\ &+ \beta_7\text{Adm}_{(t-1)} + \beta_8\text{Mar}_{(t-1)} + \beta_9\text{Job}_{(t-1)} + \beta_{10}\text{Age} + \beta_{11}\text{Union} + \beta_{12}\text{LDummy} \\ &+ \beta_{13}\text{StateFE}\end{aligned}$$

where *PT* is a dummy for job type (1 if part-time, 0 otherwise), *Exp* is the total years of full-time experience, *Age* is teacher age, *Union* is a dummy for union membership (1 is

[10] In the SASS, over 98% of school districts reported having a set salary schedule for teachers in their district. This means that virtually every teacher in the sample who moved within district did not face salary differences between the schools in their district

[11] Although other types of compensation such as incentive pay are an important component to one's decision to transfer, those types of compensation could be endogenously determined and therefore inappropriate to include.

[12] The Free-Lunch variable is the only variable in the regression where observations were missing. Roughly 157 (or 9%) of observations were missing this data.

[13] Never the less I show on Table 3-7 that the inclusion of district fixed-effects does not alter the results.

unionized, 0 otherwise) and *Mar* is a dummy for marriage status (1 is married, 0 otherwise).

3.3 Data

The data for this model comes from the 1999 – 2000 Schools and Staffing Survey (SASS) and the 2000 – 2001 Teacher Follow-up Survey (TFS) produced by the National Center for Education Statistics (NCES).[14] The data are comprised of individual (teacher) level survey information that can be matched across surveys, as well as relevant survey information for the relevant schools, principals and school districts. In the original 1999-2000 SASS survey there were over 52,000 respondents, but the TFS consists of only a subset of those original respondents.

In aggregate the TFS includes 6,758 teachers, however not all of the teachers in the survey are included in this analysis.[15] Since this analysis focuses on public school teachers who move within district relative to those who do not change schools, I eliminate those respondents who either leave their district, or leave teaching altogether. Later in the paper I will use other types of movement (interdistrict and interstate movements) to show that PDA hours do not influence other mobility decisions, particularly those decisions where networks should not matter.

[14] The datasets utilized in this paper contain identifiable and sensitive information. While there are versions of these data that are available to the general public, the datasets used in this paper are only available at the discretion of the NCES, and with their proper approval.

[15] Those respondents in the TFS who left the teaching profession after the 1999-2000 school year (2,374 respondents) are given a version of the TFS that is different from the survey given to those who did not leave teaching.

In addition, I eliminate those teachers who initially teach at private, Indian, or charter schools, teachers who reported their move was mostly involuntary[16], and those teachers who were not considered full or part time regular teachers in 1999 (e.g., specialists, substitutes, student teachers and itinerant teachers). Once these criteria are imposed on the data set, 1,688 observations remain. Of the 1,688 observations in the dataset, 349 (or approximately 20.7%) of those were teachers who transferred within their district.

3.4 Empirical Evidence

Because the dependent variable is a binary choice variable with a successful transfer equal to 1, probit estimation is appropriate. Column 1 of Table 3-3 reports a preliminary regression of PDA Hours onto the dependent variable, with the specified covariates also included.[17] At first glance it seems that networking does not have a significant impact on the likelihood of transfer. Networks though are a story of collegial kinship, and like any friendship, benefits may not be realized until the parties involved are familiar and comfortable enough with one another to begin a relationship. Thus networks may be difficult to establish initially. Further, networks may require a level of maintenance. If that is the case, then teachers who engage in PDA frequently may have an increasing advantage over those who do not. This may imply that there are increasing returns to hours spent in PDA in terms of networking. A simple test of this assumption is to include a squared term of PDA Hours.

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[16] It is also worth noting that any involuntary teacher movements who may be unavoidably included in the sample (thus creating noisy measures of intradistrict movement), are likely to bias any positive estimates of the effect of networking towards zero.

[17] All regression estimates are clustered at the district level to control for possible standard error correlation at the district level. In addition, all regression estimates include robust standard errors, to account for possible heteroskedasticity.

In Column 2 of Table 3-3, the addition of a squared measure of PDA Hours in the regression seems to suggest that there may be some value to networking. Specifically, the coefficient on the squared term is positive and significant at the 0.025 level, which may indicate that while having only a few hours networking may have no impact on transferring, positive returns to networking may exist for large values of PDA Hours. To further test whether this is the case, I use a spline to allow the effect of an additional hour of PDA to be greater at high levels of PDA hours then at low levels of PDA hours. The spline estimates a continuous, piecewise-linear relationship between PDA Hours and transfer probability, and is defined as follows:

PDA Hours (H) : from [0 – H*] hours:
$= H$ if $H < H^*$
$= H^*$ if $H \geq H^*$

PDA Hours (H) : from [H* - H_{max}] hours:
$= 0$ if $H \leq H^*$
$= (H - H^*)$ if $H > H^*$

Table 3-4 reports the coefficients of several regressions using the spline. I test various cutoffs of H*, where H* is chosen at decile intervals along the distribution of PDA Hours. Table 3-4 illustrates that the 2nd segment of the spline is positive and significant for most decile cutoffs[18], but the best fit for the regression is where the cutoff is at the 80th percentile. This table confirms that additional PDA Hours, particularly for those teachers in the top 20% of the overall distribution of PDA Hours, seem to have a positive and significant impact on a teacher's transfer decision. Interpretation of the coefficients suggests that a one standard deviation increase in the total number if PDA

[18] For every cutoff chosen at or above the 20th percentile of the distribution, the 2nd segment of the spline estimation is positive and significant.

Hours (approximately 46.6) can lead to a 0.020 to 0.078 increase in the probability of transferring within one's own school district.

Other covariates also seem to have a significant impact of the transfer decision. The evidence seems to indicate that teachers with less experience and those who work only part-time are significantly more likely to move between schools. Intuitively this seems valid since teachers with little experience have had arguably less time to sort into a desirable school and should therefore be more likely to transfer than teachers who are further along the career path. In addition, part-time employees stand to secure higher wages if they search for full-time employment, and have lower time-cost to job searching.

Besides the natural sorting taking place for younger/less experienced teachers, teachers who work in low SES schools and who face undesirable working conditions clearly have a greater incentive to sort into better schools, especially given that additional/incentive pay is rarely available for teachers to work in less desirable schools. The evidence in Tables 3-3 and 3-4 seem to support this claim. Teachers at schools where student SES as a whole is lower than the district average, schools where their overall job satisfaction is generally low, and teachers that feel administrative quality is generally poor, seem to have a higher propensity to sort.

Lastly, Tables 3-3 and 3-4 show that there is also some advantage to a teacher being part of a teacher union in terms of increased mobility. This could be because teachers who are part of a union receive priority in filling a position vacancy. Another interpretation of this union effect could also be that unionized teachers are difficult to fire, and therefore get shuffled around more often. Overall the results shown in Tables 3-

3 and 3-4 are consistent with previous findings in the literature, and not at all surprising given the economics going on. These regression specifications provides some valuable insight into the teacher transfer decision, and also tells us that teacher networks are not an integral component to teacher mobility.

3.5 Robustness Checks

The evidence in the previous section suggests that there is a significant relationship between teacher networks and transfer behavior. However it is important to test the robustness of these results. If the time a teacher spends in PDA is simply correlated with some other (possibly unobservable) characteristic(s), then the explanation provided by networking may be inaccurate. If it is true that teachers who spend a great deal of time doing professional development activities are simply more likely to transfer, then the results are spurious in a networking framework.

Before I test the robustness of the results in the previous section, I look to see if those teachers who engage in the most professional development look substantially different from the average teacher. I compare the characteristics of teachers in every decile of the distribution of PDA hours to the average characteristics of the entire sample of full and part-time teachers (2,280 observations). The results of these comparisons are reported in Figure 3-1. Each graph on Figure 3-1 shows the mean of the given characteristic for each interval (0% – 10%, 10% - 20%, etc) of the PDA distribution, centered about the mean for the entire sample. I use the standard errors at each interval to construct a 95% confidence interval on which the mean lies, and test whether the sample average falls in the range of the confidence interval.

There are only a few characteristics where teachers who engage in the most PDA look significantly different from the sample average. The graphs of Age and Full-Time

Experience illustrate that teachers in the top end of the distribution of PDA Hours are significantly older and more experienced. However, previous evidence suggests that more experienced teachers, who are also generally older, are also less likely to transfer schools. Therefore, if older and more experienced teachers are taking the most hours of professional development, then we should expect PDA Hours to be negatively correlated with the probability of transferring, which does not seem to be the case.

The graphs in Figure 3-1 may also suggest that those teachers at the very bottom of the distribution of PDA Hours may be disproportionately white, male part-time teachers, with respect to the entire distribution of hours in PDA. However it is not clear that this presents a problem of selection bias in who decides to take PDA as it relates to transfer rates. Currently there is no evidence to suggest that qualities of teacher race or gender are effective at predicting/explaining teacher movement. In addition, Table 3-4 showed that part-time teachers are more likely to move than full-time teachers. If part-time teachers are more likely to spend only a small amount of time in PDA, this should also downwardly bias the estimated effect of PDA Hours on transfers. These results are promising, however they are not conclusive. Teachers may still be sorting into the distribution of PDA Hours based on some characteristic that is not observable.

To test whether teachers are sorting into high or low levels of professional development activity, I begin by looking at other types of teacher movement. If it is true that teachers with high levels of PDA participation are simply more likely to move, then one might expect these same teachers are also more likely to move in ways other than just intradistrict transfer. However, since most PDAs are activities limited to one's own school district, then networks should be largely ineffective for movement outside the

scope of the immediate district. So if PDA hours are a strong predictor of other types of movement, then that may be an indication that the network relationship is unjustified. Tables 3-5 and 3-6 report similar regressions similar to those reported in Table 3-4.

Table 3-5 shows the effect of a change in PDA Hours on the probability of movement to another school district within one's state. Table 3-6 shows the effect of a change in PDA Hours on the probability of movement to another school district in another state. In both cases, the effect of increased PDA Hours is not significantly different from zero. For Tables 3-5 and 3-6, neither spline segment is significant, which suggests that the amount of time a teacher spends in PDAs has no significant impact on how likely they are to move out-of-district (within the state), or even out-of-state. This helps to quell some suspicion that teachers with high levels of PDA are simply more mobile.

It may also be possible that the relationship between PDA and mobility is a function of district characteristics or behavior. For example, if districts with "naturally" high levels of teacher mobility also demand an unusually high number of PDA hours for their faculty (arguably this may be the case with some urban school districts), then the positive effect of networking is simply a reflection of high levels of mobility in a subset of districts. To account for this possibility, I repeat the regressions in Table 3-4 with district fixed effects. Because the use of district-fixed effects limits the sample to districts where both movement and nonmovement occur, the number of observations drops to only 398. Naturally this limits the power of the analysis, and is the main problem with using district fixed effects throughout. Table 3-7 reports the results of these regressions. According to the data, high levels of PDA have a positive effect on the

transfer decision and low levels of PDA have a negative effect on the transfer decision. The results in Table 3-7 confirm that district specific behavior does not fully explain the relationship hours in PDA and mobility.

3.6 Conclusion

The evidence presented in this paper clearly seems to indicate that time spent in professional development activities leads to networks that enhance teacher mobility within their own district. These PDAs arguably provide a forum in which teachers can build networks to gather important information about school characteristics, work environments and job availability from which they can sort in a less costly, more efficient way. It also seems plausible that teachers can use their networks with favored colleagues to reveal their aptitude for a job vacancy, which may allow teachers access to transfer options that might have previously been closed to them.

Results show that a one standard deviation increase in the amount of networking leads to a 0.020 to 0.078 increase in the probability of transferring within one's own school district. If teachers use these networks to more effectively sort away from less desirable schools, such as low-income or failing schools, then networks arising from PDAs may contribute to the disparities in teacher quality across school types. It is also possible that if networks make it more difficult for low achieving schools to retain qualified teachers, then student achievement at these schools may also be diminished by networks. Oddly enough, this unintended consequence of staff development policies would be subversive to the goal of PDAs, which is to improve education through greater teacher quality.

Table 3-1: Professional Development Activities Summary Statistics

	Number of Teachers Who Reported		
	Yes	No	Percent Who Participated
University Course in your Main Teaching Field	785	1495	34.4 %
University Course NOT in your Main Teaching Field	614	1666	26.9 %
Observational Visits to Other Schools	766	1514	33.6 %
Individual or Collaborative Reseach on a Topic of Interest	1032	1248	45.3 %
Regularly Scheduled Collaboration with other Teachers on Issues of Instruction	1561	719	68.5 %
Mentoring or Peer Observation	1014	1266	44.4 %
Participating in a Network of Teachers (e.g. Internet Organization)	559	1721	24.5 %
Attending Workshops, Conferences or Training	2141	139	93.9 %
Presenter at a Workshop, Conference or Training	452	1828	19.8 %

Table 3-2: Summary Statistics for Various Teacher Groups

	No Move		Move		
			Intra-District	Inter-District	Inter-State
Number of Teachers	1400 (61.4 %)		370 (16.2 %)	484 (21.2 %)	96 (3.9 %)
	Mean	Std. Dev.	Mean	Mean	Mean
PDA Hours	55.728	46.662	59.000	49.636	48.220
% Free-Lunch Eligible	0.002	0.225	0.045	-0.014	-0.021
Bonus Pay	0.137	0.344	0.113	0.101	0.146
Student Threat	0.095	0.294	0.127	0.130	0.083
Job Dissatisfaction	0.021	0.144	0.048	0.066	0.072
Full-Time Experience	10.915	9.856	7.959	5.134	4.583
Part-time employment	0.040	0.197	0.081	0.041	0.010
Poor Administration	0.069	0.254	0.102	0.101	0.104
Union Member	0.783	0.411	0.778	0.652	0.667
Age	40.484	11.340	37.956	33.634	32.030
Gender (Male)	0.282	0.450	0.235	0.299	0.292
Race (White)	0.902	0.297	0.862	0.902	0.927
Marital Status	0.695	0.460	0.678	0.665	0.667

Table 3-3: Probit Regression of Intra-District Movement

Variables	1	2
PDA Hours	-0.0010626 (1.27)	-0.0006944 (0.62)
$(\text{PDA Hours})^2$	— —	0.00003 (2.30)**
% Free-Lunch Eligible	0.301017 (1.84)*	0.3069657 (1.87)*
Bonus Pay	-0.1669206 (1.40)	-0.1629131 (1.37)
Student Threat	0.1009573 (0.87)	0.1002069 (0.86)
Job Dissatisfaction	0.4224374 (1.85)*	0.4353552 (1.92)*
Full-Time Experience	-0.0143332 (2.34)**	-0.0131213 (2.13)**
Part-time employment	0.5452476 (3.43)**	0.5394231 (3.41)**
Poor Administration	0.3542717 (2.55)**	0.3600298 (2.59)**
Union Member	0.2372197 (2.36)**	0.2386774 (2.37)**
Age	-0.005178 (1.01)	-0.0055949 (1.09)
Marital Status	0.0237581 (0.29)	0.0261624 (0.32)
Dummy for Missing % Free Lunch Eligible	0.1770793 (1.20)	0.176266 (1.20)
Constant	0.2702219 (1.37)	0.2997908 (1.50)
Pseudo R-squared	0.0955	0.0984
Number of Obs.	1627	1627
Robust SE, clustered	Y	Y
State FE	Y	Y

* denotes significance at the 0.05 level
** denotes significance at the 0.025 level

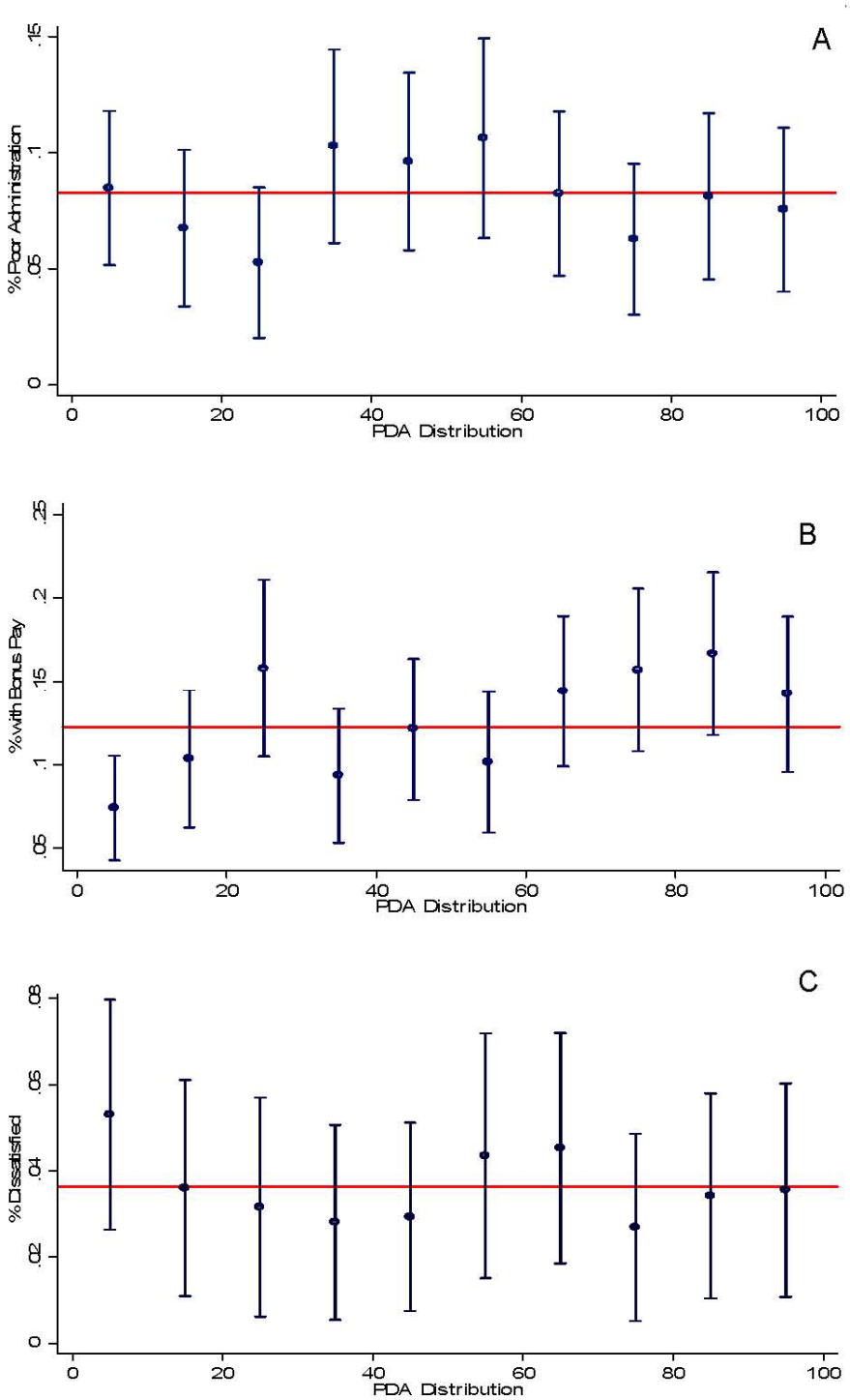
Table 3-4: Probit Regression of Intra-District Movement

Regression of PDA Hours with H* Chosen at Decile Intervals Along the Distribution of PDA Hours

Variables	H* = 9 (10%)	H* = 17 (20%)	H* = 22.5 (30%)	H* = 29.5 (40%)	H* = 40.5 (50%)	H* = 51.5 (60%)	H* = 68 (70%)	H* = 87.5 (80%)	H* = 122.5 (90%)
PDA Hours 0 - H*	-0.0262	-0.0170	-0.0117	-0.0074	-0.0047	-0.0033	-0.0022	-0.0018	-0.0006
	(1.12)	(1.68)	(1.63)	(1.43)	(1.34)	(1.27)	(1.12)	(1.15)	(0.55)
PDA Hours > H*	0.0013	0.0017	0.0018	0.0020	0.0022	0.0026	0.0031	0.0043	0.0061
	(1.52)	(1.85)*	(1.94)*	(1.94)*	(2.00)**	(2.08)**	(2.16)**	(2.45)**	(2.51)**
% Free-Lunch Eligible	0.3041	0.3066	0.3077	0.3074	0.3067	0.3086	0.3117	0.3081	0.3010
	(1.86)*	(1.87)*	(1.88)*	(1.88)*	(1.88)*	(1.89)*	(1.91)*	(1.88)*	(1.83)*
Job Dissatisfaction	0.4214	0.4254	0.4282	0.4285	0.4306	0.4338	0.4377	0.4385	0.4288
	(1.86)*	(1.87)*	(1.88)*	(1.88)*	(1.89)*	(1.90)*	(1.92)*	(1.93)*	(1.89)*
Full-Time Experience	-0.0141	-0.0136	-0.0135	-0.0134	-0.0132	-0.0130	-0.0130	-0.0130	-0.0134
	(2.29)**	(2.22)**	(2.19)**	(2.17)**	(2.13)**	(2.10)**	(2.10)**	(2.10)**	(2.18)**
Part-time employment	0.5383	0.5362	0.5351	0.5373	0.5417	0.5458	0.5479	0.5477	0.5381
	(3.37)**	(3.36)**	(3.36)**	(3.38)**	(3.41)**	(3.44)**	(3.45)**	(3.46)**	(3.41)**
Poor Administration	0.3575	0.3616	0.3636	0.3641	0.3638	0.3621	0.3595	0.3596	0.3593
	(2.57)**	(2.59)**	(2.61)**	(2.61)**	(2.61)**	(2.60)**	(2.58)**	(2.58)**	(2.59)**
Union Member	0.2344	0.2406	0.2419	0.2411	0.2415	0.2416	0.2399	0.2387	0.2373
	(2.32)**	(2.38)**	(2.40)**	(2.39)**	(2.39)**	(2.39)**	(2.37)**	(2.37)**	(2.36)**
Student Threat	0.1011	0.1016	0.1022	0.1025	0.1016	0.1000	0.0984	0.0987	0.0991
	(0.87)	(0.87)	(0.87)	(0.88)	(0.87)	(0.86)	(0.85)	(0.85)	(0.85)
Bonus Pay	-0.1674	-0.1639	-0.1639	-0.1657	-0.1658	-0.1650	-0.1635	-0.1633	-0.1658
	(1.41)	(1.38)	(1.38)	(1.39)	(1.40)	(1.39)	(1.38)	(1.38)	(1.40)
Age	-0.0053	-0.0054	-0.0054	-0.0055	-0.0055	-0.0056	-0.0057	-0.0058	-0.0056
	(1.02)	(1.05)	(1.06)	(1.07)	(1.08)	(1.09)	(1.10)	(1.12)	(1.09)
Dummy for Missing % Free Lunch Eligible	0.1789	0.1779	0.1798	0.1796	0.1790	0.1777	0.1772	0.1807	0.1776
	(1.22)	(1.21)	(1.22)	(1.22)	(1.22)	(1.21)	(1.21)	(1.23)	(1.21)
Marital Status	0.0232	0.0224	0.0232	0.0245	0.0256	0.0265	0.0266	0.0277	0.0264
	(0.28)	(0.27)	(0.28)	(0.30)	(0.31)	(0.32)	(0.33)	(0.34)	(0.32)
Constant	-0.9070	0.5313	0.4935	0.4260	-0.9811	0.3674	0.3596	-0.9753	0.3402
	(1.29)	(2.15)**	(2.09)**	(1.93)*	(1.45)	(1.78)*	(1.75)*	(1.44)	(1.69)
Pseudo R-Squared	0.0993	0.1004	0.1004	0.1002	0.1002	0.1003	0.1005	0.1012	0.1009
Number of Obs.	1627	1627	1627	1627	1627	1627	1627	1627	1627
State FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Robust SE, clustered	Y	Y	Y	Y	Y	Y	Y	Y	Y

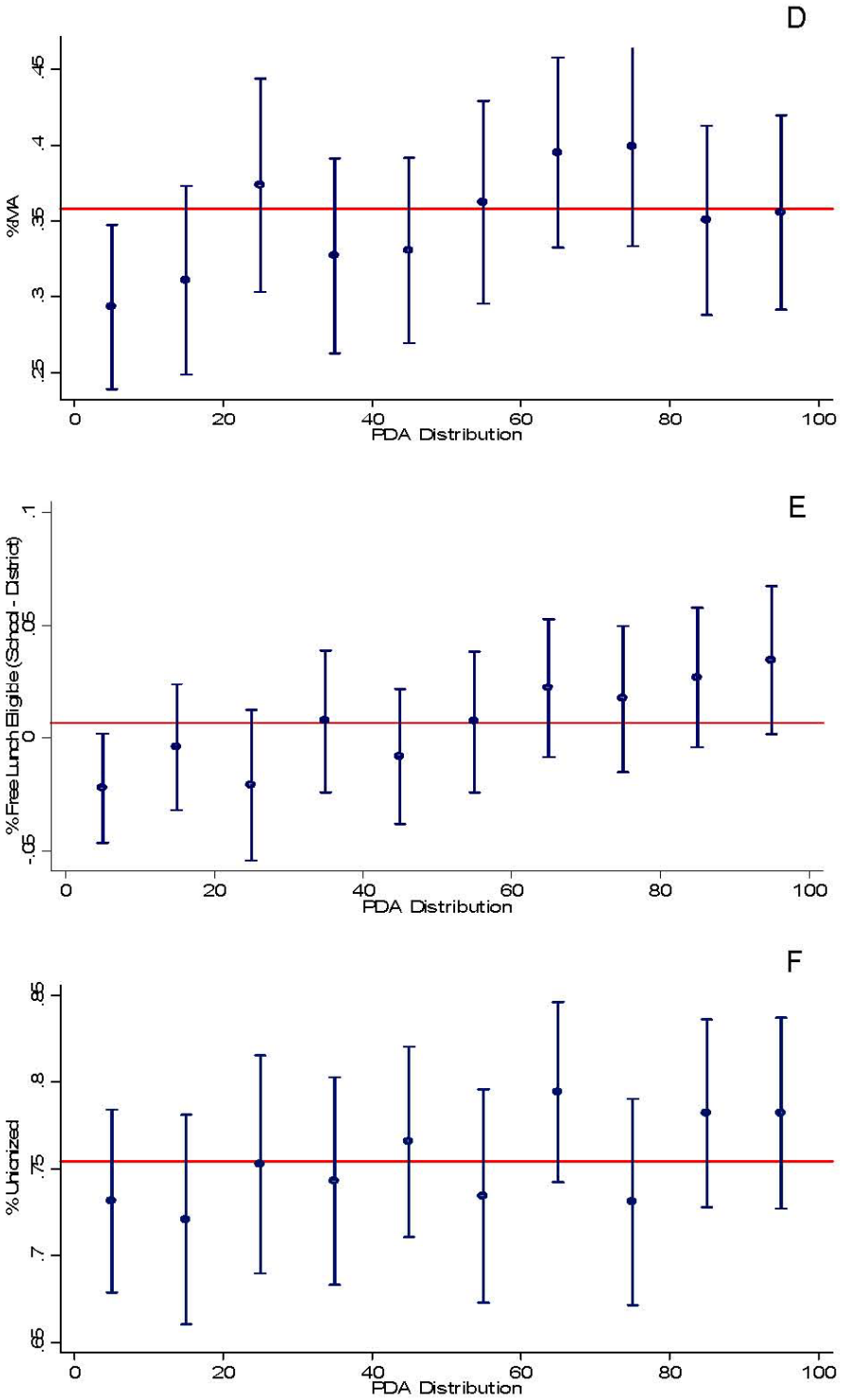
* denoted significance at the 0.05 level

** denoted significance at the 0.025 level



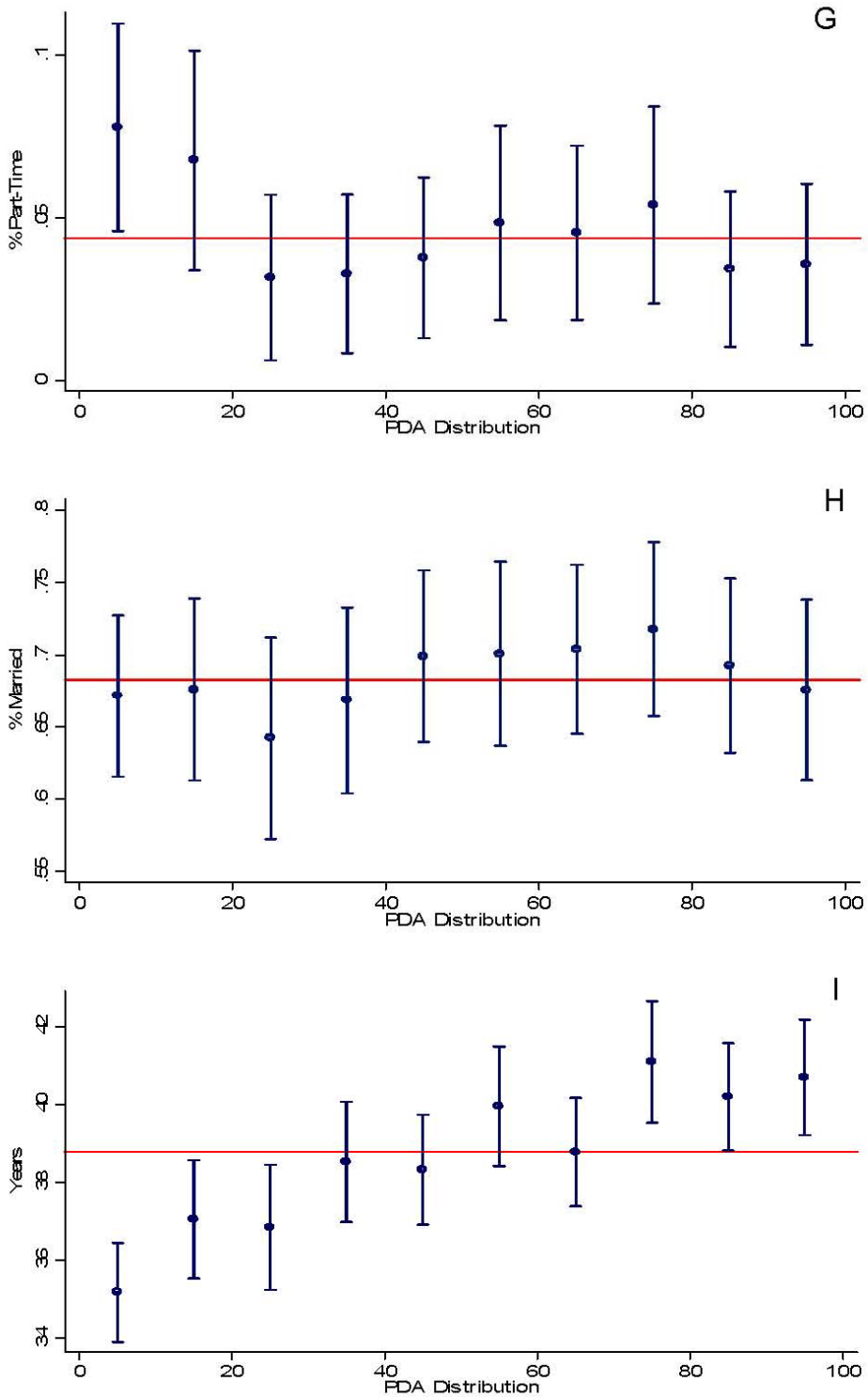
A) Percent of teachers receiving poor administrative support
B) Percent of teachers receiving bonus pay
C) Percent of teachers reporting overall job dissatisfaction

Figure 3-1. Comparisons of Teacher Characteristics Along the Distribution of PDA.



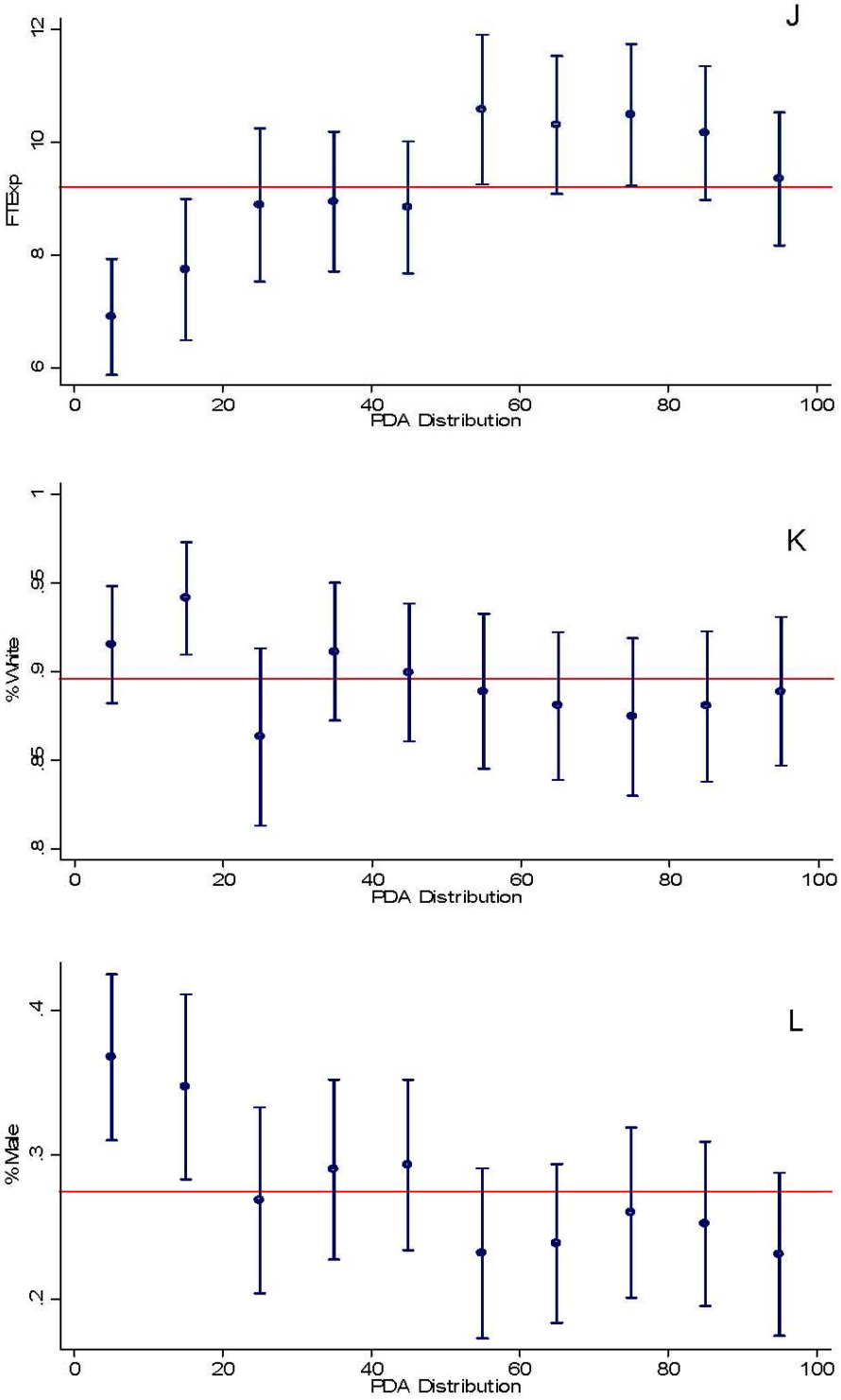
D) Percent of teachers with a master's degree
E) Percent of free-lunch eligible relative to the district average
F) Percent of teachers who belong to a union

Figure 3-1. Continued.



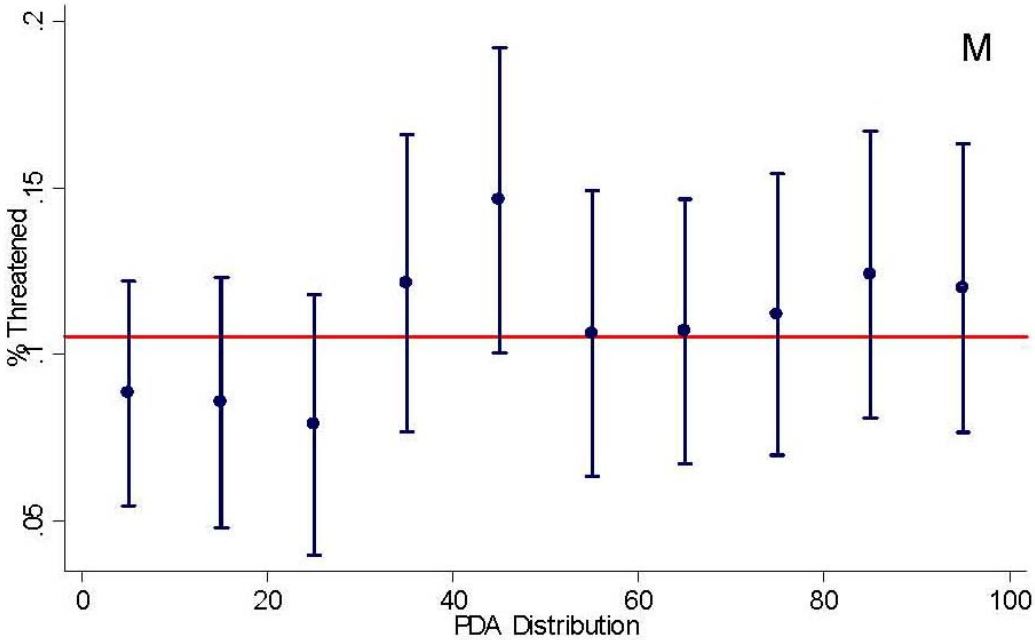
G) Percent of teachers who are employed part-time
H) Percent of teachers who are married
I) Average teacher age

Figure 3-1. Continued.



J) Average years of full-time experience
K) Percent of teachers who are white
L) Percent of teachers who are male

Figure 3-1. Continued.



M) Percent of teachers who reported being threatened by a student

Figure 3-1. Continued.

Table 3-5: Within State Probit Regression of Interdistrict Movement

Regression of PDA Hours with H* Chosen at Decile Intervals Along the Distribution of PDA Hours

	H* = 9	H* = 17	H* = 22.5	H* = 29.5	H* = 40.5	H* = 51.5	H* = 68	H* = 87.5	H* = 122.5
Variables	(10%)	(20%)	(30%)	(40%)	(50%)	(60%)	(70%)	(80%)	(90%)
PDA Hours 0 - H*	-0.0035	-0.0053	-0.0059	-0.0063	-0.0045	-0.0033	-0.0026	-0.0020	-0.0012
	(0.16)	(0.54)	(0.86)	(1.29)	(1.34)	(1.31)	(1.36)	(1.27)	(1.03)
PDA Hours > H*	-0.0002	-0.0000	0.00016	0.00051	0.00079	0.00100	0.00151	0.00211	0.00348
	(0.24)	(0.05)	(0.16)	(0.51)	(0.70)	(0.81)	(1.03)	(1.17)	(1.25)
% Free-Lunch Eligible	-0.1968	-0.1979	-0.1992	-0.2014	-0.2036	-0.2032	-0.2021	-0.2028	-0.2019
	(1.11)	(1.12)	(1.12)	(1.14)	(1.15)	(1.15)	(1.14)	(1.14)	(1.13)
Job Dissatifaction	0.76287	0.76015	0.76046	0.76279	0.76613	0.76885	0.77090	0.77125	0.76829
	(3.67)**	(3.65)**	(3.65)**	(3.65)**	(3.65)**	(3.66)**	(3.67)**	(3.67)**	(3.68)**
Full-Time Experience	-0.0331	-0.0331	-0.0330	-0.0328	-0.0327	-0.0326	-0.0326	-0.0326	-0.0326
	(4.51)**	(4.51)**	(4.50)**	(4.48)**	(4.44)**	(4.42)**	(4.42)**	(4.40)**	(4.41)**
Part-time employment	0.00475	0.00152	-0.0011	-0.0045	-0.0010	0.00166	0.00252	0.00442	0.00656
	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)
Poor Administration	0.03717	0.03778	0.03775	0.03936	0.03753	0.03605	0.03455	0.03386	0.03687
	(0.24)	(0.24)	(0.24)	(0.25)	(0.24)	(0.23)	(0.22)	(0.21)	(0.23)
Union Member	-0.0888	-0.0900	-0.0909	-0.0910	-0.0904	-0.0887	-0.0877	-0.0874	-0.0863
	(0.91)	(0.92)	(0.93)	(0.93)	(0.92)	(0.91)	(0.90)	(0.89)	(0.88)
Student Threat	0.12178	0.12207	0.12313	0.12641	0.12717	0.12681	0.12620	0.12655	0.12332
	(0.96)	(0.97)	(0.97)	(1.00)	(1.00)	(1.00)	(0.99)	(1.00)	(0.97)
Bonus Pay	-0.2414	-0.2381	-0.2352	-0.2327	-0.2327	-0.2332	-0.2324	-0.2331	-0.2390
	(1.79)*	(1.77)*	(1.74)*	(1.72)*	(1.72)*	(1.73)*	(1.72)*	(1.73)*	(1.78)*
Age	-0.0157	-0.0156	-0.0156	-0.0155	-0.0156	-0.0157	-0.0157	-0.0158	-0.0159
	(3.03)**	(3.02)**	(3.01)**	(3.00)**	(3.02)**	(3.03)**	(3.03)**	(3.04)**	(3.06)**
Dummy for Missing % Free Lunch Eligible	-0.3381	-0.3357	-0.3342	-0.3336	-0.3331	-0.3369	-0.3376	-0.3342	-0.3324
	(1.80)*	(1.79)*	(1.78)*	(1.78)*	(1.78)*	(1.79)*	(1.79)*	(1.78)*	(1.77)*
Marital Status	-0.0054	-0.0049	-0.0043	-0.0019	-0.0013	-0.0014	-0.0006	0.00009	-0.0000
	(0.06)	(0.06)	(0.05)	(0.02)	(0.02)	(0.02)	(0.01)	(0.00)	(0.00)
Constant	-0.0104	0.03170	0.32786	0.35249	0.33986	0.32819	0.32307	0.31912	0.28890
	(0.02)	(0.05)	(0.69)	(0.75)	(0.72)	(0.70)	(0.68)	(0.67)	(0.6)
Pseudo R-Squared	0.1686	0.1689	0.1694	0.1695	0.1695	0.1696	0.1695	0.1694	0.1689
Number of Obs.	1635	1635	1635	1635	1635	1635	1635	1635	1635
State FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Robust SE, clustered	Y	Y	Y	Y	Y	Y	Y	Y	Y

* denoted significance at the 0.05 level

** denoted significance at the 0.025 level

Table 3-6: Out Of State Probit Regression of Interdistrict Movement

Regression of PDA Hours with H* Chosen at Decile Intervals Along the Distribution of PDA Hours

	H* = 9	H* = 17	H* = 22.5	H* = 29.5	H* = 40.5	H* = 51.5	H* = 68	H* = 87.5	H* = 122.5
Variables	(10%)	(20%)	(30%)	(40%)	(50%)	(60%)	(70%)	(80%)	(90%)
PDA Hours 0 - H*	0.02337	0.01003	-0.0003	-0.0014	0.00059	0.00215	0.00216	0.00292	0.00281
	(0.65)	(0.62)	(0.03)	(0.18)	(0.10)	(0.48)	(0.63)	(1.03)	(1.35)
PDA Hours > H*	0.00101	0.00089	0.00142	0.00168	0.00148	0.00088	0.00053	-0.0014	-0.0079
	(0.65)	(0.54)	(0.85)	(0.97)	(0.78)	(0.42)	(0.21)	(0.44)	(1.27)
% Free-Lunch Eligible	-0.6608	-0.6655	-0.6616	-0.6636	-0.6633	-0.6593	-0.6593	-0.6549	-0.6604
	(2.51)**	(2.54)**	(2.52)**	(2.52)**	(2.51)**	(2.50)**	(2.51)**	(2.50)**	(2.53)**
Job Dissatifaction	0.41903	0.41354	0.41948	0.42437	0.41961	0.41483	0.41369	0.40598	0.40404
	(1.05)	(1.03)	(1.05)	(1.06)	(1.05)	(1.03)	(1.03)	(1.01)	(1.00)
Full-Time Experience	-0.0440	-0.0440	-0.0434	-0.0433	-0.0435	-0.0437	-0.0438	-0.0440	-0.0443
	(2.79)**	(2.77)**	(2.75)**	(2.74)**	(2.75)**	(2.76)**	(2.77)**	(2.81)**	(2.82)**
Part-time employment	-0.8839	-0.8876	-0.8871	-0.8903	-0.8874	-0.8842	-0.8842	-0.8852	-0.8912
	(1.63)	(1.64)	(1.63)	(1.64)	(1.64)	(1.63)	(1.63)	(1.62)	(1.63)
Poor Administration	0.27761	0.27740	0.26798	0.26595	0.26815	0.27250	0.27307	0.27725	0.27726
	(1.05)	(1.05)	(1.01)	(1.00)	(1.01)	(1.03)	(1.03)	(1.05)	(1.05)
Union Member	-0.0159	-0.0188	-0.0172	-0.0164	-0.0170	-0.0186	-0.0191	-0.0213	-0.0249
	(0.11)	(0.13)	(0.12)	(0.11)	(0.11)	(0.12)	(0.13)	(0.14)	(0.16)
Student Threat	0.05111	0.05212	0.05034	0.05093	0.05066	0.05101	0.05180	0.05261	0.06140
	(0.23)	(0.24)	(0.23)	(0.23)	(0.23)	(0.23)	(0.24)	(0.24)	(0.28)
Bonus Pay	0.20266	0.20067	0.20190	0.20185	0.20131	0.20238	0.20180	0.20068	0.21043
	(1.09)	(1.08)	(1.09)	(1.09)	(1.09)	(1.10)	(1.09)	(1.08)	(1.14)
Age	-0.0394	-0.0396	-0.0394	-0.0394	-0.0394	-0.0394	-0.0394	-0.0392	-0.0388
	(3.67)**	(3.69)**	(3.67)**	(3.67)**	(3.67)**	(3.66)**	(3.65)**	(3.64)**	(3.59)**
Dummy for Missing % Free Lunch Eligible	0.03832	0.04128	0.05240	0.05589	0.05176	0.04787	0.04829	0.04654	0.03925
	(0.11)	(0.12)	(0.15)	(0.16)	(0.15)	(0.14)	(0.14)	(0.14)	(0.11)
Marital Status	0.06546	0.06408	0.06081	0.06151	0.06151	0.06037	0.05910	0.05554	0.04640
	(0.46)	(0.45)	(0.43)	(0.44)	(0.44)	(0.43)	(0.42)	(0.39)	(0.33)
Constant	0.76726	0.07371	0.96390	0.98918	0.95216	0.91634	0.34887	0.87351	0.86222
	(1.02)	(0.10)	(1.36)	(1.42)	(1.39)	(1.33)	(0.41)	(1.28)	(1.27)
Pseudo R-Squared	0.2489	0.2488	0.2484	0.2485	0.2484	0.2484	0.2485	0.2492	0.2508
Number of Obs.	1108	1108	1108	1108	1108	1108	1108	1108	1108
State FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Robust SE, clustered	Y	Y	Y	Y	Y	Y	Y	Y	Y

* denoted significance at the 0.05 level

** denoted significance at the 0.025 level

Table 3-7: Probit Regression of Intradistrict Movement with District Fixed Effects
Regression of PDA Hours with H* Chosen at Decile Intervals Along the Distribution of PDA Hours

	H* = 9	H* = 17	H* = 22.5	H* = 29.5	H* = 40.5	H* = 51.5	H* = 68	H* = 87.5	H* = 122.5
Variables	(10%)	(20%)	(30%)	(40%)	(50%)	(60%)	(70%)	(80%)	(90%)
PDA Hours 0 - H*	-0.2037	-0.0707	-0.0443	-0.0282	-0.0147	-0.0099	-0.0065	-0.0047	-0.0035
	(2.12)**	(2.20)**	(2.03)**	(1.82)*	(1.45)	(1.31)	(1.22)	(1.25)	(1.26)
PDA Hours > H*	0.00242	0.00303	0.00330	0.00357	0.00371	0.00416	0.00509	0.00678	0.01231
	(1.13)	(1.38)	(1.49)	(1.52)	(1.44)	(1.48)	(1.59)	(1.79)*	(2.34)**
% Free-Lunch Eligible	0.57160	0.50149	0.46890	0.45768	0.44608	0.45569	0.45798	0.44090	0.45394
	(1.10)	(0.95)	(0.89)	(0.87)	(0.85)	(0.87)	(0.88)	(0.85)	(0.87)
Job Dissatifaction	1.24178	1.22087	1.22145	1.21289	1.21081	1.20811	1.20478	1.17851	1.13832
	(2.09)**	(2.02)**	(1.98)**	(1.96)**	(1.96)**	(1.96)**	(1.96)**	(1.93)*	(1.88)*
Full-Time Experience	-0.0119	-0.0098	-0.0099	-0.0094	-0.0086	-0.0089	-0.0093	-0.0099	-0.0098
	(0.68)	(0.55)	(0.56)	(0.53)	(0.48)	(0.51)	(0.53)	(0.56)	(0.56)
Part-time employment	0.51540	0.69751	0.72376	0.73810	0.73554	0.73359	0.72474	0.71323	0.71233
	(1.09)	(1.51)	(1.59)	(1.65)	(1.67)	(1.67)	(1.65)	(1.62)	(1.62)
Poor Administration	0.77191	0.73451	0.73095	0.72802	0.73031	0.72283	0.71226	0.70846	0.67968
	(1.79)*	(1.68)	(1.66)	(1.65)	(1.66)	(1.64)	(1.61)	(1.60)	(1.55)
Union Member	0.01757	0.08987	0.09814	0.09679	0.09406	0.09087	0.08021	0.08181	0.09168
	(0.06)	(0.31)	(0.33)	(0.33)	(0.32)	(0.31)	(0.27)	(0.28)	(0.32)
Student Threat	0.41600	0.42832	0.42130	0.41446	0.41373	0.40975	0.40086	0.39627	0.39301
	(1.30)	(1.32)	(1.29)	(1.29)	(1.30)	(1.30)	(1.27)	(1.26)	(1.26)
Bonus Pay	-0.4831	-0.4094	-0.3959	-0.3940	-0.3839	-0.3700	-0.3651	-0.3550	-0.3483
	(1.26)	(1.08)	(1.03)	(1.02)	(0.99)	(0.96)	(0.95)	(0.93)	(0.90)
Age	-0.0154	-0.0161	-0.0156	-0.0154	-0.0155	-0.0154	-0.0153	-0.0152	-0.0156
	(0.96)	(1.01)	(0.98)	(0.97)	(0.98)	(0.98)	(0.98)	(0.98)	(1.00)
Dummy for Missing % Free Lunch Eligible	-0.0759	-0.0769	-0.0674	-0.0824	-0.1056	-0.1187	-0.1165	-0.0904	-0.0660
	(0.20)	(0.19)	(0.17)	(0.21)	(0.27)	(0.30)	(0.30)	(0.23)	(0.17)
Marital Status	-0.0479	-0.0328	-0.0319	-0.0328	-0.0396	-0.0419	-0.0485	-0.0521	-0.0487
	(0.21)	(0.15)	(0.14)	(0.15)	(0.18)	(0.19)	(0.22)	(0.23)	(0.22)
Constant	2.09461	1.55653	1.29478	1.16611	1.02917	0.98271	0.96313	0.99048	1.00050
	(2.62)**	(2.25)**	(2.00)**	(1.82)*	(1.62)	(1.57)	(1.54)	(1.57)	(1.62)
Pseudo R-Squared	0.1760	0.1710	0.1693	0.1675	0.1640	0.1632	0.1631	0.1646	0.1683
Number of Obs.	398	398	398	398	398	398	398	398	398
State FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Robust SE, clustered	Y	Y	Y	Y	Y	Y	Y	Y	Y

* denoted significance at the 0.05 level
** denoted significance at the 0.025 level

APPENDIX A
SUMMARY STATISTICS AND SUPPLEMENTARY REGRESSION ANALYSIS

Table A-1: Strike Summary Statistics

Total Number of Strike and Strike Treatments in the 22 Year Sample:

Total Strikes: 102 (64)**

Total Strike Treatments: 2,351

Total Strikes and Strike Treatments in Each Community:

Urban:	Total Strikes:	74 (43)**
	Total Strike Treatments:	1,858 (1616)**
Suburban	Total Strikes:	31 (17)**
	Total Strike Treatments:	97 (67)**
Rural	Total Strikes:	56 (32)**
	Total Strike Treatments:	398 (275)**

Longest Strike Length: 25 Days

Mean Strike Length: 6.7 Days (Median is 5 Days)

Distribution of Strike Days:		Total Strike Treatments by Strike Year:	
1 Day	321	1980	106
2 Days	211	1981	24
3 Days	202	1982	0
4 Days	197	1983	74
5 Days	188	1984	2
6 Days	177	1985	567
7 Days	179	1986	27
8 Days	170	1987	87
9 Days	58	1988	0
10 Days	49	1989	0
11 Days	45	1990	222
12 Days	44	1991	1016
13 Days	44	1992	9
14 Days	39	1993	4
15 Days	39	1994	90
16 Days	39	1995	75
17 Days	38	1996	0
18 Days	38	1997	0
19 Days	34	1998	18
20 Days	34	1999	1
21 Days	30	2000	0
22 Days	3	2001	11
23 Days	3		
24 Days	3		
25 Days	3		

**The numbers in parenthesis represent the value in Type IV specifications

Table A-2: Regressions of Full Data Set excluding Zip Codes with less than 500 students

Variables	(I)	(II)	(III)	(IV)	(V)
Strike	**0.187****	**0.177****	**0.200****	**0.246****	**0.212****
	(3.88)**	**(3.64)****	**(3.94)****	**(4.77)****	**(4.27)****
Median Income	-0.00003**	-0.00003**	-0.00003**	-0.00003**	-
	(103.76)**	(85.14)**	(61.69)**	(54.36)**	-
Welfare	1.120**	1.149**	1.250**	1.572**	-
	(18.28)**	(14.20)**	(11.22)**	(11.09)**	-
Urban	0.110**	0.089**	0.071**	0.046**	-
	(22.30)**	(14.42)**	(8.38)**	(5.23)**	-
Poor Parental Educ.	0.503**	0.523**	0.570**	0.802**	-
	(14.02)**	(11.83)**	(9.34)**	(9.39)**	-
Juvenile Maleness	0.266**	0.195**	0.0829	0.265**	-
	(5.66)**	(3.33)**	(1.01)	(2.66)**	-
Student Employment	0.224**	0.228**	0.225**	0.192**	-
	(11.67)**	(9.61)**	(6.88)**	(5.07)**	-
Alpha	0.922**	0.941**	0.927**	0.889**	0.705**
	(153.67)**	(117.63)**	(92.70)**	(85.48)**	(76.58)**
Number of obs.	879,441	582,543	301,056	249,745	249,745
Time Fixed Effects	Y	Y	Y	Y	Y
Zip Fixed Effects	N	N	N	N	Y
Pseudo R-Squared	0.0397	0.0396	0.040	0.0308	0.0556

APPENDIX B
COMPLETE RESULTS OF SPECIFIED REGRESSIONS

Table B-1: Reported on Table 3-4
Regression of PDA Hours with H* chosen at a cutoff on the Distribution of PDA Hours

Variables	10%	20%	30%	40%	50%	60%	70%	80%	90%
PDA Hours 0 - H*	-0.0262	-0.0170	-0.0117	-0.0074	-0.0047	-0.0033	-0.0022	-0.0018	-0.0006
	(1.12)	(1.68)	(1.63)	(1.43)	(1.34)	(1.27)	(1.12)	(1.15)	(0.55)
PDA Hours > H*	0.0013	0.0017	0.0018	0.0020	0.0022	0.0026	0.0031	0.0043	0.0061
	(1.52)	(1.85)*	(1.94)*	(1.94)*	(2.00)**	(2.08)**	(2.16)**	(2.45)**	(2.51)**
% Free-Lunch Eligible	0.3041	0.3066	0.3077	0.3074	0.3067	0.3086	0.3117	0.3081	0.3010
	(1.86)*	(1.87)*	(1.88)*	(1.88)*	(1.88)*	(1.89)*	(1.91)*	(1.88)*	(1.83)*
Job Dissatifaction	0.4214	0.4254	0.4282	0.4285	0.4306	0.4338	0.4377	0.4385	0.4288
	(1.86)*	(1.87)*	(1.88)*	(1.88)*	(1.89)*	(1.90)*	(1.92)*	(1.93)*	(1.89)*
Full-Time Experience	-0.0141	-0.0136	-0.0135	-0.0134	-0.0132	-0.0130	-0.0130	-0.0130	-0.0134
	(2.29)**	(2.22)**	(2.19)**	(2.17)**	(2.13)**	(2.10)**	(2.10)**	(2.10)**	(2.18)**
Part-time employment	0.5383	0.5362	0.5351	0.5373	0.5417	0.5458	0.5479	0.5477	0.5381
	(3.37)**	(3.36)**	(3.36)**	(3.38)**	(3.41)**	(3.44)**	(3.45)**	(3.46)**	(3.41)**
Poor Administration	0.3575	0.3616	0.3636	0.3641	0.3638	0.3621	0.3595	0.3596	0.3593
	(2.57)**	(2.59)**	(2.61)**	(2.61)**	(2.61)**	(2.60)**	(2.58)**	(2.58)**	(2.59)**
Union Member	0.2344	0.2406	0.2419	0.2411	0.2415	0.2416	0.2399	0.2387	0.2373
	(2.32)**	(2.38)**	(2.40)**	(2.39)**	(2.39)**	(2.39)**	(2.37)**	(2.37)**	(2.36)**
Race (White)	-0.0906	-0.0924	-0.0931	-0.0950	-0.0970	-0.0987	-0.1000	-0.1015	-0.0953
	(0.76)	(0.77)	(0.78)	(0.80)	(0.81)	(0.83)	(0.84)	(0.85)	(0.80)
Marital Status	0.0232	0.0224	0.0232	0.0245	0.0256	0.0265	0.0266	0.0277	0.0264
	(0.28)	(0.27)	(0.28)	(0.30)	(0.31)	(0.32)	(0.33)	(0.34)	(0.32)
Dummy for Missing % Free Lunch Eligible	0.1789	0.1779	0.1798	0.1796	0.1790	0.1777	0.1772	0.1807	0.1776
	(1.22)	(1.21)	(1.22)	(1.22)	(1.22)	(1.21)	(1.21)	(1.23)	(1.21)
Bonus Pay	-0.1674	-0.1639	-0.1639	-0.1657	-0.1658	-0.1650	-0.1635	-0.1633	-0.1658
	(1.41)	(1.38)	(1.38)	(1.39)	(1.40)	(1.39)	(1.38)	(1.38)	(1.40)
Student Threat	0.1011	0.1016	0.1022	0.1025	0.1016	0.1000	0.0984	0.0987	0.0991
	(0.87)	(0.87)	(0.87)	(0.88)	(0.87)	(0.86)	(0.85)	(0.85)	(0.85)
Age	-0.0053	-0.0054	-0.0054	-0.0055	-0.0055	-0.0056	-0.0057	-0.0058	-0.0056
	(1.02)	(1.05)	(1.06)	(1.07)	(1.08)	(1.09)	(1.10)	(1.12)	(1.09)
Gender (Male)	-0.1157	-0.1180	-0.1186	-0.1188	-0.1188	-0.1188	-0.1188	-0.1176	-0.1132
	(1.35)	(1.37)	(1.38)	(1.38)	(1.38)	(1.38)	(1.38)	(1.37)	(1.32)
Master's Degree	0.1211	0.1211	0.1212	0.1204	0.1202	0.1210	0.1215	0.1210	0.1180
	(1.43)	(1.43)	(1.43)	(1.42)	(1.42)	(1.43)	(1.43)	(1.43)	(1.39)
Constant	-0.9070	0.5313	0.4935	0.4260	-0.9811	0.3674	0.3596	-0.9753	0.3402
	(1.29)	(2.15)**	(2.09)**	(1.93)*	(1.45)	(1.78)*	(1.75)*	(1.44)	(1.69)
R-Squared	0.0993	0.1004	0.1004	0.1002	0.1002	0.1003	0.1005	0.1012	0.1009
Number of Obs.	1627	1627	1627	1627	1627	1627	1627	1627	1627
State FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Robust SE, clustered	Y	Y	Y	Y	Y	Y	Y	Y	Y

* denoted significance at the 0.05 level, ** denoted significance at the 0.025 level

Table B-2: Reported on Table 3-5
Regression of PDA Hours with H* chosen at a cutoff on the Distribution of PDA Hours

Variables	10%	20%	30%	40%	50%	60%	70%	80%	90%
PDA Hours 0 - H*	-0.0035	-0.0053	-0.0059	-0.0063	-0.0045	-0.0033	-0.0026	-0.0020	-0.0012
	(0.16)	(0.54)	(0.86)	(1.29)	(1.34)	(1.31)	(1.36)	(1.27)	(1.03)
PDA Hours > H*	-0.0002	-0.0000	0.00016	0.00051	0.00079	0.00100	0.00151	0.00211	0.00348
	(0.24)	(0.05)	(0.16)	(0.51)	(0.70)	(0.81)	(1.03)	(1.17)	(1.25)
% Free-Lunch Eligible	-0.1968	-0.1979	-0.1992	-0.2014	-0.2036	-0.2032	-0.2021	-0.2028	-0.2019
	(1.11)	(1.12)	(1.12)	(1.14)	(1.15)	(1.15)	(1.14)	(1.14)	(1.13)
Job Dissatifaction	0.76287	0.76015	0.76046	0.76279	0.76613	0.76885	0.77090	0.77125	0.76829
	(3.67)**	(3.65)**	(3.65)**	(3.65)**	(3.65)**	(3.66)**	(3.67)**	(3.67)**	(3.68)**
Full-Time Experience	-0.0331	-0.0331	-0.0330	-0.0328	-0.0327	-0.0326	-0.0326	-0.0326	-0.0326
	(4.51)**	(4.51)**	(4.50)**	(4.48)**	(4.44)**	(4.42)**	(4.42)**	(4.40)**	(4.41)**
Part-time employment	0.00475	0.00152	-0.0011	-0.0045	-0.0010	0.00166	0.00252	0.00442	0.00656
	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)
Poor Administration	0.03717	0.03778	0.03775	0.03936	0.03753	0.03605	0.03455	0.03386	0.03687
	(0.24)	(0.24)	(0.24)	(0.25)	(0.24)	(0.23)	(0.22)	(0.21)	(0.23)
Union Member	-0.0888	-0.0900	-0.0909	-0.0910	-0.0904	-0.0887	-0.0877	-0.0874	-0.0863
	(0.91)	(0.92)	(0.93)	(0.93)	(0.92)	(0.91)	(0.90)	(0.89)	(0.88)
Race (White)	-0.1287	-0.1324	-0.1355	-0.1382	-0.1378	-0.1367	-0.1374	-0.1366	-0.1335
	(1.00)	(1.03)	(1.05)	(1.07)	(1.07)	(1.06)	(1.06)	(1.06)	(1.03)
Marital Status	-0.0054	-0.0049	-0.0043	-0.0019	-0.0013	-0.0014	-0.0006	0.00009	-0.0000
	(0.06)	(0.06)	(0.05)	(0.02)	(0.02)	(0.02)	(0.01)	(0.00)	(0.00)
Dummy for Missing % Free Lunch Eligible	-0.3381	-0.3357	-0.3342	-0.3336	-0.3331	-0.3369	-0.3376	-0.3342	-0.3324
	(1.80)*	(1.79)*	(1.78)*	(1.78)*	(1.78)*	(1.79)*	(1.79)*	(1.78)*	(1.77)*
Bonus Pay	-0.2414	-0.2381	-0.2352	-0.2327	-0.2327	-0.2332	-0.2324	-0.2331	-0.2390
	(1.79)*	(1.77)*	(1.74)*	(1.72)*	(1.72)*	(1.73)*	(1.72)*	(1.73)*	(1.78)*
Student Threat	0.12178	0.12207	0.12313	0.12641	0.12717	0.12681	0.12620	0.12655	0.12332
	(0.96)	(0.97)	(0.97)	(1.00)	(1.00)	(1.00)	(0.99)	(1.00)	(0.97)
Age	-0.0157	-0.0156	-0.0156	-0.0155	-0.0156	-0.0157	-0.0157	-0.0158	-0.0159
	(3.03)**	(3.02)**	(3.01)**	(3.00)**	(3.02)**	(3.03)**	(3.03)**	(3.04)**	(3.06)**
Gender (Male)	0.0217	0.0199	0.0181	0.0167	0.0176	0.0194	0.0212	0.0227	0.0245
	(0.26)	(0.23)	(0.21)	(0.20)	(0.21)	(0.23)	(0.25)	(0.27)	(0.29)
Master's Degree	-0.0153	-0.0162	-0.0173	-0.0156	-0.0156	-0.0129	-0.0115	-0.0126	-0.0139
	(0.17)	(0.18)	(0.19)	(0.17)	(0.17)	(0.14)	(0.13)	(0.14)	(0.15)
Constant	-0.0104	0.03170	0.32786	0.35249	0.33986	0.32819	0.32307	0.31912	0.28890
	(0.02)	(0.05)	(0.69)	(0.75)	(0.72)	(0.70)	(0.68)	(0.67)	(0.6)
R-squared	0.1686	0.1689	0.1694	0.1695	0.1695	0.1696	0.1695	0.1694	0.1689
Number of Obs.	1635	1635	1635	1635	1635	1635	1635	1635	1635
State FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Robust SE, clustered	Y	Y	Y	Y	Y	Y	Y	Y	Y

* denoted significance at the 0.05 level
** denoted significance at the 0.025 level

Table B-3: Reported on Table 3-6
Regression of PDA Hours with H* chosen at a cutoff on the Distribution of PDA Hours

Variables	10%	20%	30%	40%	50%	60%	70%	80%	90%
PDA Hours 0 - H*	0.02337	0.01003	-0.0003	-0.0014	0.00059	0.00215	0.00216	0.00292	0.00281
	(0.65)	(0.62)	(0.03)	(0.18)	(0.10)	(0.48)	(0.63)	(1.03)	(1.35)
PDA Hours > H*	0.00101	0.00089	0.00142	0.00168	0.00148	0.00088	0.00053	-0.0014	-0.0079
	(0.65)	(0.54)	(0.85)	(0.97)	(0.78)	(0.42)	(0.21)	(0.44)	(1.27)
% Free-Lunch Eligible	-0.6608	-0.6655	-0.6616	-0.6636	-0.6633	-0.6593	-0.6593	-0.6549	-0.6604
	(2.51)**	(2.54)**	(2.52)**	(2.52)**	(2.51)**	(2.50)**	(2.51)**	(2.50)**	(2.53)**
Job Dissatifaction	0.41903	0.41354	0.41948	0.42437	0.41961	0.41483	0.41369	0.40598	0.40404
	(1.05)	(1.03)	(1.05)	(1.06)	(1.05)	(1.03)	(1.03)	(1.01)	(1.00)
Full-Time Experience	-0.0440	-0.0440	-0.0434	-0.0433	-0.0435	-0.0437	-0.0438	-0.0440	-0.0443
	(2.79)**	(2.77)**	(2.75)**	(2.74)**	(2.75)**	(2.76)**	(2.77)**	(2.81)**	(2.82)**
Part-time employment	-0.8839	-0.8876	-0.8871	-0.8903	-0.8874	-0.8842	-0.8842	-0.8852	-0.8912
	(1.63)	(1.64)	(1.63)	(1.64)	(1.64)	(1.63)	(1.63)	(1.62)	(1.63)
Poor Administration	0.27761	0.27740	0.26798	0.26595	0.26815	0.27250	0.27307	0.27725	0.27726
	(1.05)	(1.05)	(1.01)	(1.00)	(1.01)	(1.03)	(1.03)	(1.05)	(1.05)
Union Member	-0.0159	-0.0188	-0.0172	-0.0164	-0.0170	-0.0186	-0.0191	-0.0213	-0.0249
	(0.11)	(0.13)	(0.12)	(0.11)	(0.11)	(0.12)	(0.13)	(0.14)	(0.16)
Race (White)	0.23557	0.2448	0.24363	0.24274	0.24431	0.24747	0.24979	0.25548	0.24977
	(0.89)	(0.92)	(0.92)	(0.91)	(0.92)	(0.93)	(0.94)	(0.95)	(0.93)
Marital Status	0.06546	0.06408	0.06081	0.06151	0.06151	0.06037	0.05910	0.05554	0.04640
	(0.46)	(0.45)	(0.43)	(0.44)	(0.44)	(0.43)	(0.42)	(0.39)	(0.33)
Dummy for Missing % Free Lunch Eligible	0.03832	0.04128	0.05240	0.05589	0.05176	0.04787	0.04829	0.04654	0.03925
	(0.11)	(0.12)	(0.15)	(0.16)	(0.15)	(0.14)	(0.14)	(0.14)	(0.11)
Bonus Pay	0.20266	0.20067	0.20190	0.20185	0.20131	0.20238	0.20180	0.20068	0.21043
	(1.09)	(1.08)	(1.09)	(1.09)	(1.09)	(1.10)	(1.09)	(1.08)	(1.14)
Student Threat	0.05111	0.05212	0.05034	0.05093	0.05066	0.05101	0.05180	0.05261	0.06140
	(0.23)	(0.24)	(0.23)	(0.23)	(0.23)	(0.23)	(0.24)	(0.24)	(0.28)
Age	-0.0394	-0.0396	-0.0394	-0.0394	-0.0394	-0.0394	-0.0394	-0.0392	-0.0388
	(3.67)**	(3.69)**	(3.67)**	(3.67)**	(3.67)**	(3.66)**	(3.65)**	(3.64)**	(3.59)**
Gender (Male)	-0.1214	-0.1187	-0.1257	-0.1286	-0.1267	-0.1219	-0.1209	-0.1176	-0.1264
	(0.78)	(0.76)	(0.80)	(0.82)	(0.81)	(0.78)	(0.77)	(0.75)	(0.81)
Master's Degree	0.53769	0.53978	0.53363	0.53165	0.5316	0.53155	0.53111	0.53137	0.54125
	(3.21)	(3.23)	(3.19)	(3.19)	(3.19)	(3.19)	(3.18)	(3.18)	(3.23)
Constant	0.76726	0.07371	0.96390	0.98918	0.95216	0.91634	0.34887	0.87351	0.86222
	(1.02)	(0.10)	(1.36)	(1.42)	(1.39)	(1.33)	(0.41)	(1.28)	(1.27)
R-squared	0.2489	0.2488	0.2484	0.2485	0.2484	0.2484	0.2485	0.2492	0.2508
Number of Obs.	1108	1108	1108	1108	1108	1108	1108	1108	1108
State FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Robust SE, clustered	Y	Y	Y	Y	Y	Y	Y	Y	Y

* denoted significance at the 0.05 level, ** denoted significance at the 0.025 level

Table B-4: Reported on Table 3-7
Regression of PDA Hours with H* chosen at a cutoff on the Distribution of PDA Hours

Variables	10%	20%	30%	40%	50%	60%	70%	80%	90%
PDA Hours 0 - H*	-0.2037	-0.0707	-0.0443	-0.0282	-0.0147	-0.0099	-0.0065	-0.0047	-0.0035
	(2.12)**	(2.20)**	(2.03)**	(1.82)*	(1.45)	(1.31)	(1.22)	(1.25)	(1.26)
PDA Hours > H*	0.00242	0.00303	0.00330	0.00357	0.00371	0.00416	0.00509	0.00678	0.01231
	(1.13)	(1.38)	(1.49)	(1.52)	(1.44)	(1.48)	(1.59)	(1.79)*	(2.34)**
% Free-Lunch Eligible	0.57160	0.50149	0.46890	0.45768	0.44608	0.45569	0.45798	0.44090	0.45394
	(1.10)	(0.95)	(0.89)	(0.87)	(0.85)	(0.87)	(0.88)	(0.85)	(0.87)
Job Dissatifaction	1.24178	1.22087	1.22145	1.21289	1.21081	1.20811	1.20478	1.17851	1.13832
	(2.09)**	(2.02)**	(1.98)**	(1.96)**	(1.96)**	(1.96)**	(1.96)**	(1.93)*	(1.88)*
Full-Time Experience	-0.0119	-0.0098	-0.0099	-0.0094	-0.0086	-0.0089	-0.0093	-0.0099	-0.0098
	(0.68)	(0.55)	(0.56)	(0.53)	(0.48)	(0.51)	(0.53)	(0.56)	(0.56)
Part-time employment	0.51540	0.69751	0.72376	0.73810	0.73554	0.73359	0.72474	0.71323	0.71233
	(1.09)	(1.51)	(1.59)	(1.65)	(1.67)	(1.67)	(1.65)	(1.62)	(1.62)
Poor Administration	0.77191	0.73451	0.73095	0.72802	0.73031	0.72283	0.71226	0.70846	0.67968
	(1.79)*	(1.68)	(1.66)	(1.65)	(1.66)	(1.64)	(1.61)	(1.60)	(1.55)
Union Member	0.01757	0.08987	0.09814	0.09679	0.09406	0.09087	0.08021	0.08181	0.09168
	(0.06)	(0.31)	(0.33)	(0.33)	(0.32)	(0.31)	(0.27)	(0.28)	(0.32)
Race (White)	-0.0808	-0.083	-0.0838	-0.0862	-0.0885	-0.0908	-0.0924	-0.0937	-0.0864
	(0.65)	(0.67)	(0.68)	(0.69)	(0.71)	(0.73)	(0.74)	(0.75)	(0.69)
Marital Status	-0.0479	-0.0328	-0.0319	-0.0328	-0.0396	-0.0419	-0.0485	-0.0521	-0.0487
	(0.21)	(0.15)	(0.14)	(0.15)	(0.18)	(0.19)	(0.22)	(0.23)	(0.22)
Dummy for Missing % Free Lunch Eligible	-0.0759	-0.0769	-0.0674	-0.0824	-0.1056	-0.1187	-0.1165	-0.0904	-0.0660
	(0.20)	(0.19)	(0.17)	(0.21)	(0.27)	(0.30)	(0.30)	(0.23)	(0.17)
Bonus Pay	-0.4831	-0.4094	-0.3959	-0.3940	-0.3839	-0.3700	-0.3651	-0.3550	-0.3483
	(1.26)	(1.08)	(1.03)	(1.02)	(0.99)	(0.96)	(0.95)	(0.93)	(0.90)
Student Threat	0.41600	0.42832	0.42130	0.41446	0.41373	0.40975	0.40086	0.39627	0.39301
	(1.30)	(1.32)	(1.29)	(1.29)	(1.30)	(1.30)	(1.27)	(1.26)	(1.26)
Age	-0.0154	-0.0161	-0.0156	-0.0154	-0.0155	-0.0154	-0.0153	-0.0152	-0.0156
	(0.96)	(1.01)	(0.98)	(0.97)	(0.98)	(0.98)	(0.98)	(0.98)	(1.00)
Gender (Male)	-0.107	-0.1103	-0.1111	-0.1113	-0.1114	-0.1117	-0.1118	-0.1099	-0.1043
	(1.30)	(1.33)	(1.34)	(1.34)	(1.35)	(1.35)	(1.36)	(1.33)	(1.26)
Master's Degree	0.1495	0.1497	0.15	0.149	0.1485	0.1495	0.1502	0.1497	0.1463
	(1.76)*	(1.75)*	(1.75)*	(1.74)*	(1.73)*	(1.74)*	(1.75)*	(1.74)*	(1.71)*
Constant	2.09461	1.55653	1.29478	1.16611	1.02917	0.98271	0.96313	0.99048	1.00050
	(2.62)**	(2.25)**	(2.00)**	(1.82)*	(1.62)	(1.57)	(1.54)	(1.57)	(1.62)
R-squared	0.1760	0.1710	0.1693	0.1675	0.1640	0.1632	0.1631	0.1646	0.1683
Number of Obs.	398	398	398	398	398	398	398	398	398
State FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Robust SE, clustered	Y	Y	Y	Y	Y	Y	Y	Y	Y

* denoted significance at the 0.05 level, ** denoted significance at the 0.025 level

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BIOGRAPHICAL SKETCH

Before beginning his doctoral studies, Jeremy Luallen attended the University of Florida as an undergraduate where he received two bachelor degrees (one in economics and one in political science) in three years. As a graduate student Jeremy has been recognized numerous times for scholarly achievement in his work. In 2004 he was the recipient of Edward Zabel Award, an award given for excellence in dissertation research and publication potential. In addition he was also recognized for excellence in completed research, as well as potential for future research, in the field of Public Policy as a recipient of the Walter-Lanzillotti Award.

Jeremy is a member of the American Education Finance Association, has been invited to present his research at several collegial conferences, such as the annual meeting of the Southern Economic Association, and at the American Education Finance Association annual conference. In addition to his schooling Jeremy has worked as a consultant for the Naples Children and Education Foundation, and will begin working as a Senior Analyst for Abt Associates after graduation.