The Effect of Charter Competition on Unionized District Revenues and Resource Allocation

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Abstract

This study examines the impact of competition due to charter school entry on the level and composition of expenditures within traditional public school districts (TPSDs). I leverage policy changes affecting the location and timing of charter entry to account for endogenous charter competition. Competition depresses appraised housing valuations, in turn causing TPSDs to lose property tax revenues resulting in a decline in overall spending. TPSDs respond to competition by allocating resources away from instructional and other expenditures towards new capital construction. Using teacher contracts, I show the declines in instructional spending are partially due to decreases in collectively bargained salaries.

JEL: C18, H52, H75, I21, I22, I28, J31, J52.

Keywords: Charter Competition, Resource Allocation, Measurement Error, School Finance, Teachers’ Unions, Union Salary

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The charter school movement is rapidly expanding across the United States. Charters are designed to be innovative laboratories for educational practices and to compete with traditional public school districts (TPSD) over student enrollment. Proponents argue that these market forces cause TPSDs to improve student achievement, but the empirical evidence is mixed (Epple et al., 2015). This literature has focused directly on student outcomes instead of the mechanisms underlying how districts respond to charter competition. Without understanding how TPSDs respond, it is difficult to disentangle why competition improves TPSD student outcomes in some contexts and reduces outcomes in others. An important way that districts may respond to competition is by adjusting how they allocate resources. Moreover, these changes provide insight into which dimension of school quality that competition affects. For example, districts competing over achievement ratings may allocate resources toward instruction or pupil services, while districts competing over school facility quality may allocate expenditures toward new capital projects. However, we understand little about the extent to which charters influence TPSD expenditure decisions.¹

Conversely, critics of the charter movement argue that charter competition puts fiscal stress on traditional schools making the remaining students worse off. Empirical evidence confirms that charters place fiscal stress on TPSDs (Bifulco and Reback, 2014) and, in general, decrease the revenues available to districts (Arsen and Ni, 2012b). Yet, we have an incomplete understanding of why TPSD revenues fall in the presence of charter competition. Some of the decline is mechanical. TPSDs directly lose state per-pupil funding as students transfer to charter schools and federal per-pupil funding as vulnerable student populations transfer. However, other mechanisms may be more nuanced. For example, if charter presence is capitalized into housing values, then charter entry would indirectly affect the TPSD local revenues raised through property taxes.²

This study explores potential mechanisms underlying how charter competition affects TPSD funding and whether TPSDs respond by adjusting the composition of their expenditures. I use a difference-in-difference-instrumental-variables framework that exploits the delayed nature of the

¹The only other evidence on within-district resource allocation comes from Arsen and Ni (2012b) who find that charter schools have a negligible effect on TPSD resource reallocation in Michigan. Due to data limitations, Arsen and Ni (2012b) impute charter competition levels for roughly 75 percent of their sample. This can introduce potentially serious attenuation bias into their results and highlights the value of analyzing this question in a setting with a more accurate measure of charter competition.

²While Imberman et al. (2016) find no evidence of charter capitalization on average in Los Angeles county, they find that housing prices outside the Los Angeles Unified School District fall in response to within-district charter entry.
charter approval process as well as policy changes that affect when and where new charters can locate. I document that charter competition directly reduces state and federal revenues through the expected channels. A key finding of this study is that charter competition also decreases the TPSD revenues raised through property taxes by depressing appraised district-level residential property values. I also find that charter competition causes districts to spend less on instructional and other current expenditures and spend more on new construction capital outlays. This reallocation is more than a simple proportional change. A one percentage point increase in charter competition increases the overall amount that TPSDs spend on capital outlays by 7.3 percent. This is consistent with qualitative evidence that administrators in Washington D.C. believe the physical appearance of their school has the greatest impact on preventing enrollment loss to charters (Sullivan et al., 2008). I discuss further explanations for these surprising results below. Additionally, I provide evidence that these findings are not driven by the passage of the No Child Left Behind Act nor the Great Recession.

I also examine the effect of charter competition on collectively bargained teacher salaries. Most studies of the effect of charter competition on teacher salaries occur in settings where collective bargaining is prohibited by law, such as Texas (Taylor, 2006, 2010) and North Carolina (Jackson, 2012). Thus, I address a gap in the teacher labor market literature by assessing how unionized markets respond to largely non-unionized charter school competition. A challenge in studying the effect of charter competition on collectively bargained teacher salaries is that contracts are negotiated intermittently and can only adjust to charter competition during negotiation years. Ignoring this problem generates an attenuation bias in applications. The bias is similar to the well-known “seam bias” in popular panel datasets such as the Survey of Income and Program Participation, which arises when respondents answer retrospective questions using current information (see Ham et al., 2009; Pei, 2015; Pischke, 1995).

I characterize this bias within my context and use Monte Carlo simulations to demonstrate that the bias is avoided by restricting the analysis sample to years when the outcome can vary (i.e., negotiation years). Using this approach and the universe of Ohio teachers’ union contracts, I estimate that a percentage point increase in charter competition decreases teacher salary contracts

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3Notable exceptions include Arsen and Ni (2012b) and Hoxby (2002).
4In my setting, estimates on annual data would theoretically attenuate my results by 7 percent.
at the top of the pay scale by around 1.0 percent. I also estimate salary decreases for entry-level teachers, but find that charter competition has no effect on mid-career salary contracts. Furthermore, I find that charter competition causes TPSDs to hire fewer new teachers, which reduces the size of the teacher labor force to maintain pupil-teacher ratios. I estimate minor teacher mobility between TPSDs and charters consistent with a model where TPSDs are competing over students instead of teachers.

The remainder of the paper is organized as follows. Section 1 overviews Ohio charter school institutional details, Section 2 describes the data, Section 3 presents the research design and evaluates its validity, and Sections 4 through 6 provide the main results for district revenues and expenditure allocation emphasizing collectively bargained teacher salaries. Section 7 discusses these results and Section 8 concludes.

1 Institutional Details

Charter schools are independently run educational organizations that sign a “charter” declaring their structure and outlining detailed plans for achieving student success. Charter schools in Ohio differ from traditional public schools in the following ways. While students may only attend a traditional public school based on the geographic location of their residence, students across the state are able to attend any charter they desire. When a student transfers to a charter from a public school their per-pupil state funding transfers as well. Any charter failing to attract the number of students needed to at least fund operating costs will eventually close.

In Ohio, there is an important distinction between a conversion and start-up charter school (ODE, 2014). The conversion schools are created by “converting” all or a portion of an existing public school into a charter school. These schools must obtain a majority vote at the school board to convert. Public schools can convert at any time across the state, conditional on receiving the necessary votes. These districts operate independently from their sponsor school district. Conversion charter schools are free to decide if they want to remain unionized.

Start-up schools on the other hand are new educational institutions and differ from conversion schools in a variety of ways. First, start-up charters can be sponsored by a larger set of entities. Sponsors for start-ups can include teachers, parents, communities, private organizations, Ohio

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5Local school districts are required to provide transportation to any student living more than two miles away from their desired charter school as long as the charter is no further than 30 minutes away from the school of residence.
universities, and even the Ohio Department of Education (ODE). Start-ups must privately fund a majority of the charter’s expenses including the large entry costs. As a result, they often try to renovate and locate in closed-down schools or shopping centers (Imberman, 2011) instead of constructing new buildings. Unlike conversion schools, start-ups are not able to open freely across the state. There is a complicated legislative history (see Section 3.2) that dictates in which districts start-up charters are permitted to open during any given year.

Ohio charter schools can be further categorized as either a traditional “brick-and-mortar” or a “digital” charter. Digital charter schools face the same legislation and requirements as traditional “brick-and-mortar” charters; however, all instruction occurs online, and schools are required to provide each student with a laptop. Ohio has the second-largest (second to Arizona) online charter presence in the nation, with over 30,000 students enrolled in a digital school in 2011-12. This represents rapid growth considering the first digital charter school opened in the 2000-01 school year. While there are a handful of digital charter schools that limit enrollment to district residents only, nearly every digital charter allows students from across the state to enroll.

2 Data
2.1 Data Description

To test the effect of charter competition on district revenues, resource allocation, negotiated contract outcomes, and teacher employment requires several datasets, each of which are summarized below in turn. Additional information is presented in Online Appendix A, which summarizes the details of each dataset including important data cleaning procedures.

Digitized union contracts are provided by the Ohio State Employment Relations Board (SERB). I observe all contracts from 1982 through the 2012-13 school year. About 95 percent of TPSDs first began collective bargaining negotiations between 1984 and 1987. The unit of observation in these data is a district, contract, salary-track observation, where salary tracks are broken out by teacher education (e.g., bachelor’s or master’s degree). To make this explicit, I present a fictitious contract in Online Appendix Table A.2. A teacher’s pay is determined solely based on their years of experience and education level. Notice that payment increases may not necessarily occur each year, for example, consider the payments for years of experience 14-15. For each district’s contract, I observe the entry- and top-level salaries (those corresponding with experience rows 0 and 28 in
Table A.2). However, for most contracts, SERB data custodians instead recorded the top-level salaries as the first year in which a salary does not change with experience, introducing additional noise into this measure. In Table A.2, this corresponds to the bold values, i.e., 8 years of experience for “Non-degree” teachers and 14 years of experience for all other education categories. Beyond salary information, I observe contract information concerning the negotiated number of hours in the work week, and the number of steps and years to reach the top of the pay scale.

In order to fill in information for mid-range and true top-level negotiated salaries, I turn to restricted-access, teacher-level data provided by the Ohio Department of Education (ODE). These data include teacher salary, experience, education, and current school of employment. Importantly, these data follow teachers as they move between public schools within the state, including charters. Furthermore, I observe all necessary information (i.e., teacher experience and education) to determine each teacher’s particular position on their district’s pay scale. As a result, I can use SERB contract negotiation dates in tandem with these teacher-level data to back out entire salary structures for each negotiated contract. This allows me to estimate the effect of charter competition across the entire negotiated salary distribution (see Online Appendix B).

To measure charter competition, I collect public school finance reports for the universe of Ohio school districts from the ODE’s school finance website. In Ohio, when a student decides to attend a charter school instead of his or her default public school, the district must directly pay the baseline per-pupil state funding amount to the charter. The ODE finance reports capture these exact payments as well as a full-time-equivalency count of the number of students each district sends to each charter school in the state, including digital charters.

In addition, I employ information from three National Center for Education Statistics data sources: the School District Universe Survey, School Building Universe Survey, and School District Finance Survey. The first two datasets provide information about student enrollment and teacher employment (see Online Appendix Table A.1 for specifics). The School District Finance Survey provides TPSD revenue and expenditure information.

Finally, I utilize property tax data from the Ohio Department of Taxation, which includes annual property valuations broken out at the district level. These valuations determine the property tax base for TPSD local revenue calculations.
2.2 Measuring Charter Competition

I measure charter competition using information about the number of students transferring from TPSDs to charter schools (Linick, 2014).\(^6\) Specifically, my preferred measure of charter competition is the fraction of a district’s potential membership that instead transfer to a charter school in the given year, i.e., Charter Competition = \(\frac{\text{# Transfers to Charter}}{\text{# Students Enrolled in TPSD} + \text{# Transfers to Charter}}\).\(^7\) This measure of competition includes student transfers to conversion, start-up, digital, and brick-and-mortar charters (see Section 1).

In 2001, the ODE began generating “District Foundation Settlement Reports” that record the number of students and accompanying funds sent by each district to each charter school across the state, including digital charters. For 2001 and later, I use this full-time equivalency count as my measure of the number of students transferring to charters from each district.

From 1998 to 2001, in order to estimate a proxy for charter transfers, I use information from several sources. Competition is proxied using the amount of district-aggregated funds transferred to charter schools each year as recorded in the Common Core of Data (CCD) School District Finance Survey. To convert these dollar values into counts of transferring students, I collected information on the baseline formula dollar amounts paid to a charter school for transferring a single student each year. Note that unlike the post-2001 data, these measures only provide the general competition a specific district faces making it impossible to disaggregate transfers between digital and brick-and-mortar charters. For twenty-one district-year observations with missing CCD charter payment information prior to 2001, I fill in the district’s charter transfers with the cumulative enrollment counts for all charters “serving” the given district.\(^8\)

In Figure 1, I plot the aggregated enrollment counts from the CCD LEA Universe Survey for all charter schools in the state, as well as an aggregated version of my measure of charter transfers. From 2001 onward, even though my measure of charter competition is not based on CCD data, I am able to closely mirror aggregated CCD charter enrollment. I take this as evidence that my

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\(^6\) Notice that student-transfer measures do not capture charter threat. For example, a charter opening within TPSD boundaries that was unable to recruit students from the given TPSD would be measured as contributing no competitive pressure. In Online Appendix C, I show that my main estimates are robust across a variety of charter competition measures.

\(^7\) Potential enrollment and charter transfers are in full-time equivalency terms.

\(^8\) While students are free to attend any charter across the state, charters are tied to a “serving” district. The eligibility of this district originally determines whether the charter could open. This assumption is reasonable particularly for the earlier years of charter entry. In 2001 and 2002, over three-fourths of all districts sent 65 percent or more of their charter transfers to charters specifically serving their district.
measure of charter competition adequately captures statewide charter enrollment. For 2001 and later, Figure 1 also decomposes the number of charter transfers into the number transferring to brick-and-mortar and digital charter schools. In 2001, charter transfers were almost entirely made up of brick-and-mortar schools, with the share of the charter market captured by digital charters steadily growing over time.

The spatial growth as measured by the fraction of TPSD enrollment that instead transfers to a charter school is shown in Figure 2. Because the most urban districts have always been eligible for charter entry, charter school hot-spots appear in Ohio’s largest cities. However, legislation passed in 1999 and 2002 that allowed charters to open in struggling districts across the state coupled with statewide digital charter admissions generate large increases in the fraction of charter transfers across rural Ohio in later years.

Taken together, Figures 1 and 2 show why Ohio is an excellent state to assess the effects of charter competition. There has been rapid and relatively recent charter introduction and expansion, which provides the necessary treatment variation. These facts coupled with the policy structure that generates exogenous variation in the timing and location of charter entry make Ohio an ideal setting to study the competitive effects of charter schools.

Table 1 presents descriptive statistics for TPSDs broken out by various levels of charter competition intensity. The top panel reports district characteristics and finance information for outcomes that vary at the district-year level. Column 1 provides the mean and standard deviation for the full sample of district-year observations that have non-missing values for all variables in the panel. Columns 2 through 4 present summary statistics for district-year combinations facing no charter competition, non-zero levels of competition, and the top quartile of charter competition, respectively.

Comparing across columns, districts facing higher levels of competition are larger (enroll more students and employ more teachers), have higher assessed total property values, have a higher proportion of black students as well as students qualified for free lunch, spend more overall, and have higher instructional and capital expenditures. I also report descriptive statistics for the amount of charter competition faced by each TPSD. It is worth noting that for the full sample, on average, the fraction of potential enrollment transferring to charters is roughly 0.014. This fraction more than triples for districts facing the top quartile of competition. In the bottom panel, I report entry-
and top-level salaries negotiated between districts and unions. The unit of observation in this panel is a contract-education cell. The pattern is not as clear across these variables, but districts facing competition tend to negotiate higher salaries. Overall, the main empirical results of this paper are not visible in these simple tabulations.

3 Methodology

3.1 Baseline Estimates: Difference-in-Difference Framework

To understand the intuition behind my baseline empirical setup, consider two districts within the same local economy in a given year where one experiences increased charter competition in the following year and the other does not. My baseline estimation strategy simply compares the change in outcomes over time between these districts. Specifically, I estimate the following model:

\[
y_{ict} = \alpha + \beta C_{it} + \gamma_{ct} + \phi_i + \epsilon_{ict}
\]

where \( y_{ict} \) is the outcome of interest for district \( i \) during school year \( t \) in commuting zone \( c \), \( \gamma_{ct} \) are commuting-zone-by-school-year fixed effects, \( \phi_i \) are district fixed effects, and \( \epsilon_{ict} \) is an idiosyncratic error term. \( C_{it} \) denotes the charter competition faced by district \( i \) during the school year \( t \). Standard errors are clustered on districts. This setting accounts for year-specific shocks affecting all districts across a given commuting zone as well as time-invariant district characteristics. The identifying assumption of equation (1) is that conditional on the fixed effects, charter competition is uncorrelated with any other determinants of the outcome.

Because students choose which school to attend as well as the timing of transfers, any trends in factors driving these choices that also correlate with trends in district outcomes are sources of bias. For instance, suppose that charters tend to locate near districts with downward trending student performance. Further, suppose that these districts experience state sanctions that restrict their budgets and induce changes to resource allocation. Without accounting for district performance trends, charter entry into these downward-trending districts would correlate spuriously with changes in district fiscal outcomes. The commuting-zone-by-school-year fixed effects \( \gamma_{ct} \) help account for

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\(^9\) Year 2000 Commuting Zones are downloaded from the Department of Agriculture (website), which are designed to delineate the local economies where people work and live.
unobservable factors potentially correlating with trends in outcomes and charter entry by forcing comparisons only to be made between districts within the same commuting zone.\textsuperscript{10} In the previous example, shifts in quality that affect all districts within a commuting zone are absorbed by $\gamma_{ct}$, but differential quality shifts within a commuting zone would still potentially induce biases.

### 3.2 Preferred Estimates: Instrumental Variables Framework

To account for potential unobservable trends that would bias my baseline estimates, I exploit both the lengthy charter approval process as well as plausibly exogenous changes to policies that determine the location and timing of charter entry in an instrumental variables framework.

In 1997, Ohio legislators passed a bill that, in addition to piloting a new start-up charter program in Lucas county, allowed new start-ups to open in the “Big 8” urban districts (Ohio HB 55).\textsuperscript{11} This bill also allowed conversion charter schools the option to open across the state. In 1999, another bill (Ohio HB 282) passed that allowed start-up charters to open in the twenty-one largest urban districts. Further, starting in the 2000 school year, start-ups across the state could open in any district rated as “Academic Emergency” (AE) in the previous school year based on Ohio’s performance index rating system.\textsuperscript{12} In 2003, legislation passed that allowed start-up charters to open in any districts rated as “Academic Watch” (AW) or AE in the previous school year, but the bill again limited new start-up charters to open in the “Big 8” districts (down from 21 eligible districts) without regard to the previous year’s performance rating (Ohio HB 364 and HB 3). These designations only affect whether charters are permitted to enter a particular district. Once opened, charters are allowed to persist without regard to their district’s current eligibility status.

Table 2 provides the number of districts eligible for new charter entry in the given year based on “Urban 8/21” policies in column 1 and district ratings during the previous school year in column 2.\textsuperscript{13} Column 3 presents the total number of districts eligible for new charters to begin the approval process to eventually open within the TPSD. It is worth noting that most of my identifying variation is coming from the 2000-2005 school years, which corresponds to the years with the largest amount

\textsuperscript{10}A specification only including year fixed effects would implicitly be assuming that charter transfer intensity varies exogenously across the entire state. Including commuting-zone-by-school-year fixed effects instead relies on the assumption that charter transfer intensity varies exogenously across school districts within commuting zones.

\textsuperscript{11}The “Big 8” urban districts are comprised of Akron, Canton, Cincinnati, Cleveland, Columbus, Dayton, Toledo, and Youngstown.

\textsuperscript{12}Performance Index ratings are calculated by taking a weighted average of the fraction of students who passed different statewide goals. See Online Appendix D for a detailed explanation of the ratings designation system in Ohio.

\textsuperscript{13}“Urban 8/21” districts that are also rated as AE/AW only appear in column 1.
of charter growth (see Figure 1). Column 4 presents the number of new charters that actually open during the subsequent school year based on district eligibility during the given year. As expected, the number of new charters tracks closely with the number of eligible TPSDs. For example, 2003 was the first year that districts with a lagged AW designation were eligible for charter entry, which led to the sharp increase in number of eligible districts. Based on the eligibility of these 78 districts in 2003, 90 new charters opened up in 2004, representing roughly a three-fold increase over the previous year.

In order for a start-up charter school to open, there is a very specific timeline that must be followed. This timeline is graphically depicted in Figure 3a for district-year outcomes. In general, Ohio policies create a one-year lag from when a district is eligible for charter entry to when the new charter can open its doors. Thus, denoting time with respect to an outcome in school year \( t \), a new charter opening in period \( t \) would have needed to initiate the filing process in the previous school year (denoted \( t - 1 \)). Further, district eligibility in \( t - 1 \) depends on whether it is a “Big 8”/“Urban 21” district in \( t - 1 \) or based on the district’s academic rating in the previous school year, \( t - 2 \).

I use as instruments the change in the differential effects of being rated in AE(AW) before and after the introduction of the 2000(2003) policies that introduced using district rating criteria to determine charter eligibility. I operationalize this by interacting both \( t - 2 \) lagged district rating indicator variables with an indicator for whether the given policy had been implemented prior to or concurrent with the \( t - 2 \) school year. I then use both interactions as instruments for charter entry and include the main effects as controls. As my third instrument, I include a binary for whether the district was eligible for charter entry during the previous year based on urban district policies (“Big 8/Urban 21”).

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14 Charter schools are required to first enter into a Preliminary Agreement with an authorizer before proceeding to finalize a charter or contract. When a charter enters into a Preliminary Agreement, they must identify their intended district of residence, which must be eligible for charter entry at the time the Preliminary Agreement is executed. Because district ratings are released September 15th each year, TPSD eligibility is based off ratings during the previous school year. Even if the district’s status changes with the release of the next state Report Card, the new charter school is still permitted to open. The Preliminary Agreement must be signed by the end of the December preceding the school year in which the new charter plans to open. Typically, Preliminary Agreements do not extend beyond twelve months. However, the contract/charter has a very specific time frame – it must be adopted no later than March 15th prior to the school year in which the school intends to open, and must be executed by May 15th following the March adoption date.

The school must open by September 30th following the contract execution date, unless it is a school serving primarily dropout prevention and recovery students, in which case the contract is valid for 12 months from the execution date. Generally the whole process takes less than 12 months.
While the timing of the urban district eligibility policies are arguably exogenous, it is plausible that districts receiving an AE/AW rating during the previous year could be subject to correctional responses that possibly correlate with district resource allocation and negotiated salaries (see Craig et al., 2013; Chakrabarti, 2014). Suppose that outcomes react directly to \( t - 2 \) AE/AW ratings. In my empirical framework, I control for this directly and assume that there is no other structural break besides from the policy introduction. The identifying assumption underlying this strategy is that the only thing changing the relationship between \( t - 2 \) AE/AW ratings and subsequent outcomes is the passage of the relevant charter law.

In addition, I exploit the delay in the charter approval process. Suppose that poor district performance affects outcomes through two channels: first, through its effect on charter entry several years into the future, and second, through its more immediate, direct effect on district outcomes. The first channel provides useful identifying variation. To isolate this variation, I include two binary controls, each respectively equal to one if the given district receives an AE or AW rating during the previous period, \( t - 1 \).\(^{15}\) In this setting, the more immediate, direct effects of poor district performance are absorbed by the \( t - 1 \) district rating indicators and the IV is identified off of the lagged structure of charter entry.

Specifically, I regress:

\[
y_{ict} = \beta C_{ict} + \delta_1 1(AW)_{i,t-1} + \delta_2 1(AE)_{i,t-1} + \phi_1 1(AW)_{i,t-2} + \phi_2 1(AE)_{i,t-2} + \gamma_{ct} + \eta_i + \epsilon_{ict}
\]

using the corresponding first stage

\[
C_{ict} = \kappa_1 1(AW)_{i,t-1} + \kappa_2 1(AE)_{i,t-1} + \xi_1 1(AW)_{i,t-2} + \xi_2 1(AE)_{i,t-2} + \theta_1 1(AE)_{i,t-2} \cdot 1(\text{Post 1999})_{t-2} + \theta_2 1(AW)_{i,t-2} \cdot 1(\text{Post 2002})_{t-2} + \theta_3 1(U)_{i,t-1} + \Gamma_{ct} + \psi_i + \nu_{ict}
\]

where \( y \) is an outcome for district \( i \) in school year \( t \) and commuting zone \( c \). \( C \) is a measure of charter

\(^{15}\)Recall that charter potential during period \( t - 1 \) is a function of \( t - 2 \) district ratings, allowing me to separately control for period \( t - 1 \) academic ratings.
Charter competition faced by district $i$ in year $t$.\footnote{Charter competition is measured with error. To the extent that the instruments are correlated with the true level of charter competition and are uncorrelated with measurement error, estimating (2) will correct for the mismeasurement.} $\mathbb{1}(AW)$ is a binary equal to one if the district was in “Academic Watch” one period ago, denoted with subscript $t-1$ or two periods ago, denoted $t-2$. $\mathbb{1}(AE)$ is similarly defined, but for districts previously rated as being in “Academic Emergency”. $\mathbb{1}(\text{Post 1999/2002})$ denote binaries equal to one if the $t-2$ school year occurred on or after 1999 and 2002, respectively.\footnote{The 2000 law used AE status as a criterion for TPSD charter eligibility starting with the 1999 school year. The 2003 policy made TPSD charter eligibility reliant on AW status starting with the 2002 school year. Hence, I interact $t-2$ TPSD ratings with $t-2$ 1999 and 2002 indicators.} $\mathbb{1}(U)$ denotes a binary for whether the district qualified for new charter entry in the previous year by being one of the urban eight/twenty-one districts or a district in Lucas county. All other variables are as previously defined.

This setup is embedded within the baseline difference-in-difference framework from (1). Thus, the remaining threat to validity comes from any change in the relationship between lagged AE(AW) status and outcomes before and after 1999(2002) that is not due to the introduction of the given charter policy across districts within a given commute-zone.

### 3.2.1 No Child Left Behind and the Great Recession

In 2002, the No Child Left Behind Act (NCLB) was signed into law as an update to the Elementary and Secondary Education Act of 1965. Because the introduction of NCLB accountability measures overlap with the implementation of the aforementioned Ohio charter policies, NCLB presents a potential concern for the validity of my identification strategy. Under NCLB, districts were rated by whether they met indicators based on the percent of various student subgroups passing standardized tests. Districts were considered to be on an acceptable trajectory if they met their “Adequate Yearly Progress” (AYP) requirements and schools/districts consecutively failing to meet AYP requirements received increasingly harsh federal sanctions as failing tenure increased.\footnote{Sanctions included setting aside part of a school’s Title I funding to allow students to transfer out of their school of residence and to provide free tutoring. Schools consecutively failing AYP even risked closure.}

AYP criteria play only a minor role in the determination of AE/AW ratings in Ohio and thus have limited potential to affect TPSD charter eligibility.\footnote{Specifically, districts with Ohio-specific state indicators between 50-74.9 percent or that have a performance index score of 80-89.9 are rated as “Continuous Improvement” if they meet AYP or “Academic Watch” if they fail AYP. For all other cases, AYP status cannot change a district’s final categorical rating (See Online Appendix Figure D.1).} However, because NCLB was implemented within a similar window to the charter policies I exploit, it is plausible that the introduction of NCLB could directly affect how TPSDs allocate their resources outside of its effect on charter school
transfers creating a potential bias in my instruments. With that said, my $t-1$ lagged district rating information will partially control for any NCLB direct effects. Further, while NCLB was signed into law in 2002, many of the sanctions could not be implemented until schools/districts repeatedly failed AYP, potentially making the full-impact of NCLB not felt until around 2004 to 2005.

In order to test the sensitivity of my estimates to potential NCLB contamination, in Online Appendix E, I present two sets of robustness checks. The first set attempts to control directly for NCLB policies using the entire regression sample, while the second uses my original specifications but limits the sample to pre-NCLB school years. Incidentally, because the Great Recession occurred several years after the passage of NCLB, this set of specifications also tests the extent to which the Great Recession may be driving my results. Estimates are stable across both sets of robustness checks providing evidence that NCLB sanctions and the Great Recession do not present a first-order validity concern.

3.2.2 Interpreting the Local Average Treatment Effect

Due to the complicated instrument structure, it is worth carefully describing the subset of districts that identify the local average treatment effect (LATE) (Imbens and Angrist, 1994). My empirical strategy estimates a LATE from the population of schools that were eligible in the previous period for new charter entry based on either low academic ratings or large urban district categorical eligibility. As a result, my estimates provide the causal effect of charter competition specifically for these low-performing districts and will miss any heterogeneous charter effects at different points of the district performance distribution.20

Further, I am only identifying the effect of charter entry from charters serving a given TPSD. Specifically, the instruments leverage only increased charter transfers resulting from the charter potential status of the given district. To see this, consider a TPSD that was ineligible for new charter entry last period but is neighboring a district that recently opened a new charter school. Even if the TPSD sends students to the new charter in the neighboring district, because the TPSD was ineligible for new charter entry these transfers do not contribute identifying variation. Thus, my strategy also abstracts from estimating the effect of any inter-district spill-overs of charter competition.

20Specifically, the LATE is an efficiently weighted average of the causal effects for districts made eligible from either low academic ratings or urban district categorical eligibility (Angrist et al., 2016). However, because urban districts often receive poor academic ratings, my estimates will primarily reflect effects for low-performing districts.
3.3 Adjusting Methodology for Contract Outcomes

Union contract outcomes have a fundamentally different data structure than the district-by-school-year outcomes discussed in 3.2. As a result, to estimate the effect of charter competition on contract outcomes, I must augment the estimation procedure. One important difference between contract outcomes and district-by-school-year outcomes is that union contracts are negotiated intermittently instead of annually. Below, I describe the bias that would result had I attempted to assess annual teacher salary measures from staff data instead of intermittent contract measures. Following this discussion, I detail how I adjust my estimation framework for collectively bargained contract outcomes.

3.3.1 Mechanical Bias from Partially Fixed Outcomes

In this section, I demonstrate that even ignoring the biases arising from selection concerns, a standard approach to estimating the effect of competition on negotiated salaries is mechanically biased if the researcher treats salaries as though they vary annually when in reality the contracts are negotiated intermittently.\textsuperscript{21} In Appendix A, I derive a closed-form solution for the amount of mechanical bias that results from treating the dependent variable as though it can vary in each period, when in reality, it only varies periodically.

Specifically, if I denote $\beta$ as the true parameter value for the variable of interest, $x$, with the accompanying estimate $\hat{\beta}$, the mechanical bias is given by

\begin{equation}
\hat{\beta} = \beta \left[1 - \delta (1 - \rho_x(g))\right] \tag{4}
\end{equation}

where $\delta$ is the fraction of outcome observations fixed in the sample, but treated as if they vary and $\rho_x(g)$ is the correlation coefficient between $x$ and $g$ lags of $x$. Because $\rho_x(g) \in [-1, 1]$ the estimate can either overstate or understate the truth depending on the autocorrelation in $x$. However, in applications using data with a positive serial correlation in $x$, the bias attenuates estimates. Notice, that the bias disappears if $\delta = 0$, i.e., that all outcomes vary annually, or if $\rho_x(g) = 1$, i.e., $x$ is perfectly serially correlated so that $x$ values during a year when the outcome can vary are a perfect representation of the $x$ values during the fixed outcome years. In Appendix A.1, I provide evidence\textsuperscript{21}For example, Vedder and Hall (2000) study the effect of private school competition on teacher salaries in Ohio and treat salaries as if they vary annually.
from Monte Carlo simulations that the predicted bias from (4) matches the estimated bias when the truth is known. I also show that restricting the sample to only observations in which the outcome can vary completely mitigates the bias.\textsuperscript{22}

This bias is similar to the well-known “seam bias” arising from telescoping behavior of respondents in important retrospective panels such as the Survey of Income and Program Participation (see Pischke, 1995; Ham et al., 2009; Pei, 2015).\textsuperscript{23} While not directly applicable to duration or event study models as estimated in Ham et al. (2009) and Pei (2015), any researcher interested in estimating the effect of some $x$ on $y$ using retrospective panels can potentially mitigate the bias resulting from telescoping behavior by restricting the sample only to observations from the month the survey was collected and omit observations from retrospective months. However, further work is needed to formalize this extension and fully characterize its implications for retrospective panels.

In the setting of this study, contracts are negotiated roughly every three years (i.e., $\delta \approx 0.667$) and the serial correlation in charter competition between non-contract years and the corresponding previous negotiation year is roughly 0.9 (i.e., $\rho_x \approx 0.9$). Thus, estimates of the effect of charter competition on annual measures are predicted to be attenuated by about 7 percent. In order to avoid this mechanical bias when estimating the effect of charter competition on collectively bargained wage contracts, the researcher must observe the contract negotiation dates to correctly specify the years in which observed outcomes are able to adjust. As a result, my empirical analysis for contract outcomes will be conducted on only the years with newly negotiated contracts.

3.3.2 Difference-in-Difference Framework

A contract-start-year-by-district uniquely identifies a particular contract. Further, there is a separate salary structure for teachers with different levels of education.\textsuperscript{24} For brevity, I designate each of these categories as different “salary tracks.” Thus, the unit of observation for a given pay scale step (e.g., entry or top salary) is at the district-by-salary-track-by-contract-start-year level and regressions are limited only to the start years of new contracts. For my baseline specification,\textsuperscript{22} For example, Card (1990) studies the effect of previous collectively bargained wage rates on subsequent wages by assessing union contract outcomes directly.\textsuperscript{23} Telescoping occurs when respondents answer retrospective questions using information from the present.\textsuperscript{24} There is a separate salary schedule for teachers with no degree, a Bachelor’s Degree, a Bachelor’s Degree and 150 semester hours, a Master’s Degree, a Master’s Degree and 15 additional graduate semester hours, a Master’s Degree and 30 additional graduate semester hours, and a Doctoral Degree.
I estimate a model similar to equation (1). Specifically, I estimate

\begin{equation}
y_{isc \tau} = \alpha + \beta \cdot C_{i, \tau-1} + \gamma_{c \tau} + \xi_s + \phi_i + \epsilon_{isc \tau}
\end{equation}

where \( y_{isc \tau} \) is a contract outcome variable occurring during the contract-start-year \( \tau \) for district \( i \), commuting zone \( c \), and salary track \( s \), \( \gamma_{c \tau} \) are contract-start-year-by-commuting-zone \( c \) fixed effects, \( \phi_i \) and \( \xi_s \) are respectively district and salary-track fixed effects, and \( \epsilon_{isc \tau} \) is the error term. \( C_{i, \tau-1} \) denotes the charter competition faced by district \( i \) during the school year prior to the contract start year \( \tau \) (denoted \( \tau - 1 \)). The construction of this variable accounts for the fact that contracts are negotiated during the year prior to the contract start year. I choose to model charter competition as the amount that would be faced during the year that the contract is negotiated as opposed to the year the contract is enforced. Standard errors are clustered by district.

### 3.3.3 Instrumental Variables Framework

For contract outcomes, I adjust (2) by adding salary track fixed effects \( \xi_s \). Further, I add an additional lag for all right-hand-side variables because the relevant charter competition is now in the school year prior to the contract-start-year \( \tau - 1 \) (see Figure 3b). Specifically, I regress:

\begin{equation}
y_{isc \tau} = \beta C_{isc, \tau-1} + \delta_1 \mathbb{1}(AW)_{i, \tau-2} + \delta_2 \mathbb{1}(AE)_{i, \tau-2} + \phi_1 \mathbb{1}(AW)_{i, \tau-3} + \phi_2 \mathbb{1}(AE)_{i, \tau-3} + \gamma_{c \tau} + \xi_s + \eta_i + \epsilon_{isc \tau}.
\end{equation}

The first stage is given by

\begin{equation}
C_{isc, \tau-1} = \kappa_1 \mathbb{1}(AW)_{i, \tau-2} + \kappa_2 \mathbb{1}(AE)_{i, \tau-2} + \xi_1 \mathbb{1}(AW)_{i, \tau-3} + \xi_2 \mathbb{1}(AE)_{i, \tau-3} + \theta_1 \mathbb{1}(AE)_{i, \tau-3} \cdot \mathbb{1}(Post \ 1999)_{\tau-3} + \theta_2 \mathbb{1}(AW)_{i, \tau-3} \cdot \mathbb{1}(Post \ 2002)_{\tau-3} + \theta_3 \mathbb{1}(U)_{i, \tau-2} + \Gamma_{c \tau} + \varphi_s + \psi_i + \nu_{isc, \tau-1}.
\end{equation}

### 3.4 Validating the Instrumental Variables Strategy

Because my instruments largely exploit variation in districts at the bottom of the performance distribution, one concern about my identification strategy is that these districts are trending downward for reasons outside of charter competition and that these unobservables correlate both with
trends in charter transfers and district finance.

I validate my empirical strategy by comparing the effects of a district receiving an “Academic Watch” (AW) rating before and after the introduction of the 2003 charter entry policy for a variety of outcomes. Recall the 2003 policy authorized charters to open within districts receiving an AW rating during the previous school year. If my empirical strategy successfully isolates the variation in charter entry driven by lagged AW status, then the effect of lagged AW ratings on any outcome relative to 2003 should be zero for 2002 and earlier and then potentially non-zero thereafter. I focus on the 2003 policy because I have the longest panel of pre- and post-policy data. Ohio first began implementing this particular rating system in 1998, making 2000 the first year I am able to observe a two-period lag of AW ratings. I implement this test by regressing

$$y_{ict} = \sum_{t=2000; t\neq 2003}^{2011} \{\beta_t \mathbb{1}(AW)_{i,t-2} \times \mathbb{1}(Year = t)\} + \delta \mathbb{1}(AW)_{i,t-2} + \gamma_{ct} + \eta_i + \varepsilon_{ict},$$

where $y_{ict}$ is the given outcome for district $i$ during school year $t$, in commute zone $c$, and $\mathbb{1}(AW)$ and fixed effects are defined as in (2). Fixed effects are included to net out unobservables already accounted for by the baseline difference-in-difference specification. Each $\beta_t$ captures the year-specific effect of receiving an AW rating two years prior to school year $t$. In 2003, TPSDs were first eligible for charters to begin the paperwork to enter based on AW status. Thus, new charters entering from the 2003 law would be able to open as early as the 2004 school year. As a result, I benchmark all estimates relative to 2003. For contract outcomes, I adapt equation (8) using the notation from equation (6) to estimate

$$y_{iset} = \sum_{t=2001; t\neq 2004}^{2011} \{\beta_{t-3} \mathbb{1}(AW)_{i,t-3} \times \mathbb{1}(Contract-start-year = \tau)\} + \delta \mathbb{1}(AW)_{i,t-3} + \gamma_{ct} + \eta_i + \xi_s + \varepsilon_{iset}.$$

Notice that these validity checks only exploit a portion of my identifying variation. The full specification benefits from the additional power provided by the urban district and 2000 AE eli-

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25 The year main effects are absorbed by $\gamma_{ct}$. 

18
ility policies.

Figure 4a displays the time-varying effects of receiving an AW rating two years earlier on the fraction of students transferring out of the district. The difference between the average effects before and after 2003 provide a visual for the first-stage estimates of the policy. For earlier school years, the differential effects of a lagged AW rating on charter entry is statistically indistinguishable from the 2003 effect. For 2004 and later, a lagged AW rating generates significantly more charter entry than in 2003. This pattern supports the validity of my instrument because charter entry is only driven by lagged AW ratings after the relevant policy change.

In the subsequent panels of Figure 4, I present the differential effects of lagged AW status on various outcomes, which depict the reduced-form estimates of the AW policy. While individual estimates are often underpowered, the overall trends are still illuminating.26 First, in Figure 4b, I plot the effect of a district receiving an AW rating on the inverse hyperbolic sine (IHS) of district-level appraised property values.27 This measure could reflect information about how the economy as a whole is affected over time. For the pre-2003 years, the effect of being rated in AW two years earlier is not statistically different than the 2003 effect, but post-2003 the effects inversely track the estimated charter entry effects. This figure foreshadows the result in Section 4.2 that charter competition depresses the appraised property values used to compute district local revenues. The absence of a pre-trend during the pre-2003 years suggests that this effect is attributable to the increase in charter competition accompanying a lagged AW rating instead of other unobserved trends not handled by my estimation strategy.

Figures 4c and 4d depict the same test on instructional expenditures and capital outlays. Overall, there is little evidence of pre-trends prior to 2003 providing support for the efficacy of the identification strategy. Further, I find evidence of negative post-2003 effects on instructional spending and noisy, positive effects on capital outlays. Finally, the results for the rest of the outcomes explored in this paper are presented in Figures F.1 through F.7 of Online Appendix F. Overall, these tests support the instrumental variables framework from (2) as a plausibly valid estimation

26For the later years in the sample, few districts were eligible for charter entry based on lagged AW ratings (see Table 2).
27The inverse hyperbolic sine of y is \( \text{sinh}^{-1}(y) = \ln \left( y + \sqrt{y^2 + 1} \right) \). Note that \( \text{sinh}^{-1}(0) = 0 \) and similar to the natural log transformation, \( \frac{\partial \text{sinh}^{-1}(y)}{\partial x} = \left( \frac{1}{\sqrt{y^2 + 1}} \right) \frac{\partial y}{\partial x} \rightarrow \frac{1}{y} \left( \frac{\partial y}{\partial x} \right) \) as \( y \rightarrow \infty \) (see footnote 17 of Cascio and Narayan, 2015). I use the IHS instead of the standard log transformation to avoid dropping observations with null values.
4 The Effect of Charter Competition on Mobility & District Revenues

4.1 Student and Teacher Mobility Responses

Before looking at how charter competition affects the budget and resource allocation among traditional public school districts, it is helpful to assess how charter competition influences student and teacher mobility. There is a mechanical relationship between charter transfers and the sending district’s total enrollment. Losing one student to a charter school will mechanically decrease the sending district’s enrollment by one. If not, then charter transfers likely correlate with other types of either student entry into or exit from the TPSD suggesting that my estimation strategy is unable to fully isolate the effect of charter transfers. To test this directly, I estimate equation (2) using “potential enrollment” (i.e., actual enrollment + charter transfers) as the outcome; however, I instead measure charter transfers and “potential enrollment” in levels. Theoretically, this regression should yield a null coefficient if charter competition does not induce any other type of entry into or exit from the district. Indeed, I estimate that the effect of transferring a single student to a charter school on the district’s overall “potential enrollment” is statistically indistinguishable from zero (a 0.099 point estimate with a standard error of 0.235). This is consistent with the idea that charter competition is not inducing additional exit to private schools for example (Chakrabarti and Roy, 2016) or exit from the state.

Table 3 displays the effect of a one percentage point increase in the fraction of TPSD students who transfer to charters on a range of mobility outcomes. The table shows the results from baseline OLS estimates of equation (1) and my preferred instrumental variables (IV) estimates of equation (2). The accompanying first-stage estimates and the weak instrument test are located in the table notes. To better understand what types of students are leaving the districts, columns 1 and 2 respectively present the effect of charter transfers on the inverse hyperbolic sine (IHS) of free/reduced-price lunch (FRL) eligible and special education student enrollment. Because these and other outcomes can take on null values, I use an IHS transformation to give the coefficients a similar interpretation as a log transformation without losing null observations. I estimate that a one percentage point increase in the fraction of charter transfers reduces FRL eligible and special education student enrollment.
education student enrollment by 6.9 percent and 3.2 percent, respectively.

In response to the overall decline in the number of enrolled students, if districts did nothing to adjust teacher labor supply, then student-teacher ratios would decrease. However, columns 3 and 4 show that districts respond to the decrease in enrollment by reducing the size of the teaching force in lock-step. Student-teacher ratios are preserved by a 3.3 percent reduction in overall teaching staff. Column 5 reveals that this reduction is partly driven by hiring 7.6 percent fewer new teachers.

Finally, columns 6 and 7 show the effect of charter competition on teacher exit and entry between charter schools and TPSDs. As is pointed out by Jackson (2012), because teachers can only move between charters and TPSDs when charters are present, there will be a mechanically positive relationship between charter competition and these measures of teacher mobility. I estimate that a percentage point increase in charter competition increases teacher exit to charters by 9.5 percent and increases teacher entry into TPSDs from charters by 4.7 percent. However, note that these estimates are off of an extremely small base. Specifically, districts at the 99th percentile of the TPSD-charter teacher mobility distribution only lose 1 teacher to charters and also only gain 1 teacher from charters. Overall, the evidence from this table supports a conceptual framework where districts are competing with charters over students instead of over teachers. Further note that across the table, the baseline estimates are qualitatively similar to my preferred specification though are more precisely estimated and are smaller in magnitude.

All three of the instruments generate statistically significant increases in charter entry (see the table note). For example, a district with a two-period lagged “Academic Emergency” rating after the 1999 policy experiences roughly a 1.5 percentage point increase in the fraction of students attending a charter school compared to an equally-rated district prior to 1999. The table note also provides a weak instrument test. The large F statistic for the excluded instruments show that these regressions do not suffer from weak instruments. Hansen J statistics and p-values for tests of overidentification are provided in Online Appendix Table G.1 for all outcomes explored in this paper. The null hypotheses for these tests are that the instruments are valid. Across the different

\[\text{In nearly every dimension, employment at a TPSD dominates employment at a charter school. Pay is often better, there is superior job security due to union affiliation, and work hours are often shorter. As a result, it is plausible that TPSDs will not be competing with charters to retain their teachers.} \]

\[\text{The urban district instrument is only marginally significant in these specifications. For the remaining specifications in Tables 4 to 6, this instrument is significant at the 1 percent level because I utilize a slightly larger regression sample.} \]
outcomes, I find suggestive evidence supporting the validity of my instruments.

### 4.2 District Revenues

With the responses of student and teacher mobility to charter transfers in mind, I now analyze how charter competition influences TPSD revenues. In Ohio, when a student transfers to a charter school, the state funding is still paid to the student’s district of residence as if the student were still enrolled. However, the district is then required to transfer a formula-derived amount of state funding directly to the charter. This transfer is recorded as an expenditure.

Panel A of Table 4 presents baseline OLS estimates from equation (1) and my preferred IV estimates from equation (2) for the IHS of real total, federal, and local revenues.\(^{30}\) Due to the slightly increased sample size as compared to the student and teacher mobility regressions from Table 3, the first-stage estimates are even more precisely estimated (see table notes). I find that increasing the fraction of students transferring to a charter school by one percentage point decreases total revenues by 1.8 percent. To put the size of this effect into context, a percentage point increase in the fraction of students transferring to a charter represents an increase of roughly half of a standard deviation (see Online Appendix Table G.2) or about half of the growth in average charter competition from 1998 to 2010.\(^{31}\)

Decomposing total revenues by government level (columns 2 and 3) reveals that charter competition induces revenue losses at both the federal and local levels. However, I estimate that charter competition has little effect on state revenues.\(^{32}\) The lack of impact that charter transfers have on state revenues is not surprising. Because state funding is paid to the sending district and then transferred to the charter as an expenditure, there is no mechanical link between charter transfers and state revenues. However, while technically counted as an expenditure, these charter payments should be thought of as effectively decreasing the state funding available to TPSDs.

The negative effect of charter competition on federal funding is also not surprising. Major federally funded programs contained in the Child Nutrition Act and the Individuals with Disabilities Education Act for example provide per-pupil funding for certain eligible student groups. Distribution of these federal entitlement grants to school districts are formula-based. Thus, when a student

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\(^{30}\)Since the treatment is a loss of students to charter schools, in order to avoid a mechanical denominator bias in my results Tables 4 through 6 present estimates for total revenue/expenditure categories instead of per-pupil values.

\(^{31}\)The fraction of students transferring to charters in 2010 was roughly 0.03 among districts in the regression sample.

\(^{32}\)For my IV specification, I estimate that a one percentage point increase in charter competition decreases state revenues by a statistically insignificant 1.0 percent.
funded under any of these federal programs transfers to a charter school the accompanying federal revenues are deducted from the TPSD and are instead allocated to the charter. Recall from Table 3 that charter competition decreases the TPSD enrollment of FRL-eligible and special education students. As a result, charter competition should mechanically decrease federal revenues.

To see these channels empirically, I further decompose federal revenue effects in Panel B. Columns 5 and 6 present estimates for two large federal programs and column 7 presents the effect on all other federal revenues. Federal funding is decreasing across both programs.

Unlike the effects of charter competition on federal and state revenues, the negative effect on local revenues is unexpected. I explore potential mechanisms in Panel C by decomposing local revenues into the contribution of local property taxes, school lunch funding, and all other local revenues in columns 8 through 10. While local revenues are decreasing across all three measures, I focus my discussion on local property taxes because they comprise 96.5 percent of local revenues (LSC, 2011). Charter competition can affect local property taxes through two main channels. First, competition can directly decrease appraised property values and, in turn, the base valuation being taxed. Second, charters can decrease the levied millage rates (i.e., one-tenth of one percent) that determine the fraction of the base property values being taxed.

I test these potential mechanisms in columns 11 through 13 of Panel D. In column 11, I present the effect of charter competition directly on the total appraised property values within the TPSD. My estimates suggest that a percentage point increase in the fraction of TPSD students transferring to charters decreases real appraised property values by 2.5 percent. This property value measure aggregates residential, agricultural, commercial, industrial, and mineral properties, as well as other public properties (LSC, 2011).

Column 12 presents the effect of competition solely on appraised residential property values. Charter competition generates nearly identical percent losses for both total property values and residential property values. In column 13, I find that the millage rates tend to decrease as charter competition increases. A simple back-of-the-envelope decomposition reveals that a majority of the decrease in total revenues is driven by the change in total property values.

33 Residential and agricultural properties make up 79.1 percent of total property values in 2008 (LSC, 2011).
34 In Section 3.2.1 and Online Appendix E, I present evidence that these depressed housing values are not driven by the Great Recession.
35 Local revenues (LR) are calculated using \( LR = \frac{\text{Millage}}{1000} \times \text{Property Values} \). The marginal effect of charter competition on property values relative to the average is roughly a $10,200,000 decrease. Relative to average millage rates,
Property values are appraised every six years by a county auditor through visual inspection. Every three years, the appraised value is updated using market transaction data and forecasting algorithms to estimate the value of the property (Sullivan and Sobul, 2010). As a result, the decrease in appraised residential property values could reflect true depreciation of underlying housing values as well as changes in the appraisal process. To rigorously assess the housing capitalization of charter schools I would need parcel-level housing sales data as in Imberman et al. (2016).

Still, the negative effect of charter competition on appraised housing valuation is unexpected. If there is an excess demand for schooling, one might suppose that additional schools would positively capitalize into local housing values. However, competing forces plausibly exist. There is evidence in the literature, and I find evidence in Ohio that charter school transfers generate fiscal stress for the sending TPSDs (Bifulco and Reback, 2014; Arsen and Ni, 2012b). In the spirit of Bifulco and Reback (2014), I estimate that for each student transferring to a charter school, the sending TPSD will on average save $4,027 in variable costs from not having to educate the transferring student. However, this savings only accounts for roughly two-thirds of the state revenue reductions accompanying each charter transfer yielding a net loss. If fiscal stress lowers perceived TPSD quality, then housing prices within the TPSD would also likely decrease to reflect these perceptions (Black, 1999). Thus, the direction of the effect of charter competition on housing prices depends on which force dominates.

There are several reasons to believe that the negative pressures could plausibly dominate in Ohio. First, if charter quality is low, then opening new charters may not generate positive housing capitalization. The Ohio Department of Education places relatively few restrictions on the eligibility of charter school authorizers and implements limited restrictions to ensure quality control. As a result, the quality of the average Ohio charter school may be lower than in other states and may be negatively capitalized into housing values.

Second, positive effects likely apply generally across large geographic areas, while negative...
pressures are likely district-specific. Neither brick-and-mortar nor digital charter schools have geographic enrollment boundaries allowing the potential capitalization gains to spread across district borders. As a result, charter presence may provide a general housing value premium across districts and only a negligible relative premium between nearby districts. Conversely, any housing valuation penalties due to charter-related fiscal stress on local TPSDs would show up as differences in relative housing values between districts. This is relevant for my setting because if new charter entry in a given district improves school choice for parents across all districts in the commuting-zone, this general effect will be subsumed by my commute-zone-by-year fixed effects.\textsuperscript{38}

Third, housing prices within the subset of districts identifying the LATE might be more sensitive to charter loss than the average district. In an unpublished working paper, Buerger (2014) finds negative effects for charter competition on housing prices in poorer neighborhoods. In Section 3.2.2, I described how my IV strategy provides LATE estimates for low-performing school districts and in Ohio the most economically disadvantaged districts also tend to be the lowest performing. Thus, consistent with Buerger’s findings, my negative housing capitalization effects are local to poorer neighborhoods.

There is additional evidence in the literature that charter competition may put downward pressure on residential property values. Imberman et al. (2016) study housing sales price responses to charter competition in the Los Angeles Unified School District (LAUSD) and find no effect on average. However, upon restricting attention to houses outside of the LAUSD, Imberman et al. (2016) find that additional charters entering within a household’s TPSD boundary have a negative effect on housing sales prices.

As students transfer to charter schools, the only mechanical effect on a district’s budget is the required transfer of state funding to the charter. A tempting yet spurious conclusion would be to infer that charters only reduce the overall size of the budget through this mechanical increase in charter-transfer expenditures. However, the main takeaway from Table 4 is that charter competition places additional fiscal stress on TPSDs as federal revenues decline from “at-risk” student transfers and local revenues are lost from depressed residential housing valuations.

\textsuperscript{38}Regressing the main IV specification in (2), but substituting year fixed effects for commute-zone-by-year fixed effects generates statistically indistinguishable estimates. As a result, this concern is not likely a first-order issue.
5 The Effect of Charter Competition on Collectively Bargained Teacher Contracts

In Ohio, instructional expenditures alone make up over half of total expenditures. Further, Ohio is a heavily unionized state and teacher pay is determined through collective bargaining between TPSDs and teachers’ unions. As a result, before assessing the effect of charter competition on general district resource allocation, I first highlight the effect of charter competition directly on collectively bargained contract outcomes.

Table 5 presents the results from estimating the OLS and IV specifications respectively from equations (5) and (6) for several collectively bargained contract outcomes. Column 1 reports that a percentage point increase in the fraction of TPSD students attending a charter decreases real entry-level salaries by 0.2 percent though this effect is not statistically significant. Column 2 presents the effect of charter transfers on top-level salaries. In both specifications, I estimate that competition does not affect top-level salaries. Recall that in the SERB data, top-level salaries are often coded as the lowest value for which an additional year of experience has no effect on salary.\(^{39}\) As a result, in column 3, I present results for top-level salaries imputed from ODE teacher-level data using the algorithm detailed in Online Appendix B.\(^{40}\) For my preferred estimates, I find that a percentage point increase in the fraction of students transferring to charter schools decreases imputed top-level salaries by 1.0 percent. Relative to the average imputed top-level salary, this translates to a $599 annual salary decrease for teachers at the top of their pay scale.

To further put these effect sizes into context with the literature, Hoxby (1996) estimates that initial unionization increases subsequent teacher salaries by 5 percent. Using this estimate as a baseline for the union wage premium, I estimate that a percentage point increase in the fraction of charter transfers erodes about 20 percent of the union wage premium for the most experienced teachers.

Column 4 characterizes whether charter competition changes the slope of the negotiated salary profile. In these regressions the outcome is the difference between real top- and entry-level salaries divided by the number of steps it takes to reach a top-level salary. I estimate that charter compe-

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39 For example, in Online Appendix Table A.2 the top salaries for non-degree, BA, and MA teachers would be coded respectively as $29,265, $40,401, and $46,616.

40 Specifically, this measure is the created by taking the maximum salary from the 15-20th imputed pay scale steps using the method in Online Appendix B and filling in missing values with SERB top-level contract outcomes.
tition flattens the pay scale, though the effect is only significant for the baseline OLS specification. In addition to affecting the negotiated salaries directly, charter competition could also change the length of time it takes to ascend the pay scale ladder. Column 5 shows the effect of charter competition on the number of steps required to fully ascend the pay scale. While marginally statistically significant, these estimates represent economically insignificant increases seeing that on average teachers face 15 pay scale steps.

Next, I estimate the effect of charter competition on the entire negotiated salary distribution rather than solely focusing on the top and bottom of the pay scale. This analysis assesses the degree to which the previous negative salary effects generalize across the rest of the pay scale. Even though SERB-provided data only include salary information for entry and top pay scale steps, I approximate the negotiated salaries for each intermediate step using teacher-level wage data to estimate equation (B.1) in Online Appendix B.

Figure 5 presents the results from estimating equations (5) and (6) for each imputed pay scale step. The baseline OLS estimates show almost no response across the pay scale. However, my preferred IV estimates suggest that both entry-level and top-level salaries decrease in the presence of charter competition. I estimate that a percentage point increase in the fraction of charter transfers decreases collectively bargained entry-level salaries by 1.8 percent and top-level salaries by 2.3 percent. Interestingly, charter competition has almost no statistically significant effect across the middle of the pay scale distribution. One explanation is that a majority of the teaching staff is comprised of new teachers at the bottom of the pay scale and veteran teachers who have already reached the top-level salaries. By decreasing salaries for these two groups, districts would be able to enjoy the largest savings.

Overall, finding that charter competition decreases collectively bargained salaries may seem unexpected, especially considering the positive effects found in non-union settings by Taylor (2006, 2010) and Jackson (2012). However, there are a few ways to frame these findings. First, because charter schools often pay lower salaries to their teachers (Arsen and Ni, 2012a), competition over students may put downward pressure on TPSD-negotiated salaries to match charter salaries.

Second, this could be a story of union/TPSD negotiating power. Recall Table 3 showed that as districts lose students to charter schools, the size of the teaching labor force reduces in lock-step. As a result, charter competition could give TPSDs leverage in contract negotiations. If TPSDs
cannot decrease teacher salaries and reallocate resources to prevent student transfers, then the size of the teacher labor force represented by the unions will drop. If the threat of downsizing provides leverage for TPSDs, then the loss of union bargaining power may reduce the artificially high union monopoly wage.

6 The Effect of Charter Competition on Resource Allocation

Table 6 presents the effect of charter competition on district expenditures. Column 1 provides an estimate of the financial burden placed on TPSDs from charter transfers. Increasing the fraction of students transferring to a charter by one percentage point, increases the amount of money transferred to charters by roughly $200,000. On average, a percentage point of a district’s potential enrollment is about 30 students, which equates to roughly $6,600 transferred per student, approximately the baseline formula amount in 2010.

To see how total expenditures are influenced by charter competition beyond the mechanical charter-transfer increase, column 2 presents the IHS of total expenditures after netting out any payments to charter schools. IV estimates suggest that total expenditures fall by 1.7 percent which matches up closely with the estimated 1.8 percent decrease in total revenues from Table 4.

Columns 3 through 5 present estimates for the effect of charter competition on the allocation of remaining district resources. I show above that increased charter transfers decrease both negotiated teacher salaries as well as the size of the overall teaching force. As a result, we should expect to see fewer resources spent on teaching expenditures in the presence of charter competition. Indeed, IV estimates suggest that TPSDs facing a percentage point increase in the fraction of students transferring to charters spend 2.3 percent less on instructional expenditures. Curiously though, these districts spend 7.3 percent more on capital outlays while spending 2.8 percent less on all other expenditures. To explore the mechanism driving the surprising increase in total capital outlays, columns 6 and 7 respectively present estimates for the IHS of new construction capital outlays and all other capital outlays. Increases in capital outlays are driven by new construction expenditures (a 11.3 percent increase). These effects are robust across a variety of alternative measures of charter competition (see Online Appendix Table C.1).
7 Discussion

There are several reasons why TPSDs might allocate resources toward capital outlays in response to charter competition. First, suppose principals believe that capital outlays enhance subsequent student performance and school ratings. Because parents factor school ratings information into student enrollment decisions (Cullen et al., 2006; Hastings and Weinstein, 2008; Hanushek et al., 2007), charter competition provides incentives for district administrators to allocate resources to areas that generate gains to student achievement and boost subsequent enrollment. Thus, if capital outlays improve student performance, then charter competition creates incentives for TPSDs to boost student achievement in ways predicted by traditional school choice theory (see Friedman, 1955, 1997; Hoxby, 2003a,b).

Second, the literature provides no clear suggestion for the ideal combination of school expenditures to optimize student achievement and subsequent school ratings given a fixed budget. Thus, it is plausible that district administrators may also be unsure how to boost achievement through resource allocation. If administrators believe that parents value facility condition, then resources may be channeled to capital outlays where each dollar spent is clearly linked to visible facility improvement regardless of subsequent student achievement. This type of allocation is consistent with qualitative survey evidence on how principals in D.C. try to insulate against charter transfers. “The physical appearance of school buildings was said to have the greatest impact on enrollment trends... We noted that principals did not tend to focus on test scores or academic achievement in their lists of attributes that parents sought when selecting schools” (Sullivan et al., 2008, p. 20). If administrators are spending money on capital because parents value it directly (Cellini et al., 2010) and not because administrators believe it improves student performance, then in this scenario, charter competition does not necessarily improve TPSD achievement efficiency. However, competition is still efficiency-enhancing in the sense that it causes districts to spend in areas valued directly by parents.

41 The literature is mixed regarding whether capital spending resulting from narrowly approved local capital bond referenda affect subsequent student achievement. Martorell et al. (2015) find precisely estimated null effects, while Cellini et al. (2010) and Hong and Zimmer (2016) provide evidence that capital outlays can have positive achievement effects several years after the bond passage.

42 “Unfortunately, identification of truly exogenous determinants of ... resource allocations ... is sufficiently rare that other compromises in the data and modelling are frequently required. These coincidental compromises jeopardise the ability to obtain clean estimates of resource effects and may limit the generalisability of any findings” (Hanushek, 2003, pg. 83).
Finally, allocation toward capital outlays may be the result of information acquisition costs. Suppose parents value student achievement, but signals of school quality vary in their acquisition costs. While motivated parents will seek out already available school quality information when provided school choice (Lovenheim and Walsh, 2014), low-cost albeit noisy signals of school quality such as facility condition potentially inform even the time-constrained or less motivated parents. In this case, TPSDs may respond to competition by allocating resources toward these salient signals regardless of the impact on subsequent achievement. This behavior is again consistent with qualitative survey evidence from D.C. principals.\textsuperscript{43} If parents only place value in facility condition because they mistakenly infer information about a school’s potential education production and if capital outlays do not affect achievement, then charter competition actually exacerbates the misallocation of TPSD resources. Under this framework, simple policies can potentially correct the incentives that charters create and instead encourage TPSDs to allocate resources in ways that improve student achievement.\textsuperscript{44}

These three scenarios highlight that charter competition has the potential, but is not guaranteed to encourage TPSDs to reallocate resources in ways that enhance student achievement as predicted by economic theory. Disentangling the mechanisms underlying why charter competition causes TPSDs to reallocate resources is a rich area for future work. Overall, because facility condition is likely valued by parents, as measured by positive housing capitalization (Cellini et al., 2010), I interpret my results as evidence that charter competition causes TPSDs to allocate resources toward areas that are likely valued directly by parents, but that do not necessarily improve student achievement.

8 Conclusion

The charter school movement is one of the fastest growing education reforms in the United States. Charters are designed in part to inject competition into the education market to boost TPSD student achievement. There is a large and mixed literature assessing the effect of charter

\textsuperscript{43}“According to our sample, it appears that most of the changes that schools are making in order to attract more students [from charters] have more to do with services for parents and the image of the school than with improving the educational attainment of students” (Sullivan et al., 2008, p. 21).

\textsuperscript{44}Reducing the cost to obtain school achievement information may help correct competitive incentives (Hastings and Weinstein, 2008). For example, adding simple statistics comparing academic ratings between the schools in the parent’s choice set onto any required school choice form could provide such salient and relevant school quality information.
competition directly on TPSD student achievement (Epple et al., 2015). However, little attention has been given to potential mechanisms. I fill this gap in the literature by analyzing the effect of charter competition on TPSD revenue and resource allocation. I also pay special attention to how charter competition affects collectively bargained teacher compensation in a strongly unionized state. To accomplish this, I exploit both the long charter approval process as well as plausibly exogenous variation in policies that determine the location and timing of Ohio charter entry in an instrumental variables framework. I collect and merge together several datasets, including the universe of Ohio public school teachers’ union contracts as well as district-level charter school transfer information from Ohio Department of Education financial reports.

I find that charter competition directly decreases TPSD revenues in excess of the mechanical loss of state resources due to lower enrollment. As vulnerable student populations transfer to charters, TPSDs lose the federal funding designated to help educate these students. Further, I show that charter competition indirectly decreases TPSD revenues by depressing the appraised value of residential properties, thus lowering the base from which local revenues are taxed. To help mitigate the erosion of TPSD local revenues, states could consider providing countervailing aid to districts facing heavy charter competition.\textsuperscript{45}

Another key finding of this paper is that charter competition causes districts to negotiate lower unionized teacher salaries, spend less on instructional and other expenditures, and spend more on new construction expenditures. Determining whether this type of charter-driven resource allocation improves student achievement is an important area for future work.

\textsuperscript{45}For example, districts facing heavy charter transfers in New York receive state transitional aid designed to mitigate negative fiscal impacts (Bifulco and Reback, 2014).


Hong, K. and R. Zimmer (2016). Does Investing in School Capital Infrastructure Improve Student Achievement?


Table 1: Descriptive Statistics by Charter Competition Categories

<table>
<thead>
<tr>
<th>District Characteristics</th>
<th>Charter Transfers</th>
<th>Full Sample</th>
<th>None</th>
<th>Some</th>
<th>Top Quartile</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>Potential Student</td>
<td></td>
<td>2,884</td>
<td>2,364</td>
<td>3,026</td>
<td>4,724</td>
</tr>
<tr>
<td>Enrollment per District</td>
<td></td>
<td>(4,951)</td>
<td>(3,849)</td>
<td>(5,202)</td>
<td>(9,086)</td>
</tr>
<tr>
<td>Teachers per School</td>
<td></td>
<td>176</td>
<td>147</td>
<td>184</td>
<td>283</td>
</tr>
<tr>
<td>Real Total Property Value (in thousands)</td>
<td></td>
<td>(729,160)</td>
<td>(506,929)</td>
<td>(776,704)</td>
<td>(1,257,359)</td>
</tr>
<tr>
<td>Fraction Black Students</td>
<td></td>
<td>0.054</td>
<td>0.041</td>
<td>0.057</td>
<td>0.134</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.133)</td>
<td>(0.116)</td>
<td>(0.137)</td>
<td>(0.217)</td>
</tr>
<tr>
<td>Fraction FRL Students</td>
<td></td>
<td>0.202</td>
<td>0.148</td>
<td>0.216</td>
<td>0.323</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.155)</td>
<td>(0.118)</td>
<td>(0.161)</td>
<td>(0.182)</td>
</tr>
<tr>
<td>Fraction Student Transfers to Charter</td>
<td></td>
<td>0.014</td>
<td>—</td>
<td>0.018</td>
<td>0.044</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.023)</td>
<td>(0.024)</td>
<td>(0.037)</td>
<td></td>
</tr>
<tr>
<td>Total Expenditures (in thousands)</td>
<td></td>
<td>33,876</td>
<td>23,651</td>
<td>36,665</td>
<td>61,227</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(67,749)</td>
<td>(42,098)</td>
<td>(72,955)</td>
<td>(131,343)</td>
</tr>
<tr>
<td>Instructional Spending</td>
<td></td>
<td>16,509</td>
<td>12,046</td>
<td>17,727</td>
<td>28,576</td>
</tr>
<tr>
<td>(in thousands)</td>
<td></td>
<td>(32,033)</td>
<td>(22,136)</td>
<td>(34,139)</td>
<td>(60,771)</td>
</tr>
<tr>
<td>Capital Outlays (in thousands)</td>
<td></td>
<td>3,780</td>
<td>2,568</td>
<td>4,110</td>
<td>7,075</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(9,904)</td>
<td>(4,487)</td>
<td>(10,902)</td>
<td>(18,574)</td>
</tr>
<tr>
<td>N</td>
<td></td>
<td>8,474</td>
<td>1,816</td>
<td>6,658</td>
<td>1,664</td>
</tr>
</tbody>
</table>

Contract Characteristics

<table>
<thead>
<tr>
<th>Entry-Level Salary</th>
<th>Charter Transfers</th>
<th>Full Sample</th>
<th>None</th>
<th>Some</th>
<th>Top Quartile</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>34,816</td>
<td></td>
<td>33,336</td>
<td>36,875</td>
<td>36,212</td>
<td></td>
</tr>
<tr>
<td>(5,097)</td>
<td></td>
<td>(4,641)</td>
<td>(4,986)</td>
<td>(4,603)</td>
<td></td>
</tr>
<tr>
<td>Top-Level Salary</td>
<td></td>
<td>57,754</td>
<td>54,696</td>
<td>62,013</td>
<td>61,657</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(11,348)</td>
<td>(10,358)</td>
<td>(11,320)</td>
<td>(10,521)</td>
</tr>
<tr>
<td>N</td>
<td></td>
<td>15,948</td>
<td>9,286</td>
<td>6,474</td>
<td>1,296</td>
</tr>
</tbody>
</table>

Notes: Means and standard deviations (in parentheses) are presented. Contract-by-Salary-Track observations represent a given district-by-contract-start-year-by-education-level cell for either entry or top levels of experience. Column 1, provides information on all district-years (contracts) missing none of the variables in the table. Columns 2 and 3 further conditions on whether a given district has transferred no students and any students to charter schools, respectively for the given year/contract-start-year. Column 4 only includes districts/contracts where the district faces the top quartile of charter competition in the given year.
<table>
<thead>
<tr>
<th>Year</th>
<th>Urban 8/21 Districts</th>
<th>Ratings (Emergency or Watch)</th>
<th>Total</th>
<th># New Charters Opening Next Year</th>
</tr>
</thead>
<tbody>
<tr>
<td>1996</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>1997</td>
<td>18</td>
<td>0</td>
<td>18</td>
<td>13</td>
</tr>
<tr>
<td>1998</td>
<td>18</td>
<td>0</td>
<td>18</td>
<td>25</td>
</tr>
<tr>
<td>1999</td>
<td>31</td>
<td>0</td>
<td>31</td>
<td>17</td>
</tr>
<tr>
<td>2000</td>
<td>31</td>
<td>21</td>
<td>52</td>
<td>33</td>
</tr>
<tr>
<td>2001</td>
<td>31</td>
<td>3</td>
<td>34</td>
<td>47</td>
</tr>
<tr>
<td>2002</td>
<td>31</td>
<td>6</td>
<td>37</td>
<td>34</td>
</tr>
<tr>
<td>2003</td>
<td>18</td>
<td>60</td>
<td>78</td>
<td>90</td>
</tr>
<tr>
<td>2004</td>
<td>18</td>
<td>31</td>
<td>49</td>
<td>69</td>
</tr>
<tr>
<td>2005</td>
<td>18</td>
<td>21</td>
<td>39</td>
<td>21</td>
</tr>
<tr>
<td>2006</td>
<td>18</td>
<td>4</td>
<td>22</td>
<td>26</td>
</tr>
<tr>
<td>2007</td>
<td>18</td>
<td>8</td>
<td>26</td>
<td>20</td>
</tr>
<tr>
<td>2008</td>
<td>18</td>
<td>6</td>
<td>24</td>
<td>14</td>
</tr>
<tr>
<td>2009</td>
<td>18</td>
<td>7</td>
<td>25</td>
<td>37</td>
</tr>
<tr>
<td>2010</td>
<td>18</td>
<td>8</td>
<td>26</td>
<td>32</td>
</tr>
<tr>
<td>2011</td>
<td>18</td>
<td>4</td>
<td>22</td>
<td>–</td>
</tr>
</tbody>
</table>

Notes: The table shows the number of districts eligible for charters to begin the process of opening in each given year. Column (1) shows the number of districts eligible based on urbanicity, i.e., whether the district is in Lucas county, or is one of the Big 8 or Urban 21 districts during a year that policy allows charter entry. Column (2) presents the number of districts eligible for new charter entry in the given year based exclusively on eligibility determined by academic ratings during the previous year. If a district is eligible for charter entry based on the criteria in both Columns (1) and (2), the district is only counted in Column (1). Column (3) gives the total number of eligible districts. Column (4) presents the number of new charter schools that will open in the subsequent year due to eligibility in the given year. For example, 25 charter schools opened in 1999 based on district eligibility during 1998. In 2011 there were 608 non-charter school districts and 355 charter schools.
Table 3: Effect of Charter Transfers on Student and Teacher Mobility

<table>
<thead>
<tr>
<th></th>
<th>IHS of Student Count</th>
<th>IHS of Teacher Count</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>FRL Eligible (1)</td>
<td>Special Education (2)</td>
</tr>
<tr>
<td>Fraction of Charter</td>
<td>-0.048***</td>
<td>-0.016***</td>
</tr>
<tr>
<td>Transfers ×100 – OLS</td>
<td>(0.009)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Fraction of Charter</td>
<td>-0.069***</td>
<td>-0.032***</td>
</tr>
<tr>
<td>Transfers ×100 – IV</td>
<td>(0.015)</td>
<td>(0.007)</td>
</tr>
</tbody>
</table>

Notes: N= 8,405 district-year observations. Standard Errors in parentheses are clustered by district. See footnote 27 on page 19 for details on the inverse hyperbolic sine transformation (IHS). First-stage estimates (and standard errors) for excluded instruments are: Post 1999_{r-2} \times \mathbb{1}(\text{Acad. E.}, r-2) = 1.440*** (0.518); Post 2002_{r-2} \times \mathbb{1}(\text{Acad. W.}, r-2) = 2.569*** (0.562); and \_t \_1 \_\_1 Academic Elig. (Urban 8/21) = 0.334 (0.720). The F statistic for excluded instruments is 8.021***. This table reports OLS and 2SLS estimates of the effect of charter competition on different forms of student and teacher mobility. The endogenous variable is the fraction of the district’s potential enrollment that instead transfers to a charter school times 100. Regressions in Panel A include district and commute-zone-by-year fixed effects. Regressions in Panel B further instrument for charter competition using the instruments and additionally include the main effects for the instruments and \_t \_1 Academic Watch/Emergency indicator variables. Each column provide the results of a separate regression. See Online Table G.2 for the mean of each dependent variable and Online Table G.1 for tests of overidentification.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.
Table 4: Effect of Charter Transfers on District Revenues

<table>
<thead>
<tr>
<th>Panel A: IHS of Total Revenues</th>
<th>Total</th>
<th>Federal</th>
<th>Local</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Fraction Charter Transfers ×100 – OLS</td>
<td>-0.007***</td>
<td>-0.027***</td>
<td>-0.021***</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.005)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Fraction Charter Transfers ×100 – IV</td>
<td>-0.018***</td>
<td>-0.041***</td>
<td>-0.034***</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.007)</td>
<td>(0.006)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel B: IHS of Federal Revenues</th>
<th>Child Nutrition Act</th>
<th>Disabilities Act</th>
<th>Other</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(5)</td>
<td>(6)</td>
<td>(7)</td>
</tr>
<tr>
<td>Fraction Charter Transfers ×100 – OLS</td>
<td>-0.033***</td>
<td>-0.065***</td>
<td>-0.010**</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.022)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>Fraction Charter Transfers ×100 – IV</td>
<td>-0.045***</td>
<td>-0.068</td>
<td>-0.003</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.073)</td>
<td>(0.010)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel C: IHS of Local Revenues</th>
<th>Property Tax</th>
<th>School Lunch</th>
<th>Other</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(8)</td>
<td>(9)</td>
<td>(10)</td>
</tr>
<tr>
<td>Fraction Charter Transfers ×100 – OLS</td>
<td>-0.020***</td>
<td>-0.060***</td>
<td>-0.008</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.009)</td>
<td>(0.007)</td>
</tr>
<tr>
<td>Fraction Charter Transfers ×100 – IV</td>
<td>-0.028***</td>
<td>-0.062***</td>
<td>-0.036**</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.016)</td>
<td>(0.015)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel D: Property Tax Decomposition</th>
<th>IHS of Property Value</th>
<th>Total</th>
<th>Residential</th>
<th>Millage</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>(11)</td>
<td>(12)</td>
<td>(13)</td>
</tr>
<tr>
<td>Fraction Charter Transfers ×100 – OLS</td>
<td>-0.023***</td>
<td>-0.017***</td>
<td>-0.002</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.003)</td>
<td>(0.086)</td>
<td></td>
</tr>
<tr>
<td>Fraction Charter Transfers ×100 – IV</td>
<td>-0.025***</td>
<td>-0.026***</td>
<td>-0.219*</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.005)</td>
<td>(0.121)</td>
<td></td>
</tr>
</tbody>
</table>

Notes: N = 11,149 district-year observations. Standard Errors in parentheses are clustered by district. First-stage estimates (and standard errors) for excluded instruments are: Post 1999 \( \tau_{-2} \) \( * \) (Acad. E.) \( \tau_{-2} = 2.306^{***} \) (0.691); Post 2002 \( \tau_{-2} \) \( * \) (Acad. W.) \( \tau_{-2} = 3.573^{***} \) (0.649); and \( t = 1 \) Char. Elig. (Urban 8/21) = 2.471** (0.927). See footnote 27 on page 19 for details on the inverse hyperbolic sine transformation (IHS). The F statistic for excluded instruments is 10.315***. This table reports OLS (see equation (1)) and 2SLS (see equation (2)) estimates for the effect of charter competition on district revenues. The endogenous variable is the fraction of the district’s membership attending charter schools times 100. Each cell provides the result of a separate regression. See Online Table G.2 for the mean of each dependent variable and Online Table G.1 for tests of overidentification. ***, **, and * represent significance at the 1, 5, and 10 percent levels, respectively.
Table 5: Effect of Charter Transfers on Collectively Bargained Contracts

<table>
<thead>
<tr>
<th></th>
<th>Log of Real Salary</th>
<th>Other Contract Outcomes</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Entry</td>
<td>Top</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>t-1 Fraction Charter Transfers ×100 – OLS</td>
<td>-0.004***</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>t-1 Fraction Charter Transfers ×100 – IV</td>
<td>-0.002</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.002)</td>
</tr>
</tbody>
</table>

Notes: N = 13,930 contract observations. Standard Errors are in parentheses and are clustered by district. First-stage estimates (and standard errors) for excluded instruments are: Post 1999τ−3 × 1(ACAD. E.)τ−2 = 3.877*** (1.427); Post 2002τ−3 × 1(ACAD. W.)τ−2 = 3.403*** (0.750); and τ−2 Char. Elig. (Urban 8/21) = 2.481*** (0.859). See footnote 27 on page 19 for details on the inverse hyperbolic sine transformation (IHS). The F statistic for excluded instruments is 10.285***. This table reports OLS (1) and 2SLS (see equation (2)) estimates of the effect of charter competition on negotiated pecuniary and non-pecuniary contract outcomes. Imputed Top in Column (3) is calculated by using the maximum salary from the 15th through 20th imputed payscale estimates as calculated from the procedure detailed in Appendix B and filling in missing values with SERB top-level contract outcomes. The endogenous variable is the fraction of the district’s total enrollment lost to any charter schools, i.e., # transferred / (# in district + # transferred). See Online Table G.2 for the mean of each dependent variable and Online Table G.1 for tests of overidentification. ***, **, and * represent significance at the 1, 5, and 10 percent levels, respectively.
Table 6: Effect of Charter Transfers on District Expenditures

<table>
<thead>
<tr>
<th>Charter Payments (100,000s)</th>
<th>IHS of Expenditure</th>
<th>IHS of Capital Outlays</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Total (Net of Charter Payment)</td>
<td>Instruction</td>
<td>Capital Outlays</td>
</tr>
<tr>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td>New Construction</td>
<td>Other</td>
<td></td>
</tr>
<tr>
<td>(7)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Fraction Charter Transfers ×100 – OLS</th>
<th>1.144***</th>
<th>-0.007***</th>
<th>-0.021***</th>
<th>0.071***</th>
<th>-0.021***</th>
<th>0.143***</th>
<th>0.002</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(0.257)</td>
<td>(0.002)</td>
<td>(0.004)</td>
<td>(0.016)</td>
<td>(0.003)</td>
<td>(0.048)</td>
<td>(0.009)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Fraction Charter Transfers ×100 – IV</th>
<th>1.997***</th>
<th>-0.017***</th>
<th>-0.023***</th>
<th>0.073**</th>
<th>-0.028***</th>
<th>0.113</th>
<th>0.001</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(0.432)</td>
<td>(0.006)</td>
<td>(0.004)</td>
<td>(0.034)</td>
<td>(0.004)</td>
<td>(0.114)</td>
<td>(0.019)</td>
</tr>
</tbody>
</table>

Notes: N= 11,449 district-year observations. Standard Errors in parentheses are clustered by district. See footnote 27 on page 19 for details on the inverse hyperbolic sine transformation (IHS). First-stage estimates (and standard errors) for excluded instruments are: Post 1999−τ∗12 (Acad. E.) = 2.306*** (0.691); Post 2002−τ∗12 (Acad. W.) = 3.573*** (0.649); and t−τ Char. Elig. (Urban 8/21) = 2.471*** (0.927). The F statistic for excluded instruments is 10.315***. This table reports OLS and 2SLS estimates of the effect of charter competition on different forms of teacher mobility. The endogenous variable is the fraction of the district’s membership attending charter schools times 100. Each regression includes district and commute-zone-by-contract-start-year fixed effects. Each Panel and column provide the results of a separate regression. See Online Table G.2 for the mean of each dependent variable and Online Table G.1 for tests of overidentification. ***, **, and * represent significance at the 1, 5, and 10 percent levels, respectively.
Figure 1: Charter Growth

Notes: Data for students attending any charter from 1998-2001 are from CCD payments to charter schools, divided by the baseline amount transferred per student in that year. From 2002 on, charter schools transfers are the sum of student transfers to digital and brick and mortar charters collected from District Foundation Settlement Reports from the ODE. Actual charter counts are the total number of students attending charter designated LEAs from the CCD LEA Universe Survey.
Figure 2: Ohio Charter Entry for 1998, 2001, 2005, and 2011

Notes: Ohio district boundaries are plotted and each district is shaded based on the fraction of potential student enrollment that instead transfers to charters for the given year, \(#\text{Enrolled in District} / #\text{Transferring to Charter}\). See Section 2.2 for a detailed explanation for how the \# of transferring students is calculated.
Figure 3: Charter Entry Timeline for District-Year and Contract Outcomes
Figure 4: Lagged “Academic Watch” Event Study: Various Outcomes

Notes: Each figure presents the effect of receiving an “Academic Watch” rating two years earlier on the given current outcome, estimated from (8) as explained in Section 3.4. Each regression is respectively run on the sample restrictions for the given outcome in Sections 4 through 6.
Figure 5: Full Imputed Salary Schedule Estimation

Notes: The figure presents the OLS/IV estimates of a 0.01 increase in the fraction of potential district enrollment that transfers to charters on imputed salaries for each experience pay scale step using regressions from equations (5)/(6). See Online Appendix B for an explanation of the pay scale step imputation method. Each cell presents the results from a separate regression.
Appendix

A Mechanical Bias for Models with Partially Fixed Dependent Variables

Consider an outcome $y$ that can only vary intermittently. Suppose that if $y$ was able to vary annually, the true model would be given by

$$y_{it} = \beta_0 + \beta_1 x_{it} + \epsilon_{it},$$

but $y$ can only vary intermittently, so instead the econometrician only observes

$$y_{it}^* = \begin{cases} y_{i,t-g} & \text{if } 1(fixed_{it}) = 1 \\ y_{it} & \text{o.w.} \end{cases}$$

where $g$ is the number of periods since the last negotiation for district $i$ during school year $t$. $1(fixed_{it})$ is an indicator variable equal to one during years in which the $y$ value is fixed for the given district. Thus, you can think of the measurement error term, $\nu$ as

$$\nu_{it} = \begin{cases} y_{it} - y_{i,t-g} & \text{if } 1(fixed_{it}) = 1 \\ 0 & \text{o.w.} \end{cases}$$

We can now rewrite (10) as

$$y_{it}^* + \nu_{it} = \beta_0 + \beta_1 x_{it} + \epsilon_{it}$$

$$y_{it}^* + 1(fixed_{it})(y_{it} - y_{i,t-g}) = \beta_0 + \beta_1 x_{it} + \epsilon_{it}$$

$$y_{it}^* = \beta_0 + \beta_1 x_{it} + \epsilon_{it} - 1(fixed_{it})(y_{it} - y_{i,t-g}) \equiv \xi_{it}$$

But now we can rewrite $\xi_{it}$ by substituting back in (10) to get

$$\xi_{it} = \epsilon_{it} - 1(fixed_{it})[\beta_0 + \beta_1 x_{it} + \epsilon_{i,t} - \beta_0 - \beta_1 x_{i,t-g} - \epsilon_{i,t-g}]$$

$$= \epsilon_{it} - 1(fixed_{it})[\beta_1 \Delta y x_{it} + \Delta y \epsilon_{it}]$$
where $\Delta_g z_{it} \equiv z_{it} - z_{i,t-g}$.

Then assessing consistency we see that

$$\text{plim} \hat{\beta}_1 = \beta_1 + \frac{\text{Cov}(x_{it}, \xi_{it})}{\text{Var}(x_{it})} \cdot \frac{\sigma_{1}^2}{\sigma_{x}^2}$$

$$= \beta_1 + \frac{\text{Cov}(x_{it}, \epsilon_{it} - \mathbb{1}(\text{fixed}_{it}) [\beta_1 \Delta_g x_{it} + \Delta_g \epsilon_{it}])}{\sigma_{x}^2}$$

$$= \beta_1 + \frac{\text{Cov}(x_{it}, \epsilon_{it})}{\sigma_{x}^2} - \frac{\text{Cov}(x_{it}, \mathbb{1}(\text{fixed}_{it}) [\beta_1 \Delta_g x_{it}])}{\sigma_{x}^2}$$

$$\to 0$$

$$- \frac{\text{Cov}(x_{it}, \mathbb{1}(\text{fixed}_{it}) [\Delta_g \epsilon_{it}])}{\sigma_{x}^2} \to 0$$

(11)

where I assume that $\mathbb{1}(\text{fixed}_{it})$ is independent of $x_{i,t}, x_{i,t-g}, \epsilon_{it}$ and $\epsilon_{i,t-g}$ so that $\mathbb{1}(\text{fixed}_{it})$ can be factored out. For the final term to go to zero, we must further suppose that $x_{i,t}$ is independent of all lagged errors. Then under these assumptions

$$\frac{\text{Cov}(x_{it}, \mathbb{1}(\text{fixed}_{it}) [\Delta_g \epsilon_{it}])}{\sigma_{x}^2} = \frac{\text{Cov}(x_{it}, \mathbb{1}(\text{fixed}_{it}) [\Delta_g x_{it}])}{\sigma_{x}^2}$$

Thus, (11) can be rewritten as

$$\text{plim} \hat{\beta}_1 = \beta_1 - \frac{\text{Cov}(x_{it}, \mathbb{1}(\text{fixed}_{it}) [\Delta_g x_{it}])}{\sigma_{x}^2}$$

$$= \beta_1 \left( 1 - \frac{\text{Cov}(x_{it}, \mathbb{1}(\text{fixed}_{it}) [\Delta_g x_{it}])}{\sigma_{x}^2} \right)$$

This expression can be simplified. Notice that

$$\text{Cov}(X,Y) = \mathbb{E}(XY) - \mathbb{E}(X)\mathbb{E}(Y)$$

$$\text{Cov}(x_{it}, \mathbb{1}(\text{fixed}_{it}) x_{it}) = \mathbb{E}(x_{it} \cdot \mathbb{1}(\text{fixed}_{it}) x_{it})$$

$$- \mathbb{E}(x_{it}) \mathbb{E}(\mathbb{1}(\text{fixed}_{it}) x_{it})$$

$$= \delta \mathbb{E}(x_{it}^2) - \delta \mathbb{E}(x_{it})^2 = \delta \sigma_{x}^2$$
\[
\text{Cov}(x_{it}, 1(fixed_{it})x_{i,t-g}) = \mathbb{E} (x_{it} \cdot 1(fixed_{it})x_{i,t-g}) \\
- \mathbb{E} (x_{it}) \mathbb{E} (1(fixed_{it})x_{i,t-g}) \\
= \delta \mathbb{E}(x_{it} \cdot x_{i,t-g}) - \delta \mathbb{E}(x_{it}) \mathbb{E}(x_{i,t-g}) \\
= \delta \left[ \mathbb{E}(x_{it} \cdot x_{i,t-g}) - \mathbb{E}(x_{it}) \mathbb{E}(x_{i,t-g}) \right] = \delta \cdot \sigma_{\Delta x}
\]

where I denote \( \delta \equiv \mathbb{E}(1(fixed_{it})) \), i.e., the fraction of observations that are fixed in \( t \).

Now I will show that \( \frac{\text{Cov}(x_{it}, 1(fixed_{it})[\Delta_g x_{it}])}{\sigma_x^2} \in [0, 2] \).

\[
\frac{\text{Cov}(x_{it}, 1(fixed_{it})[\Delta_g x_{it}])}{\sigma_x^2} = \frac{\text{Cov}(x_{it}, 1(fixed_{it})x_{it})}{\sigma_x^2} - \frac{\text{Cov}(x_{it}, 1(fixed_{it})x_{i,t-g})}{\sigma_x^2}
\]

\[
= \delta \frac{\sigma_x^2}{\sigma_x^2} - \frac{\delta \cdot \sigma_{\Delta x}}{\sigma_x^2} \\
= \delta \left( 1 - \frac{\sigma_{\Delta x}}{\sigma_x^2} \right) \\
= \delta \left( 1 - \frac{\sigma_{\Delta x}}{\sigma_x^2} \right) \rho_x(g) \in [0, 2] \\
\]

where \( \rho_x(g) \) is the autocorrelation function for \( g \) lags and is obtained by assuming stationarity in \( x \).\(^{46}\)

Finally, we see that

\[
\text{plim} \ \hat{\beta} = \beta_1 [1 - \delta (1 - \rho_x(g))]
\]

Thus, the sign of the bias is dependent on the sign of the autocorrelation function \( \rho_x(g) \).

### A.1 Monte Carlo Simulations

In this section, I provide Monte Carlo Evidence of the bias formula in (12). To do this, I generate 3 sets of data with varying levels of serial correlation. For the \( \rho_x(3) = 1 \) case, \( x \) is a 10,000 observation array of sequential positive integers. For \( \rho_x(3) = 0.85 \), \( x \) follows an AR(1) process yielding the given serial correlation. For \( \rho_x(3) = 0 \), \( x \) is drawn from a uniform distribution with

\(^{46}\)The autocorrelation function provides the correlation coefficient for a given variable between two different periods of time. Refer to Okui (2014) for estimators of autocovariance and autocorrelations for panel data with individual and time effects.
values from 0 to 100. For each of these $x$ variables, I allow $y$ only to vary every third observation (i.e., $\delta = 0.66\bar{7}$). For the observations when $y$ can vary, I set $y$ using

\begin{equation}
    y_t = 10 + 1 \cdot x_t + \epsilon_t
\end{equation}

where $\epsilon_t \sim N(0, 1)$. In this data generating process $\beta_{\text{true}} = 1$ is the benchmark for each bias test. For all values of $y$ that are fixed, $y$ is set to equal the most recent $y$ value that could vary.

I then calculate the theoretical bias predicted by (12) for these three scenarios as well as estimate the empirical bias by regressing $y$ on $x$ regardless of $y$’s fixed status and subtracting the true $\beta$ from my estimate. Finally, I also estimate the same regression, but omit observations with fixed $y$ values. I repeat this exercise 1,000 times and calculate the mean and standard deviation of each statistic.

Table 1 displays the results from these simulations. Each rows presents the results for the varying levels of serial correlation in $x$. In columns 1 and 2, I present the mean and standard deviation of 1,000 simulations of the calculated theoretical and estimated bias respectively. column 3, presents the absolute value of the difference between each predicted and estimated bias. Column 4, presents the estimated bias for the regression that omits observations with fixed $y$ values.

As expected, when the $x$ values are perfectly serially correlated, there is no bias from estimating on the full or restricted samples. However, with strong, yet imperfect positive serial correlation, I predict about a 10 percent attenuation that is confirmed in the actual estimation. When $x$ values have no serial correlation, the bias matches the extent to which $y$ values are fixed, a $66.\bar{7}$ percent attenuation in this case. Across each specification, the bias is completely mitigated by regressing only on years for which outcomes can vary. In my setting, $\rho_x(g) \approx 0.9$ and $\delta \approx 0.66\bar{7}$ meaning that naive estimates on annual data are theoretically predicted to be attenuated by about 7 percent.
Table 1: Simulations to test Mechanical Bias

<table>
<thead>
<tr>
<th>$\rho_x(3)$</th>
<th>Theoretical Bias (1)</th>
<th>Estimated Bias (2)</th>
<th>Abs(Difference) (3)</th>
<th>Negotiation Years Only Estimated Bias (4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>0.85</td>
<td>-0.097</td>
<td>-0.097</td>
<td>0.006</td>
<td>-0.000</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.005)</td>
<td>(0.005)</td>
<td>(0.007)</td>
</tr>
<tr>
<td>0</td>
<td>-0.667</td>
<td>-0.667</td>
<td>0.003</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.008)</td>
<td>(0.003)</td>
<td>(0.001)</td>
</tr>
</tbody>
</table>

Notes: N= 10,000 simulated observations. $\delta = 0.667$. The theoretical bias, estimated bias, and absolute value difference in biases are calculated 1,000 times for the given autocorrelation values and the mean and standard deviations of these simulations are reported in columns 1-3. In all regressions, the true parameter value was unity. For $\rho_x(3) = 1$, $x$ is simply an array of sequential positive integers. For $\rho_x(3) = .85$, $x$ follows an AR(1) process that yields the given serial correlation. For $\rho_x(3) = 0$, $x \sim U[0, 100]$. For all regressions, $y$ values are calculated as $y = 10 + 1 \cdot x + \epsilon$, where $\epsilon \sim N(0, 1)$. Column 4 presents the estimated bias for regressions run only on observations for which the outcome can vary (i.e., contract negotiation years).