

Inclusive and Accessible Education: Three Essays on Child Care,
Kindergarten and K-12 Education

by

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A Dissertation Presented in Partial Fulfillment
of the Requirements for the Degree
Doctor of Philosophy

Approved April 2022 by the
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ARIZONA STATE UNIVERSITY

May 2022

ABSTRACT

This dissertation examines how a variety of demographic factors and policy instruments interact and affect the extent to which education remains inclusive and accessible to the needs of diverse students and families. The dissertation does so in a three-essay format, with each essay respectively aimed at three different stages of education (i.e., Early Childhood Education and Care (ECEC), Kindergarten, and K-12 schools) within the U.S. context. The first essay examines regulations in the context of child care market. Regulations are often justified as a mechanism to enhance quality of child care, but unintended labor market consequences could instead reduce the quantity and quality of services provided. Using a continuous and binary treatment difference-in-differences strategy that exploits unprecedented variation in child care regulations during the period surrounding COVID-19, this study finds that regulations reduced the demand for child care labor and motivated providers to substitute away from high-skilled labor. The second essay examines Arizona's full-day kindergarten expansion between 2004-2010. This expansion implicitly acted as a subsidy for child care, by reducing the required number of after-school care hours. Using a difference-in-differences framework, this study finds that the availability of full-day kindergarten increased employment among some subgroups of mothers with age-eligible children. Finally, the third essay is set in the context of immigration and K-12 demographics. The theory of native flight suggests that U.S. born subpopulations move away from neighborhoods which receive higher numbers of immigrants. Merging school level data with the U.S. census data over the years 2005-2019, I use an instrumental variable approach to find that U.S. born students show heterogeneous patterns of flight in response to immigration. The dissertation showcases the relevance of certain policy

variations and demographic characteristics in the context of affordability, diversity, and access in education.

DEDICATION

Dedicated to my family, for their unwavering support and patience. My parents sacrificed a lot for me to be here today. My spouse has been so patient and supportive.

ACKNOWLEDGMENTS

I have been always grateful for the support of my doctoral advisor, Dr. Herbst for his dedication and keen interest in my academic success. Ever since I joined the program, he has been instrumental in shaping my research ideology. I learnt a lot from Dr. Herbst, while working on this dissertation as well as other projects. He has been extremely kind, super-patient and really supportive of my career. He has been available and very approachable for the entire period of my graduate study. He gave me detailed and relevant feedback on every piece that I wrote, every conference paper, and every presentation. Above all, he encouraged me during difficult times, and ensured that I made through them, battle-hardened.

My committee members, Dr. Maroulis and Dr. Howell provided valuable feedback on my work at important junctures. Dr. Maroulis's precise and focused comments on my work before and after my prospectus really improved these essays. Dr. Howell has always been supportive. His feedback and comments on my papers were the most comprehensive comments that I had on my work, all in one place. Those served as a checklist for me to make sure that I keep track of my research design as well as limitations. Both Dr. Maroulis and Dr. Howell have been approachable as committee members, and have always provided insightful comments and feedback.

A number of colleagues and faculty provided comments at various presentations, conferences and workshops. Dr. Makridis provided access to labor market data for one of my papers. I would also like to acknowledge the faculty and staff at the School of Public Affairs for their role in shaping and building our research capabilities. The administrative staff at School of Public Affairs have always been so kind and there for us. My friends and colleagues have also been super supportive throughout this period.

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Chapter 1

INTRODUCTION

Education would ideally be inclusive and accessible to diverse students and families. A variety of demographic factors and policy instruments however interact with the education system and contribute towards determining the extent to which these education systems remain inclusive and accessible. This three-essay dissertation examines the impact of these demographic factors and policy instruments at three stages of education (i.e., Early Childhood Education and Care (ECEC), Kindergarten, and K-12 schools) within the U.S. context.

Public policies and regulations have always been important but they gain center attention during the time of economic challenges. In particular, policies and regulations that are relevant to the entire population of the country deserve greater attention, many of these policies can potentially contribute to the short and long-run economic outlook. In the context of early education, child care regulations have recently gained more attention in the context of Covid-19. Regulations in the child care market have existed for a long time (Blau and Hagy, 1998; Blau, 2003), and can have a number of macroeconomic consequences such as the availability, affordability, and quality of child care (Blau, 2001, 2007; Hotz and Xiao, 2011). During the Covid-19 pandemic, states introduced or altered existing child care regulations at an unprecedented rate of variation.

In this context, the first essay of my dissertation examines regulations in the context of child care market. Minimum quality regulations are often justified in the child care market because of the presence of information frictions between parents

and providers. However, regulations can also have unintended consequences for the quantity and quality of services provided. In this paper, I merge new data on states' child care regulations for maximum classroom group sizes and child-to-staff ratios with the universe of online job postings to study the impact of regulations on the demand for and characteristics of child care labor. My identification strategy exploits the unprecedented variation in regulatory reform during the COVID-19 pandemic, relying on changes both within states over time and across children's age groups. Using a binary and continuous treatment difference-in-differences model, I find robust evidence that these regulations reduce the number of child care job postings and encourage providers to substitute away from higher-skilled postings, thereby increasing the number of positions that are out-of-compliance with state law. In sum, the results imply that child care regulations may reduce the demand for child care labor, while simultaneously altering the composition of the workforce.

The second essay of this dissertation is institutionally contextualized in the next stage of education which is kindergarten. Existing literature has made it evident that investment in kindergarten education is associated with long-run economic benefits. In particular, full-day kindergarten attendance has been linked with higher student achievement in the subsequent years (Cannon et al., 2006; Friesen et al., 2022), while the availability of full-day kindergarten is associated with higher labor force participation rates for mothers (Cascio, 2009; Dhuey et al., 2021).

In the second essay, I examine publicly funded kindergarten within the state of Arizona. Between the school years 2004-05 and 2009-2010, the state of Arizona experimented with kindergarten expansion, increasing public funding for kindergarten from half-day to full-day. This funding for full-day expansion of kindergarten was however withdrawn after 2009-2010. One can draw parallels across full-day kindergarten and

a child care subsidy as publicly funded kindergarten contributes to lower overall costs of child care by reducing the required number of after-school care hours. Using monthly data for the mothers of age-eligible children from the Current Population Survey (CPS) across U.S. states over the years 2000 to 2010, the paper relies on a difference-in-differences approach to estimate the impact of full-day kindergarten on maternal labor supply on the intensive and extensive margins, while paying specific attention to heterogeneity across race, marital status, age and education levels of mothers. Employment increases among different subgroups of mothers, both on the extensive as well as the intensive margin. Relying on the fact that schools are usually not open during summer months of June through August, triple differences estimates are run as a robustness test.

Finally, the third essay is set in the context of K-12 demographics. Over the past few decades, the U.S. has received an increasing number of immigrants (Migration Policy Institute, 2022a). Immigration poses challenges relating to diversity, inclusion and cohesion in education systems, including K-12 education (Grigorenko and Takanishi, 2010). In the context of immigration, the theory of native flight argues that U.S. born populations move away from neighborhoods when an increasing number of immigrants move in (Frey, 1979; Wurdock, 1981).

In this third essay of my dissertation, I test the theory of native flight in the context of K-12 school enrollments, by examining the impact of immigrant influx on public, private and public charter school enrollments, differentiating across U.S. born races and ethnicities. To do so, I rely on data from public and private school files from the Common Core of Data (CCD) and immigration data from the American Community Survey (ACS) over the years 2005-2019. Using an instrumental variables approach (2SLS) to address potentially endogenous settlement patterns

of immigrants into Metropolitan Statistical Areas (MSAs), I find that students of U.S. born race/ethnicities display heterogeneous enrollment responses to immigrant influx. Shares of White students and Black students in public non-charter schools decrease significantly in response to an increase in immigration. At the same time, the shares of Hispanic students and Asian students increase significantly in public non-charter schools. Analogous estimates for native flight into private schools lend further credence to public school estimates. The share of White students in private schools increases significantly in response to immigration. The share of Black students decreases across private schools as well, signalling a crowding-out effect. There are two key implications. First, significant White flight from the public-school system still exists over the past decade and a half. Second, while the increasing shares of White students in private schools might compensate for White students leaving the public school system, the shares of Black students are dropping across private and public schools.

Together, the three empirical essays of this dissertation combine to examine the interaction of demographic variables and policy instruments with macroeconomic and socio-demographic factors at three different stages of education. The first essay examines the demand for child care labor in the context of regulations. The second essay examines the impact of another policy instrument, i.e. the full-day expansion of public kindergarten on labor force participation for mothers with diverse needs and skills. The third essay links immigration – an example of large-scale demographic change – with the response to increased immigrant diversity in K-12 schools. The dissertation thus showcases how tangible and traceable policies and regulations interact with macroeconomic variations and demographic environments to influence the availability and characteristics of education.

Chapter 2

MINIMUM QUALITY REGULATIONS AND THE DEMAND FOR CHILD CARE LABOR

2.1 Introduction

There is a large literature on the economics of licensing and minimum quality standards enacted in a variety of industries, including child care, for the purpose of increasing quality and ensuring the safety of market-provided goods and services (Leland, 1979). The key rationale for regulating the child care market is to mitigate the negative consequences of informational frictions between parents and providers, in which the former is poorly informed about the quality of care received by children. In such cases, regulations may be necessary to ensure that a minimally acceptable level of quality exists throughout the market. In addition, regulations may be desirable if any quality improvements translate into health and developmental benefits for children.

However, the benefits of child care regulations must be weighed against their potential costs. Indeed, the imposition of minimum quality standards may reduce the quantity of child care services available, distort providers' input decisions, and lead to higher prices without improving quality (Shapiro, 1986; Blau, 2001). Over the last few decades, a large empirical literature has assessed the predictions of the canonical model of regulations in the center-based child care market.¹ This work shows that regulations reduce establishment-level supply and parental utilization of

¹See, for example, Chipty and Witte (1999), Gormley (1991), Chipty (1995), Blau (2003), Blau and Currie (2006), Blau (2007), Boyd-Swan and Herbst (2018), and Hotz and Xiao (2011)

child care, but do not influence prices or quality. Furthermore, tougher regulations induce higher rates of non-compliance, in part because they encourage providers to substitute between inputs.

Nevertheless, estimating the impact of child care regulations is challenging because much of the variation exploited by previous studies is cross-sectional, raising concerns over whether the results are biased from unobserved state-level confounders. Furthermore, the studies relying on within-state over-time variation in regulatory policy are also limited because changes to regulations are fairly infrequent and restricted to a subset of states.² Thus, the estimates in these studies are generated using only a modest amount of variation from a small number of states, increasing the possibility that the results are either spurious or of limited generalizability.

In this paper, I estimate dichotomous and continuous treatment difference-in-differences models to study how the introduction and increased stringency of regulations in the market for center-based child care influence the demand for teacher labor, characteristics of the workforce, and compliance with other regulations. My research covers the period surrounding the onset of the COVID-19 pandemic, when nearly all states enacted dramatic changes to the regulatory environment in an attempt to mitigate the spread of the virus. My study focuses on revisions to maximum classroom group sizes and child-to-staff ratios, arguably two of the most important labor-related regulations. During the first nine months of 2020, which coincides with this study's time frame, 31 states reformed their group size regulations and 18 states reformed their child-to-staff ratios, with most of these states making multiple changes over

²For example, a study by Blau (2003) on group sizes and child-to-staff ratios over a 13-year period finds that such regulations changed only 0.44 times per state, on average. Similarly, Hotz and Xiao (2011) report that only nine states altered their child-to-staff ratios over their 10-year study period.

this period. At the height of states’ regulatory stringency (lasting from late-May to early-June), approximately 85% of states had a group size regulation in place (up from 70% prior to the pandemic), the average group size fell below 14 children (from 20), and the average child-to-staff ratio dropped to 10:1 (from 12:1).

Although I rely on within-state over-time changes in child care regulations, this study exploits a largely unused source of policy variation: the differential regulatory stringency across children’s age groups. Indeed, classrooms serving younger children in center-based settings are exposed to tougher standards than their older counterparts. For example, prior to the pandemic 79% of states regulated group sizes for infant/toddler classrooms, compared to 63% for school-age classrooms. Furthermore, maximum group sizes averaged 11 children for infants/toddlers, while those at the school-age level averaged 31 children. Together, these two sources of policy variation – changes over time within states as well as across children’s age groups within states – provides me with the flexibility to test different assumptions about the nature of the unobservables.³

Data on these regulations are matched to the universe of online child care teacher job postings in state $\times$ day $\times$ child age group cells over the period January 1 to September 30, 2020. These data are provided by the labor analytics company EMSI, which scrapes and organizes job postings from all of the major online job banks (e.g., Indeed, Glassdoor, and LinkedIn). I conduct extensive keyword searches within the postings to classify each according to the sector in which the position is based (center-based child care, Head Start, or pre-k), the rank of the teacher (assistant or lead), the child age group to be served (infants/toddlers, preschool-age, or school-age),

³To my knowledge, the only other study to exploit variation in regulations across children’s age groups is Blau (2007). However, his study relies on data from only four states, and it does not exploit temporal variation in regulatory stringency (i.e., it is a cross-sectional study.)

and the level of education required. Therefore, these data allow me to estimate the impact of regulations on several dimensions of labor demand during the initial stage of the teacher hiring process.

Although my study represents a unique opportunity to examine a period in which most states enacted meaningful changes to their regulatory environment, it is nevertheless difficult to isolate the causal effect of regulations in this context, given the large number of economic and pandemic-related factors that changed contemporaneously with the regulations. For example, states enacted a number of policies—ranging from stay-at-home orders to mandatory school and business closures—aimed at mitigating the spread of COVID-19. These policies, coupled with the recession-driven shock to the labor market, likely influenced the demand for child care labor independent of any regulatory effects. Thus, my study takes a number of steps to mitigate the influence of these time-varying state confounders. First, in addition to incorporating two-way fixed effects for state, time, and child age groups, my baseline model controls for 25 state characteristics. These variables account not only for the severity of the pandemic and states’ policy responses to it, but also changes in key child care supply and demand, labor market, and political characteristics that may influence providers’ hiring behavior. Second, my baseline model includes state-specific time trends to control for any component of the unobservables that trends within states. Third, whenever possible I exploit the panel structure of the data (organized into state $\times$ day $\times$ age group cells) by incorporating more restrictive sets of two-way fixed effects, including a model with state $\times$ day effects, which leverages only between-age-group (i.e., within-state and -day) variation in regulatory stringency. Finally, I implement a falsification test in which job postings for child care teachers are replaced with those for Head Start and pre-k positions. Given that the regulatory

changes I study are aimed at center-based child care services, I would not expect them to influence hiring behavior at Head Start and pre-k programs, which are not exposed to these reforms.

My results are quite robust to changes in the specification. In the most stringent model, in which I control for all shocks that are common to a state over time, I find that the introduction of a group size regulation (for providers not already exposed to such a requirement) reduces the number of child care job postings by 9.2% per day, which is indicative of a drop in labor demand. In addition, the introduction of a group size regulation increases the demand for lead teachers, but reduces the demand for those with a bachelor's degree and increases the odds that a posting's teacher education requirement is out-of-compliance with the corresponding state education regulation. In contrast, increasing the stringency of group sizes (for providers already exposed to such a requirement) leads to a small increase in labor demand, but does not influence the demand for specific teacher characteristics. Finally, I show that increasing the stringency of staff-to-child ratios (for providers already exposed to such a requirement) reduces the demand for labor, encourages providers to hire fewer lead teachers and those with a bachelor's degree, and increases rates of non-compliance with states' teacher education regulations. In an encouraging sign for the credibility of my results, I show that none of the regulations are associated with labor demand in the market for Head Start and pre-k teachers.

Together, these results indicate that child care regulations have heterogeneous effects on providers. Those operating in states that did not regulate group sizes prior to the pandemic—but began regulating them for the first time after the pandemic—experienced significant challenges adjusting to the new regulatory environment. Such providers not only hired fewer teachers, but sought to downskill the teacher positions

being advertised, at least as it relates to education. Conversely, providers in states already regulating group sizes prior to the pandemic appear to have adapted well to the post-pandemic period, when these regulations became tougher. Indeed, increasing the strictness of group sizes altered virtually no aspect of providers' hiring behavior. However, changes to child-to-staff ratios were costly for providers, leading to lower demand for teachers, a decrease in the skill requirements for the job, and an increase in non-compliance with other regulations.

Insofar as these new regulatory changes persist after the pandemic, my results shed light on how the child care labor market may continue to operate. Indeed, although Panel A of Figure 2.1 shows that child care job postings had already begun to return to trend levels toward the end of 2020, Panel B shows that the share of job postings requiring a bachelor's degree has fallen considerably, thereby influencing the share of postings that comply with the corresponding state education requirements (Panel C). Although educational attainment may be a noisy proxy for teacher quality, these descriptive patterns nonetheless point to the limits of regulations as a policy to improve child care quality (Boyd-Swan and Herbst, 2018).

This paper builds on an empirical literature in labor economics that studies the impact of regulations on the child care and maternal labor markets. In particular, previous work focuses on child care supply (e.g., Gormley, 1991; Hotz and Xiao, 2011) and quality (Blau, 2007; Hotz and Xiao, 2011; Boyd-Swan and Herbst, 2019), families' use of and expenditures on child care (Blau, 2003; Hotz and Kilburn, 1994), and parental employment (Blau, 2003). Far less attention has been given to how regulations influence the demand for and characteristics of child care labor. In fact, Blau (2007) is the only paper to my knowledge that examines these outcomes. My paper contributes to this literature in several ways. First, I introduce a new data

source—the universe of online job postings—to study how child care providers respond to regulations. These data, which reflect providers’ contemporaneous preferences for teacher characteristics, are well-suited for assessing not just the labor-demand response to child care regulations, but also whether providers trade-off certain teacher characteristics as regulations become tougher. Second, as previously mentioned, my study period contains substantially more variation in regulatory policy than that exploited in previous studies. In addition, the magnitude of the changes are sizable, so my parameter estimates reflect meaningful, not marginal, policy changes. Finally, my identification strategy exploits not just within-state over-time variation regulations, but also between child-age-group (within states and days) variation, allowing us to control for all time-varying shocks within states.

This paper also contributes to a literature on the effects of recessions generally and the pandemic specifically on the child care market. For example, Brown and Herbst (2021) examine how the supply and quality of child care varies over the business cycle. Other papers focus on the pandemic period, estimating changes in the supply of child care in specific states (e.g., Bryson, 2020; Sonnier-Netto et al., 2020). Similarly, Ali et al. (2021) show that states’ pandemic containment policies, specifically stay-at-home orders, reduced the demand for child care labor. Finally, a parallel body of work assesses how the pandemic influenced teachers’ caregiving experiences, routines, and mental health (Delap et al., 2021; Carr, 2020), as well as parents’ ability to manage their new (in-home) caregiving routines in tandem with changes to employment schedules (Del Boca et al., 2020).

The structure of the paper is as follows. Section 2 provides a summary of the regulatory landscape in the U.S. child care market. Section 3 introduces the data on child care teacher job postings along with newly collected data on child care regulations

across age groups and states. Section 4 describes the identification strategy. Section 5 presents the main results. Section 6 examines several dimensions of heterogeneity and robustness. Section 7 concludes with a summary of policy implications.

2.2 The Child Care Regulatory Landscape

Child care in the U.S. is regulated at the state-level, with the goal of mitigating the risk of harm to young children from being exposed to low-quality providers. The risks targeted by regulations range from cognitive and social-emotional impairment to physical injury and disease-spreading (Blau, 2001). Regulations require providers to be licensed and to meet a series of requirements related to the physical attributes of the setting. Specifically, regulations can be characterized as governing two broad areas of the child care environment: health/safety and labor.

Those related to health and safety include immunizations, food safety, ventilation and lighting, and playground equipment, while the labor-related regulations generally include maximum classroom group sizes, child-to-staff ratios, and the experience and education of program staff. Such requirements vary extensively by state, program setting (i.e., center- versus home-based providers), children’s age group, and type of staff. For example, infant and toddler classrooms are generally subject to tougher requirements on group sizes and ratios than those for older children, and lead teachers are required to have more experience and education than assistant teachers.

Table 2.1 sheds light on the extent of cross-state variation in the center-based child care regulatory environment. Specifically, it shows the maximum group sizes and child-to-staff ratios for three-year-olds as well as the education requirements for assistant teachers, lead teachers, and directors as of January 2020. Fourteen states did not

regulate group sizes, and among those that did, the requirement varies substantially, ranging between eight and 30 children. Child-to-staff ratios were regulated in all but one state (Idaho), with the ratios ranging between seven and 15 children. There is similar variation in staff members' education requirements. Forty states either did not have an explicit education requirement or mandated only a high school diploma (or less) for assistant teachers, while six states required some college credits or a Child Development Associate (CDA) credential. As for lead teachers, all but nine states had an education requirement, and among those that did, the requirement varied from a high school diploma (17 states) to a completed associate's or bachelor's degree (three states). Finally, all but one state (Idaho) regulated education levels for program directors, with most states requiring either college credits or a completed college degree, although nine states required no more than a high school diploma.

Regulations are justified on the basis that they mitigate information asymmetries in the child care market where parents are poorly informed about the quality of care received by children. Parents may be poorly informed because they lack the resources to assess program quality or they cannot efficiently monitor staff. Thus, providers have an incentive to produce lower-quality services by reducing health and safety investments or hiring less productive workers than would be the case if parents were perfectly informed. Regulations may therefore be necessary because they ensure that a minimally acceptable level of quality exists throughout the market, primarily by forcing low-quality providers to improve or exit the market. However, even if parents were perfectly informed, regulations may be desirable if any quality improvements translate into health and developmental benefits for children.

The theoretical predictions from models of minimum quality standards depend crucially on whether child care providers engage in input substitution, whether the

regulations are binding, and whether consumers are willing to pay for a higher price for regulated care. On the one hand, if regulations are binding, then providers face a higher cost of providing child care. Under a pass-through to the consumer, higher prices reduce the quantity of child care demanded, reduce the supply of such care, and cause a reduction in parental labor supply. On the other hand, if regulations raise the quality of child care, and consumers recognize and sufficiently value the increase, then the demand for and supply of child care can rise and lead to an increase in parental labor supply. Given that regulations do not dictate the level of quality per se—rather, they influence the measurable inputs to the production of quality—such requirements may distort provider behavior regarding the deployment of these inputs. For example, to comply with a tougher regulation on group sizes (leading to smaller classrooms), a provider may respond by hiring more teachers but favoring those with lower levels of education. Such input substitution means that program quality may not increase, and could in fact decrease.

If regulations are not binding on many child care providers, it is unlikely they would have a large impact on the market. For example, if a provider would chose a maximum group size of 15 in the absence of a regulation, then requiring group sizes to be 20 would not influence its behavior. The larger the number of providers that face a “binding regulation,” the more that regulations will influence supply and prices within the market. Unfortunately, whether a regulation is binding has received only scant attention by researchers. Blau (2001) compares the observed child-to-staff ratios within center-based classrooms with the relevant state regulation and finds that most providers exceed (i.e., perform better than) the state ratio requirement. Boyd-Swan and Herbst (2018) also compare the experience and education requirements posted in

online child care job advertisements with the corresponding state regulations, again finding that most providers are willing to exceed the state requirements.

Although this work suggests that regulations are not likely to be binding for many providers, some caution is warranted, particularly as it relates to the current study. Recall that this study examines the period surrounding the COVID-19 pandemic, during which states enacted unprecedented changes to their child care regulations. The magnitude of such changes, which will be detailed in a forthcoming section, likely means that a much larger share of providers face binding regulations than before the pandemic and, as a result, the new policy landscape may have forced providers to alter their hiring and human capital decisions more dramatically than in previous years.

Finally, if parents are unwilling or unable to pay a higher price for regulated child care, perhaps because they have weak preferences for quality or because they cannot afford higher-quality services, then the demand for regulated care will unambiguously fall, thereby causing decreased demand for the regulated inputs as well as a reduction in supply. The evidence suggests that the demand for child care services and for quality are moderately sensitive to prices (Blau and Hagy, 1998), as are parental employment decisions (Anderson and Levine, 2000; Herbst, 2010b), again implying a limited scope for regulation-effects. However, these price sensitivities may have increased significantly during the pandemic. For example, while parents may recognize the importance of a clean and safe child care environment, their ability to purchase such care during the pandemic—a period of rapid job loss—may be adversely affected. Moreover, as more information is now available about the relatively low risk of COVID-19 transmission among young children (Rothwell et al., 2021), parents may place a low

valuation on these regulations. Insofar as any of these hypotheses hold, the demand for and quality of child care labor will fall as regulations become tougher.

2.3 Data and Measurement

2.3.1 Labor Demand for Child Care

Data on the demand for and characteristics of child care labor come from EMSI, an analytics company that scrapes every job that is posted across online job banks (e.g., Indeed, Glassdoor, and LinkedIn). From this database, I retrieve the universe of advertisements for center-based child care teacher positions, including those in private for- and non-profit centers and publicly-funded programs.⁴ All job postings in the EMSI database include detailed information on the characteristics of the position, such as its name and title, the name of the hiring firm, and geographic identifiers for the city, county and state of the job location. Each job posting also lists the required skills and education for the position as well as its salary structure. Finally, posted positions are classified as part- or full-time jobs.

Relying on information in each job posting, I employ keyword search methods to classify all child care job postings in a variety of ways. Specifically, I search within the “position title” field to classify postings according to the sector in which the job is based (center-based child care, Head Start or pre-k), the level/type of teacher position (assistant or lead teacher), and the child age group with which the teacher will work (infants and toddlers, preschool-age children, or school-age children). I define infants

⁴In the EMSI database, child care job postings are a subset of all education job postings identified with the two-digit Standard Occupational Classification Code (SOC) 25.

and toddlers as children ages 0 to 2, preschool-age children as those ages 3 to 5, and school-age children as those ages 6 to 10. For example, I identify pre-k positions through the search terms “Pre-K,” “Pre-Kindergarten,” and “UPK” (among many others), while Head Start positions are found primarily by searching “Head Start” and “Early Head Start.” Similarly, search terms such as “Infant,” “1’s,” and “2yo” are used to identify infant/toddler teacher positions, while terms like “preschool,” “3’s,” and “4 Year Old” are used to identify preschool-age positions.⁵

I use these job postings data to examine the impact of regulations on four key outcomes. I begin by studying the demand for child care labor, defined as the total number of child care job postings. Specifically, I collapse the data into state $\times$ day $\times$ age group cells, counting the number of job postings in each cell. The data therefore consist of 41,922 observations (51 states $\times$ 274 days $\times$ 3 child age groups) over the period January 1, 2020 to September 30, 2020.⁶ One might be concerned that job postings (vacancies) are not a good proxy for actual employment levels. While Ali

⁵It is important to note a few other characteristics of the job postings. First, the postings data do not cover the home-based child care market, nor employment opportunities for au pairs or babysitters. Thus, the current study provides evidence on the formal, center-based child care market. Second, in a small number of cases, multiple positions are advertised within the same job posting (e.g., an infant/toddler teacher as well as a preschool-age teacher). In such cases, I code a given posting to be included in as many categories as is relevant, but I conduct a number specification checks to ensure that my results are not sensitive to a particular decision about how to treat the posting in the analysis. For example, although I code job postings for infant/toddler and preschool-age teachers in both child age group categories, I conduct separate analyses that treat these postings as either for infant/toddler or preschool-age teachers (but not both), as included in both categories, and as excluded entirely from the analysis. Results reported here are based on analyses that treat these postings as appearing in multiple analyses, although the results are not sensitive to the choice of categorization.

⁶Given that the outcome is a count variable, it is not surprising that there is a mass of data points at zero. Specifically, approximately two-thirds of the state $\times$ day $\times$ age group cells include zero job postings. Although the main analysis measures the outcome variable as the log number of job postings, I conduct a number of specification checks to ensure robustness. I estimate an OLS regression on the (non-logged) number of postings as well as Poisson and negative binomial regressions. Results from these alternative analyses are quite similar to those reported in the paper.

et al. (2021) show that there is a 0.91 correlation between log employment and log job posting levels, suggesting that the trends match each other quite closely, the focus in this paper is on the demand for child care labor. While the composition of actual employees on the job can alter the demand (e.g., an endogenous turnover hypothesis), I view job postings as a mechanism for influencing the composition of supply.

I then turn my attention to studying the characteristics of the teacher job postings, thereby allowing us to shed light on the degree of skill-based input substitution. First, I estimate the demand for lead teachers as compared to the demand for assistant teachers. To do so, I classify each job posting as either a lead or an assistant teacher posting, as mentioned above, and create a binary indicator that equals one if a given posting is for a lead teacher. Second, I evaluate whether child care providers hire for similar roles, but alter the skill level required for the positions. To do so, I exploit information in the job postings data that lists the minimum education level required for each position. I focus on whether a given child care job posting lists a bachelor's degree (or more) as a job requirement, and again construct a binary indicator equal to one if a posting requires a bachelor's degree and zero otherwise. The unit of analysis for these outcomes is the job posting, and the sample consists of a maximum of 49,045 observations, treating each posting as an individual observation.

Finally, I examine whether compliance rates with other regulations change in response to increasingly strict group sizes and child-to-staff ratios. Specifically, I study compliance with state regulations on teachers' minimum education requirements. For this analysis, I begin by coding each state's center-based (minimum) education requirement for lead teachers. As shown in Table 2.1, I focus on lead teachers because most states regulate their education levels, and there is substantial variation in these regulations. Indeed, the lead teacher education requirements range from a high school

diploma (17 states) to a college degree (4 states), with the remaining states requiring some number of college credits or a professional credential. I then compare the education requirement in each job posting with the corresponding state education regulation. I code compliance as a binary indicator, which takes a value of one if the posted education requirement meets or exceeds the state requirement. This analysis is conducted on the subset of lead teacher job postings, for which there is a total of 20,296 observations.

Figure 2.1 presents the daily time series for the four outcomes of interest over the study period (i.e., January 1 to September 30, 2020): total number of child care job postings (Panel A), share of lead teacher job postings (Panel B), share of job postings requiring a bachelor's degree or more (Panel C), and share of lead teacher job postings in compliance with the state education regulation (Panel D). Prior to the pandemic (in January and February), the U.S. averaged between 200 and 300 child care job postings each day, but which fell below 100 in the early months of the pandemic. However, job postings fully recovered to, and even exceeded, their pre-pandemic levels by the end of the summer. Interestingly, there was an immediate increase after the pandemic began in the share of job postings for lead teachers and those requiring a bachelor's degree. However, the demand for bachelor's degrees fell throughout the summer, eventually receding to its pre-pandemic levels. Finally, nearly 70% of lead teacher job postings in the pre-pandemic months (of January and February) were in compliance with the state education regulation, a share that increased between March and May before falling to its pre-pandemic levels by the end of the study period.

2.3.2 Child Care Regulations

My analysis examines the impact of two labor-related child care regulations: center-based maximum classroom group sizes and child-to-staff ratios. A key feature of both regulations is that they vary by single-year of children’s age, up to age 10. In order to exploit the substantial age-based variation in these regulations while keeping the number of such variables at a manageable level, I create averages for group sizes and ratios across three age groups, as previously described: ages 0 to 2 (infants and toddlers), ages 3 to 5 (preschool-age children), and ages 6 to 10 (school-age children).⁷ Thus, I code these regulations in state $\times$ day $\times$ age-group cells over the period January 1 to September 30, 2020.⁸ I rely on multiple data sources to obtain an accurate and consistent time series in these regulatory changes, including Child Care Aware of America, National Governors Association (NGA), Hunt Institute, and state governor’s office and Department of Health websites.⁹

⁷For example, the value of group sizes and child-to-staff ratios for school-age children would represent the average of these regulations across 6-, 7-, 8-, 9-, and 10-year-olds.

⁸That my data exploits states’ child-age-specific variation in regulations is advantageous for two reasons. First, it provides us with a third source of identifying variation, which can be used to generate estimates that rely on regulatory differences between child age-groups within states and days, thereby allowing us to control for time-varying unobservables. In contrast, most previous work relies on cross-sectional or within-state over time variation in child care regulations (e.g., Blau (2003), Boyd-Swan and Herbst (2019), and Hotz and Xiao (2011)). Second, the additional age-group variation reduces the correlation between the variables measuring group sizes and child-to-staff ratios, allowing us to separately identify the impact of both regulations. The presence of strong correlations between regulations has been noted elsewhere, and has forced previous studies to construct summary indices of multiple regulations to circumvent the problem (e.g., Blau (2003), Boyd-Swan and Herbst (2019), Hotz and Xiao (2011), and Currie and Hotz (2004)).

⁹Child Care Aware data are available here: <https://www.childcareaware.org/coronavirus-hub/coronavirus-landing-page/state-policies-and-ratio-changes-during-covid-19/>, NGA website resources are found here: <https://education.nga.org/section-statetable>, while the Hunt Institute’s data summary can be seen here <https://hunt-institute.org/covid-19-resources/state-child-care-actions-covid-19/>.

I measure center-based maximum group size regulations in two ways. First, I examine whether any group size regulation is enacted within each state $\times$ day $\times$ age-group cell. Thus, this variable is coded as binary indicator, which takes a value of one if a group size regulation is in effect and a value of zero if there is no group size regulation in effect. Second, I measure the actual value of the regulated group size within each state $\times$ day $\times$ age-group cell, making this a continuous measure of the stringency of the group size regulation. I assign a value of zero to cells without a group size regulation in effect and then invert the variable (i.e., $1/\text{maximum group size}$), so that higher values indicate increasingly strict group size regulations. Finally, I standardize the variable to have a mean of zero and a standard deviation of one so that its coefficient can be interpreted as the change in a given outcome due to a one standard deviation increase in the stringency of the group size regulation. As will be shown, these variables allow for the estimation of regulatory effects along two policy-relevant margins that were altered during the observation period: the average treatment effect of enacting a groups size regulation for the first time by many states during the pandemic, and the marginal effect of increasing the stringency of group sizes in states that had already enacted such requirements prior to the pandemic.

My final measure of child care regulations captures the stringency of center-based child-to-staff ratios. As with group sizes, child-to-staff ratios vary by single year of children's age, up to age 10. Thus, I calculate the average value of the ratio in the three age groups defined above, and allow it to vary within each state $\times$ day $\times$ age-group cell, again making this a continuous measure of the stringency of the ratio regulation. I impute a value of zero to cells without a ratio in effect, and I invert the variable so that its values indicate increasingly strict regulations. This variable is also standardized to have a mean of zero and a standard deviation of one. Given

that nearly every state regulated child-to-staff ratios prior to the pandemic, and that no state changed its implementation status during the pandemic, my measure of ratios captures only the marginal effect of a change in the toughness of a pre-existing standard.

Table 2.2 provides descriptive evidence on the child-age-specific variation in group sizes and child-to-staff ratios, while Figures 2.2 through 2.4 shed light on how these regulations evolved over the study period. It is clear from Table 2.2 that center-based classrooms serving younger children are subjected to more stringent labor regulations than those serving older children. For example, prior to the pandemic (that is, January and February of 2020), 79% of states enacted a group size regulation for infant/toddler classrooms, compared to 63% for school-age classrooms (Panel A). Similarly, among states with a group size regulation in effect, the maximum classroom sizes were mandated to be considerably smaller in infant/toddler settings (11) than in school-age settings (31) (Panel B). Such differences are also quite large in states regulating child-to-staff ratios: the average ratio in infant/toddler classrooms was approximately 6:1, while that for school-age classrooms was nearly three times higher, at 17:1 (Panel C).

Figures 2.2 through 2.4 reveal how these pre-pandemic levels evolved throughout the first seven months of the pandemic. Specifically, Figure 2.2 documents the daily time series variation in the share of states regulating group sizes, both overall and by age group. Figure 2.3 shows the time series in the regulated value of group size (among states with a regulation in effect), while Figure 2.4 shows the analogous data for child-to-staff ratios. In each figure, the mean daily regulation across all ages (0 to 10) is displayed in the upper-left-hand corner, while the age-group-specific means are shown in the remaining graphs.

As shown in Figure 2.2, the share of states with any group size regulation enacted increased considerably in the early months of the pandemic, rising from 70% to 85% between March and June. Interestingly, some states subsequently allowed these regulations to lapse, such that by the end of the study period (September) the share of states regulating group sizes had fallen to around 80%. Furthermore, it is clear from the age-group-specific figures that the enactment and subsequent rescission of group size regulations affected classrooms serving all age groups in center-based programs. Figure 2.3 similarly shows that among states regulating group sizes, those regulations grew tougher throughout the pandemic, falling from an average of about 20 in March to a low of 14 in June, at which point some states allowed classroom sizes to increase gradually. However, it is noteworthy that by the end of my study period (September 30), average group sizes still remained far below their pre-pandemic levels. Finally, Figure 2.4 reveals a similar time path for child-to-staff ratios: the enactment of tougher regulations between March and June, resulting in substantially smaller ratios, followed by a loosening of the regulations throughout the remainder of the study period, even though the ratios did not return to their pre-pandemic levels.

In summary, states made substantial changes to their group size and child-to-staff ratio regulations during the period covered by my analysis. Indeed, these policy changes were both widespread and numerous, providing substantially more policy variation than was available in previous studies. Between January and October of 2020, 31 states reformed their group size regulations, with the average state making over 18 changes to its age-group-specific standards. Similarly, 18 states revised their child-to-staff ratios, and these states made an average of 14 changes to its age-group-specific standards. I exploit this policy variation within and across states and age groups in the empirical models, as described in the next section.

2.4 Empirical Strategy

I begin by studying the impact of child care regulations on the demand for child care labor. I then turn my attention to analyzing the demand for teacher characteristics, such as preferences for lead teachers and those with four-year college degrees. Finally, I assess whether states' group size and ratio regulations have implications for providers' compliance with other labor-related regulations. My specifications contain elements of binary and continuous treatment difference-in-differences designs, capitalizing on variation in treatment intensity from states that enacted new child care regulations or made further changes to existing ones. Specifically, this policy variation is driven primarily by within-state over-time changes in regulatory stringency as well as between child-age-group differences in regulations.

2.4.1 Demand for Child Care Labor

To understand the relationship between regulations and the demand for child care labor, I start by estimating regressions of the following form:

$$Y_{gstm} = \gamma r_{gst}^e + \phi r_{gst}^i + X_{st}' + \zeta_g + \xi_s + \eta_t + Y_{st}' * \theta_m + \xi_s * f(time)_t + \varepsilon_{gstm} \quad (2.1)$$

where Y_{gstm} denotes the log number of child care job postings in child-age-group g , state s , day-of-the-year t , and month m . The key variables of interest are r^e , which denotes an indicator for whether the state has any group size regulation in place, and r^i , which denotes a vector of standardized measures of group sizes and child-to-staff ratios within a given state $\times$ day $\times$ age group cell. Whereas r^e captures whether any group size regulation is enacted, r^i focuses on the (continuous) regulated value of group

size, conditional on having such a regulation in place. This allows us to distinguish between provider responses to newly-enacted regulations (i.e., regulations implemented for the first time during the pandemic) versus responses to incrementally increasing the stringency of existing regulations. Therefore, the coefficient γ represents the percent change in the number of child care job postings among providers in states that newly-enact a group size regulation for a given child age group, while the coefficient ϕ represents the percent change in the number of job postings for providers in states that reduce by one standard deviation the regulated group size and child-to-staff ratio. My reported standard errors are adjusted for clustering at the state $\times$ day level.

The baseline model includes a set of two-way fixed effects for states, days, and child age groups. The state fixed effects, ξ , account for any time-invariant unobserved differences across states that may be correlated with decisions regarding child care regulatory stringency. For example, states with tougher regulations may have other quality-related early education initiatives in place or include parents with stronger preferences for quality, both of which may influence providers' labor demand. The day-of-the-year fixed effects, η , control for any unobserved time-varying national shocks that influence states' regulatory decisions and providers' labor demand. Such controls are particularly important in this context, given the rapidly changing macroeconomic and pandemic-related conditions throughout my study period. Finally, child-age-group fixed effects, ζ , absorb any differences across children's age groups that may explain the differential stringency of regulations across those age groups. For example, the importance of child care quality might vary across children's ages, thereby leading to an age-specific component in the demand for regulated care (e.g., targeting younger children).

Given that the baseline model includes state and day fixed effects, the coefficients

γ and ϕ represent the causal effect of child care regulations on labor demand as long as there are no time-varying differences within states that are correlated with the timing of the adoption of new regulatory policies within those states. Such an assumption is likely problematic in my context, given the large number of economic and pandemic-related factors that changed contemporaneously with the enactment of states’ new child care regulations. For example, the new regulations might be a response to changing preferences for enhanced health and safety controls as well as supervision as the risk of COVID-19 transmission changed. In addition, states enacted a number of policies (e.g., state-at-home orders and mandatory business closures) aimed at mitigating the spread of COVID-19, all of which likely influenced the demand for child care labor (Ali et al., 2021). Finally, there may be pre-pandemic differences across states in the functioning of the child care market (i.e., supply and demand conditions) whose impact on labor demand during the pandemic evolved differentially over time. Failure to control for these time-varying factors could bias the coefficients on regulations if they are correlated with child care providers’ hiring behavior.

I take a number of steps to mitigate the influence of these potential time-varying confounders. First, I control directly for the evolving severity of the pandemic in each state by including in model the log number of COVID-19 cases in each state $\times$ day cell. In addition, I include a proxy for changes in household child care demand by controlling for Google Trends search intensity scores for the topic “child care”, also measured in state $\times$ day cells. Second, states enacted several new health and safety requirements for center-based providers aimed at mitigating the spread of COVID-19. Therefore, the empirical model controls for whether programs were required to provide sanitizing and disinfecting supplies to children and staff on site, required sanitize and disinfect facilities daily, precluded from allowing inter-group (i.e., classroom) mixing

for all activities, and precluded from allowing all visitors to enter the facility. All four health and safety requirements are coded as binary indicators equal to one if a given requirement is enacted in each state $\times$ day cell.¹⁰ Third, I include a variety of controls for states' COVID-19 containment policies that were enacted contemporaneously with the child care regulations. These variables include binary indicators for the enactment of state-of-emergency declarations, stay-at-home orders, child care program closure mandates, public school closures, business closures (and re-closures), and statewide indoor mask mandates.¹¹ All of these controls are included within the X' variable matrix.

Fourth, I attempt to further account for time-varying unobservables by including in the model an additional 12 state-level variables, each of which is interacted with a set of month fixed effects, as denoted by the term $Y'_{st} * \theta_m$. These variables are the log population ages 0 to 4, black share of the child population, marriage rate, share of the population ages 25 and over with a bachelor's degree, a binary indicator equal to one if the state's governor is a Republican, unemployment rate, median household income, log number of child care establishments, log number of child care industry employees, log weekly earnings for child care employees, log number of children attending Head Start, and log per child education spending. These data are collected for the year 2019, and are aimed at capturing potentially important child care supply and demand, labor market, and political conditions that may influence within-state over-time changes in

¹⁰Key resources used in the extraction of these data on health and safety regulations are relevant policy documents issued by state agencies, as outlined and summarized by Child Care Aware of America website, the National Governors Association (NGA) state-action tracker, and the Hunt Institute Database.

¹¹These data have been obtained from the COVID-19 U.S. State Policy Database (CUSP) (Raifman et al., 2021), the National Governors Association (NGA) state-action tracker, and the Hunt Institute database.

providers' hiring behavior. Furthermore, the model includes $\xi_s * f(time)_t$, which is a set of state-specific time trends, using a linear function of time. Such controls are important if any component of the unobservables is trending within states.

Once the results from the basic model are established, I fully exploit the panel structure of the data (organized into state $\times$ day $\times$ age group cells) to test additional assumptions about the nature of the unobservables. In particular, I experiment with more restrictive two-way fixed effects models, beginning with those that include state $\times$ child age group fixed effects as well as day $\times$ child age group fixed effects. The former assumes that any permanent state-specific unobservables have an age group component, while the latter assumes that any time-varying shocks have an age group component. I then estimate what is perhaps my most stringent specification, which includes state $\times$ day fixed effects. In this model, I use only within-state and -day (i.e., between-child-age-group) variation in the child care regulations to identify their impact on labor demand. Thus, this specification controls for all within-state time-varying shocks, including changes to labor market and political conditions, the enactment of COVID-19 containment policies, and other pandemic-induced shifts in the child care market.¹²

As a final robustness check to validate that the coefficients γ and ϕ are not picking up any state $\times$ day $\times$ age group shocks to the child care market, I implement a falsification test in which the number of child care job postings is replaced with the number of Head Start and pre-kindergarten job postings as the outcome. Although the labor markets for Head Start and pre-kindergarten teachers are comparable to

¹²Of course, this model does not include the time-varying state controls or the state-specific time trends, given that they are collinear with the state $\times$ day fixed effects. However, I incorporate the day $\times$ child age group fixed effects as well as interactions of the 2019 state characteristics with age group fixed effects to account for an unobserved heterogeneity that has an age group component.

that for child care (and are exposed to the same macroeconomic, policy, and pandemic conditions), changes to child care regulations should not influence labor demand in these publicly-provided early education sectors, given that the regulatory changes were directed only at center-based child care providers. Thus, if γ and ϕ in Equation 4.1 are to be interpreted as causal estimates, changes to group sizes and child-to-staff ratios should not affect job postings for Head Start and pre-kindergarten teachers.

2.4.2 Workforce Characteristics

My identification strategy for the analysis of workforce characteristics differs from the one shown in Equation 4.1, relying only on within-state over-time variation in child care regulations. I estimate regressions for workforce characteristics as follows:

$$Y_{istm}^k = \gamma r_{st}^e + \phi r_{st}^i + P'_{ist} + X'_{st} + \xi_s + \eta_t + Y'_{st} * \theta_m + \varepsilon_{istm} \quad (2.2)$$

where Y_{istm} in Equation 2.2 denotes an indicator variable for whether a given child care job posting i in state s , day-of-the-year t , and month m corresponds to a particular characteristic. Thus, the unit of analysis for these outcomes is the job posting, rather than the state $\times$ group $\times$ day panel structure used in the previous analysis. The r^e denotes the indicator for whether the state has any group size regulation in place (averaged over child ages 0 to 10), and r^i denotes a vector of standardized measures of group sizes and child-to-staff ratios (averaged over child ages 0 to 10). The model includes a number of job posting characteristics, denoted by P' , including a set of binary indicators for whether the posting is for an infant/toddler, preschool-age, or school-age teacher; a set of binary indicators for the education requirement of the position; a binary indicator for whether the position is part- or full-time; and a binary

indicator for whether the position requires shift work. I also include X' and $Y'_{st} * \theta_m$, which denote the same set of time-varying state characteristics outlined in Equation 4.1. Finally, the model includes state (ξ_s) and day (η_t) fixed effects. Standard errors are clustered on state $\times$ day cells.

As outlined in the previous section, I study three outcomes using this specification, all of which are expressed as binary indicators. First, I differentiate across the level of each job posting i and create a variable denoting whether the job posting corresponds to a lead teacher or an assistant teacher. The second outcome examines whether each job posting requires at least a bachelor's degree. For the third outcome, I attach state-specific minimum teacher education requirements to each job posting, and define my outcome as a binary indicator for whether the education requirement in the job posting meets the state education regulation. I rely on the full set of job postings for the first two outcomes, but limit the sample to only lead teacher job postings for the third outcome. I limit the sample to lead teachers because for most states the minimum education requirements for assistant teachers are either undefined or much more lenient.

2.5 Main Results

2.5.1 The Demand for Child Care Labor

I now present the main results for child care labor demand in Table 2.3 associated with Equation 4.1. I present results from seven specifications, each one either enriching the set of control variables or making different assumptions about the nature of unobserved heterogeneity. Specifically, the results in column (1) come from a model

that includes age group, state, and day fixed effects as well as the basic set of time-varying state covariates (i.e., X'). Column (2) includes the additional 12 state controls (separately interacted with month fixed effects and age group fixed effects), while column (3) adds the state-specific linear time trends. I then add the state $\times$ age group fixed effects to the model in column (4) and the day $\times$ age group fixed effects to the model in column (5). Both sets of fixed effects are included together in column (6). Finally, column (7) incorporates the state $\times$ day (along with day $\times$ age group) fixed effects.¹³

Looking at the results in Table 2.3, two observations are immediately apparent: the regulations have different effects on labor demand, and the estimates are quite similar across the various specifications. Generally speaking, the results indicate that the enactment of any group size regulation reduces the demand for child care labor, as proxied by the number of job postings. Indeed, the estimate in column (1), which corresponds to the most sparsely controlled model, implies that enacting a group size regulation is associated with a 7.5% reduction in the number of child care job postings, while that in Column (7), which corresponds to the most richly controlled model, reveals a 9.2% reduction in the number of job postings.¹⁴ Conversely, increasing the stringency of (already-existing) group size regulations appears to increase the demand for child care teachers. The coefficient in column (1) suggests that a one standard deviation (SD) reduction in the group size is associated with a 1.0% increase in the number of job postings, an effect whose magnitude increases to 6.1% in column

¹³The model in column (4) also includes day fixed effects, while that in column (5) includes state fixed effects.

¹⁴A key observation is that, if anything, the magnitude of the coefficients in column (7) is consistently larger than their counterparts in columns (1) through (6), implying that the latter estimates may be biased toward finding no impact of regulations. Thus, the results in column (7) suggest that there are no time-varying state-specific sources of unobserved heterogeneity.

(7). Finally, my results suggest that increasing the stringency of (already-existing) child-to-staff ratios reduces the demand for child care labor. Column (1) shows that a one SD reduction in the child-to-staff ratio is associated with a 0.8% drop in job postings, while that in column (7) implies a 1.8% drop.

As a final robustness check, I implement a falsification test, exploring whether center-based child care regulations influence the number of Head Start and pre-kindergarten job postings. Such results are presented in Table 2.4, and the columns follow the same model progression as in Table 2.3. I find consistently null associations: all of the coefficients are small in magnitude, inconsistently signed, and never statistically significant. If the results in 2.3 were simply an artifact of unobserved time-varying or age-group-specific shocks, I would expect to see an effect on labor demand in the Head Start and pre-k markets, which I do not. Overall, these results are consistent with those in Ali et al. (2021), who find that the decline in overall early care and education job postings throughout the pandemic was driven by the substantial reduction in the center-based child care sector.

In results not reported in the paper, I undertake two additional robustness checks. First, I remove the state characteristics interacted with month fixed effects and replace those controls with a set of interactions in the changes in state characteristics between 2015 and 2019 with linear time trends. These controls account for any pre-trends in states' characteristics that may drive changes in labor demand. My results are robust to the inclusion of these controls. Second, I test for the symmetry of my results across periods in which the regulations became tougher and when they became more lenient. Recall from Figures 2.2 through 2.4 that the regulations grew tougher during the first part of my analysis period, peaking in late-May to early-June, after which they became more lenient. Thus, to test for symmetry I estimate the model in column (7) of Table

2.3 separately for the period January 1 to June 30 and for July 1 to September 30. My results reveal substantial symmetry: the impact of regulations during a period in which regulatory stringency was increasing is similar to their impact during a period in which regulatory stringency was decreasing.

To put my results in perspective, consider that the daily average number of child care job postings across states and age groups is 3.5. This implies that the enactment of any group size regulation leads to roughly 0.32 fewer job postings each day, or nearly 10 fewer job postings each month. However, increasing the stringency of already-existing group size regulations (which corresponds to a one SD reduction in classroom group sizes) would result in over 6 more job postings each month. Finally, increasing the stringency of already-existing child-to-staff ratios (which corresponds to a one SD reduction in ratios) would result in approximately two fewer job postings each month. Given that there were fewer than 100 job postings per day nationally at the height of the pandemic in 2020, the estimated coefficients in this paper are economically meaningful.

In sum, the results for labor demand are robust to the inclusion of a variety of controls for unobserved state, time, and age group heterogeneity. I find that child care providers, when making decisions about labor demand, respond differently depending on the regulation. In particular, the enactment of any regulation on classroom group sizes as well as increasing the stringency of child-to-staff ratios reduce the demand for child care labor, while increasing the stringency of group size regulations increases the demand for child care labor. Thus, I uncover a strong negative effect of group size regulations on providers located in states that enacted such a regulation for the first time during the pandemic. On the other hand, it appears that making an already-existing group size regulation marginally tougher induces providers to slightly

increase labor demand. However, as it relates to child-to-staff ratios, increasing the toughness of an already-existing ratio encourages providers to reduce labor demand. I discuss potential explanations for this pattern in the Conclusion.

2.5.2 The Characteristics of Child Care Labor

I now study the impact of child care regulations on the characteristics of the job postings. In doing so, my goal is to understand whether child care providers respond to tougher regulations by engaging in input substitution, or varying the skill composition of the workforce. As shown in Table 2.5, I begin by studying a binary indicator equal to one if a given job posting is for a lead teacher and zero if it is for an assistant teacher. Column (1) presents results from a model that includes the basic set of time-varying state covariates as well as the state and day fixed effects, while Column (2) adds the 12 additional state controls (each one interacted with month fixed effects). Finally, column (3) adds the job posting characteristics.

My results consistently show that the enactment of a group size regulation increases the odds that a given job posting advertises for a lead teacher, which is indicative of an increased demand for such teachers relative to assistant teachers. Indeed, the estimate in column (3) implies that having a group size regulation in place increases the odds of advertising for a lead teacher by 3.9 percentage points. Among providers located in states with a group size regulation already enacted, increasing the stringency of that regulation does not appear to alter the demand for lead (versus assistant) teachers. However, Table 2.5 also shows that increasing stringency of already-enacted child-to-staff ratios reduces the demand for lead teachers. Specifically, the coefficient

in column (3) implies that a one SD decrease in the child-to-staff ratio reduces the probability of advertising for a lead teacher by 2.1 percentage points.

Table 2.6 further tests for input substitution by examining whether a given job posting requires at least a bachelor’s degree.¹⁵ Panel A is restricted to the subset of assistant teacher postings, Panel B is restricted to lead teacher postings, and Panel C is restricted to preschool-age lead teacher postings. I conduct the analysis separately on the subset of lead preschool-age job postings because this is the most prominent age-group in terms of child enrollments and teacher hiring. Results in Panel A reveal that the enactment of any group size regulation may increase the demand for bachelor’s degrees among assistant teachers. Although the coefficient is large and statistically significant in column (1), its magnitude is reduced by about one-half and is statistically insignificant in column (3). Furthermore, increasing the regulatory stringency at the intensive margin (for group sizes and child-to-staff ratios) does not appear to alter the education requirements for assistant teachers, suggesting that any rise in demand for these more educated child care teachers is driven by the extensive margin (i.e., providers required to comply with newly-enacted group size regulations).

Turning to Panels B (all lead teachers) and C (lead preschool-age teachers), I find that states’ newly-enacted group size regulations encourages child care providers to reduce their demand for (four-year) college educated lead teachers, particularly in the labor market for lead preschool-age teachers. Indeed, the coefficient in column (3) of Panel C implies that the likelihood of requiring such a degree declines by 8.8 percentage points. Conversely, there is some evidence to suggest that increasing the stringency of an already-enacted group size regulation encourages education-based upskilling by increasing the demand for bachelor’s degrees among lead teachers. However, increasing

¹⁵The same model progression used in Table 2.5 applies to Tables 2.6 and 2.7.

the stringency of child-to-staff ratios has the opposite effect, lowering the odds that job postings require bachelor's degrees, particularly for lead preschool-age teachers.

In sum, these results yield important insights about the ways in which regulations alter the demand for child care labor. Providers that are required to comply with a newly-enacted group size regulation respond by decreasing their demand for labor (i.e., posting fewer teacher advertisements). However, when they do recruit teachers, they are more likely to hire lead teachers, as opposed to assistant teachers. This finding makes intuitive sense, because, at a minimum, a lead teacher is required to operate a child care classroom. Furthermore, the adoption of a group size regulation is associated with an increased number of postings that require a bachelor's degree, conditional on posting for an assistant teacher, but a decreased number of such postings for lead teachers. This suggests that child care regulations lead to a substitution away from higher-skilled lead teacher jobs towards higher-skilled assistant teacher jobs. In addition, such downskilling may occur in order to offset the increased cost of compliance with a (newly-enacted) regulation aimed at reducing classroom group sizes.

On the other hand, I find little response from providers located in states that increase the stringency of preexisting group size regulations. Although there is a small change in overall labor demand, I uncover no evidence of a change in the demand for lead teachers, and, if anything, I find some evidence of lead teacher upskilling (i.e., lead teacher advertisements are more likely to require a bachelor's degree) as the group size regulations become tougher. Finally, my results suggest that increasing the stringency of preexisting child-to-staff ratios have important effects on providers' hiring behavior. In particular, providers are encouraged to decrease their overall labor demand, reduce the demand for lead teachers, and downskill their workforce by becoming less likely to require lead teachers to have a bachelor's degree. Again,

such results indicate that increasing the strictness of child-to-staff ratios is sufficiently costly that providers respond by substituting away from higher-skilled labor in order to comply with the requirements.

2.6 Extensions and Robustness

The results presented so far highlight the unintended consequences of child care regulations for child care labor demand and the composition of the workforce. In this section, I examine another implication of regulations via their impact on compliance with other state regulations. I end with an additional robustness check.

2.6.1 Non-compliance with State Regulations

In order to comply with tougher regulations in some domains, it is possible that some providers reduce their regulatory compliance in other domains. Such a response may be particularly relevant if the new regulations are highly costly from a compliance perspective (e.g., because they require more staff) or if states' enforcement efforts are weak. To test this proposition, I construct for each job posting a binary indicator for whether the advertised education requirement meets the corresponding state education requirement for lead teachers. I then estimate models that relate compliance with the measures of group sizes and child-to-staff ratios, exploiting variation within states over time. Again, the goal of this exercise is to test whether providers are less likely to comply some regulations as others become increasingly strict. Table 2.7 presents results from this analysis, with Panel A restricted to the subset of lead teachers and Panel B restricted to lead preschool-age teachers.

Looking at column (3) of Panel A, I find that the enactment of any group size regulation reduces the likelihood of complying with states' teacher education requirements by 5.1 percentage points. I also find that increasing the stringency of already-enacted group size regulations is associated with a small but statistically insignificant increase in the probability of meeting the state requirements, while increasing the stringency of child-to-staff ratios reduces the probability of meeting the requirements by 1.9 percentage points. When the sample is restricted to lead preschool-age job postings, I see even larger and more statistically significant changes in the likelihood of meeting the state education requirements. Specifically, providers located in states with any group size regulation in place are 8.3 percentage points less likely to be in compliance with the corresponding state education requirement, while those exposed to tougher child-to-staff ratios are 2.7 percentage points less likely to be in compliance.

These results, together with those presented in Table 2.6, underscore the potential unintended consequences associated with supply-side regulations: in an endeavor to comply with tougher regulations, providers may be encouraged to violate other standards to offset the increased the cost of compliance. Furthermore, enacting tougher regulations may induce more violations if states' enforcement efforts are weak.

2.6.2 Robustness Check

As a final robustness exercise, I ask whether any of my results on child care labor demand could be driven by a key time-varying shock that coincided with the introduction and increased stringency of child care regulations. Specifically, many states fundamentally altered the health and safety standards for center-based child care programs in order to mitigate the transmission of the virus, and they did so

contemporaneously with the reforms to group sizes and child-to-staff ratios. Specifically, states mandated face coverings and health screenings for staff and children, social distancing, the provision of sanitizing and disinfecting supplies, enhanced cleaning protocols, staggered arrival and departure times, changes to visitation protocols, among other things. I code 10 such requirements in state $\times$ day cells over the period January 1 to September 30, and I use these data to answer two questions: do these policies have an independent effect on the labor demand outcomes studied earlier, and does their inclusion in the model alter the estimates on group sizes and child-to-staff ratios?¹⁶ The analyses implied by these questions serve as a final test of whether other time-varying policies that were enacted contemporaneously with changes to states' group sizes and child-to-staff ratios bias the estimates on these regulations.

Table 2.8 presents the results associated with regressions of the labor demand outcomes on a standardized index of 10 COVID-19 health and safety regulations. Interestingly, these regulations are not correlated with overall labor demand (Panel A), nor are they correlated with any dimension of the child care workforce (Panels B through D) or with compliance rates (Panel E). Indeed, the coefficients are consistently small in magnitude and never statistically significant. Furthermore, in results omitted for brevity, inclusion of this index variable in the model does not alter the estimated effects of group sizes and child-to-staff ratios. Together, results from these analyses suggest that my main estimates are not confounded by these health and safety standards, even though they were sometimes introduced by states alongside changes to

¹⁶Each health and safety regulation is coded as a binary indicator equal to one if a given regulation is in effect, and zero otherwise. I then average and standardize the set of 10 regulations, with a mean of zero and a standard deviation of one. Thus, increasing values on the index imply more stringent health and safety standards are in place in a given state and day.

group sizes and child-to-staff ratios. Such evidence therefore provides more confidence in the credibility of my results.

2.7 Conclusion

Over the last several decades, there has been an ongoing debate over the welfare effects of licensing and minimum quality standards. On the one hand, in the presence of information frictions, they can provide a much needed quality control, which is especially important in markets like child care, in which quality can influence early childhood health and development, as well as long-run adult outcomes. On the other hand, regulations can lead to negative unintended consequences for both the quantity and quality of services provided.

In this paper, I combine unique data on online child care job postings with newly available information on child care regulations to study impact of regulations on the demand for child care labor. My analysis exploits the substantial variation in states' regulations that arose during the COVID-19 pandemic. During the first nine months of 2020, nearly all states enacted new regulations or expanded existing ones. My identification strategy therefore exploits variation in regulatory stringency both within states over time and across children's age groups to estimate the causal effect of maximum classroom group sizes and child-to-staff ratios. Importantly, my variation across age groups within the same state allows us to introduce state $\times$ time fixed effects, purging within-state over-time variation in the severity of COVID-19 as well as the macroeconomic and policy responses to it.

This paper has three key findings. First, the introduction and increased stringency of group size and child-to-staff ratio regulations reduce the demand for child care

labor. This is particularly true for providers located in states that are exposed to a newly-enacted group size requirement, which reduces the number of child care job postings by 9.2%. Second, my results indicate that such regulations distort the demand for specific workforce characteristics. Specifically, the enactment of a new regulation on group sizes increases the number of lead teacher postings by 3.9 percentage points, but reduces the probability that these postings require a bachelor's degree by 4.2 percentage points, while also reducing the probability that the education requirements comply with the corresponding state education regulations for lead teachers by 5.1 percentage points. Third, although increasing the stringency of group sizes in states already regulating this feature appears to have weaker effects on the demand for child care teachers, I find that increasing the stringency of child-to-ratios has strong effects—leading to fewer job postings, fewer lead teacher and bachelor's degree postings, and more postings in violation of the state education regulations.

These results indicate that child care providers operating in states that did not regulate group sizes prior to the pandemic—but began regulating them after the pandemic—experienced significant challenges adjusting to the new regulatory environment. Such providers not only preferred to hire fewer teachers, but sought to downskill the teacher positions being advertised, at least as it relates to education. Conversely, providers in states already regulating group sizes prior to the pandemic appear to have adapted well to the post-pandemic period, when these regulations became tougher. Indeed, increasing the strictness of group sizes has smaller effects on providers' hiring behavior. However, changes to child-to-staff ratios are costly for providers, leading to lower demand for teachers and a decrease in the skill requirements for the job.

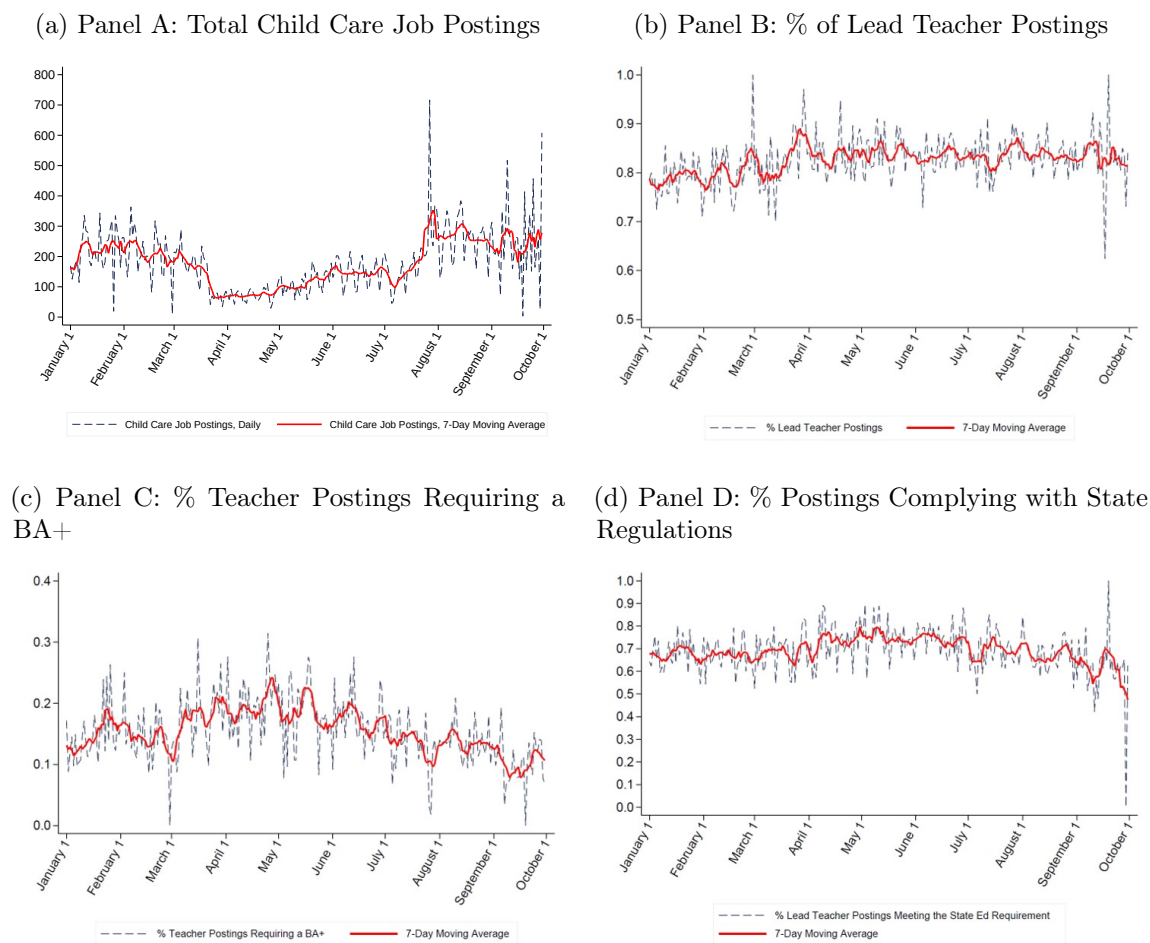
The finding that regulations decrease labor demand in the child care market is consistent with a related line of work showing that regulations also reduce the number

of center-based establishments (Gormley, 1991; Hotz and Xiao, 2011). Together, this work suggests that the supply of child care may decline through a contraction in the size of the workforce and a drop in the number of providers. In addition, my work shows that providers engage in input substitution in ways that may not increase quality, as intended. Specifically, as regulations become tougher, child care providers appear to trade-off some quality-related characteristics (i.e., by downgrading teachers' education requirements) in order to comply with regulations set in other domains. This result is consistent with those in Blau (2007), who finds similar evidence on input substitution as well as null effects of regulations on a measure of classroom quality. In sum, my paper suggests that regulations may not be the most effective policy tool for improving quality within the child care market. In addition, the unintended consequences of such regulations should be considered by policymakers when weighing alternative policies for improving program quality, including consumer subsidies and quality certification initiatives.

Results in this paper leave several questions open for future research. First, have these regulations affected child development? Given the decline in job postings in the center-based child care market, the caregiving burden shifted onto families. To what extent have these regulatory changes altered the mix of parental versus non-parental time with young children as well as the quality of those time investments? Second, how have different child care providers adapted to these regulations? Although the regulations are uniform in the same state and age group, some child care providers may have more margins to adjust than others. Third, how have the employees in the child care market fared, and have those who were laid off returned to work in the child care market? These are just a sample of future research questions.

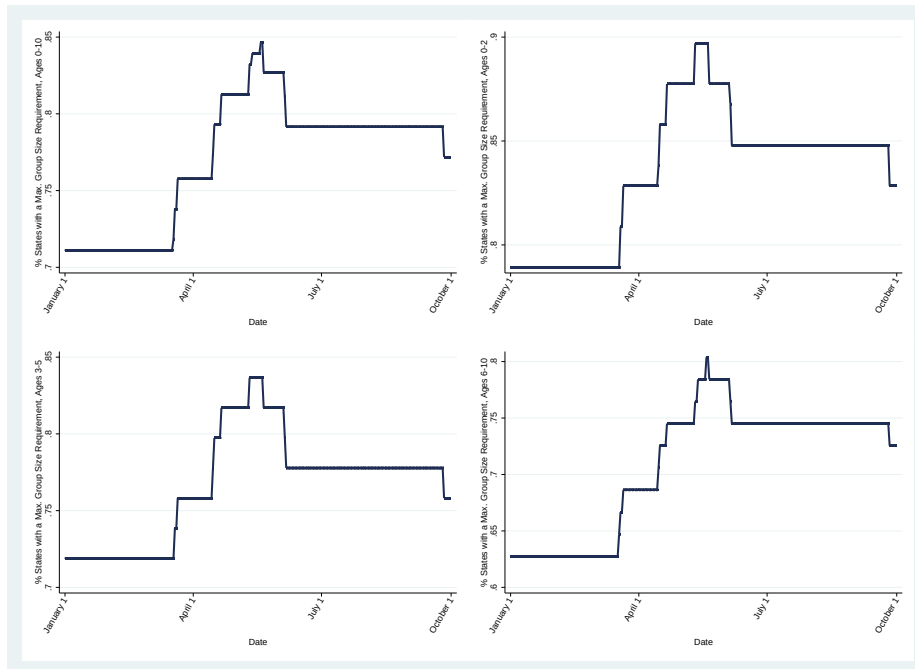
Figures and Tables

Figure 2.1. Characteristics of Child Care Job Postings



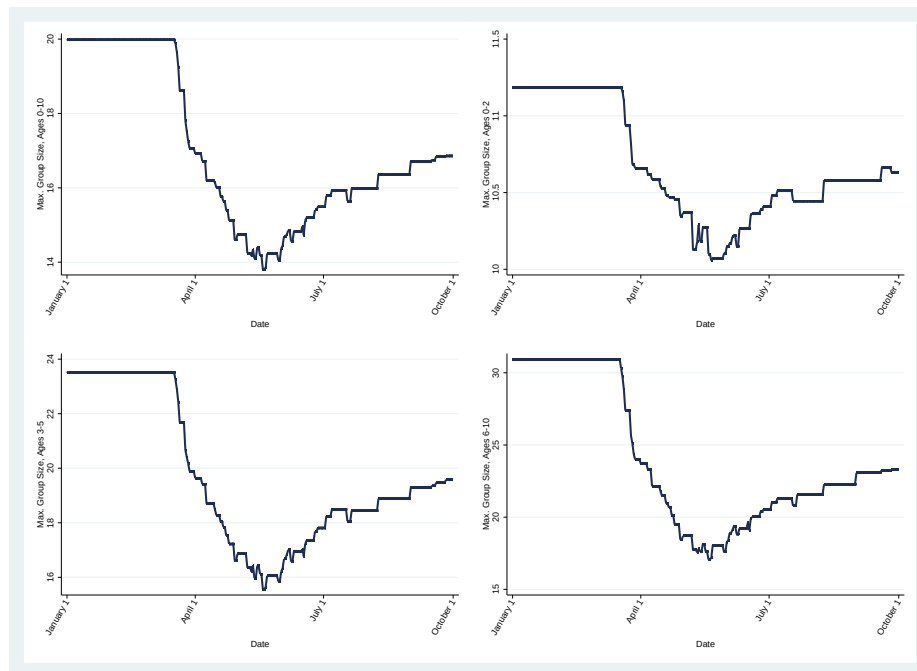
Source: Emsi, 2020. Panel A shows the daily (and 7-day moving average) number of child care job postings. Panel B shows the share of job postings that advertise for a lead teacher. Panel C shows the share of job postings that require a bachelor's degree. Panel D shows the share of job postings that comply with the state's teacher education regulatory requirements.

Figure 2.2. % States Regulating Maximum Classroom Group Size, by Age Group



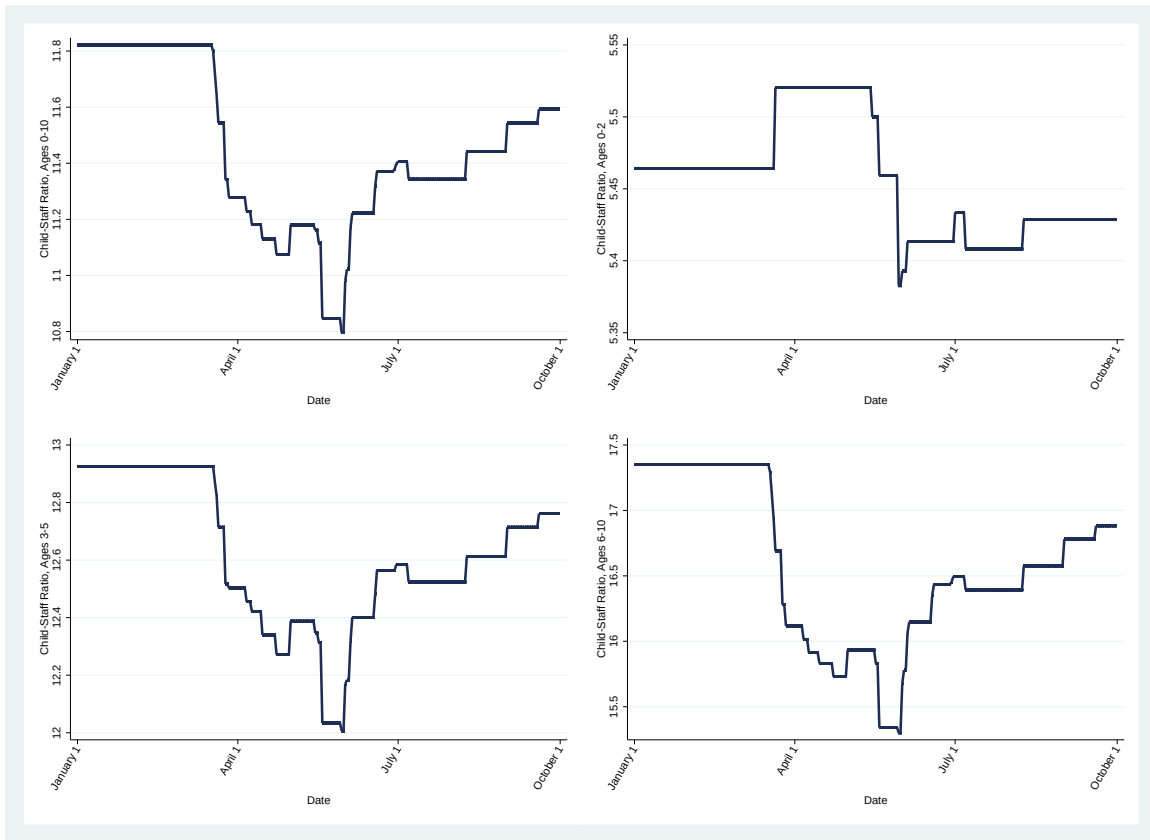
Source: Child Care Aware of America, Hunt Institute, National Governor's Association, and states' Department of Health websites. The figure plots the share of states regulating center-based maximum classroom group sizes over time for each age-group.

Figure 2.3. Time Series in States' Maximum Classroom Group Size, by Age Group



Source: Child Care Aware of America, Hunt Institute, National Governor's Association, and states' Department of Health websites. The figure plots the average value of the center-based maximum classroom group size regulation over time for each age-group.

Figure 2.4. Time Series in States' Child-to-Staff Ratio, by Age Group



Source: Child Care Aware of America, Hunt Institute, National Governor's Association, and states' Department of Health websites. The figure plots the average value of the child-to-staff ratio regulation over time for each age-group.

Table 2.1. Summary of the Child Care Regulatory Environment

State	Group Size	Child to Staff Ratio	Minimum Staff Education Requirement		
			Assistant Teacher	Lead Teacher	Director
Alabama	—	8	8th grade	HSD	HSD & 8 semester hrs or CDA
Alaska	20	10	—	—	HSD & 12 semester hrs or CDA
Arizona	—	13	—	HSD	HSD & 6 credit hrs
Arkansas	24	12	HSD	HSD	CDA
California	—	12	HSD & 6 semester credits	HSD & 6 semester units or CDA	HSD & 15 semester units
Colorado	20	10	HSD & 1 ECE course	HSD & 12 semester hrs or CDA	HSD & 60 semester hrs or CDA
Connecticut	20	10	or HSD (ECE experience or HSD)	HSD & 12 credit hrs or CDA	HSD & 3 ECE credit hrs
Delaware	15	10	HSD & 6 credits	HSD & 9 credits or CDA	AA
District of Columbia	16	8	HSD	HSD & 48 credit hrs or CDA	HSD & 48 credit hrs
Florida	—	15	—	AA	HSD & 30 ECE credit hrs
Georgia	30	15	—	HSD & 15 semester hrs or CDA	HSD & 15 semester hrs or CDA
Hawaii	—	12	HSD & CDA or 60 credit hrs	HSD & 60 credit hrs or CDA	HSD & 60 credit hrs or CDA
Idaho	—	—	—	—	—
Illinois	20	10	HSD	HSD & 30 semester hrs or CDA	AA
Indiana	20	10	HSD	CDA	AA
Iowa	—	13	—	HSD	HSD
Kansas	24	8	—	—	Observation Hours or CDA
Kentucky	24	12	—	HSD	HSD
Louisiana	26	13	—	—	HSD & 6 credit hrs
Maine	22	9	—	—	HSD
Maryland	20	10	HSD	HSD & 6 semester hrs or CDA	HSD & 6 semester hrs or CDA
Massachusetts	20	10	HSD	HSD & 12 credit hrs or CDA	HSD & 16 credit hrs or CDA plus 4 credit hrs
Michigan	—	10	—	HSD & 6 semester hrs	HSD & 60 semester hrs or CDA plus 18 ECE semester hrs
Minnesota	20	10	HSD & 8 credit hrs	HSD & 16 credit hrs ¹	HSD & 6 credit hrs ¹
Mississippi	14	14	—	HSD	HSD & 24 ECE credit hrs or CDA
Missouri	—	10	—	—	HSD & 6 semester hrs or CDA
Montana	8	—	—	—	HSD
Nebraska	—	10	—	HSD	HSD & 6 credit hrs or CDA
Nevada	—	13	—	—	HSD & 15 semester hrs in ECE or CDA
New Hampshire	24	8	—	HSD & 18 ECE credit hrs or CDA	HSD & 60 credit hrs plus 6 credit hrs CD/Mngt ²
New Jersey	20	10	CDA	BA	BA
New Mexico	—	12	—	—	HSD & 3 ECE courses or CDA
New York	18	7	or HSD	HSD & 9 college credits	CDA
North Carolina	25	15	HSD	HSD	HSD & 12 semester hrs (CD) or CDA
North Dakota	14	7	—	HSD	CDA
Ohio	24	12	HSD	HSD	HSD
Oklahoma	24	12	—	HSD & 6 ECE/CD credit hrs or CDA	HSD & 6 ECE/CD credit hrs or CDA
Oregon	20	10	—	HSD & 10 semester hrs or CDA	or HSD & 30 semester hrs or CDA
Pennsylvania	20	10	HSD	AA	AA
Rhode Island	18	9	HSD	HSD	BA
South Carolina	—	13	—	HSD	HSD
South Dakota	20	10	—	—	HSD & 32 credit hrs CD/Mngt or CDA
Tennessee	18	9	—	HSD	HSD
Texas	30	15	HSD	HSD	HSD & 18 credit hrs CD/Mngt
Utah	24	12	—	—	CDA
Vermont	20	10	HSD & 3 credit hrs	BA	HSD & 21 credit hrs
Virginia	—	10	—	HSD	HSD & CDA or 30 semester hrs
Washington	20	10	—	HSD	HSD & CDA or 6.66-30 credit hrs
West Virginia	20	10	HSD	HSD	HSD
Wisconsin	20	10	HSD	HSD & 2 non-credit ECE courses	HSD & 2 non-credit/for-credit ECE courses ³
Wyoming	24	10	—	—	HSD

Source.—Child Care Aware of America, Hunt Institute, National Governor's Association, and states' Department of Health websites

Notes.—The data presented in this table summarizes states' child care regulations as of January 1, 2020. [1] RN/LPN meets requirements for infants, [2] or CDA plus 6 credit hours in Child Development or Management, [3] 4 courses if the center has an enrollment of 51 or more

Table 2.2. Child Care Regulations: Summary Statistics

Panel A: Any Group Size Regulation Enacted (%)	
Ages 0-10	0.711 (0.007)
Ages 0-2	0.789 (0.397)
Ages 3-5	0.719 (0.445)
Ages 6-10	0.627 (0.484)
Panel B: Maximum Group Size (no.)	
Ages 0-10	20.0 (5.4)
Ages 0-2	11.2 (2.9)
Ages 3-5	23.5 (4.9)
Ages 6-10	30.9 (7.4)
Panel C: Child-to-Staff Ratio (no.)	
Ages 0-10	11.8 (2.3)
Ages 0-2	5.5 (1.0)
Ages 3-5	12.9 (2.6)
Ages 6-10	17.4 (3.8)

Source.—Child Care Aware of America, Hunt Institute, National Governor’s Association, and states’ Department of Health websites.

Notes.—The table shows means and standard deviations for different child care regulations for respective age group over the Jan-Feb 2020, pre-pandemic period.

Table 2.3. Estimates of the Impact of Regulations on the Demand for Child Care Labor

	Dep Var: ln(number of job postings)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Any Group Size Regulation	-0.075*** (0.013)	-0.086*** (0.014)	-0.088*** (0.015)	-0.066*** (0.024)	-0.083*** (0.015)	-0.055** (0.024)	-0.092*** (0.016)
(1/Group Size)	0.010** (0.005)	0.040*** (0.005)	0.046*** (0.005)	0.028*** (0.007)	0.046*** (0.006)	0.027*** (0.007)	0.061*** (0.007)
(1/Child-to-Staff Ratio)	-0.008 (0.005)	-0.014** (0.005)	-0.015*** (0.005)	-0.018** (0.007)	-0.017*** (0.005)	-0.022*** (0.007)	-0.018*** (0.006)
Mean Daily No. of Job Postings	3.509	3.509	3.509	3.509	3.509	3.509	3.509
Observations	41,922	41,922	41,922	41,922	41,922	41,922	41,922
State-Level Controls: Time-Varying	Yes	Yes	Yes	Yes	Yes	Yes	No
State-Level Controls: Interactions	No	Yes	Yes	Yes	Yes	Yes	Yes
Age-Group Fixed Effects	Yes	Yes	Yes	No	No	No	No
State Fixed Effects	Yes	Yes	Yes	No	Yes	No	No
Day Fixed Effects	Yes	Yes	Yes	Yes	No	No	No
State-Specific Time Trends	No	No	Yes	Yes	Yes	Yes	No
State x Age Group Fixed Effects	No	No	No	Yes	No	Yes	No
Day x Age Group Fixed Effects	No	No	No	No	Yes	Yes	Yes
State x Day Fixed Effects	No	No	No	No	No	No	Yes

Source.—Emsi, 2020.

Notes.—This table reports the coefficients associated with regressions of log number of child care job postings on an indicator for whether there is any classroom group size regulation in effect, the inverse of average classroom group sizes, and the inverse of average child-to staff-ratios. Standard errors are clustered at the state $\times$ day level and the observations are unweighted.

Table 2.4. Estimates of the Impact of Regulations on the Demand for Head Start and Pre-K Labor

	Dep Var: ln(number of job postings)					
	(1)	(2)	(3)	(4)	(5)	(6)
Any Group Size Regulation	-0.006 (0.019)	0.001 (0.021)	0.020 (0.030)	0.020 (0.030)	0.020 (0.030)	0.020 (0.030)
(1/Group Size)	-0.002 (0.006)	0.004 (0.007)	0.006 (0.008)	0.006 (0.008)	0.006 (0.008)	0.006 (0.008)
(1/Child-to-Staff Ratio)	0.016 (0.013)	-0.004 (0.014)	-0.003 (0.016)	-0.003 (0.016)	-0.003 (0.016)	-0.003 (0.016)
Observations	27,948	27,948	27,948	27,948	27,948	27,948
Mean Daily No. of Job Postings	0.594	0.594	0.594	0.594	0.594	0.594
State-Level Controls: Time-Varying	Yes	Yes	Yes	Yes	Yes	Yes
State-Level Controls: Interactions	No	Yes	Yes	Yes	Yes	Yes
Age-Group Fixed Effects	Yes	Yes	Yes	No	No	No
State Fixed Effects	Yes	Yes	Yes	No	Yes	No
Day Fixed Effects	Yes	Yes	Yes	Yes	No	No
State-Specific Time Trends	No	No	Yes	Yes	Yes	Yes
State x Age Group Fixed Effects	No	No	No	Yes	No	Yes
Day x Age Group Fixed Effects	No	No	No	No	Yes	Yes

Source.—Emsi, 2020.

Notes.—This table reports the coefficients associated with regressions of log number of Head Start and pre-k job postings on an indicator for whether there is any classroom group size regulation in effect, the inverse of average classroom group sizes, and the inverse of average child-to staff-ratios. Standard errors are clustered at the state $\times$ day level and the observations are unweighted.

Table 2.5. Estimates of the Impact of Regulations on the Demand for Lead Teachers

	Dep Var: =1 if Job Posting is for a Lead Teacher		
	(1)	(2)	(3)
Any Group Size Regulation	0.040** (0.019)	0.049*** (0.019)	0.039** (0.019)
(1/Group Size)	-0.001 (0.007)	-0.004 (0.006)	-0.003 (0.006)
(1/Child-to-Staff Ratio)	-0.016* (0.009)	-0.024** (0.010)	-0.021** (0.010)
Dep Var Mean	0.792	0.792	0.792
Observations	49,045	49,045	49,045
State-Level Controls: Time-Varying	Yes	Yes	Yes
State-Level Controls: Interactions	No	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Day Fixed Effects	Yes	Yes	Yes
Job Posting Characteristics	No	No	Yes

Source.—Emsi, 2020.

Notes.—This table reports the coefficients associated with regressions of an indicator variable for whether a given job posting is for a lead teacher on an indicator for whether there is any classroom group size regulation in effect, the inverse of average classroom group sizes, and the inverse of average child-to staff-ratios. Standard errors are clustered at the state $\times$ day level and the observations are unweighted.

Table 2.6. Estimates of the Impact of Regulations on the Demand for Teachers with a BA

	Dep Var: =1 if Job Posting Requires a BA+		
	(1)	(2)	(3)
Panel A: Assistant Teacher Job Postings			
Any Group Size Regulation	0.068**	0.033	0.032
	(0.033)	(0.028)	(0.028)
(1/Group Size)	-0.012	-0.009	-0.009
	(0.008)	(0.007)	(0.007)
(1/Child-to-Staff Ratio)	0.011	0.008	0.008
	(0.011)	(0.013)	(0.013)
Dep Var Mean	0.044	0.044	0.044
Observations	8,684	8,684	8,684
State-Level Controls: Time-Varying	Yes	Yes	Yes
State-Level Controls: Interactions	No	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Day Fixed Effects	Yes	Yes	Yes
Job Posting Characteristics	No	No	Yes
Panel B: Lead Teacher Job Postings			
Any Group Size Regulation	-0.029	-0.040*	-0.042*
	(0.021)	(0.022)	(0.022)
(1/Group Size)	0.006	0.003	0.003
	(0.008)	(0.008)	(0.008)
(1/Child-to-Staff Ratio)	-0.005	-0.001	-0.001
	(0.011)	(0.012)	(0.012)
Dep Var Mean	0.173	0.173	0.173
Observations	40,360	40,360	40,360
State-Level Controls: Time-Varying	Yes	Yes	Yes
State-Level Controls: Interactions	No	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Day Fixed Effects	Yes	Yes	Yes
Job Posting Characteristics	No	No	Yes
Panel C: Lead Preschool-Age Teacher Job Postings			
Any Group Size Regulation	-0.069**	-0.088***	-0.088***
	(0.029)	(0.030)	(0.030)
(1/Group Size)	0.027**	0.029**	0.029**
	(0.011)	(0.012)	(0.012)
(1/Child-to-Staff Ratio)	-0.022**	-0.026*	-0.026*
	(0.011)	(0.013)	(0.014)
Dep Var Mean	0.161	0.161	0.161
Observations	13,090	13,090	13,090
State-Level Controls: Time-Varying	Yes	Yes	Yes
State-Level Controls: Interactions	No	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Day Fixed Effects	Yes	Yes	Yes
Job Posting Characteristics	No	No	Yes

Source.—Emsi, 2020.

Notes.—This table reports the coefficients associated with regressions of an indicator variable for whether a given job posting is for a lead teacher on an indicator for whether there is any classroom group size regulation in effect, the inverse of average classroom group sizes, and the inverse of average child-to staff-ratios. Standard errors are clustered at the state $\times$ day level and the observations are unweighted.

Table 2.7. Estimating the Probability of Job Posting Compliance with State Regulations

	Dep Var: =1 if Job Posting Complies with the State Educ Regulation		
	(1)	(2)	(3)
Panel A: Lead Teacher Job Postings			
Any Group Size Regulation	-0.025	-0.015	-0.051**
	(0.026)	(0.026)	(0.021)
(1/Group Size)	0.020**	0.015	0.011
	(0.010)	(0.009)	(0.007)
(1/Child-to-Staff Ratio)	-0.033***	-0.032***	-0.019*
	(0.012)	(0.012)	(0.011)
Dep Var Mean	0.667	0.667	0.667
Observations	20,295	20,295	20,295
State-Level Controls: Time Varying	Yes	Yes	Yes
State-Level Controls: Interactions	No	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Day Fixed Effects	Yes	Yes	Yes
Job Posting Characteristic	No	No	Yes
Panel B: Lead Preschool-Age Teacher Job Postings			
Any Group Size Regulation	-0.060	-0.088**	-0.083**
	(0.043)	(0.041)	(0.034)
(1/Group Size)	0.045***	0.046***	0.023**
	(0.015)	(0.015)	(0.011)
(1/Child-to-Staff Ratio)	-0.039***	-0.066***	-0.027**
	(0.014)	(0.015)	(0.012)
Dep Var Mean	0.684	0.684	0.684
Observations	6,947	6,947	6,947
State-Level Controls: Time Varying	Yes	Yes	Yes
State-Level Controls: Interactions	No	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Day Fixed Effects	Yes	Yes	Yes
Job Posting Characteristic	No	No	Yes

Source.—Emsi, 2020.

Notes.—This table reports the coefficients associated with regressions of an indicator variable for whether a given job posting complies with the state's education regulation for lead teachers on an indicator for whether there is any classroom group size regulation in effect, the inverse of average classroom group sizes, and the inverse of average child-to staff-ratios. Standard errors are clustered at the state $\times$ day level and the observations are unweighted.

Table 2.8. Effects of COVID-19 Health and Safety Regulations on Child Care Labor Demand

Panel A: All Child Care Job Postings	
Dep Var: $\ln(\text{number of job postings})$	
Standardized Index of 10 COVID-19 Regulations	0.007
	(0.005)
Dep Var Mean	3.509
Observations	41,922
Panel B: Lead Teacher Job Postings	
Dep Var: =1 if Job Posting is for a Lead Teacher	
Standardized Index of 10 COVID-19 Regulations	-0.004
	(0.005)
Dep Var Mean	0.792
Observations	49,045
Panel C: Assistant Teacher Job Postings	
Dep Var: =1 if Job Posting Requires a BA+	
Standardized Index of 10 COVID-19 Regulations	-0.008
	(0.006)
Dep Var Mean	0.044
Observations	8,685
Panel D: Lead Teacher Job Postings	
Dep Var: =1 if Job Posting Requires a BA+	
Standardized Index of 10 COVID-19 Regulations	0.004
	(0.006)
Dep Var Mean	0.173
Observations	40,360
Panel E: Lead Teacher Job Postings	
Dep Var: =1 if Job Posting Complies with the State Education Regulations	
Standardized Index of 10 COVID-19 Regulations	-0.002
	(0.008)
Dep Var Mean	0.667
Observations	20,296

Source.—Emsi

Notes.—The table reports the coefficients associated with regressions of different measures of child care job postings on an index of 10 COVID-19 health and safety regulations. Standard errors are clustered at the state-day level and the observations are unweighted.

Chapter 3

MATERNAL EMPLOYMENT RESPONSES TO PUBLICLY FUNDED FULL-DAY KINDERGARTEN IN ARIZONA

3.1 Introduction

Provision of publicly funded kindergarten varies across different states of the U.S. One significant source of this variation lies in differences in levels of provision of kindergarten across states. Parker et al. (2016) note that while some states – for example West Virginia and Oklahoma – have mandated the provision of full-day kindergarten financed by public/state funds¹, others (for example New Hampshire and Nebraska) have chosen to mandate only half-day kindergarten. Parker et al. (2016) also note how others like Iowa, New Jersey and Wyoming have chosen to not mandate any level of publicly funded kindergarten. Across states, policies also vary in the provision of public funds for kindergarten for constituent school districts which voluntarily choose to offer kindergarten (Loewenberg, 2017).

The state of Arizona provides an interesting and unique case in this context. Arizona has traditionally only mandated half-day kindergarten across public school districts. However, this policy has gone through considerable variation during the last 20 years (Libassi, 2014). The state of Arizona experimented with full-day kindergarten during the school years 2004-05 and 2009-10 (Figure 3.1). Libassi notes that Arizona legislators introduced bills mandating the provision of full-day kindergarten in the

¹As of 1998, Alabama, Arkansas, DC, Florida, Georgia, Louisiana, Mississippi, North Carolina, North Dakota, South Dakota, Texas, Vermont offered full-day kindergarten (Chmelynski, 1998).

poorest school districts. Selection of the poorest school districts for funding full-day kindergarten was based on the percentage of total student population eligible for free or reduced meals. For school year 2004-05, only those school districts were funded for full-day kindergarten in which at least 90 percent student population was receiving either free or reduced-price lunches. In school year 2005-06, full-day kindergarten was funded across school districts in which at least 80 percent student population was eligible for either free or reduced-price lunches. From school year 2006-07, all school districts were funded for full-day kindergarten². As Libassi further notes, funding for full-day kindergarten was abolished in March 2010 (for the 2010-11 school year) under the new administration. Later, Loewenberg (2017) notes that the governor of Arizona once again signed legislation to partially fund some of the poorest school districts in Arizona for full-day kindergarten from the 2018-19 school year.

Existing literature has found positive impacts of availability of full-day kindergarten on maternal employment (Cannon et al., 2006; Cascio, 2009; Gibbs, 2014), although many studies find that stronger positive impacts in employment are concentrated among certain subgroups of mothers, such as mothers who are single or have lower levels of skills (Cascio, 2009; Dhuey et al., 2021). For the case of Arizona, public provision of kindergarten is free of any costs for parents. Given that the free full-day kindergarten has the potential to substitute other paid child care settings, it is likely that full-day kindergarten could provide the required substitution effect for an increase in maternal employment. The paper is an attempt to find empirical evidence that

²For school year 2006-07, the then Arizona governor Napolitano decided to change the way how full-day kindergarten was being funded. From the school year 2006-07, the legislation increased the per-pupil funding weight of kindergarten – to amount to 62 percent of a regular elementary school pupil funding. Although, this funding was not at par with the elementary school grades, this allowed all school districts willing to offer full-day kindergarten an opportunity to offer six hours of full-day kindergarten.

supports this connection between full-day kindergarten availability and maternal employment.

By using a difference-in-differences specification, my estimation strategy exploits the variation in the timing of availability of full-day kindergarten in Arizona as compared to 39 other states which did not make any substantial changes to the availability of kindergarten. Other than Arizona, a number of states indeed legislate to change the mandatory length of kindergarten day during 2000-2010. As show in Table 3.1, 13 states (excluding Arizona) changed the availability or length of kindergarten day during this time period. These 13 states are excluded from the analysis. While Arizona did offer full-day kindergarten to age-eligible children during the treatment years, it cannot be assumed that full-day kindergarten treatment was actually taken up by families of five year old children. To estimate treatment compliance, I start by examining the treatment impact on enrollment in full-day kindergarten in a difference-in-differences setting based on data from the annual CPS³ October supplements from 2000 to 2010⁴. This analysis is followed up by an analysis of maternal employment in a similar difference-in-differences setting, based on data extracted from CPS monthly samples from September to May⁵ for each year from 2000-2010. Specifically, the paper targets mothers of age-eligible children within the State of Arizona, analyzing maternal employment outcomes, both on the extensive and the intensive margin. I follow up the baseline difference-in-differences specifications with two robustness tests. First, I use a triple-difference (DDD) estimator to examine maternal outcomes, where

³Current Population Survey (Flood et al., 2021).

⁴Post 2010, the policy environment with respect to kindergarten underwent rapid changes, making the time period unsuitable to be examine under a difference-in-differences setting.

⁵A period of September to May resonates closely with the school year.

the third difference⁶ lies across maternal outcomes during regular school-year months (Sept-May) as compared to maternal outcomes in summer months (June-August) when schools are not in session. Second, I use a basic synthetic control approach to examine if the overall employment estimates hold.

Existing literature suggests small positive impacts for mothers of age-eligible children, specifically for groups (e.g. single, low-skilled mothers) where substitution effect is larger than the income effect for maternal employment. This hypothesis is not only rooted in existing literature but is also suggested by the research approach taken in this paper. There are three concerns that indicate that my estimates are indeed lower-bound estimates. First, the intervention occurred at the intensive margin of kindergarten availability, it only expanded the length of kindergarten day. The state-funded half-day kindergarten – both before and after the intervention – already consisted of 2.5 hours per day (Libassi, 2014). The full-day kindergarten intervention added another 2.5-3.5 hours of time to this half-day kindergarten during the time of full implementation of the intervention (2007 to 2010). Second, as earlier described, only specific qualifying school districts were eligible for full-day funding during the phase-in (2004/05 school year - 2006/07 school year) of the intervention. Third, my analysis includes all mothers of age-eligible children for full-day kindergarten, regardless of treatment compliance⁷. Hence it is reasonable to assume that the estimates from this

⁶As is the case with the baseline difference-in-differences specifications, the first difference is between post and pre-treatment employment levels within Arizona, and the second difference lies between the post and pre-treatment employment levels in control states.

⁷While I present overall descriptive evidence of increased take-up of full-day kindergarten during the intervention, one cannot ensure perfect compliance, because there are mothers of children in the sample who were either outside the school-districts eligible to participate during 2004/05-2006/07, or who simply chose not to send their children to full-day kindergarten. I do not have sufficient data at this point of time to claim that everybody complied with the availability of the full-day kindergarten even when it was mandatory to be offered by school-districts.

study are lower-bound, intent-to-treat (ITT) effects. In this context, it is likely that indirect income effect for certain subgroups such as married or high-income mothers, remains higher than the substitution effect realized from the implicit subsidy of child care costs provided through full-day kindergarten.

For other groups however, such as mothers who are either single or have low-income or are low-skilled, one would expect that a child care subsidy in the form of free full-day kindergarten would increase the opportunity cost of the foregone income in case of not working. This means that one can expect stronger positive impacts on employment among these subgroups of mothers.

I contribute to the existing literature in two ways. First, I add Arizona based evidence to the growing literature on the labor supply impacts of full-day kindergarten. Second, I exploit the structure of the CPS monthly data, so that it closely resonates with the school year, an approach which might be helpful for future research under similar settings.

3.2 Theoretical Considerations

The theory of maternal labor supply decisions in relation to the availability and affordability of child care was first presented by Ribar (1992) and Connelly (1992) for the case of married mothers. Kimmel (1998) builds upon Ribar's and Connelly's work and presents a unified maternal labor supply model for single and married mothers. Maternal labor supply or labor force participation is a function of the standard utility maximization, where a mother maximizes her utility as a function of child care quality, market goods, and leisure (Kimmel, 1998). In the above model time allocation for the mother is constrained within 24 hours, the utility from child care depends on child

care prices (at a given acceptable quality), while the utility of market goods depends upon the hourly wage rate that a mother can acquire in the market (Kimmel, 1998). The respective utility that an individual derives from leisure and market goods is a function of hourly wage rates. Wage rates decide the accessibility of market goods. Wage rates also help an individual compare the respective utility obtained from work and leisure. Under the standard assumption that the labor force participation is not time constrained other than the total hours available in a day (Kimmel, 1998), it can be deduced that the relative employment elasticity to income and child care costs are significant factors in maternal labor supply decisions.

For a mother to participate in the labor force, the utility from labor force participation and the utility from child care quality must be higher than the foregone utility of leisure and material goods in order to pay for child care. For instance, if the hourly wage is lower than (or even for some cases not substantially higher than) child care prices per hour, the mother is not likely to participate in the labor force unless the realized quality of child care increases indirect utility⁸.

The utility driven from the consumption of full-day kindergarten is expected to be heterogeneous across various subgroups of mothers. Heterogeneous utility gains from kindergarten consumption across married and single mothers are driven by the respective strength of substitution and income effects. Ribar (1992) has argued that total family income directly impacts the utility maximization function for married mothers. For the case of married mothers, the substitution effect of full-day kindergarten subsidy is not as strong as it is for single mothers (Blau and Hagy, 1998; Tekin, 2007). In the absence of informal child care or child care subsidy, when publicly funded child care becomes available in the form of public school, subgroups

⁸Such as for example, the utility from child development

of mothers who are either single or low-skilled, are likely to substitute other child care arrangements with this source of care (Blau and Hagy, 1998; Tekin, 2007). Holding non-labor income constant (Ribar, 1992), it is also plausible that single mothers have more motivation to enter the labor force as compared to married mothers, who are more likely to have access to overall higher levels of household income.

Previous literature has shown that early childhood education and care (ECEC) costs are a major factor affecting maternal labor supply decisions (Baum, 2002; Blau and Tekin, 2007; Herbst, 2010a; Kimmel, 1998; Morrissey, 2017). Publicly available early childhood education (preschool, kindergarten etc.) is a highly subsidized, low-cost alternative to child care.

Provision of publicly funded, full-day kindergarten in connection with employment amongst mothers of age-eligible children has been studied by Cannon et al. (2006), Cascio (2009), Dhuey et al. (2021), and Gibbs (2014). All of the above studies find maternal labor supply impacts of full-day kindergarten, although they find heterogeneous employment outcomes for different subgroups of mothers. Cannon et al. (2006) find that mothers of children attending kindergarten are more likely to participate in the labor force. Cascio (2009) finds positive impacts on employment participation amongst single mothers of five-year-olds. Dhuey et al. (2021) do not find any significant impact on overall maternal employment. For single mothers however, they find strong evidence of a positive impact on maternal employment, both on the extensive as well as the intensive margin of work. Gibbs (2014) finds small but significant positive impacts on maternal employment outcomes.

Full-day kindergarten includes an implicit child care subsidy for age-eligible children. The theoretical linkages as well as the literature cited above suggest strong substitution effects from the availability of full-day kindergarten, as suggested by

increased employment responses among only subgroups of mothers such as those who are low-skilled or are single by marital status. For other maternal groups (married, or skill-specific distinction across mothers), the substitution effect from full-day kindergarten appears to be much weaker in the present literature.

3.3 Data Description

The primary source of data for this paper comes from the Current Population Survey (CPS) spanning school years 1999-00 to 2009-10 (Flood et al., 2021). I use CPS samples for the months September to May of each calendar year between 1999 and 2010. There is a compelling reason to do so. A typical school year runs from September to May. For this reason, the months from each CPS year are carefully matched to synchronize with the school year. For example, school year 1999-2000 (or school year 2000) is matched with CPS months Sept 1999 to May 2000. This allows for a more reliable estimate of enrollment changes for identification purposes.

For maternal labor supply analysis, the sample includes all mothers aged 20 to 55 with a youngest child aged 5. To be eligible for kindergarten enrollment in Arizona, a child must be 5 years of age. A child is deemed to be age 5 only if the child reached the age of 5 before September 1 of the respective school year. Hence it is logical that only mothers with age-eligible children are included in the analysis sample. In order to reliably isolate the treatment effects among mothers with age-eligible children, I exclude mothers of children aged 5 who have other children younger than 5 in the household, as their employment decisions might be affected by the younger child in the household. I also exclude mothers who are currently enrolled in any kind of schooling. Since Arizona started offering full-day kindergarten in school year 2005, this provides

me with five pre-treatment years before the start of the intervention, as a baseline for comparison. School year 2005 to school year 2010 are coded as the primary treatment years. I treat school years 2005-2010 as a single unified treatment era, to allow enough sample size of mothers with the youngest child aged 5, based on yearly observations from Arizona.

From school year 2011, Arizona abolished full-day kindergarten funding. However, even without state funding, many local school districts still continued to offer full-day kindergarten based on secondary funding sources, such as local property taxes. This brought about an era of uncertain and muddled policy environment, during which it is not really possible isolate the effect of one policy from another, at least under the current difference-in-differences framework. I therefore end my study period post school year 2009-10.

3.3.1 Full-Day Kindergarten Enrollment

One key challenge that comes with the use of CPS data is that only CPS-October education supplement differentiates across full-day kindergarten enrollment and half-day kindergarten enrollment. This information is not present for other months of CPS. For the analysis of maternal employment, I include all mothers who have their youngest child aged five, regardless of compliance with full-day kindergarten enrollment. To ensure that a sizeable proportion of five-year-olds in the state of Arizona complied with the intervention by enrolling in full-day kindergarten, a preliminary analysis of kindergarten enrollment based on the data from CPS-October supplement is needed.

In Figure 3.2, I use data extracted from the CPS-October education supplement to analyse how full-day vs half-day kindergarten enrollment changes over the years, and

particularly so over 2005-2010, the intervention years for this study. For each year on the observation, I use a moving average of two preceding years and the current CPS year, to smooth out the curve as the number of observations within a single year of data from Arizona are too few, and the resulting volatility induced by smaller sample size makes it a bit harder to interpret. The vertical dotted line in Figure 3.2 indicates the beginning year of the intervention. As one would expect, full-day kindergarten enrollment increases from 45% to close to 90% during the intervention years. Half-day kindergarten enrollment mirrors the trend, with a sharp decline over the same years. It is important to note that the full-day kindergarten enrollment rose as children who were earlier attending half-day kindergarten, moved to full-day kindergarten.

3.3.2 Treatment Compliance

While the State of Arizona offered full-day Kindergarten – it is not yet clear if families and parents do indeed respond by enrolling their children into full-day Kindergarten. There is a need to verify that the full-day kindergarten enrollment trends in Arizona are indeed driven by the intervention within Arizona and are not driven by any national or other region-specific trends in education.

Figure 3.3 graphs trends for full-day kindergarten enrollments in Arizona as compared to the control states during 2000-2010. Prior to the intervention, full-day kindergarten enrollment in Arizona stood around 20-25%, comparable to the national average across the 37 control states. Post-intervention, full-day kindergarten enrollment in Arizona gradually increases, peaking out around 2009 when 33% children (age 4 or age 5) attended full-day kindergarten. Post 2005, the increase in full-day kindergarten attendance across Arizona is remarkably higher than the control states,

but in the years prior to the treatment, trends across Arizona and the control states overlap and exhibit similar patterns.

To do so and make a stronger case for identification, I run a difference-in-differences (DD) specification for full-day kindergarten enrollment amongst five-year-olds, across the sample drawn from October CPS Education supplement during the study period 2000-2010. Specifically, I run a regression of the following form:

$$Y_{i,s,t} = \gamma(AZ_s X FDK_t) + X'_{i,s,t} + \theta_s + \zeta_t + \eta_{s,t} + \varepsilon_{i,s,t} \quad (3.1)$$

Where $Y_{i,s,t}$ is the key outcome variable, a binary indicator indicating if the five-year-old i in state s in year t is enrolled in full-day kindergarten or not, AZ is a binary indicator if individual i is located in the state of Arizona or not, FDK is a binary indicator indicating if the observation year for each observation lies within the time period of the treatment years (2005 to 2010). Key coefficient of interest lies on the interaction between the AZ and FDK indicators. I control for a matrix of maternal and family characteristics X' , including age, race, education levels, marital status and family size and income. State fixed effects (θ) control for time-invariant state fixed effects, while year fixed effects (ζ) control for year specific macroeconomic and relevant policy shocks that systematically affect all states in the country. To account for potential unobserved confounding factors that might correlate with the plausibly exogenous policy introduction of full-day Kindergarten, $\eta_{s,t}$ is a vector containing state-specific linear time trends.

3.3.2.1 Estimates for Treatment Compliance

The difference-in-differences estimates from equation 3.1 are presented in Table 3.2. Controlling for maternal and family demographics in column (2), the estimates indicate that the intervention led to an increase in full-day kindergarten enrollment in Arizona by 8.4 percentage points over 2005-2010. The estimates however exhibit a great degree of heterogeneity across different sub-populations. Large increases in full-day kindergarten enrollments are found among children of high skilled mothers (24 percentage points), single mothers (38.3 percentage points), White mothers (17.9 percentage points), and high income mothers (17.9 percentage points). For children of low-skilled, married, non-white, or low-income mothers, effects on full-day kindergarten enrollments are either absent or negative.

Children of high-skilled, high-income mothers have been traditionally more likely to attend nursery schools (Davis and Bauman, 2013) and child care (Baum, 2002). Previous literature also links full-day kindergarten availability with maternal employment of single mothers (Cascio, 2009; Dhuey et al., 2020). Davis and Bauman (2013) report that 25% of children enrolled in full-day kindergarten across the U.S. belonged to households with incomes higher than \$75000. Given these trends, many of these results are not surprising. For high-skilled and high-income mothers or single mothers, it is likely that the expected substitution effects from any subsequent increases in maternal employment are strong enough for them to enroll their children in full-day kindergarten. Since high-income households are also more likely to consume child care in absence of kindergarten (Anderson and Levine, 1999), it might be the case they move their children from child care to full-day kindergarten. Full-day kindergarten consumption across White and non-White mothers can also be attributed to the

income differences across these demographics. To summarize, there is enough evidence of a substantive increase in full-day kindergarten enrollment during the treatment years, although the estimates are heterogeneous across different sub-populations.

3.3.3 Measures of Maternal Labor Supply

There are two key outcome measures of maternal labor supply. First is the measure for employment which is derived from the employment status variable in the CPS dataset. This is a binary indicator variable coded as 1 when a mother is either at work during the week she was interviewed or is on granted leave. Everyone else including those out of work, out of labor force, retired or unable to work are coded as 0.

Mean female employment comparison trends over the study period (2000-2010) across Arizona and other states are presented in Figure 3.4. The vertical red lines in both parts of the graph separate pre-treatment years prior to 2004 and intervention years during 2005-10. The upper part of the graph consists of all females aged 20-55, regardless of maternal characteristics. This increases the sample size within Arizona to allow for more stable plotting. This graph shows that female employment trends in Arizona are identical to the national average, and female employment in Arizona has remained consistent throughout the time, although it has remained systematically below the national average. The sample for the lower part of the graph consists of mothers aged 20-55 with youngest child aged 5. The trends signal that on average, female employment among the sample of mothers with five year old children follows the national trend. However, since the number of mothers with the youngest child aged 5 is so small within a given year (in the CPS sample), the trend for Arizona is

too volatile in the lower part of the graph⁹. As expected employment among mothers with five-year-old children remains 5-10% lower than female employment in general, but the trends remain similar. Even with some volatility in the lower part of Figure 3.4, there is no obvious violation of parallel trends.

As explained above, the intervention only adds 2.5 hours to the half-day kindergarten. Theoretically, it is likely that if the full-day kindergarten expansion does affect female employment on the extensive margin, the magnitude of this effect would be small. I estimate a measure of intensive margin of labor supply by using a measure of the weekly number of hours worked by mothers with age-eligible children. The measure of weekly hours worked is a continuous variable which relies on the responses to the question in CPS which asks individuals about usual hours worked per week. Mean number of hours worked by females in Arizona are compared to females across all other states in Figure 3.5. The upper half of the graph is based on all females aged 20-55 regardless of maternal status, while the lower half consists of mothers with a youngest child aged 5. Once again, smaller sample confined to mothers with young children age 5 mean that the trend for Arizona in the lower part of the graph is too volatile, but for all mothers, the trend for Arizona mirrors the national average. For mothers with five-year-old children, the trends are more stable in three years immediately prior to the intervention. Similar to the trends on the extensive margin of labor supply, women in Arizona consistently trend below the national average on the intensive margin of labor supply as well. To be noted, it is only during the peak of full-day kindergarten availability (2007 or so) that maternal labor supply on the intensive margin peaks above the national average.

Besides these, a matrix of control variables is also constructed from the CPS data.

⁹To address this issue to some extent, I use a moving average of three years for plotting purposes.

CPS details race, marital status, education, income as well as total number of children for all individuals in the sample. Yearly state level unemployment data has been retrieved from the Bureau of Labor Statistics.

3.4 Empirical Strategy

Based on the timing of the full-day kindergarten Arizona, I use a difference-in-differences approach to estimate changes in maternal labor supply during the treatment. The control group for these estimates are 36 states (Table 3.1), which did not change the length of state-funded kindergarten during 2000 and 2010.

The key outcome of interest for this study is maternal employment on the extensive and intensive margin of labor force participation. I run a slightly modified form of the specification (1) in the following forms:

$$Y_{i,s,t} = \gamma(AZ_s X FDK_t) + X'_{i,s,t} + Z'_{s,t} + \theta_s + \zeta_t + \sigma_m + \eta_{s,t} + \varepsilon_{i,s,t} \quad (3.2)$$

$Y_{i,s,t}$ is the key outcome variable, a binary indicator indicating main employment outcomes which are employment and weekly hours for mothers. AZ is a binary indicator if individual i is located in the state of Arizona or not, FDK is a binary indicator indicating if the observation year for each observation lies within the time period of the treatment years (2005 to 2010). The key coefficient of interest lies on the interaction between the AZ and FDK indicators. I control for a matrix of maternal and family characteristics X' , including age, race, education levels, marital status as well as family size and income. Z' is a vector of time-variant state level control for unemployment. State fixed effects (θ) control for time-invariant state fixed effects, while year fixed effects (ζ) control for year specific macroeconomic and relevant policy

shocks that systematically affect all states in the country. To counter seasonal trends in employment, σ is a vector of month fixed effects. Threat to identification exist from unobserved variables which can vary across states over time – to counter this, η is a vector of state-specific linear time trends.

One important difference from specification 3.1 is that the specifications in 4.1 relies on the entire sample, not just the October-CPS. Here I am combining CPS months from September to May, corresponding to each school year. Although this convention of matching CPS monthly data to school-year is a bit novel, it is not the first instance in labor economics. Similar approach has been adopted in time-use studies for school-age students by Morisi (2008) and Porterfield and Winkler (2007). This strategy allows for a larger sample size to generate more precise estimates, and also rules out sampling bias from the estimates, specially so when I limit my analysis to any subgroup of mothers.

As discussed above, each specification contains state-specific linear time trends as well a control matrix of maternal socio-demographic characteristics. To account for seasonal variation in employment characteristics, I also include month fixed effects across all specifications. The sample consists of mothers aged 20 to 55, with a youngest kid aged 5. All mothers who have kids younger than 5 are excluded from this analysis. I cluster standard errors within state-specific cells, to account for state-specific trends in employment.

There are several limitations to this identification strategy. One of the most difficult one is the issue that I cannot trace each age-eligible child and his/ her mother to estimate Treatment-on-the-Treated (ToT) effects for maternal employment. However, given that I show robust evidence of program uptake in the above subsection, a major source of this threat to identification related to the compliance of treatment is removed.

Given data limitations, I am only able to generate lower-bound ITT estimates, but I have good reason to believe that ToT effects would not have been much different.

There are two identification threats in a difference-in-differences context. While the first assumption is of parallel trends, which requires that trends of treatment and control groups in pre-treatment outcomes should remain same. Another identification threat emerges from the fact that the Arizona economy might have been very different prior to the intervention, and full-day kindergarten might have been pushed by unobserved political and economic factors. Although I present some graphic evidence of these trends in section 2, and my difference-in-differences strategy relies on a bunch of demographic controls, control for time-variant state-level unemployment, two-way fixed effects as well as state-specific linear time trends to isolate the impact of the kindergarten expansion, these threats might nevertheless still exist to a limited extent.

Two strategies are frequently used to alleviate identification concerns. The first potential method is to use a triple-difference design. A triple-difference design has an advantage that it only requires that parallel trends across the two differences persist over time. If the time period of the intervention was a little brief a simple triple-difference design with mothers of 4 year or 6 year old children as a comparison group would have been ideal. But the structure of the data and the length of the Arizona kindergarten experiment does not allow me this luxury. For example, a 6 year old in 2006 would still have been treated in 2005, a 10 year old in 2010 would have been 5 in 2005, and still treated. On the other hand a 3 or 4 year old in 2005 would be likely to be treated under the intervention during 2007 or 2006. Larger age gaps between control and treatment groups are also not ideal.

However, there is one other way a triple-difference strategy can be taken under the current settings. Since the school year in the U.S. roughly lasts from September

to May, schools are usually not in session during the summer months of June, July and August, and hence children are not likely to be attending kindergarten during the summer months. I exploit this timing of the school year in my triple-difference setting. The third difference¹⁰ is provided by using the summer months (June-August) as a control period, and the school-year months (Sept-May) as a treated period.

I modify equation 4.1 in the following form to run triple-difference regressions for labor supply outcomes:

$$Y_{i,s,t} = \beta_1(FDK_t X Schyr_m) + \beta_2(AZ_s X Schyr_m) + \beta_3(AZ_s X FDK_t) + \gamma(AZ_s X FDK_t X Schyr_m) + X'_{i,s,t} + Z'_{s,t} + \theta_s + \zeta_t + \sigma_m + \eta_{s,t} + \varepsilon_{i,s,t} \quad (3.3)$$

AZ is a binary indicator if individual i is located in the state of Arizona or not, FDK is a binary indicator indicating if the observation year for each observation lies within the time period of the treatment years (2005 to 2010), and $Schyr_m$ is a binary indicator indicating if the observations occur during the school year ($=1$) or the summer months ($=0$). The key coefficient of interest is γ which lies on the triple interaction between the AZ , FDK and $Schyr$ indicators. The remaining structure of equation 3.3 is exactly identical to equation 4.1.

A second potential method to ensure robustness of estimates is to rely on matching or synthetic controls. First conceptualized by Abadie and Gardeazabal (2003) and further developed by Abadie et al. (2010), synthetic control methods have gained popularity over the recent past. Synthetic control methods help formulate a synthetic counterfactual of the treated unit, by closely matching outcome predictors in treated units to outcome predictors from a pool of donor units (control units in this case) – assigning a weight matrix to predictor variables from all donor units. Thus, a single

¹⁰Already used in equation 4.1, the first difference exists between pre and post-treatment Arizona, and the second difference lies across Arizona and untreated states.

placebo control group is generated which is a combination of all control groups, with predictor units aggregated according to assigned weights. The resulting counterfactual control group is very closely matched to the treated group during the pre-treatment years – and the combination of control groups present a counterfactual of treatment group in the absence of a treatment (Abadie et al., 2010). Abadie et al. (2010) further present methodological advantages of synthetic control groups in terms of (i) transparency of assigned weights to quantify exact contributions from each control group, and (ii) the explicit avoidance of extrapolation.

In the current study, synthetic control method is applied to Arizona as suggested in Abadie et al. (2010) and, Abadie and Gardeazabal (2003), leading to the generation of a counterfactual Arizona in the absence of the treatment. The procedure to generate a synthetic control group is further aided by a companion Stata software package “synth” developed by Abadie et al. (2011).

There are limitations with the application of synthetic controls and synth package in Stata software. A key limitation is that synthetic control is extracted from a balanced panel dataset – which rules out using microdata without aggregation. When data is aggregated, for each treated unit there is only one counterfactual or synthetic control available for comparison. On argumentative basis, estimates obtained from a single counterfactual can be spurious. The arising concerns from this limitation can however be reduced to some degree, by running placebo tests as suggested in Abadie et al. (2010). Using placebo tests, the significance of the estimate is tested by running iterative placebo tests – by generating placebo treatment units across other states. It is done by iteratively assigning each control state a placebo treatment one by one in an iterative manner and by generating counterfactuals for the placebo states, while putting Arizona in a donor pool in each iteration. As Abadie et al. (2010) suggest, if

the gap for the outcome variable(s) between Arizona and counterfactual Arizona is persistent or bigger than the gap for other placebo treated states, this should suffice as sufficient proof.

Indeed both synthetic controls and matching have similar issues, trading one form of bias for another (Kellogg et al., 2021). While synthetic controls minimize extrapolation bias (Abadie et al., 2010), they do it at the expense of interpolation bias (Kellogg et al., 2021), but propensity score matching has the exact opposite limitation. The selection of control states under the synthetic control method is relatively formal, and synthetic control has an added advantage that re-weighting on pre-treatment outcomes and control variables corrects for potential issues with common trends assumption under a difference-in-difference setting (Bonander et al., 2021).

3.5 Results

Difference-in-differences results for employment and weekly hours are presented in Table 3.3 and Table 3.4. Mean pre-treatment employment statistics for each sample are included in each table to form a point of reference.

Column 1 in Table 3.3 presents the baseline OLS specification, while column 2 adds demographic and state level controls. For estimates on the extensive margin of employment during the intervention, column 2 in Table 3.3 indicates a 2.5 percentage point increase in employment for the overall sample. For high-skilled mothers, there is a 5.8 percentage point increase in maternal employment, for single mothers there is a huge 16.7 percentage point increase, and low-income mothers experience an increase in employment by 4.2 percentage points. Surprisingly non-White mothers also exhibit

a substantial 8.2 percentage point increase in employment. For separate subgroups that include low-skilled mothers, White mothers, and high-income mothers, there are no changes in employment, and for married mothers there is a 3.3 percentage point reduction in employment.

As expected, the results are heterogeneous across different groups. It could be the case for low-skilled mothers, the substitution effect is not as strong to motivate them for employment. Also, the full-day enrollment response in children of low-skilled mothers was slightly negative in column 4 of Table 3.2, which might explain lack of employment effect to some extent. For high-skilled mothers however, there is a statistically significant, 5.8 percentage point increase in employment. Given that mean pre-treatment employment levels for high-skilled mothers are so high to begin with (72.5%), this is a substantive effect, although it is only around one fourth of the 24 percentage point increase in full-day enrollment response among children of high-skilled mothers in Table 3.2. This signals that for high-skilled mother the substitution effect is quite substantial. For married mothers there is a slightly negative employment effect, a 3.3 percentage point decrease. For married mothers, with access to other sources of household income, the income effect can be greater than the substitution effect, which explains this decline. For single mothers however, I find the largest employment response, an increase of 16.7 percentage points. Again, this was expected as indicated by previous research on the topic (Cascio, 2009). In comparison, there was a 38.3 percentage point increase in full-day enrollment response among children of single mothers in Table 3.2, almost twice the effect size of employment. Mean treatment employment level among single mothers was already at 77.6%, which signals that substitution effect dominates income effect for the case of single mothers. For mothers of non-White origin, there is a positive employment effect of 8.2 percentage

points, while the mean pre-treatment employment levels were around 62%. For mothers of White origin, there is no employment effect, while the mean pre-treatment employment levels were around 70%. Given that there was no effect on full-day kindergarten enrollments across children of non-White origin, and there was a large, positive effect (17.9 percentage points) on full-day kindergarten enrollments across children of White origin in Table 3.2, these estimates are a bit surprising. For mothers of White origin, the already high employment levels and likelihood of higher income effects can explain the absence of any employment effect. The positive treatment effect for employment among non-White mothers is more puzzling. In the analysis sample, around 38 percent of non-White mothers are single, while only 22.5 percent mothers of White origin are single. Given that single mothers are more likely to experience a higher substitution effect, this could be driving the positive employment effect among non-White mothers. This argument is more accurate for the case of low-income mothers. While the employment effect for low-income mothers is a statistically significant increase of 4.2 percentage points (from a mean pre-treatment employment level of about 60%), the employment effect for high-income mothers is more modest, a statistically insignificant increase of 2.6 percentage points. But then again, in our sample only 10 percent of high income mothers are single, but among low-income mothers, 40 percent are single.

Difference-in-differences estimates for weekly number of hours worked, i.e. the intensive margin employment estimates are presented in Table 3.4. Overall, I find a small but statistically insignificant impact on number of hours worked, the point estimate showing an increase of 0.83 hours per week over a baseline of 23.32 mean number of hours worked per week. Only subgroups such as single mothers or those mothers who are non-White, show an increase in number of hours worked per week.

Specifically, for single mothers, there is a 4.2 units (hours) increase in the number of weekly hours, and for non-White mothers there is a 1.9 units (hours) increase in weekly hours. These estimates are also expected. In a majority of cases, labor force participants can presumably choose whether to work or not, but only a few have control over the number of hours that they work. This explains why the intensive margin estimates are more modest as compared to the employment estimates.

3.5.1 Triple-difference estimates

Triple-difference estimates for employment and weekly number of hours are presented in Table 3.5 and Table 3.6 respectively. In Table 3.5, the coefficients of interest on the triple interaction capture the difference across the treatment effect for sample population during the school-year months (Sept-May) and the treatment effect for sample population during the summer months (June-July). The triple-difference estimate would thus depend on the sensitivity levels of the treatment effects with respect to the summer months, when schools are closed.

The overall maternal employment estimate in column 2 of Table 3.5 shows a 5.1 percentage point increase in employment. This goes on to show that for the overall sample, the estimate in column 2 of Table 3.3 is almost exclusively driven by a strong employment effect in the months when school is in session. This also lends strong credibility to the fact that increases in employment among mothers of 5- year-olds are indeed linked with the offering of full-day kindergarten in the state.

For most sub-populations, the triple-difference employment estimates in Table 3.5 are positive and statistically significant for the following subgroups of mothers: (i) high-skilled mothers, (ii) married mothers, (iii) non-White mothers, (iv) White

mothers, (v) low-income mothers and (vi) high-income mothers, the effect size ranging from 3.3 to 8.2 percentage points. The positive triple-difference estimates for high-skilled, non-white and low-income mothers are in line with the double differences estimates in Table 3.3, and somewhat expected, giving my identification assumption that these employment estimates are indeed driven by FDK enrollment, which is only possible during the months when school-year is in session.

The estimates that examine heterogeneous effects across single mothers and married mothers were the most surprising. It appears that married mothers had a strong income effect during the school year, and it dominated the substitution effect. During the summer months, the substitution effect goes away, but because the summer months in this setting is just a very short window, married mothers do not realize the existing strong income effect due to reduced child care expenses has also gone away. Even if they realize it, they might not be able to get back into employment just for 3 months of private child care costs. For single mothers, it appears that the substitution effect is so dominant that a short window of summer months does not motivate them to leave employment. I run the difference-in-difference regression (similar to equation 4.1) for single as well as married mothers separately over summer and school-year months to test this, and found this to be correct. The point estimates for single mothers are exactly identical for summer and school year months, but not so for married mothers. For high-income mothers, the positive triple-difference estimates are also somewhat unexpected. For the case of high-income mothers, the point estimate in Table 3.3 was positive though borderline insignificant, which can explain why the treatment effects could have been stronger in school-year months, and could have been weaker in the summer months, which would make the triple-difference estimate positive and statistically significant.

Triple-difference estimates on the intensive margin are presented in Table 3.6. Analogous to the estimates on the extensive margin, estimates in column (1) and column (2) for weekly hours are statistically significant and almost double in magnitude as compared to the baseline specifications in Table 3.4. Column 2 of Table 3.6 shows a 2.1 hours increase in weekly hours worked. Across all sub-populations of analysis, the triple-difference intensive margin estimates are positive and statistically significant, ranging from a 1 hours increase to a 4.15 hours increase in weekly hours. The highest increase in weekly hours are seen for the following, separate subgroups of mothers: (i) single mothers, (ii) non-White mothers and, (iii) high-income mothers. The estimates for the subgroup of single mothers and the subgroup of non-White mothers are inline with the baseline estimates in Table 3.4. triple-difference estimates stronger for cases when the double differences estimates were statistically insignificant in Table 3.4. Once again, this lends further credibility to the identification assumption that increases in maternal employment are indeed driven by the full-day intervention.

3.5.2 Synthetic Control Method

Using synthetic control method, I construct a synthetic Arizona¹¹ for the overall employment outcome for mothers of 5-year-olds. The employment trend across Arizona and synthetic Arizona is plotted in panel A of Figure 3.6. The trends overlap each other during the pre-treatment but post treatment there is a persistent 1 to 2 percentage point difference across Arizona and synthetic Arizona. This estimate is quite close to the 2.5 percentage point increase in employment (column 2, Table 3.3).

To provide further robustness to the synthetic control, I run placebo tests by

¹¹The constituent control states and corresponding weights are available with me.

assigning treatment to the six neighboring states of Arizona which include California, Nevada, Utah, Colorado, New Mexico and Texas. The treatment effects are plotted in panel B of Figure 3.6. The black line presents the state of Arizona and the six gray lines present placebo treatment states. While many placebo treatments show some increase post the treatment year, it is only for the case of Arizona that the treatment effect remains positive and consistent over time from 2005-2010.

3.6 Conclusion

My estimates confirm that full-day kindergarten enrollment increased as a response to the intervention, signalling treatment compliance, although the estimates for compliance are heterogeneous across different sub-populations. The first finding is that for the overall sample as well as sub-populations of single or high-skilled mothers, increases in full-day kindergarten enrollment map onto increases in employment, although the magnitudes of treatment effects for employment are considerably smaller. This finding is consistent with the observation that Gibbs (2014) makes in her study on full-day kindergarten impacts in Indiana.

Difference-in-differences estimates for the intervention period are not surprising. The most relevant work in this regard includes that of Cascio (2009) and Gibbs (2014). On the extensive margin, I find positive employment estimates for the following subgroups: (i) single mothers, (ii) high-skill mothers, (iii) non-White mothers, and (iv) low-income mothers. For single mothers, Cascio (2009) also finds the highest treatment effects, although she does not study skill, income or ethnicity-based heterogeneity. Indeed Cascio (2009) finds negative but statistically insignificant negative treatment effects for married mothers, while the negative employment estimate for married

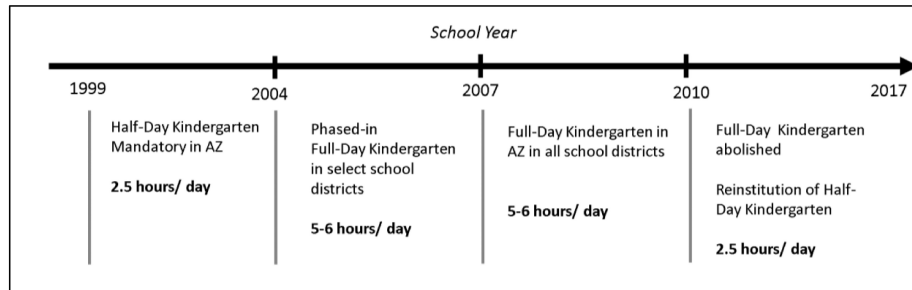
mothers in my study is statistically significant. On the intensive margin of employment, I find positive treatment effects for weekly hours for the subgroup of single mothers and the subgroup of non-white mothers, Cascio (2009) finds the same for single mothers. Similarly, Dhuey et al. (2020) finds positive treatment effects for single mothers, both on the extensive and intensive margin on employment. Although my estimates resonate more closely with what Cascio (2009) finds, the results are not much different from Gibbs (2014).

In sub-populations for which positive treatment effects are found, the extensive margin estimates across sub-population range from an increase of 4.2 to 16.7 percentage points, which are quite a big effect in the context of (i) existing higher levels of employment amongst these subgroups and (ii) these are lower-bound intent-to-treat estimates. Also, the increase in availability of full-time kindergarten occurs only on the intensive margin – from half-day to full-day kindergarten – which adds a mere two or so hours of free kindergarten each day. However, this is not surprising as these groups are more likely to experience dominant substitution effect from the availability of kindergarten. A slightly unexpected estimate is the employment estimate for the overall sample, which is a smaller, yet statistically significant positive effect of 2.5 percentage points. Although this impact is not totally surprising, this effect is not that small in the context of a small increase in the availability of full-day kindergarten.

The triple differences framework in this paper is an innovative use of the school-year cycle, and helped me test my identification assumption. For the majority of the employment estimates on the extensive as well as the intensive margin, the triple estimates lend credibility to the argument that maternal employment does respond to the availability of full-day kindergarten. Finally, synthetic control method also confirms the initial findings from the baseline double differences estimations.

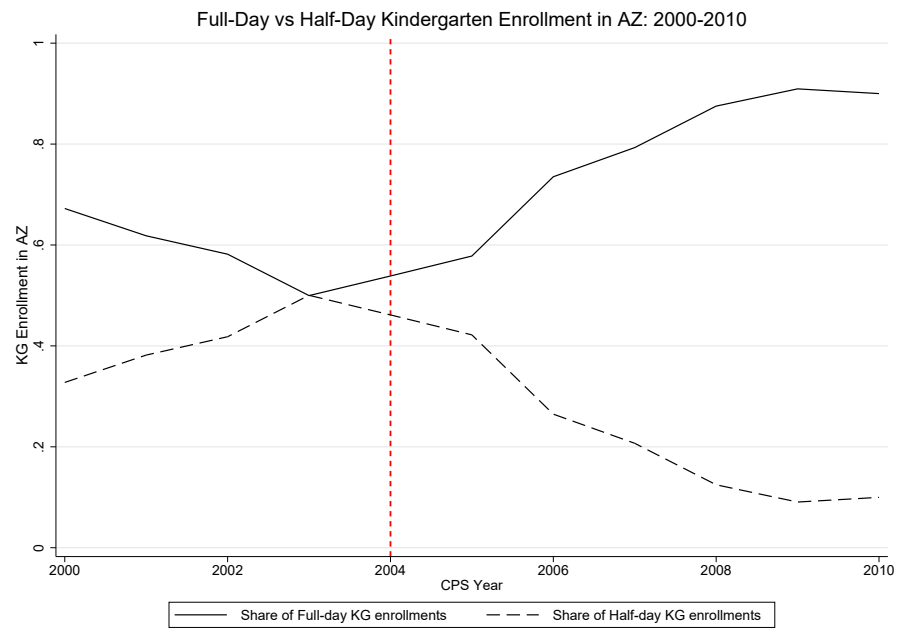
This paper was an attempt to identify if there are any maternal employment effects of full-day kindergarten availability in Arizona during 2005-2010. I present robust evidence that there are indeed positive treatment effects both on the extensive as well as intensive margins of employment, although these are driven by certain sub-populations, such as mothers who are either single, or those who are non-White or those who are low-skilled. Most of these findings are consistent with the existing literature. I add to the literature by providing Arizona based evidence to the full-day kindergarten literature on employment effects. In addition, I pay attention to heterogeneity across skill, income, and ethnicity groups, which is relatively new in the context of this body of literature. The triple differences strategy is novel and innovative and strengthens the credibility of estimates from my paper as well as previous literature. I provide further evidence of robustness through a limited application of the synthetic control method.

Figure 3.1. Timeline of Kindergarten Expansion in Arizona



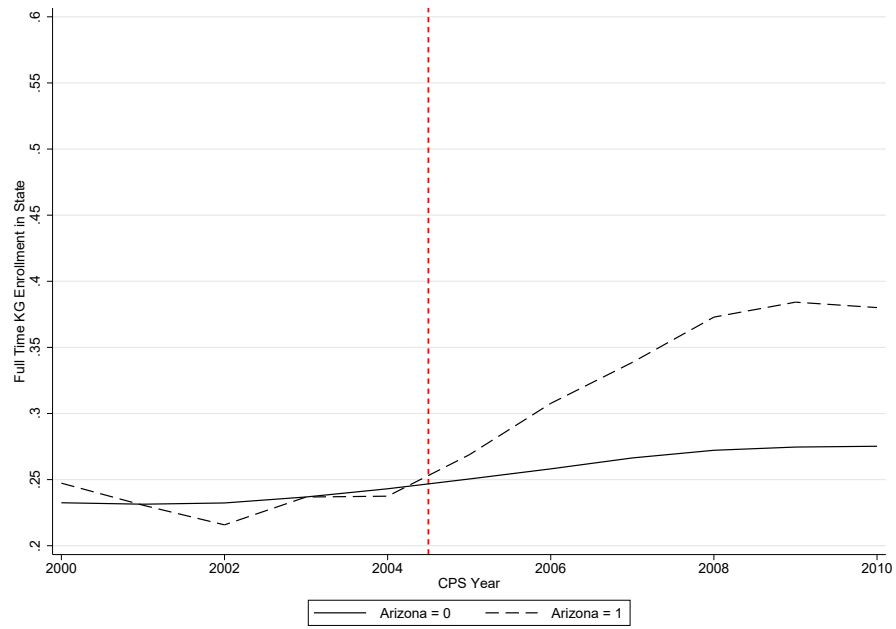
Source: Libassi (2014)

Figure 3.2. Enrollment Impacts of Kindergarten Expansion in Arizona



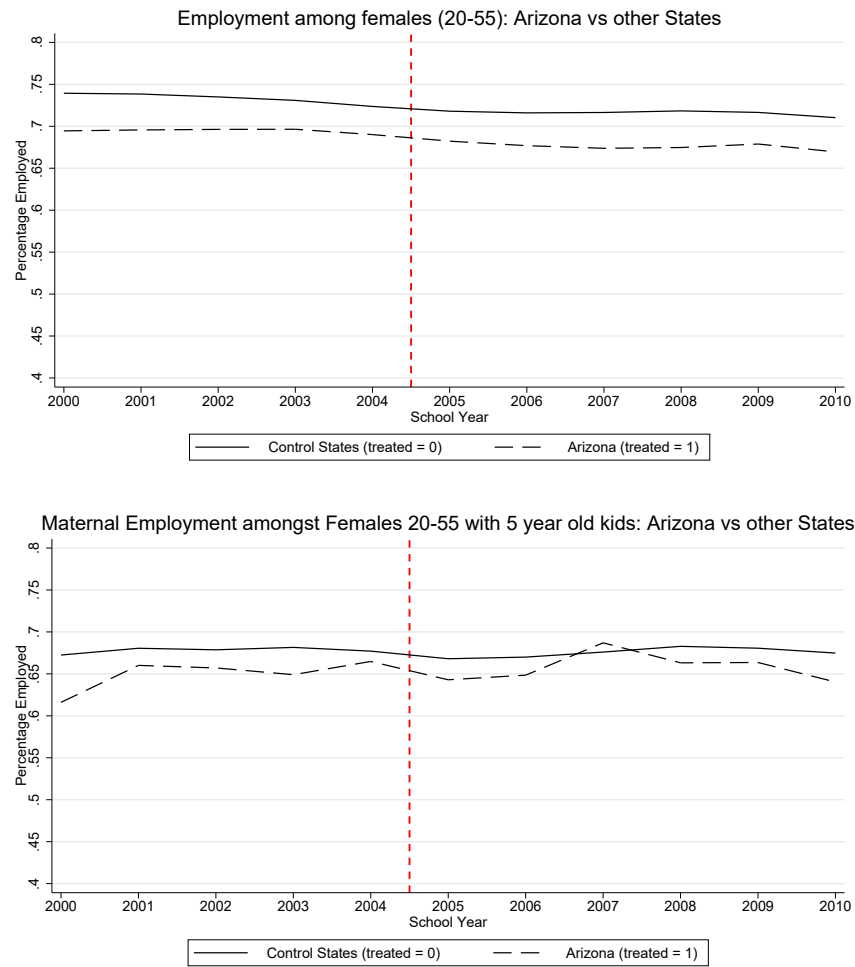
Source: Flood et al. (2021)

Figure 3.3. Full-day Kindergarten Enrollment



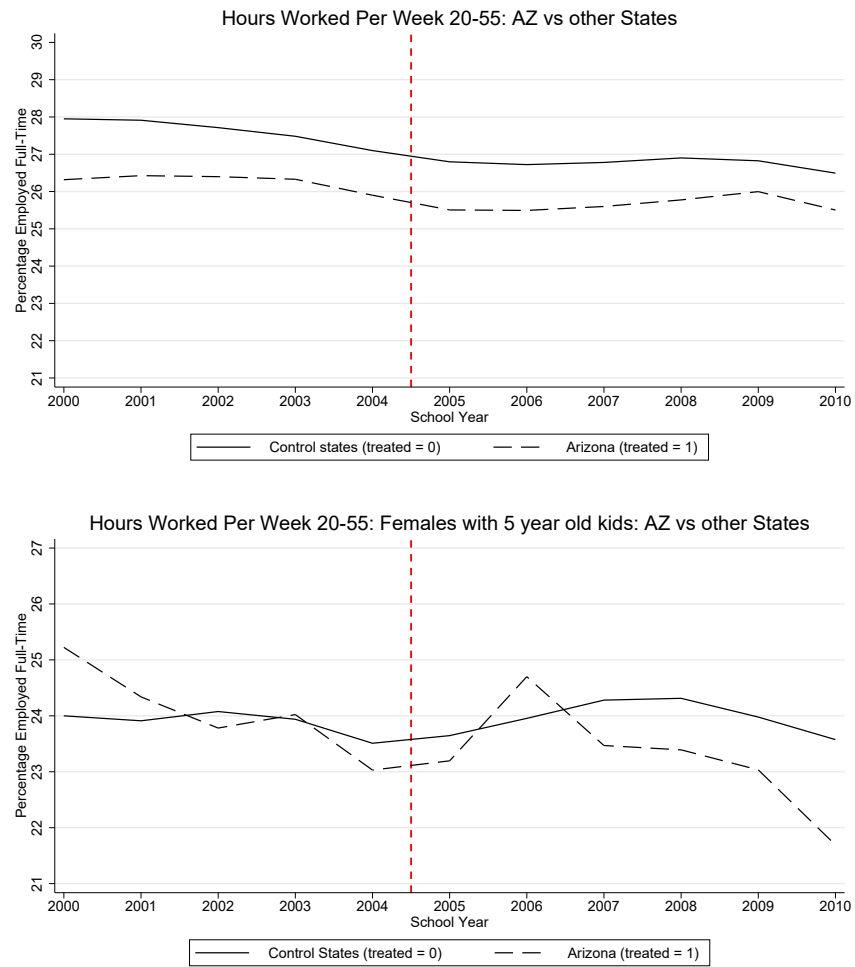
Source: Flood et al. (2021)

Figure 3.4. Employment among Females: Arizona vs Other States



Source: Flood et al. (2021)

Figure 3.5. Weekly Hours Worked among Females: Arizona vs Other States



Source: Flood et al. (2021)

Table 3.1. State Mandated Levels of Kindergarten Offered

State	Level of Kindergarten	
	2000	2010
Alabama	Full-day	Full-day
Alaska	–	–
Arizona	Half-day	Full-day
Arkansas	Full-day	Full-day
California	Half-day	Half-day
Colorado	–	–
Connecticut	Half-day	Half-day
Delaware	Half-day	Half-day
District of Columbia	–	Half-day
Florida	Full-day	Half-day
Georgia	Full-day	Full-day
Hawaii	Full-day	Half-day
Idaho	–	–
Illinois	Half-day	Half-day
Indiana	Half-day	Half-day
Iowa	Half-day	Half-day
Kansas	–	Half-day
Kentucky	Half-day	Half-day
Louisiana	Full-day	Full-day
Maine	–	Half-day
Maryland	Half-day	Full-day
Massachusetts	Half-day	Half-day
Michigan	–	–
Minnesota	Half-day	Half-day
Mississippi	Full-day	Full-day
Missouri	Half-day	Half-day
Montana	Half-day	Half-day
Nebraska	Half-day	Half-day
Nevada	Half-day	Half-day
New Hampshire	–	–
New Jersey	–	–
New Mexico	Half-day	Half-day
New York	–	–
North Carolina	Full-day	Full-day
North Dakota	Full-day	–
Ohio	Half-day	Half-day
Oklahoma	Half-day	Half-day
Oregon	Half-day	Half-day
Pennsylvania	Half-day	–
Rhode Island	Half-day	Half-day
South Carolina	Full-day	Full-day
South Dakota	Full-day	Half-day
Tennessee	Half-day	Half-day
Texas	Full-day	Half-day
Utah	Half-day	Half-day
Vermont	Full-day	Half-day
Virginia	Full-day	Half-day
Washington	–	Half-day
West Virginia	Full-day	Full-day
Wisconsin	Half-day	Half-day
Wyoming	Half-day	Half-day

Source.— National Center for Education Statistics (2022)

Table 3.2. DD Estimates for Full Day Kindergarten Enrollment in 5 year olds

	Dep Var: Full Day Kindergarten Enrollment=1									
	(1) OLS	(2) OLS+	(3) LS	(4) HS	(5) Married	(6) Single	(7) Non-White	(8) White	(9) LowInc	(10) HighInc
AZ X FDK intervention	0.140*** (0.019)	0.084*** (0.020)	-0.070** (0.034)	0.240*** (0.022)	-0.002 (0.024)	0.383*** (0.039)	-0.029 (0.048)	0.179*** (0.027)	-0.022 (0.034)	0.268*** (0.031)
Mean Pretreatment Enrollment	0.205	0.205	0.247	0.158	0.212	0.187	0.214	0.192	0.234	0.151
Observations	13994	13994	5586	8408	10472	3522	5084	8910	8252	5742
Demographic Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Specific Linear Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source.—Current Population Survey October Supplement (Flood et al., 2021)

Notes.—*** p<0.01, ** p<0.05, * p<0.1

The table reports coefficients and standard errors associated with difference-in-differences regressions of full day kindergarten enrollment on a binary indicator interacting an indicator for the state of Arizona with an indicator for the years in which full day kindergarten was available in Arizona. Demographic controls include mothers age, race, Hispanic origin, detailed marital status, educational achievement, total number of children in the house, and nativity status. All regressions include state and year fixed effects, as well as state specific linear time trends. Standard errors are clustered within states.)

Table 3.3. DD Employment Estimates for Mothers of 5 year olds

	Dep Var: Mother is employed == 1									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	OLS	OLS+	LS	HS	Married	Single	Non_White	White	LowInc	HighInc
AZ X FDK intervention	0.044*** (0.013)	0.025** (0.011)	0.001 (0.030)	0.058*** (0.016)	-0.033** (0.015)	0.167*** (0.028)	0.082*** (0.019)	-0.019 (0.017)	0.042** (0.020)	0.026 (0.017)
Mean Pretreatment Employment	0.659	0.659	0.587	0.725	0.617	0.776	0.619	0.696	0.604	0.744
Observations	66698	66698	27303	39395	48078	18620	22599	44099	38867	27831
Demographic & State Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Specific Linear Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source.—Current Population Survey (Flood et al., 2021)

Notes.—*** p<0.01, ** p<0.05, * p<0.1

The table reports coefficients and standard errors associated with difference-in-differences regressions of employment on a binary indicator interacting an indicator for the state of Arizona with an indicator for the years in which full day kindergarten was available in Arizona. Demographic controls include mothers age, race, Hispanic origin, detailed marital status, educational achievement, total number of children in the house, and nativity status. Time-variant state level control includes yearly unemployment. All regressions include state, year and month of the year fixed effects, as well as state specific linear time trends. Standard errors are clustered within states.)

Table 3.4. DD Estimates for Mothers of 5 year olds: Weekly Hours

	Dep Var: Weekly Hours Worked									
	(1) OLS	(2) OLS+	(3) LS	(4) HS	(5) Married	(6) Single	(7) Non_White	(8) White	(9) LowInc	(10) HighInc
AZ X FDK intervention	1.729** (0.647)	0.832 (0.509)	1.733 (1.245)	0.615 (0.723)	-0.627 (0.652)	4.274*** (1.283)	1.953** (0.771)	0.159 (0.829)	1.315 (0.854)	1.553 (0.990)
Mean Pretreatment Hours	23.32	23.32	20.53	25.96	20.79	30.25	23.21	23.43	21.05	26.87
Observations	63012	63012	25938	37074	45427	17585	21685	41327	36705	26307
Demographic & State Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Specific Linear Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source.—Current Population Survey (Flood et al., 2021)

Notes.—*** p<0.01, ** p<0.05, * p<0.1

The table reports coefficients and standard errors associated with difference-in-differences regressions of weekly hours on a binary indicator interacting an indicator for the state of Arizona with an indicator for the years in which full day kindergarten was available in Arizona. Demographic controls include mothers age, race, Hispanic origin, detailed marital status, educational achievement, total number of children in the house, and nativity status. Time-variant state level control includes yearly unemployment. All regressions include state, year and month of the year fixed effects, as well as state specific linear time trends. Standard errors are clustered within states.)

Table 3.5. DDD Employment Estimates for Mothers of 5 year olds

	Dep Var: Mother is employed == 1									
	(1) OLS	(2) OLS+	(3) LS	(4) HS	(5) Married	(6) Single	(7) Non_White	(8) White	(9) LowInc	(10) HighInc
AZ × School-Year Months	-0.045*** (0.009)	-0.028*** (0.007)	-0.028* (0.014)	-0.036*** (0.013)	-0.030*** (0.007)	-0.049*** (0.015)	-0.021 (0.014)	-0.028** (0.011)	-0.043*** (0.010)	0.009 (0.010)
FDK × School-Year Months	0.004 (0.012)	0.007 (0.011)	-0.013 (0.022)	0.016 (0.016)	0.007 (0.013)	0.008 (0.016)	0.000 (0.027)	0.011 (0.012)	0.014 (0.015)	-0.002 (0.013)
FDK × AZ	-0.019 (0.013)	0.003 (0.015)	0.017 (0.024)	-0.003 (0.018)	-0.052** (0.022)	0.173*** (0.029)	0.045 (0.028)	-0.032 (0.023)	0.062** (0.025)	-0.068*** (0.025)
FDK × AZ × School-Year Months	0.088*** (0.012)	0.051*** (0.009)	0.031 (0.024)	0.082*** (0.016)	0.058*** (0.012)	0.021 (0.018)	0.056*** (0.019)	0.045*** (0.013)	0.033** (0.013)	0.056*** (0.017)
Mean Pretreatment Employment	0.659	0.659	0.587	0.725	0.617	0.776	0.619	0.696	0.604	0.744
Observations	81170	81170	32610	48560	58487	22683	27670	53500	47255	33915
Demographic & State Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Specific Linear Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source.—Current Population Survey (Flood et al., 2021)

Notes.—*** p<0.01, ** p<0.05, * p<0.1

The table reports coefficients and standard errors associated with triple differences regressions of employment on a indicator which is a three-way interaction between: (i) an indicator for the years in which full day kindergarten was in operation, (ii) an indicator for the state of Arizona, and (iii) an indicator for the months of regular school year Sept to May, conditional on demographic controls, and a combination of fixed effects. Demographic controls include mothers age, race, Hispanic origin, detailed marital status, educational achievement, total number of children in the house, and nativity status. Time-variant state level control includes yearly unemployment. All regressions include state, year and month of the year fixed effects, as well as state specific linear time trends. Standard errors are clustered within states.)

Table 3.6. DDD Estimates for Mothers of 5 year olds: Weekly Hours

	Dep Var: Weekly Hours Worked by Mother									
	(1) OLS	(2) OLS+	(3) LS	(4) HS	(5) Married	(6) Single	(7) Non_White	(8) White	(9) LowInc	(10) HighInc
AZ × School-Year Months	-0.314 (0.359)	-0.049 (0.282)	-0.431 (0.627)	0.020 (0.465)	0.259 (0.318)	-1.884*** (0.610)	-2.038*** (0.658)	1.195** (0.448)	-0.312 (0.372)	0.529 (0.470)
FDK × School-Year Months	0.243 (0.564)	0.449 (0.519)	-0.918 (0.864)	1.297* (0.644)	0.511 (0.565)	0.313 (0.778)	-0.211 (1.109)	0.818 (0.591)	0.482 (0.633)	0.388 (0.592)
FDK × AZ	-0.380 (0.466)	0.003 (0.534)	1.766** (0.793)	-1.035 (0.689)	-0.667 (0.839)	2.373* (1.366)	-0.837 (1.028)	-0.056 (0.877)	2.292** (0.883)	-2.752** (1.347)
FDK × AZ × School-Year Months	3.398*** (0.571)	2.110*** (0.459)	1.822* (1.006)	2.816*** (0.682)	1.246** (0.521)	3.945*** (0.815)	4.152*** (0.876)	1.190* (0.618)	1.015* (0.567)	3.001*** (0.790)
Mean Pretreatment Hours	23.32	23.32	20.53	25.96	20.79	30.25	23.21	23.43	21.05	26.87
Observations	76671	76671	30931	45740	55239	21432	26506	50165	44633	32038
Demographic & State Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Specific Linear Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

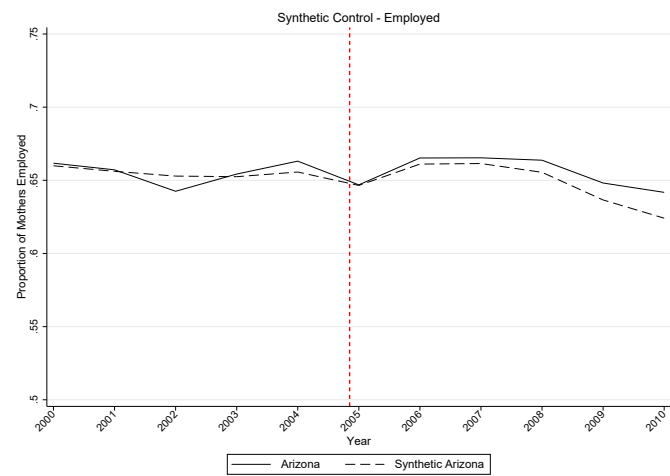
Source.—Current Population Survey (Flood et al., 2021)

Notes.—*** p<0.01, ** p<0.05, * p<0.1

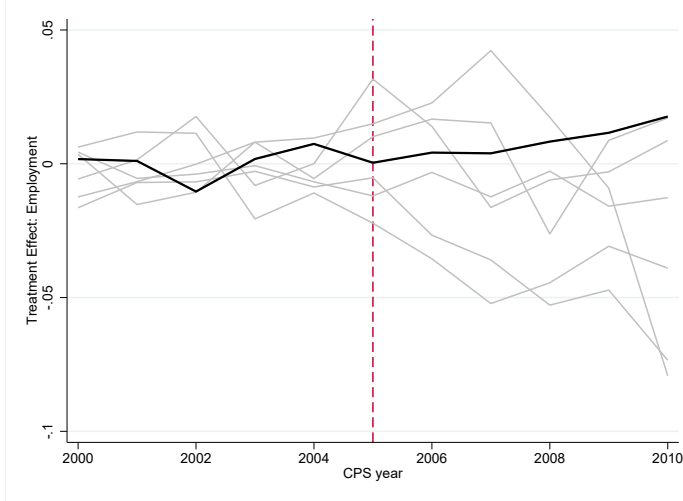
The table reports coefficients and standard errors associated with triple differences regressions of weekly hours worked on a indicator which is a three-way interaction between: (i) an indicator for the years in which full day kindergarten was in operation, (ii) an indicator for the state of Arizona, and (iii) an indicator for the months of regular school year Sept to May, conditional on demographic controls, and a combination of fixed effects. Demographic controls include mothers age, race, Hispanic origin, detailed marital status, educational achievement, total number of children in the house, and nativity status. Time-variant state level control includes yearly unemployment. All regressions include state, year and month of the year fixed effects, as well as state specific linear time trends. Standard errors are clustered within states.)

Figure 3.6. Employment of Mothers with youngest child aged 5: Synthetic Control Method

(a) Panel A: Arizona & Synthetic Arizona



(b) Panel B: Placebo Treatments



Source: Current Population Survey (Flood et al., 2021)

Chapter 4

NATIVE FLIGHT RESPONSES TO IMMIGRATION: EVIDENCE FROM K-12 SCHOOL ENROLLMENTS

4.1 Introduction

The share of immigrants in the U.S. has grown rapidly from 4.7% in 1970 to 13.7% in 2019 (Migration Policy Institute, 2022a). During this time the total number of immigrants in the U.S. grew more than four times (Figure 4.1). The share of children under 18 in the U.S. population living with at least 1 immigrant parent rose sharply from 13.4% in 1990 to 26% in 2017 (Migration Policy Institute, 2022a)¹. A demographic change of this extent has substantial social, political and economic consequences.

For example, the impacts of immigration have been extensively studied in relation to the U.S. labor market (Altonji and Card, 1991; Foged and Peri, 2015; Llull, 2017; Ottaviano and Peri, 2012); native women’s fertility (Furtado, 2015, 2016); social welfare programs (Borjas and Hilton, 1996; Borjas and Trejo, 1991), urbanization (James et al., 1998), healthcare (Lowell and Gerova, 2004) and educational achievement across different groups Jensen and Rasmussen (2011).

Altonji and Card (1991) were the first to provide empirical evidence on the impact of immigration on low-skilled natives². While Altonji and Card (1991) found immigration

¹The share of first-generation immigrant children in the U.S. has steadily declined over the past two decades, as per Migration Policy Institute, the term first generation immigrant however includes only those who are born outside the U.S.

²Throughout this paper, “natives” refers to the overall U.S. born population.

had an ambiguous impact on the employment rates of low-skilled natives, they found that immigration had a significantly negative impact on wages for low-skilled native labor. More recent studies however explore other outcomes of labor supply. For example, Ottaviano and Peri (2012) found immigration had a small positive impact on wages of low-skilled native workers and had a significantly negative impact on wages for previous waves of low-skilled immigrant workers. Foged and Peri (2015) found that increased immigrant influx had a positive effect on native wages because low-skilled natives moved into occupations of more complex nature. Similarly, Llull (2017) found that native males either switch to white-collar occupations or increase their skill-levels to neutralize almost half of the negative impact that immigration has on native wages. In terms of women's fertility, Furtado (2015) found that low-skilled immigration could explain a significant portion of increases in the likelihood of child bearing and labor force participation amongst native skilled women. In her later work, Furtado (2016) argued that highly skilled, married women with college degrees become significantly more likely to bear children when immigration increases.

Immigration also poses challenges to the administration of the state welfare programs. Borjas and Trejo (1991) found that immigration was associated with significant increases in spending on income transfer programs. Similarly, Borjas and Hilton (1996) found that immigrant households are not only significantly more likely to receive state welfare, but are also likely to have longer spells of staying on welfare. Similar studies exist in other sectors. For example, James et al. (1998) analyzed the impact of immigration on urbanization and found that immigrants have caused burgeoning of population in major urban cities since 1970, particularly growing in large urban centers with healthier economies. In the healthcare sector, Lowell and

Gerova (2004) argued that immigrants have been important to fulfill the demand shortage of both high-skilled and low-skilled labor force.

Given that immigration has been so extensively linked with different social and economic implications, it is likely that immigration would also impact the demographics in K-12 schools. Contemporaneous immigration trends would suggest that school demographics are likely to undergo significant changes, similar to changes observed across other sectors within the U.S. In particular, this paper is motivated by existing literature on native flight³ (as further discussed the subsection below) in the context of neighborhood and/ or school compositions.

The theory of native flight suggests that U.S. born students, regardless of race/ethnicity would move out of public schools and enroll in private schools or public charter schools as the immigrant population rises in their respective neighborhoods. The driving mechanisms of native flight are however not entirely clear (Murray, 2016). It could be the case that U.S. born groups do not find homophily with immigrant students (Kruse et al., 2016), and thus choose to move out to private schools in search for homophily. The reasons and motivations of the search for homophily among school students are also not very understood. For example, Kruse et al. (2016) studied homophily in Dutch and German schools and could not find any plausible explanation for the existence of ethnically homogeneous friendship groups that dominate the school friendship networks, but nevertheless discuss that they do find such ethnically homogeneous networks. Another possible mechanism for native flight could be the perception that increasing numbers of immigrant students decrease quality of education (Hunt, 2012) and services in the public schools. This could

³For example: Betts and Fairlie (2003); Gerdes (2013); Murray (2016); Rangvid (2009); Renzulli and Evans (2005); Wurdock (1981); Schneider (2008); Frey (1979)

drive U.S. born populations away from public schools with relatively higher shares of immigrant enrollments.

In this context, two research questions emerge, (1): Is there evidence of native flight from public schools in response to immigrant influx? and (2): Do different race/ethnicity groups differ in terms of their native flight response to immigration? Existing literature suggests that there would be significant native flight from public schools into private schools as immigrant influx increases across a geography. Similarly, the existing literature also indicates that flight patterns would be different across different race/ ethnicity groups. To be specific, one would expect that White students would show significantly higher rates of flight as compared to students of other race/ethnicities.

To arrive at a reliable and causal estimate of the impact of immigration on native flight in K-12 schools, the essay relies on an instrumental variables approach (2SLS) to address potentially endogenous settlement patterns of immigrants across Metropolitan Statistical Areas (MSAs)⁴. I examine race/ethnicity shares of enrollments across public, private and public-charter schools as key outcome variables, using immigration as a key independent variable in a exploiting the variation in immigration influx across MSA $\times$ year cells.

I add to the existing literature in multiple ways. To start with, it is likely that native flight patterns could have changed over the past decade or so, since demographics have been changing so rapidly that a saturation point might have reached. I rely on the most recent years of data to confirm previous trends. Second,

⁴A Metropolitan Statistical Area (MSA) is a geographical division drawn for statistical purposes around mean urban centers of business where large population packets begin to aggregate. An MSA can include more than 1 or 2 or 3 nearby adjacent cities and suburbs. For more information: https://usa.ipums.org/usa-action/variables/METAREA#description_section.

I estimate native flight while simultaneously analyzing public schools and private schools as well as public charter schools. This approach helps distinguish the impact of native flight from other potential confounding reasons that could drive a reduction in public school enrollments. Relying on innovative data apportionment practices, I generate consistent MSA definitions over the years 2005-2019, which further helped in maintaining the validity of shift-share immigration instrument over these particular years. As detailed further in the paper, this approach to measure immigration at the MSA level is arguably better, allowing me to capture the socioeconomic characteristics of immigration settlement patterns which are usually more identifiable at the MSA level. Finally, I investigate if native flight trends are heterogeneous across U.S. born race/ethnicities.

After a brief discussion on the theory of native flight in the subsequent subsection, I present the data sources and discuss the descriptive statistics in section 2. In the third section, I present the empirical specification for data analysis. In the fourth section, I present key results. I conclude with a discussion and policy implications. Some robustness estimates based on a lagged measure of immigration and a measure of immigrants in young school-age population follow in the two appendices.

4.1.1 Native flight

The theory of “native flight” emerged from the idea of White flight (Frey, 1979; Wurdock, 1981) which argues that White households leave neighborhoods or urban centers when the concentration of non-White households begins to increase in a neighborhood or geographical area. The reasons for White flight lie in a search for racial homogeneity as well as fiscal features of the neighborhood (Frey, 1979).

Extending this concept, a related term coined to study the flight of U.S. born sub-populations is the term “native flight”. When U.S. born sub-populations witness and experience higher levels of immigration, they tend to geographically distance themselves from those immigrants due to various underlying reasons. In most cases, the natives do so by moving into suburban neighborhoods and schools. The motivations for native flight could be any mix of political, economic, and social reasons. The term “native flight” has been used by Betts and Fairlie (2003), Gerdes (2013), Rangvid (2009), and Murray (2016), to study the response of different U.S. born groups as a response to immigration. Native flight in response to immigration has been studied to some extent in relation to K-12 education settings. Murray (2016) found evidence of increases in private school enrollment for White native students in smaller school districts in response to increased foreign born enrollment in public schools, particularly across suburbs and states which traditionally do not receive a large number of immigrants. Murray (2016) also found increased private school enrollment among Hispanic and native minorities in the states which have historically received larger shares of immigrants. Similarly, Schneider (2008) examined data for race/ethnicity wise student enrollments across Urban Los Angeles during the period of 1966-1980 and found evidence of White flight as a response to sub-urbanization. Schneider (2008) found that K-12 enrollments of White students were significantly lower in the urban Los Angeles during the period of 1966-1980, an era of sub-urbanization and desegregation movement in schools. At the same time, Schneider (2008) found increasing enrollments of students of other races. This literature suggests that it is important to examine the racial/ ethnic heterogeneity in the response of native flight to immigrant influx. It is possible that other U.S. populations such as Black or

Hispanic sub-populations might also respond the same way one would expect for the White sub-population.

As outlined earlier, existing literature has found associations across immigration and native flight, although the number of studies on the topic remains low. The most recent work by Murray (2016) on this topic has examined if increased enrollment of foreign-born students in a public-school district leads to increases in U.S. born enrollment in private schools in the same school district. Murray (2016) has confirmed increased levels of White and Hispanic Student enrollments in private schools, but he does not consider race-ethnicity wise enrollments in public schools as outcomes. Instead, race-ethnicity wise enrollment outcomes in private schools are treated as outcome variables. Any estimates confirming analogous reductions in White student enrollments across public schools could strengthen the evidence of White flight. Other literature either uses older data as is the case with Betts and Fairlie (2003), focuses on particular, smaller geographies (for example see Schneider (2008)); or is based on geographies outside the U.S. (for example see Rangvid (2009)).

Based on Danish datasets, Jensen and Rasmussen (2011) provided evidence that children in schools with higher concentration of immigrants had lower test scores on maths and reading skills. Brunello and Rocco (2013) used a cross country analysis and provided similar evidence that test scores of native students dropped with increasing number of immigrant children in the classroom. This might explain why U.S. born students tend to move out of public schools when the share of immigrants rises within their neighborhoods or schools. It also, might be the case that the quality of education and services remains lower at schools with higher concentration of immigrants, thus motivating U.S. born students to move out of such schools.

The issue of native flight has important policy considerations. For example,

the literature on school choice has argued that private school and charter school enrollment adds to racial and social segregation (Bifulco and Ladd, 2006; Carlson, 2014; Soderstrom and Uusitalo, 2010). Bifulco and Ladd (2006) and Hoxby (2001) link increased segregation with negative effects on student achievement. Bifulco and Ladd (2006) have also argued that public school finance which relies on per pupil funding, also suffers when students move out to private or charter schools as they take their public finance with them. Therefore, it is important that this policy issue gains research attention.

4.2 Data Description

Data for the paper are derived from multiple sources. School level enrollments and other data on characteristics of public schools come from the Common Core of Data (CCD) collected by the National Centre of Education Statistics (NCES) (U.S. Department of Education, 2019a), while data on private school enrollment and school characteristics come from the Private School Universe Survey (U.S. Department of Education, 2019b). Elementary and Secondary Information System (*Els**i*) by NCES maintains data for each school in two universes: (i) Public School Universe Survey which maintains data from public schools and public charter schools from school year 1987-88 on-wards, and (ii) Private School Universe Survey maintains biennial data from school year 1997-98 onwards. Using *Els**i*, I extract public school data for the years 2005-2019 and private school data for each available year during the same period i.e. 2005-2019. To construct a measure for immigration, data for 166 Metropolitan

Statistical Areas (MSAs)⁵ is sourced from the U.S. Decennial Census of 1980 and the U.S. Census Bureau’s annual American Community Survey (ACS) estimates for the years 2005-2019 (Ruggles et al., 2019).

4.2.1 Measures of school enrollment

The sample includes all public and private schools that existed⁶ in 166 identified MSAs through the years 2005-2019. NCES maintains data for each public school data and private school, although while it maintains public school universe data for every year, it offers comparable data for private schools only for each alternating year. A strength of the NCES public and private school universe survey is that it provides several geographic identifiers for individual schools allowing them to be linked to MSAs, which allowed the author to attach MSA identifiers to each school. Since charter public schools are often specialized schools, charter public schools are identified and considered as a different category of schools different from regular, non-charter public schools. The overall sample consists of 63,860 public schools, and 30,578 private schools and 9489 public charter schools⁷, across 166 MSAs. Descriptive statistics for public schools, private schools and public-charter schools are respectively presented in Table 4.1, Table 4.2 and Table 4.3.

The first outcome of interest is a measure of mean annual total enrollment. The

⁵Out of possible 384 MSAs, only 166 MSAs have been consistently identified in the ACS data across 2005-2019.

⁶and enrolled at least 1 student.

⁷The number of unique school in a calendar year might be different, as old schools shut down and new schools open.

overall enrollment trends across public and private schools have not changed much during the past few years. Mean total enrollment in public and private schools remains consistent over 2005-19 (Table 4.1 and Table 4.2), while mean total enrollment across public charters schools increases nearly two-fold over 2005-19 (Table 4.3). The overall enrollment trends from statistics provided by a recent report from the National Center for Education Statistics confirm the same, regular public school enrollments decreased slightly (from around 47.5 million to 47.1 million) over the past decade (U.S. Department of Education, 2021), the total private school enrollment fluctuate between 5 and 6 million students from 1999-2017 (U.S. Department of Education, 2017), and public charter school enrollments more than doubled during 2009-2018 (U.S. Department of Education, 2021).

The key outcome measures of school enrollment include race/ethnicity shares of enrollment for White, Black, Hispanic and Asian students. For public schools, mean percentage shares of race/ethnicity specific enrollment shares⁸ are presented in Table 4.1 and graphed in figure 4.3. White student enrollments across public schools represented 52.2% of total enrollments in 2005, which dropped to 42.7% in 2019. The share of Black student enrollments across public schools dropped slightly from 18% in 2005 to 15.3% in 2019. The share of Hispanic student enrollments in public schools increased from 23.4% in 2005 to 30.8% in 2019, while the share of Asian student enrollments in public schools increased modestly from 5.7% in 2005 to 6.9% in 2019. In public schools, the reduction in the shares of White and Black student enrollments might not present the complete picture. As discussed above, the mean enrollment size across U.S. public schools is not increasing. Could it be the case that actual number of schools is increasing which could mean that White

⁸weighted for total school enrollments

students and Black students are simply more spread across more number of schools? This is however not the case, not at least within the 166 sample MSAs. In 2005, the total student enrollment across non-charter public schools in 166 MSAs was 31.63 million. Out of these 31.63 million total enrollments, 16.53 million were White student enrollments, 5.69 million were Black student enrollments, 7.41 million were Hispanic student enrollments whereas 1.81 million were Asian student enrollments. In 2019, the same 166 MSAs had 32.95 million total student enrollments across public schools. Out of these 32.95 million enrollments, 13.90 million were for White students, 5.03 million were for Black students, 10.15 million were for Hispanic students and 2.26 million were for Asian students.

This shows that in terms of the shares of total enrollments as well as in terms of absolute numbers, White and Black student enrollments have consistently declined across public schools across 166 urban MSAs, even when total enrollments slightly increased over time. Overall, these trends signal that demographics within the public K-12 schools are changing rapidly with schools become more diverse over time, reflecting the changing demographics of the country.

Similar demographic changes are observed across private and public charter schools. The share of White student enrollments fall across all types of schools, and the share of Hispanic and Asian student enrollments make up slightly higher proportions of total enrollments. Across private schools (Table 4.2.), the overall share of White student enrollments fell from 72.9% in 2006 to 63.2% in 2018. The shares of Black student enrollments across private schools remained consistent around 10% while the share of Hispanic student enrollments increased from 10.7% in 2006 to 12.8% in 2018. The shares of Asian student enrollments in private school was 4% in 2006, which increased to 7.3% in 2018. These trends are shown in figure 4.4. Besides shares of

enrollment, it is also important to contextualize these statistics while keeping in mind the total number of enrollments. In 2006, there were a total of 3.16 million student enrollments across private schools within the 166 analysis MSAs. Out of these 3.16 million enrollments, 2.30 million were White student enrollments, 0.33 million were Black student enrollments, 0.34 million were Hispanic student enrollments, while 0.13 million were Asian student enrollments. In 2018, total private school enrollments across 166 sample MSAs dropped to 2.67 million. Out of these, 1.69 million were White student enrollments, 0.27 million were Black student enrollments, 0.34 million were Hispanic student enrollments and 0.20 million were Asian student enrollments. Overall, two important trends emerge for private school enrollments. First, across the 166 urban MSAs in the sample, overall private school enrollment has decreased by around 15% over 2006-2018. Overall, the share of White students and Black students has dropped across private schools, while the shares of Hispanic and Asian student enrollments has grown to some degree.

Across public charter schools (Table 4.3), White student enrollments were 37.2% of total enrollments in 2005, which fell to 27.7% in 2019. The share of Black enrollments in public charters fell from 35% in 2005 to 26.7% in 2019. Shares of Hispanic students in public charters increased the most over this time period, from 23.5% in 2005 to 36.3% in 2019. The shares for Asian students in public charters increased from 3.4% in 2005 to 4.5% in 2019. These trends are presented in figure 4.5. It also appears that enrollments across public charter schools have consistently increased over time. In 2005, only 0.71 million students attended public charters. Total enrollments across public charters increased by 400% to 2.82 million in 2019. In 2005, 0.26 million were White student enrollments, 0.25 million were Black student enrollments, 0.16 million were Hispanic student enrollments and 0.02 million were Asian student enrollments across

public charter schools. In 2019, White student enrollments increased to 0.78 million, Black student enrollments increased to 0.75 million, Hispanic student enrollments increased the most to 1.02 million and Asian student enrollment increased to around 0.13 million. It could be worthwhile to explore if these demographic changes across public charter schools are linked with changes in the shares of foreign born population across metro areas.

Besides the key outcome measures, a matrix of control variables for each public school includes pupil-to-teacher ratio, number of full-time employed teachers, the level of school⁹, schools agency type¹⁰, and the Title-1 status of the school. For private schools, the control matrix also includes if the school has a religious or community affiliation.

4.2.2 The Share of Immigrants in the Population

The sample for immigration estimates includes all native (outside group quarters) and foreign born individuals in the American Community Survey (ACS) microdata, regardless of age, race or any other demographics. An immigrant is identified as someone who is foreign born and who is either not a citizen or is a naturalized citizen¹¹. The key independent variable in the paper is the immigrant share of population within

⁹the highest and the lowest grades taught.

¹⁰This variable captures if the school agency is a regular local school district, or a regular local school district that is NOT a component of a supervisory union, or a local school district that is a component of a supervisory union, or an independent charter district, a regional education service agency (RESA), a state agency providing elementary and secondary education or a supervisory union administrative center or any other form of educational agency

¹¹which would mean was not a citizen at birth.

MSA $\times$ year cells. I attach this MSA $\times$ year level immigration measure to each school, which keeps my unit of analysis at School $\times$ MSA $\times$ year level.

This approach has been widely used in previous immigration literature dating back to Altonji and Card (1991), and has been used until recently, for example by Furtado (2016) and Jaeger et al. (2017). It could also be the case that U.S. born families do not move their children to private schools in response to immigration, but instead move towards suburban areas, with possibly lower levels of immigrant enrollments in public schools (Betts and Fairlie, 2003). If I were to take a county level or a school district level measure of immigration, such an approach would exclude specific smaller suburban counties from my sample altogether since smaller counties are not identified in ACS microdata extracts. This would introduce an upward bias in the estimate on public school flight, when despite changing schools students are in fact only moving to other public schools. The flight from urban public schools to suburban public schools is an intriguing question, but my focus for the paper remains on the flight out of public schools. On the contrary, as Betts and Fairlie (2003) have argued that MSA definition's usually encapsulate suburban geographies within the boundaries of the MSA, and thus a measure of immigration on the MSA level allows me to reduce the possible bias that could arise from taking other geographical approaches to measure immigration.

166 MSAs are consistently recognized across 2005-2019. I restrict my analysis to these 166 MSAs to ensure consistency of geographical coverage for estimation purposes. The key independent variable is thus a measure of immigrants as a share of overall population within MSA $\times$ year cells. Mean yearly shares of immigrants in the sample population are presented in figure 4.2. Across the sample MSAs, the share of immigrants was around 14.9% in 2005, which increased to 16.7% in 2019.

Before concluding this section it is important to note that I use the immigrant and native population of all ages (in contrast to just the school-age population) to calculate the share of immigrants in the population. The median age of an immigrant is higher than the median age in the U.S., which means that school aged children (ages 0-17) make up around 5 percent of immigrants, whereas the same age bracket accounts for about 18 percent of the U.S. population (Migration Policy Institute, 2022b). Children of these immigrants born in the U.S. however are U.S. citizens by birth, but could nevertheless motivate native flight due to been ethnically different. For this reason, the political or racial dimension of the motive for native flight is perhaps better captured by an MSA level measure of immigration. Furthermore, the influx of immigrants in an MSA is more politically visible as compared to the influx of foreign-born students in schools. The higher median age of immigrant profile in the U.S. and its connection with possible influx of U.S. born but ethnically different¹² students in the public school system also suggests that an appropriate measure of immigration should consider immigrants of all ages, not just the age-bracket of immigrants that corresponds to school going age¹³.

Immigrants have a tendency to settle into geographies in clusters, based on earlier settlement patterns of previous waves of immigrants (Altonji and Card, 1991; Card, 2009). They do so in order to gain network and peer support from previous immigrant settlers, which could possibly help them settling in, form connections and find work. For these reasons, some MSAs traditionally receive high shares of immigrants and some do not, introducing overtime variation in immigrant influx across MSAs. In

¹²being born to immigrant parents

¹³As a robustness test though, I later include the estimates based on an alternative measure of immigrants aged 3 to 20 in the paper.

2019 for example, Table 4.4 lists the top 10 and bottom 10 MSAs with respect to the shares of immigrants. On one extreme, some southern MSAs like Miami-Fort Lauderdale-West Palm MSA in the state of Florida, those closer to the border of Mexico like El Paso MSA in the state of Texas, or some MSAs near the ports such as Los Angeles-Long Beach-Anaheim MSA in the state of California had immigrant shares as high as 24 to 40 percent. On the other hand, share of immigrants across many rural and far-lying MSAs such as Anniston-Oxford-Jacksonville MSA in the state of Alabama or the Saginaw MSA in the state of Michigan were as minimal as about 1 percent. This variation in immigrants shares of populations across MSAs can be exploited to estimate the impact of immigration on school enrollment demographics.

4.3 Empirical Strategy

The paper aims to estimate the impact of immigration on native flight in K-12 schools, with the key independent variable being the share of immigrants in the population within each $\text{MSA} \times \text{year}$ cell. Compared to measuring immigration on the county or school-district level, an MSA level measure of immigration offers several key advantages. An MSA level measure of immigration resonates more closely with political reasons of native or white flight. For example, an alternative approach could use a direct measure of foreign-born enrollment within the public schools district, as a key independent variable (for example in Murray (2016)). However, the rationale for not adopting this alternate approach to measure immigration lies in the multiple mechanisms which motivate native flight from public schools. The first mechanism is the search for homophily among students and families. Students and families tend to find and form networks with ethnically homogeneous people due to cultural and

sociological reasons (Shrum et al., 1988). The second mechanism is the search for better quality schools, conditional on the assumption that families think that higher levels of immigrant shares in schools are associated with lower school quality. The third mechanism is political or racial motivation or desire to stay apart from immigrants. While it is admittedly hard to identify and isolate these three mechanisms of native flight in any empirical approach, I would argue that the measure of immigration on the MSA level provides a better approach to capture these mechanisms that motivate native flight. A measure of immigration capturing foreign shares of enrollment within schools would capture the motivation of the native population to keep their children in homogeneous groups with similar social, cultural and economic features. However, this approach still would not fully capture the political or racial mechanisms motivating native flight. Since parents arguably have more decision making responsibility in most school choice decisions, their decisions are logically more likely to be driven by the political visibility of immigrants within neighborhoods or geographies rather than the actual enrollment of foreign born students in the schools which their children attend. As I have earlier argued, the influx of immigrants in an MSA is more politically visible as compared to the influx of foreign-born students in schools. For this reason, I argue that while MSA level influx of immigrants will correlate with increases in foreign-born student enrollment in public schools for obvious reasons, using MSA level immigrant patterns as a measure of immigration patterns offers a more direct and relevant mechanism to capture the political and racial dimension of native flight.

4.3.1 Empirical Specification

The main specifications for this paper take the following form:

$$Y_{s,m,t} = \gamma I_{m,t} + X'_{s,m,t} + Z'_{m,t} + \zeta_m + \eta_t + \varepsilon_{i,m,t} \quad (4.1)$$

In equation 4.1, the key dependent variable Y depicts various forms of outcomes which include log total enrollment and race/ ethnicity shares of White, Black, Hispanic and American Asian school enrollments¹⁴ in school s located in MSA m in year t . Key independent variable I_{mt} in equation 4.1 is the share of immigrants in the population in MSA m in year t . X' is a vector of school level controls that control for time variant school characteristics such as pupil to teacher ratio, Title-1 status of school, number of full time employed teachers and type of school district. Z' is a vector for controls that control for time variant MSA characteristics, MSA fixed effects (ζ) control for time-invariant MSA fixed effects, while year fixed effects (η) control for year specific macroeconomic and policy shocks that systematically affect all metropolitan areas in the country¹⁵. The main specification also includes the disturbance term ε . Analogous specifications are run for the public (non-charter) school, private school, and public charter school samples. The key coefficient of interest γ is the coefficient on the share of immigrants in MSA m in year t . To account for the size of school, all regressions

¹⁴For example, Share of White students = (total White students enrolled in school s in year t / total enrollment in school i in year t) $\times 100$.

¹⁵In equation 4.1 above, the year fixed effects are not interacted with MSA fixed effects, this is due to data limitations. Across many MSA $\times$ year cells, the number of schools allocated remains too few, which over-inflates the standard errors on the specifications. The instrument is also considerably weaker in settings with MSA $\times$ year fixed effects. Jaeger et al. (2017) in fact argue that a weak shift-share immigration instrument is more likely to meet the exclusion criteria. This would mean that perhaps the instrument is best suited to specification as shown in column (4) of Table 4.5. If the instrument becomes any weaker, it better meets the exclusion criteria, but then the IV2SLS estimates could be biased.

for race/ethnicity shares of enrollment are weighted for total school enrollment. Since the residuals are likely to be correlated within MSA $\times$ year cells, the standard errors are clustered at the same level, within MSA $\times$ year cells.

4.3.2 Identification

Immigration and school enrollment decisions in equation 4.1 are potentially endogenous due to various reasons. Public school spending and immigrant patterns could be correlated (Mavisakalyan, 2011), as newer waves of immigrants could be inclined to settle in geographies with better schooling opportunities. Settlement decisions and patterns of both native and immigrant families influenced by factors other than immigration such as search for work, economic mobility or other socioeconomic changes, could also be spuriously correlated with children moving in and out of public or private schools (Betts and Fairlie, 2003). A similar source of endogeneity identified by Cascio and Lewis (2012) relates to the socioeconomic characteristics of a geography which might attract both immigrants and native populations at the same time, thus changing the composition of immigrants within both public and private schools. All these sources of endogeneity create a potential identification problem, which has been countered using an instrumental variable approach¹⁶.

To counter this threat of endogeneity, the share of foreign-born population in this paper is instrumented via a shift-share past settlements instrument. Shift-share instrumental variable approach to instrument for immigration was first detailed out

¹⁶Instrumental variable approach in econometrics was first outlined by Reiersol (1941), it is useful in estimating unbiased causal relationships between two variables, when the explanatory and dependent variable are endogenous, and/ or correlated with the error term of the estimating equation (Angrist et al., 1996). Under this approach an instrument variable is identified which is correlated with the endogenous explanatory variable but is uncorrelated with the error term.

by Bartik (1991) and has been since extensively used in immigration related studies¹⁷. The shift-share past settlements instrument predicts current immigration share in MSA m at year t from country j , based on the shares of past settlements of immigrants from country j in MSA m at year $t - 20$ or $t - 30$. To illustrate, the instrument variable for this paper takes the following form:

$$Predicted\ Immigration_{m,t} = \sum_j^n \left(\frac{Immigrant_{m,j}^{1980}}{Immigrant_{US,j}^{1980}} \times Immigrants_{j,t} \right) \quad (4.2)$$

where: MSA= m , year= t , and j =origin country of the immigrants.

Thus, the predicted immigration in MSA m in year t is determined by the following three steps: (i) for each country of origin j , all immigrants are allocated to MSAs as per the actual distribution of immigrants from country j in the year 1980^{18,19}, (ii) In each predicted year, same share of immigrants from country j are allocated to each MSA m as per the MSA allocations in 1980 above, and (iii) all predicted shares

¹⁷(For example see: Furtado (2015), Goldsmith-Pinkham et al. (2020), Jaeger et al. (2017), Murray (2016), and Romiti (2018) amongst others).

¹⁸The MSA definitions were revised in 2013 by the U.S. Census Bureau, and the older MSA definitions (as captured by the variable `metarea` in the ACS-IPUMS extracts) are not available for ACS data past 2011. The newer definitions of MSAs are instead captured by variable `met2013` in ACS-IPUMS extracts, which is only available past the year 2005 onwards. The new MSAs and their boundaries, do not correspond directly to the older MSAs or their boundaries. However, the new 2013 based MSAs (variable `met2013` in ACS-IPUMS), consist of entire counties, the boundaries for which seldom change. Some MSAs were merged and some were split, mostly added or subtracting entire counties. Since I wanted to include most recent data on school enrollment, for my independent variable I use new MSA definitions, defined by the variable `met2013`. To construct a reliable instrument based on 1980 definition, I manually construct the `met2013` variable based on a combination of prior MSA codes and county fips codes where available. For example, say an MSA in 2005-2019 has 4 counties but the same MSA had only 2 counties in the year 1980. If the additional counties are identifiable in the year 1980, I add those to 1980 MSA definition. I was able to construct a reliable instrument this way for the 166 sample MSAs, as evidenced by the sample first stage. For more information: <https://www.census.gov/geographies/reference-files/time-series/demo/metro-micro/historical-delineation-files.html>

¹⁹A similar approach to manually match `met2013` to `metarea` has been taken by Charles et al. (2018) in their AER paper on housing booms, labor market busts and college attendance, for more information please see: <https://www.aeaweb.org/content/file?id=8025>

of immigrants from all j countries are summed up for each MSA m for year t , i.e. 2005-2019.

An example of this approach will work as follows. If 30% of immigrants from Italy were to settle in the MSA of Phoenix-Mesa-Chandler in 1980, and say if 10,000 immigrants from Italy came into the U.S. in 2018, this instrument will allocate 30% of these immigrants, i.e. 3000 immigrants from Italy to Phoenix-Mesa-Chandler for the year 2018. This is iterated over all countries of origin and summed up to find total predicted number of immigrants within each MSA $\times$ year cell.

The validity of the instrument lies in the argument that for several reasons, the past settlement patterns of the immigrants could not be correlated with the current socioeconomic changes in an MSA m . First, the national inflow of immigrants from any country at time t can be expected to be exogenous to the local conditions of an MSA m (Altonji and Card, 1991; Jaeger et al., 2017). Second, since the instrument allocates the share of current immigrants to MSAs based on past settlement patterns, even if the national inflow of immigrants from any country is not deemed to be exogenous to local economic conditions of the U.S. in the strictest sense, the current immigrant inflow trends cannot be related to local economic conditions²⁰ 20 or more years prior to today or in the year 1980 (Card, 2009; Altonji and Card, 1991). Finally, since the immigrants tend to settle in clusters in vicinity of geographic settlement locations of previous settlements, the previous settlement patterns serve as a good prediction mechanism for future settlement patterns (Card, 2009). Indeed, the instruments accuracy to predict future immigrant inflows is cited as one of its greatest weaknesses, as strong auto-correlation across yearly values of instrumented immigration can potentially conflate short-run impact with long-run impacts of immigration, particularly so for

²⁰within the U.S. MSAs

case of labor markets outcomes (Jaeger et al., 2017). If this is indeed the case, the instrument is also likely to fail in isolation of the exogenous and endogenous components of immigration (Jaeger et al., 2017). However, while dynamic adjustment of local wages in the long-run²¹ in addition to the short-run shock on local wages²² is a well documented process, there is no evidence that similar concerns exist in terms of school enrollment outcomes. Typical long-run adjustment in wages occurs because native workers either gain upward skill mobility or change careers. There can be no such response when it comes to school enrollments. In a sense, school enrollments can be measured as a stock rather than a flow²³. The baseline year selected to calculate MSA shares of immigrants is 1980, which provides for a gap of at least 25 years between the baseline year and the prediction years.

To further control for systematic and contemporaneous economic pull and push factors affecting immigrant locating decision across cities, I control for MSA and year fixed effects, as well as a set of MSA specific time-variant controls, following the approach proposed in earlier immigration literature (Card and Peri, 2016; Cortes and Tessada, 2011; Llull, 2017; Foged and Peri, 2015; Furtado, 2015).

To test the strength of the instrument, I run the following first stage equation:

$$I_{m,t} = \gamma \ln(\text{Predicted Immigration})_{m,t} + Z'_{m,t} + \zeta_m + \eta_t + \varepsilon_{m,t} \quad (4.3)$$

where the dependent variable I_{mt} in equation 4.1 is the share of immigrants in the population in MSA m in year t , regressed on the instrumented predicted immigration

²¹usually an upward impact on wages in the long-run

²²usually negative pressure on local wages.

²³A similar argument has been made in Butcher et al. (2021)

in MSA m in year t . Analogous to equation 4.1, Z' is a vector for controls that control for time variant MSA characteristics, MSA fixed effects (ζ) control for time-invariant MSA fixed effects, while year fixed effects (η) control for year specific macroeconomic and policy shocks.

The first stage estimates for strength of the instrument are presented in Table 4.5. Column (1) presents the baseline OLS specification with the instrument regressed on the shares of immigrants in the population. MSA level time variant controls are added in column (2), such as controls for mean poverty rates, mean household income, and mean household value. In column (3), MSA fixed effects are added, while the key specification of interest in column (4) includes both MSA and year fixed effects. For a robustness check, column (5) is a limited sample that excludes the cities of Miami, New York and Los Angeles as well as all the other MSAs in the state of California, as these cities and regions have historically received higher number of immigrants (Cortes and Tessada, 2011; Ruggles et al., 2019). All regressions are weighted for MSA populations, and standard errors are clustered into MSA $\times$ year cells.

In the key specification of interest presented in column (4), the coefficient on the instrument shows that for a 10 percent increase in the number of immigrants in an MSA m in year t , the share of immigrants in the population increases by around 0.47 percent. The F-statistic on this coefficient is 68.00, which is well above the recommended strength of F-statistic on the first stage (Cameron and Trivedi, 2010). Throughout columns (1) through (3), the instrument passes the required strength test. In column (5), when LA, Miami, New York and the cities in the state of California are removed from the model, the instrument remains reasonably robust, with an F-statistic of 23.09.

4.4 Results

The estimates for public school enrollment are presented in Table 4.6. The first column presents baseline OLS estimates, while the second column adds controls for time-variant MSA characteristics. In column (3), MSA fixed effects are added to the specification, in column (4), year fixed effects are added and MSA fixed effects are removed from the specification, while in column (5), both MSA and year fixed effects are included. Column (6) presents the IV2SLS estimates. Table 4.7 presents analogous estimates for private school enrollment and Table 4.8 presents analogous estimates for public charter school enrollment.

4.4.1 Public School Enrollment

I start by estimating the log of mean total enrollment in Panel A of Table 4.6. The baseline OLS estimate in column (1) indicates that a one percent increase in the share of immigrants in the population is associated with a 1.06 percent increase in mean total school enrollment. The estimate remains significant but drops slightly in magnitude in column (2) when controls for time-variant MSA characteristics are added. When both MSA and year fixed effects are added in the fixed effects model, the effect size becomes statistically insignificant. However, throughout column (1) through (5), the estimates could be biased due to concerns for endogenous school enrollment and immigrant patterns. The IV2SLS estimate in column (6) indicates that mean total enrollment across public schools increases by 2.5 percent in response to a 1 percent increase in the share of immigrants in the population. Considering that log total enrollment is the outcome here, this finding indicates that the average

school size (by enrollments) is generally growing in metropolitan areas with higher immigration levels.

Estimates for the share of White enrollment are presented in Panel B of Table 4.6. OLS estimates in columns (1) and (2) are both negative and significant, indicating a 1.274 percent to 1.386 percent decline in the share of White enrollment for a one percent increase in the share of immigrants in the population. Estimates are pretty similar in columns (4) and (5) when either of the MSA and year fixed effects are included in the specification. When the specification adds both MSA and year fixed effects, the magnitude of the slightly negative effect on shares of White enrollment is smaller and statistically insignificant. Once again, this is understandable in the context of endogeneity concerns. The IV2SLS estimate in column (6) is more likely to be unbiased, which indicates a 0.55 percent decline in the share of White enrollment for a one percent increase in the share of immigrants in the population.

Panel C of Table 4.6 presents estimates for the share of Black enrollment in public schools. Baseline OLS estimate indicates that the share of Black enrollment decreases by 0.35 percent for a one percent increase in the share of immigrants in the population. This small negative effect on the share of Black enrollment is sensitive to the inclusion of MSA level time variant controls and MSA or year fixed effects, but remains the same in magnitude and statistically significant when MSA level controls as well as MSA and year fixed effects are added in column (5). The effect size becomes bigger in magnitude in the IV2SLS estimate in column (6), which indicates a 0.76 percent decline in the share of Black enrollment for a one percent increase in the share of immigrants in the population.

Estimates for the share of Hispanic enrollment are presented in Panel D of Table 4.6. Baseline OLS estimate indicates that the share of Hispanic enrollment increases by 1.41

percent for a one percent increase in the share of immigrants in the population. This estimate remains robust across specifications (1) through (4). In column (5), when both year and MSA fixed effects are included, the effect size reduces to 0.26 percent, but is still strongly significant. As seen for the shares of White and Black enrollment, IV2SLS estimate is bigger than the OLS estimate and statistically significant, indicating that Hispanic enrollment increases by 0.48 percent for a one percent increase in the share of immigrants in the population.

Panel E of Table 4.6 presents estimates for the share of American Asian enrollment across public schools. Baseline OLS estimate indicates that the share of American Asian enrollment increases by 0.33 percent for a one percent increase in the share of immigrants in the population. This estimate remains robust across specifications (1) through (5). As seen previously, the second stage instrumental variable estimate is bigger than the OLS estimate and statistically significant, indicating that American Asian enrollment increases by 0.57 percent for a one percent increase in the share of immigrants in the population.

As expected, for the most cases IV2SLS estimates across Panel (A) through (E) are stronger and statistically significant than OLS estimates. This shows that OLS estimates are more likely to be biased towards zero and estimate a smaller effect size for the race/ethnicity wise shares of enrollment. Native flight from public schools is not homogeneous across all race/ethnicity groups. While the shares of White and Black enrollments fall by 0.55 and 0.76 percent, the shares of Hispanic and American Asian enrollment respectively increase by 0.48 percent and 0.57 percent. These findings perhaps also provide some insight into the mechanisms for native flight. While Hispanic and American Asian students might find some degree of homophily with immigrants, the White and Black sub-populations might not find the same degree of homophily

and move away from public schools. In the context of increasing shares of immigrants in the U.S. population (figure 4.1), these estimates could be quite meaningful.

4.4.2 Private School Enrollment

Estimates on the log of total enrollment across private schools are presented in Panel A of Table 4.7. The slightly positive OLS estimate in column (1) indicates that private school enrollment increases by about one percent for a one percent increase in the share of immigrant population. However this estimate is not robust to changes in the specifications. The IV2SLS estimate in column (6) is small and statistically insignificant. This finding resonates with the descriptive evidence presented in section 2, indicating that total private school enrollment has remained consistent overtime.

Panel B of Table 4.7 contains estimates for the share of White student enrollments. While the OLS estimates in column (1) and (2) as well as column (3) and (4) are negative and statistically significant, the estimate turns positive but insignificant in the most stringent fixed effects specification in column (5). The IV2SLS estimate though is positive, bigger, and statistically significant, showing a 1.36 percent increase in the share of White student enrollments in private school, in response to a 1 percent increase in immigrant share on the MSA level. This findings confirm the theory that White students move out of public school and move into private schools in response to increased shares of immigrants on the MSA level.

In Panel C of Table 4.7, the estimates for the share of Black student enrollment across private schools are presented. The OLS estimate in column (2) is positive and significant but turns negative and significant when both MSA and year fixed effects are added in column (5). IV2SLS estimate in column (6) is greater in magnitude, and

indicates that in response to a 1 percent increase in immigrant share on the MSA level, the share of Black student enrollments in private schools drops by 0.72 percent. This finding is a little bit surprising, yet explainable. It appears that the Black students are dropping out of both public and private schools at the same time. This might indicate some crowding out effect in both public and private schools for Black students, given that significant socioeconomic differences exist across Black and White households making them less likely to attend private schools or to keep attending and finishing public schools.

Panel D of Table 4.7 contains estimates for the share of Hispanic student enrollments. The positive OLS estimates on the share of Hispanic student enrollments are not robust to inclusion of both MSA and year fixed effects, and the same is confirmed via IV2SLS estimates. Once again the IV2SLS estimate is stronger in magnitude, however remains statistically insignificant. Although changes in the shares of immigration on the MSA level do not appear to influence Hispanic students to move out of private schools, yet this finding is expected given that the share of Hispanic public school enrollment was increasing in Table 4.6.

Panel E of Table 4.7 contains estimates for the share of American Asian student enrollments. For American Asian students as well, the positive OLS estimates on the share of Hispanic student enrollments are not robust to inclusion of MSA level controls or the inclusion of MSA and year fixed effects, and the IV2SLS estimates are also insignificant.

To sum up these findings, private school enrollments do not respond to the share of immigrants, except for the shares of White and Black students in private schools. While the effect on the share of White students is positive and understandable, the negative effect on the share of Black students is a little bit harder to interpret. It

could be the case that the socioeconomic status of Black households and the resulting crowding out of Black students in private schools is driving these estimates. On the other hand, it might be also be a case that perhaps Black students are dropping out from K-12 schools altogether. Some descriptive analysis might shed some light on this, while in the 166 MSA's the total number of Black enrollments dropped across public and private schools (as noted in section 2), statistics from the Pew Research Center (2021) show that the Black population across the U.S. has indeed increased from 35 million to 47 million over the past two decades. This could indicate that Black students could be dropping and crowding out from both public and private schools. Alternatively, Black students might be moving outside these 166 MSAs, a theory which I cannot test due to data limitations.

4.4.3 Public-Charter School Enrollment

The estimates for public charter school enrollments are presented in Table 4.8. The IV2SLS estimates for the log total enrollment show a statistically significant decline of 3.75 percent in response to 1 percent increase in immigrant shares in the population. Given that the overall public charter school enrollment grew by around 400% over 2006-2018, this finding is intriguing. Perhaps, this finding is driven by the fact that there is some anecdotal evidence that immigrants might prefer public charter schools for specialized education (The New York Times, 2009; Learning for Justice, 2010). If indeed immigrants students are increasingly enrolling into public charter schools, the theory of native flight would predict that U.S. born students would move out of public charter schools.

For race-ethnicity shares, none of the IV2SLS estimates are statistically significant.

Perhaps this is a small sample, and the smaller number of schools within MSA $\times$ year clusters probably inflates the standard errors. Nevertheless, while the coefficient on the shares of other races/ ethnicities is negative, the coefficient on the share of White enrollment is positive.

4.5 Robustness

Two concerns remain with the specification in equation 4.1. The first one is the possibility that school enrollment responses are lagged by one year, and any estimates that estimate contemporaneous school enrollment measures at the same time when shares of immigrants are measured could be spurious. While this is unlikely, it is still a possibility. A second concern is that school enrollment responses to share of immigrants in the younger population could be more relevant and different from the estimates presented in Table 4.6, Table 4.7 and Table 4.8. To address these concerns, I run additional specifications as robustness tests which are briefly discussed in this section.

4.5.1 Lagged Responses to Immigration

I run a slightly modified version of the specification from equation 4.1, with the independent and instrumental variables replaced by first lags (as observed in year $t-1$) of each respective variable. This allows to check if there is any lagged response to immigration. The estimates for public school enrollment are presented in appendix Table A1, the estimates for private school enrollment are presented in appendix Table A2, while the estimates for public charter school enrollment are presented in appendix

Table A3. Across public, private and public-charter schools, the OLS estimates and IV2SLS are remarkably similar to the main estimates discussed earlier in section 4. The estimates are just a tad bit stronger, which is expected since these estimates now might include the conflated effect of lagged immigrant share as well as the omitted contemporaneous immigration share in the population in year t .

4.5.2 Share of Immigrants in School-age Population

To address for this concern, I modify the independent variable to the share of immigrants in population aged 3 to 20, since this age resonates more closely with the school age native population. The instrument is still the same, which is the predicted number of immigrants in each MSA m in year t , based on past settlement patterns in year 1980. The first stage estimates using the modified independent variable are presented in Table B1. Across specification (4) and specification (5) of the first stage, the instrument passes the strength test, with the F-statistic well above the required threshold. Analogous estimates to those presented in Table 4.6, Table 4.7 and Table 4.8 are shown in Table B2, Table B3 and Table B4. Estimates for public school enrollments as presented in Table B2 as well as estimates for public charter school enrollments presented in Table B4 are almost identical to the baseline estimates in Table 4.6 and Table 4.8, though a little smaller in magnitude as compared to the main estimates. The IV2SLS estimates for private school enrollment in Table B3 are however insignificant across Panel A to E, which were significant for the share of White as well as Black enrollments in private schools in Table 4.7. The effect sizes on both estimates are almost identical to the main estimates in Table 4.7 but the standard errors in Table B3 are a bit inflated. This is perhaps expected, as shares of

immigrants in young population are relatively smaller (about 1/4th of the share in all population). Considered together, these estimates based on immigrant share in young population indicate that the native flight phenomena is indeed better explained by political visibility of immigrants on the geographic level rather than actual enrollment of immigrant students within K-12 schools.

4.6 Conclusion

Estimates confirm the theory of native flight from public school although not surprisingly, native flight is not homogeneous across U.S. born race/ ethnicities. The strongest evidence for native flight is supported for White students. There is sufficient evidence to suggest that the share of White student enrollments across public schools drop by as much as 0.55 percent in response to a 1 percent increase in the share of immigrant population. This decrease in the share of public school enrollments is supported by a corresponding increase in share of White student enrollment in private schools by 1.36 percent. Shares of Black student enrollments drop across public and private schools, and hence the theory of native flight for Black students is not entirely supported. For Hispanic and American Asian students, an opposite mechanism appears to work, their public school enrollments increase as a response to immigration. At the same time, estimates do not suggest that the shares of Hispanic or American Asian students across private schools change significantly in response to immigrant influx. This can perhaps be best explained by better homophily for them in public schools. Estimates for all non-White race/ethnicity groups are insignificant across private school enrollments, but share of White student enrollments increase across

private schools. This difference is perhaps also driven by socioeconomic difference across White and non-White race/ethnicity groups.

Estimates from this paper are consistent with existing literature, although some small differences are also found. Betts and Fairlie (2003) found limited evidence of White flight from secondary schools, while Murray (2016) found evidence of native White flight in smaller school districts from states which do not traditionally receive immigrants. The estimates from this paper find overall evidence of White flight out of public schools as well as White flight towards private schools. Murray (2016) found evidence of Hispanic and minority native populations in states which traditionally receive immigrants. I do not find any evidence of Hispanic flight. On the contrary, Hispanic enrollments are indeed increasing across public schools. The case for American-Asian students is identical to that of Hispanics, American-Asian share of enrollments increase across public schools. Evidence of flight among Black students is not really supported, as it appears that Black enrollments are dropping across public and private schools.

The estimates for public charter schools are a bit puzzling. Total enrollment across public charter schools increased four-folds across the study period, but none of this can be attributed towards immigration, despite some thinking that immigrants might prefer to enroll in public charter schools (The New York Times, 2009; Learning for Justice, 2010). There are two questions which I am unable to answer. First, who is using public charter schools? Second, if immigrants indeed enroll in public charter schools, why does immigrant influx not impact public charter schools enrollment shares? A consistent theory of native flight would argue that if immigrants are likely to enroll in public charters, native populations should move away from them. There is no way in current data to confirm shares of immigrants within K-12 schools. It

could also be the case that standard errors are inflated due to small sample size, and I simply do not have enough statistical power to identify a causal estimate.

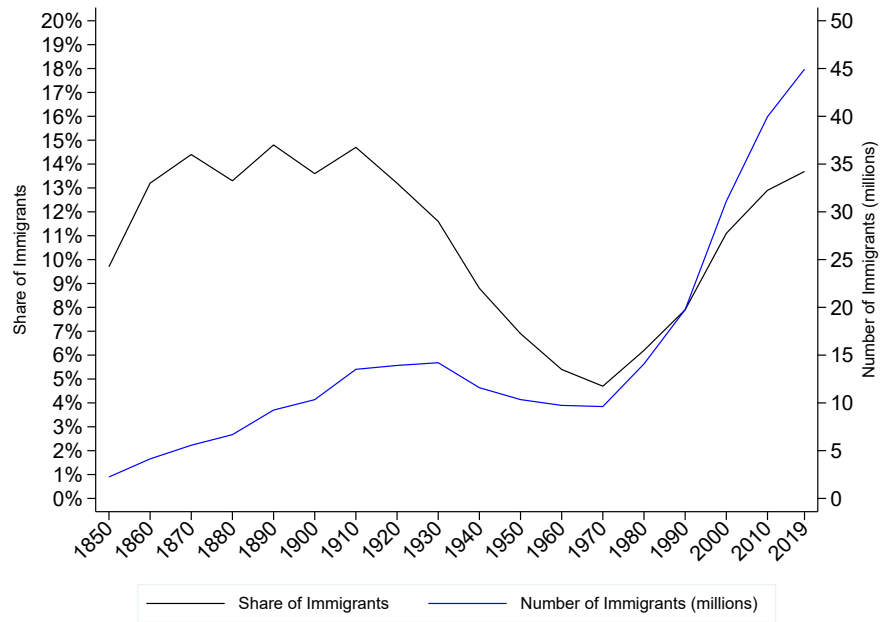
Robustness tests in appendix B show that estimates are weaker though comparable when the independent variable is switched to the share of young population in the MSA. This lends some credibility to the theory that mechanisms for the native flight are perhaps embedded in political visibility at the geographic level, and perhaps not related to actual foreign-born enrollment in schools.

There are many research implications from this study. First, students of White origin are still leaving the public-school system as theory suggests. This is in line with the findings in Murray (2016), and Betts and Fairlie (2003). Future research should consider a strategy that includes areas outside the urban and suburban MSA geographies. Second, only White students are still more likely to enroll in private schools in response to immigrant influx. This points to the historical socioeconomic gaps across White population and other race/ ethnicities and presses the need for bridging the socioeconomic gap. Third, while this paper finds empirical evidence of native flight, the mechanisms for native flight are not entirely clear, and warrant further research. Future research should also consider the impact of native flight on segregation. If White students are enrolling in private schools as a response to increased immigration, do they move to more segregated schools? Are their motives better explained by the quality of instruction in private schools or available resources etc.? It is important that the motive of such differences in enrollment levels across different races and ethnic groups is further examined.

In terms of policy implications, I have earlier discussed existing research that has provided evidence that test scores of students drop when they are in the schools with higher concentration of immigrants. This could indicate that school resources, and the

quality of education and services do not remain the same when there is an increase in the share of immigrant children in school. While in this paper, I have shown that students of White origin are indeed moving to private schools, policy-wise it becomes necessary to investigate about the level of school resources, quality of education and quality of services in schools with higher number of immigrant students. If this is not the case, educationists need to disseminate this info to students of such neighborhoods and schools where immigrant levels are high. Also, Bifulco and Ladd (2006) have argued that greater segregation in schools leads to wider test score gaps between students of minority and White origin. If students of White origin are indeed moving to more segregated schools, policy intervention is clearly needed to initiate behavioral nudges in such school districts to counter higher levels of segregation.

Figure 4.1. Share of Immigrants in the U.S. 1850-2019



Source: Migration Policy Institute (2022a)

Table 4.1. Race/Ethnicity Shares of Enrollment in Public Schools

Year	Mean Total Enrollment	Race/Ethnicity Shares of Enrollment				
		White	Black	Hispanic	Asian	Other
2005	622.257 (503.091)	0.522 (0.342)	0.180 (0.249)	0.234 (0.281)	0.057 (0.101)	0.006 (0.018)
2006	622.671 (504.320)	0.516 (0.340)	0.181 (0.247)	0.239 (0.281)	0.058 (0.101)	0.006 (0.018)
2007	614.763 (495.871)	0.508 (0.340)	0.179 (0.245)	0.247 (0.284)	0.059 (0.103)	0.006 (0.018)
2008	609.243 (493.043)	0.500 (0.338)	0.177 (0.242)	0.255 (0.286)	0.062 (0.105)	0.006 (0.019)
2009	611.626 (486.299)	0.491 (0.336)	0.175 (0.239)	0.258 (0.286)	0.062 (0.105)	0.014 (0.031)
2010	609.245 (476.974)	0.484 (0.334)	0.174 (0.236)	0.265 (0.287)	0.063 (0.105)	0.015 (0.041)
2011	616.759 (474.809)	0.472 (0.325)	0.165 (0.228)	0.271 (0.285)	0.060 (0.099)	0.033 (0.044)
2012	621.494 (472.099)	0.464 (0.322)	0.162 (0.224)	0.278 (0.286)	0.061 (0.101)	0.035 (0.043)
2013	620.371 (469.701)	0.459 (0.320)	0.160 (0.222)	0.283 (0.286)	0.061 (0.102)	0.037 (0.043)
2014	622.580 (470.584)	0.452 (0.318)	0.159 (0.220)	0.288 (0.287)	0.062 (0.103)	0.039 (0.042)
2015	624.237 (475.205)	0.445 (0.315)	0.158 (0.217)	0.293 (0.286)	0.064 (0.105)	0.041 (0.042)
2016	624.949 (479.389)	0.439 (0.312)	0.156 (0.215)	0.298 (0.285)	0.065 (0.107)	0.043 (0.043)
2017	625.079 (482.574)	0.433 (0.309)	0.155 (0.212)	0.302 (0.284)	0.066 (0.109)	0.045 (0.044)
2018	624.866 (484.410)	0.427 (0.306)	0.154 (0.210)	0.305 (0.283)	0.068 (0.111)	0.047 (0.044)
2019	622.489 (484.819)	0.422 (0.304)	0.153 (0.208)	0.308 (0.282)	0.069 (0.113)	0.049 (0.045)
Total	619.515 (483.599)	0.468 (0.326)	0.166 (0.228)	0.275 (0.286)	0.062 (0.105)	0.028 (0.041)

Source.—Public and Private School Universe Survey 2005-2019.

Notes.—The table reports means and standard deviations of mean total enrollment and race/ethnicity shares of enrollment in public schools across US Metropolitan Statistical Areas. All observations for race/ethnicity specific enrollment shares are weighted by the total enrollment size of the school.

Table 4.2. Race/Ethnicity Shares of Enrollment in Private Schools

Year	Mean Total Enrollment	Race/Ethnicity Shares of Enrollment				
		White	Black	Hispanic	Asian	Other
2006	169.975 (222.960)	0.729 (0.299)	0.105 (0.204)	0.107 (0.191)	0.040 (0.097)	0.019 (0.065)
2008	169.803 (230.880)	0.717 (0.298)	0.107 (0.203)	0.111 (0.191)	0.061 (0.120)	0.003 (0.020)
2010	169.018 (229.480)	0.689 (0.299)	0.103 (0.197)	0.110 (0.189)	0.058 (0.108)	0.039 (0.078)
2012	164.066 (227.191)	0.674 (0.297)	0.100 (0.190)	0.116 (0.193)	0.062 (0.110)	0.048 (0.084)
2014	167.444 (226.842)	0.658 (0.296)	0.101 (0.187)	0.120 (0.197)	0.067 (0.114)	0.054 (0.089)
2016	172.913 (237.560)	0.644 (0.297)	0.101 (0.186)	0.126 (0.200)	0.070 (0.115)	0.059 (0.093)
2018	178.279 (259.351)	0.632 (0.296)	0.101 (0.182)	0.128 (0.198)	0.073 (0.122)	0.066 (0.106)
Total	169.943 (232.957)	0.680 (0.300)	0.103 (0.194)	0.116 (0.194)	0.061 (0.113)	0.040 (0.082)

Source.—Public and Private School Universe Survey 2005-2019.

Notes.—The table reports means and standard deviations of mean total enrollment and race/ethnicity shares of enrollment in private schools across US Metropolitan Statistical Areas. All observations for race/ethnicity specific enrollment shares are weighted by the total enrollment size of the school.

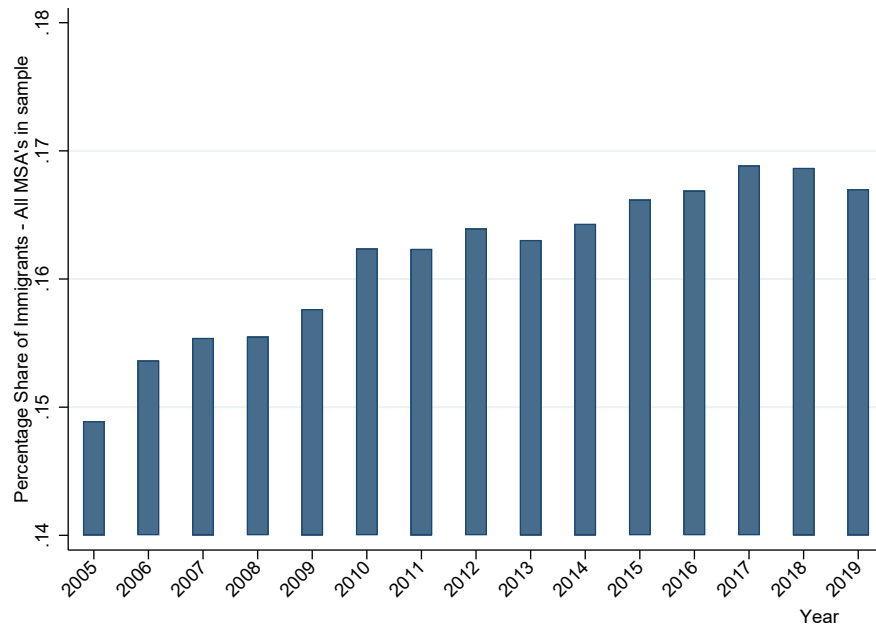
Table 4.3. Race/Ethnicity Shares of Enrollment in Public Charter Schools

Year	Mean Total Enrollment	Race/Ethnicity Shares of Enrollment				
		White	Black	Hispanic	Asian	Other
2005	293.771 (351.645)	0.372 (0.337)	0.350 (0.363)	0.235 (0.277)	0.034 (0.077)	0.010 (0.048)
2006	297.714 (365.366)	0.359 (0.337)	0.356 (0.367)	0.240 (0.282)	0.036 (0.083)	0.009 (0.043)
2007	313.868 (379.145)	0.345 (0.332)	0.347 (0.362)	0.262 (0.295)	0.037 (0.085)	0.009 (0.043)
2008	323.484 (392.513)	0.342 (0.332)	0.345 (0.363)	0.265 (0.297)	0.039 (0.089)	0.008 (0.040)
2009	336.269 (425.053)	0.333 (0.325)	0.336 (0.359)	0.271 (0.298)	0.040 (0.088)	0.020 (0.051)
2010	354.846 (451.946)	0.327 (0.323)	0.328 (0.355)	0.281 (0.301)	0.039 (0.088)	0.025 (0.085)
2011	371.531 (485.647)	0.319 (0.315)	0.312 (0.348)	0.294 (0.303)	0.035 (0.077)	0.040 (0.076)
2012	398.108 (550.133)	0.317 (0.312)	0.307 (0.343)	0.300 (0.304)	0.038 (0.084)	0.038 (0.066)
2013	411.446 (580.487)	0.315 (0.309)	0.295 (0.336)	0.312 (0.307)	0.040 (0.087)	0.038 (0.056)
2014	426.986 (593.661)	0.311 (0.304)	0.289 (0.332)	0.322 (0.306)	0.040 (0.086)	0.039 (0.057)
2015	438.039 (587.893)	0.301 (0.298)	0.287 (0.330)	0.329 (0.308)	0.042 (0.087)	0.041 (0.060)
2016	449.385 (587.785)	0.291 (0.292)	0.284 (0.327)	0.340 (0.310)	0.042 (0.089)	0.043 (0.055)
2017	463.327 (596.710)	0.284 (0.287)	0.279 (0.323)	0.349 (0.311)	0.043 (0.092)	0.045 (0.053)
2018	469.339 (543.240)	0.283 (0.284)	0.271 (0.319)	0.354 (0.313)	0.044 (0.094)	0.047 (0.059)
2019	478.522 (552.510)	0.277 (0.280)	0.267 (0.315)	0.363 (0.311)	0.045 (0.096)	0.048 (0.055)
Total	404.723 (527.766)	0.308 (0.305)	0.298 (0.337)	0.317 (0.307)	0.041 (0.088)	0.036 (0.060)

Source.—Public and Private School Universe Survey 2005-2019.

Notes.—The table reports means and standard deviations of mean total enrollment and race/ethnicity shares of enrollment in public charter schools across US Metropolitan Statistical Areas. All observations for race/ethnicity specific enrollment shares are weighted by the total enrollment size of the school.

Figure 4.2. Share of Immigrants in the Sample MSA's 2005-2019



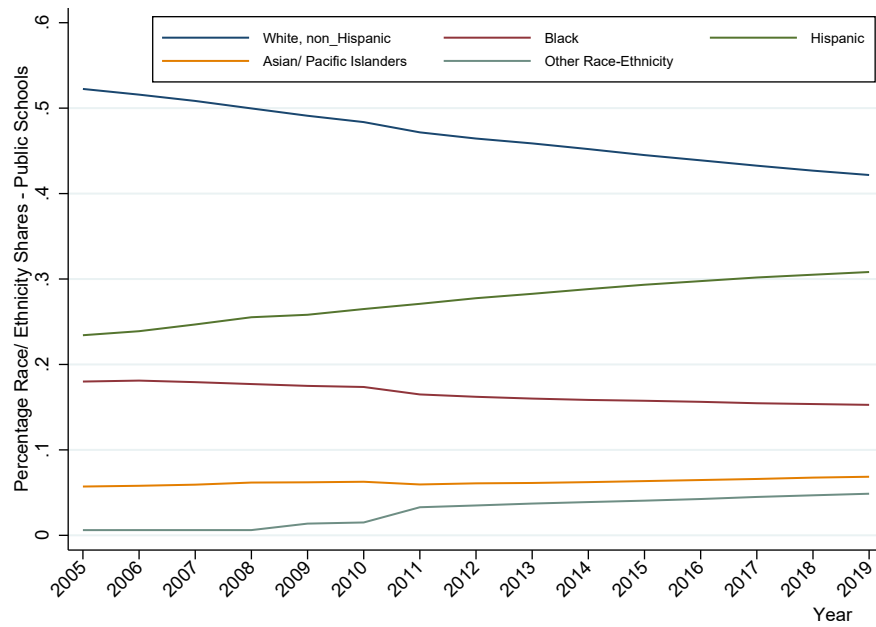
Source.—ACS 2005-2019, Ruggles et al. (2019)

Table 4.4. Variation in Immigrant Shares of Population across MSAs - 2019

MSA's with the highest shares of immigrants		MSA's with the lowest shares of immigrants	
MSA Name	Share of foreigners	MSA Name	Share of foreigners
San Diego-Carlsbad, CA	0.223	Johnstown, PA	0.005
Brownsville-Harlingen, TX	0.231	Anniston-Oxford-Jacksonville, AL	0.008
Trenton, NJ	0.235	Saginaw, MI	0.013
Salinas, CA	0.238	Muncie, IN	0.015
McAllen-Edinburg-Mission, TX	0.239	Canton-Massillon, OH	0.016
El Paso, TX	0.241	Youngstown-Warren-Board, OH-PA	0.017
New York-Newark-Jersey City, NY-NJ	0.278	Michigan City- Laporte, IN	0.019
San Francisco-Oakland-Hayward, CA	0.299	Jackson, MI	0.019
Los Angeles-Long Beach-Anaheim, CA	0.329	Eau Claire, WI	0.020
Miami-Fort Lauderdale-West Palm, FA	0.391	Springfield, MO	0.021

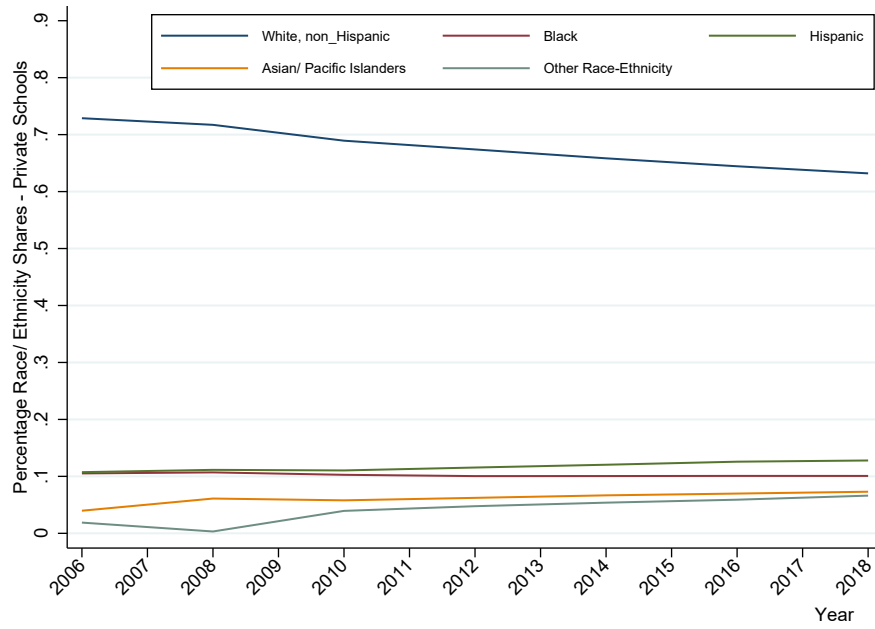
Source.—ACS 2005-2019, Ruggles et al. (2019)

Figure 4.3. Race/ Ethnicity Enrollment Shares in Public Schools: 2005-2019



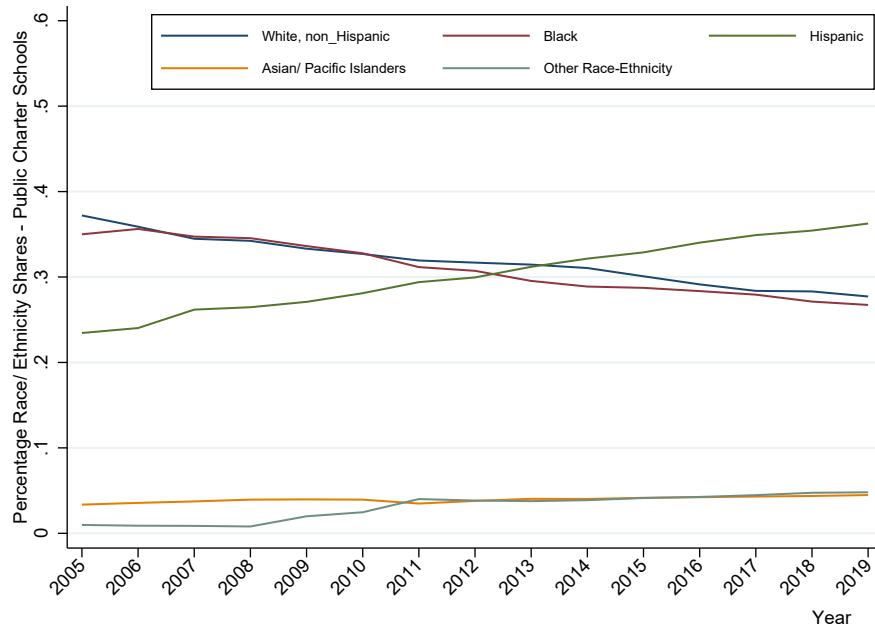
Source.—Public and Private School Universe Survey 2005-2019.

Figure 4.4. Race/ Ethnicity Enrollment Shares in Private Schools: 2005-2019



Source.—Public and Private School Universe Survey 2005-2019.

Figure 4.5. Race/ Ethnicity Enrollment Shares in Public Charter Schools: 2005-2019



Source.—Public and Private School Universe Survey 2005-2019.

Table 4.5. Sample First Stage

	Share of Immigrants in the Population				
	(1)	(2)	(3)	(4)	(5)
$\text{Log}(\sum_j \text{share}_{i,j,1980} \times \text{Total Immig}_{j,t})$	3.594*** (0.099)	2.579*** (0.084)	6.285*** (0.380)	4.651*** (0.564)	2.124*** (0.442)
First-Stage F-Statistic	1317.91	942.64	273.55	68.00	23.09
Number of MSAs	166	166	166	166	148
Controls	No	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes
Exc. Miami, New York, LA, and all cities of California	No	No	No	No	Yes

Source.— ACS 2005-2019 Ruggles et al. (2019).

Notes.— *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

OLS Regressions for the first stage (Total observations: $N=2490$; 166 MSA's $\times$ 15 years). The table presents coefficients and standard errors obtained by regressing the share of immigrants in the population on the predicted/ instrumented number of immigrants within MSA $\times$ year cells, conditional on MSA-level time variant controls. All regressions are weighted by the size of MSA population. Additional controls include MSA-level time-varying parameters which include percentage of population living below poverty level, mean housing values, and mean MSA family income. Standard errors clustered within MSA $\times$ year cells.

Table 4.6. Public School Enrollment Estimates

	OLS		Fixed Effects			IV 2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log of Total Enrollment						
(%) Share of Immigrants	1.060*** (0.073)	0.706*** (0.081)	0.309 (0.307)	0.676*** (0.079)	-0.051 (0.337)	2.573*** (0.738)
Observations	770,909	770,909	770,909	770,909	770,909	770,909
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel B: Share of White Enrollment						
(%) Share of Immigrants	-1.386*** (0.036)	-1.274*** (0.059)	-1.208*** (0.136)	-1.266*** (0.059)	-0.177 (0.131)	-0.555* (0.310)
Observations	770,909	770,909	770,909	770,909	770,909	770,909
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel C: Share of Black Enrollment						
(%) Share of Immigrants	-0.346*** (0.034)	-0.061 (0.051)	-1.359*** (0.088)	-0.060 (0.053)	-0.293*** (0.062)	-0.760*** (0.186)
Observations	770,909	770,909	770,909	770,909	770,909	770,909
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel D: Share of Hispanic Enrollment						
(%) Share of Immigrants	1.409*** (0.048)	1.284*** (0.080)	1.109*** (0.086)	1.312*** (0.081)	0.256*** (0.077)	0.482** (0.213)
Observations	770,909	770,909	770,909	770,909	770,909	770,909
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel E: Share of Asian Enrollment						
(%) Share of Immigrants	0.334*** (0.020)	0.132*** (0.022)	0.557*** (0.048)	0.115*** (0.022)	0.373*** (0.050)	0.572*** (0.091)
Observations	770,909	770,909	770,909	770,909	770,909	770,909
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes

Source.—Public and Private School Universe Survey 2005-2019 & ACS 2005-2019 Ruggles et al. (2019)

Notes.—*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The table reports coefficients and standard errors associated with regressions of various public school enrollment outcomes on percentage share of immigrants in the population, conditional on school level characteristics that include school-level, type of school, student-teacher ratio, total number of full-time employed teachers, and Title-1 status of school. Time-variant MSA level controls include mean poverty levels, mean household values, and mean household income. All regressions in Panel B through E are weighted for the total school enrollments. Standard errors are clustered within MSA $\times$ year cells. The sample does not include public charter schools.)

Table 4.7. Private School Enrollment Estimates

	OLS		Fixed Effects			IV 2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log of Total Enrollment						
(%) Share of Immigrants	0.194*** (0.049)	0.035 (0.087)	-1.551*** (0.485)	0.068 (0.083)	0.109 (0.535)	0.310 (1.417)
Observations	119,197	119,197	119,197	119,197	119,197	119,197
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel B: Share of White Enrollment						
(%) Share of Immigrants	-1.179*** (0.029)	-1.189*** (0.054)	-1.798*** (0.320)	-1.194*** (0.053)	0.434 (0.319)	1.357* (0.755)
Observations	119,197	119,197	119,197	119,197	119,197	119,197
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel C: Share of Black Enrollment						
(%) Share of Immigrants	0.020 (0.039)	0.294*** (0.048)	-0.371*** (0.139)	0.292*** (0.048)	-0.412*** (0.152)	-0.716* (0.422)
Observations	119,197	119,197	119,197	119,197	119,197	119,197
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel D: Share of Hispanic Enrollment						
(%) Share of Immigrants	0.783*** (0.048)	0.917*** (0.053)	0.463*** (0.143)	0.955*** (0.045)	-0.067 (0.158)	-0.132 (0.485)
Observations	119,197	119,197	119,197	119,197	119,197	119,197
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel E: Share of Asian Enrollment						
(%) Share of Immigrants	0.259*** (0.032)	0.010 (0.036)	0.380*** (0.088)	-0.003 (0.035)	0.294* (0.175)	-0.092 (0.279)
Observations	119,197	119,197	119,197	119,197	119,197	119,197
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes

Source.—Public and Private School Universe Survey 2005-2019 & ACS 2005-2019 Ruggles et al. (2019)

Notes.—*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The table reports coefficients and standard errors associated with regressions of various private school enrollment outcomes on percentage share of immigrants in the population, conditional on school level characteristics that include school-level, type of school, student-teacher ratio, total number of full-time employed teachers, and any religious affiliation of school. Time-variant MSA level controls include mean poverty levels, mean household values, and mean household income. All regressions in Panel B through E are weighted for the total school enrollments. Standard errors are clustered within MSA $\times$ year cells.

Table 4.8. Public Charter School Enrollment Estimates

	OLS		Fixed Effects			IV 2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log of Total Enrollment						
(%) Share of Immigrants	0.771*** (0.117)	1.084*** (0.138)	1.942** (0.880)	1.022*** (0.144)	-3.383*** (0.657)	-3.745* (2.084)
Observations	64,265	64,265	64,265	64,265	64,265	64,265
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel B: Share of White Enrollment						
(%) Share of Immigrants	-0.893*** (0.044)	-1.152*** (0.076)	-0.584*** (0.177)	-1.156*** (0.079)	-0.073 (0.187)	0.316 (0.520)
Observations	64,265	64,265	64,265	64,265	64,265	64,265
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel C: Share of Black Enrollment						
(%) Share of Immigrants	-0.489*** (0.082)	0.160 (0.108)	-1.802*** (0.190)	0.165 (0.109)	-0.488*** (0.145)	-0.017 (0.521)
Observations	64,265	64,265	64,265	64,265	64,265	64,265
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel D: Share of Hispanic Enrollment						
(%) Share of Immigrants	1.317*** (0.055)	1.045*** (0.085)	1.519*** (0.140)	1.065*** (0.090)	0.641*** (0.118)	-0.507 (0.367)
Observations	64,265	64,265	64,265	64,265	64,265	64,265
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel E: Share of Asian Enrollment						
(%) Share of Immigrants	0.084*** (0.014)	0.038** (0.017)	0.318*** (0.057)	0.028* (0.017)	0.123* (0.064)	-0.095 (0.131)
Observations	64,265	64,265	64,265	64,265	64,265	64,265
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes

Source.—Public and Private School Universe Survey 2005-2019 & ACS 2005-2019 Ruggles et al. (2019)

Notes.—*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The table reports coefficients and standard errors associated with regressions of various public charter school enrollment outcomes on percentage share of immigrants in the population, conditional on school level characteristics that include school-level, type of school, student-teacher ratio, total number of full-time employed teachers, and Title-1 status of school. Time-variant MSA level controls include mean poverty levels, mean household values, and mean household income. All regressions in Panel B through E are weighted for the total school enrollments. Standard errors are clustered within MSA $\times$ year cells.

CONCLUSION

The three essays in this dissertation contribute to develop an understanding of the current state of inclusion and accessibility in the U.S. education system. The essay on the child care regulations is an effort to assess the impact of policy regulations on affordability and quality of child care services. Similarly, the essay on full-day kindergarten pays special attention to heterogeneous employment impacts across different subgroups of mothers. Finally, the chapter on the impact of urban immigration on native flight from K-12 schools shifts the focus on population behaviors in relation to inclusion and demographic diversity. In summary, all three chapters of the dissertation are an effort to draw policy implications which speak to an overarching objective of inclusive access to education for an increasingly diverse U.S. population.

All three papers demonstrate a robust and state-of-the art approach to research. In particular, in the first essay on child care regulations, new data on child care regulations is merged with job market data which brings a fresh perspective on child care regulations. The findings are relevant for immediate policy information, as well as long-run policy information in similar circumstance. The second paper on maternal employment impacts of full-day kindergarten expansion applies innovative estimation strategies to arrive at plausibly exogenous estimates for a local, Arizona based policy-intervention. The third paper on K-12 school enrollments also uses a creative data appropriation approach to a research problem to construct the data.

Potential short-run and long-run implications from this dissertation are important from a policy perspective. Policy implications from the first paper are valid for short

and long run policy information. The impact of regulations as a policy instrument in the short-run have been relevant in the context of child care markets recovering from the pandemic. The paper also contributes to child care regulations literature in general. For example, a lack of substantial policy variation has always lead to doubts about the true nature of the impacts of regulations on the demand for child care labor. The current context only provides an unprecedented window of plausibly exogenous policy shocks to these regulations, hence providing unprecedented levels of within-state over-time variation in these regulations.

The implications from the second paper inform medium to long-term labor policies. I find robust evidence of heterogeneous maternal employment gains within Arizona, and also add to a growing literature which points towards the need and potential benefits of publicly funded universal kindergarten across the U.S.

The third paper examines a consistent trend of demographic changes across the U.S., within the K-12 education context. Using fresh data, the paper revisits the existing evidence on the impacts of immigration on enrollment changes within the K-12 school system, augmented with a robust estimation strategy. The paper highlights policy relevant issues within school education context that arise from a demographic change of this scale.

To summarize, the findings from this dissertation point out the need for investments in child care, kindergarten and K-12 schooling for long-run outcomes such as human capital development and closing of academic achievement gaps across races and ethnicities.

As is the case for many research works, this dissertation has several limitations. While I do try my best to minimize limitations through research design, some of these limitations still exist and I acknowledge some of the major limitations in this

paragraph. The paper on child care regulations focuses on formal care, and does not take into account informal care. Since the study is focused on a period surrounding COVID-19, it could be likely that informal care substituted formal care, specially when parents remained at home. For the case of low-skilled workforce, it could be the case that reduced demand in the formal sector might have led them to enter the informal market. The empirical strategy works well with binding regulations¹, but there were many non-binding recommendations communicated from governors or state health offices. There is no easy way to assess the labor market impact of unbinding regulations or recommendations, although I do speculate that any compliance with unbinding regulations would have remained minimal, since it is apparent that providers were already facing severe financial constraints. The paper on full-day kindergarten expansion in Arizona has a robust difference-in-differences design, and further utilizes the difference across the school-year months and summer months to run a version of triple differences. But data limitations and the sample structures of the CPS data, do not allow for a triple differences specification where the third difference could be based on age. The third paper that examines native flight is focused on urban metropolitan areas. Although, immigrant settlement patterns tend to be concentrated across urban metropolitan areas, the findings are most likely limited to urban areas, and not applicable to rural areas.

The dissertation serves to open future research directions. In the context of child care regulations, supply-side effects on the child care market are only one piece of the puzzle. Future research should look at the demand for child care consumption, and perhaps try to dissect possible channels for any changes in the demand for child care. Quantification of increasing costs could also be a fruitful exercise. Another future

¹Binding regulations are the only regulations that are considered in this paper

direction is the question of how these regulations affect the quality rating mechanisms for child care businesses. In the context of full-day kindergarten, there are other natural experiments that have started within the state of Arizona, specifically after the year 2017. There are slight changes in how some individual school districts have started to fund full-day kindergarten. An interesting question would be a comparison of the outcomes from current changes to the one under examined in this dissertation. Also, a related outcome is school readiness among participating kids, subject to data availability. On similar notes, the study of K-12 enrollment points to heterogeneous effects which indicates potential for further research, investigating why some sub-populations are more likely to exhibit patterns of native flight. In summary, the dissertation adds to the existing literature, informing policy in a meaningful and relevant way, while serving as a stepping-stone towards a sustained research agenda.

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APPENDIX A

LAGGED ENROLLMENT RESPONSES TO IMMIGRATION

Table A1. Public School Enrollment Estimates

	OLS		Fixed Effects			IV 2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log of Total Enrollment						
(%) L1.Share of Immigrants	1.047*** (0.077)	0.639*** (0.084)	0.585*** (0.200)	0.647*** (0.083)	0.085 (0.233)	2.397*** (0.664)
Observations	702,905	702,905	702,905	702,905	702,905	702,905
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel B: Share of White Enrollment						
(%) L1.Share of Immigrants	-1.389*** (0.038)	-1.269*** (0.061)	-1.515*** (0.119)	-1.277*** (0.061)	-0.387*** (0.122)	-0.628** (0.309)
Observations	702,905	702,905	702,905	702,905	702,905	702,905
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel C: Share of Black Enrollment						
(%) L1.Share of Immigrants	-0.351*** (0.035)	-0.070 (0.053)	-1.387*** (0.088)	-0.073 (0.055)	-0.195*** (0.062)	-0.744*** (0.200)
Observations	702,905	702,905	702,905	702,905	702,905	702,905
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel D: Share of Hispanic Enrollment						
(%) L1.Share of Immigrants	1.418*** (0.050)	1.300*** (0.084)	1.257*** (0.085)	1.342*** (0.085)	0.375*** (0.085)	0.575** (0.227)
Observations	702,905	702,905	702,905	702,905	702,905	702,905
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel E: Share of Asian Enrollment						
(%) L1.Share of Immigrants	0.332*** (0.020)	0.129*** (0.023)	0.565*** (0.046)	0.112*** (0.022)	0.384*** (0.056)	0.657*** (0.100)
Observations	702,905	702,905	702,905	702,905	702,905	702,905
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes

Source.—Public and Private School Universe Survey 2005-2019 & ACS 2005-2019 Ruggles et al. (2019)

Notes.—*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The table reports coefficients and standard errors associated with regressions of various public school enrollment outcomes on lagged percentage share of immigrants in the population, conditional on school level characteristics that include school-level, type of school, student-teacher ratio, total number of full-time employed teachers, and Title-1 status of school. Time-variant MSA level controls include mean poverty levels, mean household values, and mean household income. All regressions in Panel B through E are weighted for the total school enrollments. Standard errors are clustered within MSA $\times$ year cells. The sample does not include public charter schools.)

Table A2. Private School Enrollment Estimates

	OLS		Fixed Effects			IV 2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log of Total Enrollment						
(%) L1.Share of Immigrants	0.195*** (0.049)	0.071 (0.092)	-1.137* (0.605)	0.096 (0.088)	0.254 (0.602)	0.570 (1.284)
Observations	119,197	119,197	119,197	119,197	119,197	119,197
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel B: Share of White Enrollment						
(%) L1.Share of Immigrants	-1.194*** (0.031)	-1.208*** (0.061)	-1.753*** (0.304)	-1.204*** (0.059)	0.480 (0.304)	1.408** (0.671)
Observations	119,197	119,197	119,197	119,197	119,197	119,197
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel C: Share of Black Enrollment						
(%) L1.Share of Immigrants	0.015 (0.040)	0.296*** (0.051)	-0.404*** (0.141)	0.293*** (0.051)	-0.498*** (0.158)	-1.005*** (0.380)
Observations	119,197	119,197	119,197	119,197	119,197	119,197
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel D: Share of Hispanic Enrollment						
(%) L1.Share of Immigrants	0.795*** (0.048)	0.951*** (0.052)	0.561*** (0.121)	0.975*** (0.047)	-0.025 (0.148)	-0.083 (0.414)
Observations	119,197	119,197	119,197	119,197	119,197	119,197
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel E: Share of Asian Enrollment						
(%) L1.Share of Immigrants	0.265*** (0.031)	0.009 (0.037)	0.420*** (0.091)	0.001 (0.036)	0.237* (0.125)	0.192 (0.249)
Observations	119,197	119,197	119,197	119,197	119,197	119,197
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes

Source.—Public and Private School Universe Survey 2005-2019 & ACS 2005-2019 Ruggles et al. (2019)

Notes.—*** p<0.01, ** p<0.05, * p<0.1

The table reports coefficients and standard errors associated with regressions of various private school enrollment outcomes on lagged percentage share of immigrants in the population, conditional on school level characteristics that include school-level, type of school, student-teacher ratio, total number of full-time employed teachers, and any religious affiliation of school. Time-variant MSA level controls include mean poverty levels, mean household values, and mean household income. All regressions in Panel B through E are weighted for the total school enrollments. Standard errors are clustered within MSA × year cells.

Table A3. Public Charter School Enrollment Estimates

	OLS		Fixed Effects			IV 2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log of Total Enrollment						
(%) L1.Share of Immigrants	0.727*** (0.120)	1.099*** (0.145)	2.971*** (0.755)	1.081*** (0.150)	-2.140*** (0.794)	4.136 (4.312)
Observations	54,972	54,972	54,972	54,972	54,972	54,972
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel B: Share of White Enrollment						
(%) L1.Share of Immigrants	-0.936*** (0.043)	-1.155*** (0.078)	-0.482*** (0.172)	-1.159*** (0.080)	-0.032 (0.187)	0.890 (0.603)
Observations	54,972	54,972	54,972	54,972	54,972	54,972
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel C: Share of Black Enrollment						
(%) L1.Share of Immigrants	-0.423*** (0.081)	0.171 (0.111)	-1.819*** (0.168)	0.164 (0.109)	-0.482*** (0.124)	-0.241 (0.395)
Observations	54,972	54,972	54,972	54,972	54,972	54,972
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel D: Share of Hispanic Enrollment						
(%) L1.Share of Immigrants	1.303*** (0.057)	1.050*** (0.088)	1.430*** (0.157)	1.074*** (0.092)	0.621*** (0.171)	-0.816 (0.538)
Observations	54,972	54,972	54,972	54,972	54,972	54,972
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel E: Share of Asian Enrollment						
(%) L1.Share of Immigrants	0.079*** (0.014)	0.034* (0.018)	0.293*** (0.048)	0.029* (0.018)	0.128** (0.062)	0.049 (0.088)
Observations	54,972	54,972	54,972	54,972	54,972	54,972
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes

Source.—Public and Private School Universe Survey 2005-2019 & ACS 2005-2019 Ruggles et al. (2019)

Notes.—*** p<0.01, ** p<0.05, * p<0.1

The table reports coefficients and standard errors associated with regressions of various public charter school enrollment outcomes on lagged percentage share of immigrants in the population, conditional on school level characteristics that include school-level, type of school, student-teacher ratio, total number of full-time employed teachers, and Title-1 status of school. Time-variant MSA level controls include mean poverty levels, mean household values, and mean household income. All regressions in Panel B through E are weighted for the total school enrollments. Standard errors are clustered within MSA $\times$ year cells.

APPENDIX B

ENROLLMENT RESPONSES TO IMMIGRANTS AGED 3-20

Table B1. Sample First Stage for Immigrant Share in Young

	Share of Immigrants in the Population Age 3-20				
	(1)	(2)	(3)	(4)	(5)
$\text{Log}(\sum_j \text{share}_{i,j,1980} \times \text{Total Immig}_{j,t})$	0.939*** (0.031)	0.671*** (0.039)	-2.439*** (0.303)	4.706*** (0.554)	2.124*** (0.442)
First-Stage F-Statistic	917.50	296.01	64.79	72.16	23.09
Number of MSAs	166	166	166	166	148
Controls	No	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes
Exc. Miami, New York, LA, and all cities of California	No	No	No	No	Yes

Source.— ACS 2005-2019 Ruggles et al. (2019)

Notes.— Standard errors clustered within MSA $\times$ year cells *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
 OLS Regressions for the first stage. (Total observations: $N=2490$; 166 MSA's $\times$ 15 years. The table presents coefficients and standard errors obtained by regressing the share of immigrants in the population aged 3-20 on the predicted/ instrumented total number of immigrants within MSA $\times$ year cells, conditional on MSA-level time variant controls. All regressions are weighted by the size of MSA population. Additional controls include MSA-level time-varying parameters which include percentage of population living below poverty level, mean housing values, and mean MSA family income.

Table B2. Public School Enrollment Estimates

	OLS		Fixed Effects			IV 2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log of Total Enrollment						
(%) Share of Immigrants in Population 3-20	3.077*** (0.229)	1.597*** (0.192)	0.321* (0.169)	1.743*** (0.194)	0.754*** (0.153)	2.454*** (0.640)
Observations	770,909	770,909	770,909	770,909	770,909	770,909
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel B: Share of White Enrollment						
(%) Share of Immigrants in Population 3-20	-3.936*** (0.144)	-2.476*** (0.161)	0.893*** (0.114)	-2.730*** (0.170)	0.150 (0.093)	-0.530* (0.288)
Observations	770,909	770,909	770,909	770,909	770,909	770,909
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel C: Share of Black Enrollment						
(%) Share of Immigrants in Population 3-20	-0.784*** (0.132)	0.027 (0.155)	0.738*** (0.072)	-0.191 (0.168)	-0.204*** (0.051)	-0.725*** (0.191)
Observations	770,909	770,909	770,909	770,909	770,909	770,909
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel D: Share of Hispanic Enrollment						
(%) Share of Immigrants in Population 3-20	3.842*** (0.189)	2.487*** (0.233)	-0.852*** (0.089)	2.817*** (0.246)	-0.229*** (0.069)	0.459** (0.207)
Observations	770,909	770,909	770,909	770,909	770,909	770,909
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel E: Share of Asian Enrollment						
(%) Share of Immigrants in Population 3-20	1.002*** (0.080)	0.284*** (0.068)	-0.037 (0.039)	0.286*** (0.069)	0.243*** (0.038)	0.545*** (0.093)
Observations	770,909	770,909	770,909	770,909	770,909	770,909
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes

Source.—Public and Private School Universe Survey 2005-2019 & ACS 2005-2019 Ruggles et al. (2019)

Notes.—*** p<0.01, ** p<0.05, * p<0.1

The table reports coefficients and standard errors associated with regressions of various public school enrollment outcomes on lagged percentage share of immigrants in the population aged 3-20, conditional on school level characteristics that include school-level, type of school, student-teacher ratio, total number of full-time employed teachers, and Title-1 status of school. Time-variant MSA level controls include mean poverty levels, mean household values, and mean household income. All regressions in Panel B through E are weighted for the total school enrollments. Standard errors are clustered within MSA × year cells. The sample does not include public charter schools.)

Table B3. Private School Enrollment Estimates

	OLS		Fixed Effects			IV 2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log of Total Enrollment						
(%) Share of Immigrants in Population 3-20	0.740*** (0.175)	0.371 (0.237)	0.970*** (0.343)	0.346 (0.231)	0.369 (0.317)	0.297 (1.367)
Observations	119,197	119,197	119,197	119,197	119,197	119,197
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel B: Share of White Enrollment						
(%) Share of Immigrants in Population 3-20	-3.653*** (0.124)	-2.694*** (0.159)	0.777*** (0.228)	-2.895*** (0.132)	-0.158 (0.153)	1.303 (0.861)
Observations	119,197	119,197	119,197	119,197	119,197	119,197
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel C: Share of Black Enrollment						
(%) Share of Immigrants in Population 3-20	0.152 (0.126)	0.641*** (0.133)	0.332*** (0.081)	0.625*** (0.131)	0.309*** (0.086)	-0.688 (0.481)
Observations	119,197	119,197	119,197	119,197	119,197	119,197
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel D: Share of Hispanic Enrollment						
(%) Share of Immigrants in Population 3-20	2.503*** (0.168)	2.374*** (0.126)	0.054 (0.127)	2.543*** (0.094)	0.330*** (0.127)	-0.126 (0.476)
Observations	119,197	119,197	119,197	119,197	119,197	119,197
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel E: Share of Asian Enrollment						
(%) Share of Immigrants in Population 3-20	0.746*** (0.134)	-0.073 (0.097)	-0.040 (0.151)	-0.089 (0.094)	0.077 (0.145)	-0.089 (0.266)
Observations	119,197	119,197	119,197	119,197	119,197	119,197
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes

Source.—Public and Private School Universe Survey 2005-2019 & ACS 2005-2019 Ruggles et al. (2019)

Notes.—*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The table reports coefficients and standard errors associated with regressions of various private school enrollment outcomes on lagged percentage share of immigrants in the population aged 3-20, conditional on school level characteristics that include school-level, type of school, student-teacher ratio, total number of full-time employed teachers, and any religious affiliation of school. Time-variant MSA level controls include mean poverty levels, mean household values, and mean household income. All regressions in Panel B through E are weighted for the total school enrollments. Standard errors are clustered within MSA $\times$ year cells.

Table B4. Public Charter School Enrollment Estimates

	OLS		Fixed Effects			IV 2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log of Total Enrollment						
(%) Share of Immigrants in Population 3-20	1.048*** (0.392)	0.504 (0.376)	-6.328*** (0.752)	1.151*** (0.332)	-2.808*** (0.703)	-3.877* (2.173)
Observations	64,265	64,265	64,265	64,265	64,265	64,265
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel B: Share of White Enrollment						
(%) Share of Immigrants in Population 3-20	-2.487*** (0.193)	-2.198*** (0.186)	0.471*** (0.171)	-2.279*** (0.205)	0.227 (0.166)	0.327 (0.541)
Observations	64,265	64,265	64,265	64,265	64,265	64,265
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel C: Share of Black Enrollment						
(%) Share of Immigrants	-1.037*** (0.254)	0.232 (0.266)	1.131*** (0.135)	-0.043 (0.267)	-0.262** (0.125)	-0.018 (0.540)
Observations	64,265	64,265	64,265	64,265	64,265	64,265
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel D: Share of Hispanic Enrollment						
(%) Share of Immigrants in Population 3-20	3.468*** (0.209)	2.219*** (0.221)	-0.839*** (0.142)	2.492*** (0.256)	-0.054 (0.119)	-0.525 (0.380)
Observations	64,265	64,265	64,265	64,265	64,265	64,265
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes
Panel E: Share of Asian Enrollment						
(%) Share of Immigrants in Population 3-20	0.230*** (0.054)	0.063 (0.046)	-0.161*** (0.056)	0.072 (0.049)	0.042 (0.054)	-0.098 (0.136)
Observations	64,265	64,265	64,265	64,265	64,265	64,265
School Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-variant MSA Controls	No	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	No	No	Yes	No	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	Yes

Source.—Public and Private School Universe Survey 2005-2019 & ACS 2005-2019 Ruggles et al. (2019)

Notes.—*** p<0.01, ** p<0.05, * p<0.1

The table reports coefficients and standard errors associated with regressions of various public charter school enrollment outcomes on lagged percentage share of immigrants in the population aged 3-20, conditional on school level characteristics that include school-level, type of school, student-teacher ratio, total number of full-time employed teachers, and Title-1 status of school. Time-variant MSA level controls include mean poverty levels, mean household values, and mean household income. All regressions in Panel B through E are weighted for the total school enrollments. Standard errors are clustered within MSA $\times$ year cells.

APPENDIX C

AUTHOR STATEMENT FOR CHAPTER 2

An earlier version of this chapter has been published as an IZA working paper with Dr. Herbst and Dr. Makridis as co-authors. I certify that I have explicit permission from my co-authors for using this subsequent paper for my dissertation chapter.

As the first author on this revised version of the paper in this dissertation, Ali conceptualized the paper, researched, collected and cleaned data on child care regulations, conducted formal analysis and contributed to visualization as well as the initial drafting of the paper. Makridis procured labor market data on job postings from Emsi, and Makridis and Herbst both led and supervised subsequent revisions and drafting of the paper.