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The Returns of an Additional Year of Schooling: The Case of State-Mandated Kindergarten

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Abstract

In this paper, we examine the effects of state mandatory kindergarten requirements on long-run educational attainment and labor market outcomes. While in most states kindergarten began as a voluntary program, starting in the 1970s some states evolved to mandating kindergarten attendance. Several changes in state mandatory school entrance laws across states over time provide an opportunity to causally identify the influence of an additional year of ECE on important individual education and labor market outcomes, comparing states with mandatory attendance to those with voluntary attendance. We exploit this natural experimental design using data from the ACS 2008-2017. Findings indicate no overall impacts of mandatory kindergarten policies on educational attainment in adulthood, but substantial heterogeneous impacts, with women and Hispanic and Black individuals benefiting most in terms of educational attainment, poverty reduction, and income. Our findings indicate that states' investments in universal early education pay off in the long run, and are equity enhancing.

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I.Introduction

In light of the evidence from neuroscience, psychology, and economics demonstrating the importance of early childhood interventions in the development of human capital (Duncan & Magnuson, 2013), a number of recent proposals at the federal and state levels aim to expand public early childhood education (ECE) programs. Because early childhood is a critical period of rapid neurological development, investments during the earliest years of life, prior to school entry, have been shown to be among the most productive social policy investments with substantial returns for both the individual and society (Heckman & Masterov, 2007).

Currently amounting to $13 billion and $6.2 billion, respectively, federal and state spending on ECE programs continues to grow, with the largest allocations going towards expansions in age-4 prekindergarten (pre-k) programs, state-developed voluntary part- or full-time educational interventions (Barnett, Carolan, Squires, Brown, & Horowitz, 2015). Though in most states, pre-k programs target low-income children, (e.g., NJ, NC), a handful of states provide universal access to all age-eligible residents (e.g., GA, OK). Recent federal policy initiatives (e.g., Obama's Preschool for All), also push for universal access to age-4 preschool programs, which enjoys support from both legislators and voters across party lines (Greenberg, 2015). However, the empirical work on the returns to ECE are generated from high-quality, small-scale interventions targeted to low-income children, and there exists limited evidence on how such universal interventions during very early childhood may influence population-level human capital development.

In this paper, we look to a similar phenomenon, the origins of, and attendance mandates for American kindergarten programs to shed light on how trends towards universal provision of public pre-school may influence one's long-run educational and economic outcomes. A very similar ECE intervention, kindergarten has its roots in Germany as a preparatory program for primary school. German immigrants established the first U.S. kindergarten programs in the late 1800s, which spread to several communities across the Midwest and Northeast by the turn of the century. Coinciding with the mechanization of factories that reduced the need for child labor, along with greater awareness of children's welfare, small community-based kindergartens continued to grow (Beatty, 10995). During the 1960s, states offered block grants to districts to expand kindergarten slots in public schools, and American kindergartens gradually moved from private and community-based facilities to becoming firmly established in public and private elementary schools by the end of 20th century. In 1970, less than half of all five-year-olds attended kindergarten, compared with more than 70% today (Digest of Education Statistics, 2015). Both Cascio (2009, 2010) and Dhuey (2011) have used variation in the distribution of these expansion grants to estimate the impact of kindergarten attendance on maternal labor force participation, and children's educational, social, and health outcomes.

Central to our study is that while in most states kindergarten (KG) began as a voluntary program, between 1970-2015 some states evolved to mandating KG attendance (Tanner and Tanner, 1973). This effectively shifted the minimum school entry age from age 7 (1st grade) to 6. Several changes in state school entrance laws across states over time provide an opportunity to causally identify the influence of an additional year of ECE on individual education and labor market outcomes. We view the impacts of mandatory kindergarten (MKG) attendance on long-run outcomes as a first look at how an additional year of education during preschool will influence long-run outcomes, in a policy context where federal and state governments are actively considering universal preschool programs.

To provide causal estimates, we use a quasi-experimental design that leverages changes in MKG attendance requirements across and within states over time beginning in the 1970s, when states began mandating KG attendance (Tanner, 1973). Our analysis sample comprises pooled repeated cross-sections of individuals born between 1965 and 1987, thus exposed to changes in KG mandates between 1970 and 1992 at ages 30 and above.[1] The source of education and labor market outcomes in this preliminary draft are the 2008-2015 ACS surveys. We are currently estimating models in the confidential Restricted Data Center (RDC) data to link exact birth dates (month and year) and state of birth as identified in the 2000 Decennial long form survey and the 2001-2015 ACS surveys with KG requirements information (i.e., mandates, birthdate/age cutoff for school entry, and the introduction of state-subsidized KG). These restricted data allow us to better match individuals to their KG enrollment year policies than a match that only uses information on quarter and year of birth (which are available in the public use data).

In this preliminary draft, we provide proof of concept for our study. We show first-order effects of kindergarten mandates on kindergarten enrollment using state-by-year data from 1979 to 2000. We find that adoption of mandatory kindergarten increased KG enrollment by 12 percentage points (around 14% of the mean). Next, using the public version of the ACS 2008-2015, we analyze the impacts of MKG on long-term educational attainment, poverty status, and income. Because the public data do not have information on month of birth, we assign individuals to kindergarten cohorts and mandates based on quarter of birth, which is problematic as explained below. Our preliminary findings indicate that exposure to MKG did not have any significant overall impacts on individual's educational attainment, income, or poverty status when we assume constant effects of the policy.

Because prior research indicates that the impacts of ECE are stronger for low-income (e.g., Cascio & Schanzenbach, 2013; Cornelissen, Dustmann, Raute, & Schönberg, 2018) and non-white children (e.g., Deming, 2009; Gormley & Gayer, 2005; Puma, Bell, Cook, & Heid, 2010), and differ by sex (Elango, García, Heckman, & Hojman, 2016), we also examine heterogeneity in the effects of MKG by race, sex, and by the poverty status of each participant's neighborhood (Census Tract) during early childhood. These analyses reveal a marked differential benefit of mandatory KG attendance for non-white, Black and Hispanic children for educational attainment and income. Our preliminary effects for Black and Hispanic children's educational attainment and income show a 5 percentage point increase for college degree completion (11% of the mean) relative to white children exposed to the mandates. Non-white children exposed to MKG also experience a nearly 6.7 percent increase in wage and total income relative to white children exposed to the policy. We also similar differential impacts on education and income for women (1-2 percentage points, 5.8%).

It is important to note how our study differs from, and complements other related work. Both Cascio (2010) and Dhuey (2011) exploit variation in the timing of large KG expansion grants from states to school districts (which largely occurred in the 1960s and 1970s) to identify the returns from KG

[1] We cannot include younger cohorts as they are not old enough to observe their long-term outcomes.

attendance. Their results differ somewhat, but do indicate that state funding of KG improved educational outcomes (grade retention, graduation), reduced institutionalization, and improved earnings for non-whites and children from low socioeconomic households. Using the same identification strategy, Cascio (2009) finds evidence of an increase in the labor supply of single mothers without other young children as a result of increased KG funding, but no effects for other groups of women. This corresponds with Gelbach's (2002) findings on maternal labor supply using quarter of birth to instrument for KG enrollment, and Fitzpatrick's (2012) estimates using within-state discontinuities in the age-eligibility for kindergarten. We use MKG attendance laws to identify the impact of ECE on children's long-term education and labor market outcomes, comparing states with mandatory attendance to those with voluntary attendance across time.

A large set of studies examines the effects of children's age at KG entry on academic and labor market outcomes using age-within-cohort comparisons. These studies use either school-entry birthdate cutoff laws or quarter of birth instruments for identification, and find mixed positive (Bedard & Dhuey, 2006, 2012; Black, Devereux, & Salvanes, 2011; Datar, 2006; Datar & Gottfried, 2014; DeCicca & Smith, 2011; McEwan & Shapiro, 2008) and null effects (Barua & Lang, 2009; Buddelmeyer & Le, 2011; Cascio & Schanzenbach, 2012; Dobkin & Ferreira, 2010) of school entry age on educational attainment, achievement, and income.[2] Rather than exploiting differences in individual age-within-cohort at KG entry, our study looks at the impact of at-scale KG attendance relative to non-attendance as a function of mandatory schooling laws.

Our study is therefore also related to the compulsory schooling literature, which use variations in minimum school leaving age policies to evaluate the returns from schooling. Several seminal papers use changes in compulsory laws in the United Kingdom in 1947 that increased the minimum school leaving age to instrument for the impact of educational attainment on earnings (Devereux & Hart, 2010; Grenet, 2013; Harmon & Walker, 1995; Oreopoulos, 2006) and health and mortality (Clark and Royer, 2013). Whereas these impacts stem from an additional year of schooling in adolescence when teenagers exert their legal agency to otherwise leave, our study examines the impact of an additional year of schooling during early life, a critical developmental period for human capital formation with the potential to yield large returns on public policy investment (Heckman & Masterov, 2007).

Most similar to our study is a recent paper by Drange, Havnes, and Sandsør (2016), which takes advantage of changes in Norway's mandatory school entry age from age 7 to 6 along with spatial variations in program roll-out across five years using difference-in-differences. They estimate little to no impact of universal mandatory ECE on educational attainment. However, their results represent a policy context with only limited generalizability to the U.S. childcare and educational landscape (e.g., long-standing public universal childcare), and do not look at labor market outcomes, as we propose to do here. Furthermore, we exploit multiple, relatively independent changes in mandatory school entrance laws across states from 1970-1992 as opposed to a single reform, allowing us to better distinguish policy effects from cohort effects.

[2] Other prominent work on kindergarten from Chetty and colleagues (2011) identify returns from different inputs in the kindergarten production function, including teacher experience and ability of classroom peers, comparing across children within the same school who were randomly assigned to kindergarten classrooms.

In summary, we aim to estimate the overall policy impact of mandating ECE to shed light on the anticipated impact of universal prekindergarten programs. We describe our data sources in section II, our empirical strategy in section III, some preliminary analyses in sections IV-VII, and discussion and next steps in section VIII.

II. Data

Our project relies on several sources of data. We describe them as follows:

State-level data

Historical kindergarten laws. Starting in the 1970s, some states began mandating kindergarten attendance (Tanner, 1973). For instance, between 1970 and 2000, 13 states and the District of Columbia switched to compulsory KG attendance.[3] We constructed a dataset at the state and year level with information on MKG adoption for the period between 1970 and 2000 from the following sources: Education Commission of the States, the Digest of Education Statistics, and each state's education department.

State-by-year school enrollment. We use state-by-year KG and first-grade public school enrollment from the Digest of Education Statistics to determine the relationship between MKG laws and enrollment as a first-stage estimate of the policy impact on KG enrollment.

State-by-year covariates. One concern is whether other state policies or factors that affect educational investments and subsequent adult outcomes changed concurrently with MKG laws. Therefore, we use state-by-year covariates such as population, Gross State Product (GSP), poverty rates, welfare use rate, legislature political party majority[4], per-pupil educational expenditures, birthdate cutoffs, and the adoption of state-subsidized KG for the period between 1979 and 2000. These data come from the following sources: Digest of Education Statistics, Common Core of Data, National Center for Education Statistics, and the University of Kentucky Center for Poverty Research. We are in the process of expanding data collection from 1960 to 1970 to improve our analysis of pre-trends.

Individual-level data

Census/ACS. We use the 2008-2017 American Community Survey (ACS) to asses our outcomes of interest. The ACS is a nationally representative, repeated cross-sectional survey that gathers annual information previously contained in the long form of the decennial census about Americans' wellbeing including educational attainment, income, and employment. Our analysis sample comprises pooled cross-sections of individuals born between 1965-1987 observed in the ACS surveys who were in kindergarten between 1970 and 1992 and are age 30 or older as survey participants. We cannot include younger cohorts as they are not old enough to observe their long-term outcomes. We will use previous censuses (i.e., 1990, 1980) to check for parallel trends.

[3] Those states are: AK, CT, DE, FL, LA, MD, NM, OK, SC, SD, TN, VA, WV and the District of Columbia (DC).
[4] Because this covariate doesn't apply to DC, we exclude DC from the regressions but results are not sensitive to its inclusion.

Our outcomes include: high school completion, college attendance, Associate's degree or college completion, earnings from wages and total income, and poverty status. Our focal cohorts were born between 1965 and 1987, allowing us to observe outcomes at ages between 30 and 50 when major educational investments are largely complete.

Assignment to KG entry year. We start by using the publicly-available ACS data, which has information on quarter and state of birth, to match individuals to their state KG mandates and other state characteristics. However, with only *quarter of birth* available in the public-use data, we do not have the level of detail necessary to precisely assign individuals to their school-entry year. Based on the modal timing and variation in state birthdate cutoffs (which also vary across years), misallocation of children to school entry cohorts will substantially affect our estimates of outcomes for children born in Quarter 3 (Jul., Aug., Sept.) and Quarter 4 (Oct., Nov., Dec.), as well as those in Quarter 1 (Jan., Feb., Mar.). Therefore, we are currently estimating models with the restricted-use Census/ACS data at UCI's RDC, which includes individual's *month of birth*.[5] These data will allow us to accurately assign individuals to KG policies in their state of birth. This approach assumes that children went to KG in their state of birth and some children could be mismatched. However, since we do not observe the migration history in the data sources of our long-term outcome variables (ACS), the place of birth is the closest in time to the place at KG compare to the place of residence. We discuss this issue further in Section IV below.

Neighborhood Change Database (NCDB) Tract Data. Available in the RDC, we will analyze heterogeneous effects of the KG mandates by poverty and socioeconomic status at birth. To do so, we will rely on NCDB data which contains information on US neighborhoods such as income, poverty status, and education at the Census-Tract level. To match these data with the Census/ACS, we will use the Census Numident dataset which has information on city/county of birth and PIKs (Protected Identify Keys) that can be linked to the 2000 decennial census and the ACS 2010-2017. We will include these estimates in a later version of the paper subject to disclosure review.

Current Population Survey (CPS) October Supplement. We use data from the 1977-1995 CPS October supplement, which collects information on school enrollment status for children ages 3-years and older in any type of school. These data allow us to examine KG enrollment responses to state mandates at the family level to provide further evidence of a first stage. Because the CPS collects rich household socioeconomic information, these data also allow us to characterize the type of families and children in the marginal group, or those who changed their enrollment behavior as a result of MKG laws (i.e., first-stage impacts at the family level). These data also allow us to check for other behavioral responses to MKG laws on maternal labor force decisions.

III. Empirical strategy

Our study aims to examine the effects of state MKG laws on long-run human capital outcomes, or in other words, the effect of an additional year of schooling at the beginning of one's school career.

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[5] In addition, access to the restricted-use data will provide us with much larger sample sizes, crucial to precisely detecting the mandate impacts.

To do this, we use a quasi-experimental approach that exploits two different sources of variation. First, there exists between state variation since not all states require KG prior to enrollment in 1st grade, and states that adopted MKG do so in different years. Therefore, children from the same birth cohort but born in different states are subject to different KG requirements. Second, we observe variation within states across time as policies change leaving some cohorts affected by MKG and others not (i.e., they are too old). Specifically, the year of birth relative to the state MKG adoption year determines who is affected by the policy change. We will estimate the following model:

$$(1)\ W_{isty} = \delta MKG_{st} + \gamma Cutoff_{st} + X_{ist}\alpha + Z_i\beta + \theta_t + \pi_s + \mu_y + \varepsilon_{isty}$$

where W_{isty} denotes the educational attainment or labor market outcome for individual *i* born in state *s* in year *t* and observed in the ACS at year *y*. MKG_{st} is a dichotomous variable equal to 1 if KG is mandatory in state *s* for a child born in year *t* and zero otherwise. The parameter of interest, δ, captures the effect of exposure to MKG. In addition, states changed their school-entry birthdate cutoff during the study time period, so we include $Cutoff_{st}$ as a covariate, which corresponds to the quarter in which the state's age/birthdate cutoff is set for a child born in state *s* and year *t*. X_{ist} represent time-varying state covariates, and Z_i reflect individual-level covariates. Birth cohort fixed effects, θ_t, capture any unobserved shock common to all children born in the same year. Similarly, state of birth fixed effects, π_s, absorb time-invariant characteristics of states, which helps to address the potential for endogeneity of MKG policy adoption. μ_y are survey-year fixed effects that account for unobserved factors common to individuals surveyed in a specific year. Estimates will be population weighted with standard errors clustered by state.

An important assumption underlying our empirical strategy is the exogeneity of state's adoption of MKG. As mentioned above, one may be concerned that other changes that would have affected children's long-term human capital outcomes occurred at the same time as states implemented MKG (Bedard & Dhuey, 2012). To address this possibility, our estimations also include a vector of state of birth characteristics measured at kindergarten entry year (X_{isb}) which include variables such as welfare use rate, poverty rate, per-pupil educational expenditures, school exit laws, and K-12 teacher-pupil ratios. We will examine the stability of our estimates to the inclusion/exclusion of these state-by-year covariates and state-specific linear trends.

Moreover, we explore the heterogeneity of the effects of MKG across demographic subgroups by performing our analysis separately for different subsamples; by race (white, non-white), and by child sex. Also, as mentioned above, we will estimate heterogeneous effects of MKG by poverty/socioeconomic status and urbanicity at birth at the city/county level using the Neighborhood Change Database (NCDB).

IV. Analysis of State-level First-Stage impacts

According to Table 1, between 1970 and 2000 14 states (including DC) have ever implemented MKG, while 37 states have not. Table 1 shows some summary statistics of socio-demographic State characteristic in 1970 by MKG status and there were some differences between states that ever adopted and never adopted mandates. States that ever adopted KG mandates had a higher proportion of black population and had a higher poverty rate in 1970.

To be sure that we are truly capturing the effect of MKG policies, an increase in KG enrollment must occur following state's adoptions. To provide evidence of a first stage, we combine the following sources of data: 1) state-by-year KG enrollment rates from 1970-2000; 2) state-by-year KG attendance mandates, which we collected based on Education Commission of the States, the Digest of Education Statistics, and each state's education department; and 3) state-by-years covariates such as population, poverty rates, per-pupil expenditures. Using these data, we tested the effects of MKG adoption on cohort-level KG enrollment from the 1970s-2000.

We then estimate a first-stage impact of mandates on state-level KG enrollment from 1970 to 2000, using the following regression:

$$(2)\ E_{st} = \alpha_0 + \beta_1 MKG_{st} + X_{st}\gamma + S_s + T_t + \varepsilon_{st}$$

where E_{st} corresponds to KG enrollment rates (as a proportion of the state population of children age 5). MKG_{st} is a dichotomous variable equal to 1 if KG is mandatory in state *s* and year *t* and zero otherwise. X_{st} is a vector of time-varying state characteristics, S_s controls for unobserved, time-invariant state characteristics, and T_t controls for unobserved nationwide year shocks. Standard errors are clustered by state. Results presented in Table 2 indicates that adoption of MKG increased KG enrollment by 11.9 percentage points (~14% of the mean). This serves as key evidence to further study the effects of state MKG requirements on later in life outcomes, which is the main goal of our study.

We also assess the degree to which variation in adoption of MKG is correlated with state characteristics. To do so, we estimate a variant of equation 2 with various state characteristics as depending variables controlling for year and state fixed effects. Results shown in Table 3 show that MKG implementation is not correlated with state characteristics. Additionally, using data from pre-MKG adoption period, column 11 in Table 3 presents some evidence that before the adoption of MKG, average KG enrollment rates were similar between states that ever adopted the mandates and states that never did, which provides evidence of common trends before the policy change.

V. Family-level First Stage Analysis

Our study results will be driven by individuals whose parents were compelled to enroll their child in kindergarten because of the state attendance mandate. Therefore, we estimate an alternate version of our state-level first-stage analysis that allows us to test for KG enrollment differences by key family characteristics. This is important for the potential heterogeneous effects we test for, which would indicate and whether KG enrollment was stronger for key subgroups, leading to potential stronger human capital effects. We use data from the October Current Population Survey (CPS) Education supplement from 1977[6]-1995, from children who were five years of age at the time of the survey, and their household heads (HH), to examine enrollment impacts at the family level. Because the October CPS contains school enrollment information for all individuals in the household, we can determine

[6] Individual state codes are not available in the CPS prior to 1977, and are only coded as state groups. Because MKG varies within these state groups, we omit individuals surveyed in the 1970-1976 CPSs.

whether each five-year-old child is or is not enrolled in kindergarten in a given year and state. Along with several other characteristics of the HH and the child, we estimate the following equation to examine the types of families that were differentially influenced to enroll in KG as a result of MKG laws:

$$E_{ist} = \alpha_0 + \beta MGK_{st} + X_i\gamma + MGK_{st} * X_i\Gamma + S_s + T_t + \varepsilon_{st}$$

where E_{ist} represents the KG enrollment status of child i, in state s and observed in year t of the CPS, MGK_{st} is whether KG is mandatory for that 5 year old in year t, X_i is a vector of characteristics about the child and HH that includes race, gender, marital status and educational attainment, and Γ is a vector of interaction terms between MKG and all covariates in X. These interaction terms are the coefficients of interest; significant interactions between MKG and family characteristics would suggest that these individuals were more likely to enroll their child in kindergarten as a result of the policy. S_s and T_t are state and year fixed effects.

Coefficients on all the MKG*X interaction coefficients are shown in Table 4. Although few terms reach statistical significance in this fully saturated model, the interactions between MKG and Hispanic and with the HH having a high school degree or less are both marginally significant and positive. This indicates that Hispanic families were more likely to send their children to KG relative to white families when subjected to MKG, corresponding with the strong positive impacts we found for Hispanic children on long-run human capital outcomes. The interaction between MKG and HH with a high school degree or less indicates that families with relatively low educational attainment were more likely to comply with KG mandates. We will further explore heterogeneous impacts of MKG by community SES and rural-urban status when we incorporate the NCDB data in our restricted-use Census analyses.

VI. Relationship between MKG and long-term human capital: "Quick and dirty" estimates from public-use data

Main Effects

We use the publicly-available ACS data to preliminarily explore the effects of MKG on long-run educational outcomes and economic self-sufficiency. As mentioned, with the public data we cannot precisely assign individuals to KG entry year based on state birthdate cutoffs since we only have information on quarter of birth (instead of month), which will also affect assignment to MKG policies.

We estimate a version of equation 1 for the following outcomes: high school completion or more, some college or more, Associate's or BA degree or more, poverty status (below 100% of the poverty line), and the log of wage income and total income. We find that exposure to MKG did not have a significant impact of any of the outcomes in our main effect analyses (Table 5).

Heterogeneity Analysis

Race. Analyses by race and ethnicity reveal important differential effects of MKG on human capital outcomes. These analyses involve interactions between MKG and non-white (Black and Hispanic) respondents. Shown in Table 6, our preliminary effect size for Black and Hispanic children's increase in college degree completion is 5 percentage points (11% of the mean) relative to white children exposed to MKG. Impacts on wage and total income are similarly strong; our exponentiated coefficients correspond to a 6.7 percent increase in wage and total income for non-white children exposed to MKG. Results for subsample analyses are noisier from the loss of power but the coefficient magnitudes are consistent with those shown here (see Appendix Table 1).

Sex. Impact analyses by sex also indicate a consistent differential benefit of MKG for women. Preliminary results in Table 7 show 1-2 percentage point increases in high school and college degree completion and some college attendance for women, and a 5.8 percent increase in women's wage and total income. Results for subsample analyses are again noisier from the loss of power (see Appendix Table 2).

Early childhood neighborhood conditions. [TBD]

VII. Robustness

Mobility concerns

As mentioned above, our empirical strategy assumes that children went to KG in their state of birth. Therefore, some children will be mismatched. To understand the magnitude of this mismatch, we use children age 5-6 in the 1980 and 1990 Census (thus born in 1975 and 1985 and part of our cohorts of interest) and quantify the fraction of children living in a different state than the one at birth. Also, to access whether selective migration is a potential threat to the internal validity of our study, we test whether mobility between birth and age 5-6 is correlated or not with the adoption of KG mandates.[7] In particular, we regress mobility between state-of-birth and state-of-residence for children age 5-6 in 1980 and 1990 as a function of MKG laws and all other covariates used in this paper.[8] This analysis, shown in Appendix Table 3, suggests that the likelihood of migration across states is not correlated with adoption of mandatory KG policies.

Other studies have use state of birth as proxy of state of residence during childhood in analyses of similar policies and long-term outcomes and have found that mobility is not a main threat to causality (e.g., Card & Krueger, 1992; Lleras-Muney, 2005). These studies have found that the likelihood of migrating between the state of birth and the state at schooling age is not correlated with exposure to their education policies of interest, which means that mobility is not likely to bias their effects.

Maternal labor market responses

Mandatory KG presents an opportunity not only to the child who is exposed to a year of early schooling, but also to the parents, particularly mothers, who may choose to change their labor supply or human capital investments in response to the policy (Cascio, 2009; Fitzpatrick, 2010). To test for this possibility, we constructed a sample of the mothers of five-year-old children from the October CPS (as opposed to information on the household heads of five-year-olds, as we do in the family-level first stage analysis), and regress employment status and number of hours worked on MKG, including state and

[7] Data from the 1990 census suggests that around 15% of children age 5 or 6 do not live in the same state as the state of birth..

[8] A similar analysis will be performed for the likelihood of moving across counties.

year fixed effects and maternal characteristics. We find that MKG did not influence the maternal labor supply at the extensive margin, but did so at the intensive margin. The estimates in the top panel of Appendix Table 4 indicate that the mothers of five-year-olds did not join the labor force as a result of KG mandates, but did increase the number of hours worked. This trend appears to be driven primarily by unmarried mothers (bottom panel).

VIII. Discussion and next steps

Investments during early childhood have been shown to be among the most productive social policy investments with substantial returns for both the individual and society (Heckman & Masterov, 2007). However, most of the evidence comes from small-scale interventions. In this paper, we examine the effects of mandating an extra year of early schooling (kindergarten) on long-term human capital outcomes. We exploit a previously unused source of variation: the introduction of mandatory KG attendance laws between 1970 and 1995. While this intervention is universal, it can be expected that this reform is more likely to affect children from disadvantaged families. Indeed, we find nontrivial effects of KG mandates on education and labor market outcomes of children from Black and Hispanic families. In addition, when exploring differential impacts by gender, we find larger effects for females than males. This evidence of heterogeneity impacts is similar to previous evidence about long-term effects of Head Start (Deming, 2009) and KG expansion grants (Dhuey, 2011). Overall, our findings highlight that early childhood educational investments pay off in the long run, and can be equity enhancing in the presence of large education and earnings gaps between non-white and white adults (Mora & Dávila, 2018) and between men and women (Blau & Kahn, 2017; Goldin, Katz, & Kuziemko, 2006).

As next steps, to correctly assign exposure to KG mandates based on exact date of birth. we have begun replicating these analyses with the Census RDC restricted-use data. We are in the process of linking the restricted use Census and ACS data to the Numident, which allows us to incorporate respondents' county of birth in our heterogeneity analyses with the NCDB. This will allow us to examine whether the impacts of MKG were differentially beneficial to individuals living in high-poverty neighborhoods, areas with lower educational attainment, or in more rural or urban communities. We will also run an alternative specification of our analyses as a two-sample instrumental variable model, examining state-level enrollment changes in the first stage, and using those projected MKG coefficients to scale the second stage results on long-run human capital outcomes. To understand potential mechanisms, we will look at the impacts on intermediate outcomes related to skill accumulation such as NAEP test scores. In addition, to validate our findings, we will perform falsification tests by looking at the effects on KG mandates on individuals too old to be subjected to the policy change, and an event study to better examine the pre-trends of states who did and did not adopt MKG. Our results will be timely and relevant for federal- and state-level policymakers as they contemplate expanding ECE programs towards universal provision.

Table 1: Summary Statistics

	Never mandatory KG			Ever mandatory KG			
	Mean	SD	Obs	Mean	SD	Obs	Mean Diff
State covariates in 1970							
Gross State Product per capita	0.00	0.00	38	0.00	0.00	13	0.000
Poverty Rate	4.92	1.42	38	4.74	1.15	13	-0.185
AFDC recipients/pop	13.50	5.44	38	18.69	6.37	13	5.192**
SNAP recipients/pop	0.01	0.00	38	0.01	0.00	13	0.001
K-12 Expenditures per pupil	4,920.55	1,134.97	38	4,545.04	1,035.31	13	-375.507
K-12 Pupil-teacher ratio	22.11	1.94	38	22.31	1.75	13	0.193
% White	0.89	0.13	38	0.80	0.18	13	-0.091^{+}
% Black	0.07	0.09	38	0.18	0.19	13	0.108*
% Other race ratio	0.03	0.11	38	0.02	0.02	13	-0.017
% of state house that is Democrat	0.50	0.25	38	0.69	0.25	12	0.191*
State-year observations 1970-2000							
Kindergarten enrollment %	0.83	0.19	1,328	0.86	0.15	242	0.034**
Primary school enrollment %	0.95	0.08	1,339	0.99	0.08	242	0.033**

note: ** p<0.01, * p<0.05, $^{+}$ p<0.1

Table 2: Relationship between KG enrollment and State KG compulsory requirements

	Kindergarten Enrollment Rate		
	(1)	(2)	(3)
	State and Year FE	State Covariates	1+2
State has a mandatory kindergarten	0.101	0.060**	0.119**
	(0.065)	(0.030)	(0.047)
Observations	1,501	1,501	1,501
R-squared	0.502	0.183	0.627

Robust standard errors in parentheses clustered at the state level. State and survey-year fixed effects included. State time-varying covariates included: GSP per capita, unemployment rate, proportion state AFDC recipients, SNAP benefits per capita, K-12 pupil-teacher ratio, proportion state black, proportion state other race, proportion of state house that is democratic, and fixed effects for school entry birthdate cutoff month. Data are from 1970-2000. KG Enrollment rate = Statewide KG enrollment / State total # children age 5

** $p<0.01$, * $p<0.05$, + $p<0.1$

Table 3: Relationship between MKG Adoption and State Characteristics

	(1) GSP per capita	(2) Unemployment rate	(3) % State AFDC & TANF recipients	(4) SNAP Benefits per capita^	(5) K-12 exp. per-pupil (ln)	(6) % State House Democrat
State has MKG Policy	0.001 (0.002)	-0.316 (0.436)	-0.001 (0.004)	0.005 (0.005)	-0.001 (0.028)	-0.020 (0.035)

	(7) Pupil-teacher ratio	(8) % State White	(9) % State Black	(10) % State Other race	(11) Pre-MKG adoption K enroll rate
State has MKG Policy	-0.503 (0.351)	-0.005 (0.009)	0.008 (0.007)	-0.002 (0.003)	0.020 (0.048)

^Total SNAP recipients by state not available before 1980. ** $p<0.01$, * $p<0.05$, + $p<0.1$

Table 4: Family-level First Stage analysis with the Current Population Survey, 1977-1995

	KG enrollment
MKG	-0.025
	(0.043)
MKG*Poverty & near poor	-0.006
	(0.012)
MKG*Male	0.007
	(0.009)
MKG*Black	-0.019
	(0.018)
MKG*Hispanic	0.047+
	(0.024)
MKG*Other	0.077
	(0.056)
MKG*HH male	0.012
	(0.019)
MKG*Married	0.001
	(0.017)
MKG*Employed	0.013
	(0.033)
MKG*Not in labor force	0.043
	(0.027)
MKG*HH HS or less	0.027+
	(0.016)
MKG*HH Some College	0.014
	(0.014)
N	39212

Notes: Main effects for MKG and all covariates not shown. Sample includes all household heads (HH) with five year old children at the time of the October survey. State and survey-year fixed effects included. State time-varying covariates included: GSP per capita, unemployment rate, proportion state AFDC recipients, SNAP benefits per capita, K-12 pupil-teacher ratio, proportion state black, proportion state other race, and proportion of state house that is democratic. ** p<0.01, * p<0.05, + p<0.1

Table 5: Preliminary Results of the Effect of MKG on Long-run Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	High School	Some College	Assoc. or BA degree	Poverty^	Wage Income (ln)	Total Income (ln)
Exposed to MKG Policy	0.0025	-0.0023	-0.0022	0.0050	-0.0149	-0.00110
	(0.0036)	(0.0068)	(0.0088)	(0.0053)	(0.0124)	(0.00770)
Y mean	0.924	0.669	0.441	0.123	10.51	10.41
Observations	5,561,620	5,561,620	5,561,620	5,210,751	4,085,327	4,684,834

Notes: Individual-level covariates and state and survey-year fixed effects included. State time-varying covariates included: GSP per capita, unemployment rate, proportion state AFDC recipients, SNAP benefits per capita, K-12 pupil-teacher ratio, proportion state black, proportion state other race, proportion of state house that is democratic, and fixed effects for school entry birthdate cutoff quarter. ^Income below 100% federal poverty level.

Table 6: Preliminary Results of the Heterogeneity of Effects of MKG on Long-run Outcomes by Race/Ethnicity

	(1)	(2)	(3)	(4)	(5)	(6)
	High School	Some College	Assoc. or BA degree	Poverty^	Wage Income (ln)	Total Income (ln)
Exposed to MKG Policy	-0.0052	-0.0125	-0.0192	0.0069	-0.0259	-0.0182
	(0.0047)	(0.0080)	(0.0119)	(0.0065)	(0.0175)	(0.0128)
Non-white	-0.067**	-0.141**	-0.184**	0.112**	-0.292**	-0.327**
	(0.0051)	(0.0079)	(0.0073)	(0.0106)	(0.0132)	(0.0183)
MKG* Non-white	0.022**	0.0294**	0.0515**	-0.0068	0.0444	0.0647*
	(0.0058)	(0.0102)	(0.0130)	(0.0113)	(0.0280)	(0.0263)
Y mean	0.924	0.668	0.439	0.122	10.51	10.40
Observations	5,408,977	5,408,977	5,408,977	5,069,552	3,978,710	4,560,865

Notes: Non-White is defined as either African American or Hispanic. White is the reference category. Individual-level covariates included. State and survey-year fixed effects included. State time-varying covariates included: GSP per capita, unemployment rate, proportion state AFDC recipients, SNAP benefits per capita, K-12 pupil-teacher ratio, proportion state black, proportion state other race, proportion of state house that is democratic, and fixed effects for school entry birthdate cutoff quarter. ^Income below 100% federal poverty level. ** p<0.01, * p<0.05, + p<0.1

Table 7: Preliminary Results of the Heterogeneity of Effects of MKG on Long-run Outcomes by Sex

	(1)	(2)	(3)	(4)	(5)	(6)
	High School	Some College	Assoc. or BA degree	Poverty^	Wage Income (ln)	Total Income (ln)
Exposed to MKG Policy	-0.002	-0.013+	-0.008	0.005	-0.039**	-0.028*
	(0.004)	(0.007)	(0.009)	(0.006)	(0.014)	(0.011)
Female	0.025**	0.095**	0.091**	0.012**	-0.40**	-0.42**
	(0.0014)	(0.0025)	(0.003)	(0.001)	(0.009)	(0.010)
MKG*Female	0.010**	0.021**	0.012*	-0.000	0.052*	0.056**
	(0.004)	(0.004)	(0.005)	(0.003)	(0.021)	(0.020)
Y mean	0.924	0.669	0.441	0.123	10.51	10.41
Observations	5,561,620	5,561,620	5,561,620	5,210,751	4,085,327	4,684,834

Notes: Individual-level covariates and state and survey-year fixed effects included. State time-varying covariates included: GSP per capita, unemployment rate, proportion state AFDC recipients, SNAP benefits per capita, K-12 pupil-teacher ratio, proportion state black, proportion state other race, proportion of state house that is democratic, and fixed effects for school entry birthdate cutoff quarter. ^Income below 100% federal poverty level. ** p<0.01, * p<0.05, + p<0.1

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Appendix

Appendix Table 1. Heterogeneity analyses by Race/Ethnicity Subsample Results

Black/Hispanic

	(1)	(2)	(3)	(4)	(5)	(6)
	High School	Some College	Assoc. or BA degree	Poverty^	Wage Income (ln)	Total Income (ln)
Exposed to MKG Policy	0.0053	0.0086	0.0072	0.016+	0.018	0.030
	(0.0054)	(0.013)	(0.011)	(0.0093)	(0.018)	(0.019)
Y mean	0.8	0.57	0.30	0.21	10.27	10.14
Observations	1,078,111	1,078,111	1,078,111	984,014	717,917	847,148

White

	(1)	(2)	(3)	(4)	(5)	(6)
	High School	Some College	Assoc. or BA degree	Poverty^	Wage Income (ln)	Total Income (ln)
Exposed to MKG Policy	0.0017	-0.0052	-0.0035	-0.0027	-0.018	-0.0065
	(0.0036)	(0.0055)	(0.0080)	(0.0034)	(0.016)	(0.0089)
Y mean	0.94	0.70	0.48	0.096	10.58	10.48
Observations	4,330,866	4,330,866	4,330,866	4,085,538	3,260,793	3,713,717

Notes: Individual-level covariates and state and survey-year fixed effects included. State time-varying covariates included: GSP per capita, unemployment rate, proportion state AFDC recipients, SNAP benefits per capita, K-12 pupil-teacher ratio, proportion state black, proportion state other race, proportion of state house that is democratic, and fixed effects for school entry birthdate cutoff quarter. ^ Income below 100% federal poverty level. ** $p<0.01$, * $p<0.05$, + $p<0.1$

Appendix Table 2. Heterogeneity analyses by Sex Subsample Results

Female

	(1)	(2)	(3)	(4)	(5)	(6)
	High School	Some College	Assoc. or BA degree	Poverty^	Wage Income (ln)	Total Income (ln)
Exposed to MKG Policy	0.003	-0.002	-0.004	0.007	-0.025	-0.014
	(0.004)	(0.006)	(0.009)	(0.005)	(0.024)	(0.016)
Y mean	0.936	0.716	0.486	0.130	10.30	10.18
Observations	2,802,674	2,802,674	2,802,674	2,594,140	1,952,923	2,259,229

Male

	(1)	(2)	(3)	(4)	(5)	(6)
	High School	Some College	Assoc. or BA degree	Poverty^	Wage Income (ln)	Total Income (ln)
Exposed to MKG Policy	0.002	-0.003	-0.000	0.003	-0.005	0.010
	(0.004)	(0.008)	(0.001)	(0.006)	(0.009)	(0.00)
Y mean	0.911	0.622	0.397	0.116	10.70	10.61
Observations	2,758,946	2,758,946	2,758,946	2,616,611	2,132,404	2,425,605

Notes: Individual-level covariates and state and survey-year fixed effects included. State time-varying covariates included: GSP per capita, unemployment rate, proportion state AFDC recipients, SNAP benefits per capita, K-12 pupil-teacher ratio, proportion state black, proportion state other race, proportion of state house that is democratic, and fixed effects for school entry birthdate cutoff quarter. ^ Income below 100% federal poverty level. ** $p<0.01$, * $p<0.05$, + $p<0.1$

Appendix Table 3. MKG and likelihood of moving between birth and age five

	(1)
	Mover
Mandatory kindergarten	-0.002
	(0.01)
Observations	658870

Notes: Sample includes 5 and 6 year-old children in the 1980 and 1990 Census. Standard errors clustered at the place of birth. ** p<0.01, * p<0.05, + p<0.1

Appendix Table 4. Maternal labor force responses to MKG

	(1) Employment		(2) Num. hours worked^	
Exposed to MKG Policy	0.010		1.528**	
	(0.034)		(0.408)	
N	38625		22067	
	Married	*Unmarried*	*Married*	*Unmarried*
Exposed to MKG Policy	0.014	0.021	0.831	1.603
	(0.030)	(0.044)	(0.855)	(1.059)
N	31981	6644	19315	2752

Notes: Sample is mothers of 5-year-old children in the CPS October Supplement. Top panel includes all mothers, and bottom panel disaggregates by marital status. ^Num hours worked only calculated for 1977-1986 CPS. ** p<0.01, * p<0.05, + p<0.1