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Abstract[1]

This paper examines whether additional time in elementary and secondary school affects economic well-being in adulthood. This paper explores a large-scale reform that increased the Chilean school day by 30 percent between 1997 and 2010, with access to longer school days varying by cohort and city. Both sources of variation are leveraged and it is found that full-day schooling increases educational attainment, delays childbearing, and increases earnings in young adulthood. The nature of these benefits is consistent with more time in school facilitating human capital accumulation, and the results show that large-scale investments in public education can generate long-term improvements in economic well-being.

JEL classification: I26; I25; J24; H52

Keywords: Economics of education, returns to education, full-day schooling, long-term investments

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1 Introduction

This paper examines whether additional time in school translates into improved economic well-being in adulthood. Although policymakers frequently advocate lengthening the school day in order to promote economic growth and competitiveness, the relationship between the length of the school day and adult outcomes has not been fully explored.[2] Despite the lack of empirical evidence on this relationship, multiple Latin American and European countries have taken drastic steps to lengthen the school day over the past thirty years. In the United States, the share of kindergarteners attending full-day school increased from less than 20 percent in 1970 to 75 percent by 2012 (Gibbs, 2014). More broadly, until the 1990s, the typical student attended school for approximately four hours in many middle-income and developing countries. While some of these countries have moved towards a 6-7 hour school day, many Latin American countries continue to operate under the half-day model (UNESCO TERCE, 2016).

This paper examines one of the first and largest full-day schooling reforms, Jornada Escolar Completa (JEC), which increased the school day for Chilean elementary and secondary school students in all publicly-funded schools by approximately 30 percent between 1997 and 2010. Due to budgetary and logistical constraints, the timing of the introduction and expansion of full-day schooling varied across both cities and birth cohorts. We leverage both sources of variation to examine the effect of additional time in school on labor market outcomes in adulthood. Specifically, we estimate the expected number of years a student would be expected to attend a full-day school using data from the Chilean Ministry of Education and match this administrative data to nationally-representative labor market data based on survey respondents' reported year and place of birth.

This approach makes an important methodological contribution to the existing literature on full-day schooling. Most existing work examines outcomes for one birth cohort or for a single jurisdiction. In contrast, our approach allows us to identify the causal effect of

[2]See, for example, President Obama's March 10, 2009 speech to the Hispanic Chamber of Commerce. `http://www.nytimes.com/2009/03/10/us/politics/10text-obama.html`

more time in school under relatively weak identifying assumptions regarding the timing and acceleration of the reform. Importantly, our sample cohorts were born before the reform was announced and our treatment variable is defined based on place of birth, rather than the city in which individuals actually attend school. Therefore, our measure of access is not affected by families choosing to move to areas that implemented the reform relatively early, attending a school outside their city of residence, or selecting a school within their municipality based on full-day access. We further account for potential non-random selection into full-day schools by limiting our comparisons to birth cohorts within a geographical region and controlling for local factors that may affect both reform implementation and long-term outcomes.

Our findings are threefold. First, we find that a longer school day increases educational attainment, earnings, and the likelihood of working in a skilled occupation in adulthood. The implied earnings gains suggest a 16 percent rate of return to an additional year-equivalent of schooling, in line with existing estimates on the returns to education during the 1990s and early 2000s in Chile (OECD, 2013; Manacorda et al., 2010).

Second, we extend the analysis of Berthelon and Kruger (2011) and find access to a longer school day delays childbearing among women. In interpreting these results, we note that women in our sample are in their prime childbearing years, and we would expect the full range of intergenerational benefits to arise in the coming decades.

Third, while access to longer school days increases educational attainment for all types of students, students from higher socioeconomic groups who have greater access to full-day schools work in managerial, professional, and technical occupations, while students from disadvantaged backgrounds are more likely to enter the workforce. These patterns suggest that, given students from different family backgrounds have heterogeneous labor market adaptations, disadvantaged students may face additional constraints in gaining access to skilled occupations.

Our estimates measure exposure to full-day schools, or the intent to treat (ITT) of having access to additional schooling. From a national government's perspective, this is a

key parameter to determine the effect of offering additional time in school. In our setting, the ITT is equivalent to the treatment-on-the-treated (TOT) for students who attend a publicly-funded school in the municipality in which they are born. During the time our sample attended school, approximately 90 percent of students attended a publicly-funded school, and 70 percent resided in the same municipality in which they were born.[3] Among those who moved, access to full-day schooling was similar in the municipality of residence as in the city of birth, with families tending to move to cities with similar access to JEC. These findings suggest the difference between the ITT and TOT is likely small for most populations in the aggregate.

This work builds upon an existing literature looking at the effectiveness of additional time in school on student outcomes. Most of the previous research examines the short-term effects of longer school days by focusing on outcomes of current students, either during large-scale reforms affecting all elementary and secondary school students, or from reforms that targeted a particular age group of students, such as the expansion of full-day kindergarten in the United States. The findings on how large-scale reforms affect academic outcomes are mixed, with some studies finding no significant effect and others finding modest positive effects on students' test scores (Bellei, 2009; Valenzuela, 2005; García, 2006; Dias Mendes, 2011; Llambí, 2013; Orkin, 2013).

Extended school days may affect outcomes other than academic performance. For young children, longer school days provide a form of subsidized childcare and may therefore increase parental employment and family income, particularly for families with young students (Berthelon et al., 2015; Contreras et al., 2010; Gibbs, 2014; Gelbach, 2002). For younger students, there is evidence of medium-term benefits, as students accessing longer days in kindergarten have higher educational attainment and lower involvement with the criminal justice system (Cascio, 2009). Older students also appear to benefit, but from a more direct "incapacitation" mechanism. For teenagers, additional time in school may reduce risky behaviors that occur outside of school, such as teen pregnancy and involvement with the

[3]For this time period, we do not have information on the municipality students attended school, which could differ from the city of residence.

criminal justice system (Berthelon and Kruger, 2011; Contreras et al., 2010). This paper contributes to previous work by evaluating the *long-run* effects of additional school time once students have completed schooling and entered the labor market.

As we measure the long-term effects of a policy that changed students' time use patterns and family resources, there are several potential mechanisms for our findings. Like all evaluations of full-day schooling reforms, we are unable to conclusively determine whether these findings are due to increased human capital attainment through additional instruction time, access to newer school facilities, changing students' time use patterns, or a combination of factors, as infrastructure changes in the education environment coincide with the expansion of the school day. Second, it is important to note that children who gain access to longer school days in early grades are also more likely to have access in later grades. We are therefore unable to fully distinguish the extent to which these findings are driven by dynamic complementarities – in which additional learning at young ages facilitates future knowledge accumulation – or simply additional schooling at any point during one's academic career. Despite these limitations, we can rule out some other possible channels for earnings gains. In particular, we do not find longer school days change migration patterns across municipalities within Chile. In addition, most of our main effects are somewhat smaller for individuals born in areas experiencing the largest increases in maternal labor supply over the reform period, inconsistent with a scenario where schooling benefits students *exclusively* through a family income subsidy.

This paper proceeds as follows. Section 2 discusses the existing literature on school day length and well-being. Section 3 describes the Chilean reform. Section 4 outlines the empirical approach and data. Section 5 presents results, and Section 6 concludes.

2 School Day Length and Student Outcomes

The time students spend in school, measured by either hours or days, depends on both individual and area characteristics that are correlated with student outcomes (Patall et al.,

2010). In addition, term length is typically strongly correlated with other quality dimensions, making it difficult to separate different aspects of educational quality (Card and Krueger, 1992; Ganimian and Murnane, 2016). Both of these points caution drawing causal conclusions from simple correlations between student outcomes and time spent in school. Perhaps unsurprisingly, early cross-sectional analyses find little association between the length of the school year and earnings in adulthood (Card and Krueger, 1992; Heckman et al., 1995).

2.1 Time in School and Academic Achievement

A large literature examines the nature of short-term benefits from more time in school. This work focuses on current students and generally finds that longer school days improve academic performance. If improved achievement translates into greater educational attainment or human capital accumulation, these studies suggest longer school days may also improve long-term outcomes as students enter the labor force.

Several recent papers focusing on developed countries use exogenous variation in the length of the school day or year and find that additional time in school improves academic performance in the short run. Using variation in the length of the school year stemming from snow days, Goodman (2014) finds that shorter school years due to building closures do not affect performance, but individual absences due to bad weather worsen math performance. Gibbs (2014) examines the effect of full-day classes for young students and finds full-day kindergarten improves test scores 0.3 standard deviations, with particularly large gains for Hispanic students. Pischke (2007) examines a reconfiguration of the West German academic calendar when several states implemented multiple short school years in order to align with federal requirements and finds that shorter school years worsened academic performance and increased grade repetition, but had no long-term effect on employment or earnings when students were in their 20s and 30s. Finally, within-student variation in subject-specific instructional time can measure subject-specific returns to greater educational attention. Using cross-country PISA score data, Lavy (2015) finds an additional hour of instructional time in a given subject improves scores in that subject by 0.07-0.15 standard deviations, with a

weaker relationship for students in lower-income countries.

A related literature examines the short-term effects of larger-scale reforms that gradually transition *all* elementary and secondary students from a part-day to full-day schedule. Since the 1990s, a number of Latin American countries and cities have expanded the school day from approximately 4 to 6-7 hours. Chile was one of the first countries to undertake such an expansion; therefore, much of the existing literature examines the Chilean reform.

The results from these reforms are mixed across countries, but generally point towards improved school performance (Glewwe et al., 2013). While some studies find worsened test scores following reforms targeted to disadvantaged or low-performing schools in Brazil (Dias Mendes, 2011) and Uruguay (Llambí, 2013), other studies find improvements in Colombia (Hincapie, 2016; Bonilla, 2011) and Buenos Aires (Llach et al., 2009). In comparing the magnitudes across different types of reforms, it is important to note that larger-scale full-day reforms are more likely to change other school resources – such as requiring new facilities, offering mid-day school meals, and providing additional recess time. Since these changes to the school day and educational system occur concurrently, examinations of large-scale full-day reforms– including the current study–cannot separate the effect of greater time in school from other concurrent changes to the learning environment. As summarized by Ganimian and Murnane (2016), this literature finds that resources need to change students' experiences – by improving the quality or quantity of instructional time, or improving nutritional intake, rather than offering "more of the same" – in order to affect achievement.

The Chilean reform is an example of a reform that extended the school day and allocated most of the additional time to greater instruction and increased teaching resources (DESUC, 2005; Barrios and Bovini, 2019).[4] Previous work has documented that the reform improved academic performance by 0.05-0.20 standard deviations (Bellei, 2009; Valenzuela, 2005; García, 2006), and accounting for non-random selection into full-day schools yields larger estimated improvements than the unadjusted results (Berthelon et al., 2016).

[4]Comprehensive information on school resources, including funding and other staff metrics is not available for the full implementation period, limiting our ability to determine the extent to which our findings are driven by increased financial or staffing resources.

Given the findings of the previous literature, we take the possibility of non-random selection into full-day schools seriously and estimate an intent-to-treat effect of having access to full-day schools. Our "access" measure is determined by factors that pre-date the reform announcement – namely city of birth – and is not affected by subsequent migration patterns. This approach is similar in spirit to Berthelon et al. (2016) who instrument full-day school enrollment using municipality rates of full-day school exposure the year before students' outcomes are measured. The main differences are that our approach aggregates access to full-day schooling over an individual's entire academic career in order to provide a measure of exposure over an individual's full career and as our data lack information on which schools a student actually attends, our approach is a reduced-form strategy based on a student's city of birth that does not allow us to precisely estimate the magnitude of endogenous mobility.

2.2 Time in School and Outcomes in Adolescence and Early Adulthood

Improvements in short-term academic performance are neither a necessary nor sufficient condition for additional school time to benefit students into adulthood. If test score gains fade over time, or if skills that are measured by standardized tests are not correlated with labor market productivity, short-term improvements may not lead to labor market advantages. As noted in Murnane et al. (2000), there is no empirical consensus on this point; some studies find a negligible relationship between short-term academic improvements and longer-term labor market outcomes in the context of full-day school reforms (Pischke, 2007; Pires and Urzua, 2015), while others find general improvements test scores are associated with higher earnings in adulthood (Rose, 2006; Chetty et al., 2011b; Hansen et al., 2004; Murnane et al., 1995). More broadly, there is a growing body of literature documenting that educational investments that exhibit short-term gains that fade out over the medium term, such as classroom sizes and early childhood education, can still generate meaningful improvements in long-term economic outcomes (Chetty et al., 2011a; Garces et al., 2002).

Even if full-day schooling does not affect academic performance, there are several factors

that may shape students' economic opportunities by changing families' and students' time use patterns. Extended school days provide families with an implicit childcare subsidy and reduce children's leisure time Contreras et al. (2010) and Berthelon et al. (2016) find that Chilean full-day schools increased female labor participation and employment Berthelon and Kruger (2011) analyze the extent to which more time in school "incapacitates" high school students from engaging with the criminal justice system or becoming a teenage mother. They find greater access to full-day schools lowers adolescent crime and reduces teenage pregnancy rates for lower-income girls in urban areas. As teen parenthood and a criminal history are associated with lower earnings later in life and higher maternal employment potentially increases family resources, all of these findings suggest channels by which additional time in school may lead to longer-term benefits. In the US context, Cascio (2009) finds that the introduction of public kindergarten increased high school graduation and reduced involvement with the criminal justice system.

The existing work on how extended time in school affects labor market outcomes is relatively limited, as in many contexts students who attended school for a full-day are still completing their schooling or only recently entered the labor market. As Chile was one of the first countries to adopt a full-day schooling reform for elementary and secondary students, much of the work on early labor market outcomes focuses on the JEC reform. Closely related to this paper, Pires and Urzua (2015) examine the medium-term effects of the Chilean reform by comparing students who attended full-day school starting at ages 14-15 (and were surveyed at ages 25-26 years old) to older cohorts who completed school prior to reform (29-30 years old). They find while attending full-day school improved academic performance, it only increased monthly wages among students who had previously attended the afternoon shift. There are several limitations of Pires and Urzua (2015) which the current study aims to address more fully. First, their treatment cohorts attended full-day schools for up to 3-4 years, less than half of the full treatment. In contrast, our sample covers the full implementation and provides labor force information for students with access to up to the entire 12 years of full-day schooling. Second, although insufficient time has elapsed

to investigate the full earnings-age profile, we are able to extend the analysis a decade and examine employment outcomes into treated students' late 30s. A greater age range is particularly important since longer school days increase secondary or tertiary educational attainment and delay labor market entry. Finally, Pires and Urzua (2015) leverage variation in the schools that students actually attended and control for observable factors that may affect school choice. We take a complementary approach, by using both cross-city and cross-cohort variation that is exogenous to the choice set students face.

3 Full-Day School Reform: Jornada Escolar Completa (JEC)

Until the late 1990s, Chilean elementary and secondary students attended school 4-5 hours a day. Under this model, many schools operated a two-shift system where some students attended school in the morning (8am to 1pm), and others attended in the same building during the afternoon (2pm to 7pm). Beginning in 1997, Chile implemented Jornada Escolar Completa (JEC), a large-scale reform that increased the school day in publicly-funded schools by an average of 1.4 hours, while keeping the total number of school days fixed.[5]

JEC gradually moved all schools to a single, full-day shift, with all students attending in the morning through mid-afternoon (8am to 3pm). This reform represents a substantial increase in schooling time: on average, instructional time increased 30 percent and the total length of the school day increased 22 percent (Berthelon and Kruger, 2011). This additional time could be used for either instructional or extra-curricular activities; the stated goal was to improve school quality (Alfaro and Holland, 2012). With the available data, we are unable to observe school-level changes in instructional time and therefore cannot speak to

[5]About 93 percent of students in the K-12 system enroll in publicly-funded schools. These schools include public schools that are managed by local municipalities and private-voucher schools that are managed by private entities but subject to central government legislation. Originally, schools were mandated to expand the school day in grades 3 to 12 by 2007, but in practice most implemented the reform in grades 1 and 2 as well. As young students also had access to longer days, we measure JEC exposure across the full 1-12 grade range. See law description at: `https://www.leychile.cl/Navegar?idNorma=76753`.

the relative productivity of additional instruction, versus extra lunch or recreation time. In the aggregate, however, most teachers, parents, and students reported that at least some of the additional time was used for language and math instruction, while only 2 percent of respondents dedicating additional time to study for standardized tests (DESUC, 2005).

While schools could choose when to begin offering an extended school day, the reform required a substantial infrastructure investment in many areas, as building and staffing resource needs nearly doubled in areas previously operating a double shift. Due to these practical considerations, schools operating under capacity were the first schools to adopt JEC (Bellei, 2009). For schools without excess capacity, the Ministry of Education prioritized funding schools in disadvantaged areas and partially offset operational costs with a 20-50 percent increase in central government funding.[6] The legislation required that all schools receiving public funding operate a full-day schedule by 2007 (public schools) or 2010 (publicly-funded voucher schools), leading to a 14-year rollout period.[7]

Not all grades within a school were required to implement JEC at the same time. The school day was lengthened at the beginning of the academic year. The youngest students typically gained access relatively early in the reform and continued receiving full-day schooling as they progressed through school. Accordingly, JEC led to variation across cohorts and municipalities in access to full-day schools. Whereas 20 percent of students attended a full-day school in 1997, this share had steeply increased to more than 80 percent in 2015. Since JEC was a grade-school specific change that was generally first introduced for younger students, the vast majority – over 90 percent – of students experience increased access over their educational career. Based on the level of exposure at the municipality level, defined

[6]In practice, JEC funds were allocated based on low switch costs and high pre-existing deficits in infrastructure. These schools tended to be relatively small and more rural (Berthelon and Kruger, 2011; Contreras et al., 2010). The exact increase in per-student revenue was school-grade-specific and depended on the grade served and other school characteristics.

[7]The reform covers public schools that are locally managed and fully funded by the central government and private subsidized voucher schools that are privately managed by receive government funds. According to administrative data from the Ministry of Education, approximately 36 percent of students attended a public school and 55 percent attended a voucher school in 2016. Private non-voucher schools were not covered by JEC and the share of students attending a private non-voucher school is small (about 6 percent) and remained relatively unchanged over the period when our sample was in school. In practice, the implementation period was delayed and by 2010, only about 75 percent of schools had a full-day schedule.

below, we observe that only 8 percent of students experience a reduction in access when moving from one grade to the next, and these changes typically occurred at the transition between primary and secondary school. Accordingly, like other work on full-day reforms, we are unable to disentangle dynamic complementarities, by which additional schooling at particular grades has especially pronounced effects, from treatment "dosage" years of full-day schooling.

We calculate exposure to JEC, ($\widehat{JEC_{cm}}$) as the expected number of years an individual born in cohort c in municipality m would attend a full-day school in grades 1 through 12 using administrative enrollment and JEC participation data from the Ministry of Education on total enrollment N for each grade g in school s serving grade g in municipality m:

$$(\widehat{JEC_{cm}}) = \frac{1}{N_{cm}} \sum_{s \in m} \sum_{g=1}^{12} \mathbb{1}\left\{JEC_{scgm}\right\} * N_{sgcm} \tag{1}$$

$\mathbb{1}\left\{JEC_{scgm}\right\}$ is an indicator function equal to one if school s in municipality m had implemented JEC for grade g when cohort c was in grade g. N_{sgcm} is the number of students enrolled in grade g in school s in municipality m, obtained from Ministry of Education administrative data. We use administrative enrollment data by school-grade from the 2013 school year as it is sufficiently late in the implementation process to provide a measure of capacity in schools that were newly built because of JEC. Moreover, since 2013 follows the formal implementation period, enrollment in this year is less prone to intra-municipality sorting between schools that offer JEC and those that do not.[8]

As this measure of access does not depend on the school a student actually attends, but is instead based on students' locations before the policy was announced, it is not biased by students selecting in to full-day schools or moving to cities with greater JEC availability.

[8]From a practical perspective, 2013 is the earliest year grade-level enrollment data are available from the Ministry of Education. If schools that adopted JEC relatively early experienced increases in enrollment relative to those that adopted later within the same municipality, using a later year will lead to our estimated ($\widehat{JEC_{cm}}$) being larger than the true $\mathbb{E}(JEC_{cm})$, and therefore our results will represent a lower bound on the returns to full-day access. We also exclude all schools with a single student in grade g in 2013. Fewer than 7 percent of school-grade observations are dropped with this restriction, and results are robust to including the full universe of schools.

This treatment measure is similar to approaches in other work examining the effect of JEC access on contemporaneous outcomes (Berthelon and Kruger, 2011; Berthelon et al., 2016), but builds upon the point-in-time estimate by summing full-day exposure across grades 1-12 in order to obtain the total number of years a student would be expected to attend a full-day school throughout his or her career.[9]

$\widehat{JEC_{cm}}$ provides a continuous measure of full-day school access rather than a discrete measure in order to be consistent with the Chilean school setting and the nature of the reform. First, multiple schools serve a single grade in nearly all municipalities (97 percent). Accordingly, the probability a student attends a full-day school in any given year is not exactly equal to 0 or 1. Moreover, students with access to full-day schooling in an early grade may lose access in their academic careers. This pattern appears to be most common in areas where a large share of elementary schools adopted full-day schooling relatively early in the roll-out period, but secondary schools adopted relatively late.[10]

There is substantial variation in access to full-day schooling both within and across birth cohorts, shown in Figure 1. Panel (a) shows the 5th, 50th, and 95th percentile of JEC access by birth cohort. This figure indicates that an individual expected to attend 4 years of full-day school was in the 95th percentile of the 1986 birth cohort, the median of the 1989 cohort, and the 5th percentile of the 1993 birth cohort. Panel (b) plots the share of students attending a JEC school each year by municipality and illustrates substantial cross-city variation in both the introduction and expansion of full-day schools, with the thick line denoting the national average. This plot shows that some areas made considerable initial progress in implementing JEC but lagged in expanding to all grades, while others started

[9]The CASEN household survey only includes information on respondents' year of birth (not month-day). The Chilean school year begins in March, and children who turn five through June are eligible to enroll (McEwan and Shapiro (2008) provide a full description on Chilean enrollment cutoffs). We define age in first grade based on a child's year of birth plus six; accordingly, for children born in January through June, our approach assigns them the JEC exposure of an younger cohort (e.g.,: weakly greater years of full-day schooling than they actually had access to). As our estimates err on the side of under-estimating JEC exposure, our results are a lower bound on the actual exposure effect.

[10]As cumulative access depends on both when a cohort first gained access to JEC and how quickly the reform was expanded, a typical event study framework is not feasible in this setting. In an event study spirit, however, Figures 3-5 and 7-8 illustrate the extent to which there are constant returns to an additional year of JEC access.

slowly but quickly accelerated coverage. While less-populated areas tended to be able to expand more quickly, other area characteristics are not significantly predictive of the pace of implementation.

Figure 2 summarizes how this varying exposure translates into the JEC exposure distribution for our main sample. About 11 percent of our sample had no access to full-day schools; we exclude this large spike from the figure. Among the remaining 89 percent with some exposure to the reform, a quarter of those are expected to attend a full-day school for at least four years, and 9 percent are expected to attend full-day schools for at least six years.

4 Empirical Approach

4.1 Data

Using an individual's year and city of birth, we map expected years of full-day schooling to data on economic outcomes in adulthood from the 2006 through 2017 waves of Chile's biennial demographic survey, the National Socioeconomic Characterization Survey (CASEN). Similar to other household surveys, such as the Current Population Survey (CPS) in the United States, the CASEN is a large, regionally- and nationally-representative household survey that provides comprehensive individual-level information on labor market participation, household structure, educational attainment, family background, and income.[11] Important for our purposes, starting with the 2006 survey, each individual was asked where his or her mother was living when he or she was born, whether the current city of residence or a different city (and if the latter, which city). We use this information to identify the city of birth, linking approximately 98 percent of respondents to a birthplace. Most of the unmatched observations result from respondents reporting mother's residence at birth at a higher level of aggregation than the municipality (e.g., the region or the province).

[11] Our main specifications use regionally-representative weights in order to provide the most comprehensive coverage of the population; results are qualitatively unchanged when using municipal- ("comuna") level weights or without weighting.

We limit the sample to individuals born between 1979 and 1992–those who were school-aged (between ages 5 and 18) the first year of the reform and were thus exposed to between zero and 12 years of full-day schooling. Our main sample limits the data to CASEN respondents between the ages of age 23 to 38 in order to explore how access to longer school days during childhood affects outcomes in adulthood.[12]

Table 1 displays summary statistics for main sample, as well as subpopulations disaggregated by gender and socioeconomic status (SES), where socioeconomic status based on maternal educational attainment.[13] The average respondent is about 28 years old and expected to have attended full-day school for 2.0 years. These characteristics are similar by gender and family socioeconomic status. Overall, about 80 percent graduated high school, 20 percent have at least a four-year university degree, and students from less-disadvantaged backgrounds have greater levels of educational attainment. About two-thirds of the full sample worked in the previous month, and women have substantially lower participation rates than men. Even though our sample is relatively young, nearly 60 percent have children.

Appendix Table A1 explores whether exposure to JEC is associated with student characteristics by regressing student characteristics (defined at birth) on access to JEC, controlling for survey year, municipality of birth, and region-specific cohort factors: $y_{icmt} = \alpha + \beta JEC_{icmt} + \delta_{cr} + \phi_t + \psi_m + \varepsilon_{icmt}$. There are no significant differences in access by maternal educational attainment, race, or gender. Nonetheless, all of our results control for these characteristics in order to improve precision. We also report results separately for men, women, and by maternal educational attainment in order to explore whether the aggregate results are driven by the experiences of a subpopulation.

[12]For results focusing on high school graduation, we extend the sample to include individuals ages 19-22 who were born in the 1979-1992 window. Results are robust to excluding these individuals.

[13]Individuals whose mother graduated high school are considered "high SES," and those whose mother did not complete high school are defined as "low SES'.' Respondents with unknown maternal educational attainment are not included in either subgroup (about 20 percent of the sample from CASEN waves 2006-2015 and 40 percent for 2017).

4.2 Exposure to JEC

The Chilean JEC reform is typical of full-day reforms in other Latin American countries. Longer school days require a substantial increase in facilities and instructional resources. At the extreme, if a single building operated two school "shifts" at capacity before the reform, the transition to a full-day school would require a doubling in building space and teaching staff. Since new facilities must be built and additional teachers and staff recruited, full-day schooling reforms are typically implemented over multiple years.

One approach to estimate the effects of a longer school day would be to assume the timing of introducing a longer school day is randomly assigned and estimate the difference between students with different levels of treatment. In its most basic form, this approach would estimate the effect on outcome y of attending a full-day school for JEC_{icmt} years for individual i living in municipality m, in birth cohort c and surveyed at time t as:

$$y_{icmt} = \alpha + \beta JEC_{icmt} + \varepsilon_{icmt} \tag{2}$$

This simple framework requires that the introduction of JEC is uncorrelated with students' potential outcomes. There are several reasons why this assumption may not hold, even after accounting for cohort- or city-specific factors. First, Chile adopted full-day schooling during a period of robust economic growth; therefore comparing outcomes of younger to older cohorts will conflate the effect of schooling with aggregate wage growth and other improvements in economic opportunities.

Second, examining the effect of full-day schools using a single cohort and only relying on geographic variation in full-day access is also potentially problematic. Given the funding requirements of a large-scale expansion, policymakers might prioritize initial funding to undersubscribed schools or those with excess capacity. Alternatively, officials with limited resources may target early adoption to areas that are better able to implement the program or maximize the effect of the funds by targeting the neediest areas. If disadvantaged areas pilot the program, a naive OLS approach comparing early- and late-adopting schools under-

states any benefits. On the other hand, if these schools are located in areas better situated to support a large-scale expansion, the basic framework will overstate any benefits of moving to a full-day schedule. We explore these patterns in two analyses discussed below and find that more rural areas tended to implement JEC earlier and more quickly. In order to account for these patterns, we follow the existing literature and only consider variation across cohorts within a given region and include a vector of controls for both contemporaneous economic conditions, as well as survey year trends in baseline (1996) poverty and employment rates in an individual's city of birth.

A final threat to the basic OLS design is that, even if JEC implementation was randomly allocated across schools over time, the within-cohort approach does not fully account for selection into full-day schools as Chilean families can choose the school their child attends, including schools outside their city of residence. Selection into full-day schools can arise from families moving across municipalities or selecting a school outside their city of residence.

We estimate that about 20 percent of school-aged children live in a different municipality than where they were born, and more disadvantaged SES populations are significantly less likely to move than students from more educated families. In our sample, most moves are local: less than 10 percent of children live in a different region than where they were born. As families that move tend to migrate to areas with nearly identical JEC access (on average, about 8 percent additional JEC among the moving sample, with a median of about 0.06 percent greater access), and we lack information on where adult CASEN respondents lived during childhood (or in which municipality they attended school), we define access to full-day schooling based on municipality of birth.

In practice, most students attend a nearby school: 95 percent travel less than 6 kilometers between home and school, and most elementary students attend a school within 2 kilometers (Gallego and Hernando, 2010; Chumacero and Paredes, 2008). Even though average travel distances are short, the nature of school selection suggests that those who enroll in full-time schools are likely those who benefit the most from the additional school time (Berthelon et al., 2016). From a practical perspective, our data do not include the exact school an

individual attended.

To overcome both potential selection bias and data limitations, we measure full-day school access as "exposure" to JEC – the expected number of years a student attends a full-day school based on his or her birth city and cohort, described in equation 1. Our main specifications omit the Santiago metropolitan region, as municipalities in Santiago are more geographically-proximate so students in Santiago are able to easily access a school other than their neighborhood school (Chumacero et al., 2011). We verify these patterns using attendance data from 2015 and find that about 15 percent of elementary school students in Santiago attend a school in a municipality different from where they live, compared to less than 10 percent in other regions.[14]

Importantly, the gradual rollout of JEC provides two sources of variation: first, children born in the same city are exposed to different amounts of full-day schooling based on the year they were born. Second, children born in the same year are exposed to different amounts of schooling depending on their city of birth.[15] We leverage both sources, comparing outcomes based on within- and across-cohort variation. A causal interpretation of our results therefore involves the identifying assumption that the *pace* of JEC implementation is uncorrelated with potential outcomes among students born in the same city in different years. Using the measure of JEC access $\widehat{JEC_{cm}}$ from Equation 1, we estimate the effect of full-day schools on outcome y_{icmt} as:

$$y_{icmt} = \beta(\widehat{JEC_{cm}}) + X_{icmt}^{'}\gamma + Z_{mt}^{'}\theta + \delta_{cr} + \phi_t + \alpha_t c Z_{1996r} + \psi_m + \varepsilon_{icmt} \tag{3}$$

where i indexes individuals in cohort c, born in municipality m and surveyed in year t. In order to improve precision, we include X_{icmt}, a vector of individual demographic characteristics, including age, gender, indigenous status, and maternal education. For labor market

[14] As shown in the final two columns Appendix Tables A3, A5, A6, A7, A10, A11, and A9, including Santiago renders results smaller in magnitude and less precise. With the available data, we are unable to determine whether this pattern is due to a weak first stage (calculated access being a noisy measure of actual access) or heterogeneity in benefits between urban and rural locations.

[15] All individuals in our sample were born in 1992 or earlier, before the reform was announced (in 1997). As we rely on location decisions before the policy was announced (e.g., at birth), our estimated access to full-day schooling is not affected by any migration decisions occurring after the policy announcement.

outcomes other than educational attainment and childbearing, we also include controls for marital status and number and presence of children interacted with gender and household size. We also include a vector of city characteristics for a respondent's current location, Z_{mt}, including employment and poverty rates and average earned income, as well as $\alpha_t c Z_{1996r}$, a separate survey year linear trend in baseline (1996) municipal employment and poverty rates.[16]

The empirical approach in equation 3 assumes a linear treatment effect – that is, that marginal benefits are constant for each additional year of JEC exposure. In order to explore the presence of increasing or decreasing marginal returns, we adopt a less parametric approach by replacing the continuous measure of full-day exposure with nine one-year exposure bins, pooling all observations with at least eight years of exposure:

$$y_{icmt} = \sum_{y=0}^{8} \beta_y \mathbb{1}\{(\widehat{JEC_{cm}}) \in [y, y+1)\} + X'_{icmt}\gamma + Z'_{mt}\theta + \delta_{cr} + \phi_t + \alpha_t c Z_{1996r} + \psi_m + \varepsilon_{icmt} \quad (4)$$

When interpreting these results and reconciling with the difference-in-difference estimates, we emphasize that access to full-day schooling is heavily skewed: 30 percent of our sample has access to one year of JEC or less, and one percent is estimated to receive more than eight years of full-day instruction. With this distribution in mind, the more flexible strategy suggests diminishing marginal benefits to each additional year of full-day schooling.

The JEC implementation window covers a time period of marked improvement in economic conditions in Chile. In particular, real GDP increased about 50 percent between when the oldest and youngest individuals in our sample were born (World Bank, 2017). In addition, secondary school became mandatory for cohorts graduating in 2003 or later (Ley 19876, 2003).[17] All of our estimates include survey year fixed effects, ϕ_t to account for level differences in economic performance at the time of the survey, as well as municipality fixed effects ψ_m to control for local time-invariant characteristics. We finally include region-specific

[16] Appendix tables show results are nearly identical when omitting baseline trends.

[17] In a series of robustness checks, we have verified the results robust to limiting the sample to cohorts graduating either before or after the compulsory schooling reform.

cohort fixed effects, δ_{cr}, in order to limit our comparisons to students born in the same year within a relatively local geographic region and capture general economic conditions that may affect each birth cohort's access to JEC and subsequent labor market outcomes.[18] As δ_{cr} varies by cohort, it accounts for national and regional-level changes in schooling requirements or education policy.[19]

To more formally explore the possibility of non-random timing at the municipality-level, we examine the extent to which JEC coverage is associated with contemporaneous economic conditions during the rollout period in Table 2. Specifically, for each year during the implementation period for which a CASEN survey was conducted (1996, 1998, 2000, 2003, and 2006), we regress the fraction of students in grades 1-12 attending a full-day school in municipality m with measures of city economic and demographic characteristics in that same year. Without including municipality fixed effects, Table 2 indicates full-day schooling was rolled out quickly in relatively low-populated areas with low levels of educational attainment (columns (1) and (2)). Columns (3) through (6) include municipality fixed effects in order to examine the extent to which increases in JEC coverage are associated with *changes* in a locality's economic conditions, and columns (5) and (6) explore the importance of alternative measures of disadvantage by including per-capita income, rather than poverty rates. Across specifications, increased JEC participation is associated with increased poverty rates, although this relationship somewhat sensitive to the measure of disadvantage (columns 3 and 4, versus 5 and 6) and economically small in magnitude: moving from a 0 percent to 100 percent poverty rate is associated with an the share of students attending full-day schools increasing by 14 percentage points. Over the 1996 to 2006 period, the poverty rate in the median municipality fell by 12.5 percentage points, therefore, scaling the estimated coefficient by the (absolute) changes in poverty within a city over a decade implies a very small change in access to full-day schooling, relative to the full possible 12 years of exposure.

[18]There are 15 regions in our analysis period. Excluding the Santiago region, we identify city of birth for respondents in between four and 54 municipalities within a region. In total, each region outside of Santiago has between 100,000 and 1.8 million residents, compared to between about 200 and 300,000 per municipality.

[19]As JEC exposure does not vary among students from the same cohort living in the same municipality, we are unable to include cohort-specific fixed effects at more granular geographies.

Nonetheless, in order to account for the possibility that the timing of JEC adoption is correlated with changes in local poverty conditions, we include survey year trends in pre-reform (1996) municipality of birth poverty and employment rates. The Appendix shows omitting these trends does not meaningfully affect our results.

We further explore the extent to which the introduction and pace of JEC is correlated with baseline characteristics regressing the first and last year of JEC implementation and the number of years it took for a municipality to go from 0-100% coverage using information from the 1992 Census in Appendix Table A2, similar to the approach in Hoynes and Schanzenbach (2009), with the caveat that not all information is available for the smallest cities.[20] Appendix Table A2 shows that while larger areas had some JEC access relatively early (columns (3) and (4)), less populated areas moved to full implementation more quickly (columns (7) and (8)). Holding other factors constant, there is no significant correlation between the timing of adoption and other baseline characteristics. Even with regional fixed effects, however, more than 40 percent of variation in the pace of implementation is unexplained.

5 Findings

5.1 Educational Attainment

We first turn to explore whether access to additional school time changes educational attainment, as changes in educational attainment provide one mechanism for any patterns in earnings or labor force attachment in the long term. *A priori*, the effect of JEC on high school and college graduation is ambiguous. On one hand, less leisure time during high school reduces the ability of teenagers to hold part-time jobs, and increases the opportunity costs of attending school, which may increase drop-out rates. On the other hand, if more time in elementary or secondary school prepares students for higher education or instills

[20]We use data from the last Census that was administered prior to the announcement of JEC rather than the CASEN as the Census provides information on more outcomes that are representative at the municipality level than the CASEN. Even with this richer data, municipality-level information is only available for relatively populous areas. We view these results, although suggestive, as complementary to the findings in Table 2 using the CASEN data.

non-pecuniary benefits of schooling (a stronger "taste" for education) longer school days may increase educational attainment.

Tables 3 and 4 show how JEC changed educational attainment by estimating the cross-city, cross-cohort framework in equation 3 on high school and university graduation, respectively. The effect of exposure to an additional year of full-day schooling is presented in the first row. There are several ways to scale these estimates to recover policy-relevant parameters. First, the effect for the average individual in our sample is obtained by multiplying this row by the average expected number of JEC years in each sample: about 2 years for college graduation, and 3 years for the slightly younger high school graduate sample. Alternatively, the implied effect of an additional year-equivalent of education, is recovered by scaling the main estimate by the average increase in instructional time under JEC ($\frac{\beta}{0.3}$).

For the full population, access to an additional year of full-day schooling increased the probability of high school graduation by 2.1 percentage points (column (1) of Table 3). The remaining columns explore whether these effects differ by gender or family socioeconomic status. Heterogeneity along these dimensions is of interest for several reasons. First, as maternal education is particularly important for child outcomes (Andrabi et al., 2013; Carneiro et al., 2011; Currie, 2009), any intergenerational benefits of full-day schooling are likely to arise through gains to women. Second, essentially all children from lower-income families attend government-subsidized, rather than private, schools, whereas most students from the highest-income families attend private schools (CASEN, 2016). Moreover, substantially fewer children from disadvantaged backgrounds move across municipalities between birth and school start (18 vs. 28 percent). Therefore, estimated access to full-day schooling is more likely to reflect actual access among this population, and we may interpret lower-income students as a "high-complier" population where the reported ITT estimates are expected to be similar to the TOT effect. In addition, evidence from other educational interventions suggests the returns to educational inputs may be larger for lower-income students (Cunha et al., 2006; Havnes and Mogstad, 2011).

Greater access to full-day schooling increased high school graduation among all sub-

groups, particularly for women (Table 3 column (2)) and students from disadvantaged backgrounds ("low SES" in column (4)).[21] These qualitative patterns are robust to alternative samples and more parsimonious controls for local economic conditions (Appendix Table A3.

Table 4 reveals different patterns for college graduation. Longer school days increase university graduation for all groups, but especially for men and higher SES populations. For men, an additional year of full-day schooling increases college completion by 1.9 percentage points (about 11 percent) for men and 1.8 percentage points (about 6 percent) for students from higher-SES families. In Appendix Table A4 we examine whether JEC led students to receive at least some college education in order to explore whether the differences between high school graduation and college graduation are due to students not beginning college, or starting college but not yet completing. For women, the primary margin appears to be high school completion: there is a small and weakly significant increase in the likelihood of receiving at least some college education. For men, these results are consistent with increases in college graduation documented in Table 4. Whereas additional time in elementary and secondary school prompted low-SES students to complete high school and enter college, higher-SES students attend and complete college at higher rates.

During the JEC implementation period, the Chilean government enacted other changes to the educational environment. Beginning in 2003, free elementary and secondary education was guaranteed and compulsory for all individuals up to age 21. By requiring students to enroll in school through their teen years, this reform may have mechanically increased high school graduation rates. Importantly, region-by-cohort fixed effects will account for this national-level reform as expanded compulsory schooling affected all cohorts born in 1982 and later, regardless of the place of birth. Compared to older (1979-1981) birth cohorts, high school graduation rates for the post-1982 cohorts in our sample are 12 percentage points higher (83, versus 71, percent), suggesting the compulsory schooling reform was effective at increasing high school graduation rates. Even after the reform, however, not all individuals

[21]The low SES sample is defined as individuals whose mothers have no more than a basic education, as reported by individuals and linked by family structure. See Data Appendix for details. We pool men and women from disadvantaged households; there are no substantial differences in outcomes by gender among this subpopulation.

completed secondary school, suggesting imperfect compliance with compulsory schooling and the scope for other interventions to induce schooling completion. We obtain similar results for both secondary and tertiary education when we limit the sample to those subject to the secondary schooling law (cohorts born after 1981), suggesting that longer school days increased educational attainment above and beyond the provisions in the secondary schooling reform (results available upon request).

A separate question is whether there are diminishing marginal benefits to additional years of full-day schooling or if there exists a threshold after which longer school days provide especially large benefits. To explore these patterns, Figure 3 plots the β_y coefficients from the less parametric approach in equation 4 and shows access to *any* full-day schooling increases high school graduation rates by approximately 3 percentage points, with relatively small marginal increases for each subsequent year of full-day schooling. Figure 4 shows the likelihood of the full population is generally increasing in exposure to JEC, with each additional year of exposure conferring a smaller marginal gain. This aggregate pattern is clearer among men, and mirroring the results in Table 4, additional time in school does not increase college graduation rates among disadvantaged students, while those from more highly-educated families incur a one-time increase that further increases after about five years of exposure.

These figures also illustrate the distributional effects of JEC. Specifically, about 30 percent of our sample is exposed to less than 1 year of JEC, while about 5 percent have at least 6 years. The vertical distance from one year of JEC to the $[6, 7)$ point then roughly corresponds to changes going from the 30th percentile to 95th percentile of JEC access (about 10 log points for high school graduation and 6 log points for college graduation).

5.2 Labor Market Outcomes

The return to secondary schooling in Chile was large during the period JEC was introduced. The existing estimates of the high school wage premium during this period range from about 34 percent relative to those with an eighth grade education (8 percent per year of secondary education) to 64 percent (11 percent a year) relative to those with a sixth grade

education (OECD, 2013; Manacorda et al., 2010). The estimated earnings premium for post-secondary education is even higher: Manacorda et al. (2010) find Chilean men with a university degree have higher labor force participation rates and earn 90 percent more than those with a secondary education. Since JEC increased educational attainment, we might expect improved economic outcomes when students in their 20s and 30s.

5.2.1 Employment

We first examine whether JEC changed employment rates, as the probability of working is increasing in educational attainment. In Table 5, we defined employment as whether an individual had at least 30,000 pesos in work income the previous month (approximately $50 in 2017 dollars).[22] For all subgroups, changes in employment are modest in magnitude, at about 1.3 percentage point or less from a base of 55 to 66 percent. While the estimate for disadvantaged populations and women suggest significant increases in employment on the order of two percent, we do not find any significant change among students from high SES backgrounds.These results contrast with the findings of Pires and Urzua (2015), which does not observe any aggregate increase in employment. One difference for these findings is that Pires and Urzua (2015) focus on for students with access to full-day schooling only in the final years of high school and who are in their mid-20s at the time of the survey. As we find increases in educational attainment, our larger employment responses point to the importance of measuring labor market outcomes after respondents have reached an age where they are expected to have completed schooling.

Figure 5 takes a less parametric approach shows the probability of employment is generally increasing in access to full day schooling, with significant employment gains among women and disadvantaged students emerging with approximately two years of JEC access. In contrast, although estimates are insignificant throughout the JEC distribution, there is no evidence access to more full-day schooling changes employment among students from more educated backgrounds. In additional results we do not find a significant change in the

[22]Results are qualitatively similar to defining work as employment in the week prior to the reference period.

probability young adults are currently in school, suggesting that the lack of an employment response among students from the highest-SES backgrounds is not driven by selection out of the labor force and into post-secondary schooling (results available upon request).

5.2.2 Earnings

Given increases in educational attainment and employment, we would expect that access to longer school days would increase earnings in early adulthood. To our knowledge, this study provides one of the first direct examinations of the relationship between earnings and full-day schooling for a full, large-scale national reform.[23]

Table 6 panel (a) reports the semi-elasticity of earnings with respect to an additional year of JEC, where earnings is measured as the log of earnings in the previous month, plus one in order to include individuals with no earnings.[24] Consistent with longer school days improving labor market outcomes, Table 6 shows additional time in school increases earnings, with each additional year of full-day access increasing earnings by 4-5 percent (columns (1) through (4)) for all groups except those from the most advantaged backgrounds. To put these numbers in context, as JEC increased instructional time by 30 percent, the results in column (1) suggest about a 16 percent ($\frac{0.048}{0.3}$) return to each year-equivalent of education. These magnitudes are on the higher end of the returns to education found in higher-income countries (Card, 1999) and consistent with the ranges found for Chile during the 1990s.

In order to examine the full distribution of earnings responses – that is, whether any gains are concentrated among especially low- or high-wage ends of the labor market, Figure 6 displays results from a series of regressions where the outcome of interest is a binary variable whether an individual has annual earnings of at least x pesos, following Carrell et al. (2018). This approach incorporates both labor force participation and earnings responses like Table 6 panel (a). Figure 6 indicates that access to longer school days had particularly

[23] Pires and Urzua (2015) examine labor market outcomes for students who were in high school when JEC was introduced and measure earnings when these students are in their mid-20s. Our study broadens our understanding of this relationship by examining labor market outcomes for students who had up to 12 years of access to longer schooling and tracking earnings through individuals' 20s and 30s.

[24] Appendix Table A7 shows larger earnings gains using levels or earnings or an inverse hyperbolic sine transformation as the dependent variable.

pronounced effects on the low end of the labor market, with negligible effects in increasing the likelihood an individual earned more than about 1.5 million pesos a month ($2,500, about the 97th percentile). The pattern is less monotonic for higher-SES individuals, for whom earnings gains are most pronounced between 0.6 and 1.2 million pesos, consistent with these individuals having relatively high earnings regardless of JEC access.

Figure 7 plots the coefficients from the less parametric approach to investigate non-linearities in access to longer school days and earnings. The figure shows that for the overall, male, and disadvantaged populations, log earnings increase approximately linearly between about 2-8 years with little evidence of diminishing marginal returns. Regardless of how many years more advantaged students are likely to have access to JEC, there is no significant change in earnings.[25]

Finally, Table 6 panel (b) limits the sample to workers (defined as in Table 5) in order to examine whether the patterns in Figure 7 are driven by more individuals entering the workforce or higher earnings among the employed. Between half and 80 percent of the overall increase is due to higher earnings among the employed for women, men, and low-SES groups. For higher SES groups, we also find evidence of longer school days increasing earnings after conditioning on employment.

5.3 Mechanisms

There are several intermediate, non-mutually exclusive channels through which longer school days could increase earnings in adulthood. We previously documented one such mechanism – greater educational attainment. This section explores other potential channels.

[25]We have examined the effect of the reform on usual hours worked and found a marginally significant increase for this group of about 0.2 hours (less than three minutes a week). We interpret these results with some caution, as this variable is likely measured with error: half of workers in our sample report working exactly 45 hours in a typical week. An alternative explanation, which we are unable to explore with the available data, is changes in part-year or seasonal work.

5.3.1 Migration

One possible explanation for increased employment and earnings is that individuals with greater educational attainment have greater ability and financial resources to migrate from rural areas to Santiago and other areas where wages are higher. Table 7 investigates the relationship between JEC exposure and subsequently moving a municipality outside the municipality of birth at any point. For all populations, there is no significant change in migration patterns from greater exposure to longer school days. Appendix Table A9, column (1) shows that migration to Santiago, the largest metropolitan area, likewise did not change. Column (2) of Appendix Table A9 takes a more general approach by multiplying an indicator for currently residing in a municipality other than the municipality of birth with a measure of economic prosperity in the current city, where we define economic prosperity as a standardized index based on the leave-out-mean individual income in each respondent's current municipality.[26] Here, we find individuals with greater access to full-day schooling, particularly those from more highly-educated families, tend to move to more prosperous areas.[27] In Section 5.4 we consider general equilibrium effects in order to analyze how JEC shaped the economic opportunities in an area.

5.3.2 Fertility Patterns

For women, motherhood is associated with labor force non-participation and lower earnings upon labor market re-entry (Waldfogel, 1998; Kleven et al., 2018; Bertrand et al., 2010; Kuziemko et al., 2018). Previous work has documented that JEC leads to lower teen pregnancy rates for disadvantaged women in urban areas (Berthelon and Kruger, 2011). We also find small reductions in teen pregnancy, consistent with these earlier findings (column (1), Appendix Table A10). When we estimate the effect of longer school days on the age at first birth among women who gave birth to at least one child, we find access to full-day schooling

[26] In particular, we calculate $\frac{\overline{y_{mt}} - \sum_m \sum_t \overline{y_{mt}}}{\sigma_{\overline{y}}}$ where $\overline{y_{mt}}$ is per capita income in municipality m surveyed at year t and $\sum_m \sum_t y_{mt}$ is the grand mean across all city-years, and $\sigma_{\overline{y}}$ is the corresponding standard deviation.

[27] As emigrants are not surveyed in CASEN, our findings also do not reflect international migration.

led women to give birth at older ages, consistent with reductions in teenage pregnancy. Each additional year of full-day schooling delayed birth by about two months (Table 8 and Figure 8). These results are slightly larger for lower-SES women (columns (2) vs. (3)). As the youngest individuals in our sample are in their early 20s and have not yet reached prime childbearing years, this estimate likely understates the full effect of JEC on family formation patterns.

5.3.3 Occupation Choice

Another mechanism by which longer school days could increase earnings is through occupational choice. As the majority of additional school time under JEC went towards reading and math instruction, students attending full-day schools are expected to have entered the labor force with greater skills, even absent a formal credential. In Table 9, we find longer school days increased the likelihood of having a managerial, professional, or technical occupation by about 1 percentage point (3.5 percent) for all individuals from non-disadvantaged backgrounds, while greater access to longer school days does not affect the share of low-SES individuals in these roles.[28]

Since most "high-skilled" occupations in Table 9 require a university degree, Appendix Table A11 column (1) explores an alternative measure of occupational prestige that captures upskilling across the entire skill distribution. For each individual, we measure the log earnings of other workers j in the same 4-digit occupation o as the leave-out mean:

$$\overline{w_{io}} = \frac{\sum_j w_{j \neq i}}{\sum_j N_{jo} - 1} \tag{5}$$

Increased access to full-day schooling increases occupational prestige (measured by salary) for both men and women, as well as for higher-SES individuals. In contrast, there continues to be no relationship between the types of occupations held by lower-SES populations and exposure to full-day schools.

[28]Following ILO, we define "skilled" occupations as the primary occupation in managerial, professional, and technical occupations (major codes 1, 2, and 3).

5.3.4 Family Resources or Academic Skills?

In addition to increasing human capital accumulation for students, longer school days provide a source of child care for families. This implicit subsidy increases family resources by reducing the cost of child care and potentially allowing parents to enter the labor force or work longer hours rather than provide home-based care. Although the CASEN does not ask whether a respondent's parents were employed during childhood, we provide suggestive evidence on the extent to which our findings are driven by increased parental employment by calculating the change in labor force participation rate among mothers with school-aged children in the 1996-2006 CASEN surveys at the municipal level. We then define a "high maternal LFP increase" sample comprised of municipalities that experienced greater-than-median increases in maternal labor supply over the first decade of JEC implementation (1996 to 2006). Cities in this subsample increased maternal labor force participation by at least 6.3 percentage points from a base of about 38 percent, and the average city in this sample increased maternal participation by about 12 percentage points.

Table 10 examines our main outcomes when we limit the sample to areas with large increases in maternal labor force participation. While we cannot rule out results of the magnitude found in the main findings for most outcomes, in general, Table 10 does not show benefits were exclusively found in areas with particularly large increases in maternal labor supply. While we are unable to directly account for changes in family income and labor supply at an individual level, these results suggest that our main findings are not exclusively driven through changes in family resources during childhood.

5.4 General Equilibrium Considerations and Robustness

During the 1990s and early 2000s, Chile underwent a period of political stability, deregulation, and economic growth. Policymakers across the political spectrum advocated policies to alleviate poverty and open the country's economy to trade (Foxley, 2004). Similar economic reforms occurred in much of Latin America and Eastern Europe over this period and

continue in many emerging economies today. Therefore, our results arguably generalize to other settings.

A separate question is the extent to which our findings have internal validity, and in particular, whether our measure of JEC access is capturing other local economic changes that affect labor market outcomes. Columns 5 and 6 of Table 2 indicate that after conditioning on year and city fixed effects, city-level labor market and demographic characteristics at the time of JEC implementation are not significantly associated with the pace of JEC adoption; here we further explore this issue by examining the relationship between full-day schooling and the entire local economy in the long-run.

JEC was a large-scale reform increasing classroom time up to 30 percent and eventually covering all students attending publicly-funded schools. Given the nature of the program, the partial equilibrium effects on the treated cohorts – the internal rate of return – may understate the full return to an additional year of schooling. Specifically, Table 3 showed JEC increased educational attainment, thereby increasing the size of the skilled labor force. In standard economic models, this increase in skilled labor supply is expected to reduce the earnings of skilled workers relative to those with less education (Goldin and Katz, 2009). To the extent that younger and older workers are imperfect substitutes, examining spillover effects to skilled and unskilled older workers can provide a sense of the magnitude of any general equilibrium effects (Khanna, 2015).[29]

In order to estimate the presence of general equilibrium and spillover effects of additional schooling, we augment equation 3 by adding the average years of JEC exposure among the full adult (ages 18 and older) population and labor force:

$$y_{icmt} = \beta_1(\widehat{JEC_{cm_b}}) + \beta_2\overline{(\widehat{JEC_{m_lt}})} + X'_{icm_lm_bt}\gamma + Z'_{m_bt}\theta + \delta_{cr_b} + \phi_t + \psi_{m_b} + \psi_{m_l} + \varepsilon_{icm_lm_bt} \quad (6)$$

where now m_l denotes the municipality in which individual i currently lives and m_b

[29]We thank the editor for this suggestion.

denotes his or her municipality of birth. As before, β_1 captures the private returns to an additional year of full-day schooling. $\overline{(\widehat{JEC_{m_l t}})}$ is the average exposure among adults living in municipality m_l at survey period t, and β_2 captures general spillover effects of the aggregate increase in educational attainment. As spillover effects are based on individuals' current city of residence, this framework incorporates all migration decisions. We include region of birth-by-cohort, municipality of birth, municipality of residence, and survey year fixed effects, as well as the standard set of individual and city controls, X' and Z', from Equation 3.[30]

We estimate equation 6 separately by skill level (those with less than a high school diploma, two measures of "high-skilled": those who graduated high school and those who completed college) and by age (individuals who were school-aged when the reform was introduced, "young" birth cohorts 1979-1993, and those who had already entered the labor market, "old" birth cohorts 1954-1978). As we have labor market information from multiple CASEN waves and m_l is not perfectly collinear with m_b, β_1 and β_2 are separately identified for individuals attending school during the implementation period. For the old cohorts who graduated high school before JEC was announced, we can only identify the parameter associated with the spillover effects,β_2.

Table 11 shows the extent to which exposing an entire population (panel (a)) or workforce (panel (b)) to longer school days affects each skill category and generation. For young cohorts in columns (1) through (3), we continue to see the internal return to education is positive and significant, on the order of about 2 percent a year (column (2)), or slightly more than half of the earnings gains estimated in Table 6. In contrast, the internal returns are small in magnitude and insignificant for both the lowest- and highest-skilled groups. Spillover effects from the overall population point to positive externalities for high-skilled workers, while any spillover effect on the least-skilled young workers is sensitive to whether aggregate access to full-day schools is measured across the population or the workforce. For older individuals, we find no significant or consistent effects across different educational groups.

Overall, these results are consistent with higher levels of education in the labor force fa-

[30]Applying the model outlined in Khanna (2015) is not feasible in this setting, as all of our "young" cohorts receive some exposure to longer school days – that is, there are no purely "untreated" municipalities.

cilitating sectoral shifts and facilitating agglomeration economies that stimulate the demand for relatively skilled workers. These patterns across age groups also suggest old and young workers are imperfect substitutes and that any negative externalities from a larger young, relatively-skilled workforce are small in this context at least for the first 20 years of the reform. As students exposed to additional years of JEC enter the labor force and progress in their careers, these dynamics may change.

As a related exercise, we conduct a placebo analysis on cohorts born between 1959 and 1973 who completed secondary schooling before 1997 and therefore did not have access to full-day schooling. Our placebo measure of JEC access is arbitrarily set at the expected number of JEC years received by the cohort born 20 years later in the same municipality:

$$\widehat{(JEC_{cm,placebo})} = \frac{1}{N_m} \sum_{s \in g} \sum_{g=1}^{12} \mathbb{1} \left\{ JEC_{sgm,(c+20)} \right\} * N_{sgm} \tag{7}$$

If our main results were simply capturing changes in local economic growth, we would expect to see improvements in labor market outcomes for these older individuals. Appendix Table A12 does not show any economically or statistically significant changes in college graduation, earnings, skilled occupation or age at first birth for any subgroup. Further, across all outcomes, the point estimates for this placebo sample are smaller in magnitude than those for students exposed to the reform. Although suggestive, combined with the general equilibrium analysis, this exercise indicates that our findings are not capturing changes in local economic conditions affecting the entire workforce.

6 Conclusion

We find that access to longer school days improves long-term economic well-being. Examining a large-scale national reform, we document that full-day schooling increases educational attainment, prompts more women and students from disadvantaged backgrounds to enter the labor force, and generates earnings gains on the order of 4-5 percent a year. The magnitude of these earnings gains is consistent with other work examining the returns to education in

Chile during this time period. The margins of adjustment vary by subgroup: students from lower SES families are more likely to enroll in college and enter the workforce, whereas those from more advantaged backgrounds complete college, work in high-skilled occupations, and live in wealthy areas at higher rates.

These results are consistent with longer school days promoting greater human capital development, as suggested by school reports that most of the additional time was dedicated to instructional activity. In our data, we do not observe systematic changes in migration patterns, and general equilibrium effects are imprecisely estimated and relatively modest in magnitude for most groups. Finally, we do not observe especially large improvements in areas that experienced the largest increases in maternal employment during the JEC rollout period, suggesting that our findings are not solely due to increases in family resources or parental employment.

While access to additional time in school benefits students, a broader question is whether such large-scale investments are worthwhile from a social welfare viewpoint. Extending the school day on a national level for all students requires substantial resources. In our setting, the move to full-day schooling increased per-pupil expenditures by at least 20 percent (an increase of approximately 18,000 pesos (31 USD) per student each month). Extrapolating our estimated earnings increase in Table 6 panel (a), we estimate additional earnings for students attending school in the first twenty years of the reform are between 60 and 120 percent as large as the increase in per-pupil spending over this period. In the steady-state (e.g.: after full implementation), we estimate the cost to government in providing 12 years of longer school days is about 10 percent the discounted value increase in earnings over a student's full career (ages 23 to 65).[31] This back-of-the-envelope calculation is not a full cost-benefit analysis – it does not include costs of infrastructure, maintenance or teacher hiring, nor does it include benefits accruing from delayed childbearing or reduced crime (Berthelon and Kruger, 2011), but it does illustrate that many important benefits of educational investments are only realized in the long-run, while costs are primarily incurred in the short-term. Altogether,

[31] We obtain a similar range when calculating the net costs of the first 20 years of implementation. Each of these estimates assume a 3 percent social discount rate.

the broad-based nature of our results shows that large-scale investments in public education can generate long-term and meaningful improvements in economic well-being.

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Table 1: Summary statistics: Main adult sample

	(1) All	(2) Women	(3) Men	(4) Low SES	(5) High SES
Demographic characteristics					
$\widehatJEC$	2.020	2.008	2.034	1.686	2.086
	(2.143)	(2.148)	(2.136)	(1.960)	(2.061)
Age	28.20	28.27	28.12	27.64	27.76
	(3.897)	(3.924)	(3.867)	(3.486)	(3.717)
Year of birth	1984.8	1984.7	1984.8	1984.0	1985.2
	(3.916)	(3.911)	(3.921)	(3.541)	(3.924)
Female	0.517	1	0	0.524	0.520
	(0.500)	(0)	(0)	(0.499)	(0.500)
Indigenous	0.104	0.107	0.101	0.133	0.0670
	(0.306)	(0.309)	(0.302)	(0.339)	(0.250)
Married	0.187	0.214	0.159	0.185	0.174
	(0.390)	(0.410)	(0.365)	(0.389)	(0.379)
Civil partnership or married	0.444	0.471	0.414	0.421	0.390
	(0.497)	(0.499)	(0.493)	(0.494)	(0.488)
Has own children (parent)	0.582	0.692	0.463	0.618	0.514
	(0.493)	(0.461)	(0.499)	(0.486)	(0.500)
Number of children	0.918	1.114	0.708	0.986	0.766
	(0.978)	(0.994)	(0.916)	(0.996)	(0.906)
Mother has $¡$ HS education	0.440	0.447	0.432	1	0
	(0.496)	(0.497)	(0.495)	(0)	(0)
College-educated mother	0.0739	0.0679	0.0804	0	0.212
	(0.262)	(0.252)	(0.272)	(0)	(0.409)
Economic well-being					
HS graduate	0.792	0.804	0.779	0.689	0.930
	(0.406)	(0.397)	(0.415)	(0.463)	(0.256)
College graduate	0.184	0.202	0.164	0.0980	0.308
	(0.387)	(0.401)	(0.371)	(0.297)	(0.461)
Worked last month	0.655	0.542	0.776	0.632	0.651
	(0.475)	(0.498)	(0.417)	(0.482)	(0.477)
Usual weekly hours worked last month	27.86	21.44	34.74	27.11	27.07
	(23.34)	(22.25)	(22.51)	(23.60)	(23.27)
Monthly earnings (1000s of 2017 pesos)	318.595	233.708	409.524	232.793	414.111
	(504.146)	(377.343)	(598.257)	(321.578)	(686.363)
Skilled occupation	0.292	0.379	0.226	0.172	0.456
	(0.455)	(0.485)	(0.418)	(0.378)	(0.498)

Table 1: (continued)

Residence characteristics					
Lives in urban area	0.852	0.854	0.849	0.779	0.942
	(0.355)	(0.353)	(0.358)	(0.415)	(0.234)
Lives in Santiago	0.110	0.112	0.108	0.0751	0.142
	(0.313)	(0.316)	(0.310)	(0.263)	(0.349)
Lives in different city	0.357	0.365	0.349	0.298	0.410
than city of birth	(0.479)	(0.481)	(0.477)	(0.457)	(0.492)
Employt rate	0.583	0.583	0.583	0.567	0.584
(city of residence)	(0.071)	(0.071)	(0.071)	(0.070)	(0.070)
Poverty rate	0.135	0.135	0.134	0.160	0.121
(city of residence)	(0.082)	(0.083)	(0.082)	(0.087)	(0.073)
Observations	157698	81210	76488	62767	48642

Notes: Table shows summary statistics for our full sample (column 1); women (column 2); men (column 3); individuals from disadvantaged backgrounds, defined as those whose mothers have less than a high school education (column 4); and individuals from advantaged backgrounds, defined as those whose mothers have at least a high school education (column 5). Expected JEC calculated from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys, using regionally-representative weights. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-38 years old at the time of survey. See text and data appendix for details.

Table 2: Predictive characteristics of JEC adoption: Economic characteristics during the roll-out period

	(1) pct JEC	(2) pct JEC	(3) pct JEC	(4) pct JEC	(5) pct JEC	(6) pct JEC
Employment rate, adults 18 +	0.2371	0.0688	0.0745	0.0245	0.0043	-0.0425
	(0.1944)	(0.2120)	(0.1551)	(0.1657)	(0.1574)	(0.1679)
Avg yrs education, adults 18 +	-0.0262***	-0.0377***	-0.0066	-0.0111	-0.0105	-0.0144
	(0.0065)	(0.0076)	(0.0108)	(0.0117)	(0.0118)	(0.0128)
Avg hh size	0.0184	0.0346	0.0238	0.0306	0.0349	0.0415
	(0.0271)	(0.0296)	(0.0301)	(0.0337)	(0.0293)	(0.0328)
Poverty rate	-0.0097	-0.0610	0.1366*	0.1403*		
	(0.0764)	(0.0849)	(0.0774)	(0.0846)		
Log population	-0.0220*	-0.0059	0.0056	0.0108	0.0040	0.0087
	(0.0117)	(0.0136)	(0.0139)	(0.0169)	(0.0140)	(0.0170)
Log autonomous income					0.0019	0.0010
					(0.0241)	(0.0261)
Observations	1368	1121	1368	1121	1368	1121
R-squared	0.692	0.676	0.880	0.874	0.879	0.874
Year FE	X	X	X	X	X	X
Municipality FE			X	X	X	X
Includes Santiago	X		X		X	

Notes: Dependent variable is the fraction of students in grades 1-12 attending a full-day school at the municipality level for each year during the JEC rollout period the CASEN was administered (1996, 1998, 2000, 2003, and 2006). Employment rate defined as the share of adults ages 18 and older in a municipality with at least 30,000 peso earnings in the last month (approximately $50); average years of education is the average number of years adults ages 18 and older in a municipality attended school from grades kindergarten through 16 (four years of university); poverty rate defined as the share of individuals in a municipality with household income below a minimum subsistence level, based on a food expenditures; autonomous income defined as per capita income in a municipality from all household sources, primarily earnings, and also rental income. See data appendix for details. Odd-numbered columns present results for all municipalities and even-numbered columns exclude municipalities in the Santiago metropolitan region. Robust standard errors clustered by municipality. *** $= p < 0.01$, ** $= p < 0.05$, * $= p < 0.10$

Table 3: Longer school days and high school graduation

	(1) All	(2) Women	(3) Men	(4) Low SES	(5) High SES
$\widehat{JEC}$	0.021***	0.024***	0.017***	0.022***	0.008***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)
Observations	248535	126929	121606	113172	79169
DV mean	0.794	0.808	0.779	0.701	0.926
Pct change	0.026	0.030	0.022	0.032	0.009
E($\widehat{JEC}$)	2.925	2.902	2.949	2.757	3.083

Notes: Dependent variable is an indicator equal to one if the respondent had completed high school at the time of the CASEN survey. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, and maternal education, as well as survey year linear trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 19-38 years old at the time of survey. Columns (2) and (3) limit the sample to women and men, and columns (4) and (5) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from columns (4) and (5). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text and data appendix for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 4: Longer school days and college graduation

	(1) All	(2) Women	(3) Men	(4) Low SES	(5) High SES
$\widehat{JEC}$	0.014***	0.009***	0.019***	0.004**	0.018***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.005)
Observations	172681	88972	83709	77796	52510
DV mean	0.182	0.199	0.164	0.0992	0.309
Pct change	0.075	0.044	0.114	0.044	0.059
E($\widehat{JEC}$)	1.958	1.944	1.973	1.768	2.041

Notes: Dependent variable is an indicator equal to one if the respondent had received a university degree at the time of the CASEN survey. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, and maternal education, as well as survey year linear trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 22-38 years old at the time of survey. Columns (2) and (3) limit the sample to women and men, and columns (4) and (5) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from columns (4) and (5). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text and data appendix for details. *** $= p < 0.01$, ** $= p < 0.05$, * $= p < 0.10$.

Table 5: Longer school days and employment in the previous month

	(1) All	(2) Women	(3) Men	(4) Low SES	(5) High SES
$\widehat{JEC}$	0.009***	0.013***	0.006*	0.011***	-0.008
	(0.003)	(0.004)	(0.003)	(0.003)	(0.005)
Observations	157696	81210	76486	70419	48641
DV mean	0.655	0.542	0.776	0.649	0.651
Pct change	0.014	0.025	0.008	0.017	-0.012
E($\widehat{JEC}$)	2.021	2.008	2.034	1.872	2.086

Notes: Employment defined as having income at least 30,000 pesos (approximately $50) in the past month. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-38 years old at the time of survey. Columns (2) and (3) limit the sample to women and men, and columns (4) and (5) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from columns (4) and (5). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text and data appendix for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 6: Longer school days and log monthly earnings

	(1) All	(2) Women	(3) Men	(4) Low SES	(5) High SES
			Panel a: All		
$\widehat{JEC}$	0.048***	0.049***	0.048***	0.039***	0.009
	(0.009)	(0.012)	(0.010)	(0.009)	(0.018)
Observations	157696	81210	76486	70419	48641
DV mean (level, 1000s pesos)	318.596	233.708	409.536	246.378	414.115
E($\widehat{JEC}$)	2.021	2.008	2.034	1.872	2.086
			Panel b: Workers		
$\widehat{JEC}$	0.036***	0.032***	0.038***	0.019***	0.043***
	(0.006)	(0.008)	(0.006)	(0.006)	(0.013)
Observations	101839	42245	59594	44852	31351
DV mean (level, 1000s pesos)	486.183	430.530	527.866	379.556	635.665
E($\widehat{JEC}$)	1.904	1.910	1.900	1.824	1.883

Notes: Log monthly earnings are defined as $log(earnings + 1)$ (in 2017 pesos) in order to account for individuals with no earnings. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-38 years old at the time of survey. Panel (b) is limited to individuals with earned income of at least 30,000 pesos in the previous month. Columns (2) and (3) limit the sample to women and men, and columns (4) and (5) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from columns (4) and (5). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text and data appendix for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 7: Longer school days and cross-municipality migration

	(1) All	(2) Women	(3) Men	(4) Low SES	(5) High SES
$\widehat{JEC}$	-0.001	-0.001	-0.002	0.000	0.000
	(0.003)	(0.004)	(0.003)	(0.004)	(0.005)
Observations	157696	81210	76486	70419	48641
DV mean	0.357	0.365	0.349	0.295	0.410
Pct change	-0.004	-0.002	-0.005	0.001	0.000
E($\widehat{JEC}$)	2.021	2.008	2.034	1.872	2.086

Notes: Dependent variable is an indicator equal to one if an individual resided in a municipality other than his or her city of birth at the time of the CASEN survey. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-38 years old at the time of survey. Columns (2) and (3) limit the sample to women and men, and columns (4) and (5) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from columns (4) and (5). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text and data appendix for details. *** $= p < 0.01$, ** $= p < 0.05$, * $= p < 0.10$.

Table 8: Longer school days and age at first birth (women)

	(1) All women	(2) Low SES	(3) High SES	(4) Urban
$\widehat{JEC}$	0.156***	0.095**	0.073	0.159***
	(0.032)	(0.045)	(0.066)	(0.038)
Observations	54128	21523	16081	42545
DV mean	21.13	20.70	21.54	21.22
E($\widehat{JEC}$)	2.462	2.185	2.302	2.370

Notes: Dependent variable is the age in years a woman gave birth to her first child. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, and maternal education, as well as survey year linear trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to women born between 1979 and 1992 outside the Santiago metropolitan region who had given birth to at least one child at the time of the survey. Column (2) limits the sample to women whose mothers had less than a high school education; column (3) limits the sample to women whose mothers had at least a high school education; column (4) limits the sample to women born in urban areas. Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text and data appendix for details. *** $= p < 0.01$, ** $= p < 0.05$, * $= p < 0.10$.

Table 9: Longer school days and working in a skilled occupation

	(1) All	(2) Women	(3) Men	(4) Low SES	(5) High SES
$\widehat{JEC}$	0.010***	0.010*	0.009***	-0.001	0.016**
	(0.003)	(0.005)	(0.003)	(0.004)	(0.007)
Observations	101209	42220	58989	44828	30829
DV mean	0.292	0.379	0.226	0.179	0.456
Pct change	0.034	0.025	0.042	-0.006	0.035
E($\widehat{JEC}$)	1.894	1.899	1.890	1.812	1.874

Notes: Dependent variable is an indicator equal to one if the respondent is employed in a skilled occupation, defined as a managerial, technical, or professional occupation, following ILO. Military members and respondents without valid occupation codes are excluded from the analysis. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-38 years old at the time of survey. Columns (2) and (3) limit the sample to women and men, and columns (4) and (5) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from columns (4) and (5). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text and data appendix for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 10: Long-term economic well-being in municipalities with largest increases in maternal LFP

	(1)	(2)	(3)	(4)	(5)	(6)
	HS grad	College	Work last year	Log(earn)	Skilled occupation	Age at 1st birth
Panel a: All						
$\widehat{JEC}$	0.015***	0.011*	0.006	0.037*	0.007	0.021
	(0.005)	(0.006)	(0.007)	(0.022)	(0.005)	(0.052)
Observations	76352	52067	50694	50694	32574	14132
DV mean	0.808	0.188	0.646	333396.4	0.308	21.49
Pct change	0.018	0.058	0.009		0.022	0.001
E($\widehat{JEC}$)	2.893	1.972	1.919	1.919	1.789	1.880
Panel b: Women						
$\widehat{JEC}$	0.021***	0.007	0.015	0.051*	0.006	0.021
	(0.006)	(0.006)	(0.009)	(0.029)	(0.010)	(0.052)
Observations	38974	26735	26069	26069	13656	14132
DV mean	0.818	0.206	0.533	244783.8	0.401	21.49
Pct change	0.025	0.032	0.027		0.015	0.001
E($\widehat{JEC}$)	2.880	1.967	1.915	1.915	1.805	1.880
Panel c: Men						
$\widehat{JEC}$	0.009	0.015**	-0.002	0.031	0.007	
	(0.006)	(0.006)	(0.007)	(0.020)	(0.006)	
Observations	37378	25332	24625	24625	18918	
DV mean	0.797	0.168	0.768	428450.2	0.238	
Pct change	0.012	0.089	-0.003		0.030	
E($\widehat{JEC}$)	2.906	1.977	1.922	1.922	1.777	

Table 10: (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	HS grad	College	Work last year	Log(earn)	Skilled occupation	Age at 1st birth
Panel d: Low SES						
$\widehat{JEC}$	0.020***	-0.000	0.016**	0.046***	-0.005	0.092
	(0.006)	(0.005)	(0.007)	(0.017)	(0.008)	(0.095)
Observations	31216	21074	20489	20489	13075	5120
DV mean	0.710	0.0974	0.635	247697.9	0.183	20.90
Pct change	0.028	-0.003	0.024		-0.027	0.004
E($\widehat{JEC}$)	2.702	1.779	1.760	1.760	1.698	1.695
Panel e: High SES						
$\widehat{JEC}$	-0.001	0.010	-0.019**	-0.022	0.007	-0.203**
	(0.005)	(0.009)	(0.008)	(0.027)	(0.012)	(0.083)
Observations	27523	17986	17557	17557	11148	4717
DV mean	0.924	0.299	0.648	430709.1	0.447	21.90
Pct change	0.00	0.03	-0.03		0.02	-0.01
E($\widehat{JEC}$)	3.023	2.026	1.970	1.970	1.763	1.753

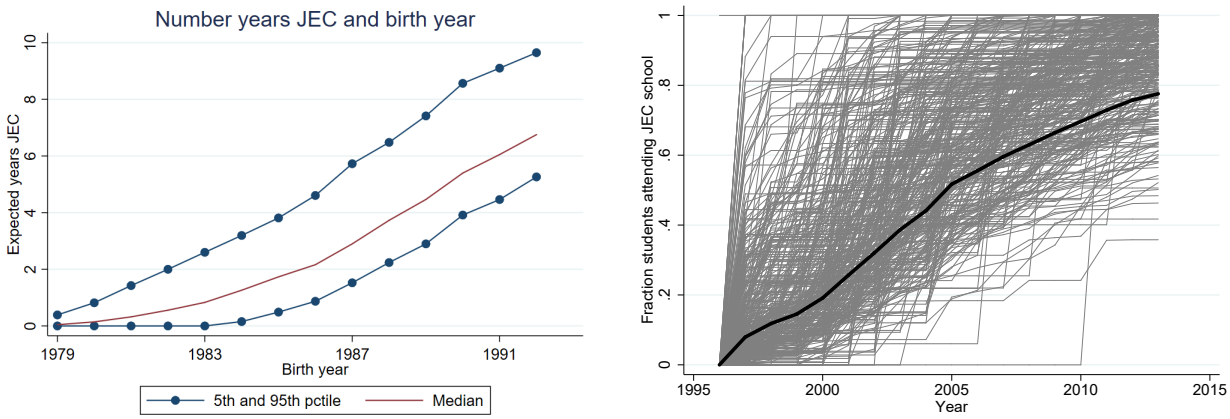
Notes: Dependent variables are as defined in Tables 3 through 9. Sample limited to respondents who were born in a municipality experiencing at least a 6.3 percentage point increase in the maternal labor force participation rate over the first decade of JEC implementation (1996-2006, from a base rate of approximately 38 percent). All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous status, maternal education, as well as survey year linear trends in baseline poverty and employment rates by region of birth from the 1996 CASEN that vary by survey year. Columns (3)-(6) additionally include controls for household size, marital status and number and presence of children, interacted with gender. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were ages 19-38 (column (1)); 22-38 (column (2)); 23-38 (columns (3)-(5)); or who had given birth (column (6)) at the time of survey and Panels (b) and (c) limit the sample to women and men, and panels (d) and (e) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from panels (d) and (e). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text and data appendix for details. *** $= p < 0.01$, ** $= p < 0.05$, * $= p < 0.10$.

Table 11: General equilibrium effects of longer school days on log earnings

	(1)	(2)	(3)	(4)	(5)	(6)
	Young cohorts			Old cohorts		
	< HS	≥ HS	≥ BA	< HS	≥ HS	≥ BA
	Panel a: $\overline{JEC}$ in population					
$\widehat{\overline{JEC}}$	-0.017	0.052	-0.111	0.018	0.026	-0.035
	(0.043)	(0.052)	(0.097)	(0.027)	(0.046)	(0.106)
$\widehat{JEC}$	0.001	0.025**	0.005			
	(0.014)	(0.011)	(0.024)			
Observations	37959	118261	24745	215239	173966	38057
DV mean (level, 1000s pesos)	174.915	355.971	693.866	201.675	529.163	1027.201
E($\widehat{\overline{JEC}}$)	1.834	1.801	1.791	1.820	1.777	1.753
	Panel b: $\overline{JEC}$ in labor force					
$\widehat{\overline{JEC}}$	0.108**	0.184***	0.054	-0.015	-0.053	0.123
	(0.042)	(0.040)	(0.097)	(0.022)	(0.036)	(0.076)
$\widehat{JEC}$	-0.001	0.020*	0.005			
	(0.014)	(0.012)	(0.025)			
Observations	37958	118261	24745	215177	173954	38056
DV mean (level, 1000s pesos)	174.919	355.971	693.866	201.680	529.167	1027.211
E($\widehat{\overline{JEC}}$)	1.074	1.117	1.127	0.968	0.947	0.861

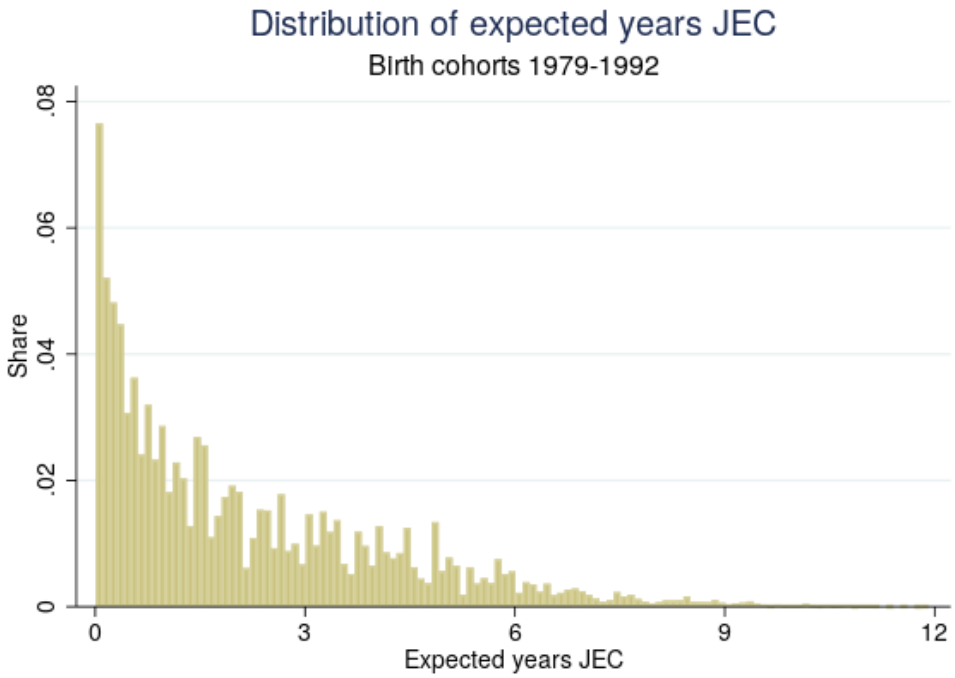
Notes: Dependent variable is the natural log of earnings in the previous month plus one to account for respondents not in the workforce. All specifications include city of birth, city of residence, survey year, and birth year-by-region fixed effects. Control variables include gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. $\widehat{\overline{JEC}}$ defined as the average expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation (1) from enrollment and JEC adoption data from the Ministry of Education for the population ages 18 and older (panel (a)) and the labor force ages 18 and older (panel (b)). "Young" sample (columns (1-3)) limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were ages 23-38 at the time of survey. "Old" sample limited to individuals born between 1955 and 1978 outside the Santiago metropolitan region who were ages 28-60 at the time of the survey. Columns (1) and (4) examine individuals with less than a high school degree; columns (2) and (5) consider those with at least a high school degree; columns (3) and (6) are limited to individuals with at least a four-year university degree. Two-way robust standard errors clustered by city of birth and city of residence. All specifications weighted using regionally-representative weights. See text and data appendix for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Figure 1: JEC timing varied across municipalities



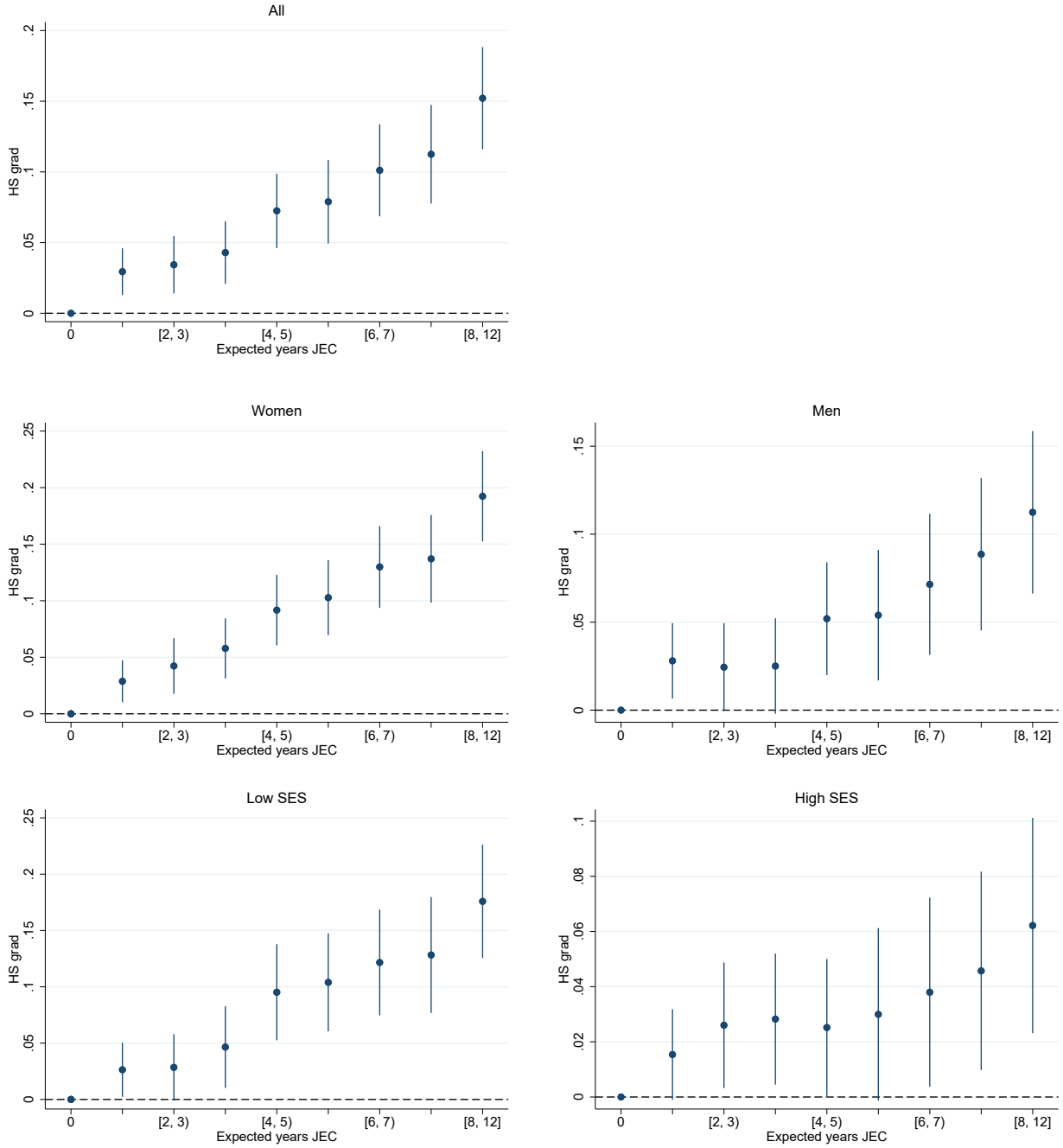
Notes: Figures shows the expected years of attending a full-day school across cities of birth by birth year. Panel (a): The bottom solid blue line denotes the expected years of full-day schooling in grades 1-12 for students at the 5th percentile of their cohort-specific distribution. The top blue line denotes the expected years of full-day schooling for students in the 95th percentile, and the red line shows the median expected years of exposure to the reform. Panel (b): Figure shows the fraction of students in grades 1-12 attending a JEC school in a given year with each line showing the pace of implementation for a single municipality. Each line shows the pace of implementation for a single municipality; bold black line is the national average. Source: Ministry of Education, 2016; CASEN, 2006-2017.

Figure 2: Expected JEC exposure



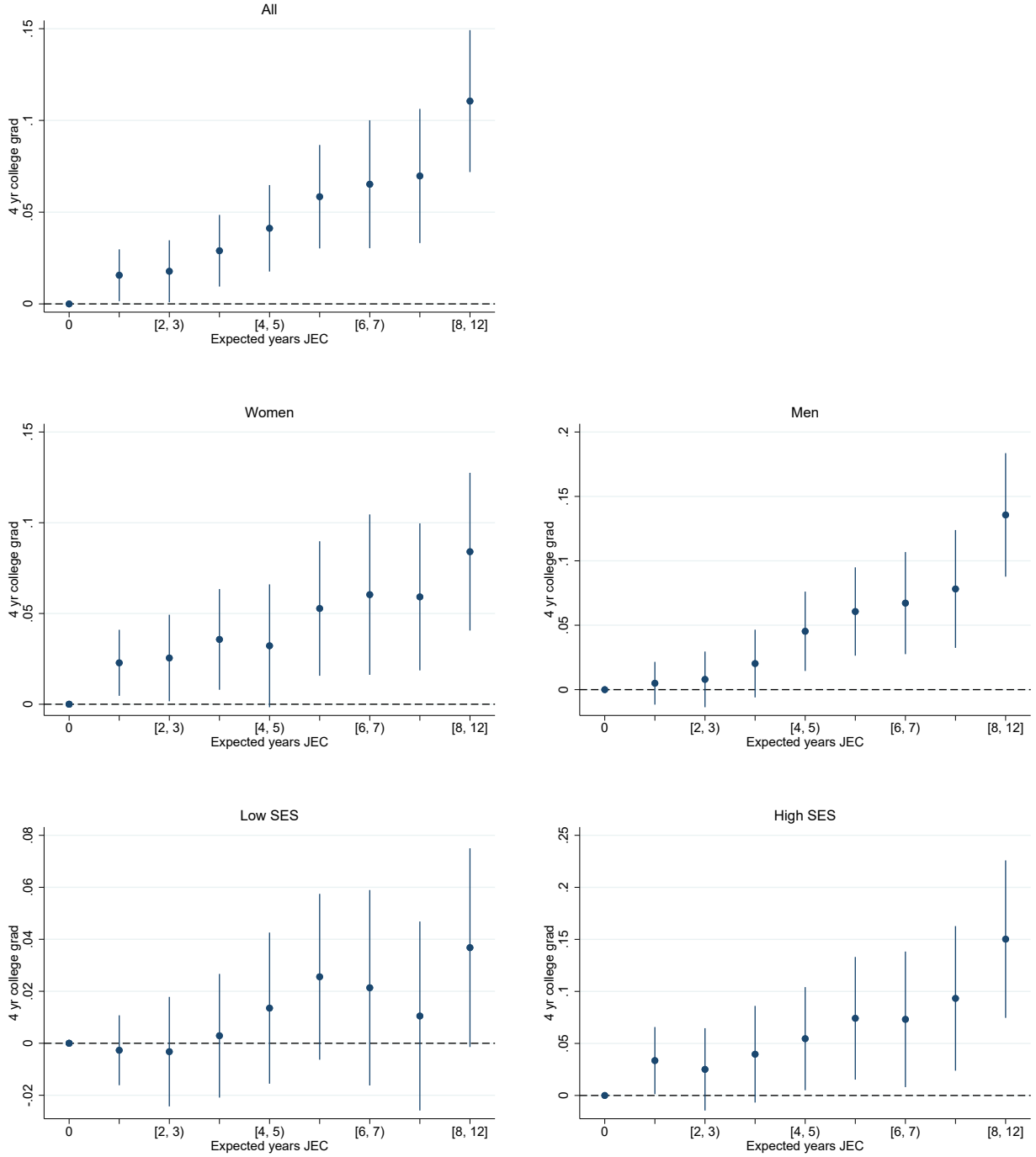
Notes: Figure shows the distribution of expected years of JEC attendance between grades 1-12 for our main sample of individuals born between 1979 and 1992 and who were 23-38 at the time of the CASEN survey and born outside the Santiago metropolitan region. The large mass point at exactly 0 years (11 percent of the sample) is omitted for visualization purposes. Source: Ministry of Education, 2016; CASEN, 2006-2017.

Figure 3: High school graduation by expected years of full-day schooling



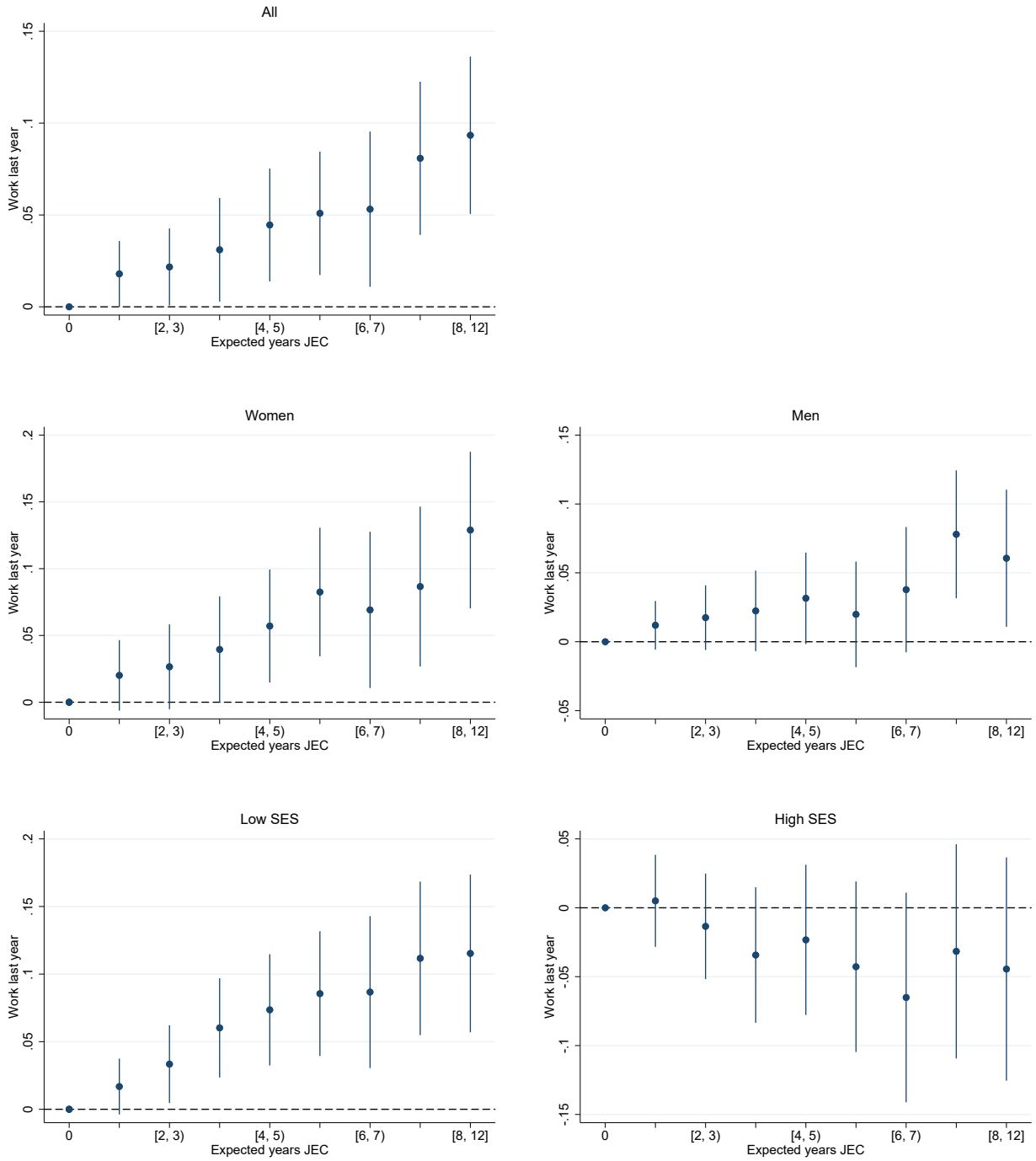
Notes: Figure shows the results from a regression as in Equation 4 where each coefficient is one of nine indicators for 0, (0,1), [1,2)...[4,5), [8, 12] years of $\widehat{JEC}$. Dependent variable is an indicator = 1 if the respondent had completed high school at the time of the CASEN survey. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, and maternal education, as well as survey year linear trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. Vertical lines denote 95 percent confidence intervals clustered by city of birth. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 19-38 years old at the time of survey. See text and data appendix for details.

Figure 4: College graduation by expected years of full-day schooling



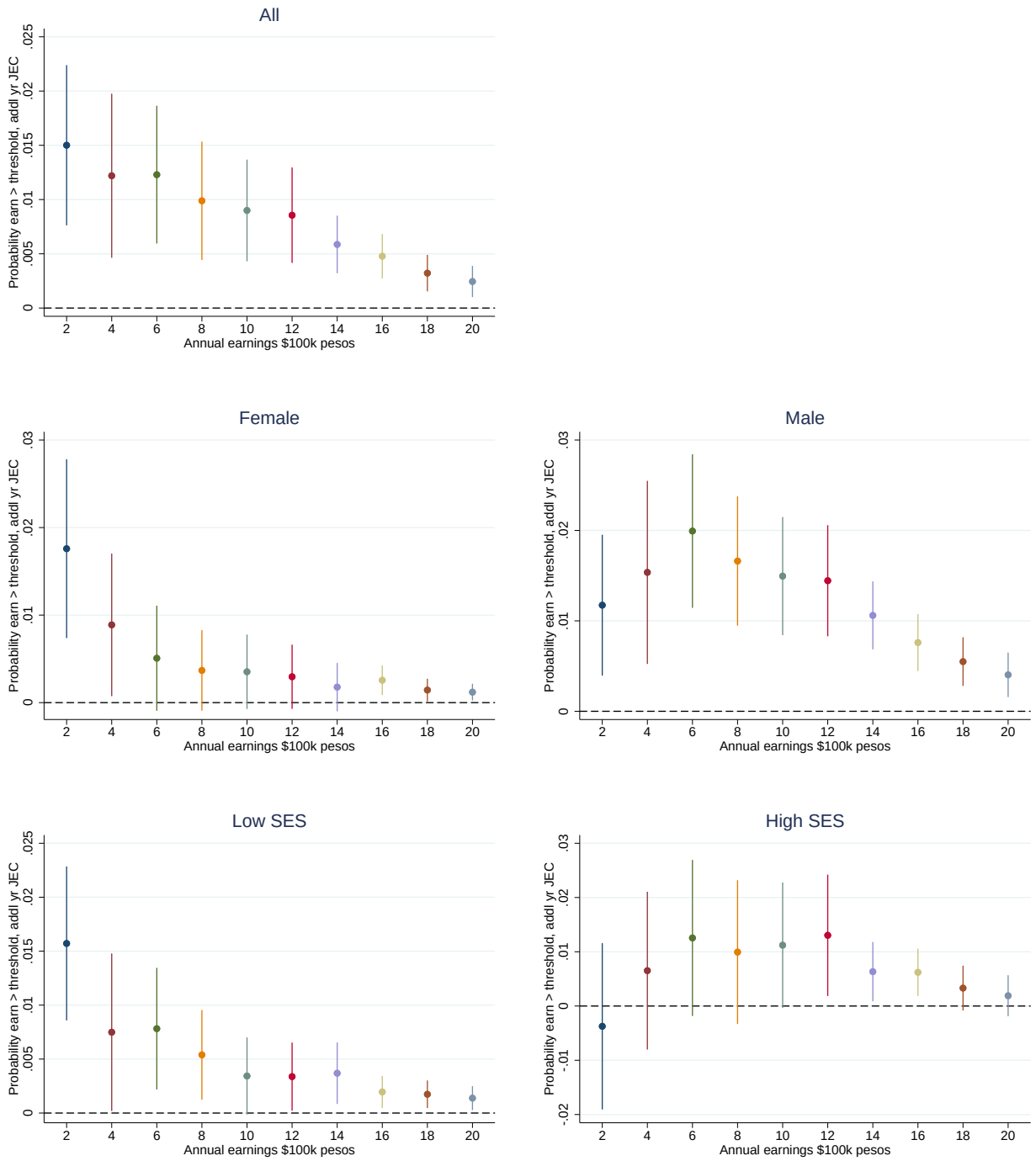
Notes: *Notes*: Figure shows the results from a regression as in Equation 4 where each coefficient is one of nine indicators for 0, (0,1), [1,2)...[4,5), [8, 12] years of $\widehat{JEC}$. Dependent variable is an indicator = 1 if the respondent had received a university degree at the time of the CASEN survey. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, and maternal education, as well as survey year linear trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. Vertical lines denote 95 percent confidence intervals clustered by city of birth. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-38 years old at the time of survey. See text and data appendix for details.

Figure 5: Employment in previous month by expected years of full-day schooling



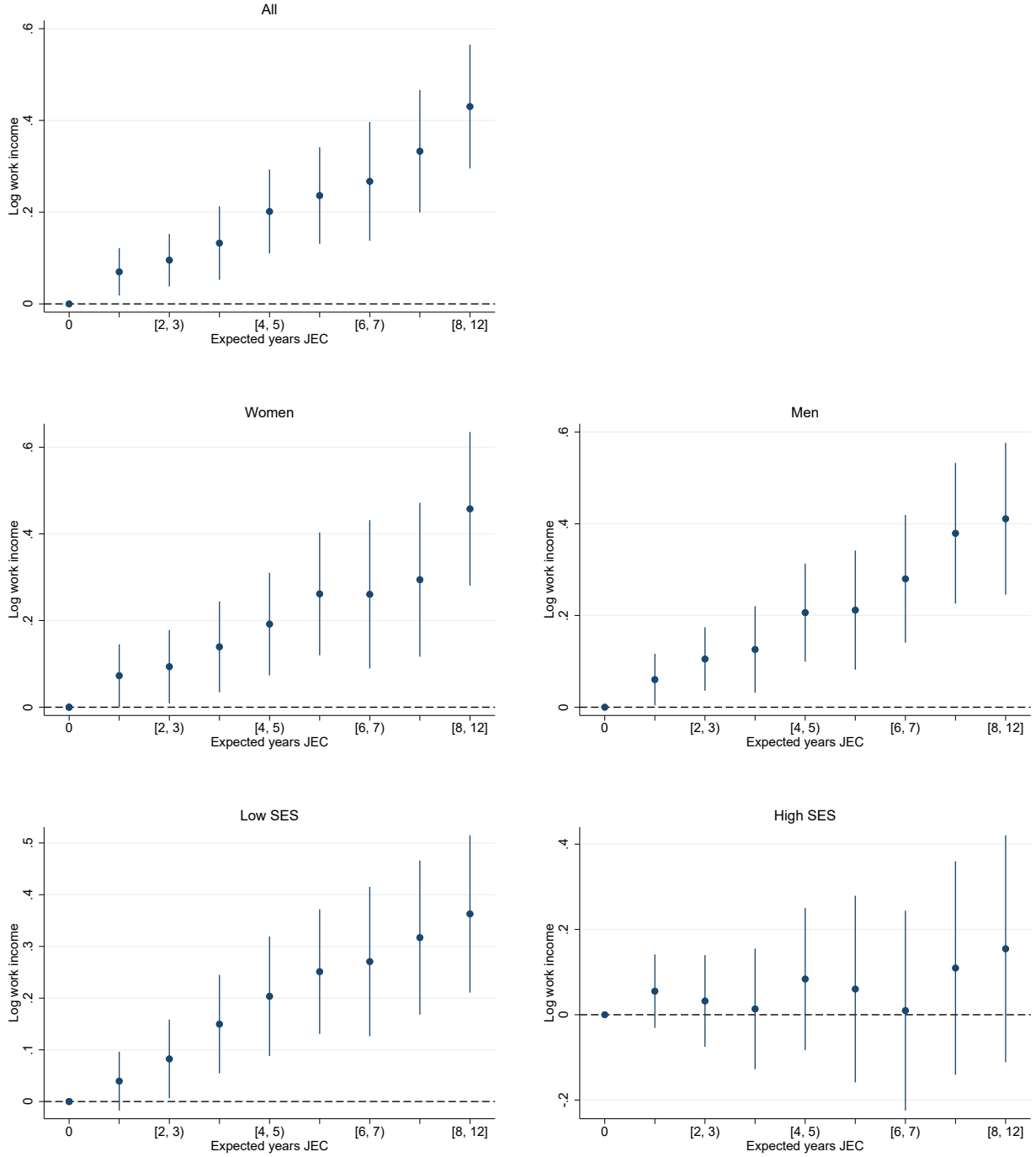
Notes: Figure shows the results from a regression as in Equation 4 where each coefficient is one of nine indicators for 0, (0,1), [1,2)...[4,5), [8, 12] years of $\widehat{JEC}$. Dependent variable is defined as having income at least 30,000 pesos (approximately $50) in the past month. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. Vertical lines denote 95 percent confidence intervals clustered by city of birth. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-38 years old at the time of survey. See text and data appendix for details.

Figure 6: Effects of JEC across the earnings distribution



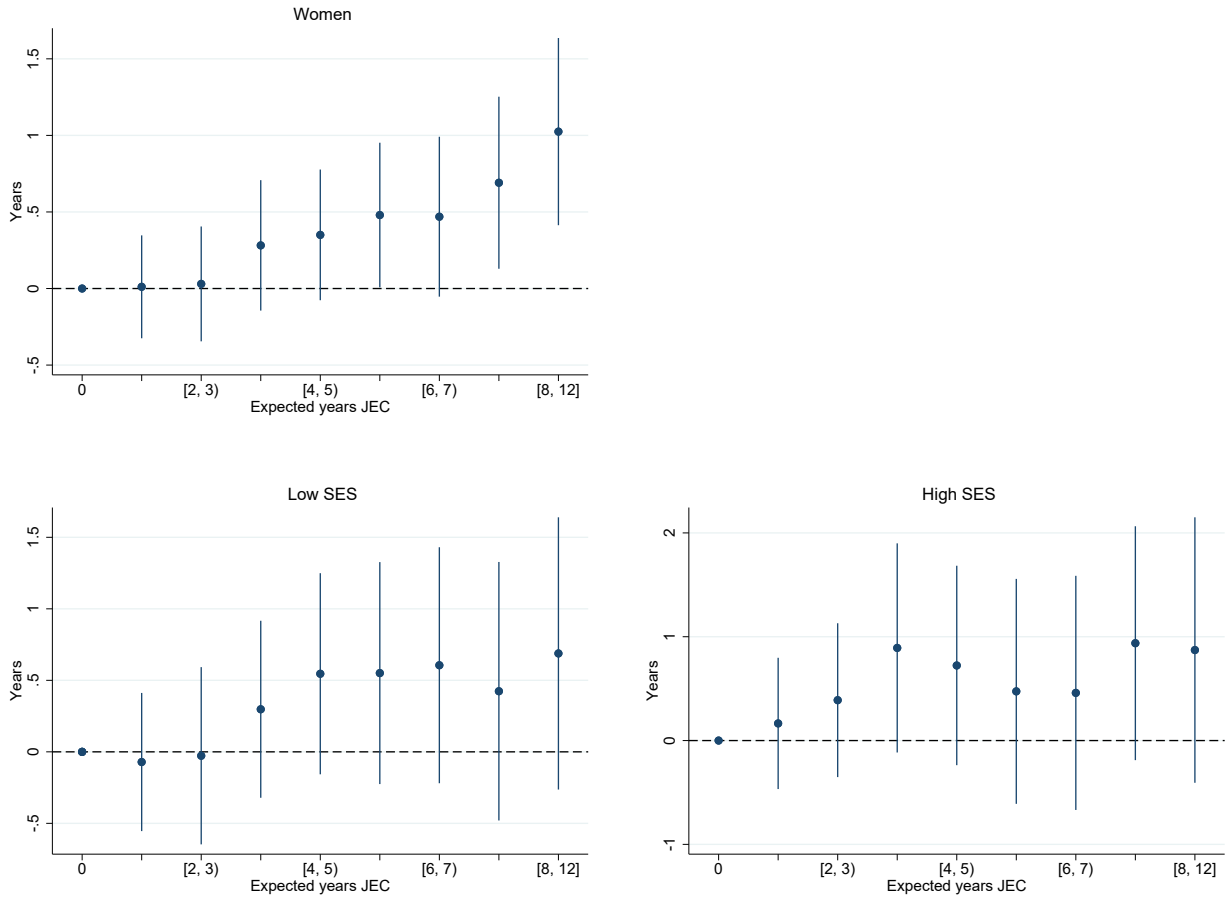
Notes: Figure shows the results from a series of regressions as in Equation 3 where the dependent variable is an indicator for whether monthly earnings were above a given threshold in 100,000s of 2017 pesos. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Vertical lines denote 95 percent confidence intervals clustered by city of birth. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-38 years old at the time of survey. See text and data appendix for details.

Figure 7: Log monthly earnings by expected years of full-day schooling



Notes: Figure shows the results from a regression as in Equation 4 where each coefficient is one of nine indicators for 0, (0,1), [1,2)...[4,5), [8, 12] years of $\widehat{JEC}$. Dependent variable is $log(monthlyearnings + 1)$ (in 2017 pesos) in order to account for individuals with no earnings. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. Vertical lines denote 95 percent confidence intervals clustered by city of birth. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-38 years old at the time of survey. See text and data appendix for details.

Figure 8: Age at first birth



Notes: Figure shows the results from a regression as in Equation 4 where each coefficient is one of nine indicators for 0, (0,1), [1,2)...[4,5), [8, 12] years of $\widehat{JEC}$. Dependent variable is the age in years a woman gave birth to her first child. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, and maternal education, as well as survey year linear trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. Vertical lines denote 95 percent confidence intervals clustered by city of birth. Sample limited to women born between 1979 and 1992 outside the Santiago metropolitan region who had given birth to at least one child at the time of the survey. See text and data appendix for details.

Appendix Tables

Appendix Table A1: Student demographic characteristics and JEC exposure

	(1)	(2)	(3)	(4)
	Female	Indigenous	Mom has $<$ HS	Mom has $\geq$ BA
$\widehat{JEC}$	0.001	-0.001	-0.001	-0.001
	(0.003)	(0.002)	(0.003)	(0.006)
Observations	157698	157698	127853	48642
DV mean	0.517	0.104	0.440	0.212
E($\widehat{JEC}$)	2.020	2.020	1.756	2.086

Notes: Dependent variables are a series of indicators equal to one if a respondent reports a given demographic or socioeconomic characteristic at the time of the CASEN survey. All specifications include city of birth, survey year, and birth year-by-region fixed effects. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation (1) from enrollment and JEC adoption data from the Ministry of Education; demographic characteristics from adults in our sample at the time of the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 19-38 years old at the time of survey. Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text and data appendix for details. *** $= p < 0.01$, ** $= p < 0.05$, * $= p < 0.10$.

Appendix Table A2: JEC rollout pace and timing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Year $\widehat{JEC} \geq 1$		Year $\widehat{JEC} \geq 0.01$		Year $\widehat{JEC} = 12$		Length of implementation	
Pct ages < 18	-0.715	8.820	0.354	7.080	1.796	10.926	1.442	3.846
	(13.171)	(11.023)	(9.244)	(10.619)	(13.238)	(11.260)	(10.173)	(9.700)
Pct ages > 65	-1.348	-17.774	-11.221	-6.583	0.219	-17.862	11.439	-11.279
	(17.765)	(15.914)	(14.061)	(16.802)	(17.847)	(16.023)	(14.855)	(16.073)
Pct in agriculture	0.753	0.995	1.160	0.101	0.615	0.833	-0.545	0.732
	(1.904)	(1.946)	(1.591)	(1.775)	(1.943)	(2.001)	(1.341)	(1.440)
Literacy rate	-0.126	14.959	6.673	7.167	1.014	15.892	-5.659	8.725
	(8.973)	(10.053)	(7.355)	(9.328)	(9.082)	(10.235)	(7.471)	(9.831)
Log pop	0.683	0.436	-0.680*	-0.731*	0.675	0.428	1.354***	1.159***
	(0.403)	(0.380)	(0.275)	(0.316)	(0.406)	(0.387)	(0.311)	(0.298)
Observations	131	131	131	131	131	131	131	131
FE	None	Region	None	Region	None	Region	None	Region
R2	0.065	0.483	0.131	0.269	0.067	0.469	0.323	0.576

Notes: Columns show relationships between baseline characteristics from the 1992 Census and the first year a birth cohort would be expected to have at least 1 year of full-day schooling (columns (1)-(2)); the first year a birth cohort would be expected to have any access to full-day schooling (at least 0.01 years, columns (3)-(4)); the first year a birth cohort would be expected to have any access to full-day schooling throughout its academic career (12 years, columns (5)-(6); the municipality-specific duration of the rollout period (year in columns (5-6) minus year in columns (3-4) in columns (7)-(8)). $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation (1) from enrollment and JEC adoption data from the Ministry of Education; municipality variables from the 1992 Census. Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text and data appendix for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Appendix Table A3: Robustness: Longer school days and high school graduation

	(1)	(2)	(3)	(4)
Panel a: All				
$\widehat{JEC}$	0.020***	0.020***	0.015***	0.015***
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	248535	248535	317260	317260
DV mean	0.794	0.794	0.812	0.812
E($\widehat{JEC}$)	2.925	2.925	2.845	2.845
Panel b: Women				
$\widehat{JEC}$	0.023***	0.024***	0.017***	0.017***
	(0.002)	(0.002)	(0.003)	(0.003)
Observations	126929	126929	161733	161733
DV mean	0.808	0.808	0.825	0.825
E($\widehat{JEC}$)	2.902	2.902	2.823	2.823
Panel c: Men				
$\widehat{JEC}$	0.017***	0.017***	0.013***	0.013***
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	121606	121606	155527	155527
DV mean	0.779	0.779	0.798	0.798
E($\widehat{JEC}$)	2.949	2.949	2.868	2.868
Panel d: Low SES				
$\widehat{JEC}$	0.023***	0.022***	0.021***	0.021***
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	113172	113172	136569	136569
DV mean	0.701	0.701	0.715	0.715
E($\widehat{JEC}$)	2.757	2.757	2.645	2.645
Panel e: High SES				
$\widehat{JEC}$	0.006***	0.008***	0.004*	0.004**
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	79169	79169	109949	109949
DV mean	0.926	0.926	0.928	0.928
E($\widehat{JEC}$)	3.083	3.083	3.002	3.002
Region X cohort FE		X	X	X
Cohort FE	X			
Includes Santiago			X	X
Baseline trends				X

Notes: Dependent variable is an indicator equal to one if the respondent had completed high school at the time of the CASEN survey. All specifications include city of birth and survey year fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, and maternal education, as well as survey year linear trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 who were 19-38 years old at the time of survey. Columns (1-3) omit trends in baseline employment and poverty rates; column (1) additionally replaces region-by-cohort fixed effects with cohort fixed effects. Columns (3-4) include respondents born in the Santiago metropolitan region. Panel (b) and (c) limit the sample to women and men, and panels (d) and (e) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from panels (d-e). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text and data appendix for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Appendix Table A4: Longer school days and college enrollment

	(1) All	(2) Women	(3) Men	(4) Low SES	(5) High SES
$\widehat{JEC}$	0.008***	0.006*	0.009***	0.010***	0.007
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Observations	172681	88972	83709	77796	52510
DV mean	0.450	0.464	0.435	0.305	0.675
Pct change	0.017	0.013	0.021	0.033	0.010
E($\widehat{JEC}$)	1.958	1.944	1.973	1.768	2.041

Notes: Dependent variable is an indicator equal to one if the respondent had attended at least some college at the time of the CASEN survey. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, and maternal education, as well as survey year linear trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 22-38 years old at the time of survey. Columns (2) and (3) limit the sample to women and men, and columns (4) and (5) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from columns (4) and (5). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text and data appendix for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Appendix Table A5: Robustness: Longer school days and college graduation

	(1)	(2)	(3)	(4)
Panel a: All				
$\widehat{JEC}$	0.012***	0.013***	0.007	0.008
	(0.002)	(0.003)	(0.005)	(0.005)
Observations	172681	172681	220786	220786
DV mean	0.182	0.182	0.203	0.203
E($\widehat{JEC}$)	1.958	1.958	1.898	1.898
Panel b: Women				
$\widehat{JEC}$	0.007***	0.008***	0.004	0.005
	(0.003)	(0.003)	(0.006)	(0.006)
Observations	88972	88972	113695	113695
DV mean	0.199	0.199	0.217	0.217
E($\widehat{JEC}$)	1.944	1.944	1.890	1.890
Panel c: Men				
$\widehat{JEC}$	0.016***	0.019***	0.011**	0.011*
	(0.003)	(0.003)	(0.005)	(0.006)
Observations	83709	83709	107091	107091
DV mean	0.164	0.164	0.188	0.188
E($\widehat{JEC}$)	1.973	1.973	1.906	1.906
Panel d: Low SES				
$\widehat{JEC}$	0.003	0.004*	0.003	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	77796	77796	93942	93942
DV mean	0.0992	0.0992	0.105	0.105
E($\widehat{JEC}$)	1.768	1.768	1.679	1.679
Panel e: High SES				
$\widehat{JEC}$	0.015***	0.019***	0.003	0.002
	(0.005)	(0.005)	(0.011)	(0.012)
Observations	52510	52510	73447	73447
DV mean	0.309	0.309	0.327	0.327
E($\widehat{JEC}$)	2.041	2.041	1.986	1.986
Region X cohort FE		X	X	X
Cohort FE	X			
Includes Santiago			X	X
Baseline trends				X

Notes: Dependent variable is an indicator equal to one if the respondent had received a university degree at the time of the CASEN survey. All specifications include city of birth and survey year fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, and maternal education, as well as survey year linear trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-38 years old at the time of survey. Columns (1-3) omit trends in baseline employment and poverty rates; column (1) additionally replaces region-by-cohort fixed effects with cohort fixed effects. Columns (3-4) include respondents born in the Santiago metropolitan region. Panel (b) and (c) limit the sample to women and men, and panels (d) and (e) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from panels (d-e). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text and data appendix for details. *** $= p < 0.01$, ** $= p < 0.05$, * $= p < 0.10$.

Appendix Table A6: Robustness: Longer school days and employment in the previous month

	(1)	(2)	(3)	(4)
	Panel a: All			
$\widehat{JEC}$	0.010***	0.009***	0.006*	0.006*
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	157696	157696	201983	201983
DV mean	0.655	0.655	0.675	0.675
E($\widehat{JEC}$)	2.021	2.021	1.947	1.947
	Panel b: Women			
$\widehat{JEC}$	0.012***	0.013***	0.008*	0.008*
	(0.004)	(0.004)	(0.005)	(0.005)
Observations	81210	81210	103958	103958
DV mean	0.542	0.542	0.576	0.576
E($\widehat{JEC}$)	2.008	2.008	1.945	1.945
	Panel c: Men			
$\widehat{JEC}$	0.008**	0.005*	0.003	0.003
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	76486	76486	98025	98025
DV mean	0.776	0.776	0.780	0.780
E($\widehat{JEC}$)	2.034	2.034	1.949	1.949
	Panel d: Low SES			
$\widehat{JEC}$	0.013***	0.011***	0.006*	0.006
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	70419	70419	85113	85113
DV mean	0.649	0.649	0.667	0.667
E($\widehat{JEC}$)	1.872	1.872	1.766	1.766
	Panel e: High SES			
$\widehat{JEC}$	-0.006	-0.008	-0.009**	-0.009**
	(0.006)	(0.005)	(0.005)	(0.005)
Observations	48641	48641	68129	68129
DV mean	0.651	0.651	0.676	0.676
E($\widehat{JEC}$)	2.086	2.086	2.020	2.020
Region X cohort FE		X	X	X
Cohort FE	X			
Includes Santiago			X	X
Baseline trends				X

Notes: Employment defined as having income at least 30,000 pesos (approximately $50) in the past month. All specifications include city of birth and survey year fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 who were 23-38 years old at the time of survey. Columns (1-3) omit trends in baseline employment and poverty rates; column (1) additionally replaces region-by-cohort fixed effects with cohort fixed effects. Columns (3-4) include respondents born in the Santiago metropolitan region. Panel (b) and (c) limit the sample to women and men, and panels (d) and (e) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from panels (d-e). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text and data appendix for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Appendix Table A7: Robustness: Longer school days and log monthly earnings

	(1)	(2)	(3)	(4)	(5)	(6)
	IHS(earn)	Earnings (level)	Log(earn)	Log(earn)	Log(earn)	Log(earn)
Panel a: All						
$\widehat{JEC}$	0.141***	20888.202***	0.048***	0.047***	0.029**	0.029**
	(0.037)	(3426.251)	(0.010)	(0.009)	(0.014)	(0.014)
Observations	157696	157696	157696	157696	201983	201983
DV mean (level, 1000s pesos)	318.596	318.596	318.596	318.596	352.110	352.110
E($\widehat{JEC}$)	2.021	2.021	2.021	2.021	1.947	1.947
Panel b: Women						
$\widehat{JEC}$	0.180***	15326.417***	0.044***	0.048***	0.030*	0.030*
	(0.052)	(3755.015)	(0.012)	(0.011)	(0.016)	(0.016)
Observations	81210	81210	81210	81210	103958	103958
DV mean (level, 1000s pesos)	233.708	233.708	233.708	233.708	267.879	267.879
E($\widehat{JEC}$)	2.008	2.008	2.008	2.008	1.945	1.945
Panel c: Men						
$\widehat{JEC}$	0.108***	27722.169***	0.051***	0.046***	0.029**	0.029*
	(0.041)	(4501.885)	(0.011)	(0.010)	(0.014)	(0.015)
Observations	76486	76486	76486	76486	98025	98025
DV mean (level, 1000s pesos)	409.536	409.536	409.536	409.536	441.759	441.759
E($\widehat{JEC}$)	2.034	2.034	2.034	2.034	1.949	1.949
Panel d: Low SES						
$\widehat{JEC}$	0.141***	12544.780***	0.044***	0.038***	0.023**	0.023**
	(0.045)	(2789.717)	(0.009)	(0.009)	(0.009)	(0.009)
Observations	70419	70419	70419	70419	85113	85113
DV mean (level, 1000s pesos)	246.378	246.378	246.378	246.378	260.551	260.551
E($\widehat{JEC}$)	1.872	1.872	1.872	1.872	1.766	1.766
Panel e: High SES						
$\widehat{JEC}$	-0.063	15757.256*	0.010	0.009	-0.014	-0.015
	(0.069)	(8789.232)	(0.020)	(0.018)	(0.023)	(0.024)
Observations	48641	48641	48641	48641	68129	68129
DV mean (level, 1000s pesos)	414.115	414.115	414.115	414.115	455.890	455.890
E($\widehat{JEC}$)	2.086	2.086	2.086	2.086	2.020	2.020
Region X cohort FE	X	X		X	X	X
Cohort FE			X			
Includes Santiago					X	X
Baseline trends	X	X				X

Notes: Column (1) transforms real earnings by the inverse hyperbolic sine; column (2) reports income in levels; and columns (3-6) report $log(earnings + 1)$. All specifications include city of birth and survey year fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 who were 23-38 years old at the time of survey. Columns (1-2) include region-by-cohort fixed effects and survey year trends in baseline municipal employment and poverty rates. Columns (3-5) omit trends in baseline employment and poverty rates; column (3) replaces region-by-cohort fixed effects with cohort fixed effects. Columns (5-6) include respondents born in the Santiago region. Panel (b) and (c) limit the sample to women and men, and panels (d) and (e) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from panels (d-e). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text and data appendix for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Appendix Table A8: Robustness: Longer school days and log monthly earnings among workers

	(1)	(2)	(3)	(4)
Panel a: All				
$\widehat{JEC}$	0.034***	0.036***	0.022**	0.021**
	(0.006)	(0.006)	(0.009)	(0.009)
Observations	101839	101839	132667	132667
DV mean (level, 1000s pesos)	486.183	486.183	521.441	521.441
E($\widehat{JEC}$)	1.904	1.904	1.829	1.829
Panel b: Women				
$\widehat{JEC}$	0.031***	0.032***	0.017*	0.016*
	(0.008)	(0.008)	(0.009)	(0.009)
Observations	42245	42245	56082	56082
DV mean (level, 1000s pesos)	430.530	430.530	464.728	464.728
E($\widehat{JEC}$)	1.910	1.910	1.846	1.846
Panel c: Men				
$\widehat{JEC}$	0.036***	0.037***	0.024**	0.024**
	(0.006)	(0.006)	(0.010)	(0.010)
Observations	59594	59594	76585	76585
DV mean (level, 1000s pesos)	527.866	527.866	565.991	565.991
E($\widehat{JEC}$)	1.900	1.900	1.816	1.816
Panel d: Low SES				
$\widehat{JEC}$	0.021***	0.019***	0.015***	0.015**
	(0.006)	(0.006)	(0.006)	(0.006)
Observations	44852	44852	54941	54941
DV mean (level, 1000s pesos)	379.556	379.556	390.543	390.543
E($\widehat{JEC}$)	1.824	1.824	1.707	1.707
Panel e: High SES				
$\widehat{JEC}$	0.041***	0.043***	0.017	0.017
	(0.012)	(0.013)	(0.017)	(0.017)
Observations	31351	31351	45011	45011
DV mean (level, 1000s pesos)	635.665	635.665	673.798	673.798
E($\widehat{JEC}$)	1.883	1.883	1.834	1.834
Region X cohort FE		X	X	X
Cohort FE	X			
Includes Santiago			X	X
Baseline trends				X

Notes: Dependent variable is defined as $log(earnings + 1)$ (in 2017 pesos). All specifications include city of birth and survey year fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 who were 23-38 years old at the time of survey and who report earned income of at least 30,000 pesos in the past month ($\approx$ \$50). Columns (1-3) omit trends in baseline employment and poverty rates; column (1) additionally replaces region-by-cohort fixed effects with cohort fixed effects. Columns (3-4) include respondents born in the Santiago metropolitan region. Panel (b) and (c) limit the sample to women and men, and panels (d) and (e) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from panels (d-e). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text and data appendix for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Appendix Table A9: Robustness: Longer school days and domestic migration

	(1)	(2)	(3)	(4)	(5)	(6)
	Moved to Santiago	Log(Avg city residence income)	Moved	Moved	Moved	Moved
Panel a: All						
$\widehat{JEC}$	-0.001	0.015***	0.001	0.000	0.000	-0.001
	(0.002)	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	157696	157696	157696	157696	201983	201983
DV mean	0.110	0.0822	0.357	0.357	0.418	0.418
E($\widehat{JEC}$)	2.021	2.021	2.021	2.021	1.947	1.947
Panel b: Women						
$\widehat{JEC}$	0.000	0.017***	0.001	0.001	0.001	0.000
	(0.003)	(0.006)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	81210	81210	81210	81210	103958	103958
DV mean	0.112	0.0811	0.365	0.365	0.428	0.428
E($\widehat{JEC}$)	2.008	2.008	2.008	2.008	1.945	1.945
Panel c: Men						
$\widehat{JEC}$	-0.003	0.013**	0.001	-0.000	-0.001	-0.002
	(0.003)	(0.006)	(0.003)	(0.003)	(0.005)	(0.004)
Observations	76486	76486	76486	76486	98025	98025
DV mean	0.108	0.0833	0.349	0.349	0.408	0.408
E($\widehat{JEC}$)	2.034	2.034	2.034	2.034	1.949	1.949
Panel d: Low SES						
$\widehat{JEC}$	0.002	0.007*	0.003	0.001	-0.006	-0.007*
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	70419	70419	70419	70419	85113	85113
DV mean	0.0746	-0.0448	0.295	0.295	0.348	0.348
E($\widehat{JEC}$)	1.872	1.872	1.872	1.872	1.766	1.766
Panel e: High SES						
$\widehat{JEC}$	-0.002	0.025***	0.001	0.003	0.004	0.002
	(0.003)	(0.008)	(0.005)	(0.005)	(0.006)	(0.006)
Observations	48641	48641	48641	48641	68129	68129
DV mean	0.142	0.235	0.410	0.410	0.477	0.477
E($\widehat{JEC}$)	2.086	2.086	2.086	2.086	2.020	2.020
Region X cohort FE	X	X		X	X	X
Cohort FE			X			
Includes Santiago					X	X
Baseline trends	X	X				X

Notes: Dependent variable is an indicator = 1 if the respondent currently lives in a municipality in the Santiago region (col (1)); a standardized index of municipality per-capita income multiplied by whether the individual lives in a city other than his/her city of birth (col (2)); or an indicator = 1 whether the respondent currently lives in a city other than his/her city of birth (col (3-6)). All specifications include city of birth and survey year fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 who were 23-38 years old at the time of survey. Col (1-2) include region-by-cohort fixed effects and survey year trends in baseline municipal employment and poverty rates. Col (3-5) omit trends in baseline employment and poverty rates; col (3) replaces region-by-cohort fixed effects with cohort fixed effects. Col (5-6) include respondents born in the Santiago region. Panel (b) and (c) limit the sample to women and men, and panels (d) and (e) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from panels (d-e). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text and data appendix for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Appendix Table A10: Robustness: Longer school days and childbearing patterns

	(1)	(2)	(3)	(4)	(5)
	Pr(teen mother)		Age at first birth		
Panel a: All Women					
$\widehat{JEC}$	-0.007***	0.198***	0.195***	0.107***	0.110***
	(0.002)	(0.042)	(0.047)	(0.039)	(0.038)
Observations	95874	28036	28036	67430	67430
DV mean	0.113	21.02	21.02	21.18	21.18
E($\widehat{JEC}$)	3.332	2.797	2.797	2.391	2.391
Panel b: Low SES					
$\widehat{JEC}$	-0.005*	0.180***	0.199***	0.062	0.062
	(0.003)	(0.050)	(0.058)	(0.046)	(0.046)
Observations	37940	13173	13173	25511	25511
DV mean	0.130	20.72	20.72	20.66	20.66
E($\widehat{JEC}$)	3.159	2.607	2.607	2.066	2.066
Panel c: High SES					
$\widehat{JEC}$	0.001	0.094	0.097	0.061	0.071
	(0.003)	(0.084)	(0.089)	(0.087)	(0.084)
Observations	33378	9964	9964	21220	21220
DV mean	0.0808	21.50	21.50	21.58	21.58
E($\widehat{JEC}$)	3.471	2.892	2.892	2.263	2.263
Region X cohort FE	X		X	X	X
Cohort FE		X			
Includes Santiago				X	X
Baseline trends	X				X

Notes: Dependent variable column (1) is an indicator equal to one if a woman gave birth before age 19 or (columns (2)-(5)) is the age in years a woman gave birth to her first child. All specifications include city of birth and survey year fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, and maternal education, as well as survey year linear trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to women born between 1979 and 1992 who had given birth to at least one child at the time of the survey. Columns (2-4) omit trends in baseline employment and poverty rates; column (2) additionally replaces region-by-cohort fixed effects with cohort fixed effects. Columns (4-5) include respondents born in the Santiago metropolitan region. Panel (b) limits the sample to women whose mothers had less than a high school education; panel (c) limits the sample to women whose mothers had at least a high school education. Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text and data appendix for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Appendix Table A11: Robustness: Longer school days and occupational upskilling

	(1)	(2)	(3)	(4)	(5)
	Log(avg occ wage)		Skilled occupation		
Panel a: All					
$\widehat{JEC}$	0.013***	0.010***	0.010***	0.007	0.006
	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)
Observations	96211	101209	101209	131792	131792
DV mean	487.765	0.292	0.292	0.325	0.325
E($\widehat{JEC}$)	1.976	1.894	1.894	1.822	1.822
Panel b: Women					
$\widehat{JEC}$	0.012**	0.011*	0.010*	0.008	0.008
	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)
Observations	40175	42220	42220	56060	56060
DV mean	473.531	0.379	0.379	0.391	0.391
E($\widehat{JEC}$)	1.975	1.899	1.899	1.839	1.839
Panel c: Men					
$\widehat{JEC}$	0.012***	0.009***	0.010***	0.006	0.005
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Observations	56036	58989	58989	75732	75732
DV mean	498.647	0.226	0.226	0.273	0.273
E($\widehat{JEC}$)	1.976	1.890	1.890	1.808	1.808
Panel d: Low SES					
$\widehat{JEC}$	-0.005	0.001	-0.000	-0.001	-0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	41867	44828	44828	54882	54882
DV mean	417.984	0.179	0.179	0.195	0.195
E($\widehat{JEC}$)	1.909	1.812	1.812	1.703	1.703
Panel e: High SES					
$\widehat{JEC}$	0.029***	0.015**	0.017**	0.009	0.008
	(0.007)	(0.006)	(0.007)	(0.008)	(0.008)
Observations	29671	30829	30829	44296	44296
DV mean	597.607	0.456	0.456	0.484	0.484
E($\widehat{JEC}$)	1.949	1.874	1.874	1.823	1.823
Region X cohort FE	X		X	X	X
Cohort FE		X			
Includes Santiago				X	X
Baseline trends	X				X

Notes: Dependent variable is the average wage in a 4-digit occupation (col (1)) or an indicator = 1 if the respondent is employed in a managerial, technical, or professional occupation (col (2-5)). All specifications include city of birth and survey year fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979-1992 who were 23-38 years old at the time of survey. Military members and respondents without valid occupation codes are excluded. Col (1) includes region-by-cohort fixed effects and survey year trends in baseline municipal employment and poverty rates. Col (2-4) omit trends in baseline employment and poverty rates; col (2) replaces region-by-cohort fixed effects with cohort fixed effects. Col (4-5) include respondents born in the Santiago region. Panel (b) and (c) limit the sample to women and men, and panels (d) and (e) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from panels (d-e). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text and data appendix for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Appendix Table A12: Placebo results: Effect of longer school days on untreated cohorts

	(1) HS grad	(2) College	(3) Log(earn)	(4) Skilled occupation	(5) Age at 1st birth
			Panel a: All		
$\widehat{JEC}$	0.006*	0.000	-0.003	-0.001	
	(0.003)	(0.001)	(0.006)	(0.002)	
Observations	166367	166367	166787	113608	
DV mean	0.497	0.112	371760.6	0.195	
Pct change	0.011	0.004		-0.004	
			Panel b: Women		
$\widehat{JEC}$	-0.000	-0.001	-0.008	0.003	-0.049
	(0.004)	(0.002)	(0.009)	(0.004)	(0.062)
Observations	88249	88249	88468	44673	51234
DV mean	0.501	0.108	219183.2	0.231	23.46
Pct change	0.000	-0.005		0.013	
			Panel c: Men		
$\widehat{JEC}$	0.012***	0.002	-0.001	-0.004	
	(0.004)	(0.002)	(0.007)	(0.003)	
Observations	78118	78118	78319	68935	
DV mean	0.492	0.118	550993.8	0.169	
Pct change	0.024	0.014		-0.022	
			Panel d: Low SES		
$\widehat{JEC}$	0.004	0.001	-0.008	0.002	-0.054
	(0.004)	(0.002)	(0.007)	(0.003)	(0.076)
Observations	91927	91927	92075	61747	27188
DV mean	0.438	0.0730	309771.4	0.145	23.09
Pct change	0.009	0.018		0.015	
			Panel e: High SES		
$\widehat{JEC}$	0.001	0.000	0.019	-0.018**	-0.371
	(0.006)	(0.006)	(0.022)	(0.008)	(0.230)
Observations	22217	22217	22263	16851	7872
DV mean	0.823	0.331	708507.9	0.440	24.67
Pct change	0.002	0.001		-0.040	

Notes: Dependent variables are defined as in Tables 3 through 9. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous status, maternal education, as well as survey year trends in baseline municipal employment and poverty rates. Columns (3)-(6) additionally include controls for household size, marital status and number and presence of children, interacted with gender. Placebo $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city of birth and the access to JEC for individuals born 20 years later in the same municipality, calculated as described in Equation (1) from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1959 and 1972 outside the Santiago metropolitan region who were 43-58 years old at the time of survey. Panel (b) and (c) limit the sample to women and men, and panels (d) and (e) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from panels (d-e). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text and data appendix for details. *** $= p < 0.01$, ** $= p < 0.05$, * $= p < 0.10$.

Data Appendix

Here we summarize the CASEN-JEC matching approach and the definition of variables used in the CASEN.

Measuring JEC exposure

The Chilean Ministry of Education reports enrollment at the school-grade level for all publicly-funded schools (public and voucher schools) beginning with the 2013 school year, as well as the year each school-grade adopted JEC. We first exclude schools reporting zero or one student in a particular grade and then estimate JEC access as the enrollment-weighted average of JEC implementation at the municipality-grade-year level for each year 1997 through 2015.

For cohorts born between 1979 and 1992, we then sum across the years a student would be expected to enroll in grades 1-12, assuming no grade retention or drop-out behavior. For example, the 1990 cohort is expected to enroll in grade 1 in 1996, prior to JEC implementation. Beginning in 1997, some schools had implemented JEC for second graders, and so on, through 2008 when respondents born in this cohort were expected to have completed secondary schooling. More formally, we calculate exposure to JEC as in Equation 1 as:

$$(\widehat{JEC_{cm}}) = \frac{1}{N_{cm}} \sum_{s \in m} \sum_{g=1}^{12} \mathbb{1} \{JEC_{scgm}\} * N_{sgcm}$$

As we note in the text, the labor market data includes information on respondents' birth years, but not birth months. The Chilean school year begins in March, and children who turn five through June are eligible to enroll that year. Accordingly, each starting class is approximately half five year olds and half six year-olds. (McEwan and Shapiro (2008) provide a full description on Chilean enrollment cutoffs). We define age in first grade based on a child's year of birth plus six; accordingly, for children born in January through June, our approach assigns them the JEC exposure of an younger cohort (e.g.: weakly greater years of full-day schooling than they actually had access to). This approach errs on the side of

under-estimating JEC exposure, thereby providing a lower-bound on the actual exposure effect.

Outcome and control variables

Similar to other household surveys, the CASEN includes a rich set of demographic and economic characteristics at the household and individual level. Here, we briefly summarize the key variables; a comprehensive data description is available from the Ministry of Finance, available online at `https://www.hacienda.cl/english/documents/statistics/casen-survey.html`.

Sample selection Our main results are limited to respondents born between 1979 and 1992 (graduating high school between 1997 (the first year of JEC) and 2010 (the year by which all schools were originally required to have adopted)). All of these individuals were born before the policy was announced; therefore, city of birth is unlikely to be correlated with access to full-day schooling, conditional on region-specific time effects and time-invariant municipality effects. We estimate all results in a weighted least squares generalized differences-in-differences framework (Equation 3), using regionally-representative weights in order to provide the most comprehensive coverage of the population.[32]

Individual characteristics

- **City of birth:** Starting with the 2006 survey, the CASEN data includes information on where respondents' mothers were living at birth, whether the current city of residence or a different city (and if the latter, which city). With this information, we identify the city of birth for approximately 98 percent of respondents. The unmatched observations result from respondents reporting mother's residence at birth at a higher level of aggregation than the municipality (e.g.: the region or the province). We do

[32]In results available upon request, our findings are qualitatively unchanged when using municipal- ("comuna") level weights or without weighting.

not include earlier CASEN waves (2000, 2003) in our analyses as we are not able to identify city of birth for respondents in these years.

- **Socioeconomic status:** We proxy for the economic conditions a child experienced while growing up by maternal education. Our "Low SES" sample includes respondents whose mothers have no more than 12 years of education (approximately 37 percent of the sample) education; the "High SES" sample includes respondents whose mothers have at least a high school education (approximately 39 percent of the sample). We construct this variable first using the direct question about mother's educational attainment from the CASEN ("Cuál fue el nivel de educacion más alto alcanzado por su madre –o figura materna?" About 55 percent respond to this question, for an additional 20 percent of individuals, we obtain maternal educational attainment based on the education level of a female head of household who have a child who is (a) in our sample and (b) in the same household). For both approaches, the share of respondents with non-missing values is lower in the 2017 wave than in previous surveys. Our results do not meaningfully change with the exclusion of the 2017 wave (available upon request).

- **Employment status:** Employment status is defined as having earnings from employment at least 30,000 pesos (approximately $50) in the last month. Results are similar when using self-reported employment status in the previous week.

- **Earnings:** Earnings are defined as self-reported earnings from employment in the previous month. We adjust all measures for inflation using the Consumer Price Index, available at `https://fred.stlouisfed.org/series/CHLCPIALLMINMEI`, and report all amounts in 2017 pesos. For results estimating the semi-elasticity of earnings with respect to an additional year of full-day schooling access (Table 6, Figure 7, and Appendix Tables A7 and A8), we define log monthly earnings as $log(earnings+1)$ in order to account for respondents without earned income. Appendix Table A7 additionally reports results where the dependent variable is the inverse hyperbolic sine of monthly

earnings (column (1)) or monthly earnings in pesos (column (2)).

- **Autonomous income:** In Table 2, we explore the relationship between the pace of JEC adoption and average per-capita autonomous income. Autonomous income defined as income from all household sources, primarily earnings, but also rental income.

- **Skilled occupation:** Following ILO, we define skilled occupations as having a primary occupation as a manager, professional, or technician/associate professional (ISCO major codes 1, 2, 3), as reported in the CASEN. Individuals in the armed forces, out of the labor force, and without a valid occupation are considered neither skilled nor unskilled.

- **Occupational prestige:** For each individual, we measure the log earnings of other workers j in the same 4-digit occupation o as occupation-average wages, excluding own earnings:

$$\overline{w_{io}} = \frac{\sum_j w_{j \neq i}}{\sum_j N_{jo} - 1} \tag{8}$$

  Military members and those without a valid occupation code are excluded from this measure. By this definition, a value of 1,000 indicates *expected* earnings of an additional 1,000 pesos based on peer earnings.

- **Area socioeconomic opportunity:** We calculate the per-capita autonomous income (earnings, rent and other sources) as the leave-out mean among individuals j other than survey respondent i in a city-survey year as:

$$\overline{y_{io}} = \frac{\sum_j y_{j \neq i}}{\sum_j N_{jo} - 1} \tag{9}$$

  And normalize the measure to have a mean of 0 and standard deviation of one across all municipalities and survey years so that a valuesof 1 indicates an area that has per-capita income 1 standard deviation higher than the average municipality. We multiply this

index by an indicator equal to 1 if an individual moved in order to capture migration to higher-income areas.

City and region characteristics

- **Employment rate baseline trends:** Baseline employment rate trends are calculated as survey year linear trends in the municipality of birth baseline share of employed adults, as reported in the 1996 CASEN. Municipalities with no baseline information (approximately 25 percent of the sample) have a separate trend on an indicator for whether baseline employment is missing.

- **Poverty rate baseline trends:** Baseline poverty rate trends are calculated as survey year linear trends in the municipality of birth baseline poverty rate, as reported in the 1996 CASEN. Poverty is calculated as an absolute measure as 1.75 (some rural areas) or 2 (urban and other rural areas) times the cost of a basic food basket. The food bundle cost is estimated using data from the Family Budget Survey. Municipalities with no baseline information (approximately 25 percent of the sample) have a separate trend on an indicator for whether baseline poverty is missing.

- **Contemporaneous employment rate:** Contemporaneous employment rate for each individual's current city of residence is calculated as the share of adults working in the previous month from the full CASEN sample. Our main specifications include this variable as a control.

- **Contemporaneous poverty rate:** Contemporaneous poverty rate for each individual's current city of residence is calculated as the share of invididuals working living in poverty the full CASEN sample. Our main specifications include this variable as a control. Poverty is calculated as an absolute measure as 1.75 (some rural areas) or 2 (urban and other rural areas) times the cost of a basic food basket. The food bundle cost is estimated using data from the Family Budget Survey.

- **Contemporaneous per-capita income:** Contemporaneous per-capita income for

each individual's current city of residence is calculated as per-person autonomous income (earnings and rental income) among the full CASEN sample. Our main specifications include this variable as a control.

- **Census variables (Appendix Table A2):** We examine how the pace and timing of JEC implementation is associated with other baseline municipal characteristics using data from the 1992 Census (available at IPUMS). These variables include the age structure of the municipal population (younger than 18, 65 and older, log population); share of the workforce in agriculture, and the literacy rate among adults ages 18 and older. As the Census is a complete count of individuals, it is representative at a finer level of geography than the CASEN survey. Even with this broader scope, information for the most rural areas is not available in the public-use files.

Data Appendix Table DA1 presents summary statistics for the city-level variables from the CASEN (panel (a)) and Census (panel (b)). Between one-quarter (employment rate) and two-thirds (poverty rate) of the cross-city variation in each control variable is between regions.

Data Appendix Table DA1: Summary statistics: City-level characteristics

	(1)
Panel (a): CASEN variables	
Employment rate	0.912
	(0.044)
Yrs education	8.260
	(1.334)
Avg hh size	3.860
	(0.296)
Poverty rate	0.257
	(0.117)
Log population	6.530
	(0.485)
Panel (b): Census variables	
Pct < 18	0.363
	(0.019)
Pct > 65	0.070
	(0.014)
Pct in ag	0.375
	(0.205)
Literacy rate	0.863
	(0.043)
Log pop	8.358
	(0.700)

Notes: Table presents means and standard deviations (in parentheses) for the city-level characteristics in Table 2 from the 1996-2006 CASEN surveys and Appendix Table A2 from the 1992 national Census. See text for greater details