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Long-Term Gains from Longer School Days

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Abstract

This paper examines whether additional time in school affects labor market outcomes and educational attainment in adulthood. We leverage within and across city and cohort variation covering a large-scale reform that increased the Chilean elementary and secondary school day by 30 percent between 1997 and 2010. Exposure to full-day school increases educational attainment and earnings when students are in their 20s and 30s. In addition, we find evidence of delayed childbearing among women, and some occupational upskilling. These labor market effects are not concentrated in any particular subgroup, but are widespread throughout the population. *JEL classification: I26; I25; J24; H52*

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1 Introduction

This paper examines whether extending the elementary and secondary school day translates into greater educational attainment or improved labor market outcomes in adulthood. Until recently, in many middle-income and developing countries, the typical student attended school for approximately four hours. Since the 1990s, a number of Latin American countries have moved away from this model towards a 6-7 hour school day. Chile undertook such a reform between 1997 and 2010, increasing the school day for elementary and secondary school students in all publicly-funded schools by approximately 30 percent. Due to budgetary and logistical constraints, the timing of the introduction and expansion of full-day schooling varied across both cities and cohorts. We leverage both sources of variation to identify the effect of additional time in school on labor market outcomes in adulthood.

This work relates to an existing literature looking at the effectiveness of additional classroom time on student outcomes. One strand of this literature examines transitions from part- to full-day schooling in middle-income countries, including Chile. Importantly, these reforms also included increases in school resources, such as improved facilities or provision of school meals. Therefore, all examinations of these reforms, including the current study, cannot separate the effect of greater time in school from other concurrent changes to the school environment. The findings on 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). However, extended school days may affect children's outcomes in dimensions 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 (Berthelon, Kruger and Oyarzun, 2015; Contreras, Sepúlveda, Cabrera et al., 2010). For older students, additional time in school may reduce risky behaviors that occur outside of school, such as teen pregnancy and involvement with the criminal justice system (Berthelon and Kruger, 2011; Contreras, Sepúlveda, Cabrera et al., 2010). While existing research focuses primarily on short-term outcomes, we extend this work by examining whether additional learning time translates to improved labor market outcomes.

A second strand of the literature examines narrower expansions in the school day in developed countries. In contrast to full-day schooling reforms in middle-income countries, these reforms tend to be limited to young students in preschool and kindergarten. These studies find short-term benefits for both children and families in terms of improvements in students' academic performance and increases in maternal labor supply (Gibbs, 2014; Gelbach, 2002), as well as long-term benefits for students, measured by increased educational attainment and lower involvement with the criminal justice system (Cascio, 2009). If additional school time affects younger and older students through similar mechanisms, our results imply these more recent reforms may also improve long-term well-being.

Whether more time in school confers economic benefits in adulthood is of interest to policymakers in diverse settings. On the one hand, countries with similar income levels, school expenditures, and observable characteristics exhibit wide variation in academic performance (OECD, 2016b; NCES, 2011, 2015). One possible explanation for these differences is variation in learning time: the time elementary-age students spend in school ranges from about 2,000 hours a year in Russia to 7,000 in Australia (OECD, 2016a). Perhaps perceiving that additional school time translates to improved long-run earnings capacity, policymakers in many countries have advocated lengthening the school day. Since the mid-1990s, Chile, Uruguay, Colombia, Peru, and cities in Brazil and Argentina have abandoned the model of half-day schools in an effort to improve academic performance, and in the US, policymakers have supported a longer academic calendar in order to promote economic growth.[1]

In order to explore the long-term effects of additional schooling, we estimate the expected number of years a student would attend a full-day school using administrative data from the Ministry of Education on student enrollment and the year full-day schooling was implemented at the school-grade level. We use this information to estimate the probability a student born in a given municipality would attend school for a full day in each grade, and sum the probabilities across grades 1 through 12 to obtain the expected number of years each cohort

[1]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

in each city would attend a full-day school. We then merge the schooling data to nationally-representative labor market data on individuals ages 23-36 based on an individual's place of birth. This approach provides information on approximately 80,000 individuals who had access to between zero and twelve years of full-day schooling. Importantly, our treatment variable is defined based on place of birth, rather than the school individuals actually attend. In addition, all observations in our main sample were born before the reform was announced. Therefore, this treatment measure is not affected byfamilies choosing to move to areas that implemented the reform relatively early attending a school outside their city of residence.

This work makes several contributions to the literature on the returns to schooling and the effects of education interventions. First, we extend the existing research by examining the long-term effects of a longer school day on human capital and economic self-sufficiency in young adulthood. If more time in school increases educational attainment, the full labor market outcomes may not be realized when individuals are in their mid-20s, ages examined in the earlier literature.

Second, we exploit both within- and across-region variation in access to full-day schooling using nationally-representative data that includes information on 24 birth cohorts in 340 municipalities. In contrast, much of the existing literature has examined outcomes for a single birth cohort or within a single jurisdiction. This extension allows us to identify the causal effect of longer school days under relatively weak identifying assumptions regarding the timing of the reform. We further account for non-random selection of full-day schools by flexibly controlling for time-varying regional factors that may affect both reform implementation and long-term outcomes.

Finally, our relatively large sample size increases statistical power and enables investigation of heterogeneous effects by gender and family socioeconomic background. In this context, we find broad earnings gains associated with longer school days; most outcomes are not statistically different for women or students from economically-disadvantaged backgrounds.

To preview our results, we find that a longer school day increases high school graduation, earnings, and the likelihood of working in a skilled occupation in adulthood. The implied earnings gains are of a magnitude consistent with the previous work on the returns to education during the 1990s and early 2000s in Chile (OECD, 2013; Manacorda, Sánchez-Páramo and Schady, 2010). Extending the analysis of Berthelon and Kruger (2011), we also find that a longer school day delays childbearing among women. Given that motherhood is associated with lower subsequent labor force attachment and earnings these findings suggest a possible channel for our findings (Waldfogel, 1998; Kleven, Landais and Søgaard, 2018; Bertrand, Goldin and Katz, 2010; Kuziemko, Pan, Shen and Washington, 2018). Our results are inconsistent with other possible channels for earnings gains. In particular, we do not find longer school days increase college graduation rates for most groups or change internal migration patterns. We are unable to conclusively state whether increases in high school graduation and earnings are driven by family effects (through subsidizing child care) or individual effects (through learning and skill development). All 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 an income subsidy channel.

This paper proceeds as follows. Section 2 discusses the existing literature. Section 3 describes Chilean reform. Section 4 outlines the empirical approach and data. Section 5 presents results, and Section 6 discusses the findings and 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, Cooper and Allen, 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 against a causal interpretation of correlations between student outcomes and the 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, Layne-Farrar and Todd, 1995).

2.1 Short-term effects

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 by comparing kindergarten test scores across five districts where full-day kindergarten access was determined by lottery. She 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 lower-income countries.

Another series of papers examines the short-term effects of moving from part-day to full-day schooling in Latin American countries, where until recently, the typical student attended school for approximately four hours. Since the 1990s, a number of countries and cities have expanded the school day to 6-7 hours. These reforms also increased other school resources by building new facilities and often providing school meals or additional recess time. Since these changes occurred concurrently, examinations of full-day reforms– including the current study–cannot separate the effect of greater time in school from other changes to the school environment. The results from these reforms are mixed across countries, but generally point towards improved school performance (Glewwe, Hanushek, Humpage and Ravina, 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, Adrogué, Gigaglia and Orgales, 2009). As summarized by Ganimian and Murnane (2016), this literature finds that resources need to change students' experiences in order to affect achievement.

In the specific Chilean case, the existing research finds longer school days improve academic performance by 0.05-0.20 standard deviations (Bellei, 2009; Valenzuela, 2005; García, 2006). These findings, however, may mask heterogeneous effects, and do not account for selection into full day schools. Similar to the approach taken in this paper, Berthelon, Kruger and Vienne (2016) address these limitations by instrumenting for full-day school enrollment using municipality rates of full-day school exposure. They show children from lower-income families are more likely to atend full-day schools. Accounting for this selection, two years of longer school days improve academic performance 0.14 standard deviations, larger than the unadjusted improvement of an (insignificant) 0.005 standard deviations.

2.2 Longer-term effects

Even if full-day schooling does not affect student performance, longer time in school may affect medium- and long-term outcomes by changing families' and students' time use patterns. For younger students, extended school days provide families an implicit childcare subsidy, while for older students, longer school days reduce leisure time. Contreras, Sepúlveda, Cabrera et al. (2010) and Berthelon, Kruger and Vienne (2016) analyze the childcare subsidy induced by the Chilean reform and find full-day schools increased female labor participation and employment. Berthelon and Kruger (2011) analyze the extent to which longer school days "incapacitates" high school students from engaging with the criminal justice system or becoming a teenage mother. They find that a 20 percentage point increase in the fraction of full-time schools in a municipality lowers adolescent crime by 11-24 percent and teenage pregnancy rates for lower-income girls in urban areas by about 3 percent. As teen parenthood and a criminal history are associated with lower earnings, while higher maternal employment potentially increases family resources, these findings suggest that test score results may not capture the full benefit of longer school days.

Several studies directly examine the effects of longer school days on medium-term economic outcomes. 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. 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 attending full-day school increased monthly wages only for students who had previously attended the afternoon shift, but full-day schools had no significant overall effects on employment or wages. However, they do find positive effects on academic outcomes and cognitive test scores and, consistent with Berthelon and Kruger (2011), lower rates of teenage motherhood. There are two important limitations of Pires and Urzua (2015), which the current study aims to address more fully. First, their treatment cohorts attended full-day schools for 3-4 years at most, 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, Pires and Urzua (2015) examine labor market outcomes for young adults in their mid-20s. Although insufficient time has elapsed to investigate the full earnings-age profile, we are able to extend the analysis several years and examine employment outcomes into treated students' early 30s. A greater age range is particularly important if one effect of longer school days is to increase educational attainment, thereby delaying labor market entry.

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

Beginning in 1997, Chile implemented Jornada Escolar Completa (JEC), a large-scale reform that substantially increased the time students spend at schools. Overall, JEC increased the school day by an average of 1.4 hours for children in grades 3-12, keeping the total number of school days fixed.[2] This additional time is equivalent to a 30 percent increase in instructional time, and a 22 percent increase in the time spent in school (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 how individual schools used the additional time, and therefore cannot speak to the relative productivity of additional instruction, versus extra lunch or recreation. 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).

Before JEC, many Chilean schools operated a two-shift-system, where some students attended school in the morning and others attended in the afternoons. JEC gradually moved all schools to a single, full-day shift. The reform required a substantial infrastructure investment, as building and staffing resource needs nearly doubled in areas previously operating a double shift. Due to these practical considerations, the first schools that adopted JEC in 1997 and 1998 were those that were operating under-capacity (Bellei, 2009). For the remaining schools, the Ministry of Education prioritized funding schools not operating a double shift and schools in disadvantaged areas. Operational costs were partially offset by a 40 percent increase in the voucher the central government paid to local schools.[3] The reform rollout

[2] Originally, schools were mandated to expand school day in grades 3 to 12 by 2007 but in practice almost all of them fully implemented the reform in grades 1 to 12. See law description at: https://www.leychile.cl/Navegar?idNorma=76753.

[3] 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,

lasted fourteen years, as the legislation required that all schools receiving public funding operate a full-day schedule by 2007 (public schools) or 2010 (voucher schools).[4] 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 fraction had skyrocketed to nearly 100 percent in 2014 (Holland, Alfaro and Evans, 2015).

Figure 1 shows the median, 5th and 95th percentile of expected JEC exposure for each birth cohort in our main sample. The figure demonstrates substantial variation within and across cohorts. For example, an individual expected to attend five years of full-day school was in the 95th percentile of the 1987 birth cohort, the median of the 1990 cohort, and the 5th percentile of the 1992 birth cohort. Figure 2 summarizes how this varying exposure translates into the JEC exposure distribution for our main sample. While about 12 percent of our sample is fully untreated by the reform, a quarter of those with some exposure are expected to attend a full-day school for at least four years, and nine percent are expected to attend full-day schools for at least six years.

Using individual's year and city of birth, we map expected years of full-day schooling to outcome data from the biennial demographic survey, the National Socioeconomic Characterization Survey (CASEN), pooling survey years 2009-2015. The CASEN is a large, regionally- and nationally-representative household survey conducted biennially over our study period. This survey provides comprehensive information on labor market participation, household structure, educational attainment, and family background. Important for our purposes, these data include information on respondents' city of birth. For our outcome measures, these data include individual-level information about educational attainment, employment, family composition, and income.

We limit the sample to individuals born between 1979 and 1992–those exposed to between zero and twelve years of the reform who graduated high school during the phase-in period.

Sepúlveda, Cabrera et al., 2010).

[4]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.

Our main sample limits the data to individuals between the ages of age 23 to 36.[5]

Table 1 displays summary statistics for main sample. The average age is about 28, with about 2.4 years of JEC exposure. These characteristics are similar by gender and family socioeconomic status. Overall, eighty percent graduated high school, 20 percent have at least a four-year university degree, though educational attainment is lower for the low SES sample. About two-thirds of the full sample worked last year, and women have substantially lower participation rates than men. Even though our sample is relatively young, about half have children and more than 40 percent are either married or in a civil partnership.

4 Empirical Approach

4.1 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" a day 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-days schools 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:

$$y_{icmt} = \alpha + \beta JEC_{icmt} + \varepsilon_{icmt} \tag{1}$$

To measure 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. This simple framework requires that the introduction of JEC is uncorrelated with students' potential

[5]For results focusing on high school graduation, we extend the sample to include individuals ages 19-22. Results are robust to excluding these individuals.

outcomes. There are several reasons why this assumption may not hold in either before-after or within-cohort analyses. In particular, 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.

Examining the effect only using within cohort variation is also potentially problematic. Given the funding requirements of a large-scale expansion, policymakers might provide high levels of initial funding to undersubscribed schools or those with excess capacity. Alternatively, officials with limited resources may target initial funds to areas that 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 understates 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.

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. Therefore, students who enroll in full-time schools are likely those who benefit the most from the additional school time. 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. Our main specifications omit the Santiago metropolitan region, as students in Santiago are most likely to attend a school other than their neighborhood school. As shown in Panel A of Appendix Tables 2-5, most results are qualitatively robust, albeit less precise, to including Santiago.

Administrative data from the Ministry of Education provide information on total enrollment N for each grade g in school s serving grade g in municipality m.[6] Specifically, we calculate the expected number of years individuals born in cohort c in municipality m attend a full-day school in grades 1 through 12:

$$(\widehat{JEC_{cm}}) = \frac{1}{N_m} \sum_{s \in m} \sum_{g=1}^{12} \mathbb{1}\left\{JEC_{scgm}\right\} * N_{sgm} \tag{2}$$

$\mathbb{1}\left\{JEC_{scgm}\right\}$ 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_{sgm} is the number of students enrolled in grade g in school s in municipality m, obtained from the Ministry of Education administrative data. As this parameter does not depend on the school a student actually attends, it is free of bias from students selecting in to full-day schools or moving to cities with greater JEC availability. Moreover, for policymakers contemplating a large-scale national education reform, the ITT is the relevant parameter of interest. If students who enroll in full-day schools are those who benefit most from the reform, the treatment-on-the-treated (TOT) will be larger than the ITT (CASEN, 2016).[7]

Importantly, the gradual rollout 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.[8] We leverage both sources, comparing outcomes based on within- and across-cohort variation. A causal interpretation of our results therefore involves the relatively weaker assumption that the pace of JEC implementation is uncorrelated with students' potential outcomes. In addition to providing empirical support for this hypothesis, our main findings will account for broad, cohort-specific patterns of regional

[6] We assign each school its 2013 enrollment value and drop observations with a single student in grade g. About 7.2 percent of school-grade observation are dropped with this restriction, and results are robust to including the full universe of schools.

[7] Under a simplifying assumption of homogeneous treatment effects, dividing the ITT by the share of students enrolled in publicly-funded schools (approximately 90 percent) uncovers the TOT. For students from disadvantaged backgrounds, enrollment in publicly-financed schools is nearly universal; for this group, the ITT approximately equals the TOT.

[8] All individuals in our sample were born in 1993 or earlier, before the reform was announced (in 1997). This timing eliminates concerns that families with the largest potential benefits migrated to areas that adopted JEC relatively early so that their children could attend full-day schools.

economic development.

In the second step, we estimate the effect of full-day schools on outcome y_{icmt} as:

$$y_{icmt} = \alpha + \beta(\widehat{JEC_{cm}}) + X'_{icmt}\gamma + \delta_c + \phi_t + \psi_m + \varepsilon_{icmt} \quad (3)$$

$$y_{icmt} = \alpha + \beta(\widehat{JEC_{cm}}) + X'_{icmt}\gamma + \delta_{cr} + \phi_t + \psi_m + \varepsilon_{icmt} \quad (4)$$

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, such as age, gender, indigenous status, household size, and maternal education, as well as marital status and number and presence of children interacted with gender. δ_c and ϕ_t are cohort and survey year fixed effects. The JEC implementation period 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). To account for level differences in economic performance across municipalities, all of our estimates include a municipality fixed effect ψ_m, while ϕ_t accounts for national changes in economic performance over time. Our preferred specifications take the form of equation (4), which also includes δ_{cr}, a region-cohort fixed effect in order to capture general economic conditions in a relatively narrow geographic region that may affect each birth cohort's access to JEC and subsequent labor market outcomes. Most results are robust to this more conservative approach. As a slight modification, Panel (B) of Appendix Tables 2-4 replace survey year fixed effects with linear trends in baseline poverty and employment rates. Our main results are nearly identical under this modification.

In the vast majority of municipalities (97 percent), there is more than one school serving a given grade. 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 a later grade. 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. Given that the probability of treatment lacks mass points on the [0,1] interval and it can turn on and off, we measure full-day school exposure as a continuous measure, rather than a simple discrete event study.

One concern with leveraging the staggered implementation is that the timing of the JEC expansion might be correlated with factors that affect students' later life economic outcomes. While cohort-region fixed effects account for regional factors affecting treatment and outcomes in adulthood, these effects do not account for more local *changes* in economic conditions. Reassuringly, our results are robust to regional linear trends or trends interacted with regional poverty and unemployment rates prior to the reform, approaches similar to those discussed in Hoynes and Schanzenbach (2009).

To more formally explore the possibility of non-random timing, we regress the fraction of students in grades 1-12 attending a full-day school in municipality m during the roll-out period with contemporaneous measures of city economic and demographic characteristics, similar to the approach in Hoynes and Schanzenbach (2009). Table 2 shows the results. Without including municipality fixed effects, we observe full-day schooling was rolled out quickly in relatively low-populated areas. None of the other coefficients are statistically significant, but suggest the reform also occurred in more disadvantaged areas with lower educational attainment, larger household size, and higher poverty rates. Results are relatively similar when we exclude Santiago (column (2)). Columns (3) through (6) include municipality fixed effects, and explore other measures of disadvantage. Across specifications, we find that only changes in poverty significantly predict the timing of the roll-out, and this relationship is only marginally significant when we exclude municipalities in the Santiago region. Moreover, the coefficient implies moving from a 0 percent to 100 percent poverty is associated with an additional 0.2 percent of students attending full-day schools. Over the 1996 to 2006 period, the poverty rate in the median municipality fell by 12.5 percentage points, therefore, scaling this effect by typical changes in poverty within a city over a decade implies a very small change in exposure to full-day schooling.

To further explore the extent to which the timing of the rollout is correlated with changes in local economic conditions, Appendix Table 1 measures the effect of full-day schooling on *contemporarenous* labor market conditions and area demographic conditions using information from the 1996-2006 CASEN surveys. As with our main long-term findings, these specifications include city and municipality fixed effects, and omit municipalities in the Santiago region. We define JEC exposure as the fraction of students in grades 1-12 attending a full-day school in the survey year. Appendix Table 1 shows that the relationship between measures of labor force attachment (columns 1 and 2), income (columns 3 and 4), proxies for household disadvantage (columns 5-6) and rural status (column 7) are all small in magnitude, and with the exception of poverty rates, are statistically insignificant. In order to account for the possibility that the timing of JEC adoption is correlated with changes in local poverty conditions, Panel (B) of Appendix Tables 2-5 augment our main specification to include linear trends in municipality poverty and unemployment rates. Our main findings are robust to this extension.

5 Findings

5.1 Educational Attainment

We first turn to the effect of access to longer school days on educational attainment. Whether more time spent in school during childhood translates into higher eventual educational attainment suggests whether we might expect to observe earnings gains or higher labor force participation 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 school (in either elementary or secondary school) prepares students for higher education or instills non-pecuniary benefits of education (a greater “taste” for education), overall educational attainment may increase with greater exposure.

Tables 3 and 4 present our findings for high school and college graduation, respectively. The effect of exposure to an additional year of full-day schools is presented in the first row.

There are several ways to scale these estimates to recover policy-relevant parameters. First, the average treatment effect for our sample is obtained by multiplying this row by the average exposure in each sample: about 2.2 years for college graduation, and 3.5 years for the (slightly younger) high school graduate sample. An alternative interpretation, the implied effect of an additional year-equivalent of education, is found by scaling the main estimate by the average increase in instructional time under JEC ($\frac{\beta}{0.3}$). To translate the reduced-form intent-to-treat into an estimate of the treatment-on-the treated (TOT) students assuming homogeneous treatment effects, the reported estimates should be scaled by the fraction of children attending a school covered by the reform (e.g.: public or public voucher schools). Over the 1996-2013 period, approximately 90 percent of children attended government-funded schools (CASEN, 2016); dividing the ITT by 0.9 will therefore approximate the TOT.

Column (1) presents the main ITT results for the entire population, described in equation (3). JEC appears to have increased educational attainment, measured by both high school and college graduation. In particular, an additional year of JEC increases the probability of high school graduation by 1.5 percentage points and college graduation by 0.9 percentage points.

Columns (2) and (3) examine the effect of longer school days on two subpopulations of interest: women and students from disadvantaged backgrounds, as identified low levels of maternal educational attainment.[9] We focus on these groups for several reasons. First, the existing literature shows that maternal education is particularly important for child outcomes (Andrabi, Das and Khwaja, 2013; Carneiro, Heckman and Vytlacil, 2011; Currie, 2009). Therefore, any intergenerational benefits of full-day schooling are likely to arise through gains to women. Second, given the structure of Chile's educational system, children from lower-income families are more likely than their higher-income peers to attend government-subsidized, rather than private, schools (CASEN, 2016). Accordingly, we may interpret lower-income students as a "high-exposure" population, or those most likely to comply with treatment. In addition, we may expect the returns to educational inputs to be larger for

[9]The low SES sample is defined as individuals whose mothers have no more than a basic education, as reported by individuals in the CASEN.

lower-income students, as found in the context of other educational interventions (Cunha, Heckman, Lochner and Masterov, 2006; Havnes and Mogstad, 2011). The effect of the reform on these populations is mixed: while longer school days have a larger impact on the likelihood women and lower-income students graduate high school than the overall population, the effect on college graduation is more muted and statistically insignificant.

Columns (4) through (6) replicate Columns (1) through (3), replacing cohort fixed effects with cohort-by-birth region fixed effects to flexibly control for other cohort-specific economic factors that may have affected JEC participation and students' long-term outcomes, as described in equation (4). In most cases, our results are robust to including these effects, although somewhat attenuated. Including cohort-by-region fixed effects, we continue to find longer school days increase high school graduation rates for each group, and college completion for the full sample.

Figures 3 and 4 replicate Columns (4) through (6) of tables 3 and 4, but do not impose a linear restriction on the the number of days with longer school days. In particular, we replace the continuous measure of full-day exposure with nine two-year exposure bins. As about three percent of our main sample has access to JEC for more than eight years, we pool all observations with at least eight years of exposure. Figure 3 shows the increases in high school graduation are concentrated among those with at least four years of extended school days. Figure 4 shows the likelihood of the full population and of women graduating college is increasing in exposure to JEC, with each additional year of exposure conferring a smaller marginal gain. Mirroring the results in 4, following JEC, college graduation rates increased, but not among more disadvantaged populations.[10]

5.2 Employment and Earnings

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

[10]Our main specifications pool men and women from low socioeconomic backgrounds, as there are no substantial differences in outcomes by gender among this subpopulation.

34 percent relative to those with an eighth grade education to 64 percent relative to those with a sixth grade education (OECD, 2013; Manacorda, Sánchez-Páramo and Schady, 2010), implying a return to education of about 8 to 11 percent a year. The estimated earnings premium for post-secondary education is even higher: Manacorda, Sánchez-Páramo and Schady (2010) find Chilean men with a university degree earn 90 percent more than those with a secondary education. Since JEC increased high school graduation for all groups, and college graduation for some groups, we might expect improved economic outcomes when students are between ages 23 and 36. In this section, we examine whether longer school days increase employment, hours worked, and earnings.

Table 5 presents the main employment findings, where the outcome of interest is any work in the past month.[11] While point estimates for all ITT specifications in Columns (1) through (6) are positive, results are small in magnitude and imprecisely estimated. Figure 5, however, shows a clear upward pattern in employment for all groups, with significant employment gains beginning with approximately three years of expected full-day schooling. As our sample is skewed towards individuals who attended full-day school for relatively few years, this pattern suggests we might observe greater employment gains as even more recent cohorts (who had greater access to full-day schooling) begin their careers (Figure 2).

Table 6 suggests those with more years of full-day schooling have higher earnings between ages 23 and 36, particularly for men. In specifications relying on cohort fixed effects, each additional year of exposure to full-day schooling leads to an average earnings gain of about 3.8 log points for the overall population. To put these numbers in context, as JEC increased instructional time by 30 percent, these gains suggest about a 13 percent return to each additional year of education. These magnitudes are slightly higher than the returns to education found in higher-income countries (Card, 1999) and on the upper end of the ranges found for Chile during the 1990s. The earnings gains are larger and more precise for students from low-income backgrounds, and imply an earnings gain of 5-7 log points for each additional

[11] We define work in the past month as having earned income greater than 600 pesos, approximately $1 in 2015. Results are qualitatively similar to defining work as employment in the week prior to the reference period.

year of JEC exposure. In contrast, there is no significant change in earnings for women.

Examining any non-linearities in earnings gains, in Figure 6, we find log earnings increase approximately linearly between about 2-6 years of exposure and show some diminishing returns beyond six years. Results for women follow this general patterns, but are somewhat noisier than results for the full sample or disadvantaged subpopulation. Importantly, Figure 6 shows significant earnings gains begin to emerge after about 3 years of full-day schooling, suggesting the importance of a cumulative effect. As our main sample is skewed towards individuals with fewer than 3 years of full-day schooling, this figure is consistent with the insignificant, albeit positive, results in Table 6.

Table 7 replicates Table 6, limiting the sample to individuals with positive earnings. Consistent with small and insignificant effects on employment for the entire population, point estimates are similar (but are more precise) when limiting the sample to workers in columns (1) and (4). In contrast, the effect for individuals from disadvantaged backgrounds is smaller in Table 7 than Table 6, although these effects are not significantly different. One possible explanation is that longer school days prompted different types of disadvantaged students to enter the labor market than under the previous schedule. Although our results in Table 5 do not show an increase in *overall* employment for this subgroup, it remains possible that intrahousehold substitution or the *intensity* of labor force attachment – either in hours or weeks worked – increased for some low-SES individuals.[12]

6 Discussion

6.1 Mechanisms

The previous section showed longer school days increase high school graduation and earnings in adulthood. We now explore several intermediate channels other than educational attain-

[12]We have examined the effect of the reform on usual hours worked and found an insignificant increase for this group of about 0.08 hours (less than five 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 the seasonality of work over the year.

ment by which longer time in school might be expected to translate to higher earnings.

6.1.1 Migration

One possible explanation for increased employment and earnings is that individuals with greater educational attainment (in this case, a high school diploma) have greater ability and financial resources to migrate from rural areas to Santiago and other urban areas where wages are higher. Table 8 investigates the relationship between JEC exposure and subsequently moving a region outside the region of birth. For all populations, there is no significant change in migration patterns from greater exposure to longer school days. Appendix Table 6 shows that migration to Santiago, where wages tend to be higher, did also not increase. Appendix Table 7 generalizes the approach from Appendix Table 6 by calculating the average earned income in each municipality and creating a standardized index of city-level income with a mean of zero and a standard deviation of one. Again, we find economically and statistically insignificant changes in the economic opportunities available to individuals with greater access to full day school (measured by average income in an area). These findings suggest that it is very unlikely that our results are driven by domestic migration.

6.1.2 Fertility patterns

For women, motherhood is associated with labor force non-participation. In the US context, this "motherhood penalty" results in lower earnings among labor market re-entry (Waldfogel, 1998; Kleven, Landais and Søgaard, 2018; Bertrand, Goldin and Katz, 2010; Kuziemko, Pan, Shen and Washington, 2018). Previous work has documented document that JEC leads to lower teen pregnancy rates for disadvantaged women in urban areas through schooling's incapacitation effect (Berthelon and Kruger, 2011). We also find small, albeit insignificant, reductions in teen pregnancy, consistent with these earlier findings. 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 in our sample, we find longer school days led women to give birth at older ages. Each additional year of full-day schooling delayed birth by about 2.3 months (Table 9 and

Figure 7). These results are similar for disadvantaged subgroups (columns 2 and 5), as well as women who were born in urban areas (columns 3 and 6). As the oldest individuals in our sample are 36 years old, and many are in their early 20s and have not yet reached prime childbearing years, this estimate likely understates the full effect.

6.1.3 Occupation choice

Another mechanism that could increase earnings, even if not necessarily affecting college graduation or labor force participation is through occupational choice. As the majority of additional school time under JEC went towards reading and math instruction, students attending full-day schools may have entered the labor force with greater skills, even absent a formal credential. Table 10 is consistent with this hypothesis, showing longer school days increased the likelihood of having a managerial, professional, or technical occupation by about 1.5 percentage points.[13] This result is driven by women's occupational choices. As an alternative measure of occupational prestige, intended to capture upskilling across the entire occupation distribution, we measure the log average earnings of other workers with the same 4-digit occupation in Appendix Table 8. Measuring skill by occupational wages shows a similar pattern to the binary skill measure: each additional year of exposure to full-day schooling increases expected salaries by about 1.4 percent due to occupational choice. While women are more likely to work in skilled professions, we find smaller and insignificant gains measured by average occupational wages, consistent with the earnings results reported earlier. Across occupations, the overall female earning distribution is less disperse than the male distribution, particularly at the upper tail of the distribution. Moreover, while women are more likely to work in professional and technical occupations, the gender wage gap is particularly large in these professions. The nature of the gender-specific earnings distribution suggests that although JEC may have increased the skill requirements of jobs women hold, these changes may not be detectable in earnings.

[13]Following ILO, we define "skilled" occupations as the primary occupation in major codes 1, 2, and 3.

6.1.4 Family resources or academic skills?

In the years following full-day school implementation, teachers and parents in most schools reported that the additional time was used for math and reading instruction (DESUC, 2005). If this additional instructional time leads to improved literacy and numeracy, the earnings gains we observe may be explained by increases in skills that employers value. Another, not necessarily competing, hypothesis is that longer school days for children provide a child care subsidy for parents. This implicit subsidy increases family resources, either by reducing the cost of child care, or allowing parents to enter the labor force rather than provide home-based care. As we lack school-level information on how additional time in school was used and the CASEN does not include information on whether a respondent's parents were employed during childhood, we are unable to fully examine whether benefits of longer school days are concentrated among individuals with working parents. However, in order to provide suggestive evidence on the extent to which our findings are driven by increased parental employment, we calculate the labor force participation rate among mothers with school-aged children in the 1996-2006 CASEN surveys. We then define a "high maternal LFP increase" sample comprised of municipalities that experienced greater-than-median increases in maternal labor supply over the course of JEC implementation. Cities in this sample increased maternal labor force participation by at least 6.3 percentage points (from a base of about 36 percent), and the median city in this sample increased maternal participation by 10.6 percentage points.

Table 11 presents our main results for this sample. Results are less precise due to relatively small sample sizes, and we are unable to reject equal treatment effects in areas with greater increases in maternal employment. Compared to our main results, Table 11 does not show benefits were concentrated 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, these results suggest that our results are not solely driven through changes in family resources.

6.2 Robustness

One concern with our results is that they are merely capturing trends towards improved economic outcomes in a country experiencing rapid economic growth. While region-cohort fixed effects account for regional factors affecting labor market outcomes, it is possible that more local economic conditions affect educational attainment and earnings. To assess whether these results are merely reflecting secular economic improvements, we repeat the analyses for an older population: those born between 1959 and 1973. All of these individuals had completed schooling prior to JEC introduction; their actual treatment is 0. We assign these workers the expected number of full-day schooling for a cohort born twenty years later. Our placebo "exposure" effect is given by:

$$(\widehat{JEC_{cm,placebo}}) = \frac{1}{N_m} \sum_{s \in g} \sum_{g=1}^{12} \mathbb{1}\left\{JEC_{sgm,(c+20)}\right\} * N_{sgm} \tag{5}$$

Using this placebo treatment exposure, we re-estimate each of our main findings in Table 12. With the exception of high school graduation among disadvantaged individuals, we do not find any economically or statistically significant changes in educational attainment, earnings, or age at first birth for any subgroup. Further, relative to our main estimates, all of the point estimates are substantially smaller in magnitude.

7 Conclusion

We find that access to longer school days increases high school graduation and earnings in adulthood. The magnitude of earnings gains is consistent with other work examining the returns to education in Chile during this time period. Higher earnings are partly driven through occupational choice: individuals exposed to longer school days are more likely to work in a skilled occupation and higher-salaried jobs. For women, we find that greater access to longer school days delays childbearing. As a whole, these results suggest that longer school days provide modest long-term benefits in economic outcomes. These benefits are not concentrated in any particular subgroup, but are relatively widespread throughout the population. A simple back-of-the-envelope calculation using the average earnings gains from column (4) of Table 6, and a 3 percent social discount rate suggests full-day schools increased national earnings by between about \$62 and \$87 billion over a 20-year period. This calculation is not a full cost-benefit analysis – it does not account for infrastructure costs and additional hiring costs, nor does it include any benefits accruing from delayed childbearing or reduced crime. Nonetheless, these results show expanded educational resources can improve long-term economic well-being.

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8 Tables

Table 1: Summary statistics: Main sample

	(1) All	(2) Women	(3) Low SES
$\widehat{JEC}$	2.413	2.390	2.418
	(2.328)	(2.337)	(2.387)
Age	28.01	28.08	28.05
	(3.668)	(3.675)	(3.687)
Year birth	1984.9	1984.9	1984.8
	(3.770)	(3.773)	(3.771)
Female	0.518	1	0.534
	(0.500)	(0)	(0.499)
Indigenous	0.102	0.107	0.133
	(0.303)	(0.309)	(0.340)
Married	0.188	0.213	0.186
	(0.391)	(0.410)	(0.389)
Civil partnership or married	0.444	0.468	0.425
	(0.497)	(0.499)	(0.494)
Parent	0.483	0.566	0.501
	(0.500)	(0.496)	(0.500)
Number of children	0.763	0.915	0.807
	(0.959)	(1.006)	(0.987)
Low-ed mother	0.432	0.446	1
	(0.495)	(0.497)	(0)
Mom college grad	0.0391	0.0402	0
	(0.194)	(0.196)	(0)
HS grad	0.793	0.805	0.732
	(0.405)	(0.396)	(0.443)
College grad	0.187	0.205	0.127
	(0.390)	(0.404)	(0.333)
Worked last yr	0.662	0.555	0.641
	(0.473)	(0.497)	(0.480)
Usual hours	27.59	21.10	26.53
	(23.45)	(22.11)	(23.57)
Earnings (2015 pesos)	293031.9	214192.7	232894.3
	(452750.9)	(349209.8)	(396004.4)
Skilled occupation	0.291	0.372	0.217
	(0.454)	(0.483)	(0.412)
Urban	0.850	0.854	0.792
	(0.357)	(0.353)	(0.406)
Lives in Santiago	0.111	0.113	0.0778
	(0.314)	(0.316)	(0.268)
Moved cities	0.356	0.363	0.308
	(0.479)	(0.481)	(0.462)
Observations	82783	42765	39576

Notes: Table shows summary statistics for our main sample (column 1), women (column 2), and individuals from disadvantaged backgrounds (column 3). Expected JEC calculated from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2009-2015 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-36 years old at the time of survey. See text for details.

Table 2: Predictive characteristics of JEC adoption

	(1) *JEC*	(2) *JEC*	(3) *JEC*	(4) *JEC*	(5) *JEC*	(6) *JEC*
Employment rate	0.2184	0.1182	0.1437	0.1048	0.0396	0.0175
	(0.2477)	(0.2761)	(0.1894)	(0.2078)	(0.1842)	(0.2006)
Yrs education	0.0017	-0.0102	0.0016	-0.0053	-0.0053	-0.0103
	(0.0089)	(0.0115)	(0.0151)	(0.0169)	(0.0158)	(0.0176)
Avg hh size	0.0132	0.0269	0.0044	0.0077	0.0235	0.0256
	(0.0336)	(0.0403)	(0.0367)	(0.0429)	(0.0361)	(0.0419)
Poverty rate	0.0977	0.0560	0.2214**	0.2033*		
	(0.1046)	(0.1195)	(0.1015)	(0.1135)		
Log population	-0.0480***	-0.0447***	-0.0329	-0.0732	-0.0254	-0.0634
	(0.0102)	(0.0128)	(0.0912)	(0.0976)	(0.0924)	(0.0961)
Log auton. income					-0.0117	-0.0161
					(0.0287)	(0.0314)
Constant	0.2221	0.3234	0.1310	0.6023	0.3423	0.8185
	(0.3086)	(0.3508)	(0.9835)	(0.9869)	(1.0251)	(1.0423)
Observations	1210	980	1210	980	1210	980
R^2	0.6838	0.6595	0.8873	0.8814	0.8865	0.8807
FE	Year	Year	Year and city	Year and city	Year and city	Year and city
Sample	All	No RM	All	No RM	All	No RM

Notes: Dependent variable is the fraction of students in grades 1-12 attending a full-day school for each year of the 1996-2006 CASEN surveys. All labor force and economic characteristics calculated as municipality averages. 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.

Table 3: Educational attainment: High school graduation

	(1) HS grad b/se/stats	(2) HS grad b/se/stats	(3) HS grad b/se/stats	(4) HS grad b/se/stats	(5) HS grad b/se/stats	(6) HS grad b/se/stats
$\widehat{JEC}$	0.015***	0.020***	0.019***	0.014***	0.019***	0.018***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Demographic control	Yes	Yes	Yes	Yes	Yes	Yes
RegionXcohort FE	No	No	No	Yes	Yes	Yes
Observations	124942	64002	60553	124942	64002	60553
FE	City cohort survey	City cohort survey	City cohort survey	City cohort survey	City cohort survey	City cohort survey
Sample	All	Women	Low SES	All	Women	Low SES
DV mean	0.798	0.811	0.749	0.798	0.811	0.749
Pct change	0.0191	0.0249	0.0247	0.0177	0.0237	0.0237
E($\widehat{JEC}$)	3.589	3.548	3.661	3.589	3.548	3.661
r2	0.0697	0.0708	0.0599	0.0746	0.0798	0.0686

Notes: All specifications include city of birth and survey year fixed effects. Columns (1-3) also include birth year fixed effects (Equation 3), columns (4-6) replace cohort fixed effects with cohort-by-region fixed effects as in Equation (4). Standard errors in parentheses clustered by city of birth. Control variables include gender, a quadratic in age, indigenous status, household size, maternal education, marital status and number and presence of children, interacted with gender. Expected JEC calculated from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2009-2015 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 19-36 years old at the time of survey. Columns (2) and (5) limit the sample to women; columns (3) and (6) limited to individuals whose mothers had no more than a basic education. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$

Table 4: Educational attainment: College graduation

	(1)	(2)	(3)	(4)	(5)	(6)
	College	College	College	College	College	College
	b/se/stats	b/se/stats	b/se/stats	b/se/stats	b/se/stats	b/se/stats
$\widehat{JEC}$	0.009**	0.002	0.001	0.007**	0.002	-0.000
	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)
Demographic control	Yes	Yes	Yes	Yes	Yes	Yes
RegionXcohort FE	No	No	No	Yes	Yes	Yes
Observations	87568	45292	41855	87568	45292	41855
FE	City cohort survey	City cohort survey	City cohort survey	City cohort survey	City cohort survey	City cohort survey
Sample	All	Women	Low SES	All	Women	Low SES
DV mean	0.189	0.206	0.126	0.189	0.206	0.126
Pct change	0.0453	0.00904	0.00772	0.0383	0.00945	-0.00248
E($\widehat{JEC}$)	2.247	2.229	2.245	2.247	2.229	2.245
r2	0.101	0.0937	0.0397	0.107	0.103	0.0514

Notes: All specifications include city of birth and survey year fixed effects. Columns (1-3) also include birth year fixed effects (Equation 3), columns (4-6) replace cohort fixed effects with cohort-by-region fixed effects as in Equation (4). Standard errors in parentheses clustered by city of birth. Control variables include gender, a quadratic in age, indigenous status, household size, maternal education, marital status and number and presence of children, interacted with gender. Expected JEC calculated from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2009-2015 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-36 years old at the time of survey. Columns (2) and (5) limit the sample to women; columns (3) and (6) limited to individuals whose mothers had no more than a basic education. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$

Table 5: Employment: Extensive margin

	(1)	(2)	(3)	(4)	(5)	(6)
	Work last yr	Work last yr	Work last yr	Work last yr	Work last yr	Work last yr
	b/se/stats	b/se/stats	b/se/stats	b/se/stats	b/se/stats	b/se/stats
$\widehat{JEC}$	0.003	0.004	0.008	0.001	0.002	0.005
	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)
Demographic control	Yes	Yes	Yes	Yes	Yes	Yes
RegionXcohort FE	No	No	No	Yes	Yes	Yes
Observations	81638	42173	38838	81638	42173	38838
FE	City cohort survey	City cohort survey	City cohort survey	City cohort survey	City cohort survey	City cohort survey
Sample	All	Female	Low SES	All	Female	Low SES
DV mean	0.661	0.554	0.641	0.661	0.554	0.641
Pct change	0.00405	0.00650	0.0121	0.00187	0.00447	0.00791
E($\widehat{JEC}$)	2.413	2.390	2.418	2.413	2.390	2.418
r2	0.146	0.0840	0.158	0.150	0.0917	0.166

Notes: All specifications include city of birth and survey year fixed effects. Columns (1-3) also include birth year fixed effects (Equation 3), columns (4-6) replace cohort fixed effects with cohort-by-region fixed effects as in Equation (4). Standard errors in parentheses clustered by city of birth. Control variables include gender, a quadratic in age, indigenous status, household size, maternal education, marital status and number and presence of children, interacted with gender. Expected JEC calculated from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2009-2015 CASEN surveys. Employment defined as having income at least 600 pesos (approximately $1) in the past month. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-36 years old at the time of survey. Columns (2) and (5) limit the sample to women; columns (3) and (6) limited to individuals whose mothers had no more than a basic education. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$

Table 6: Earnings

	(1)	(2)	(3)	(4)	(5)	(6)
	Log real earnings	Log real earnings	Log real earnings	Log real earnings	Log real earnings	Log real earnings
	b/se/stats	b/se/stats	b/se/stats	b/se/stats	b/se/stats	b/se/stats
$\widehat{JEC}$	0.038*	0.040	0.069**	0.027	0.032	0.052*
	(0.023)	(0.029)	(0.029)	(0.023)	(0.030)	(0.028)
Demographic control	Yes	Yes	Yes	Yes	Yes	Yes
RegionXcohort FE	No	No	No	Yes	Yes	Yes
Observations	81638	42173	38838	81638	42173	38838
FE	City cohort survey	City cohort survey	City cohort survey	City cohort survey	City cohort survey	City cohort survey
Sample	All	Female	Low SES	All	Female	Low SES
DV mean	293132.1	214105.2	232841.0	293132.1	214105.2	232841.0
Pct change						
E($\widehat{JEC}$)	2.413	2.390	2.418	2.413	2.390	2.418
r2	0.183	0.105	0.191	0.188	0.113	0.199

Notes: All specifications include city of birth and survey year fixed effects. Columns (1-3) also include birth year fixed effects (Equation 3), columns (4-6) replace cohort fixed effects with cohort-by-region fixed effects as in Equation (4). Standard errors in parentheses clustered by city of birth. Control variables include gender, a quadratic in age, indigenous status, household size, maternal education, marital status and number and presence of children, interacted with gender. Expected JEC calculated from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2009-2015 CASEN surveys. Log earnings defined as log earnings (in thousands of inflation-adjusted pesos), plus one, in order to account for individuals with no earnings. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-36 years old at the time of survey. Columns (2) and (5) limit the sample to women; columns (3) and (6) limited to individuals whose mothers had no more than a basic education. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$

Table 7: Earnings:Workers

	(1)	(2)	(3)	(4)	(5)	(6)
	Log real earnings	Log real earnings	Log real earnings	Log real earnings	Log real earnings	Log real earnings
	b/se/stats	b/se/stats	b/se/stats	b/se/stats	b/se/stats	b/se/stats
$\widehat{JEC}$	0.033***	0.024	0.037**	0.032***	0.023	0.036**
	(0.010)	(0.016)	(0.015)	(0.011)	(0.018)	(0.015)
Demographic control	Yes	Yes	Yes	Yes	Yes	Yes
RegionXcohort FE	No	No	No	Yes	Yes	Yes
Observations	50585	20624	23107	50585	20624	23107
FE	City cohort survey	City cohort survey	City cohort survey	City cohort survey	City cohort survey	City cohort survey
Sample	All	Female	Low SES	All	Female	Low SES
DV mean	446280.8	390373.5	368943.7	446280.8	390373.5	368943.7
Pct change						
E($\widehat{JEC}$)	2.209	2.203	2.230	2.209	2.203	2.230
r2	0.113	0.113	0.0904	0.120	0.125	0.104

Notes: All specifications include city of birth and survey year fixed effects. Columns (1-3) also include birth year fixed effects (Equation 3), columns (4-6) replace cohort fixed effects with cohort-by-region fixed effects as in Equation (4). Standard errors in parentheses clustered by city of birth. Control variables include gender, a quadratic in age, indigenous status, household size, maternal education, marital status and number and presence of children, interacted with gender. Expected JEC calculated from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2009-2015 CASEN surveys. Log earnings defined as log earnings (in thousands of inflation-adjusted pesos), plus one, in order to account for individuals with no earnings. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-36 years old at the time of survey. Columns (2) and (5) limit the sample to women; columns (3) and (6) limited to individuals whose mothers had no more than a basic education. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$

Table 8: Moved regions

	(1) Moved regions b/se/stats	(2) Moved regions b/se/stats	(3) Moved regions b/se/stats	(4) Moved regions b/se/stats	(5) Moved regions b/se/stats	(6) Moved regions b/se/stats
$\widehat{JEC}$	-0.004	-0.004	-0.009	-0.003	-0.002	-0.006
	(0.004)	(0.006)	(0.005)	(0.004)	(0.006)	(0.005)
Demographic control	Yes	Yes	Yes	Yes	Yes	Yes
RegionXcohort FE	No	No	No	Yes	Yes	Yes
Observations	81638	42173	38838	81638	42173	38838
FE	City cohort survey	City cohort survey	City cohort survey	City cohort survey	City cohort survey	City cohort survey
Sample	All	Female	Low SES	All	Female	Low SES
DV mean	0.356	0.362	0.308	0.356	0.362	0.308
Pct change	-0.0110	-0.0112	-0.0286	-0.00982	-0.00555	-0.0204
E($\widehat{JEC}$)	2.413	2.390	2.418	2.413	2.390	2.418
r2	0.0981	0.0925	0.105	0.103	0.101	0.116

Notes: All specifications include city of birth and survey year fixed effects. Columns (1-3) also include birth year fixed effects (Equation 3), columns (4-6) replace cohort fixed effects with cohort-by-region fixed effects as in Equation (4). Standard errors in parentheses clustered by city of birth. Control variables include gender, a quadratic in age, indigenous status, household size, maternal education, marital status and number and presence of children, interacted with gender. Expected JEC calculated from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2009-2015 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-36 years old at the time of survey. Columns (2) and (5) limit the sample to women; columns (3) and (6) limited to individuals whose mothers had no more than a basic education. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$

Table 9: Age at first birth

	(1) Age at 1st birth b/se/stats	(2) Age at 1st birth b/se/stats	(3) Age at 1st birth b/se/stats	(4) Age at 1st birth b/se/stats	(5) Age at 1st birth b/se/stats	(6) Age at 1st birth b/se/stats
$\widehat{JEC}$	0.195***	0.171***	0.180***	0.190***	0.194***	0.171***
	(0.043)	(0.052)	(0.052)	(0.049)	(0.061)	(0.059)
Demographic control	Yes	Yes	Yes	Yes	Yes	Yes
RegionXcohort FE	No	No	No	Yes	Yes	Yes
Observations	26913	12536	20929	26913	12536	20929
FE	City cohort survey	City cohort survey	City cohort survey	City cohort survey	City cohort survey	City cohort survey
Sample	All women	Low SES women	Urban women	All women	Low SES women	Urban women
DV mean	21.12	20.84	21.19	21.12	20.84	21.19
Pct change	0.00926	0.00820	0.00850	0.00898	0.00929	0.00809
E($\widehat{JEC}$)	2.861	2.683	2.801	2.861	2.683	2.801
r2	0.112	0.111	0.123	0.123	0.133	0.135

Notes: Each column provides results from the preferred specification with cohort-region, survey year, and city of birth fixed effects as in Equation (4). Standard errors in parentheses clustered by city of birth. Control variables include a quadratic in age, indigenous status, household size, maternal education, marital status, and number and presence of children, as well as survey year, municipality, and cohort-by-region fixed effects. Expected JEC calculated from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2009-2015 CASEN surveys. Sample limited to women born between 1979 and 1992 outside the Santiago metropolitan region who were 23-36 years old at the time of survey and 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 no education beyond a basic education; column (3) limits the sample to women living in urban areas. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$

Table 10: Skilled occupation

	(1) Skilled b/se/stats	(2) Skilled b/se/stats	(3) Skilled b/se/stats	(4) Skilled b/se/stats	(5) Skilled b/se/stats	(6) Skilled b/se/stats
$\widehat{JEC}$	0.016***	0.019***	0.006	0.015***	0.018**	0.003
	(0.005)	(0.007)	(0.006)	(0.004)	(0.008)	(0.006)
Demographic control	Yes	Yes	Yes	Yes	Yes	Yes
RegionXcohort FE	No	No	No	Yes	Yes	Yes
Observations	52005	21787	23775	52005	21787	23775
FE	City cohort survey	City cohort survey	City cohort survey	City cohort survey	City cohort survey	City cohort survey
Sample	All	Female	Low SES	All	Female	Low SES
DV mean	0.292	0.372	0.218	0.292	0.372	0.218
Pct change	0.0550	0.0514	0.0281	0.0505	0.0479	0.0151
E($\widehat{JEC}$)	2.203	2.193	2.223	2.203	2.193	2.223
r2	0.155	0.136	0.105	0.162	0.150	0.120

Notes: All specifications include city of birth and survey year fixed effects. Columns (1-3) also include birth year fixed effects (Equation 3), columns (4-6) replace cohort fixed effects with cohort-by-region fixed effects as in Equation (4). Standard errors in parentheses clustered by city of birth. Control variables include gender, a quadratic in age, indigenous status, household size, maternal education, marital status and number and presence of children, interacted with gender. Expected JEC calculated from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2009-2015 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-36 years old at the time of survey. Columns (2) and (5) limit the sample to women; columns (3) and (6) limited to individuals whose mothers had no more than a basic education. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$

Table 11: High maternal LFP increase

	(1) HS grad b/se/stats	(2) College b/se/stats	(3) Work last yr b/se/stats	(4) Log real earnings b/se/stats	(5) Skilled b/se/stats	(6) Age at 1st birth b/se/stats
$\widehat{JEC}$	0.004	0.002	0.001	0.020	0.008	0.087
	(0.004)	(0.004)	(0.007)	(0.041)	(0.006)	(0.056)
Demographic control	Yes	Yes	Yes	Yes	Yes	Yes
RegionXcohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	39077	27289	25564	25564	16291	8767
FE	City	City	City	City	City	None
Sample	All	All	All	All	All	All women
DV mean	0.809	0.197	0.653	307624.6	0.308	21.06
Pct change	0.00457	0.0119	0.00169		0.0270	0.00411
E($\widehat{JEC}$)	3.495	2.198	2.359	2.359	2.153	21.06
r2	0.0674	0.111	0.153	0.189	0.173	0.111

Notes: Column reports results from preferred specification with cohort-region, survey year, and city of birth fixed effects as in Equation (4). Standard errors in parentheses clustered by city of birth. Control variables include gender, a quadratic in age, indigenous status, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as survey year, municipality, and cohort-by-region fixed effects. Expected JEC calculated from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2009-2015 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-36 years old at the time of survey and who were born in an area experiencing greater-than-median increase in maternal labor force participation over the JEC rollout period (approximately 6.3 percentage points over 14 years). See text for details. *** $= p < 0.01$, ** $= p < 0.05$, * $= p < 0.10$

Table 12: Placebo Results

(a) All

	(1) HS grad b/se/stats	(2) College b/se/stats	(3) Log earn b/se/stats	(4) Skilled b/se/stats
$\widehat{JEC}$	0.009***	0.000	-0.021	-0.004
	(0.003)	(0.002)	(0.018)	(0.004)
Demographic control	Yes	Yes	Yes	Yes
RegionXcohort FE	Yes	Yes	Yes	Yes
Observations	123303	93588	87475	59409
FE	City cohort survey	City cohort survey	City cohort survey	City cohort survey
Sample	All	All	All	All
DV mean	0.516	0.115	341361.8	0.194
Pct change	0.0172	0.00165	-6.25e-08	-0.0207
r2	0.159	0.139	0.261	0.128

(b) Women

	(1) HS grad b/se/stats	(2) College b/se/stats	(3) Log earn b/se/stats	(4) Skilled b/se/stats	(5) Age at 1st birth b/se/stats
$\widehat{JEC}$	0.003	-0.001	-0.032	-0.003	0.090
	(0.004)	(0.003)	(0.028)	(0.005)	(0.096)
Demographic control	Yes	Yes	Yes	Yes	Yes
RegionXcohort FE	Yes	Yes	Yes	Yes	Yes
Observations	65403	49567	46300	23201	35902
FE	City cohort survey	City cohort survey	City cohort survey	City cohort survey	City cohort survey
Sample	Women	Women	Female	Female	All women
DV mean	0.522	0.111	197998.4	0.229	23.49
Pct change	0.00536	-0.00499	-0.000000160	-0.0135	0.00382
r2	0.156	0.141	0.105	0.128	0.0473

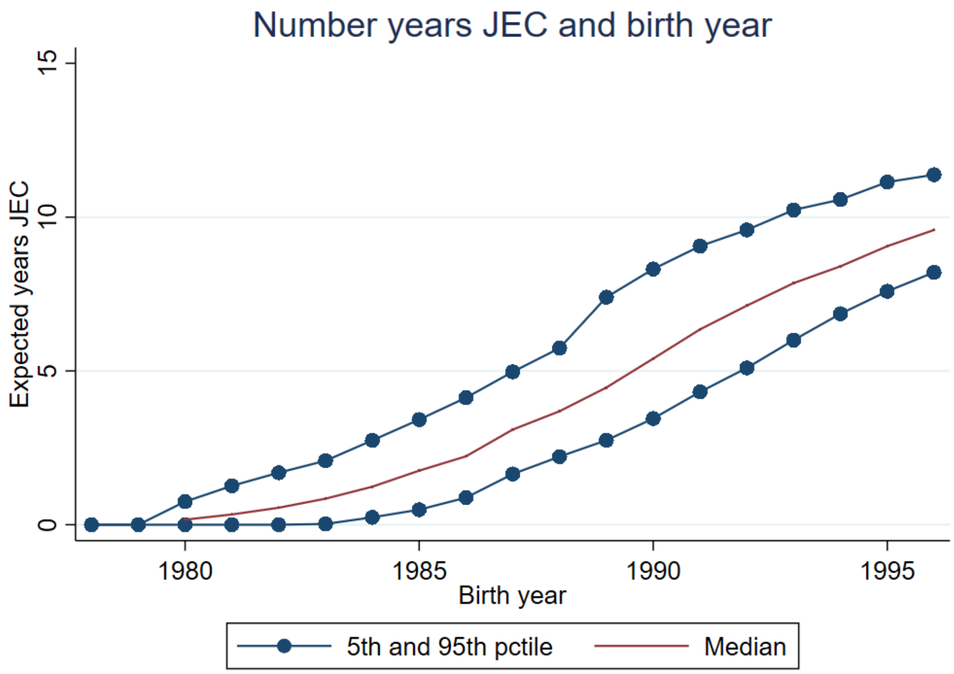
(c) Low SES

	(1) HS grad b/se/stats	(2) College b/se/stats	(3) Log earn b/se/stats	(4) Skilled b/se/stats	(5) Age at 1st birth b/se/stats
$\widehat{JEC}$	0.003	0.000	-0.042*	0.000	0.028
	(0.004)	(0.002)	(0.022)	(0.005)	(0.080)
Demographic control	Yes	Yes	Yes	Yes	Yes
RegionXcohort FE	Yes	Yes	Yes	Yes	Yes
Observations	70404	54008	50420	33087	21647
FE	City cohort survey	City cohort survey	City cohort survey	City cohort survey	City cohort survey
Sample	Low SES	Low SES	Low SES	Low SES	Low SES women
DV mean	0.452	0.0736	277866.4	0.152	23.03
Pct change	0.00635	0.00484	-0.000000150	0.00160	0.00123
r2	0.0937	0.0570	0.281	0.0545	0.0578

Notes: Standard errors in parentheses clustered by city of birth. Control variables include age, gender, indigenous status, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as survey year, municipality, and cohort-by-region fixed effects. Expected JEC calculated as the expected JEC in Equation (2) for a cohort born 20 years after cohort c. Enrollment and JEC adoption data available from the Ministry of Education; adult outcomes from the 2009-2015 CASEN surveys. Sample limited to individuals born between 1959 and 1973 outside the Santiago metropolitan region who were 43-56 years old at the time of survey. Column (2) limits the sample to women; column (3) limits the sample to individuals whose mothers had no more than a basic education. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$

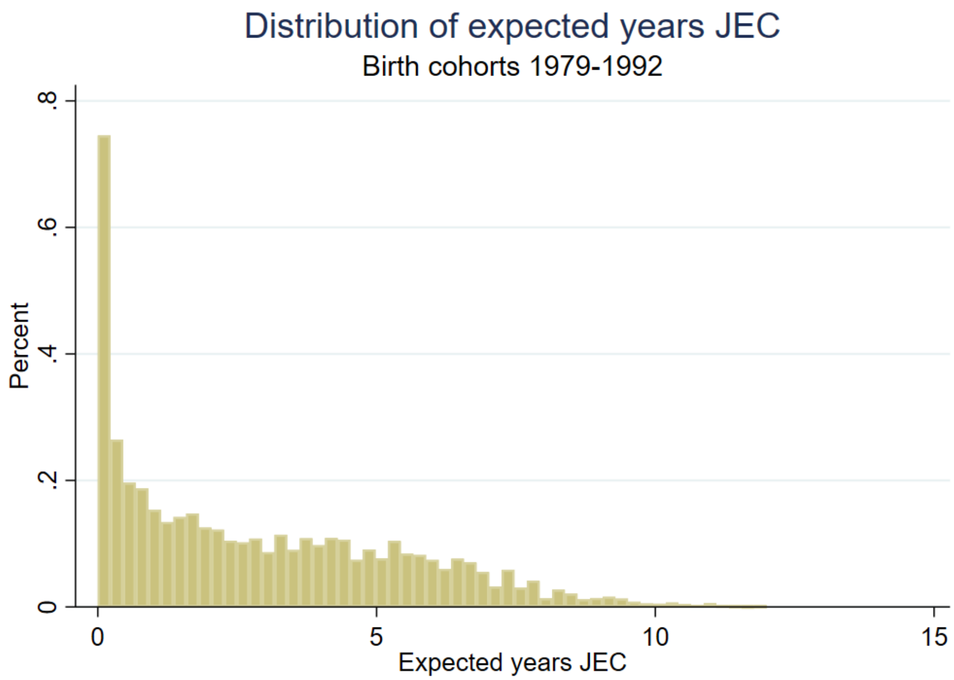
9 Figures

Figure 1: JEC timing varied across regions



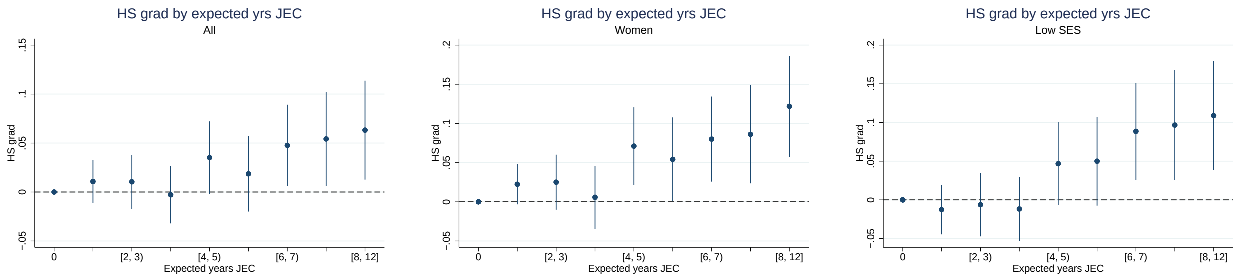
Notes: Figure shows the expected years of attending a full-day school across cities of birth by birth year. The bottom solid blue line denotes the expected years of full-day schooling in grades 1-12 for students at the 5th percentile of their cohort-specific distribution. The top blue line denotes the 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. Source: Ministry of Education, 2016; CASEN, 2009, 2011, 2013, 2015.

Figure 2: Expected JEC exposure, full sample



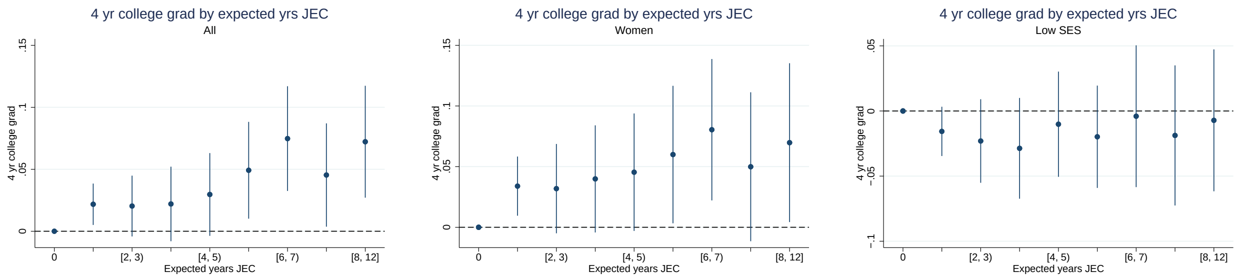
Notes: Figure shows the distribution of expected years of attending a full-day school between grades 1-12 for our main sample of individuals born between 1979 and 1992 and who were 23-36 at the time of the CASEN survey. Individuals born in the Santiago metropolitan region are excluded from the analysis. Source: Ministry of Education, 2016; CASEN, 2009, 2011, 2013, 2015.

Figure 3: High school graduation



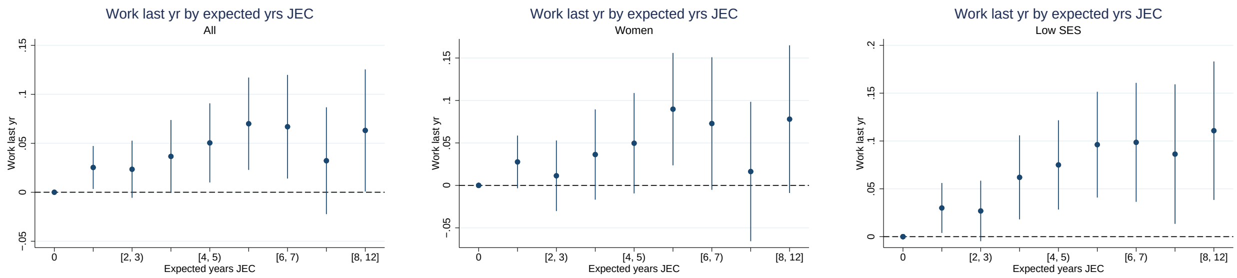
Notes: Figure shows the results from a regression taking the form of Equation (4), but replacing the continuous treatment variable ($\widehat{JEC_{cm}}$) with nine indicators for 0, (0,1], (1,2]...(4,5], (8, 12] years of expected JEC and plotting each of the JEC treatment variables. Vertical lines denote 95 percent confidence intervals clustered by city of birth. Control variables include gender, a quadratic in age, indigenous status, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as survey year, municipality, and cohort-by-region fixed effects. Expected JEC calculated from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2009-2015 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 19-36 years old at the time of survey. See text for details.

Figure 4: College graduation



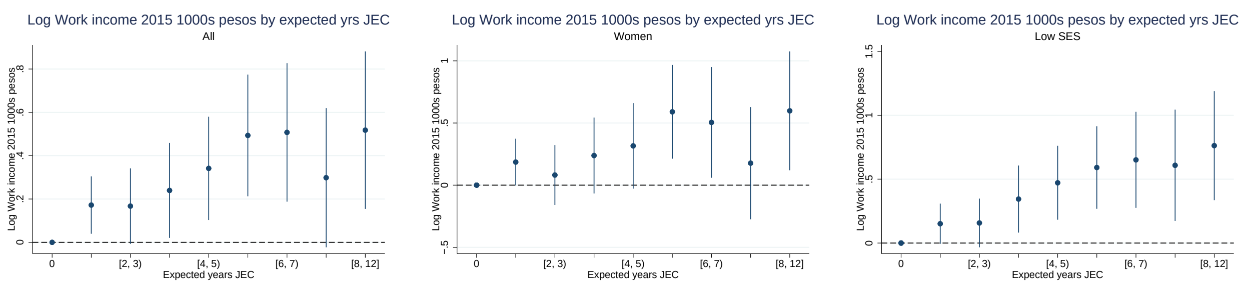
Notes: Figure shows the results from a regression taking the form of Equation (4), but replacing the continuous treatment variable ($\widehat{JEC_{cm}}$) with nine indicators for 0, (0,1], (1,2]...(4,5], (8, 12] years of expected JEC and plotting each of the JEC treatment variables. Vertical lines denote 95 percent confidence intervals clustered by city of birth. Control variables include gender, a quadratic in age, indigenous status, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as survey year, municipality, and cohort-by-region fixed effects. Expected JEC calculated from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2009-2015 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-36 years old at the time of survey. See text for details.

Figure 5: Employment



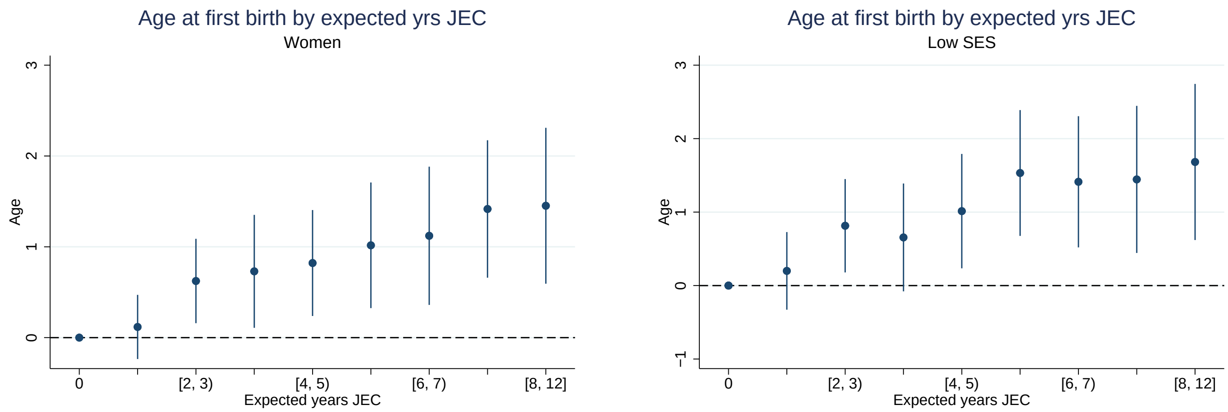
Notes: Figure shows the results from a regression taking the form of Equation (4), but replacing the continuous treatment variable ($\widehat{JEC_{cm}}$) with nine indicators for 0, (0,1], (1,2]...(4,5], (8, 12] years of expected JEC and plotting each of the JEC treatment variables. Vertical lines denote 95 percent confidence intervals clustered by city of birth. Control variables include gender, a quadratic in age, indigenous status, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as survey year, municipality, and cohort-by-region fixed effects. Employment defined as having earned income greater than 600 pesos (approximately \$1) in the past month. Expected JEC calculated from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2009-2015 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-36 years old at the time of survey. See text for details.

Figure 6: Earnings



Notes: Figure shows the results from a regression taking the form of Equation (4), but replacing the continuous treatment variable ($\widehat{JEC_{cm}}$) with nine indicators for 0, (0,1], (1,2]...(4,5], (8, 12] years of expected JEC and plotting each of the JEC treatment variables. Vertical lines denote 95 percent confidence intervals clustered by city of birth. Control variables include gender, a quadratic in age, indigenous status, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as survey year, municipality, and cohort-by-region fixed effects. Expected JEC calculated from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2009-2015 CASEN surveys. Log earnings defined as log earnings (in thousands of inflation-adjusted pesos), plus one, in order to account for individuals with no earnings. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-36 years old at the time of survey. See text for details.

Figure 7: Age at first birth



Notes: Figure shows the results from a regression taking the form of Equation (4), but replacing the continuous treatment variable ($\widehat{JEC_{cm}}$) with nine indicators for 0, (0,1], (1,2]...(4,5], (8, 12] years of expected JEC and plotting each of the JEC treatment variables. Vertical lines denote 95 percent confidence intervals clustered by city of birth. Control variables include gender, a quadratic in age, indigenous status, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as survey year, municipality, and cohort-by-region fixed effects. Expected JEC calculated from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2009-2015 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-36 years old at the time of survey. See text for details.