Essays on Labor and Development Economics

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Abstract

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This dissertation consists of three separate essays on topics in labor and development economics. The first chapter examines the impact that the increased demand for foreign players in US Major League Baseball(MLB) has had on male youth's educational attainment in the Dominican Republic. I use a triple difference strategy (DDD) and exploit the expansion of athlete visas by the US government as an exogenous source of variation. Contrary to concerns expressed by journalists and international policy researchers about the negative impacts of MLB's overseas player development, my findings suggest an absence of meaningful negative effects, ruling out a decrease in schooling greater than 0.07 years.

The second chapter studies the short-term effects of an unconditional transfer on the labor supply in Seongnam city, Korea. In 2016, Seongnam started a "Youth Dividend" program, which paid out gift vouchers of 1,000,000 won (USD 950) to all of its 24-year-olds. The transfer differs from other programs in that it is explicitly unconditional and targets a specific age. Using data from the Local Area Labor Force Survey and the synthetic control method, I show that the unconditional transfers had no effect on the recipients' labor supply at neither the extensive nor intensive margin.

The third chapter focuses on the relationship between statutory work-hour reductions and labor supply. Reducing the number of working hours and improving work-life balance has been an important challenge for industrialized economies. In July of 2018, South Korea

lowered its maximum working hours from 68 hours a week to 52 hours. The policy reduced the standard hours at different times according to industry and firm size. I take advantage of this quasi-natural experiment setting to identify the impact of standard hour reductions on working hours and employment. Using a triple difference approach, I find that female workers in affected firms worked 3.59 hours more per week than those in the control firms, but that there was no significant difference for male workers. My findings show no significant relationship between work-hour reductions and job creation.

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DEDICATION

To my husband, Gilho, and my daughter, Chloe.

Chapter 1

THE EFFECTS OF THE US ATHLETE VISA POLICY CHANGE ON DOMINICAN BOYS' EDUCATION

1.1 Introduction

The present value of lifetime earnings in the source country, as compared to the destination country, guides workers' immigration decisions. When there is an increase in demand for immigrant workers with certain skills in the destination country, it raises the expected returns for those workers with such skills in source countries. In particular, when the destination country enjoys a higher level of economic development than the source country, the increased prospect of immigration if in-demand skills are acquired may change individuals' human capital accumulation in the source country. In this regard, immigration policy changes that are based on a destination country's labor demand affect the human capital accumulation decisions of the individuals in source countries by incentivizing acquisition of a particular skill over others.

Economic theories regarding immigration decisions explain why there is much controversy surrounding the recruitment practices by MLB academies in the Dominican Republic. There is a tradeoff relationship between a career in baseball and education for boys who pursue their baseball dreams in the Dominican Republic. Figure 1.1 illustrates the education path of a male youth considering a baseball career in the Dominican Republic. To secure a spot in one of the MLB academies, boys in the Dominican Republic leave school between ages 11-14 and join informal training programs ran by local independent trainers. Given the competitive nature of the recruitment process, most of the boys do not get the opportunity to play in an official MLB academy, and even less make it onto a US Minor League Baseball (MiLB) roster. The Dominican children left behind gave up their education to play baseball,

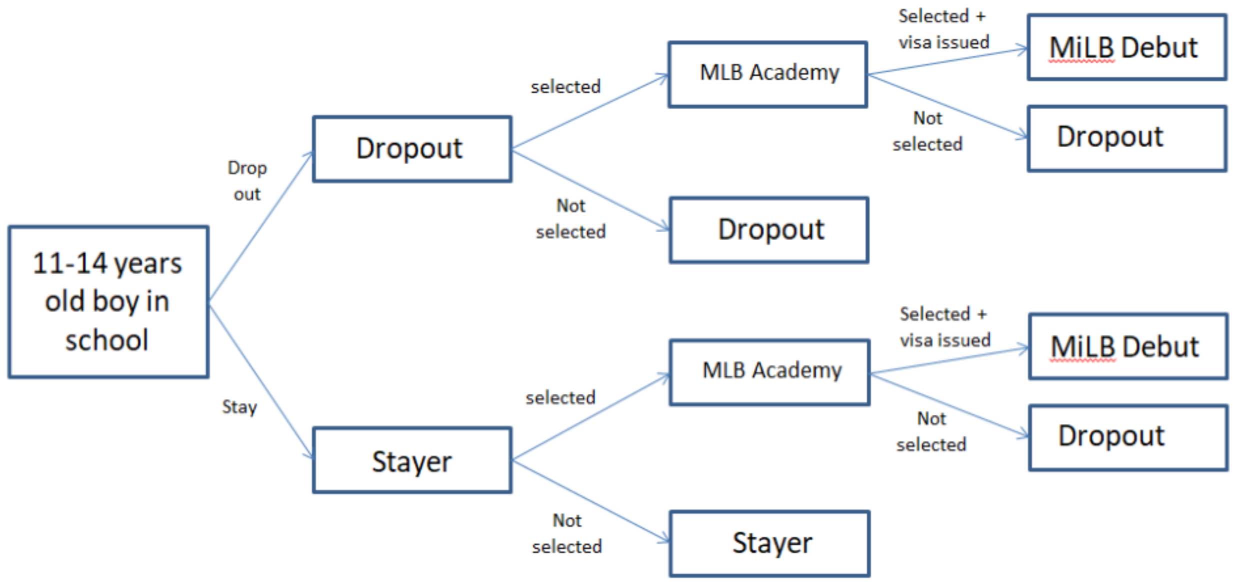

Figure 1.1: Education Path of a Dominican Boy Considering Baseball Career

which leaves them uneducated and untrained for anything besides baseball once they leave the training system.

Journalists, education policy-makers, and lawyers have raised concerns over Dominican boys' welfare and the MLB's player development business in the Dominican Republic. For instance, Zirin (2005) raises concerns about Major League Baseball "harvesting talent on the cheap with no responsibility for who gets left behind." Ghosh (2014) points out factors such as "rampant poverty, few economic opportunities for its poor and working class, a deeply entrenched baseball culture and, now, a strong connection to Major League Baseball through an efficient network of training academies" that have turned the Dominican Republic into a "giant incubator for baseball players." Lisman (2019) cautions against this phenomenon by noting that the "average path a low-income Dominican boy takes towards a highly unlikely baseball career is a well-charted trajectory towards certain socio-economic poverty." To eliminate the tradeoff between education and baseball in the Dominican Republic, lawyers have proposed self-regulation and institutional reform on the part of MLB(Spagnuolo (2003);

Wasch (2009)).

Despite the criticism over MLB's operations in the Dominican Republic, there are relatively few quantitative assessments to evaluate the validity of these concerns. Wasch (2009)'s analysis is the only work to my knowledge that attempts to make a quantitative assessment. Based on the low performance of Dominican boys in terms of school enrollment rate, primary education completion rate, and persistence to the fifth grade when compared to girls, Wasch (2009) argues that families send their boys to the ball fields rather than to school.

However, there are opposing effects from the two main channels through which the US athlete visa expansion affects the educational attainment of Dominican boys. First, as previous literature has pointed out, the visa expansion may encourage more boys to drop out of school to play baseball.[1] The increased possibility of playing baseball in the US lowers the returns from schooling relative to the returns from a baseball career. Human capital theory predicts that individuals invest less in schooling when they expect low returns on their education. That is, Dominican boys will drop out of school to practice baseball in order to increase their chance of getting into the MLB academies which, in turn, maximizes their expected lifetime earnings.

Second, the increased number of local jobs created by MLB academies may positively affect household income. Jobs created by MLB academies extend beyond those directly related to the construction and operation of such facilities. As a spillover effect, MLB academies create additional jobs, including local businesses such as hotels, restaurants and private baseball academies.[2] With an increase in household income, parents may invest more

[1] Black et al. (2005) study the effect of the Appalachian coal boom on high school enrollments during the 1970s, which increased the earnings of high school dropouts relative to those of graduates. They estimate that a long term 10% increase in the earnings of low-skilled workers could decrease high school enrollment rates by as much as 5-7%. If I use the change in proportion of Dominican players in MiLB between 2006-2010, which increased from 11.27% to 12.32%, as a rough measure of increased possibility of Dominican players playing baseball in the US, and assume everything else(signing bonus, US minor league salary, time spend in minor league) to be constant, visa expansion increased expected earnings of a baseball player by 9.29%. Using the estimates from the study, 9.29% increase may decrease school enrollment rate by 4.6-6.5%.

[2] Based on interviews with a MLB academy official and a local community officer in the Dominican Republic, Spagnuolo (2003) documents how communities that host the academies benefit from MLB's

in their children's education, potentially leading to a decline in the male dropout rate. On the other hand, male dropout possibilities could go up if the increased demand for labor includes a demand for child labor. In developing countries, if children have job opportunities, they are more likely to drop out of school to support their family.

Since the MLB's increased demand for Dominican baseball players and the ensuing visa expansion together create effects that move in opposite directions, the effect on Dominican boys' educational attainment becomes indeterminate. The sign of the overall effect depends on the perceived returns for a baseball career in the US as well as the income elasticity of education. The theoretically ambiguous effects of changes from the US athlete visa expansion prompts my empirical investigation. Following Duflo (2001) and Muralidharan and Prakash (2017), my identification strategy is motivated from the fact that the date of birth and the municipality of birth jointly determine a boy's exposure to the impact of the visa expansion. Contrary to concerns expressed by journalists and policy researchers, my results rule out the existence of effects greater than a mere 0.07 years decrease in Dominican boys' educational attainment.

My paper relates to studies that document the development impacts of immigration. Recent studies have found that the human capital accumulation decisions of residents in source countries evolve to correspond with the immigrant selection criterion in destination countries. Chand and Clemens (2008) find evidence of increased tertiary education completion in Fiji in response to the selection criteria set by Australian and New Zealand immigration policies that emphasize education. Shrestha (2017) documents an increase in educational investments among Nepalese men after the British Army introduced education as a selection criterion when recruiting. Khanna and Morales (2017) finds that the H-1B program in the US induced Indian workers to switch to computer science(CS) occupations, increasing the CS workforce and raising overall IT output in India.

presence. Specifically, local entrepreneurs and motor-taxi drivers benefit from increased business generated by baseball games which are well attended by locals. Figure 1.2 shows that the number of games in Dominican Summer League increased by an average of 140 games per year after the visa policy change.

However, the spillover effects in my paper exhibit a different mechanism. Whereas changes in the immigration policies of destination countries encouraged human capital accumulation in Fiji, Nepal and India, it promotes the opposite among the boys in the Dominican Republic. In this regard, my paper is closely related to existing literature documenting the negative spillovers that migration policies have on a source country's labor market decisions. For example, Theoharides (2020) shows that when Japan introduced a policy change that imposed barriers on the migration of Overseas Performing Artists (OPAs) from the Philippines, child labor increased and more individuals engaged in short-term work in Philippines.

The remainder of this paper proceeds as follows : Section II discusses the background, Section III presents the data, the identification strategy and estimating equations, Section IV reports results and Section V concludes.

1.2 Background

1.2.1 US athlete visa policy change

In December of 2006, the US Congress passed the Creating Opportunities for Minor League Professionals, Entertainers, and Teams through Legal Entry Act of 2006 (COMPETE Act of 2006), which expanded the P-1 nonimmigrant visa classification and allowed international athletes and coaches to qualify. Previously, foreign baseball players who wanted to play professional baseball in the US had to enter the country as part of the H-2B seasonal worker visa program. Since there was a cap on the number of H-2B visas available each year, MLB teams used to be limited in their ability to recruit Dominican players.[3] The COMPETE Act of 2006 removed the numerical cap on the number of visas given out to foreign athletes by switching their visa category from H-2B to P-1.

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[3]Congressional record reports this has been a problem in years 2004 to 2006 because the annual H2-B cap of 66,000 visas has been met in the first few months of the fiscal year, leaving foreign minor league athletes with severely limited opportunity to enter the United States (GovTrack.US (2006)).

1.2.2 MLB academies in Dominican Republic and the visa policy change

Unfortunately, there is no data publicly available on the number of players in MLB academies in the Dominican Republic.[4] However, since MLB academies make up the Dominican Summer League(DSL), the number of games, teams and player rosters within the DSL provide a good estimate of the size of the MLB operation in the Dominican Republic. Figure 1.2 shows that both the number of games and teams have increased from the pre-visa expansion period. However, the data for the number of players only shows a slight increase(3.1%) after the visa expansion, because each teams' roster is capped at 35. Moreover, not all players in the MLB academies appear on the roster since recently signed boys spend their first year in an informal league comprised of recent signees only.

Figure 1.3 reports the number of Dominican players in the US Minor League before and after the visa expansion. The total number of Dominican players in the US domestic Minor League has increased from 800-900 players in pre policy years to over 1,000 players immediately following the visa expansion.[5] Specifically, the total number of Dominican players in 2010 increased by 21.2% from 2006, a year immediately preceding the implementation. During the same period, the number of Dominican players in the US domestic Rookie League also increased by 18.0%. Only the best players from MLB academies graduate to the US domestic Rookie League, while the rest gets released. Increases in the number of Dominican players in the US domestic Rookie League indicate that more players are indeed being picked up in the academies to play in the US. Since the size of the Dominican Summer league has stayed more or less the same during this time, more Dominican rookies in the US indicates that MLB academies are recruiting Dominican boys with greater frequency, training them for a shorter period of time, and sending them off to the US at a faster rate. Whether this

[4]According to Inter-American Dialogue March 2019 blog post, it is estimated that 2,000- 4,000 Dominican boys participate in contracted training activities at the Dominican academies each year. In addition, there are roughly 1,500 local independent academies that act as feeder programs to MLB academies. The annual estimate for boys in these programs range from 20,000 to 80,000.

[5]Minor league Baseball consists of Triple A, Double A, Class A advanced, Class A, Class A short season, Rookie advanced, Rookie Leagues.

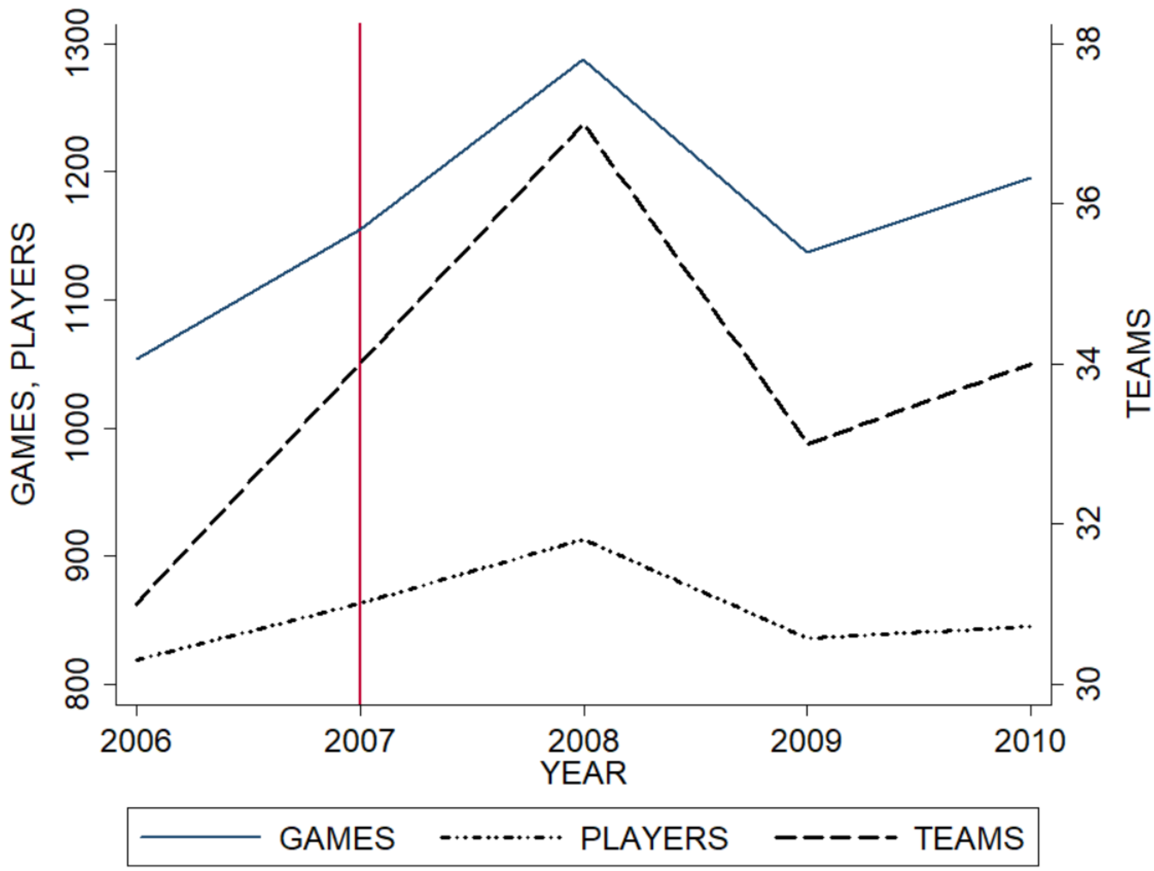

Figure 1.2: Number of Teams, Games and Dominican Players in the Dominican Summer League

Source: Author's calculations using data from www.baseball-reference.com.

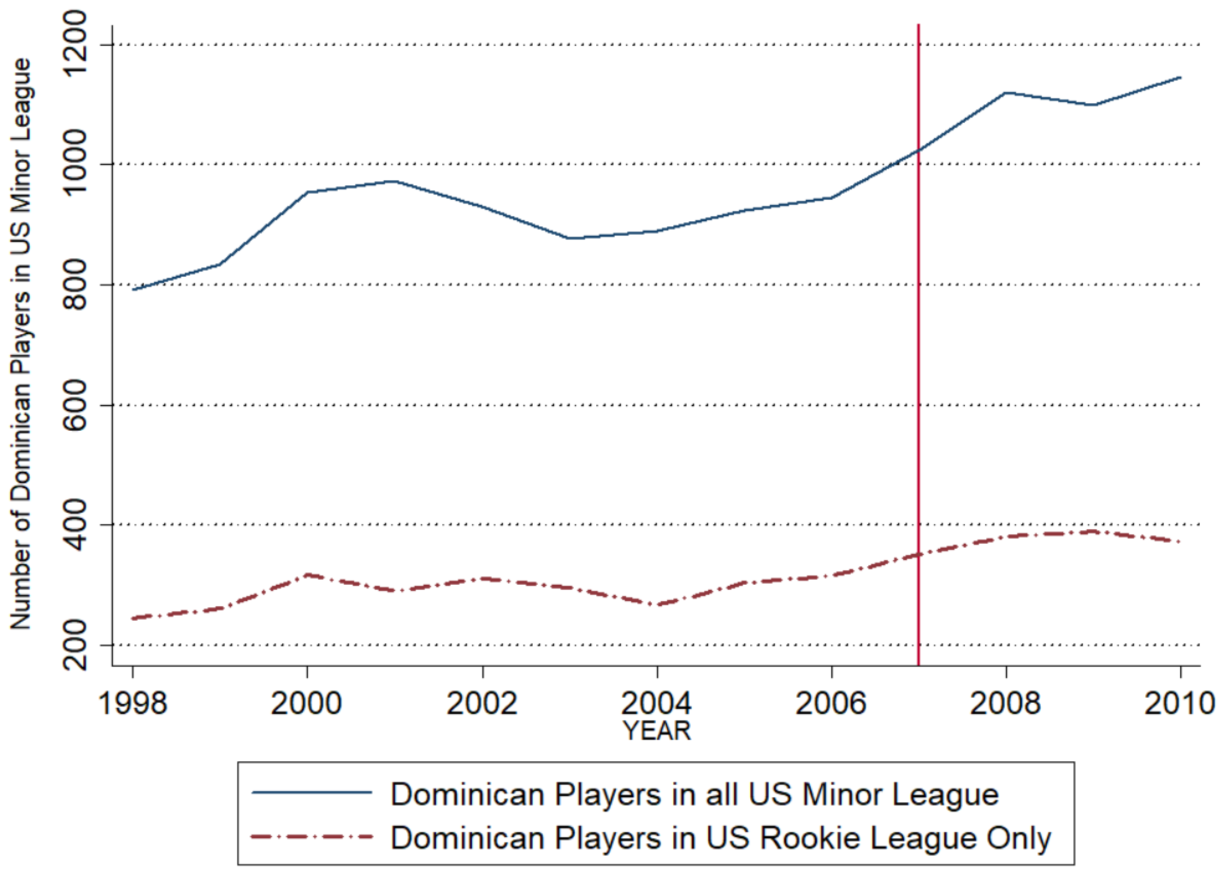

Figure 1.3: Number of Dominican Players in US Minor League Baseball

Source: Author's calculations using data from https://www.baseball-reference.com/

increase provides a big enough of an incentive to induce an average Dominican boy to drop out of school and dedicate his life to baseball training is an empirical question.

1.3 Data, Identification Strategy, and Estimating Equations

1.3.1 Data

My main data source is the Dominican National Population and Housing Census of 2002 and 2010 from the IPUMS-International database Center (2002, 2010). The data includes each individual's educational attainment and current schooling status as well as household characteristics. The timing of the census is ideal for my analysis because the visa policy change took effect in 2007, roughly splitting the time between census dates into two equal periods for analysis (5 years of pre-intervention and 4 years of post-intervention).

1.3.2 Identification Strategy

I compare the changes in male education between the treatment group of boys with the control group. I define the treatment group using two dimensions. The first dimension is the boy's age. I define the treatment age cohort as boys who were in primary school[6] during the policy intervention period(2007-2010). The second dimension is the boy's place of birth. The treatment municipality cohort are boys born in municipalities close to MLB academies. Putting the two dimensions together, boys in the treatment group are those who were young enough for academy tryouts and exposed to the environment where intense player development and recruitment took place.

The two-dimensional identification strategy is motivated from the two key characteristics of the MLB academies. First, MLB scouts must wait until a Dominican boy turns 16 years old before they can sign him for a team's baseball academy. However, these boys typically drop out of school between the ages of 11 and 14 to start their training with local independent agents. Therefore, I define my treatment group as boys aged 11-14 in 2007 who were young enough to prepare for MLB academy tryouts. All boys born in 1992 or before, on the other hand, were 15 or older in 2007. They attended schools without such opportunities opening up throughout their elementary school education and were too old to prepare for MLB academies when the visa expansion took effect.

One may argue that the age cut-off for treatment and control group should be 15 since the visa expansion may still induce boys in secondary school, aged 15, to drop out of school and prepare for tryouts. I argue that this is unlikely because, only about 25%-30% of youths

[6]Education system in Dominican Republic consists of three stages, primary(grade 1- grade 8, for children 6 to 14 years olds), secondary(grade 1 to grade 4, 15 to 18 years olds) and higher(university).

complete secondary school in the Dominican Republic (Oficina Nacional de Estadísticas (2002, 2010)). Jensen (2010) finds that perceived returns to secondary school are extremely low among eighth-grade boys in the Dominican Republic.[7] Jensen (2010) lists poverty, credit constraints, high discount rates and low perceived returns to schooling as potential explanations for this puzzle. Since there are various reasons that it is more common to drop out of secondary school than it is to attend aside from preparing for a baseball career, including 15-16 years olds in the treatment group can lead to overestimation of the true effect.

Second, MLB academies in the Dominican Republic are clustered along the southeast coast of the island(see Figure 1.4). 27 of the 30 MLB academies are located between Boca Chica and San Pedro Macorís. Only Oakland(Villa Mella), St.Louis (Villa Mella) and San Diego (in San Cristóbal) have academies located outside the area, but they are still fairly close to other academies. Figure 1.5 shows that local independent baseball academies are predominantly found in areas where MLB academies are located. Together with local independent baseball academies, MLB academies form a "baseball town" near their establishments. The intensity of competition surrounding player development and recruitment must be stronger for municipalities with these baseball towns. I therefore define the treatment and control municipality based on the distance from MLB academies. Specifically, municipalities that are located within 30km of MLB academies are classified as treatment municipalities(see Figure 1.6). [8] Moreover, since it is typical for an 11-14 year old boy to live at home with his

[7]Interestingly, Jensen (2010) finds that in Dominican Republic, mean earnings or workers who complete secondary school are over 40% greater than those of workers who only complete primary school. However, only about 25%-30% of youths complete secondary school despite the fact that 80%-90% complete primary schooling (Oficina Nacional de Estadística, República Dominicana 2002). According to the National Labor Force Survey (ENFT), average years of education of the population aged 15 years and over, is 7.5 years in 2002 and 8.3 in 2010, indicating low secondary school completion rate persisted throughout the period under study.

[8]Possible alternative specification is to use the birth places of current minor league Dominican players as treatment municipality. Results from this alternative specification is similar to my preferred model. The estimate is statistically insignificant and falls in at a large confidence interval, ruling out effects larger than 0.5 years of decreased schooling among treatment age cohorts in treatment municipalities. See tables A.2 and A.3. in the appendix for detailed results.

Figure 1.4: Location of MLB Academies in the Dominican Republic

family rather than to move independently across municipalities to live by himself, I argue that the moves between treatment and control groups are limited.

The main challenge of identifying the visa expansion impact is that it was implemented during a time of economic growth in the Dominican Republic. GDP increased from 27.36 billion US dollars in 2002 to 53.86 billion US dollars in 2010 (The World Bank Group (2002, 2010)). Moreover, there was a change emphasizing education at the national level. Government expenditure on education as a % of GDP also rose from 1.91% in 2002 to 2.05% in 2007 (UNESCO Institute for Statistics (UIS) (2002, 2010)). The Ministry of Education of the Dominican Republic increased teacher training during this time, resulting in a steady rise

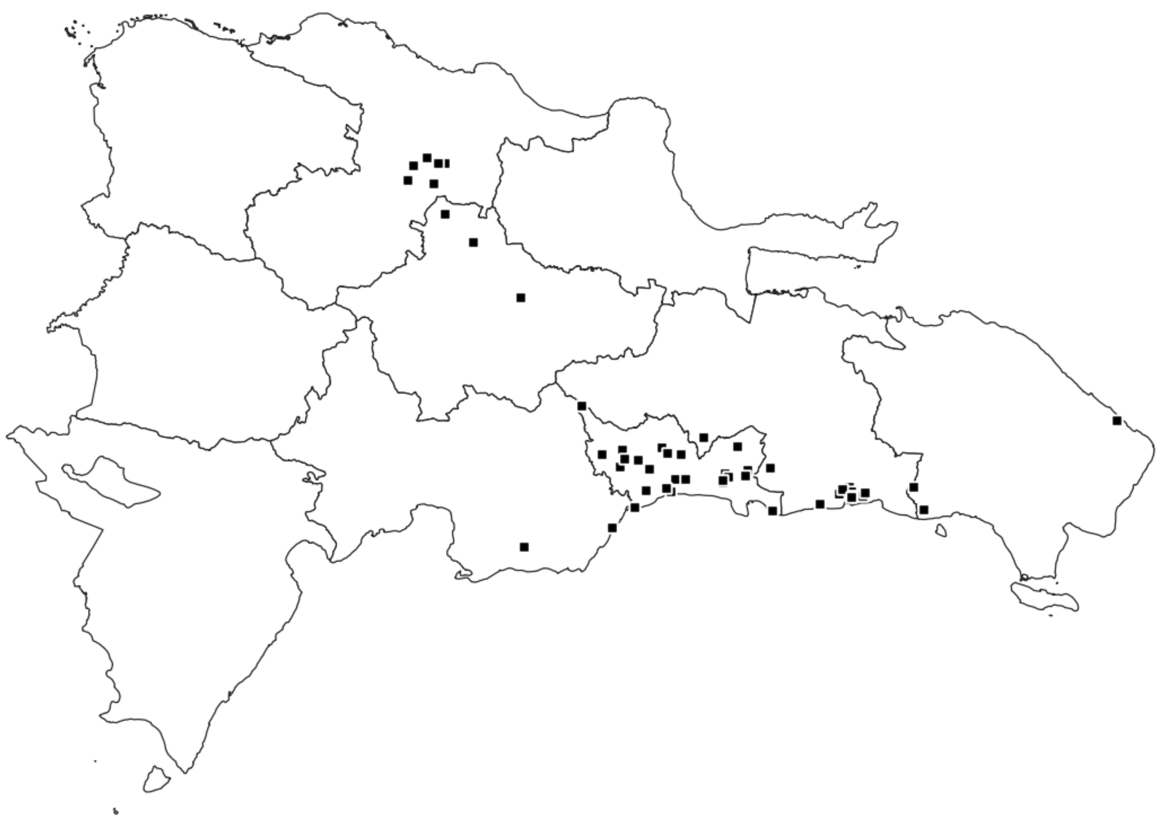

Figure 1.5: Location of Local Independent Baseball Academies in the Dominican Republic

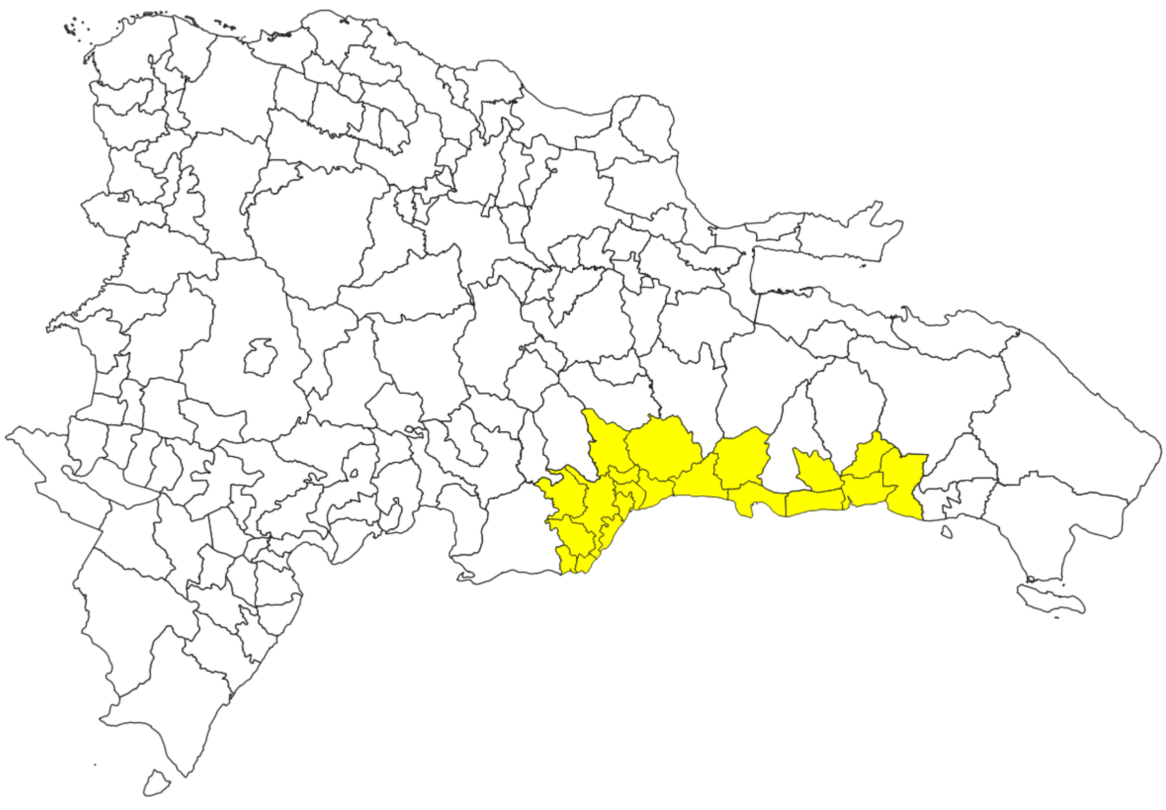

Figure 1.6: Treatment Municipalities in the Dominican Republic

of well-trained teachers from 75% in 1999, to 85% in 2012 (UNESCO Institute for Statistics (UIS) (2002, 2010)).

Therefore, results from a before-and–after comparison of boy's educational attainment (difference in difference) between the treated and control groups might simply reflect these broader trends and not be caused by the visa expansion. Moreover, the difference in education between the treated-age and control-age groups within an MLB municipality could be correlated with age-varying unobserved variables. I address the identification challenge by using a triple difference strategy. The DDD estimator removes fixed time trends, fixed municipality effects and age-varying characteristics that could directly affect educational attainment (Imbens and Wooldridge (2007)).

I illustrate the basic idea behind the identification strategy in Table 1.1. The data is split into four cells. The columns split data by time : pre-policy change versus post-policy change. The rows split the data by municipality : treated versus control. Each cell shows the mean years of schooling for the group, along with the standard error and the number of observations. Note that since my dataset is limited to observations from the 2002 and 2010 census only, my treatment group(boys aged 11-14 in 2007) converts to boys aged 14-17 in 2010. Likewise, my control group(boys 15-18 years old in 2007) converts to boys 18-21 years old in 2010.

Within the treatment municipality, the years of schooling for treated age cohorts increased by an average of 0.87 years after the visa expansion. Meanwhile, years of schooling for the control age cohort went up by 0.89 years. The difference-in-difference estimation shows that the years of schooling in the treatment age group decreased relative to the control age group by 0.03 years on average (statistically insignificant). This change of DD_{TM} is the "within-treatment municipality" difference-in difference estimate of the impact of visa expansion. Likewise, the "within-control municipality" difference-in difference estimate is DD_{CM}= -0.08. I then report a "triple difference"(DDD) estimate of the effect of the visa expansion at the bottom of Table 1 by putting together the upper and lower panels. This estimate is $DDD = DD_{TM} - DD_{CM} = 0.05$. It is statistically insignificant, failing to reject the null hypothesis that visa expansion has no effect on the years of schooling attained by young Dominican boys. Although imprecisely estimated, the results suggest that a boy young enough, and born in an MLB municipality, received an average of 0.05 more years of education. These results contradict the claims made in journals and newspapers.

Table 1.1: Mean Years of Schooling

Dependent variable : Years of Schooling			
	Before Visa Expansion (2002 Census)	After Visa Expansion (2010 Census)	Difference
Panel A. Treatment Municipality			
Treated age: male aged 14-17	7.1045	7.9706	0.8661***
	(0.0254)	(0.0230)	(0.0856)
	[11,241]	[14,235]	[25,476]
Control age: male aged 18-21	9.3434	10.2372	0.8938***
	(0.0332)	(0.0291)	(0.1096)
	[9,752]	[12,975]	[22,727]
Difference	-2.2389***	-2.2666***	DD_{TM}= -0.0277
	(0.1030)	(0.0962)	(0.0682)
	[20,993]	[27,210]	[48,203]
Panel B. Control Municipality			
Treated age: male aged 14-17	6.4679	7.4101	0.9422***
	(0.0192)	(0.0189)	(0.0389)
	[19,833]	[22,137]	[41,970]
Control age: : male aged 18-21	8.3492	9.3731	1.0239***
	(0.0256)	(0.0243)	(0.0487)
	[17,614]	[20,787]	[38,401]
Difference	-1.8813***	-1.9630***	DD_{CM}= -0.0817
	(0.0385)	(0.0395)	(0.0386)
	[37,447]	[42,924]	[80,371]
DDD Estimator			DDD = 0.0540
			(0.0774)
			[128,574]

Notes : Municipalities that are located within 30km distance to the MLB academies are classified as MLB municipalities. The standard errors(clustered by municipality) in parentheses, number of observation in square brackets. ***Significant at the 1 percent level. **Significant at the 5 percent level.

There could be two potential reasons for an insignificant result. First, it could be that the visa expansion alleviates binding constraints for some boys, but it does not affect an average boy in the Dominican Republic. This is possible because MLB recruitment is an extremely selective process even at low levels like baseball academies in the Dominican Republic. Even though the increase in the number of visas available allows MLB clubs to ship more baseball players to the US, thereby creating more space in the MLB academies, the effect is not big enough to incentivize the average Dominican boy to drop out of school to pursue baseball.

Second, the increased income for those residing in municipalities with MLB academies may offset the school dropout effect, attenuating the negative effect of visa policy on education. If the income elasticity of demand for education is big, then it is less likely that the boys' willingness to substitute their education for a baseball career will dominate. To separately measure these effects, data on individual household income, parents' involvement in their child's education, and also detailed local labor market characteristics at the municipality level would be needed. In the absence of this data, I instead control for household wealth in the later analysis by presenting specifications such as home ownership and urban residency.

1.3.3 Parallel Trends

The identification relies on the parallel trend assumption that, in the absence of treatment, the schooling decisions for treatment and control groups would have evolved similarly. To evaluate the validity of this identification assumption, I compare the change in school attendance of the treatment and control age cohorts across the treatment and control municipalities in the four years prior to the visa policy change. Over the period 2002-2006, for boy i in year t:

$$
\begin{aligned}
1(attendingschool)_{it} = {} & \gamma_0 + \gamma_1 \cdot T_{it} \cdot preCOMPETEyear \cdot MLB_{it} \\
& + \gamma_2 \cdot T_{it} \cdot preCOMPETEyear \\
& + \gamma_3 \cdot preCOMPETEyear \cdot MLB_{it} \\
& + \gamma_4 \cdot T_{it} \cdot MLB_{it} + \gamma_5 \cdot T_{it} + \gamma_6 \cdot preCOMPETEyear \\
& + \gamma_7 \cdot MLB_{it} + \varepsilon_{it}
\end{aligned}
\tag{1.1}
$$

where $1(attendingschool)_{it}$ is an indicator for boy i attending school in year t. MLB_{it} is a dummy variable indicating whether the individual is born in an MLB municipalities. T_{it} is a dummy indicating whether the individual belongs to the treated cohort (young boys, being aged 14 to 17 in 2010). $preCOMPETEyear$ is a measure of pre trends before the visa policy change, which codes year 2002, 2003, 2004, 2005 and 2006 as -4, -3, -2, -1 and 0 respectively. Table 1.2 shows that the coefficient on the triple interaction term is close to zero(-0.001) which falls in at large confidence interval [-0.02, 0.01]. The estimate on the pre-trends thus fail to reject that there is no difference in the trends before the visa expansion across the control and treatment age cohorts in control and treatment municipalities.

1.3.4 Estimating Equations

The above identification strategy can be generalized to a regression formulation. I begin my analysis with a before and after intervention period comparison within the treated age cohort, in each treated municipality,

$$
y_i = \gamma_0 + \gamma_1 \cdot Post_i + \gamma_2 \cdot X_i + \varepsilon_i \tag{1.2}
$$

where y_i is the outcome variable of interest corresponding to boy i of an age between 14-17, born in an MLB municipality. $Post_i$ is an indicator for observation after the visa policy change. That is, observations from the 2010 census are 1, and 0 for 2002. X_i is a set of controls for household level characteristics. Although the coefficient γ_1 is intended to give the effects of the 2007 visa policy change among the treatment age cohort of boys born in municipalities with MLB academies nearby, it may be biased. For example, growth in GDP

Table 1.2: Testing the Parallel Trends Assumption

Dependent variable : 1[attending school]	
Point estimate	-0.0010
Lower bound 95% conf. interval	-0.0154
Upper bound 95% conf. interval	0.0133
Number of observation	561,602

Notes : Each observation corresponds to the indicator for boy i attending school in year t prior to to visa expansion(2002-2006). Year 2002, 2003, 2004, 2005 and 2006 are coded -4, -3, -2, -1 and 0 respectively. I test for parallel trends in the difference-in-difference across the MLB and non –MLB municipalities in the five-year period. Standard errors are clustered by individual level.

between 2002-2010 may introduce bias to results from simple comparisons over time within the treated age cohort.

In order to remove the bias that could be a result of the trends, I determine what was happening in comparable groups of boys, using an age group that did not experience the visa policy change while in primary school. In 1.2, I estimate the coefficient γ_1 for boys who were born in an MLB municipality but aged 18-21 in 2010. I then take a difference between the differences of the treatment-age and control-age cohorts before and after the visa policy change.

The above identification strategy can be expressed using the difference-in-difference model of the following form:

$$y_i = \delta_0 + \delta_1 \cdot T_i \cdot Post_i + \delta_2 \cdot T_i + \delta_3 \cdot Post_i + \delta_4 \cdot X_i + \varepsilon_i \tag{1.3}$$

where y_i is the outcome variable of interest corresponding to boy i in an MLB municipality.

T_i is an indicator for an observation from the treatment-age cohort. That is, T_i is equal to one if a boy is 14-17 years old in 2010(11-14 years old in 2007). In this setup, the analysis is between the two groups of boys in the treated municipality. I compare the before and after changes of the treatment-age cohort to that of the control-age cohort within the treated municipality. The potential problem with this DD analysis is that other factors unrelated to the visa expansion, such as changes in the emphasis on education at the national level, might affect the schooling decisions of the treatment-age cohort relative to the control-age cohort in treated municipalities.

I also consider a different DD analysis by using a treatment-age cohort in control municipalities as the control group. In this setup, the analysis is between treated and control municipalities. I compare the before and after changes in the treatment-age cohort's education within municipalities where MLB academies are close by to those without academies. This DD specification can be written

$$y_i = \delta_0 + \delta_1 \cdot MLB_i \cdot Post_i + \delta_2 \cdot MLB_i + \delta_3 \cdot Post_i + \delta_4 \cdot X_i + \varepsilon_i \tag{1.4}$$

where y_i is the outcome variable of interest corresponding to boy i in a treatment-age cohort. MLB_i is an indicator for an observation from municipalities that are close to MLB academies. That is, MLB_i is equal to one if the municipality is located within 30km of an MLB academy. The problem with this analysis is that changes in the treatment-age cohort's education might be systematically different across municipalities due to regional and demographic differences rather than the policy change.

I therefore construct a triple difference (DDD) estimate of the impact from the visa policy change by using two control groups simultaneously. The first control group has boys who are in the control-age cohort, while the second control group is comprised of boys who reside in municipalities that do not have MLB academies close by. Boys in non-MLB municipalities serve as a useful control group against the visa policy change because they would have been exposed to all the other changes that were taking place in the Dominican Republic, such as GDP growth and the increased emphasis on education, but they were distanced from the MLB academies. Distance from an MLB academy is a relevant factor because MLB academies are the only place where Dominican boys can get the MLB contract to apply for the visa. Thus, the DDD estimate is immune to municipality-specific shocks as well as age-cohort specific shocks. The triple difference estimate of exposure to the increased number of US visas available is estimated by

$$\begin{aligned} y_i = \beta_0 + \beta_1 \cdot T_i \cdot Post_i \cdot MLB_i + \beta_2 \cdot T_i \cdot Post_i + \beta_3 \cdot Post_i \cdot MLB_i \\ + \beta_4 \cdot T_i \cdot MLB_i + \beta_5 \cdot T_i X_i + \beta_6 \cdot Post_i + \beta_7 \cdot MLB_i + \beta_8 \cdot X_i + \varepsilon_i \end{aligned} \tag{1.5}$$

The coefficient of interest is β_1, the coefficient on the triple interaction term $T_i \cdot Post_i \cdot MLB_i$. The OLS estimate $\hat{\beta}_1$ can be expressed as follows:

$$\begin{aligned} \hat{\beta}_1 = &(\bar{y}_{youngboy,MLB,Post=1} - \bar{y}_{youngboy,MLB,Post=0}) \\ &- (\bar{y}_{oldboy,MLB,Post=1} - \bar{y}_{oldboy,MLB,Post=0}) \\ &- (\bar{y}_{youngboy,nonMLB,Post=1} - \bar{y}_{youngboy,nonMLB,Post=0}) \\ &- (\bar{y}_{oldboy,nonMLB,Post=1} - \bar{y}_{oldboy,nonMLB,Post=0}) \end{aligned} \tag{1.6}$$

The summary statistics in Table 1.3 break down the means of the selected variables by both the age cohort and the MLB municipality cohort. Characteristics at the individual level indicate that boys in the treatment municipality have a greater possibility of going to school for a longer period of time, whereas characteristics at household level are fairly identical across treatment and control municipalities. At the municipality level, treatment municipalities are more urban than the control. As accounted for in my model above, in order to remove fixed municipality effects and time trends, a before and after comparison between treatment and control-age boys within the municipality cohort is needed. Moreover, my triple difference model subtracts the double difference in the non-MLB municipality from the double difference in the treatment municipality to net out any age-varying characteristics that could affect educational attainment.

Table 1.3: Summary Statistics of Individuals in 2002

Municipality	Treatment Municipality		Control Municipality	
Age Cohort	Treatment Age Cohort 14-17 YRS	Control Age Cohort 18-21 YRS	Treatment Age Cohort 14-17 YRS	Control Age Cohort 18-21 YRS
A.Individual Characteristics				
Age	15.4565	19.4372	15.4669	19.4479
	(1.1138)	(1.1222)	(1.1213)	(1.1139)
Attending School	0.9292	0.6968	0.8900	0.6033
	(0.2565)	(0.4597)	(0.3129)	(0.4892)
Years of Education	7.1045	9.3434	6.4679	8.3492
	(2.6923)	(3.2782)	(2.6986)	(3.3921)
Total sample	11,241	9,752	19,833	17,614
B.Household Characteristics				
Family size	5.3030	4.9745	5.4399	5.1262
	(1.9418)	(2.1183)	(2.1404)	(2.3085)
Has access to piped water	0.8533	0.8557	0.8533	0.8557
	(0.3538)	(0.3514)	(0.3538)	(0.3514)
Has access to sewage system	0.7218	0.7394	0.7218	0.7394
	(0.4481)	(0.4390)	(0.4481)	(0.4390)
Has access to internet	0.0560	0.0659	0.0560	0.0659
	(0.2300)	(0.2481)	(0.2300)	(0.2481))
Total sample	8,133	8,507	14,258	15,263
C.Municipality Characteristics				
Distance to MLB academies	11.4717	11.4620	143.1465	142.2618
	(6.1725)	(5.9957)	(61.7541)	(61.0611))
Percentage rural	0.2669	0.2456	0.4493	0.4062
	(0.4423)	(0.4305)	(0.4974)	(0.4911)
Total sample	27	27	198	198

1.4 Results

1.4.1 Educational Attainment Impact

Table 1.4 Panel (b) compares the before and after educational attainment of the treatment and control age cohorts. Difference in difference estimate, δ_1 in estimating equation 1.3 and 1.4, is shown in columns (2) and (3) respectively. When treatment-age boys are compared with control-age boys within the treatment-municipality, the average years of education increased by 0.028 years. When the treatment-age boys are compared across treatment and control municipalities, average years of education decreased by 0.068 years.

Panel (c) reports a "triple difference"(DDD) estimate of the effect of the visa expansion effect. This estimate is close to zero (0.086) and falls in at a large confidence interval [-0.0673, 0.2389], failing to reject the null hypothesis that visa expansion has no effect on the educational attainment of young Dominican boys. From the confidence intervals, I can rule out effects larger than 0.07 years of decreased years of schooling.

Table 1.4: Impact on Years of Schooling of Dominican Boys

Dependent variable : Years of Schooling	(1)	(2)	(3)	(4)
Panel (a) : Difference				
Post	0.439***	0.318**	0.531***	0.514***
	(0.096)	(0.115)	(0.037)	(0.053)
Male Aged 14-17		-2.180***		-1.788***
		(0.094)		(0.046)
MLB Municipality			0.147	0.451**
			(0.110)	(0.178)
Panel (b) : Difference in Difference				
Post ×Male Aged 14-17		0.028		-0.059
		(0.069)		(0.040)
Post ×MLB Municipality			-0.068	-0.165
			(0.097)	(0.115)
Male aged 14-17 ×MLB Municipality				-0.396***
				(0.106)
Panel (c) : Difference in Difference in Difference				
Post ×Male Aged 14-17 × MLB Municipality				0.086
				(0.078)
Control variables				
Household Characteristics	Yes	Yes	Yes	Yes
Observations	25,257	47,659	66,894	127,279

Notes : Household characteristics include home ownership, family size, whether the household is located in a place designated as urban or as rural and whether or not the household has access to piped (running) water, a sewage system, electricity and internet. ***Significant at the 1 percent level. **Significant at the 5 percent level.

Table 1.5 presents the effects from the visa expansion on the primary school completion possibility. My preferred estimate, triple difference result in column (4), is 0.010 and not statistically different from zero. From the confidence interval[-0.0102, 0.0302], I can rule out that the visa expansion reduces the likelihood of completing primary school by an effect larger than one percentage point.

Table 1.5: Impact on Primary School Completion

Dependent variable : 1[completed primary school]	(1)	(2)	(3)	(4)
Panel (a) : Difference				
Post	0.057***	-0.009	0.080***	0.018**
	(0.018)	(0.014)	(0.007)	(0.007)
Male Aged 14-17		-0.250***		-0.245***
		(0.010)		(0.005)
MLB Municipality			0.030	0.039***
			(0.021)	(0.014)
Panel (b) : Difference in Difference				
Post ×Male Aged 14-17		0.078***		0.068***
		(0.007)		(0.007)
Post ×MLB Municipality			-0.017	-0.024
			(0.018)	(0.015)
Male aged 14-17 ×MLB Municipality				-0.005
				(0.011)
Panel (c) : Difference in Difference in Difference				
Post ×Male Aged 14-17 × MLB Municipality				0.010
				(0.010)
Control variables				
Household Characteristics	Yes	Yes	Yes	Yes
Observations	25,257	47,659	66,894	127,279

1.4.2 Robustness

An important consideration when making quasi-experimental evaluations regarding the effect of the visa expansion is a possible contamination of the control group. Contamination occurs when the control group receives some or all of the intervention intended for the treatment group. This may occur with boys in control municipalities if there are local independent baseball academies in those municipalities.[9] Boys in control municipalities might then be exposed to the treatment because local independent baseball academies could "leak" the visa expansion effects by recruiting more boys. If this occurs, it will reduce the magnitude of the point estimate differences between the treatment and control municipalities and underestimate the true effect of the visa policy change. Therefore, as a robustness check, I drop observations from municipalities with local independent academies and estimate the triple difference (DDD) in equation 1.5.

Table 1.6 Panel (c) reports a DDD estimate of the effect of the visa expansion, after excluding observations from non-MLB municipalities with local independent baseball academies. From the large confidence interval [-0.0256, 0.2986], I fail to reject the null hypothesis and rule out effects larger than a 0.03 year decrease in schooling. Likewise, the effect of the visa expansion on the primary school completion possibility, shown in Table 1.7, is not statistically different from zero and I can rule out effects larger than a 0.1 percentage point decrease in the likelihood of completing primary school. These results are consistent with the results found in Tables 1.4 and 1.5.

[9] These are municipalities outside the 30km distance to MLB academies, which are Higüey, Guaymate, Villa Hermosa, La Vega, Jima Abajo, Baní, Yaguate, Villa Altagracia, Los Llanos, Fantino, Santiago, Licey Al Medio, Tamboril, Sabana Iglesia, Monte Plata and Yamasá

Table 1.6: Impact on Years of Schooling of Dominican Boys, Local Baseball Academies Excluded

Dependent variable : Years of Schooling	(1)	(2)	(3)	(4)
Panel (a) : Difference				
Post	0.441***	0.311**	0.562***	0.590***
	(0.098)	(0.114)	(0.045)	(0.056)
Male Aged 14-17		-2.189***		-1.745***
		(0.092)		(0.040)
MLB Municipality			0.144	0.486***
			(0.110)	(0.174)
Panel (b) : Difference in Difference				
Post ×Male Aged 14-17		0.036		-0.102**
		(0.069)		(0.047)
Post ×MLB Municipality			-0.100	-0.248**
			(0.102)	(0.116)
Male aged 14-17 ×MLB Municipality				-0.450***
				(0.102)
Panel (c) : Difference in Difference in Difference				
Post ×Male Aged 14-17 × MLB Municipality				0.136
				(0.082)
Control variables				
Household Characteristics	Yes	Yes	Yes	Yes
Observations	24,967	47,103	55,377	105,430

Notes : Observations from non MLB municipalities with local independent baseball academies are excluded.

Table 1.7: Impact on Primary School Completion, Local Baseball Academies Excluded

Dependent variable : 1[completed primary school]	(1)	(2)	(3)	(4)
Panel (a) : Difference				
Post	0.057***	-0.010	0.081***	0.029***
	(0.018)	(0.014)	(0.009)	(0.007)
Male Aged 14-17		-0.249***		-0.243***
		(0.010)		(0.006)
MLB Municipality			0.028	0.040***
			(0.020)	(0.014)
Panel (b) : Difference in Difference				
Post $\times$Male Aged 14-17		0.079***		0.059***
		(0.007)		(0.008)
Post $\times$MLB Municipality			-0.019	-0.035**
			(0.018)	(0.015)
Male Aged 14-17 $\times$MLB Municipality				-0.007
				(0.012)
Panel (c) : Difference in Difference in Difference				
Post $\times$Male Aged 14-17 $\times$ MLB Municipality				0.02
				(0.011)
Control variables				
Household Characteristics	Yes	Yes	Yes	Yes
Observations	24,967	47,103	55,377	105,430

Notes : Observations from non MLB municipalities with local independent baseball academies are excluded.

1.4.3 Transition

It is possible that equation 1.5 does not fully capture the dynamic response to the policy shock. The visa expansion immediately created more openings in MLB academies as the increased number of visas catered to the pent-up demand for Dominican players in the US.

Reacting to the news that there is availability within the MLB academies, Dominican boys might then raise their expected return on a baseball career. This may lead to rise in the school dropout rate for years following the visa expansion. However, due to the slow diffusion of information about the new visa regime, the impact may be delayed and may keep dropout rates unchanged for an initial period.

In order to trace out the response adjustment path by year, I need annual data on years of schooling for boys in the Dominican Republic by municipality. Unfortunately, there is no such data publicly available and the most recent census data is 2010. I therefore create annual data on years of schooling for Dominican boys before and after the visa expansion based on schooling information given in census data. That is, I construct annual data for the years after the visa expansion (2007, 2008 and 2009) using the 2010 census. For example, an observation from the 2010 census of a 15-year-old boy with 8 years of schooling converts into a 14-year-old boy with 7 years of schooling in 2009, a 13 year old boy with 6 years of schooling in 2008 and a 12 year old boy with 5 years of schooling in 2007. Likewise, I create annual data for the years before the visa expansion (1999, 2000 and 2001) using the 2002 census. I then pair up the years before & after the policy change as shown in Table 1.8.

Table 1.8: Observation Year for 2007-2010 Difference in Differences Analysis Treated(14-17 years old boys) - Control(18-21 years old boys)

DD by Year	Observation year for “Before”	Observation year for “After”
2007 DD	1999	2007
2008 DD	2000	2008
2009 DD	2001	2009
2010 DD	2002	2010

Next, following Muralidharan and Prakash (2017), I estimate equation 1.3 for each before

& after year pair, 1999 & 2007, 2000 & 2008, 2001 & 2009 and 2002 & 2010. Note the 2002 & 2010 pair is used for the DD estimate in the main model. The analysis in this setup is between treatment and control age cohorts in the treated municipality and the result is shown in Figure 1.7 Panel A. Finally, I estimate the same equation for the control municipality and plot results in Figure 1.7 Panel B.

Panels A and B in Figure 1.7 present the nonparametric plots of the DD estimates for MLB municipalities(treatment) and non-MLB municipalities(control), respectively as a function of years since the visa policy change. The plots include bootstrapped 95 percent confidence intervals. Panel C is the main figure of interest which shows the DDD plot. Consistent with the expected response if considering the slow diffusion of information, I see a flat line close to zero for the first 3 years(2007-2009) immediately following the policy, and then a slight downward sloping curve after 2009. However, I only have 4 years of observations after the policy change, which makes it difficult to determine if this is in fact an effect of pent-up demand surfacing after the diffusion of information regarding the visa regime change.

In addition to the year-to-year effects presented above, I show the effects of the visa expansion according to the youth's age in the year 2007. I test for the hypothesis that the younger the boy was in 2007, the more exposure he had to the increased opportunity to play baseball in the US, and thus, is more likely to drop out of school. I test for this hypothesis by estimating δ_1 in 1.4 separately for ages 11 to 18 in 2007. The estimates of δ_1s are plotted in Figure 1.8. δ_1s fluctuate around zero, and are statistically insignificant for all ages. I cannot detect a coherent pattern by age, which further supports that it is unlikely that the visa expansion had a large negative effect on Dominican boys' years of schooling.

1.5 Conclusion

Economic theory predicts that the rise in demand for foreign baseball players in US Major League Baseball increases the expected returns to careers in baseball while relatively decreasing returns to education in the Dominican Republic. Extending this theory, one might predict that the MLB operations in the Dominican Republic encourage Dominican boys to

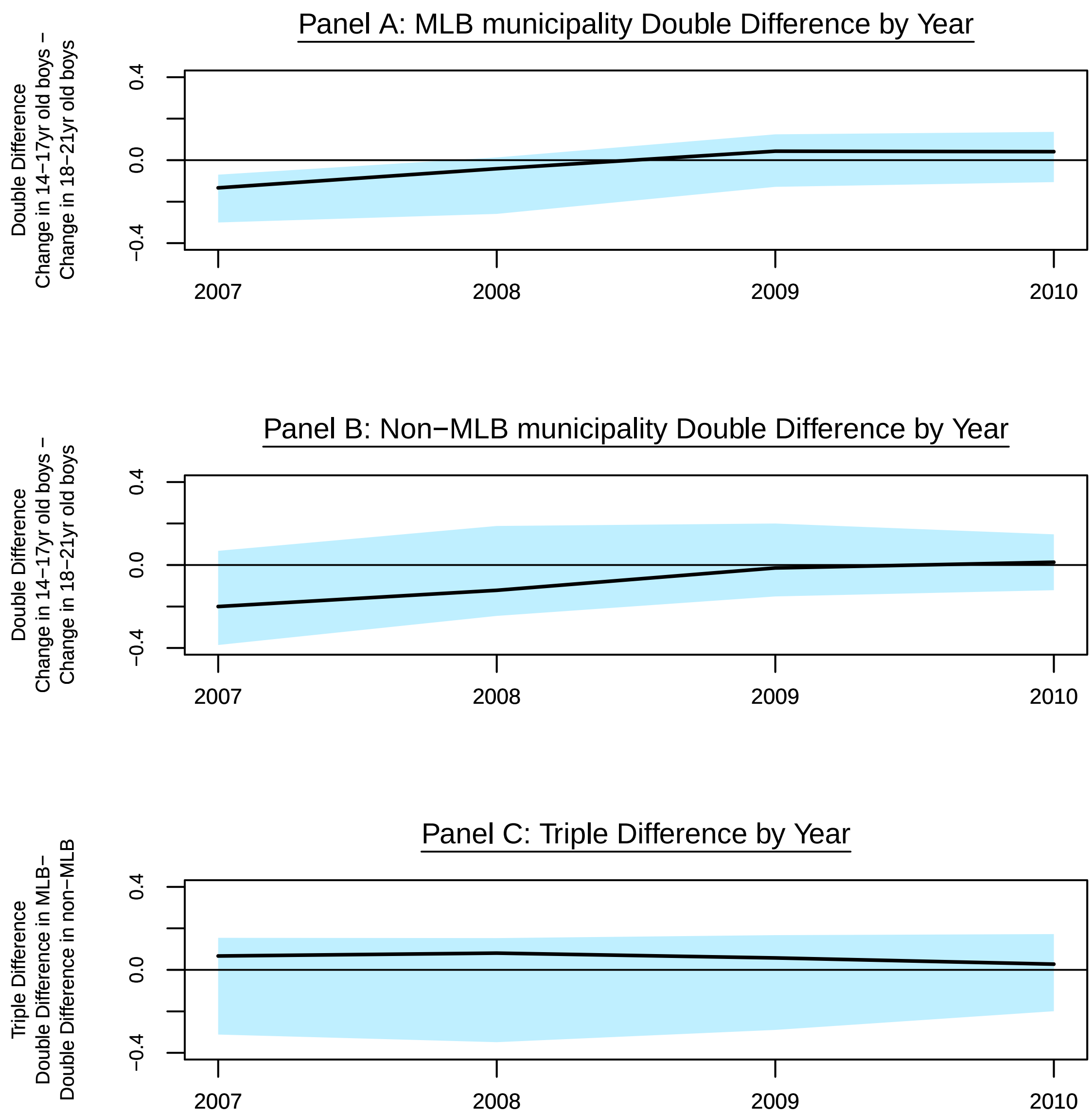

Figure 1.7: Non-parametric Double and Triple Difference Estimates by Year

Notes : I first calculate the municipality-level double difference estimate for each municipality in the sample. I then plot Panel A and Panel B based on a lowess smoothing across the municipality-level double difference estimates by each year after the year the law passed. The triple difference plots the difference between the smoothed double difference plots. To construct the bootstrapped confidence intervals, I calculate DD and DDD estimate from 1,000 resamples of the original data. I restrict the analysis to municipalities where both pre- and post- data exist to make sure I am capturing change in years of schooling at the municipality level.

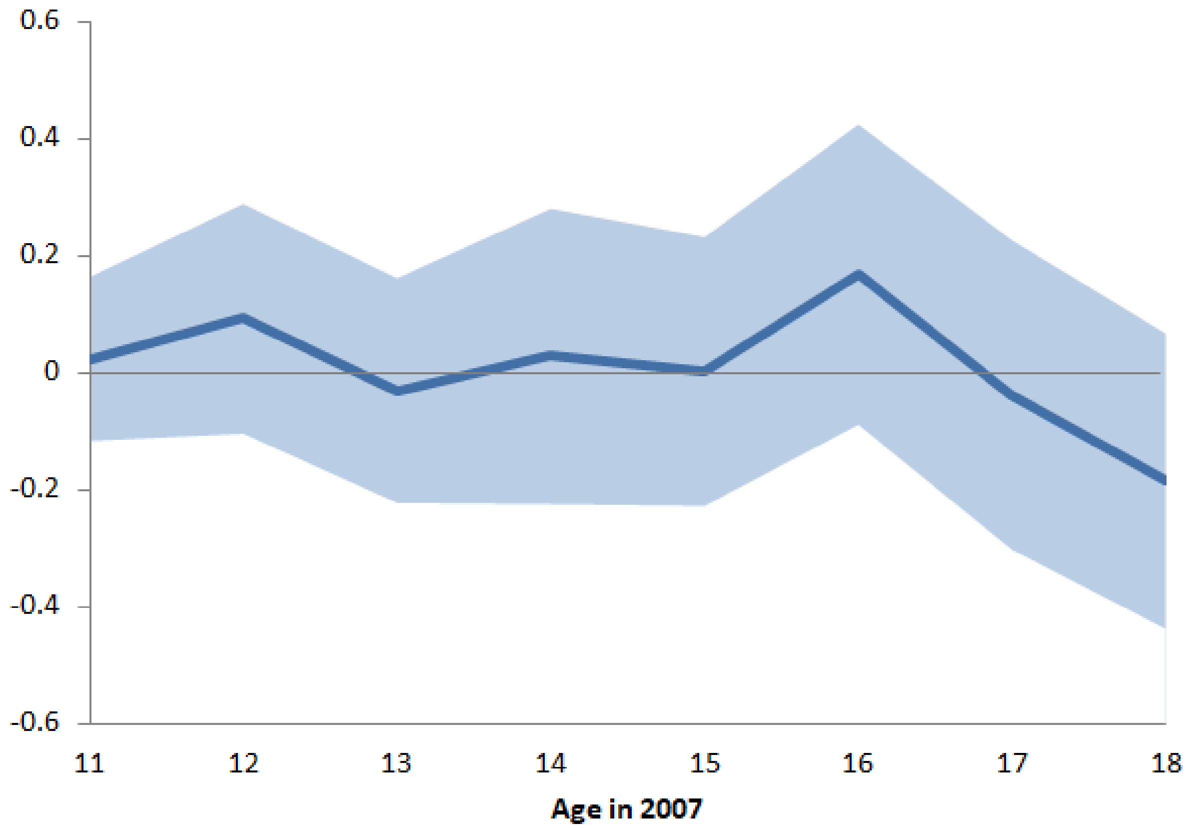

Figure 1.8: Double Difference Estimate of Impact of the COMPETE ACT (by age in 2007)

Notes : The figure plots δ_1s in 1.4 with a 95% confidence interval for subjects aged 11 to 18 in 2007.

drop out of school at a young age in order to play baseball. In this paper, I show that this interpretation may be wrong. My estimates allow me to rule out effects larger than a decrease in schooling of 0.07 years and a 1 percentage point reduction on the likelihood of a boy completing primary school. This could be an indication that the substitution effect is indeed negligible, and boys do not substitute education with baseball. Substantial negative effects on Dominican boys' educational attainment are especially unlikely considering that the selection process for the MLB academies is competitive, with a high minimum skill level required.

However, it may also reflect the fact that the data is not detailed enough and does not cover a sufficient number of periods to identify an underlying relationship. It is possible that the increased number of US visas available to athletes has significantly increased the school dropouts rates of certain groups of boys, for example, boys at the margin in terms of baseball skills. Analysis using large national representative data like the census may have masked the magnitude of such effects on specific groups. Moreover, changes in the athlete visa policy may have different effects in the short-run and long-run. Once the diffusion of information and response to the pent-up demand phase is over, a flow of school dropouts might exhibit different dynamics in the long run. Examining an extended number of periods following the visa expansion, and using baseball-specific population data will provide further insight into this relationship.

Chapter 2

LABOR MARKET CONSEQUENCES OF PROVIDING UNCONDITIONAL TRANSFERS TO YOUNG INDIVIDUALS : EVIDENCE FROM A SOCIAL WELFARE EXPERIMENT IN SEONGNAM CITY, KOREA

2.1 Introduction

A common concern is that unconditional transfers diminish the incentive to work and make recipients lazy. Indeed, economic theory predicts that if leisure is a normal good, recipients will consume more of it as their non-labor income increases. However, there are other potential channels through which unconditional transfers can affect labor market outcomes. The impact also depends on whether the transfer is permanent or temporary, and who the target population is. Given that the theory is ambiguous, and the context of the unconditional transfer plays a determining role, it is important to find empirical evidence.

I investigate empirically the effect of unconditional transfers to young people in Seongnam, South Korea. My goal is to answer the question: How does receiving an unconditional transfer affect a recipient's labor supply? In doing this, I consider factors such as whether a recipient will be more or less likely to take on a job, or, if she already has a job, whether she would cut down on the number of hours worked or work more. Since the transfer was made to a specific group of people residing in a particular city in Korea, I use the synthetic control method by Abadie and Gardeazabal (2003) and Abadie et al. (2010) to estimate the effect. From a control group of cities in Gyeonggi province, I construct the counterfactual "synthetic Seongnam", which consists of a weighted combination of cities that are similar to Seongnam in terms of key labor supply predictors prior to the treatment. The results of my study contradict the view that unconditional transfers discourage people from working.

My result is consistent with previous literature which argues that the income effect from unconditional transfers is negligible. Jones and Marinescu (2018) do not find there to be a negative effect on labor supply from Alaska's cash transfer program. Salehi-Isfahani and Mostafavi-Dehzooei (2018) examine the impact of a national cash transfer program in Iran and find no evidence that cash transfers reduced labor supply, in terms of hours worked or labor force participation. In an overview of quantitative studies on cash transfers, Baird et al. (2018) find that transfers made without an explicit employment focus tend to result in little to no change in adult labor. Additionally, a review on papers regarding universal basic income in developing countries (Banerjee et al. (2019)) provides that, in general, there is no systematic evidence that unconditional transfers discourage work.

Indeed, the theoretical effects of unconditional transfers on work are ambiguous. On one hand, unconditional transfers may reduce the incentive to work. Recipients may spend part of their extra income on leisure or shift to spending time on other productive ways besides work, such as enrolling in school or raising children. The income effect underlying the labor-leisure trade off appears to be most apparent when transfers are large.[1]

However, alternative channels appear to be more relevant to the effects of small, one-time unconditional transfers like Seongnam's Youth Dividend. Unconditional transfers can help young individuals overcome liquidity constraints and therefore change labor supply through a channel other than the income effect. For example, unconditional transfers may fund more extensive human capital accumulation and job search efforts. Individuals may take more time to prepare for a good employment match, but will eventually end up with more stable employment and higher wages.[2] There may also be a human capital depreciation/scarring effect. Recipients will be reluctant to reduce labor if time out of the workforce can cause

[1]Recent studies of lottery winners in Sweden (Cesarini et al. (2017)) and the Netherlands (Picchio et al. (2018)) are in line with this prediction, with winning a prize reducing the number of hours worked and the amount of income earned.

[2]For those individuals who are self-employed, unconditional transfers may fund capital equipment which allows for more businesses and income. However, Haushofer and Shapiro (2016) find that the windfall income in Kenya was used primarily for consumption, assets, and upgrading to metal roofs, rather than for investments that cause changes in labor market outcomes.

workers to lose skills and make it harder to find jobs in the future (Baird et al. (2018)). Unconditional transfers could also have a health productivity effect which would enable poor individuals to maintain a basic standard of living and improve their productivity (Dasgupta and Ray (1986)).

These potential channels may work in directions that reinforce, offset, or even move the overall impact of unconditional transfers to an opposite direction. For example, a human capital accumulation channel may reinforce the income effect in the short run as young individuals invest more time in preparing for a good job match. Human capital depreciation/scarring effects and health productivity effects, on the other hand, may offset the income effects of an unconditional transfer.

My paper contributes to the existing empirical literature on the short term effect of unconditional transfers on labor supply. Effects of transfers differ widely depending on the setting and the context of the program. The fact that I do not find a significant effect on labor supply, and that this is true at both the extensive and intensive margin, suggests that the program could have a human capital depreciation/scarring effect and a health productivity effect that offset the income effect of an unconditional transfer. My paper, therefore, adds to a growing body of research that questions and elaborates on the simple labor-leisure trade off assumptions regarding unconditional transfers.

The remainder of the paper is organized as follows. Section II presents the unconditional transfer program in Seongnam. Section III presents the data and empirical strategy. Section IV presents the results of the empirical analysis, and section V concludes.

2.2 Background

Seongnam is the fourth largest city(out of 31) in Gyeonggi province, South Korea. Figure 2.1 presents a map of Gyeonggi province at the city-level. The Korean government's efforts to disperse the population from Seoul led to Seongnam's rapid population growth in the 1990s.[3]

[3]Korean government provided fiscal incentive packages to large public corporations and private companies to be headquartered in Bundang district in Seongnam. Korea's leading IT companies,

Seongnam now has a population of approximately 1 million, making it the 13th largest city in South Korea.

In 2016, under the leadership of a strong liberal mayor, Seongnam began to give out gift vouchers worth 1,000,000 won(USD 950) to all of its 24 year-olds, regardless of income or economic status. The mayor of Seongnam came up with the idea of a "Youth Dividend" to help young people go through a challenging stage of life, such as graduating from college and getting their first real job. The 1,000,000 won dividend was paid out in the form of four voucher[4] installments, each worth 250,000 won (about USD 210), that were issued every quarter during the year in which the recipient is 24 years old. The program began in 2016 and expanded to all cities in Gyeonggi province in 2019.

2.3 Data and Identification Strategy

2.3.1 Data

The data comes from Local Area Labor Force Survey, a bi-annual survey that provides microdata of individuals from 199,000 households. It contains a comprehensive record of various individual data, such as labor force status, hours of work, wages, age, sex, and educational attainment. The data is available from 2010, so I construct a dataset for Gyeonggi province from years 2010-2018, which gives me 6 years of pretreatment data and 3 years of post-treatment data. Since the transfer program was targeted at 24-year-olds only, I restrict my observations to individuals who are 24 years old at the time of survey. To understand how labor supply responses relate to the performance of the economy, I use annual city-level Gross Regional Domestic Product(GRDP) from Statistics Korea.[5]

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game/entertainment/technology companies have since relocated or established their headquarters in Bundang, Seongnam, making it one of the most affluent and developed city centers in Korea.

[4]Seongnam made transfers in the form of gift vouchers that can be used only in Seongnam's small local businesses. For example, vouchers were redeemable at local farmer's markets, stores, restaurants and parking lots but not at large retail stores and supermarket chains.

[5]All data is publicly available from Korean Statistical Information Service (Statistics Korea (2010-2018))

Figure 2.1: Map of Seongnam City in Gyeonggi Province, South Korea

Source: Gyeonggi province website (https://english.gg.go.kr/31-si-gun-shortcut)

2.3.2 Difference in Differences

I aim to compare labor market outcomes after the introduction of the youth dividend in Seongnam to a group of control cities, the 30 out of 31 cities in Gyeonggi province that were not issuing youth dividends. Difference-in-differences (DiD) analysis is widely used in comparative studies to estimate the impact of a policy change. To ensure its internal validity, DiD estimator must satisfy a parallel trends assumption which assumes that the influence of unobserved confounders on the outcome variable is constant. When the parallel trends assumption is satisfied, DiD estimator is unbiased because one can eliminate the effect of unobservable impacts on the outcome variable by taking the difference between the pre and post treatments. Moreover, under parallel trends assumption, the treated unit and the control group have been subject to common trends in the economy such as macroeconomic shocks. Therefore, if the parallel trends assumption is satisfied in my analysis, I can make a valid argument that the control group of cities provides the counterfactual of the trend that Seongnam would have followed had it not been treated.

The parallel trends assumption is hard to verify, but a visual inspection of the outcome variable before treatment gives an idea of whether pre-treatment trends were similar between Seongnam and the control cities. Figure 2.2 plots the employment-population ratio for the years 2010-2018. There are a large number of ups and downs in both Seongnam, and in the average of the 30 control cities. It is difficult to establish parallel trends because the pre-trend lines cross each other at multiple points in time.

Another indirect test for the parallel trends assumption is to test the hypothesis that the probability of employment was the same prior to the Youth Dividend period in both Seongnam and the group of 30 control cities. To conduct this test, I regress a binary outcome, 1(employed) over the period 2010–16, against an indicator of whether the individual is from Seongnam. The hypothesis is rejected if the coefficient on the age dummy is significantly different from zero. Results presented in column Table 2.1 reject the hypothesis that the probability was the same in both control and treatment cities in the pre Youth Dividend

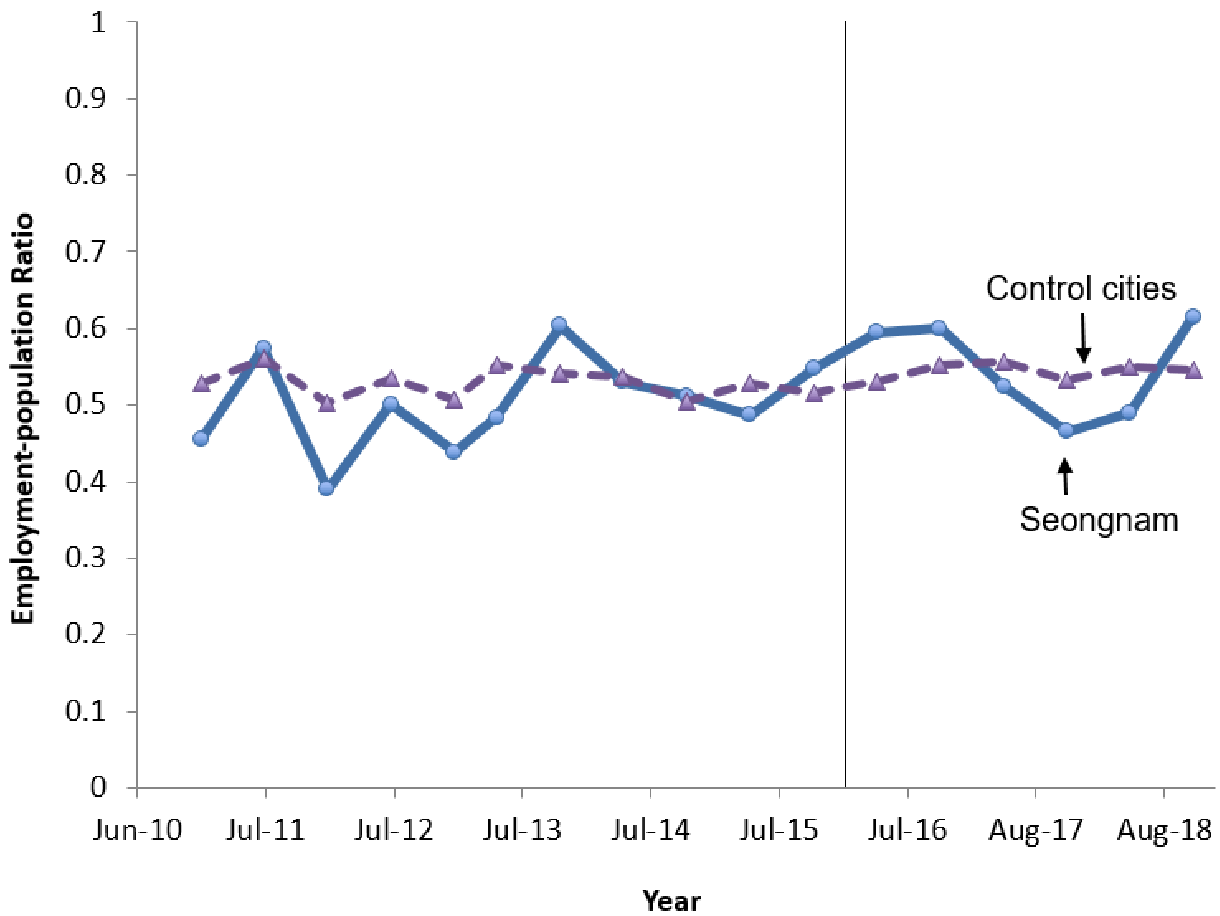

Figure 2.2: Path Plot of Employment-Population Ratio : Seongnam versus Average of 30 Gyeonggi cities

period. 30-control cities in Gyeonggi province, therefore, may not be an appropriate counterfactual for Seongnam.[6] Employing a model that relaxes the parallel trends assumption is desirable.

[6]As an alternative specification, 25 year-olds in Seongnam can be used as a control group. However, it is difficult to establish parallel trends between 24 year-olds and 25 year-olds in Seongnam. See Figure B.1 and Table B.1 in the appendix for details.

Table 2.1: Testing the Parallel Trends Assumption : 30-City Average as a Control Group

Dependent variable : 1(Employed)	Linear Probability	Probit
Point estimate	-0.0303**	-0.0311**
Lower bound 95% conf. interval	-0.0524	-0.0527
Upper bound 95% conf. interval	-0.0083	-0.0095
Number of observation	8,833	8,833

2.3.3 The Synthetic Control Method

The synthetic control method eases the parallel trends assumption that is crucial for the difference-in-difference estimator (Abadie et al. (2010)). By weighting the control group, the synthetic control method creates a counterfactual that resembles Seongnam on a number of key predictors of labor supply before the treatment. If synthetic Seongnam can replicate Seongnam's labor supply trajectory prior to treatment, it must be that it is similar to Seongnam in terms of observed predictors, as well as unobservable confounders and their influence on labor supply. Thus, taking the difference between the synthetic Seongnam and Seongnam eliminates the unobserved confounders.

The synthetic control method is ideal for the setting of this paper because out of 31 cities in Gyeonggi province, Seongnam is the only city which implemented the transfer program, leaving a large pool of 30 "uncontaminated" donor cities from which to construct a synthetic control group. Let Y_{it}^N be the outcome of interest that would be observed for city i in Gyeonggi province at time t in the absence of the intervention, for cities i=1,. . . ., 19, and time periods t=1,. . . ,17.[7] Let Y_{it} be the outcome that would be observed for city i at time t if city i is exposed to the intervention in periods t=12 to t=17. The observed outcome for city i at time t takes the following form, where α_{it} is the effect of the intervention and D_{it} is

[7] I have 17 periods for analysis, starting from t=1 for second half(2H) of 2010, t=2 for first half of(1H) of 2011 and so on, ending at t=17 which is second half(2H) of 2018.

an indicator that equals to one if city i is exposed to the intervention at time t.

$$Y_{it} = Y_{it}^N + \alpha_{it} D_{it} \tag{2.1}$$

Since only the first city, Seongnam with the subscript i=1 is exposed to the intervention, my parameter of interest is

$$\alpha_{1t} = Y_{1t} - Y_{1t}^N \tag{2.2}$$

I estimate the counterfactual of Seongnam, Y_{it}^N, given by the model

$$Y_{it}^N = \delta_t + \theta_t Z_i + \lambda_t \mu_i + \varepsilon_{it} \tag{2.3}$$

Where δ_t is a time varying, unobservable common factor across cities, Z_i is an observable vector of covariates with parameter θ_t, λ_t is a vector of unobserved common factors and μ_i is unobservable factor loadings. The error term ε_{it} is unobservable, mean zero, city-by-time shocks. This model accounts for time varying and city specific unobservable factors by the presence of the $\lambda_t \mu_i$ term.

I construct synthetic Seongnam by seeking a vector of weights $W = (w_2, \cdots, w_{J+1})$, with $0 \leq w_j \leq 1$ and $w_2 + \cdots + w_{19} = 1$ that combines the control cities in Gyeonggi province. I choose a set of weights with a goal of minimizing the difference between Seongnam and synthetic Seongnam on key predictors of the outcome variable and the outcome variable itself. I focus on three main outcomes of interest, which are; the employment-population ratio (the proportion of the population that is employed), the labor force participation rate(the labor force as a percent of the population) and the number of hours worked. For employment-population ratio, for example, I use years of schooling, sex, the rate of wage growth between t=6 (2013 1H) and t=12 (2015 2H), and Gross Regional Domestic Product for years 2010 and 2013. I average the key predictors over the entire pretreatment period (t=1 to t=11). Lastly, I add three lagged years of employment-population ratio, t=1 (2010 2H), t=9 (2014 2H), t=10 (2015 1H). I then choose a vector of weights that solve the following

$$w^*(V) = \underset{w}{\operatorname{argmin}}(X_1 - \sum_{i=2}^{19} w_i \cdot X_i)'V(X_1 - \sum_{i=2}^{19} w_i \cdot X_i) \tag{2.4}$$

where X_i is a vector consisting of elements of Z_i and the average of realized outcome in the pretreatment period. I jointly solve for matrix V and the vector of city weights W that minimizes the mean square prediction error(MSPE) of the synthetic control estimator over the entire pretreatment period. That is, V is chosen as follows through an iterative process:

$$V^* = \underset{v}{\operatorname{argmin}} \frac{1}{11} \sum_{t=1}^{11} (Y_{1t} - \sum_{i=2}^{19} w_i^*(V) \cdot Y_{it})^2 \tag{2.5}$$

Once I derive a set of weights that combines cities to create synthetic Seongnam, my parameter of interest α_{it} is estimated by the expression

$$\widehat{\alpha_{1t}} = Y_{1t} - \sum_{i=2}^{19} w_i^*(V^*) \cdot Y_{it} \tag{2.6}$$

I first establish that synthetic Seongnam resembles the level and trends of labor supply in Seongnam over the pretreatment period. Next, I interpret $\widehat{\alpha_{1t}}$, a discrepancy in labor supply following the Youth Dividend program, as the program's casual effect on labor supply.

In addition, I conduct "in-space" placebo test similar to Abadie et al. (2010) to assess the validity of my results. I iteratively assign treatment to every city in the donor pool and construct synthetic counterparts. If the magnitude of the estimated effect for Seongnam falls well inside the distribution of placebo effects, I can establish that the effect of Seongnam's Youth Dividend is not anything unusual or significant. An in-space placebo test also allows me to estimate p-values by calculating the fraction of cities with placebo effects at least as large as the estimated effect for Seongnam when the treatment effect is at its maximum. Following Jones and Marinescu (2018), I define p-value as :

$$p_1 = \frac{\sum_i \sum_t 1[|\hat{\alpha}_{1,t=max}| \leq |\hat{\alpha}_{it}|]}{N_{it}} \tag{2.7}$$

where $\hat{\alpha}_{1,t=max}$ is estimate for Seongnam(i=1) at time t in which the treatment effect is the greatest. Likewise, $\hat{\alpha}_{it}$ is the estimate for city i in time t. N_{it} is the total number of estimates. The statistic p_1 measures the share of placebo effects that are greater than or equal to that of Seongnam when the treatment effect is at its maximum.

Finally, to complement my findings, I estimate the impact of the Youth Dividend by running a difference-in-difference regression that compares Seongnam to its synthetic counterpart. That is, the regression involves two cities, Seongnam and the synthetic Seongnam, for a total of 17 observations(total number of periods). The interpretation is that if the estimate is close to zero and falls in at a large confidence interval, I fail to reject the null hypothesis that the Youth Dividend had no effect on recipients' labor supply in Seongnam.

2.4 Results

I examine the labor market response to Seongnam's youth dividend program by considering the two margins of response, extensive and intensive. Extensive margin outcomes are the employment-population ratio and the labor force participation rate. For intensive margin outcomes, I examine number of hours worked in the reference week.

To improve the fit of the model that analyzes extensive margin outcomes, I exclude 6 cities that are close to the borders with North Korea from the initial donor pool of 30 cities in Gyeonggi province. These cities have military facility protection zones that restrict civilian access. They are Yeoncheon, Pocheon, Yangju, Gimpo, Paju and Dongducheon. In addition, I also exclude 6 cities (Namyangju, Icheon, Gwangju, Yeoju, Gapyeong and Yangpyeong) that surround lake Paldang. These cities have areas designated as Paldang lake special measure areas for water quality conservation.[8] Within protection areas, development projects are restricted in order to preserve the ecosystem and water quality. My donor pool, therefore, consists of 18 cities (Suwon, Uijeongbu, Anyang, Bucheon, Gwangmyeong, Pyeongtaek, Ansan, Goyang, Gwacheon, Guri, Osan, Siheung, Gunpo, Uiwang, Hanam, Yongin, Anseong and Hwaseong) all in Gyeonggi province.[9]

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[8]These areas are designated under the Framework Act on Environmental Policy. Although Yongin has Paldang lake special measure areas within its boundaries, I did not exclude it from the donor pool. Yongin is the 3rd largest city in Gyeonggi province and shares its north border with Seongnam. Including Yongin in the donor pool improves the fit for the pre-treatment outcomes.

[9]My results remain unchanged whether or not I include military and water protection cities in the donor pool. When included in the donor pool, these cities receive either zero or extremely small weights in synthetic Seongnam. For example, all cities, except for Yeoncheon and Icheon, receive zero weight

2.4.1 Employment-population ratio

I begin with the employment-population ratio among 24 years olds in Seongnam. By construction, the synthetic Seongnam closely follows Seongnam in terms of pretreatment values of employment ratio predictors. Table 2.2 presents the pretreatment values of the actual Seongnam with that of the Synthetic Seongnam and the average of the 30 control cities in Gyeonggi province. For all predictors of employment ratio, Synthetic Seongnam provides a better fit than the 30-city-average. Since Seongnam has one of the most developed city centers in Korea, the 30-city-average values diverge greatly from Seongnam, particularly in scale of the economy expressed in GRDP and average years of schooling.

Table 2.3 lists the weights(the V matrix) used to predict probability of working among 24-year-olds. Employment ratio in the second half of 2010 receives over 60% of the total weight. Table 2.4 reports the weights of each control city in the synthetic Seongnam. The large weight given to Anyang is reasonable because Seongnam and Anyang have geographic and historical similarities. They both share city borders with Seoul and simultaneously received city status in 1973.

Figure 2.3 plots the employment-population ratio for Seongnam and synthetic Seongnam during 2010-2018. The vertical line indicates January of 2016, the time in which Youth Dividend was first given out. The figure indicates that the synthetic control method provides a good fit for the employment-population ratio prior to the Youth Dividend program. The root of the average squared differences between Seongnam and synthetic Seongnam, expressed in RMSPE(root mean squared prediction error) is 0.02 percentage points. Synthetic Seongnam closely tracks the trajectory of this variable particularly from year 2013 to 2015. However, in 2016, the year of treatment, Seongnam and Synthetic Seongnam diverge in a sporadic way, making it difficult to identify a clear pattern. Figure 2.4 shows that immediately following the youth dividend disbursement, the employment ratio went up in Seongnam while that of

in synthetic Seongnam for employment-population ratio. Yeoncheon and Icheon obtains 0.06 and 0.012 weight respectively, which is too small to drive results differently. See Table B.2 and B.3 in the appendix for Synthetic Seongnam donor city weights with all 30 Gyeonggi cities in the donor pool.

synthetic Seongnam fell sharply(positive gap). Seongnam and synthetic Seongnam seemed to converge by the end of 2016, however, the gap between the two reappear and grow during 2017-2018(negative gap). The plot suggests that youth dividend did not have clear impact on the employment-population ratio of 24 year-olds in Seongnam.

Table 2.2: Actual and Synthetic Seongnam Predictor Means

Variables	Seongnam	Synthetic Seongnam	Average of Gyeonggi cities
Years of schooling	14.27	14.32	13.97
Percentage of male population	0.45	0.50	0.51
Rate of wage growth	-0.05	-0.04	-0.01
Ln(GRDP) 2010	16.80	16.61	15.50
Ln(GRDP) 2013	16.89	16.67	15.60
Employment-population ratio in 2010 2H	0.45	0.45	0.53
Employment-population ratio in 2014 2H	0.51	0.51	0.50
Employment-population ratio in 2015 1H	0.49	0.48	0.54

Notes : All variables except Ln(GRDP) and lag of Employment-population ratio are average for 2010-2016 period. Ln(GRDP) is in million Korean Won, deflated by 2010 price.

Table 2.3: Synthetic Seongnam Predictor Weights

Predictor Variable	Weight
Years of schooling	0.034
Percentage of male population	0.033
Rate of wage growth	0.001
Ln(GRDP) 2010	0.170
Ln(GRDP) 2013	0.050
Employment ratio in 2010 2H	0.643
Employment ratio in 2014 2H	0.015
Employment ratio in 2015 1H	0.054

Table 2.4: Synthetic Seongnam Donor City Weights

City	Weight	City	Weight
Suwon	0	Guri	0
Uijeongbu	0	Osan	0
Anyang	0.441	Siheung	0
Bucheon	0	Gunpo	0
Gwangmyeong	0	Uiwang	0
Pyeongtaek	0.054	Hanam	0
Ansan	0	Yongin	0.217
Goyang	0.288	Anseong	0
Gwacheon	0	Hwaseong	0

Figure 2.5 presents the results of the in-space placebo test. The gap in employment-population ratio for Seongnam falls well inside the distribution of 18 placebo effects. The

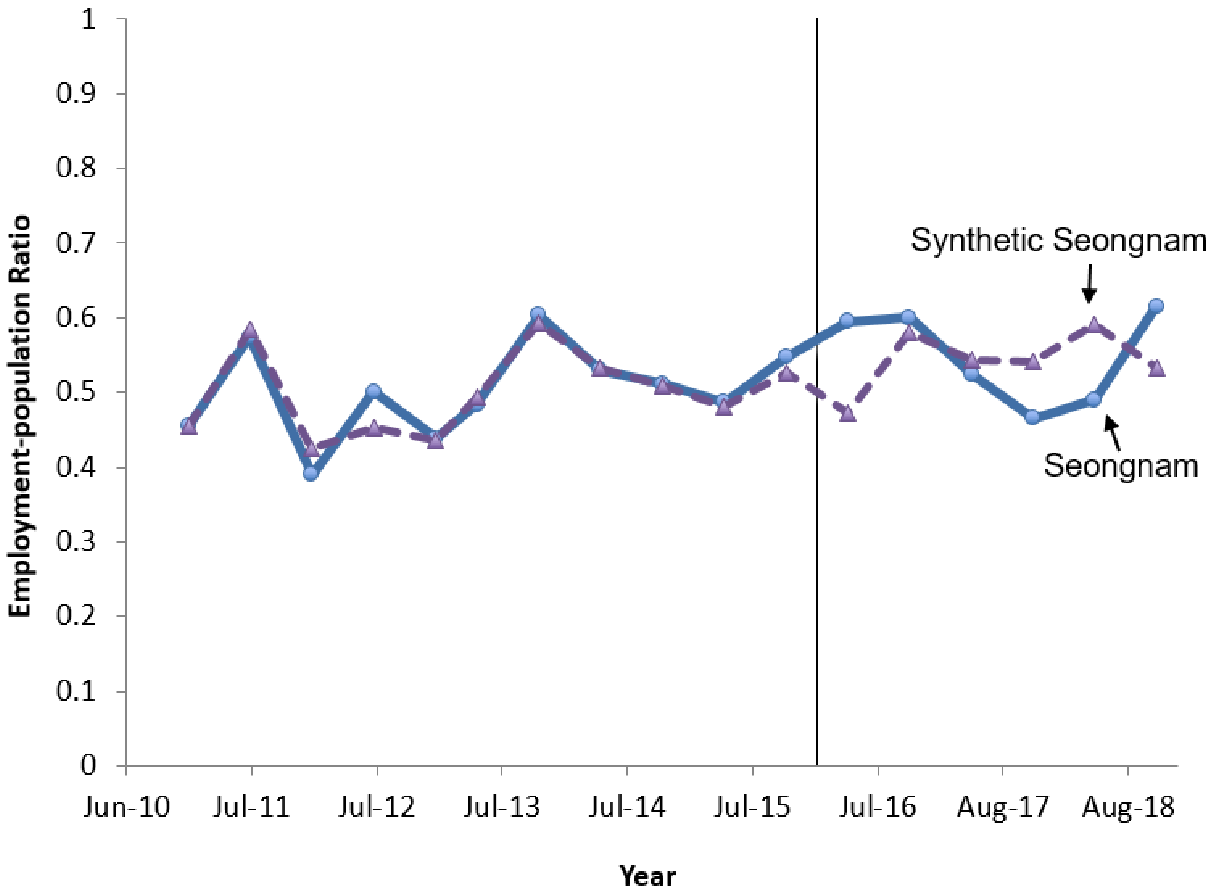


Figure 2.3: Path Plot of Employment Population Ratio

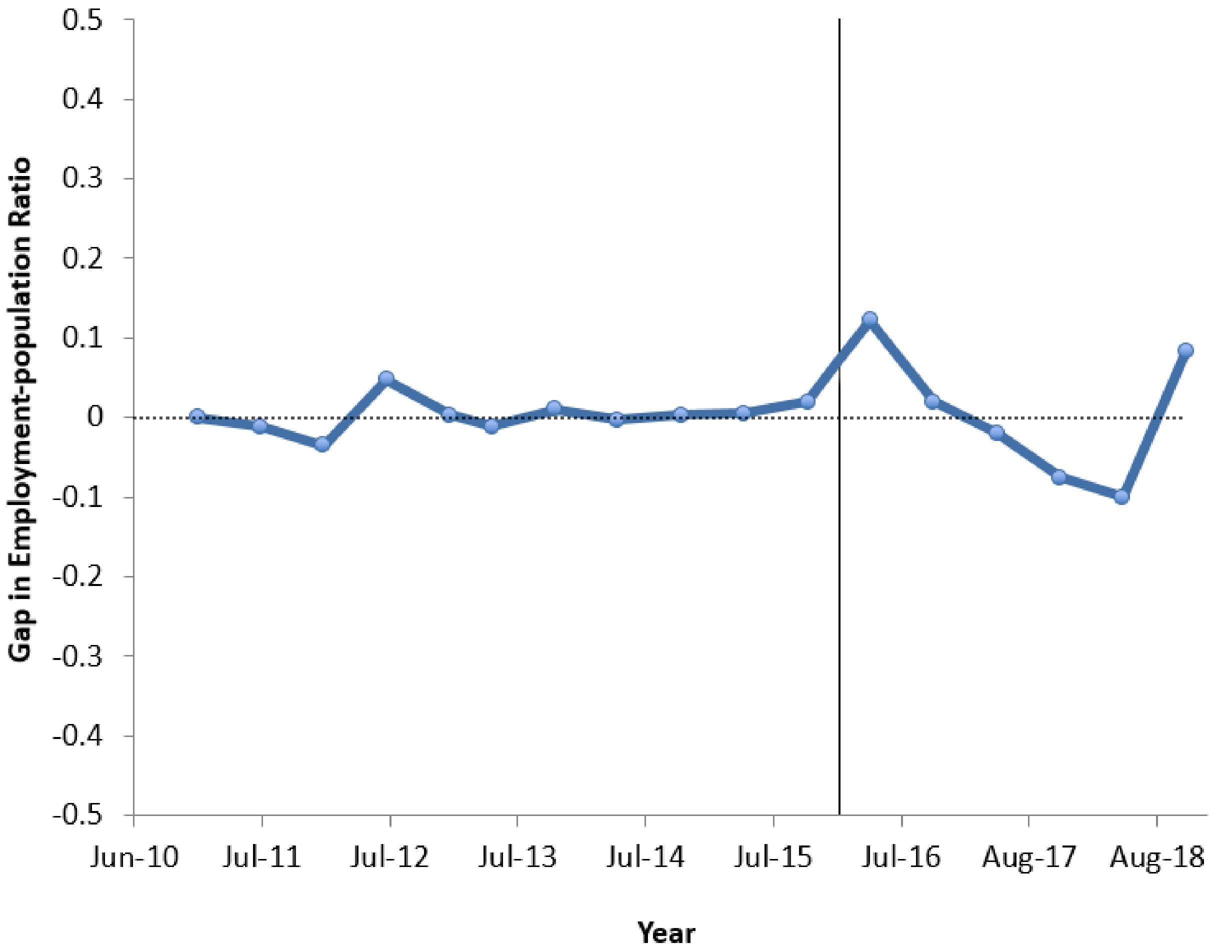


Figure 2.4: Gap in Employment Population Ratio

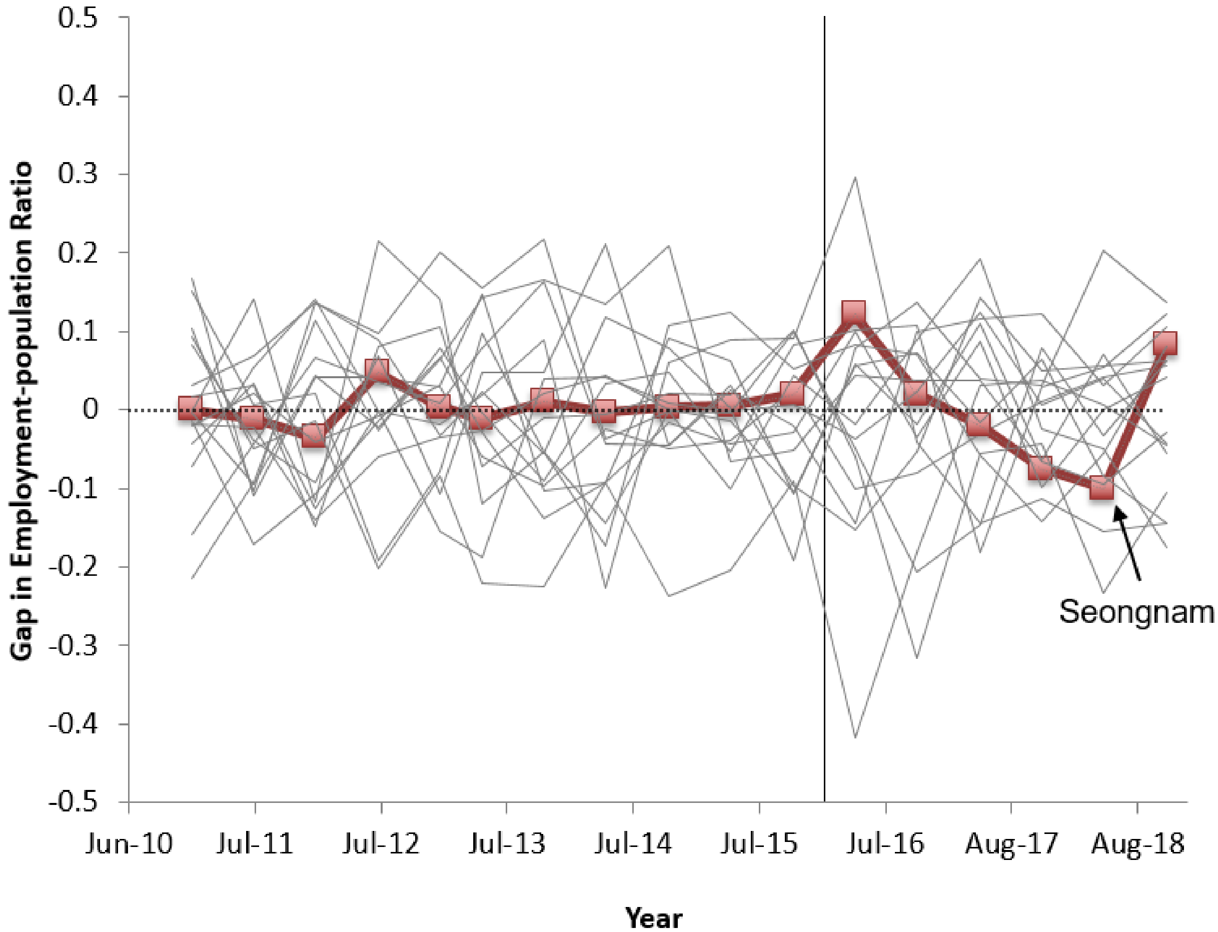


Figure 2.5: Permutation Test, Employment Population Ratio

p-value from this empirical distribution of placebo treatment effect is 0.2012. That is, over 20 percent of treatment effects generate a larger estimate than Seongnam's observed estimate in t=12 (2016 1H). Moreover, the estimate from difference-in-difference regression model (Column (1) in Table 2.5) is close to zero (0.0045) and falls in at large confidence interval [-0.073, 0.082], failing to reject the null hypothesis that Youth Dividend had no effect on the employment of 24 year-olds in Seongnam. These results are consistent with the lack of a significant effect suggested by the synthetic control method.

Table 2.5: Summary of Synthetic Control Estimates

Estimate	Employment-Population Ratio (1)	Labor Force Participation Rate (2)	Hours worked in Reference Week (3)
Panel (a) :Synthetic Control Method			
α_1	0.0049	0.0382	-1.6112
p-value	0.2012	0.2384	0.1239
Pre-Period RMSPE	0.0198	0.0275	0.8466
Panel (b) : Difference in Differences			
Seongnam x Post 2016	0.0045	0.0327	-1.4895
Robust Standard Errors	(0.0362)	(0.0210)	(1.0412)
95% Confidence Interval	[-0.073, 0.082]	[-0.012, 0.078]	[-3.709, 0.730]

Notes :The treatment effect, α_1, is averaged over the years 2016 to 2018. The p-value is constructed using the placebo test. RMSPE is calculated using 2010-2015 data. Following Borjas (2017), I weighted difference-in-difference regression by the number of observations used to calculate the dependent variable. The number of observations of a synthetic control is calculated by multiplying the sample size in the actual cities by the weight of each city given by the synthetic control method.

2.4.2 *Labor Force Participation Rate*

To complement my findings on how Youth Dividend affects labor supply at the extensive margin, I examine another outcome of interest, labor force participation rate.[10] Figure 2.6 presents the labor force participation rate for Seongnam and synthetic Seongnam. The labor force participation rate in synthetic Seongnam continues to closely track the trajectory of Seongnam after the Youth Dividend is paid. Figure 2.7 plots the discrepancies between Seongnam and synthetic Seongnam. The graph fluctuates around zero, suggesting a negligible impact from the Youth Dividend on labor force participation.

The placebo test result displayed in Figure 2.8 is consistent with this null-result. The actual treatment difference for labor force participation rate in Seongnam lies squarely inside the range of placebo differences. Lastly, Table 2.5 column (2) reports a large p-value(0.2384). The difference-in-difference regression result is 0.03 and not statistically different from zero. From the confidence intervals [-0.012, 0.078], I can rule out effects larger than 1.2 percentage points on the likelihood of recipients dropping out of labor force. On the whole, I find no significant effect from the Youth Dividend on labor supply at the extensive margin.

2.4.3 *Number of hours worked*

Figure 2.9 presents Youth Dividend's effects on labor supply at the intensive margin-the number of hours worked. Seongnam's post treatment path is erratic and trend lines cross each other at multiple points in time. Figure 2.10 displays the gap between Seongnam and synthetic Seongnam, which switches between negative and positive gaps. The placebo test result in Figure 2.11 shows that the treatment effect on the number of hours in Seongnam was not of an unusual magnitude compared to other placebo effects. Lastly, in Table 2.5, column (3), I estimate an average decrease of 1.5 hours in the total number of hours worked in the previous week. Based on difference-in-difference regression results, I can rule out that the treatment decreases work hours in any given week by more than 3.7 hours. In addition,

[10] A list of synthetic control cities, predictors and weights are provided in Appendix B, Table 2.3 and 2.4.

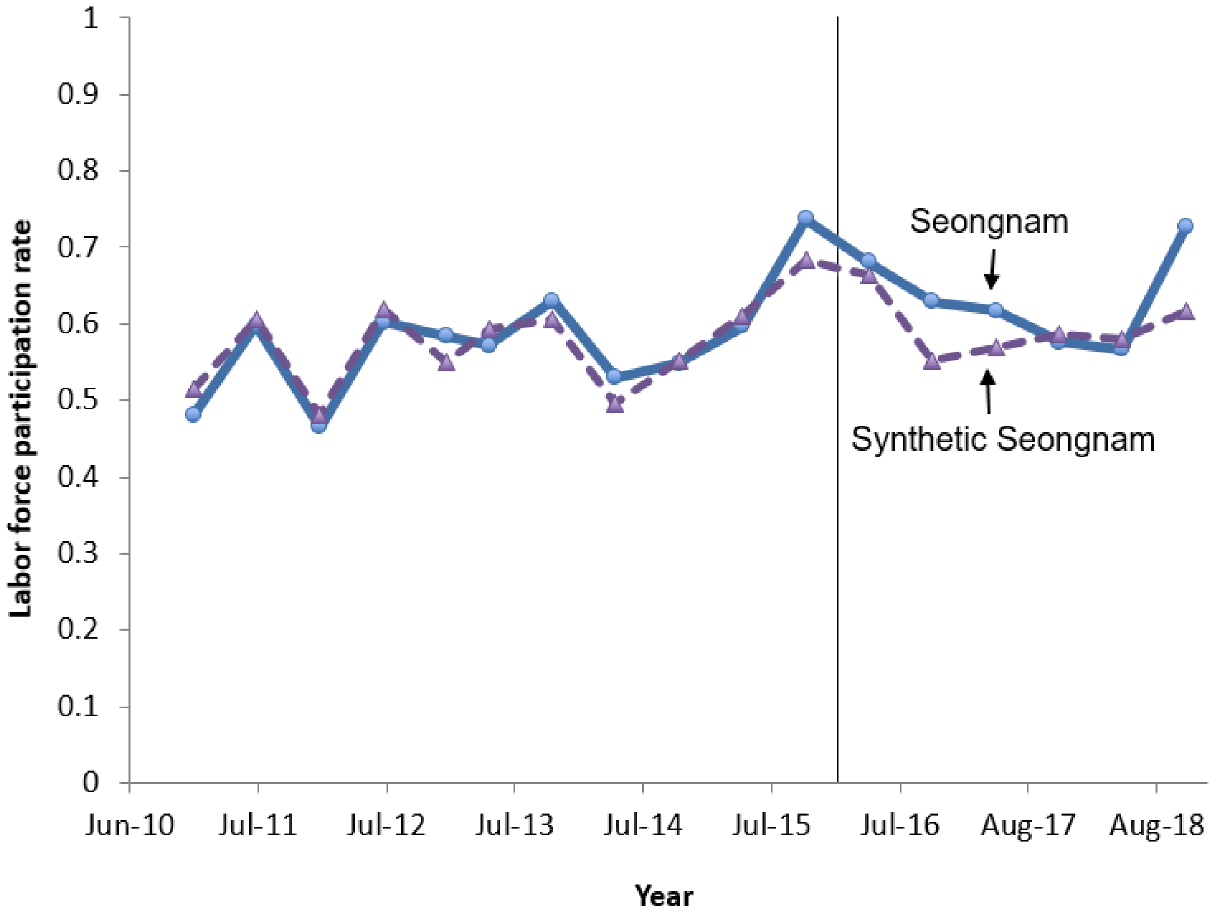


Figure 2.6: Path Plot of Labor Force Participation Rate

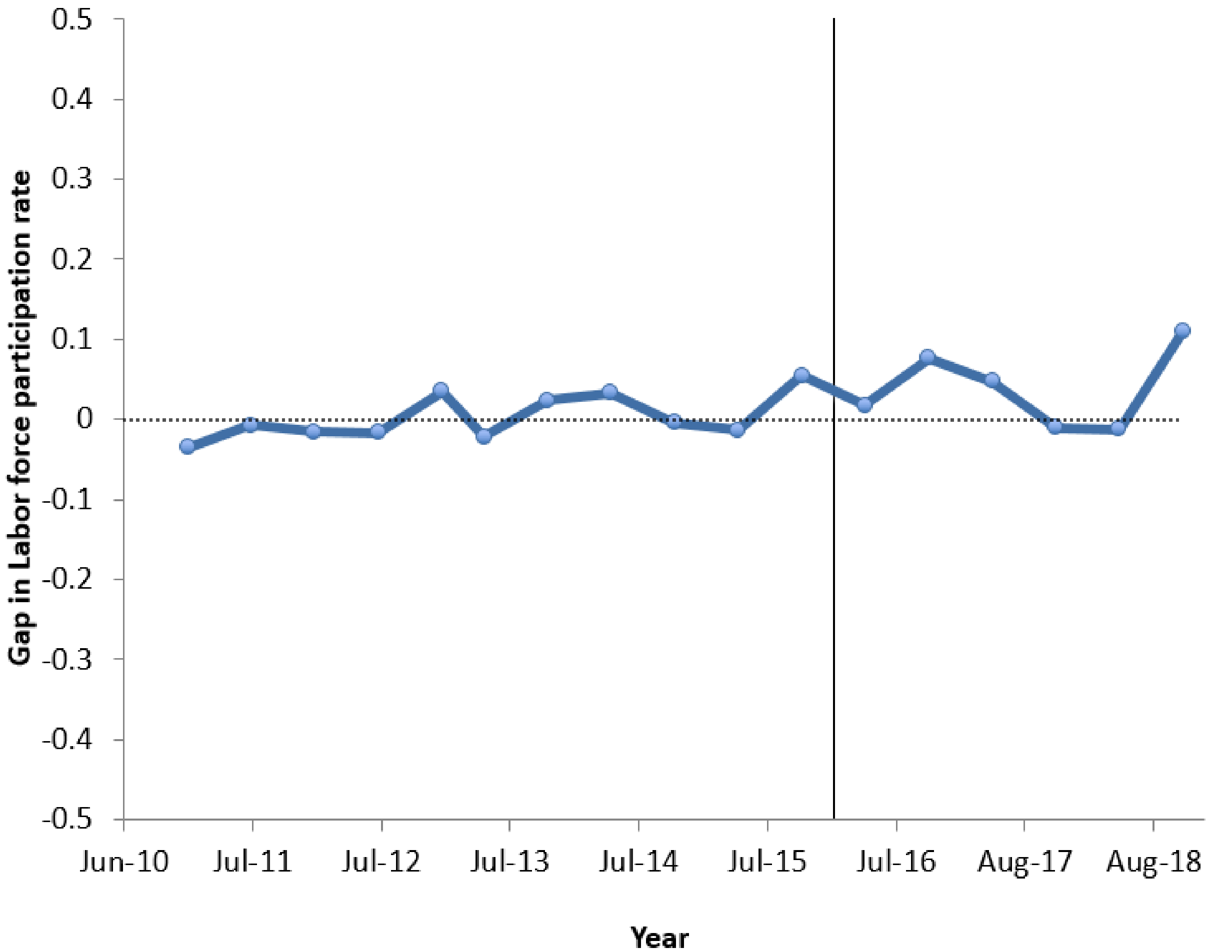


Figure 2.7: Gap in Labor Force Participation Rate

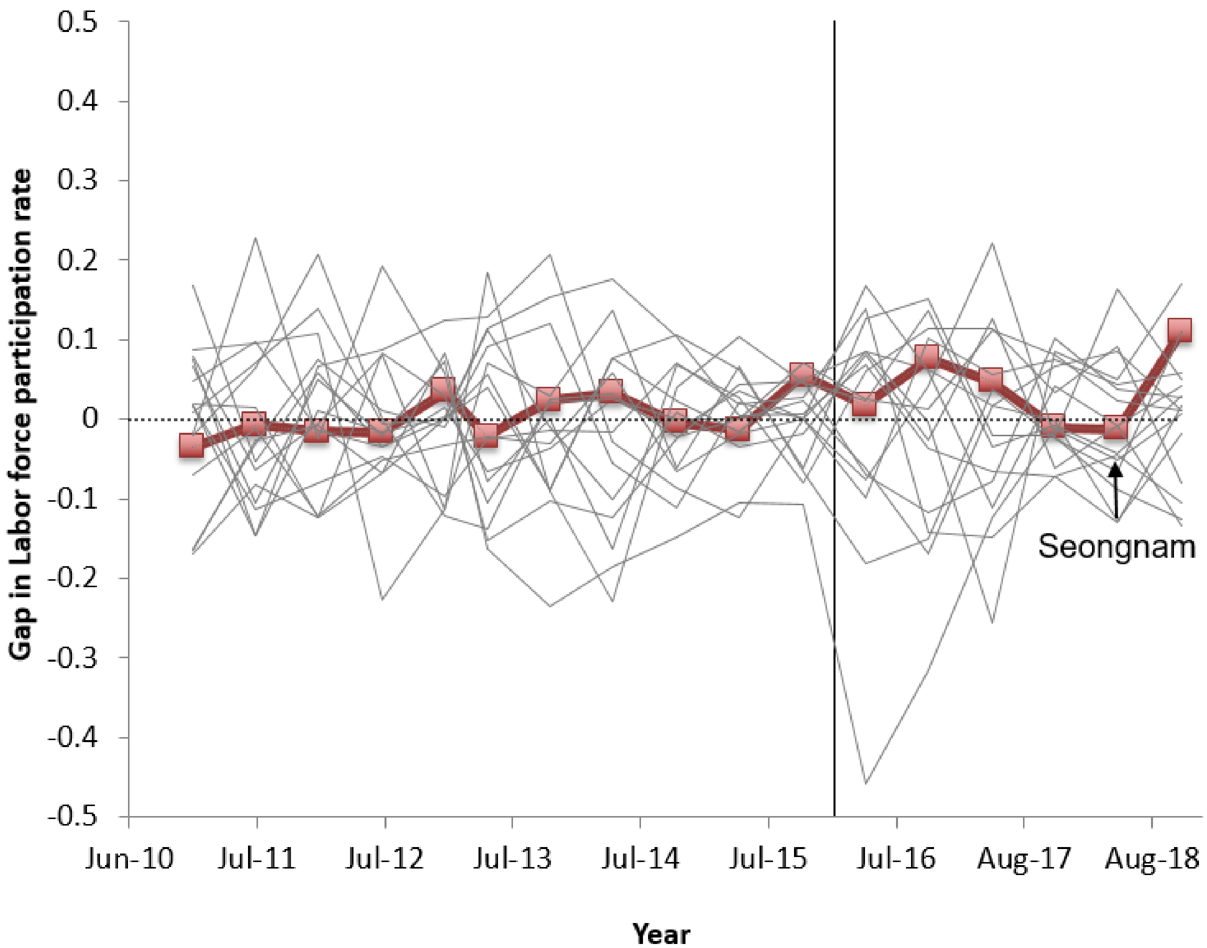


Figure 2.8: Permutation Test, Labor Force Participation Rate

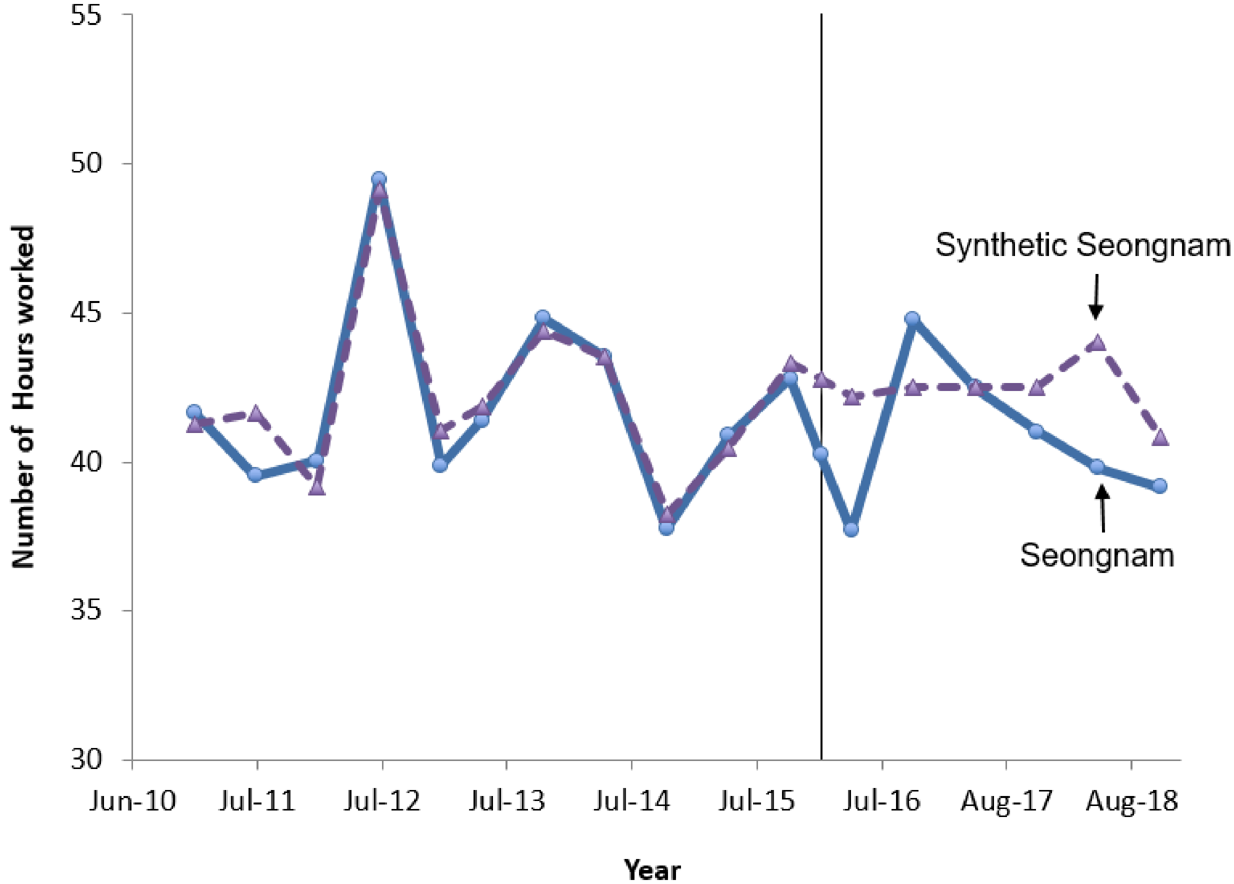


Figure 2.9: Path Plot of Number of Hours Worked

the effect is insignificantly different from zero. Overall, my analysis on the Youth Dividend program suggests the lack of a systematic impact on both the extensive and intensive margins of labor supply.

2.4.4 Transition

For a more concrete analysis, I consider possible anticipation effects and a delayed response to the treatment during transition. It is possible that individuals adjust their labor supply before the year they are 24 years old in anticipation that they will receive a transfer every

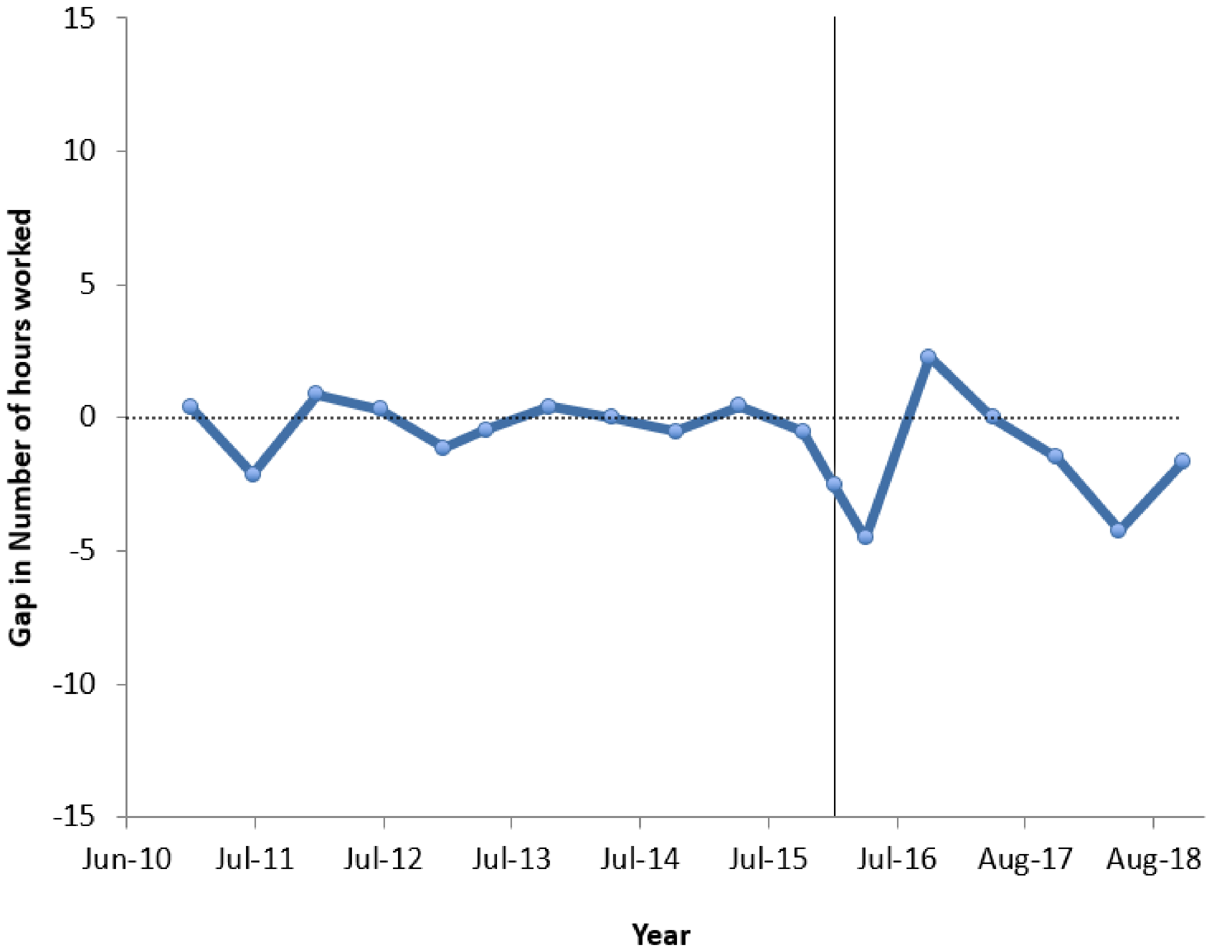


Figure 2.10: Gap in Number of Hours Worked

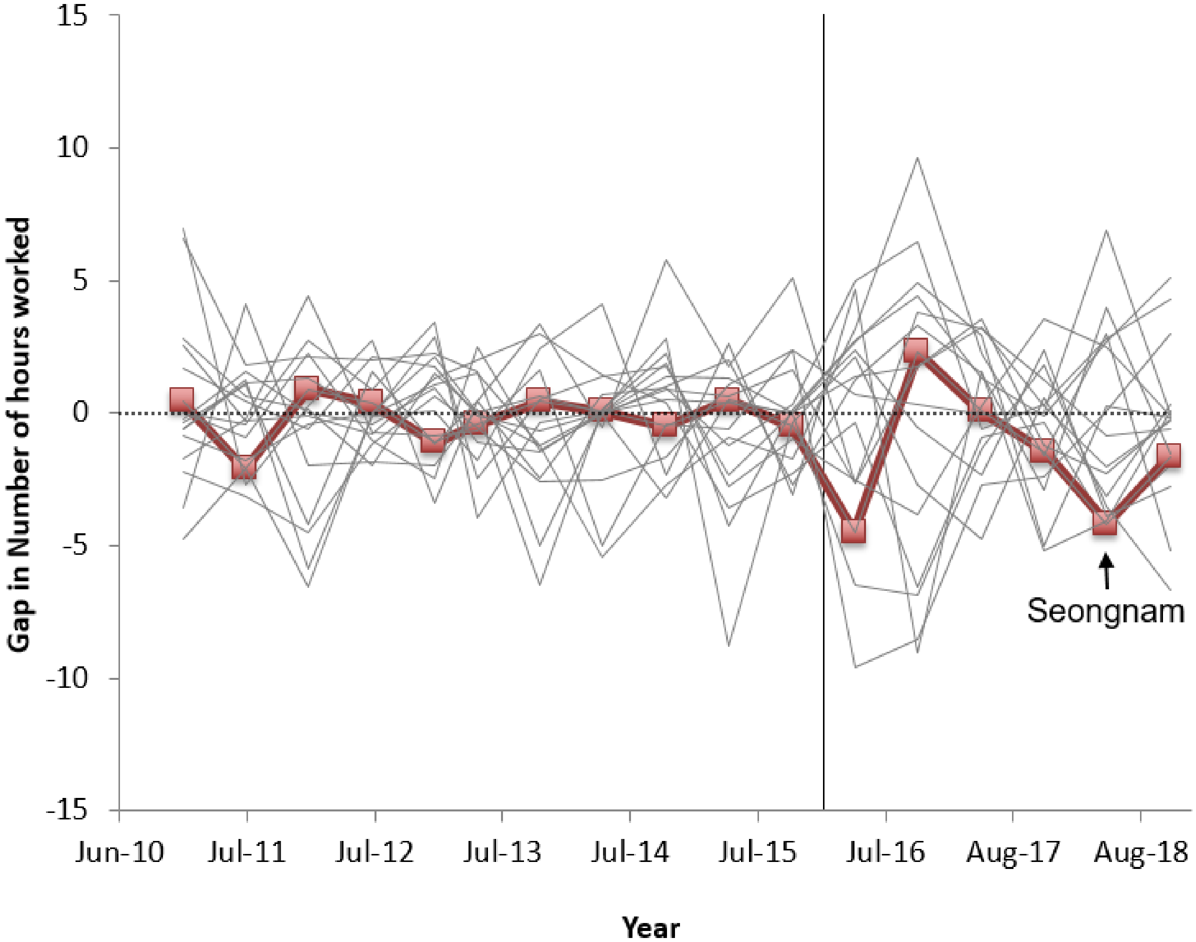


Figure 2.11: Permutation Test, Number of Hours Worked

quarter once they turn 24. If anticipation leads to changes in outcomes before treatment, I might observe no significant effect. However, there are two reasons why anticipation effects cannot be the main force that drives the results of my model.

First, due to strong opposition from the central government, Seongnam was not able to carry out the program as planned in the first year. 24-year-olds in 2016 received only half of the amount for the first three quarters in 2016, and received the remaining balance only at the end of 2016. It is unlikely that 23-year-olds in 2015 would change their behavior in expectation of receiving a transfer in the future. Second, although 24-year-olds in 2017 and 2018 knew prior to turning 24 that they would receive a transfer once they turn 24, changing labor supply decisions in the present based on a future dividend of this kind is not consistent with the behavior of a forward-looking agent in economic theory. As Malani and Reif (2015) argue, anticipation is a reasonable interpretation if there is a welfare gain to acting before a treatment is adopted. There is little economic benefit for young people to begin changing their behavior in response to the Youth dividend because it is only a small, one-time, transfer.

It is also important to consider the possibility of a delayed response to the treatment. Seongnam's Youth Dividend did not have a smooth start and it took almost a full year to implement the program as planned. It is possible that I might see a different result if I assign the treatment year to be 2017, a year after the official start of the program.[11] Figure 2.12 presents results from an in-time placebo test for a delayed response. The year of treatment is shifted to 2017 and the synthetic control is constructed based on 2010-2016 data. Immediately after the placebo treatment year, 2017, actual Seongnam shows lower employment-population ratio than the synthetic Seongnam. However, Seongnam's

[11] Mayor Lee stood firm and carried out the program despite the central government's objections. In November 2015, President Park Geun-hye rebuked local governments for promising welfare benefits. In December 2015, Ministry of Health and Welfare officially announced that Seongnam's Youth Dividend program was unacceptable. Claiming that the central government was excessively harming the local government's interests, Seongnam asked the Constitutional Court to settle the dispute. While waiting for Constitutional Court's ruling, Seongnam nevertheless went ahead and disbursed transfers in January 2016. Seongnam eventually dropped all charges against the central government in May 2017 when Moon Jae-in was elected president after the impeachment of Park Geun-hye.

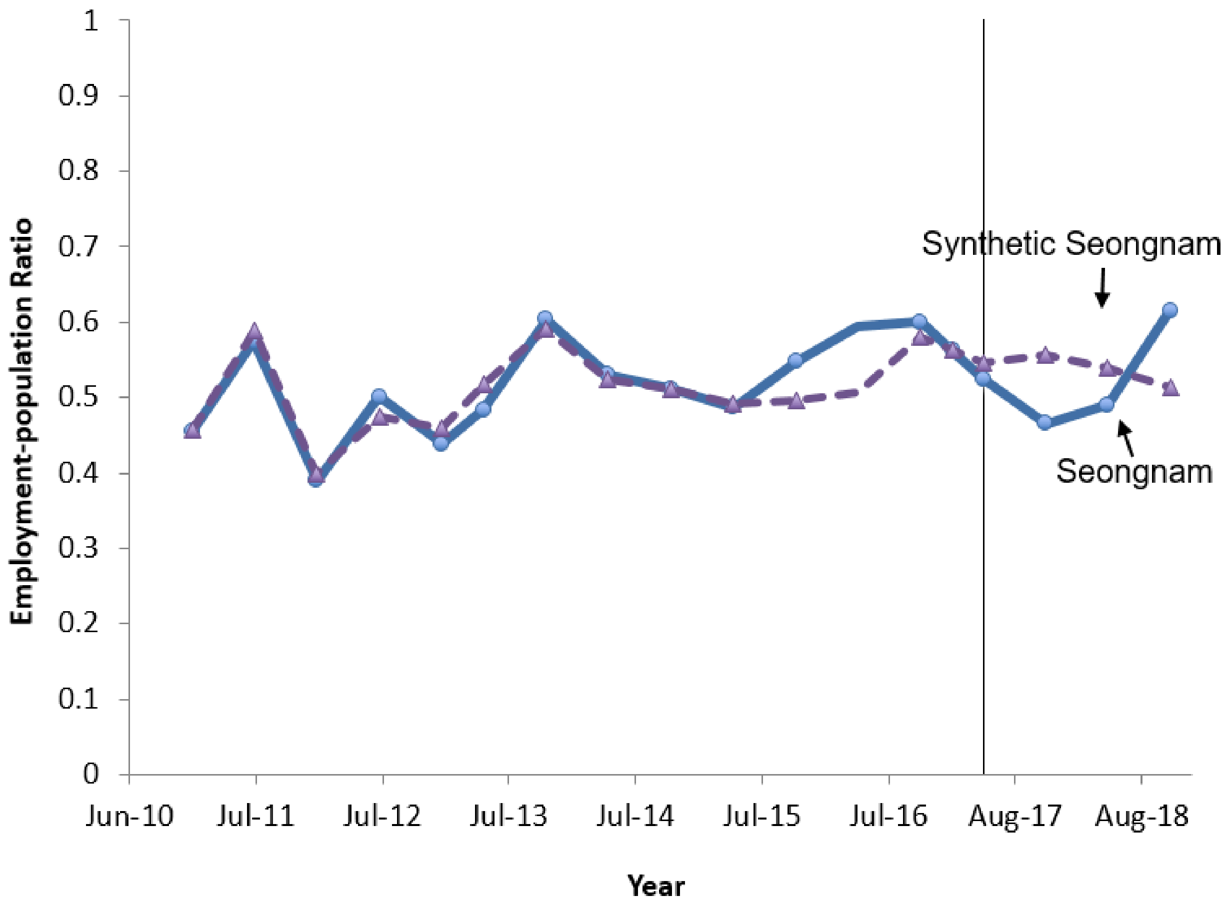


Figure 2.12: Test for Delayed Response, Employment-Population Ratio

employment-population ratio takes over its synthetic counterpart by the end of 2018. Similar to the main result where 2016 is used as the actual treatment year, it is hard to diagnose causal effect in any direction. In other words, the in-time placebo test shows that a possible delayed response does not drive the main result.

2.5 Conclusion

In this paper, I analyzed the case of Seongnam's Youth Dividend program to investigate the impact of an unconditional transfer on recipient's labor supply. By analyzing labor market outcomes both at the extensive and intensive margin, I show that there is no evidence of a large impact from the unconditional transfer. From the path plots of Seongnam and its synthetic counterpart, I can rule out any post-treatment trends that suggest the presence of a program effect. Moreover, the placebo test results show that the labor supply trends for 24-year-olds in Seongnam following the dividend payout are not anything strikingly abnormal relative to the distribution of the placebo effects. Lastly, consistent with the results from the synthetic control method, the difference-in-difference regression results fail to reject the null hypothesis that the Youth Dividend had no effect on the individual's labor supply in Seongnam.

This result adds to the growing body of research that finds little evidence for unconditional transfers discouraging labor participation. It is possible that the potential negative effects from unconditional transfers are reduced by positive responses through alternative channels. For young individuals like Seongnam's Youth Dividend recipients, a human capital depreciation/scarring effect and health productivity effect may have played an important role in shaping the overall effect. Future research that directly tests these alternative channels will provide a deeper understanding the effects of unconditional transfers on young individuals.

Chapter 3

EMPLOYMENT EFFECTS OF THE 52-HOUR WORKWEEK REGULATION IN KOREA

3.1 Introduction

Reducing the number of working hours and improving work-life balance are important challenges for industrialized economies. Several studies associate long working hours with negative outcomes on workers' health (Virtanen et al. (2010), Virtanen et al. (2012)), safety (Dembe et al. (2005), Wagstaff and Lie (2011)), and productivity (Pencavel (2015)). Not only can productivity fall and health problems increase, but long work hours can create gender disparities and limit women's opportunities in the workplace (Bertrand et al. (2010)), particularly in more traditional cultures. With an aim to improve quality of life, create more jobs, and boost the country's declining birth rate, South Korea lowered its maximum working hours from 68 hours a week to 52 hours in July of 2018. The standard hours were reduced at different times by industry and firm size.

Theoretically, the employment effects of a reduction in working hours is ambiguous. On the labor supply side, a compulsory reduction in hours may make workers work less if they prefer shorter work hours, but are currently stuck with long working hours because of a competition mechanism in the work place. When the number of hours an employee is willing to work plays a role in hiring or promotion decisions, it leads to a "rat-race" equilibrium where workers work too many hours (Landers et al. (1996)). On the other hand, it is also possible that individuals whose hours are reduced may decide to work over-time or look for a second job to supplement reduced income from their main job.

On the labor demand side, reductions in the number of standard work hours reduces the hours of work for currently employed workers and increases new hires if perfect substitutes are available. However, in real life, firms may "hire" more hours from current employees and employ fewer new people. To do so, firms may introduce flexible work-schedule arrangements. It is also possible that firms may produce less under reduced work hours.

Since a broad range of outcomes are consistent with the different theories regarding the effects from work-hour reductions, an empirical analysis is needed to determine the policy impact. Through my empirical analysis, I aim to answer three questions. First, does a reduction in standard work hours lead to a drop in actual working hours? Second, do workers experience the increased possibility of using a flexible working-time arrangement after the working-hour reduction? Lastly, does a compulsory reduction in hours worked for individual employees contribute to job creation?

Using individual-level data from the Korean Labor Force Survey, I employ a triple difference approach and find that female workers in affected firms worked 3.59 more hours per week than those in the control firms. Moreover, these women were more likely than their counterparts to use flexible working time arrangements. However, there was no significant difference for male workers. On the whole, my findings show no significant relationship between work-hour reductions and job creation.

My paper builds on labor supply literature that investigates the employment effects of work hour reductions. Several studies including Hunt (1999), Crépon and Kramarz (2002), Chemin and Wasmer (2009), Estevão and Sá (2008) document either a decrease or no change to employment after the work hour reduction. However, one study, Raposo and Van Ours (2010), found evidence that working hour reductions had a positive effect on employment through a fall in job destruction. This study is relevant to my work because it considers the implications of flexible working-time arrangements. However, in the previous study, a quantitative assessment of the effects of flexible working-time arrangements had yet to be explored. I contribute to the literature by showing empirically the relationship between work hours and the use of flexible working arrangements. In terms of econometric analysis, I present a model that provides an estimator that is much less vulnerable to the confounding effects of trends.

The rest of this paper is structured as follows : Section II discusses the background, Section III presents the data, identification strategy and estimating equations, Section IV reports results, and Section V concludes.

3.2 Background

On February 28, 2018, South Korea introduced a labor market reform which reduced maximum weekly working hours from 68 to 52. The reform not only reduced the number of hours, but also expanded the number of industries subject to a weekly work hour limit. Reducing long work hours was one of president Moon Jae-in's main election pledges when he took office in May 2017 following the impeachment and removal of Park Geun-hye. Moon's administration moved quickly, and the law went into effect in July 2018.

The 52-hour workweek system was implemented in phases, starting with phase I firms in July of 2018, and followed by phase II, III and IV firms in July of 2019, 2020, and 2021 respectively. Table 3.1 shows the time schedule of the implementation by firm size and industry. The Korean government set different deadlines by firm size to allow employers the time needed to make adjustments. At the same time, industry-level variation was introduced during implementation because the Korean government gave a 1-year grace period to industries that were previously exempt from the work hour limit.

Besides limiting the number of work hours, the labor reform gave rise to an increase of firms using flexible working time arrangements. Providing flexible working hour arrangements to employees became an important component to maintaining employee productivity after the maximum working hours were reduced from 68 hours to 52 hours per week. In fact, the Korean government, as well as economic interest groups, encouraged firms to implement flexible working hour arrangements following the policy change.[1]

[1] In June 2018, Korea Employers Federation(KEF) published a 'Guidebook on Working Time Reduction'. As one of the five action plans to help employees adapt to the new system, KEF recommended active use of flexible working hours.

Table 3.1: The Implementation of 52-Hour Workweek Regulation in Korea

Phase	Starting date	Industry and Size of the workforce
I	July 1, 2018	Firms in industry A, B, C, D, F, L, O, R, U with more than 300 employees
II	July 1, 2019	Firms in industry G, I, K, P with more than 300 employees
III	July 1, 2020	All Firms with 50-299 employees
IV	July 1, 2021	All Firms with 5-29 employees

Note :Industry codes follow International Standard Industrial Classification of All Economic Activities (ISIC) by the International Labor Organization(ILO). Further descriptions on the industry codes can be found in the appendix Table C.3.

3.3 Data, Identification Strategy, and Estimating Equations

3.3.1 Data

I analyze data from the August 2017 and August 2018 Economically Active Population Survey(EAPS).[2] My sample includes individuals aged between 18 and 60 who held jobs, worked a positive number of hours and earned income in the past three months. I exclude self employed and unpaid family workers from the analysis, and I include only individuals working for firms with at least 30 employees.[3] At the industry level, I make three exclusions for a

[2]All datasets are publicly available online at Korean Statistical Information Service website(kosis.kr)

[3]Firms with less than 5 employees are exempt from the work hour regulation while those with 5-49 employees have until July 2021 to comply with 52-hour workweek. Since EAPS does not disclose firm size data in numbers but in discrete categories (1-4 employees, 5-9 employees, 10-29 employees, 30-99 employees, 100-299 employees, 300 and more employees coded as 1, 2, 3, 4, 5 and 6 respectively), I make a cut off at code 4 and include individuals working for firms with 30-49 employees. Although I have individuals in the control group who have an extra year to comply with the law, I do not expect it to drive the overall findings of my paper. I argue that my findings show the lower bound of the true effect of the 52-hour workweek regulation which may be even greater if those individuals working for firms with 30-49

more focused analysis. First, I exclude individuals who work for 5 industry divisions that are exempt from the 52-hour workweek regulation.[4] Second, I exclude industries which have both both treatment and control group of workers within the industry section boundary due to data disclosure restrictions.[5] Third, I exclude the industry section K.Financial and insurance activities from the analysis, as there are firms within the section boundary which made an early transition in response to the policy change before the legal mandatory deadline.[6]

There are two other datasets that I use in this paper. First, I use the EAPS dataset from 2014 to 2017 to test for parallel trends in the work hours of large firms and small firms in both the treated and control industries. Second, I use firm-level data from the October 2017 and October 2018 Labor Force Survey at Establishments(LFSE) for the analysis on the number of people employed after the policy change.

3.3.2 Identification Strategy

The impeachment of a conservative president and subsequent replacement with a liberal successor create an ideal experiment where the introduction of a 52-hour workweek regulation is an independent, random, event that varied in its timing with no spillover effects on the

employees are excluded from the data

[4]These industry divisions are 49.Land transport and transport via pipelines/ 50.Water transport/ 51.Air transport/ 529.Support activities for transportation/ 86.Human health activities. I exclude the whole industry sections which include these divisions(H.Transportation and storage and Q.Human health and social work activities) from the analysis because EAPS discloses industry information at section level only.

[5]Exemptions for standard work hour reductions were given at the division level. However, publicly available EAPS data does not provide information by the division of industries but only by the section. For example, I excluded industry section E, which is “Water supply; sewage,waste management,materials recovery” from the analysis because big firms in one of its division, “37. Sewage, wastewater,human and animal waste treatment services” is subject to the regulation from July, 2019 while the rest has to implement shorter standard work hours from July, 2018. In other words, industry E has both treatment and control group of workers within its boundary and it is impossible to separate one from the other using publicly available data.

[6]Although the financial industry was granted a one-year grace period for adopting the new 52-hour workweek, in April, 2018, Minister of Employment and Labor Kim Young-joo called for early adoption of 52-hour workweek to the banking industry. Other financial sectors such as securities, insurance and non-banking industries have also implemented 52-hour work week related measures such as forced lunch breaks and PC shutdown ahead of time.

control group of firms. With a correctly specified model, I can provide an unbiased estimate of the average treatment effect. However, the policy was introduced at a time of economic growth and declining work hours across the country at large, which makes identifying its impact challenging. During this period(2017-2018), Korea's GDP grew at an average of 3.075 % per year. Conventional economic theory predicts that an increase in income reduces hours worked by increasing the demand for all normal goods, including leisure.[7]

Figure 3.1 presents the average hours worked annually between 2008-2017. Korea's average hours worked decreased steadily from over 2,228 hours per year in 2008 to 1,967 hours per year in 2019. Figure 3.2 provides additional evidence that the downward trend in working hours is a country-wide phenomenon. After the mandatory reduction to work hours went into effect in August 2018, the percentage of individuals working between 29-32 hours increased in all firms across both treated and control industries. Therefore, any change in the number of hours worked during this period could be an indication of broader trends that have little to do with 52-hour work week reform.

To address this identification challenge, I use a triple difference strategy. I define the treatment group as workers in large firms within industries that are subject to the 52-Hour workweek regulation in 2018(phase I). This identification strategy is ideal because the work hour reduction takes place at different times for each firm depending on its size and industry. Specifically, during phase I, all large firms are subject to a 52-hour workweek except for those that belong to industries that qualified for a 1-year grace period.

I illustrate my identification strategy in Table 3.2. The data is split into four cells. The columns split data by time : pre-52-hour work week versus post-52-hour work week. The rows split data by industry ; treated versus control. Each cell shows the mean hours worked weekly for the group, along with the standard error and the number of observations. Within the treated industry, the weekly hours worked by employees of treatment-sized firms

[7]Bick et al. (2018) analyse household survey data from 80 countries of all income levels and find the pattern of decreasing hours with aggregate income holds for both men and women and for adults of all ages and education levels.

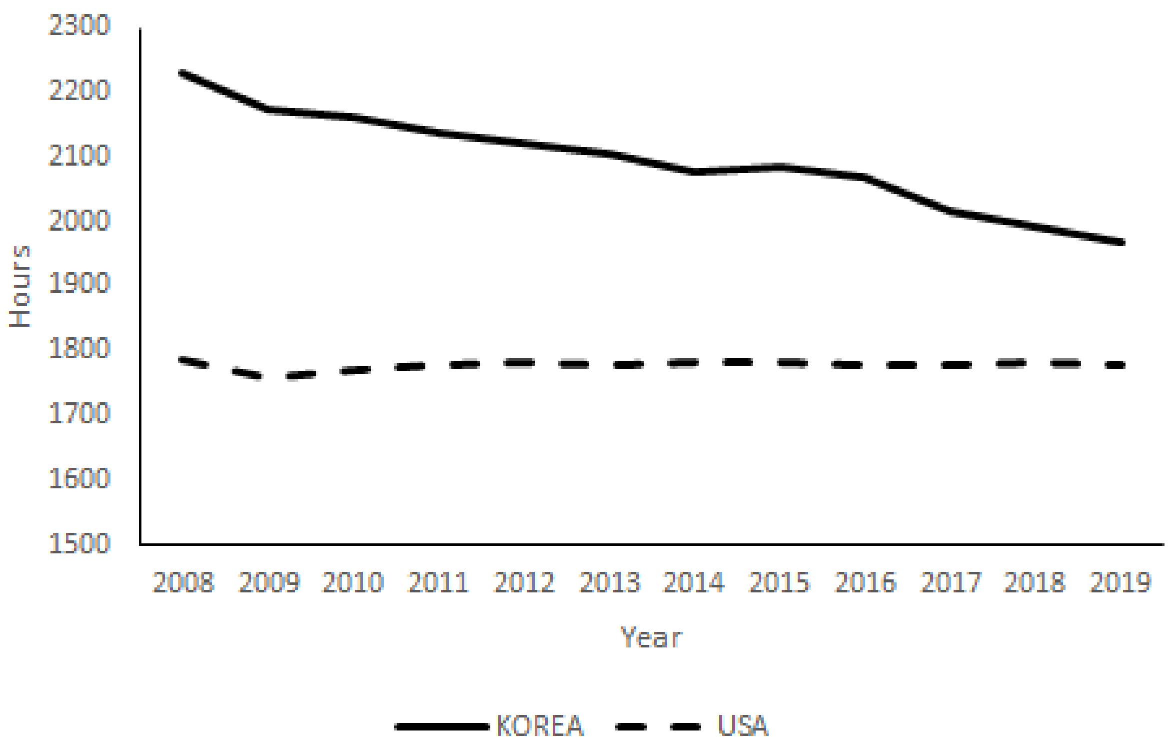
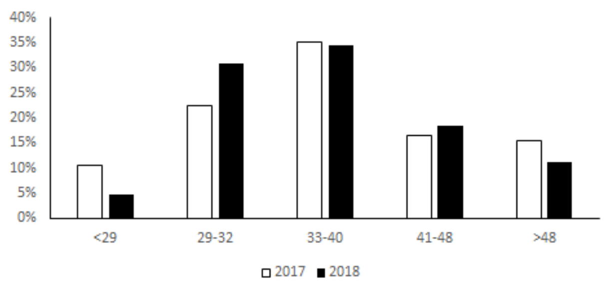
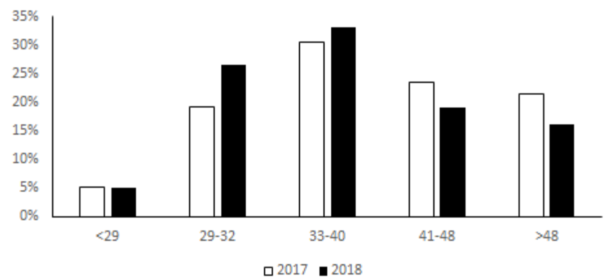


Figure 3.1: Average Annual Hours Worked by Year

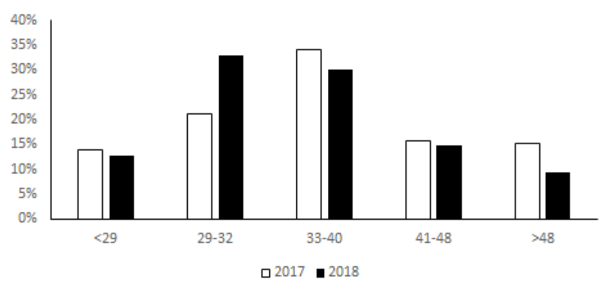
Note : Average annual hours worked is defined as the total number of hours actually worked per year divided by the average number of people in employment per year. (*Source* : OECD Employment and Labor Market Statistics)



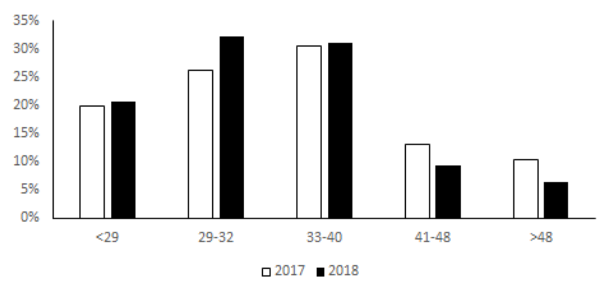
(a) Big Firms in Treated Industry



(b) Small Firms in Treated Industry



(c) Big Firms in Control Industry



(d) Small Firms in Control Industry

Figure 3.2: Distribution of Weekly Hours Worked

decreased by an average of 0.76 hours. Meanwhile, the hours worked weekly for the control firm cohort dropped even more, by 2.11 hours. The difference-in-difference estimation shows that the hours of work increased for the treatment workers by an average of 1.35 hours relative to the control workers. This change DD_{TI} is the 'within treatment industry' difference in difference. Likewise,the 'within control industry' difference in difference estimate is DD_{CI} =-0.66. I then report a 'triple difference' estimate of the effect from the work hour reduction at the bottom of Table 3.2 by putting together the upper and lower panels. This estimate is DDD=2.01 hours. It is statistically significant, which indicates that employees working for big firms in treated industries worked 2.01 more hours on average per week after the 52-hour workweek regulation came into effect.

Table C.1 and Table C.2 breaks down the sample by sex. The average weekly hours worked for the treatment group of men increased by a statistically insignificant 1.4 hours. On the other hand, women in the treatment group worked 3.34 hours more on average. The results suggest that treated women worked more by taking advantage of the flexible working time arrangements that became widely available in treated firms after the policy change. Given that workers were averaging hourly totals below the legal maximum per week before the policy change, it could have been the flexibility in daily working hours rather than the statutory limit that drove the workers' weekly average to rise. That is, women may have experienced an increased willingness to work more hours as a result of the improved flexibility in work schedules.[8] I will explore and elaborate more on this interpretation in the remainder of the paper.

[8]Raposo and Van Ours (2010) argues that the increased flexibility in the use of the standard workweek made it easier to adjust the workforce at the intensive margin. This in turn, reduced job destruction which led to positive employment effects. Moreover, Herr and Wolfram (2012)'s study on the relationship between workplace flexibility and the labor force participation of mothers finds that inflexible work environments detach women from labor force at motherhood.

Table 3.2: Mean Hours of Work

Dependent Variable : Hours Worked per week, All Workers	Before(2017)	After(2018)	Difference
Panel A. Treated Industries			
Treatment size firms (employees $\geq$ 300)	39.29	38.93	-0.76
	(0.25)	(0.21)	(0.39)
	[1,778]	[1,804]	[3,582]
Control size firms (employees : 30-299)	41.82	39.91	-2.11***
	(0.19)	(0.18)	(0.28)
	[3,005]	[2,871]	[5,876]
Difference	-2.17	-0.82	DD_{TI}=1.35**
	(1.36)	(1.03)	(0.46)
	[4,783]	[4,675]	[9,458]
Panel B. Control Industries			
Treatment size firms (employees $\geq$ 300)	38.29	36.24	-2.31
	(0.77)	(0.65)	(0.56)
	[250]	[243]	[493]
Control size firms (employees : 30-299)	35.32	33.58	-1.65**
	(0.32)	(0.32)	(0.32)
	[1,363]	[1,315]	[2,678]
Difference	2.72	2.05	DD_{CI} =-0.66
	(2.80)	(2.45)	(0.57)
	[1,613]	[1,558]	[3,171]
DDD Estimator			DDD = 2.01**
			(0.72)
			[12,629]

Notes : There are 9 Treatment Industries(industry code : A, B, C, D, F, L, O, R, U) and 3 Control Industries(G, I, P) Standard errors clustered by industry and firm size are shown in parentheses. Number of observations are in square brackets. Significance levels marked as ** significant at 5% and *** at 1%.

3.3.3 Estimating Equations

The above identification strategy can be generalized to a regression formulation. I begin my analysis with before and after comparisons within the treated size firm, in treated industries,

$$y_i = \gamma_0 + \gamma_1 \cdot Post_i + \gamma_2 \cdot X_i + \varepsilon_i \tag{3.1}$$

where y_i is the outcome variable of interest corresponding to worker i working for a large firm that belongs to the treatment industry. $Post_i$ is an indicator for observation after the work hour reduction is in effect. That is, the observation is 1 from the 2018 survey and 0 for 2017. X_i is a set of controls for individual level characteristics. Although the coefficient γ_1 is intended to give the effects of the work hour reduction among workers in treatment size firms within treatment industries, it may be biased. For example, the GDP growth in between 2017-2018[9] may create a bias in the results from simple comparisons made over time within the treated firm-size cohort.

In order to remove the bias that could be a result of trends, I determine what was happening in a comparable group of workers; a group who worked in small firms that had less than 300 employees and therefore, were not subject to the policy. That is, I estimate the coefficient γ_1 in (1) for employees of small firms in 2018 that work for the treatment industry. I then take a difference between the differences between the treatment-size firms and control-size firms before and after the work hour reduction.

The above identification strategy can be expressed using the difference-in-difference model of the following form:

$$y_i = \delta_0 + \delta_1 \cdot S_i \cdot Post_i + \delta_2 \cdot S_i + \delta_3 \cdot Post_i + \delta_4 \cdot X_i + \varepsilon_i \tag{3.2}$$

where y_i is the outcome variable of interest corresponding to worker i working in the treatment industry. S_i is an indicator for an observation from a treatment-size firm cohort. S_i is equal to one if a worker is working for a large firm, defined as a firm with 300 or more

[9]Korea's GDP grew by 3.2% in 2017 and 2.9% in 2018.(Korean Statistical Information Service, https://kosis.kr)

employees. In this setup, the analysis is between two worker groups in the treated industry. I compare before and after changes of the treatment-size firm cohort to that of control-size firm cohort within the treated industry. The potential problem with this DD analysis is that other factors unrelated to the work hour reduction, such as changes in the emphasis on supporting small and medium sized businesses at the national level[10], might affect the work hour decision of the treatment-size firm cohort relative to the control-size cohort in treated industries.

I also consider a different DD analysis by using large firms in a control industry as the control group. In this setup, the analysis is between large firms in the treated and control industries. I compare the before and after changes of the large firm's work hours in industries that were subject to the work hour reduction regulation to those that were not. This DD specification can be written

$$y_i = \delta_0 + \delta_1 \cdot I_i \cdot Post_i + \delta_2 \cdot I_i + \delta_3 \cdot Post_i + \delta_4 \cdot X_i + \varepsilon_i \tag{3.3}$$

where y_i is the outcome variable of interest corresponding to worker i working for a large firm that is either in the treated or the control industry. I_i is an indicator which equals one if the observation is from a worker employed by a firm that belongs to the treatment industry. The problem with this analysis is that changes in the work hours of large firms might be systematically different across industries due to inherent differences in the way the business is carried out rather than the policy change.

I therefore construct a triple difference (DDD) estimate of the work hour reduction impact by using two control groups simultaneously. The first control group is comprised of workers in control-size firms while the second control group is of workers in control industries. Workers in control industries serve as a useful control group for the work hour reduction because they

[10]Supporting small and medium enterprises in the country's conglomerate-driven economy was one of the key election pledges of president Moon Jae-in. Shortly after Moon came into office, a new ministry supporting small and medium-sized enterprises and venture startups was created. During 2017-2018, various initiatives were taken to support small, medium enterprises which include providing subsidies and tax incentives. In addition, South Korea's Financial Services Commission and the Ministry of SMEs and Startups launched a 9 billion dollar investment fund to foster start-ups.

would have been exposed to all the other changes that were taking place in Korea, such as the GDP growth and increased emphasis on supporting small and medium sized businesses, but they were exempt from the work hour reduction policy. Thus, the DDD estimate is immune to industry-specific shocks as well as firm size-cohort specific shocks. The triple difference estimate of exposure to the decreased number of standard hours is estimated by

$$\begin{aligned} y_i = {} & \beta_0 + \beta_1 \cdot S_i \cdot Post_i \cdot I_i + \beta_2 \cdot S_i \cdot Post_i + \beta_3 \cdot Post_i \cdot I_i + \beta_4 \cdot S_i \cdot I_i + \beta_5 \cdot S_i \\ & + \beta_6 \cdot Post_i + \beta_7 \cdot I_i + \beta_8 \cdot X_i + \varepsilon_i \end{aligned} \tag{3.4}$$

The coefficient of interest is β_1, the coefficient on the triple interaction term $S_i \cdot Post_i \cdot I_i$. The OLS estimate $\hat{\beta}_1$ can be expressed as follows:

$$\begin{aligned} \hat{\beta}_1 = {} & (\bar{y}_{largefirm,Treatindustry,Post=1} - \bar{y}_{largefirm,Treatindustry,Post=0}) \\ & - (\bar{y}_{smallfirm,Treatindustry,Post=1} - \bar{y}_{smallfirm,Treatindustry,Post=0}) \\ & - (\bar{y}_{largefirm,Controlindustry,Post=1} - \bar{y}_{largefirm,Controlindustry,Post=0}) \\ & - (\bar{y}_{smallfirm,Controlindustry,Post=1} - \bar{y}_{smallfirm,Controlindustry,Post=0}) \end{aligned} \tag{3.5}$$

Summary statistics in Table 3.3 break down the means for selected variables by both firm size and industry cohort. Characteristics at the individual level indicate that workers in the treatment industry are older, have less years of schooling, are more likely to be male and to reside in non-metropolitan areas than their counterparts in control industries. At the employment characteristic level, workers in the treated industry are more likely to be regular workers which means they have more stable contracts with their employers.[11] In order to remove fixed industry effects and time trends, I make before-and-after comparison of treatment and control group workers within both treatment and control industries. I then subtract the double difference in the control industry from the double difference of the treatment industry to net out firm size-varying characteristics that could affect hours of work.

[11] Regular employees are those employees with stable contracts for whom the employing organisation is responsible for payment of taxes and social security contributions and/or where the contractual relationship is subject to national labour legislation.(The OECD Glossary of Statistical Terms, https://stats.oecd.org/glossary)

Table 3.3: Summary Statistics of Individuals in 2017

Industry	Treatment Industry		Control Industry	
Firm size Cohort	Large Firm	Small Firm	Large Firm	Small Firm
A.Individual Characteristics				
Age	40.658	41.489	39.776	41.477
	(0.191)	(0.165)	(0.546)	(0.238)
Years of Education	14.603	13.632	16.316	15.086
	(0.042)	(0.037)	(0.150)	(0.049)
Male	0.784	0.719	0.594	0.432
	(0.008)	(0.007)	(0.024)	(0.011)
With Spouse	0.741	0.646	0.617	0.663
	(0.009)	(0.007)	(0.025)	(0.011)
Metropolitan Area	0.865	0.786	0.939	0.904
	(0.006)	(0.006)	(0.011)	(0.006)
Regular Worker	0.963	0.892	0.866	0.837
	(0.004)	(0.005)	(0.017)	(0.008)
Total Sample	1,778	3,005	250	1,363

3.3.4 Parallel Trends

The identification relies on the parallel trend assumption that, in the absence of treatment, the number of hours worked would have evolved similarly for the treatment and the control. To evaluate the validity of this identification assumption, I follow Muralidharan and Prakash (2017) and compare the change in number of hours worked for treatment and control size firms across treatment and control industries in the 4 years leading up to 2017.

$$\begin{aligned} Hours_i = \gamma_0 + \gamma_1 \cdot I_i \cdot S_i \cdot T_i + \gamma_2 \cdot I_i \cdot S_i + \gamma_3 \cdot I_i \cdot T_i + \gamma_4 \cdot S_i \cdot T_i \\ + \gamma_5 \cdot I_i + \gamma_6 \cdot S_i + \gamma_7 \cdot T_i + \gamma_8 \cdot X_i + \varepsilon_i \end{aligned} \tag{3.6}$$

where $Hours$ is the average number of weekly hours worked for worker i. I_i is a dummy indicating whether an individual works in a treated industry. S_i is an indicator variable identifying workers in large firms of 300 or more employees. T_i is a measure of pre trends before the work hour reduction, which codes year 2014, 2015, 2016 and 2017 as 1, 2, 3 and 4 respectively.

Table 3.4 shows that the coefficients on the triple interaction term for all groups fall in at a large confidence interval which includes zero. Estimates on the pretrends thus fail to reject that there is no difference in the trends before the work hour reduction across the control and treatment firms in control and treatment industries.

In addition, I follow Hamermesh and Trejo (2000) and report triple difference estimates in 3.4 using a placebo year, 2017. If a statistically significant estimate appears over 2016-2017 period, it would raise concerns that my model could be simply reflecting ongoing trends inherent to the treatment group of firms. Table 3.5 presents estimates comparing 2016 and 2017 period when there were no major changes to standard work hour law. The estimate of the triple difference terms in all columns are not statistically different from zero, providing further support that the difference in work hours between the large and small firms would not have systematically differed across treatment and control industries.

Table 3.4: Testing the Parallel Trends Assumption, 2014-2017

Dependent Variable : Hours Worked per week			
	All	Men	Women
Treatment industry x Large firm x Year	-0.551	-1.262	0.373
	(0.735)	(0.910)	(0.533)
Large firm x Treatment industry	-2.822	-0.365	-4.228***
	(1.523)	(2.081)	(1.137)
Treatment industry x Year	-0.247	0.081	-0.433
	(0.309)	(0.326)	(0.348)
Large firm x Year	0.634	1.206	0.116
	(0.709)	(0.890)	(0.476)
Demographic controls	Yes	Yes	Yes
Observations	24,780	16,534	8,246

Notes : The analysis uses four years of data prior to 52-hour work week regulation with years being coded as 1,2,3 and 4. Demographic controls include age, years of education, dummies for 12 industries, regular workers, male workers and having a spouse. Robust standard errors, corrected for clustering by industry and firm size are shown in parenthesis.

Table 3.5: Testing the Parallel Trends Assumption, 2016-2017

Dependent Variable : Hours Worked per week			
	All	Men	Women
Treatment industry x Large firm x Post	2.179	2.125	0.629
	(1.508)	(2.026)	(1.045)
Large firm x Treatment industry	-6.253***	-6.077***	-3.465**
	(1.008)	(0.950)	(1.278)
Treatment industry x Post	-1.197***	-1.013	-1.270
	(0.421)	(0.639)	(0.651)
Large firm x Post	-1.552	-1.243	-1.251
	(1.173)	(1.760)	(0.641)
Demographic controls	Yes	Yes	Yes
Observations	12,557	8,267	4,290

Notes : The analysis uses 2016 and 2017 data with *Post* dummy being 1 for observations from 2017. Robust standard errors, corrected for clustering by industry and firm size are shown in parenthesis.

3.4 Results

3.4.1 Number of Hours worked

Table 3.6 reports a "triple difference"(DDD) estimate of the effect from the reduced work hour maximum by all workers, male, and female workers, respectively. Column 1 presents estimates for all workers. β_1 is 1.92 hours and is statistically significant. This means that in 2018, workers from big firms in affected industries out-worked their counterparts by 1.92 more hours per week than they did in 2017. In column 2, the sample is restricted to male workers and there is no significant effect from the work hour reduction. The estimate is statistically indistinguishable from zero and the coefficient estimate on the triple interaction term is 3 times smaller in magnitude compared to women sampled. Results from column 2 suggest that the overall positive effect comes from the change in the number of hours women worked following the policy implementation. In column 3, I estimate that the work hour reduction increased hours worked by female workers in large firms within treated industries by 3.59 hours per week. This increase represents a 9.5%(3.59/37.86=9.5) increase when measured against the average number of weekly hours worked for women working for large firms in treated industries in 2017.

Table 3.6: Impact on the Number of Hours Worked

Dependent Variable : Hours Worked per week			
	All	Men	Women
Large firm x Post x Treatment Industry	1.922***	1.052	3.593***
	(-0.69)	(-0.659)	(-0.841)
Large firm x Post	-0.422	0.353	-1.829***
	(-0.490)	(-0.382)	(-0.485)
Large firm x Treatment industry	-4.318***	-4.158**	-3.230**
	(-1.407)	(-1.705)	(-1.390)
Treatment industry x Post	-0.716	-0.539	-1.177**
	(-0.420)	(-0.463)	(-0.565)
Large firm	2.346	1.717	3.168**
	(-1.366)	(-1.646)	(-1.349)
Post	-1.477***	-1.529***	-1.512***
	(-0.287)	(-0.287)	(-0.3)
Treatment Industry	-0.144	2.762	3.719***
	(-3.449)	(-4.538)	(-0.787)
Demographic Controls	Yes	Yes	Yes
Observations	12,629	8,254	4,375

Notes : Demographic controls include age, years of education, dummies for 12 industries, regular workers, male workers and having a spouse. Robust standard errors, corrected for clustering by industry and firm size are shown in parenthesis.

3.4.2 Working Time Flexibility

Next, I explore the relationship between the possibility of using flexible work hours and the 52-hour work week regulation. In Korea, the working hours for women are more constrained than they are for men, due to the time women devote to their household responsibilities.[12] Work by Messenger et al. (2007) argues that the availability of policies designed to support workers who have family responsibilities can help increase working hours for women. If, after the work hour reduction, women in the treated group are more likely to have working time flexibility than their peers in the control group, this may explain the increased number of hours worked shown in Table 3.6. Table 3.7 indicates there is a greater possibility of flexible working hours being used by the treatment group of female workers after the policy change. In column 3, I find that female workers working for large firms in a 52-hour workweek industry are 6.5 percentage points more likely to have used flexible working hour arrangements than female workers in control firms.

[12] Results from time-use survey conducted by Statistics Korea in 2014 show that women spent an average of 259 minutes per day on household tasks and family care, compared with only 50 minutes for men.

Table 3.7: Impact on Using Flexible Working Time Arrangement

Dependent Variable : 1[Using Flexible Working Time Arrangement]			
	All	Men	Women
Large firm x Post x Treatment Industry	10.018	0.001	0.065***
	(0.017)	(0.019)	(0.023)
Large firm x Post	0.059***	0.077***	0.014
	(0.013)	(0.016)	(0.013)
Large firm x Treatment industry	-0.014	-0.011	-0.056
	(0.026)	(0.022)	(0.049)
Treatment industry x Post	0.011	-0.010	0.026
	(0.014)	(0.016)	(0.018)
Large firm	0.071***	0.081***	0.067***
	(0.013)	(0.013)	(0.023)
Post	0.029***	0.050***	0.013*
	(0.010)	(0.014)	(0.008)
Treatment Industry	0.024	0.037*	0.163
	(0.028)	(0.021)	(0.102)
Demographic Controls	Yes	Yes	Yes
Observations	12,629	8,254	4,375

3.4.3 Number of people employed

Finally, I examine the employment effect of the reduced work week at the firm level. If the 52-hour workweek regulation led to more weekly work hours per worker, then there should be only a limited effect on the number of jobs created within a treated group of firms. While the Labor Force Survey at Establishments(LFSE) provides information on the number of employees at the firm level, it does not disclose how the overall workforce is split between men and women. Therefore, I confirm my results in the previous section only on overall population. The results presented in Table 3.8 suggest no meaningful effect of the work hour reduction on job creation. The DDD coefficient is 0.911 and the confidence interval is large [-0.591, 2.413] at the 95% level. From the confidence intervals, I can rule out effects larger than a 0.6 decrease in the number of employees. This result is also consistent with studies that find little support for a positive employment effect from a standard work hour reduction (Chemin and Wasmer (2009), Estevão and Sá (2008))

3.5 Conclusion

In this paper, I examine how a statutory work hour reduction affects the actual number of hours worked by employees. To do so, I combine a triple difference strategy and a large national labor force survey. After the weekly work hour maximum was reduced to 52 hours, work hours increased for treated workers relative to the workers in control firms. The data gives strong evidence that women worked more after the policy change while men's work hours remained largely the same. Moreover, women working for firms subject to reduced work hour regulations were more likely to use flexible working time arrangements. My findings suggest that this side-effect from the work hour reduction, that firms offered more flexible working hour arrangements, drove an adjustment of the workforce at the intensive margin rather than at the extensive margin.

Table 3.8: Impact on Number of Employees

Dependent Variable : Number of Employees	
	All
Large firm x Post x Treatment Industry	0.911
	(0.731)
Large firm x Post	-0.612***
	(0.218)
Large firm x Treatment industry	16.691***
	(5.185)
Treatment industry x Post	0.141
	(0.09)
Large firm	25.164***
	(4.682)
Post	-0.286***
	(0.031)
Treatment Industry	-0.202
	(1.898)
Regional Fixed Effects	Yes
Observations	157,413

Notes : Each observation corresponds to a job classification within a firm. For example, a firm with positions for managers, technicians and administrative assistants counts as 3 observations. Robust standard errors, corrected for clustering by industry and firm size are shown in parenthesis.

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Appendix A

THE EFFECTS OF THE US ATHLETE VISA POLICY CHANGE ON DOMINICAN BOYS' EDUCATION

As an alternative specification, I use variation in the birthplaces of Dominican minor league players. Using the 2006-2010 minor league roster, I ranked municipalities in the Dominican Republic by the number of players who were born there. Table A.1 reports the number of players and the distance from MLB academies for the top 12 municipalities.

Table A.1: Birth Place of Minor League Baseball Players from the Dominican Republic

Rank	Place of Birth	number of players	Distance(mi) from MLB academies
1	Santo Domingo de Guzman	1,317	9.9
2	San Pedro de Macorís	568	0
3	Baní	353	32.7
4	Santiago	334	165
5	San Cristóbal	292	16.2
6	La Romana	240	43.4
7	Azua	138	83.2
8	Bonao	80	91.6
9	San Francisco de Macorís	79	142
10	San Juan	77	161
11	Cotuí	77	95.7
12	El Seibo	76	60.5

Table A.2: Impact on Years of Schooling of Dominican Boys, Using Variation in the Place of Birth

Dependent variable : Years of Schooling	(1)	(2)	(3)	(4)
Panel (a) : Difference				
Post	0.369***	0.366**	0.567***	0.394***
	(0.085)	(0.135)	(0.056)	(0.085)
Male Aged 14-17		-2.003***		-1.896***
		(0.136)		(0.067)
MLB Municipality			0.094	0.081
			(0.122)	(0.140)
Panel (b) : Difference in Difference				
Post ×Male Aged 14-17		-0.102		0.049
		(0.067)		(0.076)
Post ×MLB Municipality			-0.189**	0.160***
			(0.088)	(0.049)
Male Aged 14-17 ×MLB Municipality				-0.084**
				(0.039)
Panel (c) : Difference in Difference in Difference				
Post ×Male Aged 14-17 × MLB Municipality				-0.245
				(0.142)
Control variables				
Household Characteristics	Yes	Yes	Yes	Yes
Observations	23,023	44,410	66,894	127,279

Table A.3: Impact on Primary School Completion, Using Variation in the Place of Birth

Dependent variable : 1[completed primary school]	(1)	(2)	(3)	(4)
Panel (a) : Difference				
Post	0.056***	-0.004	0.080***	0.008
	(0.017)	(0.017)	(0.011)	(0.008)
Male Aged 14-17		-0.240***		-0.254***
		(0.010)		(0.006)
MLB Municipality			0.010	0.002
			(0.024)	(0.015)
Panel (b) : Difference in Difference				
Post ×Male Aged 14-17		0.070***		0.074***
		(0.010)		(0.007)
Post ×MLB Municipality			-0.021	0.005
			(0.017)	(0.008)
Male Aged 14-17 ×MLB Municipality				0.022
				(0.017)
Panel (c) : Difference in Difference in Difference				
Post ×Male Aged 14-17 × MLB Municipality				-0.008
				(0.011)
Control variables				
Household Characteristics	Yes	Yes	Yes	Yes
Observations	23,023	44,410	66,894	127,279

Appendix B

LABOR MARKET CONSEQUENCES OF PROVIDING UNCONDITIONAL TRANSFERS TO YOUNG INDIVIDUALS : EVIDENCE FROM A SOCIAL WELFARE EXPERIMENT IN SEONGNAM CITY, KOREA

Table B.1: Testing the Parallel Trends Assumption, 25 Year-Olds as a Control Group

Dependent variable : 1(Employed)	Linear Probability	Probit
Point estimate	-0.0950*	-0.0979***
Lower bound 95% conf. interval	-0.1644	-0.1080
Upper bound 95% conf. interval	-0.0256	-0.0877
Number of observation	762	762

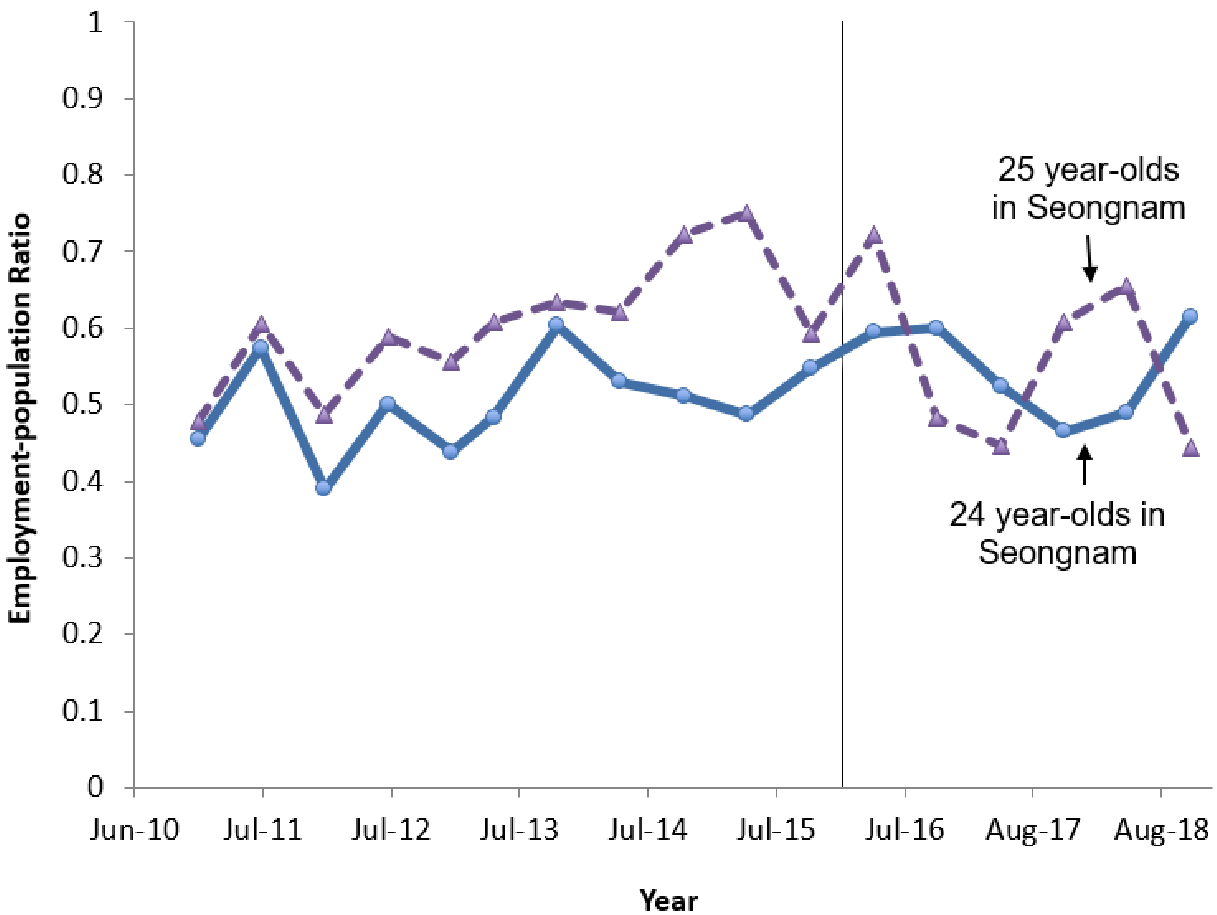

Figure B.1: Testing the Parallel Trends Assumption, 25 Year-Olds as a Control Group

Table B.2: Synthetic Seongnam Donor City Weights for Employment Population Ratio

City	Weight	City	Weight
Suwon	0	Uiwang	0
Uijeongbu	0	Hanam	0
Anyang	0.369	Yongin	0.231
Bucheon	0	Paju(M)	0
Gwangmyeong	0	Icheon(W)	0.012
Pyeongtaek	0.058	Anseong	0.035
Dongducheon(M)	0	Gimpo(M)	0
Ansan	0	Hwaseong	0
Goyang	0.236	Gwangju(W)	0
Gwacheon	0	Yangju(M)	0
Guri	0	Pocheon(M)	0
Namyangju(W)	0	Yeoju(W)	0
Osan	0	Yeoncheon(M)	0.06
Siheung	0	Gapyeong(W)	0
Gunpo	0	Yangpyeong(W)	0

Notes : Weights are obtained from a donor pool which includes all 30 cities in Gyeonggi province. Cities with (M) have military protection zones within their city boundaries. Cities with (W) have areas designated as Paldang lake special measure areas for water quality conservation.

Table B.3: Synthetic Seongnam Donor City Weights for Labor Force Participation Rate

City	Weight	City	Weight
Suwon	0	Uiwang	0.342
Uijeongbu	0	Hanam	0
Anyang	0	Yongin	0.126
Bucheon	0	Paju(M)	0
Gwangmyeong	0	Icheon(W)	0
Pyeongtaek	0	Anseong	0
Dongducheon(M)	0	Gimpo(M)	0
Ansan	0	Hwaseong	0
Goyang	0.036	Gwangju(W)	0
Gwacheon	0	Yangju(M)	0.048
Guri	0	Pocheon(M)	0
Namyangju(W)	0	Yeoju(W)	0
Osan	0	Yeoncheon(M)	0
Siheung	0.253	Gapyeong(W)	0
Gunpo	0.195	Yangpyeong(W)	0

Notes : Weights are obtained from a donor pool which includes all 30 cities in Gyeonggi province. Cities with (M) have military protection zones within their city boundaries. Cities with (W) have areas designated as Paldang lake special measure areas for water quality conservation.

Table B.4: Synthetic Seongnam Predictor Weights

Predictor Variable	Weight
Panel (a) : Employment Population Ratio	
Years of schooling	0.034
Percentage of male population	0.033
Rate of wage growth	0.001
Ln(GRDP) 2010	0.170
Ln(GRDP) 2013	0.050
Employment ratio in 2010 2H	0.643
Employment ratio in 2014 2H	0.015
Employment ratio in 2015 1H	0.054
Panel (b) : Labor Force Participation Rate	
Years of schooling	0.065
Percentage of male population	0.057
Rate of wage growth	0.018
Ln(GRDP) 2013	0.026
Labor Force Participation Rate in 2012 2H	0.092
Labor Force Participation Rate in 2014 2H	0.552
Labor Force Participation Rate in 2015 1H	0.064
Labor Force Participation Rate in 2015 2H	0.125
Panel (c) : Hours worked Last Week	
Years of schooling	0.023
Percentage of male population	0.007
Rate of wage growth	0.107
Ln(GRDP) 2014	0.012
Hours worked Last Week 2012 1H	0.432
Hours worked Last Week 2013 1H	0.284
Hours worked Last Week 2014 1H	0.102
Hours worked Last Week 2015 2H	0.032

Table B.5: Synthetic Seongnam Donor City Weights

City	Weight
Panel (a) : Employment Population Ratio	
Anyang	0.441
Goyang	0.288
Yongin	0.217
Pyeongtaek	0.054
Panel (b) : Labor Force Participation Rate	
Uiwang	0.419
Siheung	0.264
Gunpo	0.160
Goyang	0.089
Yongin	0.054
Anyang	0.013
Panel (c) : Hours worked Last Week	
Yangpyeong	0.414
Anyang	0.221
Goyang	0.188
Gwangmyeong	0.103
Paju	0.074

Appendix C

EMPLOYMENT EFFECTS OF THE 52-HOUR WORKWEEK REGULATION IN KOREA

Table C.1: Mean Hours of Work, Men

Dependent Variable : Hours Worked per week, Men	Before(2017)	After(2018)	Difference
Panel A. Treated Industries			
Treatment size firms (employees $\geq$ 300)	39.66	39.41	-0.77
	(0.28)	(0.25)	(0.42)
	[1,408]	[1,395]	[2,803]
Control size firms (employees : 30-299)	42.90	40.93	-2.06***
	(0.22)	(0.22)	(0.39)
	[2,075]	[2,021]	[4,096]
Difference	-2.67*	-1.37	DD_{TI}=1.30*
	(1.10)	(0.69)	(0.56)
	[3,483]	[3,416]	[6,899]
Panel B. Control Industries			
Treatment size firms (employees $\geq$ 300)	39.04	37.11	-1.78*
	(1.11)	(0.89)	(0.39)
	[141]	[140]	[281]
Control size firms (employees : 30-299)	37.95	36.25	-1.68**
	(0.48)	(0.48)	(0.35)
	[550]	[524]	[1,074]
Difference	0.72	0.62	DD_{CI}=-0.11
	(2.65)	(2.32)	(0.48)
	[691]	[664]	[1,355]
DDD Estimator			DDD = 1.40
			(0.72)
			[8,254]

Table C.2: Mean Hours of Work, Women

Dependent Variable : Hours Worked per week, Women	Before(2017)	After(2018)	Difference
Panel A. Treated Industries			
Treatment size firms (employees $\geq$ 300)	37.86	37.29	-0.58
	(0.49)	(0.43)	(0.40)
	[370]	[409]	[779]
Control size firms (employees : 30-299)	39.41	37.47	-2.33***
	(0.34)	(0.33)	(0.33)
	[930]	[850]	[1,780]
Difference	-1.60	0.15	DD_{TI}=1.75**
	(1.93)	(1.74)	(0.50)
	[1,300]	[1,259]	[2,559]
Panel B. Control Industries			
Treatment size firms (employees $\geq$ 300)	37.32	35.07	-3.18
	(1.02)	(0.94)	(0.84)
	[109]	[103]	[212]
Control size firms (employees : 30-299)	33.53	31.80	-1.59**
	(0.41)	(0.41)	(0.37)
	[813]	[791]	[1,604]
Difference	3.92	2.33	DD_{CI}=–1.59
	(2.80)	(2.37)	(0.81)
	[922]	[894]	[1,816]
DDD Estimator			$DDD =$ 3.34**
			(0.92)
			[4,375]

Table C.3: Industry classification and DDD Identification

Level 1: Section	Description	Level 2: Division	Division that received extension until June, 2019	Division that is exempt from 52-hour workweek	Classification in DDD analysis
A	Agriculture, forestry and fishing	(1-3)			Treatment
B	Mining and quarrying	(5-8)			Treatment
C	Manufacturing	(10-34)			Treatment
D	Electricity, gas, steam and air conditioning supply	(35)			Treatment
E	Water supply; sewage, waste management, materials recovery	(36-39)	37. Sewage, wastewater, human and animal waste treatment services		Excluded
F	Construction	(41-42)			Treatment
G	Wholesale and retail trade	(45-47)	45.Sale of motor vehicles and parts/ 46.Wholesale trade on own account or on a fee or contract basis/ 47.Retail trade, except motor vehicles and motorcycles		Control
H	Transportation and storage	(49-52)	521.Warehousing and storage	49. Land transport and transport via pipelines /50.Water transport/ 51.Air transport/ 529.Support activities for transportation	Excluded
I	Accommodation and food service activities	(55-56)	55. Accommodation/ 56. Food and beverage service activities		Control
J	Information and communication	(58-63)	611.Postal activities/ 612. Telecommunications/ 59. Motion picture, video and television programme production, sound recording and music publishing activities/ 60. Broadcasting activities		Excluded
K	Financial and insurance activities	(64-66)	64. Financial service activities, except insurance and pension funding/ 65. Insurance and pension funding/ 66. Activities auxiliary to financial service and insurance activities		Excluded
L	Real estate activities	68			Treatment
M	Professional, scientific and technical activities	(70-73)	70. Research and development/ 714. Market research and public opinion polling/ 713. Advertising		Excluded
N	Business facilities management and business support services; rental and leasing activities	(74-76)	742. Cleaning and pest control services of building and industrial facilities		Excluded
O	Public administration and defence; compulsory social security	(84)			Treatment
P	Education	(85)	85.Education		Control
Q	Human health and social work activities	(86-87)	87.Social work activities	86. Human health activities	Excluded
R	Arts, sports and recreation related services	(90-91)			Treatment
S	Membership organizations, repair and other personal services	(94-96)	961.Personal care services		Excluded
U	Activities of extraterritorial organizations and bodies	(99)			Treatment