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Luis Diego Granera Vega

Essays on Health and Development

Luis Diego Granera Vega

A dissertation
submitted in partial fulfillment of the
requirements for the degree of

Doctor of Philosophy

University of Washington

2022

Reading Committee:

Rachel Heath, Chair

Alan Griffith

Melissa Knox

Program Authorized to Offer Degree:
Economics

University of Washington

Abstract

Essays on Health and Development

Luis Diego Granera Vega

Chair of the Supervisory Committee:
Rachel Heath
Department of Economics

This dissertation is comprised of three related essays pertaining to healthcare and its relationship to economic development using data from Costa Rica. In particular, it discusses the economic, educational, and public health implications of a wide-ranging health reform in the context of a country with universal healthcare.

In chapter 1, I study whether a coordinated effort by a government to expand access to healthcare through increased expenditure would bring about higher incomes at the household level. Using data from the major healthcare reform that Costa Rica underwent starting in 1994, which greatly increased access to primary healthcare, I explore the effects of getting local clinics (known as *EBAIS*) in their district as a result of the reform on household and personal income. Based on theoretical work by Strauss and Thomas (1998) and a seminal paper by Grossman (1972), my main hypothesis is that, by producing healthier individuals, having increased access to healthcare incentivizes people to pursue better-paying jobs, thereby increasing their income. I proceed by difference-in-difference, as well as by an event study specification, both of which point to a positive average impact of around 4% of getting a clinic on income.

In chapter 2, I explore the effects of the healthcare reform on education. A difference-in-difference analysis points to an increase of 0.07 to 0.11 years of education as a result of getting

an *EBAIS* stemming from the reform. Additionally, using the statistical tool of mediation, I find that as a result of getting an *EBAIS*, and through the channel of household income, the average person increased their level of education by a significant 0.69 years (with people in the study having on average 5.6 years of education). The mediation analysis demonstrates that increases in household income from the reform, shown to exist in Chapter 1, account for a significant 4.3% of the increase in years of education, with the rest attributable to other channels.

In chapter 3, I revisit and update the results on mortality from Rosero-Bixby (2004) [67]. Using close to a decade's-worth of new data, my goal was update his results and determine if they had persisted since the publication of his study. I find a highly statistically significant 5.62% decrease in general mortality, a 22.75% decrease in under-five infant mortality, both attributable to the healthcare reform that created the *EBAIS*, and through the channel of reductions in infectious and chronic illnesses. These numbers are higher than Rosero-Bixby's results of a 2% reduction in general mortality and an 8% decrease in infant mortality, thereby showing that the reform has continued to have effects since his initial paper.

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GLOSSARY

BCCR: Spanish abbreviation for "Banco Central de Costa Rica", the Costa Rican Central Bank.

CCP: Spanish abbreviation for "Centro Centroamericano de Población", the Central American Center for Population Research at University of Costa Rica.

CCSS: Spanish abbreviation for "Caja Costarricense de Seguro Social", the Costa Rican Social Security Administration, overseer of the *EBAIS* program.

EBAIS: Spanish abbreviation for "Equipos Básicos de Atención Integral en Salud", or Basic Integral Healthcare Teams, the basis of the 1994 Costa Rican healthcare reform.

INEC: Spanish abbreviation for "Instituto Nacional de Estadística y Censos", or National Statistics and Censuses Institute.

ACKNOWLEDGMENTS

I wish to express my sincerest gratitude to the members of my committee for their guidance every step of the way: my advisor, Dr Rachel Heath, for her continued help and encouragement; Dr Melissa Knox, for providing me with key insights and invaluable advice; and Dr Alan Griffith, for believing in me and my project and getting me started in the research process. I could not have done this without you.

Moreover, this work was facilitated through the use of advanced computational, storage, and networking infrastructure provided by the Hyak supercomputer system at the University of Washington. Partial support for this research came from a Eunice Kennedy Shriver National Institute of Child Health and Human Development research infrastructure grant, P2C HD042828, to the Center for Studies in Demography & Ecology (CSDE) at the University of Washington.

In addition, I want to thank the people at CCSS and INEC for helping me get the data that I used for this project, as well as Dr Luis Rosero-Bixby, Costa Rican pioneer in the study of the EBAIS program.

I can also not submit this dissertation without thanking the University of Washington's Economics Department, for believing in me when others would not.

A mi familia en Costa Rica, todo mi agradecimiento por su apoyo durante tantos años. Sin mi mamá y mis hermanos no estaría donde estoy hoy.

And finally, this entire process would not have been possible without the unfailing support of my wife, Michele Howard Granera. Thank you for standing by my side and spurring me on every day. "Two are better than one".

DEDICATION

to my dear wife, Michele

Chapter 1

GOVERNMENT, PRIMARY HEALTHCARE, AND STANDARD OF LIVING: EVIDENCE FROM COSTA RICA

1.1 Introduction

There is ample evidence of a directional relationship between household income and individual health through better insurance and improved access to healthcare. From a panel of 100 countries, Moreno-Serra and Smith (2012) [54] established that broader health insurance coverage leads to better access to healthcare and improved health, particularly for lower income groups. In addition, Case, Lubtsky and Paxson (2002) [17] found that health in children is positively correlated with household income. To that effect, Fichera (2015) [32] observed that increases in income reduce illness, while Contoyannis and Dooley (2010) [23] noted that socioeconomic status as children affects health as young adults.

On the other hand, Strauss and Thomas (1998) [71] laid theoretical groundwork to a causal positive impact of health on wages, while Thomas and Frankenberg (2002), in their Bulletin of the World Health Organization article [72] shared empirical results suggesting nutrition as a link between healthier workers and higher wages. And even whole populations can benefit economically from healthcare, as McDermott, Cornia, and Parsons (1991) concluded that getting a hospital in a rural community makes "significant economic contributions to the community they serve" [51].

We can therefore observe that, in one direction, higher income results in better health. The opposite direction, from better health to higher income, has been studied far less, which

leads me to the current study. We do know that adequate health insurance results in better care, which promotes improved health [54], which translates into higher wages, with theory [71] and suggestive empirical results [72], though so far this chain of events has not been studied as a whole. I propose making use of a government intervention to explain this process. In particular, I explore whether a coordinated effort by a government to increase access to healthcare through increased expenditure would bring about higher incomes at the household level. To that end, I use difference-in-difference and event study specifications with data from Costa Rica, finding an average increase of 4% in household incomes from getting a clinic through the reform. This comes to strengthen Thomas and Frankenberg's conclusions [72] through a different channel (improved health), provides further empirical evidence for Strauss and Thomas's theoretical model, and advances the understanding of government interventions in the healthcare sector in the context of universal health insurance.

There is evidence connecting access to healthcare to individuals' health level. For instance, it is known that bigger distances to healthcare clinics are correlated with worse health outcomes. Croke et al (2020) [24] studied a reform in Ethiopia that built almost 3,000 government clinics and its impact on maternal utilization and birth outcomes. They found increased prenatal care of 0.38 visits and a 7.2% increase in deliveries in-place per every new clinic opened within 5km. Additionally, focusing on the construction of maternity facilities in Malawi, Quattrochi et al (2020) [62] established that bigger distances to the clinics are related to lower healthcare utilization and higher under-five mortality. And similarly, using data from India, Kumar, Dansereau, and Murray (2014) [49] observed that women living farther from health facilities are less likely to give birth there. Each extra kilometer to the nearest clinic is associated with a 4.4% decrease in the probability of giving birth in-facility.

Finally, there has also been important previous work on whether health interventions can cause changes in income, just as educational interventions like building schools in Indonesia

did for those in treatment, as expressed in Duflo (2001) [28]. In their 'Worms at Work' paper (2016) [7], Baird, Hicks, Kremer, and Miguel studied the long-term effects of a de-worming intervention in Kenya (cf. Kremer and Miguel (2004) [52]). They found that men who were exposed to the treatment as children worked 17% more hours and that overall, incomes were on average 26.9% higher. Moreover, they concluded that the financial returns to de-worming of 32% more than made up for the cost of the program, underscoring the economic potential interventions like this one can have.

In summary, the sequence of events to test is that better health insurance increases healthcare availability and use, which improves health, increasing income. With near-universal health insurance, Costa Rica provides an ideal case study to test this hypothesis, as it allows me to skip the step of changing the health insurance status quo and start the analysis at changes in healthcare availability. In addition, Rosero-Bixby (2004) [67, 68] established the health improvements that came as a result of the reform. Consequently, the exercise is reduced to showing that increased availability of healthcare increased incomes.

A place where access to healthcare is determined by *physical availability* [50], not its *affordability*, Costa Rica underwent a healthcare reform in the mid-1990s that greatly expanded access to primary healthcare. Did this have an impact on household income? My analysis concludes an increase in household income of around 4% as a result of furthering access to primary healthcare.

1.2 Background

Before 1994, the management of the Costa Rican healthcare sector was shared between the Ministry of Health and the Social Security Administration [77]. The former was established in 1922 and, from its inception, its functions were focused on prevention and education, along with the provision of some basic medical care. The latter came to be in 1941 and it was designed to provide healthcare and pensions to all workers, minors, pregnant women, and the

poor, funded by a combination payroll taxes, employer taxes, and government contributions. For over fifty years they would share this partial overlap of functions.

The 1980s brought with the economic downturn both a steep increase in national debt and deep austerity measures, including cuts to healthcare spending [42]. This resulted in a decrease in service quality and patient satisfaction. As a response, in 1994 the government decided to enact a major reform to the healthcare system, switching all preventative, public health, and medical treatment responsibilities from the Ministry of Health to the Social Security Administration (*CCSS*), centralizing both funding and decision-making in order to improve efficiency. For comparison, achieving a similar level of integration in the United States would require merging the Centers for Disease Control and Prevention (CDC) with the Department of Veterans' Affairs (VA), and the Centers for Medicare and Medicaid Services [57].

The reform divided the country into 104 areas of $40,000$ to $100,000$ people each, where it would establish small clinics responsible for approximately $4,500$ people [57] led by teams comprised of a physician, a nurse, a community health worker, a pharmacist, and a clerk in charge of detailed health data collection [78]. (The first teams were established at already-existing clinics, to be followed by building more clinics.) This would provide patients with a first point of contact to healthcare, combining both preventative, acute, and chronic support, increasing the depth and breadth of care. These clinics received the name of *EBAIS*, which stands for *Equipos Básicos de Atención Integral en Salud*, Spanish for "Basic Integral Healthcare Teams".

As a result of this reform, and despite being a middle-income country, as defined by the World Bank [8], Costa Rica now exhibits improved health outcomes while spending less than most of the world [57] both per capita and as a proportion of real gross domestic product: Costa Rica spends only about \$970 per person per year, compared to $\$1,061$ in the rest of the world (measured in US \$ of 2014), and 9.3% of GDP, compared to 9.9%. Moreover, maternal,

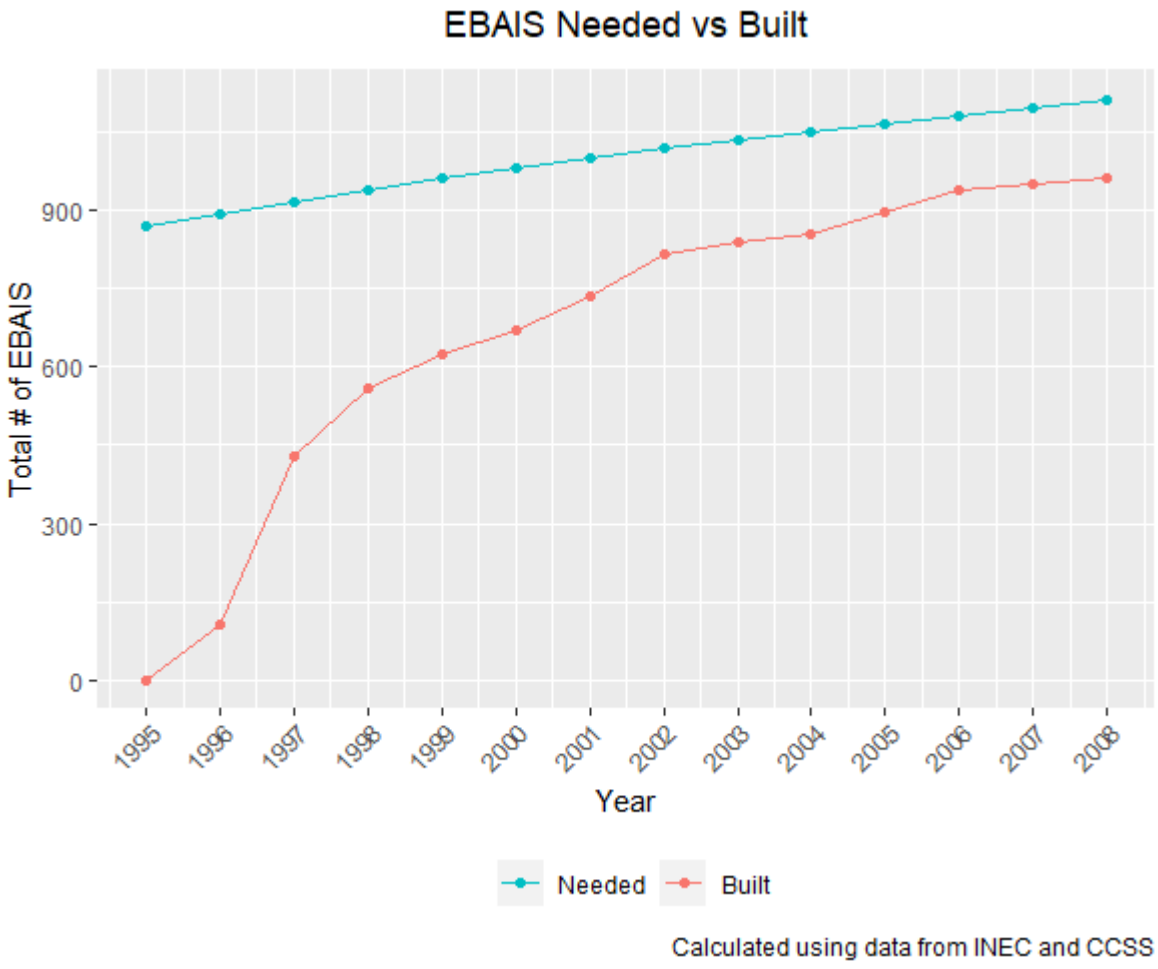

Figure 1.1: EBAIS built, compared to those needed to have one per every 4,500 people

infant, and under-five mortality indicators are low and have decreased steadily over the last quarter century, as established in his seminal papers on the Costa Rican healthcare reform by Rosero-Bixby (2004) [67], [68]. Also, life expectancy in Costa Rica is now nearly five years higher than the rest of Latin America, and almost eight years more than the world average. In fact, in the Western Hemisphere, only Chile and Canada have a higher life expectancy [58]. This gives Costa Rica the unlikely honor of having become a public health "positive deviant" [57].

The main policy implication of the reform was that the proportion of the national population with access to primary care (that is, within 4 km of their home [68]) went from 30% to 93% in the first twelve years alone. In 2014, the *EBAIS* clinics provided three-quarters of all medical consultations and covered the medical needs of 80% of all national patients, giving some much-needed relief to hospitals [57]. It was learning about this increase in access that piqued my interest, making me ask whether, in addition to public health improvements, any increases in household standard of living could be attributed to the reform.

1.3 Data

My main source of data is the *Encuesta de Hogares de Propósitos Múltiples* (*EHPM*), or "Multiple Purpose Household Survey", an annual, nationally-representative survey of Costa Rican households given by the Costa Rican Statistics and Censuses Institute (*INEC*) between 1987 and 2008. This provides me with a twenty-two-year pooled cross-section of household data of $30,000$ to $50,000$ observations per year that includes their district, number of people per household, age, education, personal and household income, as well as marital and insurance status of each member, among others. Moreover, in order to ensure that income variables are comparable across time, I use the Consumer Price Index (CPI), as calculated by the Costa Rican Central Bank (*BCCR*), in order to normalize incomes into constant July 2015 *colones*, the local currency. Additionally, to minimize the effect of outliers I winsorize

the personal and total household income data, substituting the extreme values beyond the 5th and 95th percentiles with the values at those percentiles.

In order to determine the districts that benefited from the healthcare reform, I use the Social Security Administration's Annual Memories (*Memorias Institucionales de la CCSS*) to record, year by year, the districts where new *EBAIS* clinics were built or opened from 1995 to 2008. This serves as a complement to the *EBAIS* district data from 1995 to 2001 kindly shared with me by Rosero-Bixby himself.

From among all the 892,317 observations, I focus on the households located in districts that did not already have a source of primary healthcare prior to the reform, like a hospital or clinic. Because the location of those is endogenous, using them would obscure the results of the analysis of the reform. Table 1.1 shows summary statistics for the remaining 313,977 observations: 157,176 individuals who lived in households whose district either never got an *EBAIS* clinic or before it gets one, along with 154,377 who lived in districts that got an *EBAIS* as a result of the reform. These will constitute the comparison and treatment groups, respectively. In terms of districts, that corresponds to 262 districts who "always" had a clinic, 59 that "never" did, and 152 that got one "later", as seen in Figure 1.2.

Both groups are distributed evenly with regards to number of observations, with around 155,000, as well as to number of people per household, with around 5. There is some difference in the proportions of males and females, with the control group having about 5 percentage points more in each. There is also a difference in age, with people in the treatment group being approximately 40 years old, as opposed to 30 in the control. Both groups are fairly similar in years of education, with the treatment group having on average only 0.4 years more. Finally, given an average exchange rate of 528 Costa Rican *colones* per US dollar at the time (income data is expressed in colones of July 2015), households in the comparison group had a monthly income of around $1040, compared to $1,000 in the treatment. Similarly, average personal monthly income in the comparison group was $205,

Table 1.1: Summary statistics.

Variable	**Control**[1], N=157,176	**Treatment**[1], N=154,1377	**p-value**[2]
# of people	5.09 (2.21)	4.80 (2.06)	<0.001
Sex			<0.001
Male	75,160/157,176 (48%)	67,147/154,377 (43%)	
Female	75,791/157,176 (48%)	66,566/154,377 (43%)	
Unknown	6,225/157,176 (4%)	20,664/154,377 (13%)	
Age	30 (24)	40 (29)	<0.001
Years of education	5.4 (4.3)	5.8 (4.0)	<0.001
Household income	549,560 (439,842)	529,473 (427,635)	<0.001
Personal income	108,573 (170,556)	111,668 (168,079)	<0.001

Notes:

[1] Mean (SD); n/N (%)

[2] Wilcoxon rank sum test; Pearson's Chi-squared test

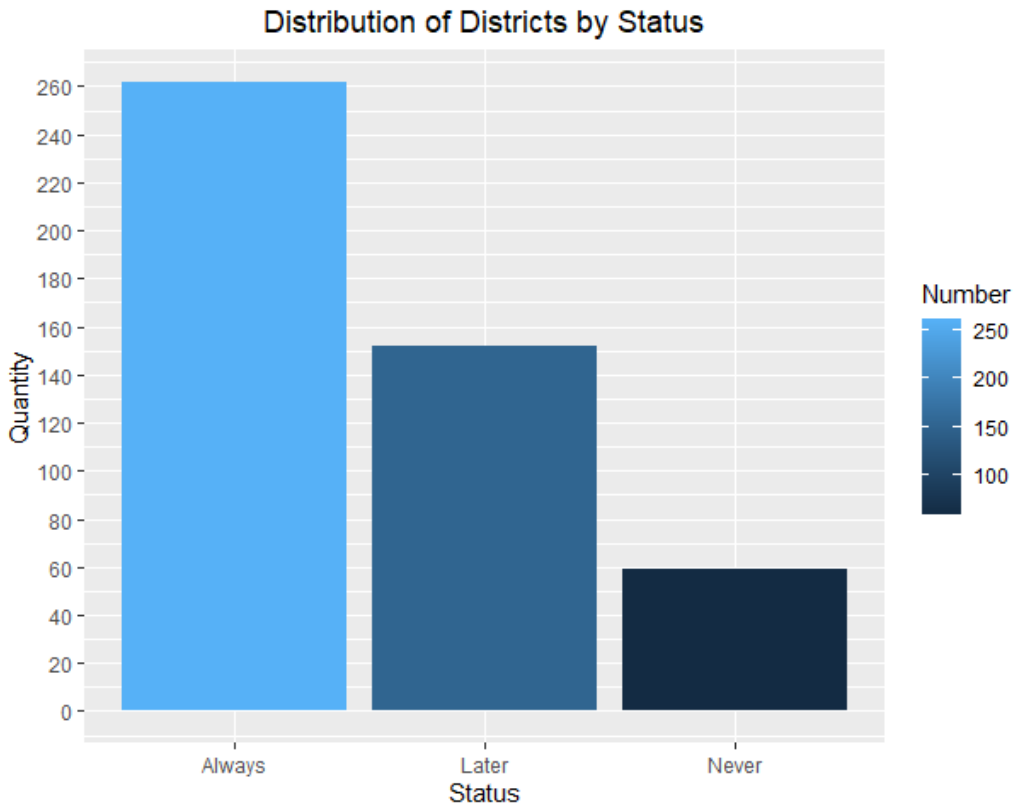

Figure 1.2: Districts and their Status.

compared to $211 in the treatment.

1.4 Empirical strategy

The 1994 Costa Rican healthcare reform has been thoroughly studied from a public health perspective, as exemplified by the seminal work by Rosero-Bixby (2004) [67] [68]. However, approaching it from an economic point of view would provide a more unique outlook, establishing the causal link, if any, that the government healthcare reform had on variables of interest in the Costa Rican economy. In particular, I wish to uncover the treatment effect of having increased access to primary healthcare on household and personal income. This comes to add to the literature on the household effects of building infrastructure, like Duflo's paper of schools in Indonesia (2001) [28], and on non-health effects of health reforms, like Miguel and Kremer's 'Worms at work' paper (2016) [52].

The rationale behind this pursuit is that having increased access to primary healthcare would bring about better health outcomes, which in turn translates into higher-quality workers. That is, healthier individuals would make for workers that have gathered enough human capital to access higher-paying jobs. This would provide strong empirical evidence for the theoretical results first mentioned by Grossman [38] and later by Strauss and Thomas [71]. Given universal health insurance, therefore, it is the *availability* of healthcare, and not its *affordability*, that is the limiting factor in receiving care.

I pursue a difference-in-difference approach, which will constitute the central part of the empirical analysis. I estimate the effect on both personal and household income Y_{idt} for household i in district d at year t of $Clinic_{dt}$, an indicator for a district d having a clinic as a result of the reform at time t that did not have any source of primary healthcare beforehand, as well a matrix of controls X, year fixed effects λ_t, and district fixed effects γ_d. It should also be noted that I am only studying districts that did not already have a clinic before 1995, as the placement of those previously-established clinics is endogenous. That way I can

better assess the difference in outcomes as a result of the healthcare reform, despite its quasi-experimental nature, and analyze the pre-treatment data to use within-district variation.

The equation to estimate is

$$Y_{idt} = \alpha Clinic_{dt} + X'_{it}\beta + \lambda_t + \gamma_d + \epsilon_{idt} \tag{1.1}$$

where the coefficient of interest is α, the treatment effect on the treated. Also, for all regressions, heteroskedasticity-robust standard errors are clustered at the district level [11].

I should acknowledge two potential limitations of this approach: First, that the decision of where to open the *EBAIS* clinics was not random, as the government gave priority to lower-income areas, so making a case for identification will be an important step in the process. And second, that there is a growing literature about potential pitfalls in the use of difference-in-difference, as exemplified by the Duflo, Bertrand, and Mulhanaithan (2004) paper [11], which warns about the potential inconsistency of the resulting standard errors. To solve this, as they suggest, I use White standard errors, which greatly reduce the problems caused by serial correlation. Moreover, if using more than two periods'-worth of data, a more recent paper by Goodman-Bacon (2021) [35] calls into question the interpretation of the coefficient of interest as an average treatment effect on the treated, suggesting instead that it is a weighed average of all two-period difference-in-difference coefficients. I will expand on this decomposition in the Robustness section.

Finally, I also analyze the long-term effects, if any, that the healthcare reform had on income. This is critical because it would provide further evidence in favor of the benefits of government interventions in the area of healthcare. For that purpose, I will use an event-study specification, given all the years'-worth of data available (8 before and 14 after the passing of the reform). My goal will be to measure how having a clinic in a treated district affected household and personal income after a varying number of years.

The equation to estimate is

$$\begin{aligned} Y_{idt} =& \alpha Clinic_{dt} + X'_{it}\beta + \lambda_t + \gamma_d \\ &+ \sum_{t=-21}^{12} \eta_t \cdot YearsElapsed_{dt} + \epsilon_{idt} \end{aligned}$$

where $YearsElapsed_{dt}$ measures the number of years since a treated district was treated, normalizing comparison group values to -30.

1.4.1 Identification strategy

To justify the use of difference-in-difference and an event study [2], I check for parallel trends in the income variables. A first intuition, providing a necessary but not sufficient condition for identification, is to look graphically at the trends in income before treatment. I would want for the slopes to be fairly similar because, if they were not, then income definitely would not follow parallel trends and I could not use difference-in-difference. Figures 1.3 and 1.4 show, using a non-parametric method for household income, as well as linear regression for personal income, that the trends for the treatment and comparison groups do appear to be parallel.

The graphs provide only suggestive evidence of parallel trends. Given that treatment was staggered and there is no single pre- and post-intervention period for the treatment group, I also follow the methodology suggested by Autor (2003) [6], cited in Pischke (2005) [59], as a more formal test of the identification assumption. In it, he introduces to the regression yearly dummies that are interacted with the treatment variable. The desired result is that the only coefficients to be statistically significant should be those post-intervention.

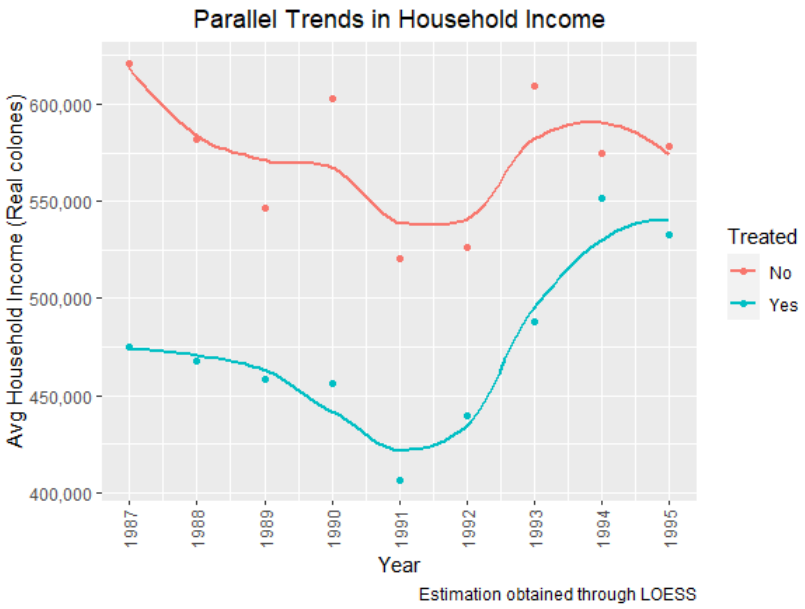

Figure 1.3: Pre-intervention Trends in Household Income

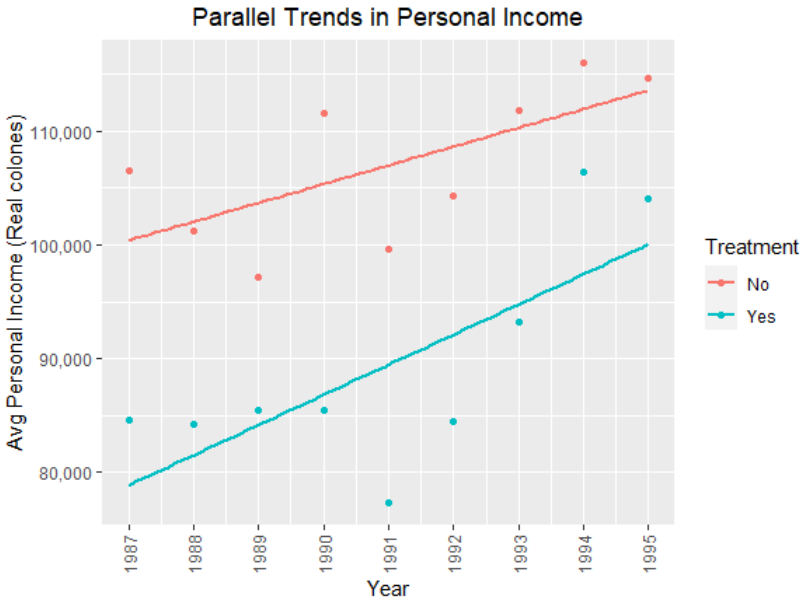

Figure 1.4: Pre-intervention Trends in Personal Income

The model becomes

$$Y_{idt} = X'_{it}\beta + \lambda_t + \gamma_d + \sum_{t=1987}^{2008} \beta_t Clinic_{dt} \cdot Year_t + \epsilon_{idt} \tag{1.2}$$

where $Year_t$ is a dummy variable corresponding to what year it is.

The test of the difference-in-difference assumption would require that all β_t be indistinguishable from 0 for the years prior to treatment and that the $Clinic \cdot Year$ variables be jointly insignificant pre-intervention. Only the coefficients post-intervention should be positive and statistically significant.

Figure 1.5 shows the results of this test graphically for personal income in levels, with the point estimates along with bars with their standard errors. Since the reform was first enacted in 1994, my expectation was only for coefficients after that to be statistically significant, and that is the case for seven out of the fourteen years post-intervention. What was somewhat surprising was that three of the seven pre-intervention years yielded significant estimates, while the remainder were statistically indistinguishable from 0, as desired. However, by testing the hypothesis that the pre-intervention coefficients are jointly insignificant, I fail reject the null at 5% significance, providing further evidence of parallel trends, thus justifying the use of difference-in-difference and an event-study specification.

Similarly, Figure 1.6 shows comparable but slightly weaker results in percentages. In this case, only three out of fourteen estimates post-intervention are statistically significant. And one more time, contrary to the expectation for the test, there were some significant estimates pre-intervention, in 1987 and 1989. However, again an F-test fails to reject the null hypothesis of joint insignificance of all pre-intervention coefficients at the 10% level. This provides even more evidence in favor of the existence of parallel trends.

For a discussion of newer developments on parallel trends, see the Robustness checks section.

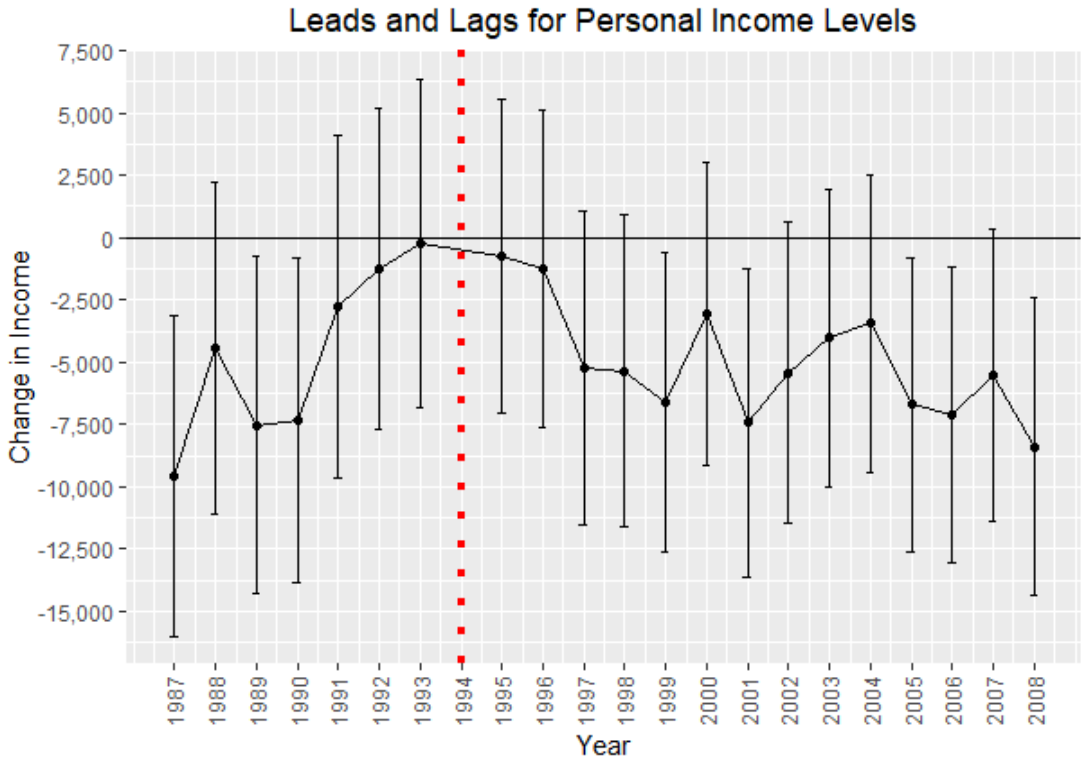

Figure 1.5: Autor parallel trends test.

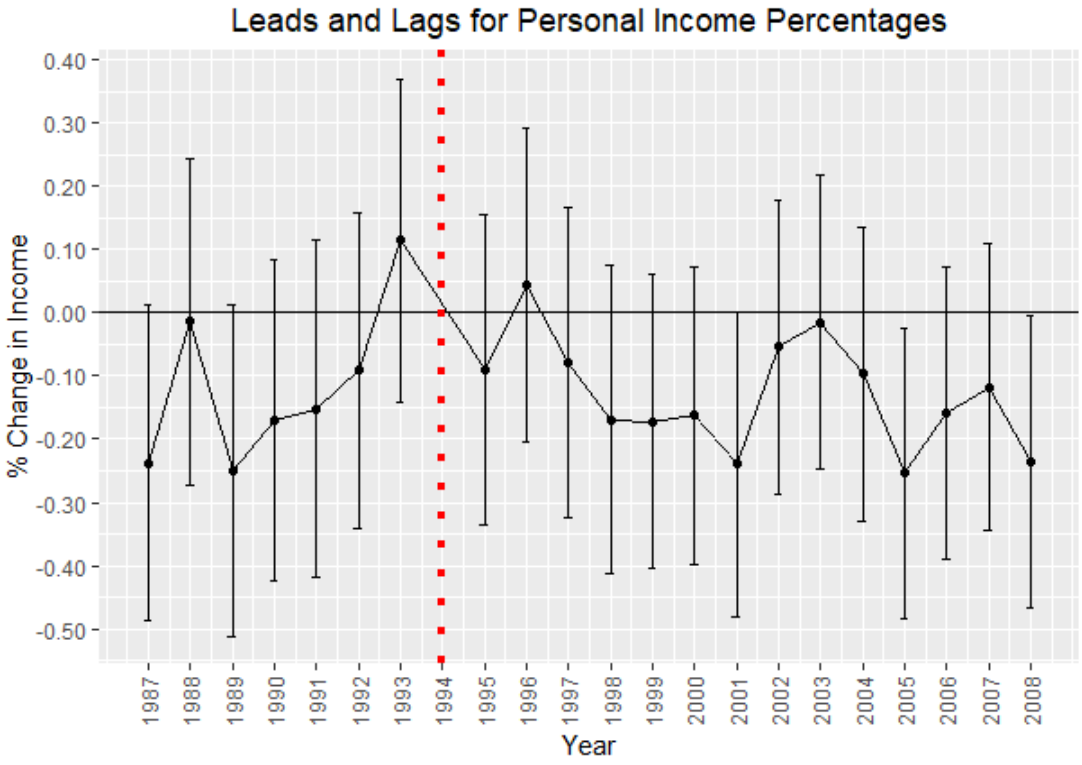

Figure 1.6: Autor parallel trends test in percentages.

1.5 Mechanism

In order to justify the proposed positive causal relationship between an increase in access to healthcare and increases in income as a result of the 1994 Costa Rican healthcare reform, I will rely on Strauss and Thomas' formulation [71] of Michael Grossman's seminal 1972 model [38] on the link between health and wages.

Here is the intuition behind the results of the model: Increased access to primary healthcare would bring about better health outcomes. This translates into higher-quality/healthier workers. Healthier individuals would make for workers that have gathered enough human capital to access higher-paying jobs. Given universal health insurance, therefore, it is the *availability* of healthcare [50] and not the *ability* to pay for it, that is the limiting factor in receiving care.

(For a full account of the theory, see Appendix A.)

Therefore, under this theoretical framework, I would expect for incomes to go up as a result of the healthcare reform.

1.6 Results

The difference-in-difference estimation yielded a positive and highly statistically significant change in income as a result of the healthcare reform. Table 1.2 shows the effect of getting an *EBAIS* on treated districts. Adding district and time fixed effects, as well as controlling for age, education, insurance type, and marital status, among others, column (2) indicates that getting an *EBAIS* led to an increase of around 17,000 real *colones* (about \$33 of 2015 [1]), or about 4% (as seen on column (4)), a positive and highly statistically significant increase in average household income as a result of the reform, compared to the average monthly income of about $540,000$ *colones*, or around \$1,022. In addition, columns (1) and (3) show

[1] Recall all income figures are normalized to colones of June of 2015, and US dollar figures come from using the average exchange rate for that time of 528 colones per dollar.

that, without district fixed effects, the estimate is negative but insignificant, a loss of about 76,000 *colones* (around $145), or 14%, in monthly household income. This reinforces the importance of the district fixed effects, since the reform happened at the district level.

Table 1.2: Difference-in-difference (DID) on household income in levels and percentages with fixed effects

Variable	Mean	Std Dev		
Household Income	539,607.02	433,951.91		
	Household Income		log(Household Income)	
	(1)	(2)	(3)	(4)
Clinic=1	$-76,279.10$	$17,607.43^{***}$	-0.14	0.04^{***}
	(1,775.32)	(2,573.48)	(0.003)	(0.005)
District FE	No	Yes	No	Yes
N	311,553	311,553	311,553	311,553
Adjusted R^2	0.28	0.39	0.30	0.42

Notes: ***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Next, Table 1.3 displays the changes in personal income levels as a result of the healthcare reform. The two specifications on columns (1) and (2), without and with district fixed effects, respectively, report small but positive changes in personal income from getting an *EBAIS*. Nevertheless, both changes prove to be statistically insignificant. Without district

fixed effects, the effect of the reform was an increase of around 1,050 real *colones*, or about $2. Meanwhile, by adding district fixed effects, the result is slightly smaller, about 610 *colones*, or around $1.20. Similarly, columns (3) and (4) report percent changes in personal income as a result of the reform, using the inverse hyperbolic sine transformation [10], as some individuals report zero income (so the logarithm would not be defined). It suggests a statistically insignificant decrease of 0.4% in personal income from the healthcare reform using time fixed effects. Without them, the resulting percent change is also a positive but insignificant 4%.

Table 1.3: DID on personal income in levels and percentages with fixed effects

Variable	Mean	Std Dev		
Personal Income	110,106.69	169,339.93		
	Personal Income		ihs(Personal Income)	
	(1)	(2)	(3)	(4)
Clinic=1	1051.92	610.63	0.04	−0.004
	(1061.71)	(1040.63)	(0.03)	(0.03)
District FE	No	Yes	No	Yes
N	311,553	311,553	311,553	311,553
Adjusted R^2	0.61	0.61	0.62	0.62

Notes: ***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

These results are puzzling, as they suggest statistically significant increases in income on families but not on individuals (even though the changes on individuals are mostly of the expected sign). This would point in one of two directions: first, that there might no certainty that getting an *EBAIS* had a positive effect on individuals; or second, more interestingly, that there might be something special about families in Costa Rica that make them able to achieve difficult or at least impractical feats for individuals. Given the collaborative nature of families in Costa Rica, I presume that latter might be at play, although more research needs to be done to conclude that definitely.

Finally, for the results of the event-study specification, Figure 1.7 visually reports the estimates for the effects on households of having been treated with a clinic in their district over several years, both before and after. As expected, the bulk of the pre-intervention coefficients, small and positive as well as negative, are insignificant. Although a couple of those coefficients are indeed significant, which would be indicative of selection, an F-test fails to reject that all the pre-intervention coefficients are jointly insignificant. In addition, as a result of getting an *EBAIS* clinic in their district, I find positive and highly statistically significant increases in the household income ranging from $19,000$ to $54,000$ *colones*, or between \$36 and \$102 of 2015, up to 10 years post-intervention. This corresponds to an increase of between 3% and 7% with respect to the year of treatment. Table B.2 in the appendix gives more details.

For personal income, Figure 1.8 shows statistically significant percent increases up to ten years post-intervention averaging around 19%. There are four instances of estimates pre-intervention being statistically significant, but one more time an F-test fails to reject the null that the pre-intervention coefficients are jointly insignificant. Similarly, looking at results in levels, I find increases ranging from $6,500$ to $12,000$ real *colones*, or \$12 to \$23 of 2015, and an F-test also fails to reject the null. That provides evidence for the strength of my event study results on personal income.

Figure 1.7: Household Event Study Results

Figure 1.8: Personal Income Event Study Results

Table B.3 in the appendix gives more details.

1.7 Robustness checks

As I explain in the Empirical Strategy section, the identification assumption of difference-in-difference, parallel trends in income, does seem to hold. First, I find suggestive evidence by graphing the trends for the treatment and control groups in Figures 1.3 and 1.4. Then, I perform a stronger test by adding time dummies and interacting them with the treatment variable. Despite the existence of individual pre-trend significant coefficients, as seen in Figures 1.5 and 1.6, as well as Table B.1, possibly explained by the quasi-experimental nature of the reform, an F-test for joint insignificance fails to reject the null, providing further evidence for parallel trends.

Another aspect to address stems from a recent paper by Kahn-Lang and Lang (2020) [44], which suggests that the plausibility of parallel trends is greater if the treatment and control groups start at similar *levels*, and not just follow similar trends. Given the quasi-experimental nature of the reform, my treatment and control groups do start at different levels, which might weaken the argument for parallel trends. To remedy this, as discussed in Griffith an Noonen (2021) [37], the authors suggest "assessing whether the treatment effect 'survives' extrapolation of a non-parallel trend into the treatment period" [37]. The assumed counterfactual would be that the treated districts would follow the same trajectory post-treatment as they did pre-treatment, as opposed to comparing them with untreated ones. Therefore, while this is not a perfect test of the parallel trends assumption, "it does provide insight into the assumption's plausibility" [37].

To perform this sensitivity test, I estimate the model

$$\begin{aligned} Y_{idt} =& \alpha_1 Year_t + \alpha_2 Year_t \cdot Clinic_{dt} \\ &+ \alpha_3 Clinic_{dt} \cdot Post_t + X'_{it}\beta + \gamma_d + \epsilon_{idt} \end{aligned}$$

where $Year$ is an indicator for each year, $Clinic$ is the treatment variable, and $Post$ is an indicator equal to 1 after the reform was passed.

The sensitivity test results, shown in Table 1.4, find a statistically significant increase of about 22,800 real colones, or about $43, in household income as a result of getting an *EBAIS* through the reform. This is qualitatively similar to the results in the regular difference-in-difference estimation, shown in Table 1.2, and in fact this analysis shows a slightly higher effect. This, together with the evidence found earlier, gives more credence to the identification assumption.

Table 1.4: Kahn-Lang & Lang sensitivity test

	Household income
Clinic · Post	22,802.19**
	(7,073.39)
N	311,553
Adjusted R^2	0.41

Notes: ***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Finally, as mentioned in the Empirical Strategy section, a paper by Goodman-Bacon (2021) [35] calls into question the interpretation of the difference-in-difference coefficient as an average treatment effect on the treated when there are more than two periods and the treatment turns on at different times. Instead, he proves the coefficient is a weighed average

of every two-way fixed effect estimators. Consequently, it becomes important to know which period comparisons are the ones driving the regression results. To that end, using the *bacon* package in R, I perform the Bacon-Goodman decomposition of the difference-in-difference results, and Figure 1.9 shows the outcome.

The graph splits comparisons into three groups: Earlier vs Later Treated, Later vs Earlier Treated, and Treated vs Untreated, represented in the figure with circles, triangles, and squares, respectively. It is worth noting that Later vs Earlier treated get a weight of 19% and an average estimated effect of $29,301.96$ *colones*, while the Treated vs Untreated, with a weight of 56%, shows an estimate of $30,993.58$ *colones*. This means that around 75% of the difference-in-difference effect is driven by comparisons in those two groups, which fits the regression results found before of around $17,000$ *colones* and in fact show they might be biased downward.

1.8 Conclusions

Costa Rica underwent a major healthcare reform starting in the 1990s that greatly expanded access to primary healthcare through its new *EBAIS* clinics. As a result the reform, now three-quarters of all medical consultations happen at an *EBAIS*. The stated goal of the reform was improving access and quality of care, which would result in better health indicators. Rosero-Bixby [67] showed just that: child mortality went down by 8%, deaths by transmissible illnesses decreased by 14%, and deaths by chronic diseases shrank by 2%, among others.

This paper discussed the possibility that this reform would also have the unintended beneficial effect of increasing household or personal incomes. My contention, stemming from Grossman's model of healthcare demand [38], was that better access to healthcare would result in healthier people who could get better-paying jobs, thus increasing their income. (This is dependent on better access necessarily implying more usage of healthcare, as in the

Figure 1.9: Goodman-Bacon

case in Costa Rica, which has near-universal health insurance.) My analysis found that, as result of getting a clinic in their district stemming from the reform that created the *EBAIS*, household income in Costa Rica went up around 4%.

The success that the Costa Rican reform had opens up the possibility that this healthcare model could be exported to other nations. This analysis gives statistical evidence of the positive effects that increased access to healthcare given universal health insurance could have on income, thereby increasing societal welfare.

Chapter 2

HOUSEHOLD INCOME AS A MEDIATOR OF THE EFFECT OF HEALTHCARE ACCESS ON EDUCATION: EVIDENCE FROM A LARGE-SCALE HEALTHCARE REFORM IN COSTA RICA

2.1 Introduction

There is an ample literature studying the relationship between education and health. Glewwe (1999) [34] finds, on mothers' schooling affecting children's health in Morocco, that mothers' years of education have a positive and statistically significant effect of children's health (0.2 z-score points). Mothers' health knowledge shows to be the strongest predictor and main pathway through which mothers' education affects children's health (1.3-2 z-score points). Moreover, he concludes that mothers' education contributes only indirectly, insofar as it teaches them the literacy and numeracy skills to acquire the health knowledge. Finally, as a policy proposal, his study suggests that health knowledge should be taught early on, before girls drop out of school, as those girls who drop out might not learn enough of the skills necessary to ever acquire that health knowledge.

Next, Bleakley (2010) [13] discusses how economists have approached the question of how disease depresses "development in human capital"; in other words, a link between health and both education and income. He mentions a study that measured labor-market returns to adult height, a proxy for health in early childhood: A paper by Ribero and Nuñez (2000) [65] finds, using microdata from Colombia, a 7-8% positive effect on wages per centimeter of height, using the availability of health resources as a child as an instrument for adult height. It was this paper that got me interested in how health can impact human capital accumulation, in this case reflected in an increase in wages.

My study pertains to the effects on education of a reform that increased access to primary healthcare through the construction of health infrastructure. I explore whether a government expansion of healthcare access led to detectable increases in people's human capital, specifically years of education. My analysis in this chapter, using data from Costa Rica, points to answers in the affirmative: having increased access to primary healthcare as part of the major reform in the mid-1990s resulted in people completing on average 0.07 to 0.11 extra years of education. This comes to enhance the literature of human capital development and its relationship to health, given a government intervention. Specifically, it bridges a gap between how changes in *health* infrastructure and access to healthcare affect education outcomes.

There are notable examples of how changes in *educational* infrastructure affect education levels, like Duflo's 2001 and 2003 papers [28] [29] on the 1970s school construction project in Indonesia. Between 1974 and 1978, the government built more than $60,000$ primary schools. The author estimates that every primary school built per $1,000$ students led to an increase of 0.12 to 0.19 years of education, along with a 1.5 to 2.7% increase in wages.

Similarly, Miguel and Kremer (2004) [52] explore how a healthcare reform can have non-health-related effects, studying the effect of a school-based de-worming program in Kenya. Their goal was to measure not only the direct impact of the program on treated students but also its indirect externality effects on untreated ones. They report an increase in student participation[1], reducing absenteeism by at least a quarter among the treated students. They estimate that school participation increased by at least 0.14 years per treated student, and that the increase in participation among untreated students was so large that the authors argue it would justify fully subsidizing the program.

[1]Participation is defined as the proportion of registered students that are present on the day of data collection.

2.2 Background

2.2.1 History and Challenges of the Education Sector in Costa Rica

Costa Rica has a long history of supporting and mandating free public education. Starting in 1869, grades 1-6 were first declared free and compulsory [25], followed by grades 7-9 in 1973 [26], preschool in 1997, and grades 10-12 in 2011 [33]. Consequently, as of 2011, per an amendment to article 78 of the Constitution, preschool through 12th grade are "free, mandatory, and paid for by the Nation" [5]. Moreover, as of 2018, the same article compels the State to spend at least 8% of gross domestic product on education, making it one of the highest in the world [8].

After a notable decrease in investments in education in the 1980s, expenditures recovered starting in the 1990s, though still not at the levels they had in the 1970s. In real terms, investment in education increased by 43% between 1994 and 2004, with most of it going toward grades 1-9, special, and distance education [26]. Figure 2.1 shows this change. As a result, in 2000 the Raw Rate of Schooling[2] recovered to the level it had before the economic crisis in the 1980s, at about 61%. Meanwhile, however, funding for public higher education diminished, as did school lunch programs, student transportation support, and scholarships [26]. It is therefore interesting that, while expenses in education increased, overall student success rates have not grown at the same rate, and in fact have decreased in elementary school. This indicates that even with massive increases in investment, the Costa Rican education system is not without its challenges, for both teachers and students, as discussed below.

For teachers, while about 82% of them work for the State, not surprising in a system

[2]The Raw Rate of Schooling is defined as the ratio of the students matriculated on a given year to all children who should be in school as determined by their age. Because some students might be repeating a course, some matriculated students might be outside the required age range, which would make a Raw rate of schooling greater than 100% possible. In high schools, only 20% of students graduate without having to repeat at least one grade [26]

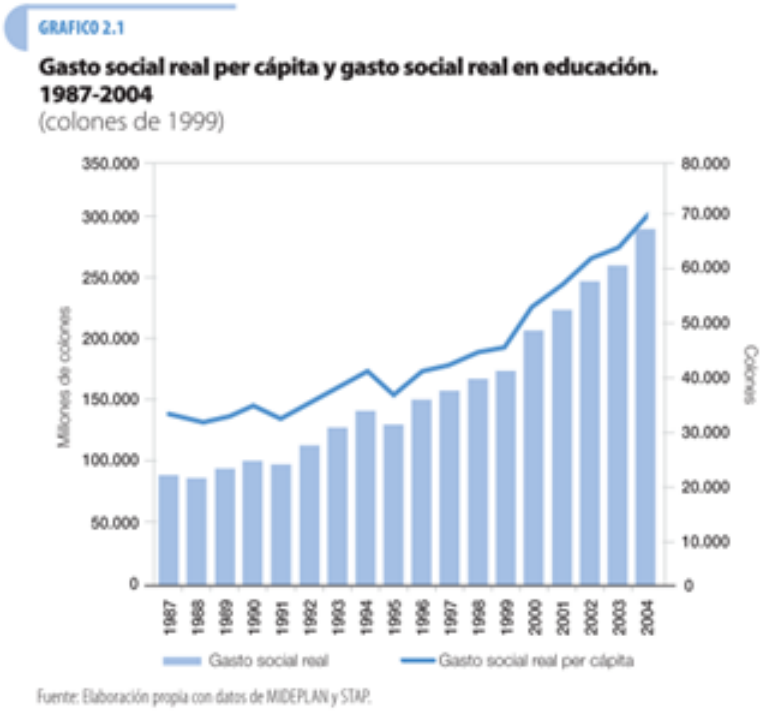

Figure 2.1: Total and per capita real social expenditure in education (1987-2004). Taken from State of the Education Report 1

where most education is public, in 2001 –part of the period I am analyzing (1987-2008)– 43% of them did not have tenure, and they had a salary below average for people with the same education level. Additionally, class size, particularly in high schools is a problem: with about 32 students per class, there are many more than recommended for adequate individualized care [26], which in turn affects student performance.

For students, while education remains a vital source of social mobility, there continue to be obstacles for economically disadvantaged students. For ages 13-17, while 95% in the first quintile attend school, in the fifth one only 72% do. Moreover, there are great disparities depending on the type of school. For instance, in 2004 only 56% of students in public schools passed the national exit examinations (*"bachillerato"*), while 84% of those in private schools did. Similarly, while 83% of teenagers in urban areas attend school, only 68% of those in rural areas do [26].

Finally, despite education being mandatory, students dropping out of school remains a challenge for the educational authorities. Between 1990 and 2004, about 4% of primary students left school each year, whereas in secondary school about 12% did, with both percentages trending downward [26], as shown in Figure 2.2. An interview with high school principal Luz Mery Monge [53] confirmed that, while students are constitutionally mandated to attend, the authorities' resources are limited: they might send social workers escorted by police, or families under social programs could lose their benefits, but ultimately the manpower necessary to enforce compulsory education is just not enough.

2.2.2 Costa Rican Healthcare Reform of 1994

Starting with the signing of Executive Order Number 30698-S in 1994 during the Figures Olsen administration, Costa Rica would radically change its approach to healthcare. The reform began with a subdivision of the country into around 100 health areas, each serving up to $100,000$ people, and created new primary healthcare clinics called *EBAIS* (Spanish for

Figure 2.2: High school (secundaria) and elementary (primaria) school annual drop-out rates (1990-2004). Taken from State of the Education Report 1

"Basic Integral Healthcare Teams") led by a physician, a nurse, a pharmacist, a community healthcare worker, and a data collection clerk. These clinics would provide the first (and usually main) point of contact with the healthcare system, giving communities a more local source of preventative and curative primary care, taking a large load off general hospitals and bigger clinics.

The two main practical results of the reform have been a dramatic increase in access to primary healthcare, as well as a shift toward most care being provided at *EBAIS*. According to Pesec et al (2017) [57], defining "access" as having a source of primary healthcare within 4km, access increased from 30% to 93% of the population in the first twelve years of the reform. Additionally, "in 2014, EBAIS teams provided three-quarters of all medical consultations in Costa Rica and cared for approximately 80 percent of Costa Ricans' health needs" [57]. Therefore, the Costa Rican healthcare reform has already been hugely consequential.

A few studies have looked into the impact of this reform. In his seminal papers [67] [68], Rosero-Bixby studies the public health repercussions of the reform. He finds an 8% reduction in child mortality, a 14% decrease in deaths by transmissible illness, and 2% drop in deaths by chronic diseases. He also estimates the reform saved 120 children and 350 adults in 2001 alone. In addition, Chapter 1 [36] suggests a causal link between the increased access to healthcare and household income, with income increasing by 4% as a result of getting an *EBAIS* in your district. (For a more detailed description of the Costa Rican healthcare reform and the *EBAIS* program, see Chapter 1 [36].) The goal of this current study is to explore the impact that the reform had in education decisions, specifically years of schooling, through the lens of difference-in-difference and statistical mediation.

2.3 Data

My outcome and demographic data come from the Multiple Purpose Household Survey (EHPM, in Spanish), an annual and nationally-representative survey of Costa Rican households given by the Costa Rican Statistics and Censuses Institute (*INEC*) between 1987 and 2008. To ensure comparability across time, I use the Consumer Price Index (CPI), taken from the Costa Rican Central Bank (*BCCR*), in order to normalize incomes into constant July 2015 *colones*, the local currency. Moreover, I winsorize the income data to minimize the effects of outliers. Finally, the clinic data was provided by the Social Security Administration's Annual Memories (*Memorias Institucionales de la CCSS*) as well as by Rosero-Bixby himself. For a more detailed description of the data, see Chapter 1 [36].

The location of clinics prior to the healthcare reform is endogenous, so I limit my analysis to households in districts that did not already have a clinic or hospital prior to the reform. Table 2.1 shows summary statistics for the $313,977$ remaining observations of the total $892,317$: $157,176$ individuals who lived in households whose district either never got an *EBAIS* clinic or before it gets one, along with $154,377$ who lived in districts that got an *EBAIS* as a result of the reform. In terms of districts, that corresponds to 262 who "always" had a clinic, 59 that "never" did, and 152 that got one "later", as seen in Figure 2.3.

Both comparison and treatment households have around 5 members, with an average age of 35 years, individual and total monthly incomes of \$205 and $\$1,040$ for the comparison group, and \$211 and $\$1,000$ for the treatment group, respectively. (These values were calculated using the average exchange rate of 528 Costa Rican *colones* per US dollar in July 2015.) Moreover, within each group, both groups are fairly evenly split by sex, and households in the treatment group have about 0.4 more years of education, with 5.8, compared to 5.4 in the comparison.

Table 2.1: Summary statistics.

Variable	**Control**[1], N=157,176	**Treatment**[1], N=154,1377	**p-value**[2]
# of people	5.09 (2.21)	4.80 (2.06)	<0.001
Sex			<0.001
Male	75,160/157,176 (48%)	67,147/154,377 (43%)	
Female	75,791/157,176 (48%)	66,566/154,377 (43%)	
Unknown	6,225/157,176 (4%)	20,664/154,377 (13%)	
Age	30 (24)	40 (29)	<0.001
Years of education	5.4 (4.3)	5.8 (4.0)	<0.001
Household income	549,560 (439,842)	529,473 (427,635)	<0.001
Personal income	108,573 (170,556)	111,668 (168,079)	<0.001

Notes:

[1] Mean (SD); n/N (%)

[2] Wilcoxon rank sum test; Pearson's Chi-squared test

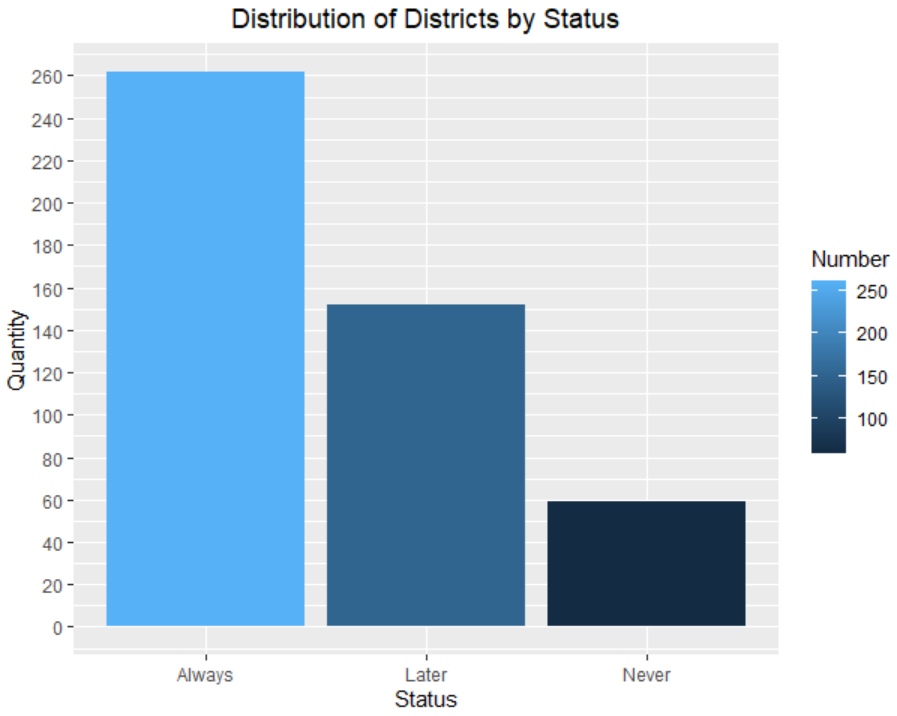

Figure 2.3: Districts and their Status. Calculated with data from CCSS

2.4 Empirical Strategy

Taking into consideration that the Costa Rican healthcare reform was not random but targeted, I begin my analysis by estimating the following difference-in-difference model:

$$Education_{idt} = \alpha Clinic_{dt} + X'_{it}\beta + \lambda_t + \gamma_d + \epsilon_{idt} \tag{2.1}$$

where $Education_{idt}$ measures the years of formal schooling of individual i in district d at time t and $Clinic_{dt}$, the treatment variable, is an indicator equal to 1 if there is an *EBAIS* clinic in a district d at year $t > 1994$ that did not have one beforehand. Moreover, I add a matrix X of controls and make use of time and district fixed effects, λ_t and γ_d, respectively. Finally, heteroskedasticity-robust standard errors are clustered at the district level [11].

A novel technique I also use in this paper is that of *mediation*, a statistical tool disseminated by Baron and Kenny (1986) [9] and refined by Preacher and Hayes (2004) [60], as well as by Hayes (2009 and 2018) [39] [40]. Mediation is similar to instrumental variables, except that instead of assuming that the instrument is the only channel through which the independent variable affects the dependent one, it measures to what extent the mediator variable is responsible for explaining changes in the dependent variable. A slightly new tool to economics [19] [41], particularly agricultural economics [81], mediation is much more common in the epidemiology [66], psychology [56], public health [75] and political science [46] literatures.

The simplest mediation model [40] is

$$M = i_M + aX + e_M$$

$$Y = i_Y + c'X + bM + e_Y$$

where the i represent constants, the e errors, X is the independent variable, Y is the depen-

dent one, and M is the mediator. The mediator is assumed to be "causally located between X and Y" [40]. In other words, it is necessary to have reason to believe that X causes M, which in turn causes Y.

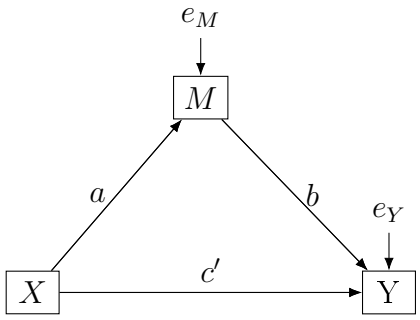

Figure 2.4: Simple mediation model (based on [40])

The coefficient c' is called the *direct effect* of X on Y, the product of a and b is the *indirect effect* of X on Y through M, and $c = c' + ab$ is the *total effect* of X on Y. It is this decomposition that permits the study of how much of an effect can be attributed to a specific mechanism.

Following the same notation for the coefficients, the model I will estimate is

$$Household\ \ Income_{idt} = a\, Clinic_{dt} + X'_{it}\beta + e_{idt} \tag{2.2}$$

$$Education_{idt} = c'\, Clinic_{dt} + b\, Household\ \ Income_{idt} + X'_{it}\gamma + \epsilon_{idt} \tag{2.3}$$

This corresponds to the model

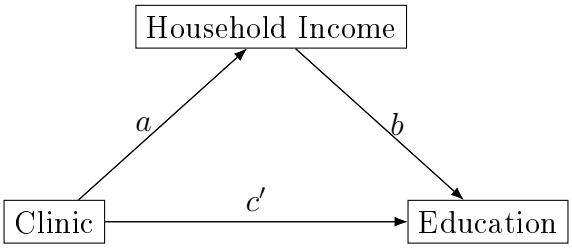

Figure 2.5: Proposed mediation model

Since *Clinic* designates individuals in treated households after they were treated, I am interested in the effect of the healthcare reform on education through the channel of household income. The intuition is that getting a clinic through the reform causes increases in household income (as argued in Chapter 1 [36]), which would in turn incentivize increases in individual education.

2.4.1 Identification strategy: difference-in-difference

The identifying assumption for the use of difference-in-difference is that of counterfactual parallel trends. Although there is no way of checking for them directly, looking at graphs of the dependent variable in question in the years prior to treatment for the treatment and comparison groups provides suggestive evidence. Figure 2.6 shows a consistent difference of about 0.75 years between the treatment and control groups that appears to persist throughout the entire pre-intervention period.

Moreover, to provide stronger evidence of parallel trends, I follow Pischke's notes (2005) [59], in which he explains Autor's 2003 [6] method of adding time dummies before and after treatment, and interacting them with the treatment variable. Among the coefficients of the

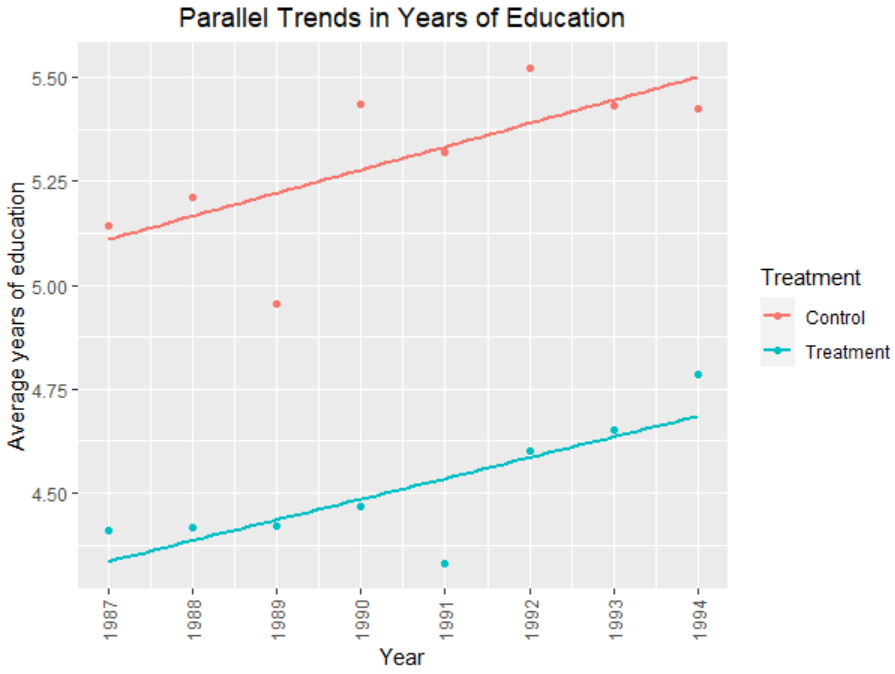

Figure 2.6: Pre-intervention Trends in Years of Education

interaction terms only those post-intervention should be statistically significant, or if not, at least the pre-intervention ones should be jointly insignificant.

The model to estimate is

$$Education_{idt} = X'_{it}\beta + \lambda_t + \gamma_d + \sum_{t=1987}^{2008} \beta_t Clinic_{dt} \cdot Year_t + \epsilon_{idt} \tag{2.4}$$

where $Year_t$ is a year dummy. I test whether all β_t equal 0 for the years prior to treatment or if at least the $Clinic \cdot Year$ variables pre-treatment are jointly insignificant.

Figure 2.7 (and Table B.4 in the Appendix) show that, contrary to what I expected, all but two of the pre-intervention coefficients are statistically significant. However, given the quasi-experimental nature of the healthcare reform, I also ran an F-test for joint insignificance. My null hypothesis was that all pre-intervention coefficients are jointly equal to zero. With a p-value of 0.09885, I fail to reject the null at a 10% significance level, providing further suggestive evidence of parallel trends. This points toward the existence of parallel trends in the levels of education between the treatment and comparison groups.

See the Robustness checks section for a deeper discussion of newer developments on parallel trends.

2.4.2 Identification Strategy: Mediation

As explained by Celli (2019) [19], in any mediation analysis there are two main identification assumptions:

1. Conditional independence of the treatment (X): $\mathbb{E}(e_M|X) = 0$

2. Conditional independence of the mediator (M): $\mathbb{E}(e_Y|M) = 0$

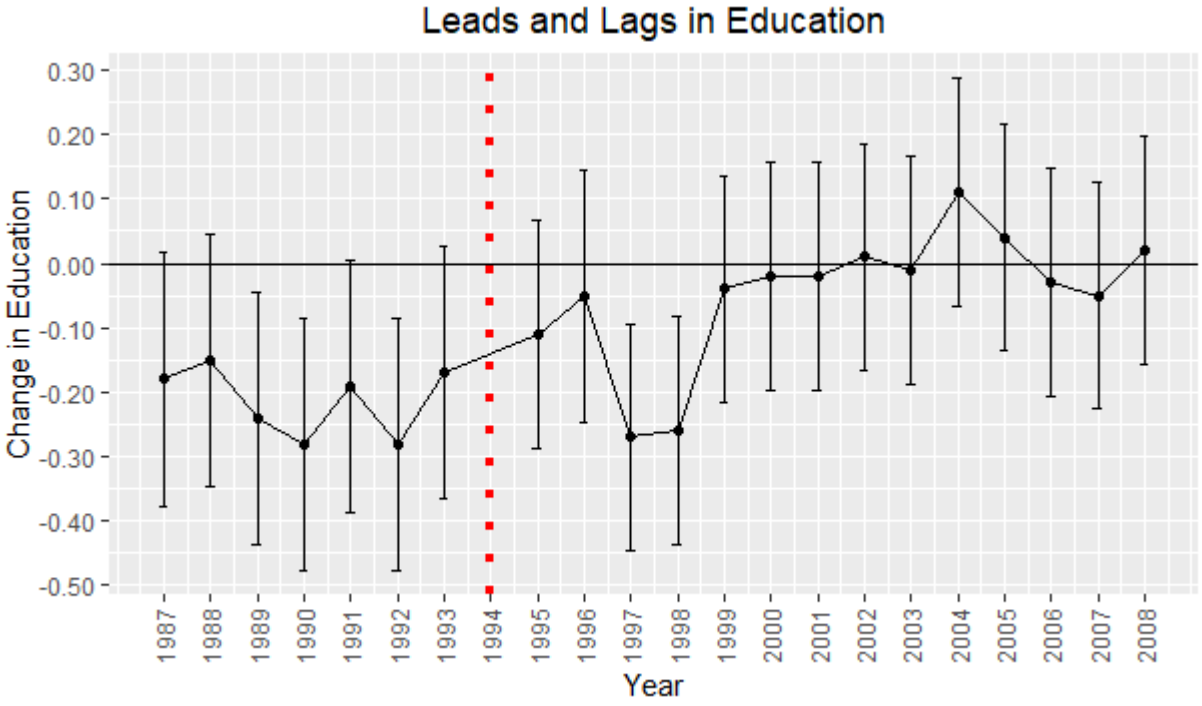

Figure 2.7: Pre-intervention Trends in Years of Education 2

In other words, there should be no confounders between the treatment and the outcome (X and Y), nor between the mediator and the outcome (M and Y) [19].

One more time, given the quasi-experimental nature of the healthcare reform, identification is a challenge. Indeed, Hayes (2018) says that "[a]bsent random assignment to values of X, *all* of the associations in a mediation model are susceptible to confounding and epiphenomenal association, not just the association between M and Y." [40] Nonetheless, Hayes (2018) [40] also points to a partial solution by adding covariates into the regressions that would otherwise be confounders. To that end, I add into the regression individuals' sex, age, age squared, marital and insurance status, individual income, district, number of overage men and women, and number of underage men and women.

Adding the covariates results in a model that can be represented as follows, with one such C_i per covariate:

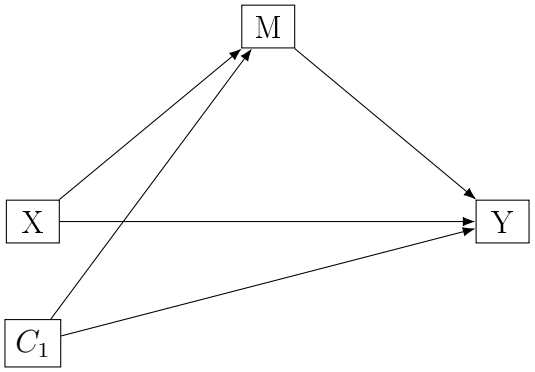

Figure 2.8: Mediation model with a covariate (based on [40])

2.5 Mechanism

Given

- X is increased access to primary healthcare as a result of the reform
- M is household income
- Y is years of education,

recall mediation requires for M to be "causally located between X and Y" [40]. The causal link from X to M has already been suggested in Chapter 1 [36]. This result rests on the theoretical foundation of the Strauss and Thomas' formulation [71] of Grossman's seminal 1972 model [38] of a health production function and the relationship between health and wages.

For the second link, between M and Y, I rely on the literature of gains in income generating increases in education, especially through improved parental quality. Arnold et al (2013) [20] share evidence of this using data from the UK Labor Force Survey. They find an increase of around 10 percentage points in the probability of finishing high school for every 1% increase in parental income. Other examples include Khanam and Nghiem (2016)[47], who use the Longitudinal Study of Australian Children; Elstad and Bakken (2015)[31], using register data from Norway; Duncat at al (1998)[30], with the Panel Study of Income Dynamics (PSID); Blau (1999)[12] as well as Burnett and Farkas (2009)[15], using the Children of the National Longitudinal Survey of Youth; and Morrissey et al (2014)[55], who use the Miami School Readiness Project.

One possible reason for concern, further addressed in the Robustness section, is the possible reverse causality from education to income, which would invalidate the use of mediation. After all, review articles such as Card (1999)[16] or Psacharopoulos and Patrinos (2018)[61] summarize scores of studies that point to a 9% increase in income per every additional year of schooling. Moreover, returns remain higher for women than for men, as well as for lower-income countries than for higher-income ones.

2.6 Results

Table 2.2 shows the results of the difference-in-difference estimation of the impact of the Costa Rican healthcare reform on years of education. I look at specifications using year and district fixed effects, both with and without personal income.

The most important result corresponds to the coefficient for *Clinic*. As indicated by both columns, getting an *EBAIS* as a consequence of the healthcare reform yielded an average increase of 0.07 to 0.11 years in the number of years of education, compared to individuals in households who never got a clinic. This represents a small but highly statistically significant $1-10\%$ increase for the treatment group over their average 5.8 years of education, as shown on Table 2.1.

This result is important because it comes to confirm and extend a combination of the results in Duflo (2001)[28] and Miguel and Kremer (2004)[52]: that investments in infrastructure and health interventions can positively affect education levels, respectively; in the Costa Rican case, the infrastructure investment was in healthcare, greatly expanding access to primary care. Consequently, this represents another benefit of the healthcare reform that could even inform policy in other places: in a country similar to Costa Rica, knowing that increasing access to primary healthcare can be expected to have positive effects even on education gives yet another incentive to enact such reforms.

Moving on to the mediation analysis, I use the PROCESS macro for R, written by Hayes [40]. It produces as its output three tables: one for each of the two mediation regressions, using bootstrapped heteroskedasticity-robust standard errors and bootstrapped 95% confidence intervals with 5000 replications; and a summary table with the direct, indirect, and total effects of the independent variable on the dependent one through the mediator. Standard errors are clustered at the district level.

Table 2.3 shows the results of the intermediate regression 2.2 of the mediator *Household Income* on the independent variable *Clinic*. I find a highly statistically significant increase

Table 2.2: Education difference-in-difference

Variable	Mean	Std Dev
Years of Education	5.6	4.2

	Years of Education	
	(1)	(2)
Clinic = 1	0.108***	0.067**
	(0.031)	(0.030)
Personal Income	Yes	Yes
Household Income	No	Yes
N	311,553	311,553
Adjusted R^2	0.544	0.554

Notes: Average age of respondents was 35, with an average education of 5.6 years. Errors were clustered at the district level. I include year and district fixed effects.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

of 14,676.37 real *colones* as a result of getting an *EBAIS* clinic in your district from the healthcare reform. Using the average exchange rate in June of 2015 of 528 *colones* per US dollar, this converts to about $28, comparable with the results obtained in the difference-in-difference estimation shown in Table 1.2 in Chapter 1 [36]. Moreover, PROCESS yields a 95% confidence interval of 11,442 to 17,999 for the effect of the reform on household income. Given 0 is not straddled in the confidence interval, there is a high level of certainty that the effect is indeed not null.

Table 2.3: Mediation results 1

	Household Income			
	Coefficient	BootMean	BootLLCI	BootULCI
Clinic = 1	14,676.37	14,655.95	11,442.34	17,999.35

Notes: First intermediate regression of the mediator *Household Income* on the independent variable *Clinic*, the treatment indicator. Errors are clustered at the district level. Bootstrapped standard errors are calculated at a 95% confidence level.

Next, Table 2.4 presents the results of the other intermediate regression, Eq. 2.3, of the dependent variable *Years of Education* on the independent variable *Clinic* and the mediator *Household Income*. It points to a highly significant increase of about 0.66 years of education as a result of getting an *EBAIS* clinic from the reform. This would correspond to the direct effect of the mediation. Moreover, there is an increase of 0.198 years of education per every additional 100,000 *colones* of household income. This converts to about 1 extra year of education per every extra $1,000 of monthly income.

Table 2.4: Mediation results 2

	Years of Education			
	Coeff	BootMean	BootLLCI	BootULCI
Clinic = 1	0.6573	0.6572	0.6273	0.6872
Household income	$1.9828 \cdot 10^{-6}$	$1.9958 \cdot 10^{-6}$	$2.5657 \cdot 10^{-6}$	$1.6389 \cdot 10^{-6}$

Notes: Second intermediate regression of the dependent variable *Years of Education on* the independent variable *Clinic* and the mediator *Household Income*. Bootstrapped standard errors are calculated at a 95% confidence level. Errors are clustered at the district level.

Finally, Table 2.5 summarizes the total, direct and indirect effects of the healthcare reform on education with household income as mediator. It shows a highly statistically significant total effect of about 0.69 extra years of education as a result of the healthcare reform. From that, about 0.66 years are a direct effect, meaning that only about 0.03 years in the total effect can be attributed to the influence of increases in income. This implies that only about 4.3% of the total increase in education happens through the channel of household income, the mediator, with the rest attributable to other mechanisms. Figure 2.9 illustrates these results.

Table 2.5: Mediation results summary

Total effect of X on Y:				
Effect	t	p	LLCI	ULCI
0.6864	44.4819	0.0000	0.6562	0.7167
Direct effect of X on Y:				
Effect	t	p	LLCI	ULCI
0.6573	43.3771	0.0000	0.6276	0.6870
Indirect effect(s) of X on Y:				
	Effect	BootLLCI	BootULCI	
Household income	0.0291	0.0227	0.0356	

Notes: Total, direct, and indirect effects of receiving a clinic as a result of the healthcare reform on *Years of Education* with *Household Income* as mediator. The mediation shows that the indirect effect through the mediator only accounts for 0.03 years of education, with other channels accounting for the rest.

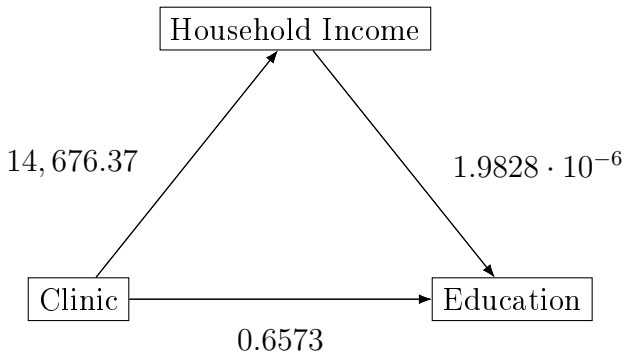

Figure 2.9: Illustration of mediation results

2.7 Robustness checks

2.7.1 Difference-in-difference

There have been recent developments calling into question whether testing for pre-trends is enough to justify counterfactual parallel trends post-treatment. To address this issue, Rambachan and Roth (2022) [64] developed a method to find how sensitive results are to a lack of parallel trends. Similarly, Kahn-Lang and Lang (2020) [44] contend that parallel trends become more plausible if the treatment and control groups are similar in levels, as opposed to only in trends. They propose a sensitivity test, which I now perform, that assesses if the treatment effect would survive the extrapolation of a differential trend into the treatment period.

The model to estimate is

$$\begin{aligned} Education_{idt} =& \alpha_1 Year_t + \alpha_2 Year_t \cdot Clinic_{dt} \\ &+ \alpha_3 Clinic_{dt} \cdot Post_t + X'_{it}\beta + \gamma_d + \epsilon_{idt} \end{aligned}$$

where $Year$ is an indicator for each year, $Clinic$ is the treatment variable, and $Post$ is an indicator equal to 1 after the reform was passed.

Table 2.6 shows the results of this test. I find an effect of 0.21 years of extra education after getting an *EBAIS* as a result of the reform. This is comparable to the result obtained before in the difference-in-difference estimation, as seen on Table 2.2. This, together with the more suggestive results found earlier, provides further evidence in favor of parallel trends, thus justifying the use of difference-in-difference.

Table 2.6: Kahn-Lang & Lang sensitivity test 2

	Years of Education
Clinic · Post	0.21**
	(0.06)
Observations	311,553
Adjusted R^2	0.50

Notes: Test of whether treatment survives extrapolation of a differential trend into treatment. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

2.7.2 Mediation

Mediation describes causal relationships. It must be the case that the mediator M is "causally located" between the independent variable X and the dependent variable Y. For this analysis I suggest household income as a mediator to the relationship between the healthcare reform and changes in education. Chapter 1 [36] suggests the causal link going from the reform to household income. In addition, part of my Mechanism section is dedicated to explaining the literature linking household income to education. Nevertheless, the main threat to the validity of my analysis is the potential existence of reverse causality due to returns to education. That would imply I could not conclude that the correlation found is indeed due to household income resulting in more education, making the use of mediation invalid.

To partially address that issue, I would now like to argue that the reverse causality is limited, as those prompted to get more education by the extra household income from the reform would not have entered the labor market yet. To show this, I extend the analysis but

focusing solely on the individuals whose education would have definitely *not* been affected by the healthcare reform. In particular, for every year, I only look at those 25 or older. My goal is to show they would see no year-to-year impact from the healthcare reform.

According to the literature on critical periods (see Bleakley (2010) [13] or van den Berg et al (2014) [74]), only those people young enough to have been exposed during their formative years should see any impact from a health intervention. In 2008, the last year of the study, any person 25 or older would have been at least 12 when the first *EBAIS* clinics were opened. Any year prior to that, individuals would have been even older. Consequently, I expect that the effect of the healthcare reform on education for those 25 or older should be close to zero, thereby suggesting that the effect found before was driven by those who were young enough to have been affected.

The model to be estimated is:

$$Education_{idt} = X'_{it}\beta + \lambda_t + \gamma_d + \sum_{t=-13}^{11} \beta_t Clinic_{dt} \cdot YearsTreated_t + \epsilon_{idt} \tag{2.5}$$

where X is a matrix of controls, λ_t and γ_d are time and district fixed effects, respectively, $Clinic$ is the treatment variable, and $YearsTreated$ indicates the years since treatment.

Figure 2.10 and Table B.5 show the results of this regression. As can be observed, with the exception of three occasions, for the years between 13 before and 11 after the reform was enacted, people 25 or older had an effect from the healthcare reform that is indistinguishable from zero. This points toward the effect of healthcare on education to indeed be driven by those young enough to be affected. Additionally, it serves as further suggestive evidence of a lack of reverse causality due to returns to education.

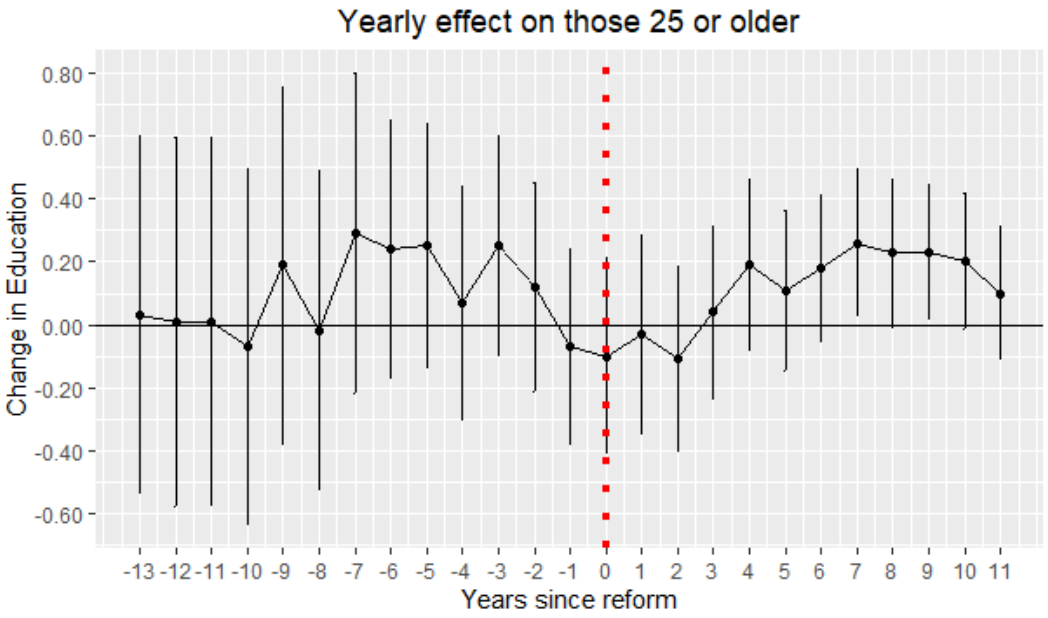

Figure 2.10: Effect on those too old to be affected

2.8 Conclusions

The Costa Rican healthcare reform greatly expanded access to primary care, going from 30% to 93% of the population in the first twelve years alone [57]. Chapter 1 [36] suggests a 4% causal increase in household income as a result of the reform. This chapter focuses on the effects of the reform on education choices through difference-in-difference and statistical mediation.

In my difference-in-difference analysis, I find an increase of 0.07 to 0.11 years of education as a result of getting an *EBAIS* clinic from the healthcare reform. This constitutes a $1-2\%$ increase in years of education. This comes to fill a gap in the literature that relates investments in health infrastructure to non-health outcomes. This paper establishes a link between investments in healthcare and effects in education, similar to how Duflo [28] finds changes in education stemming from educational investments, and Miguel and Kremer [52] find non-health-related effects to a healthcare intervention.

Finally, using the statistical tool of mediation, I find an increase of about 0.69 years of education from getting an *EBAIS* clinic in your district as a result of the healthcare reform. I suggest household income as a mediator of this relationship; that is, as a possible channel through which additional access to healthcare increased years of education. My analysis finds that about 0.03 years of education from the total effect are attributable to the influence of the mediator, household income, with the rest stemming from other sources.

Chapter 3

A NEW LOOK AT ROSERO-BIXBY'S RESULTS ON EBAIS CLINICS

3.1 Introduction

> "Mortality in the United States is 18% higher than in Costa Rica among adult men and 10% higher among middle-aged women, despite the several times higher income and health expenditures of the United States."
>
> –Rosero-Bixby and Dow, Proceedings of the National Academy of Sciences, 2016 [69]

What can a government do to improve the health of its population and decrease its mortality in the context of universal health insurance? According to the World Bank, Costa Rica, a country where it is the availability of healthcare [50], not its affordability, that determines treatment, has achieved promising health outcomes through higher-quality healthcare services [76] over the past two decades. Life expectancy has grown to the third highest in the continent, surpassed only by Canada and Chile [57]. Also, maternal, infant, and under-five mortality have steadily decreased [58]. And all of this has been achieved while spending less than the rest of the world, both as a percentage of GDP, 7.6% versus a world average of 10.0% in 2016; and per capita, $889 versus $1,026 [76].

It would be tempting to think that this improvement in health indicators could be explained by simple economic growth, but according to Pesec et al (2017), "studies have shown that 75 percent of Costa Rica's impressive infant mortality reduction between 1971 and 2000 can be attributed to improvements in its health services, not its economic growth." [58].

What stands at the heart of these health results is a major healthcare reform that Costa Rica underwent starting in the mid-1990s, which greatly increased access to primary healthcare: Defining "access" as having a source of outpatient care within 4km, population access increased from 30% to 93% in the first twelve years of the reform alone [57].

Studies of this healthcare reform have been conducted: Chapter 1 of this document [36] looks at the economic impacts, finding an average 4% increase in household income as a result of having increased access to healthcare. Meanwhile, Chapter 2 [36] studies the impact in educational choices, finding through statistical mediation an increase of 0.69 years of education, 4.3% of which is attributable to higher household income. However, in the public health sphere, it is two papers by Rosero-Bixby (2004) [67, 68] that have long been held as evidence that the program worked at reducing mortality: Among other results, he finds an 8% decrease in child mortality as well as a 2% decrease in overall mortality.

The purpose of this paper is to update those results, making use of newer methods and techniques, as well as nearly 10 years of extra data available. To that end, I run an analysis similar to his, exploring different specifications, complemented with an event study. I find a highly statistically significant 5.62% decrease in general mortality, as well as a 22.75% decrease in under-five mortality. Both of these estimates are higher than the corresponding figures found by Rosero-Bixby, suggesting that the healthcare reform has continued to have effects on mortality well past the publication of his original study.

3.2 Background

Starting in 1994, Costa Rica underwent a major healthcare reform during the Figueres Olsen administration. What had been a disparate set of hospitals and clinics became a unified three-tiered system with hospitals at the top, middle-sized clinics in the middle, and new, small, local clinics offering primary care at the bottom. The flagship component of the reform, the new local clinics, received the name of *EBAIS*, a Spanish acronym for "Basic Integral

Healthcare Teams". For more details on the reform, see Chapter 1 [36].

Previous work investigating the public health impacts of this reform have studied the effects on access equity and on mortality. On access equity, Rosero-Bixby [67, 68] finds that the proportion of the population with inequitable access to outpatient healthcare went down from 30% to 22% in the first six years of the reform alone. On mortality, he finds an 8% decrease in child mortality as well as a 2% decrease in the general rate as a result of getting an *EBAIS* clinic in your district. Moreover, his analysis finds that every additional five years of having a clinic reduced child mortality by 13% and adult mortality by 4%.

At the time of those studies, these results were highly encouraging for Costa Rica, especially since the reform had high initial costs, beginning with loans from the World Bank and the Inter-American Development Bank [58]. Because of the strong effects the healthcare reforms seems to have had on mortality, it is my goal for this paper to extend these results with almost 10 years of new data as well as newer methods, to measure whether the results have persisted.

3.3 Data

My data come from the Central American Center for Population Research (CCP in Spanish) at University of Costa Rica. They consist of all recorded deaths per district per year, separated by sex, and grouped by age, between 1987 and 2008. Over that period there was a total of 109,701 deaths, evenly split between men and women, with a statistically significant difference in the mean of yearly deaths per district. In addition, about 80% of the observations come from districts that benefited from the healthcare reform. Table 3.1 summarizes these results.

Furthermore, deaths are grouped by age into nine categories (0-8), corresponding to children under 5 years old, ages 5–14, 15–24, 25–34, 35–44, 45–54, 55–64, 65–74, and those 75 or older. Note that, over all years, not surprisingly the two oldest groups account for

Table 3.1: Deaths summary table

Variable	**Control**[1], N=19,458	**Treatment**[1], N=90,243	**p-value**[2]
# of Deaths [3]	1.73 (4.02)	1.09 (2.70)	<0.001
Sex			<0.001
Female	9,729/19,458 (50%)	45,099/90,243 (50%)	
Male	9,729/19,458 (50%)	45,144/90,243 (50%)	

Notes:
[1] Mean (SD); n/N (%)
[2] Wilcoxon rank sum test; Pearson's Chi-squared test
[3] Average number of yearly deaths per district.

the majority of deaths (see Figures 3.1 and B.1). Because I want to observe the evolution of yearly deaths per age group, I decided to exclude those two groups, focusing on those ages under 5 through 64 years old. Figures 3.2 and 3.3 show the number of yearly deaths per group, for both the treatment and comparison groups. This provides the first suggestive evidence of a decrease in mortality in the first two groups, ages under 5 and $5-14$, the ones who should have had the strongest effect of the healthcare reform.

Finally, in order to determine the districts that benefited from the reform, I used the Social Security Administration's Annual Memories (*Memorias Institucionales de la CCSS*) to record, year by year, the districts where new *EBAIS* clinics were built or opened from 1995 to 2008. This serves as an extension to the *EBAIS* district data from 1995 to 2001 kindly shared with me by Rosero-Bixby himself.

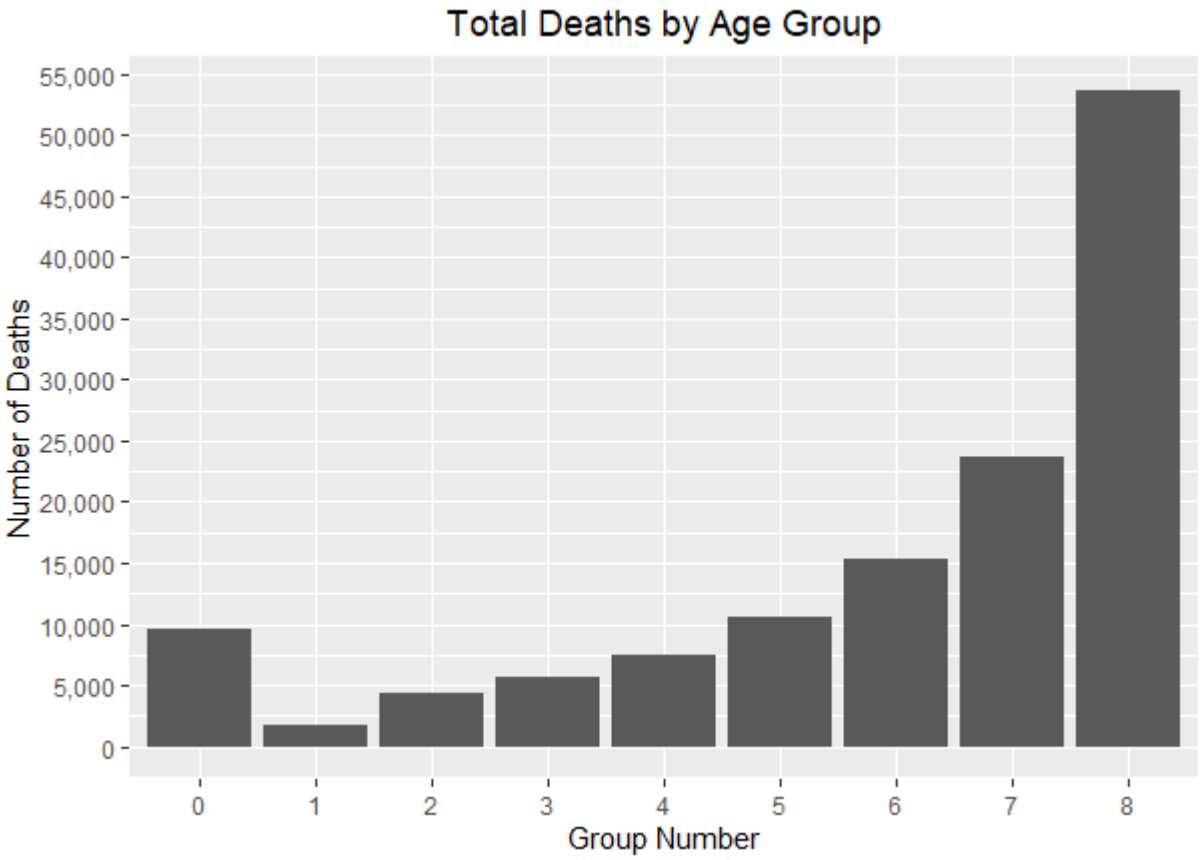

Figure 3.1: Accumulated deaths by age group over all years

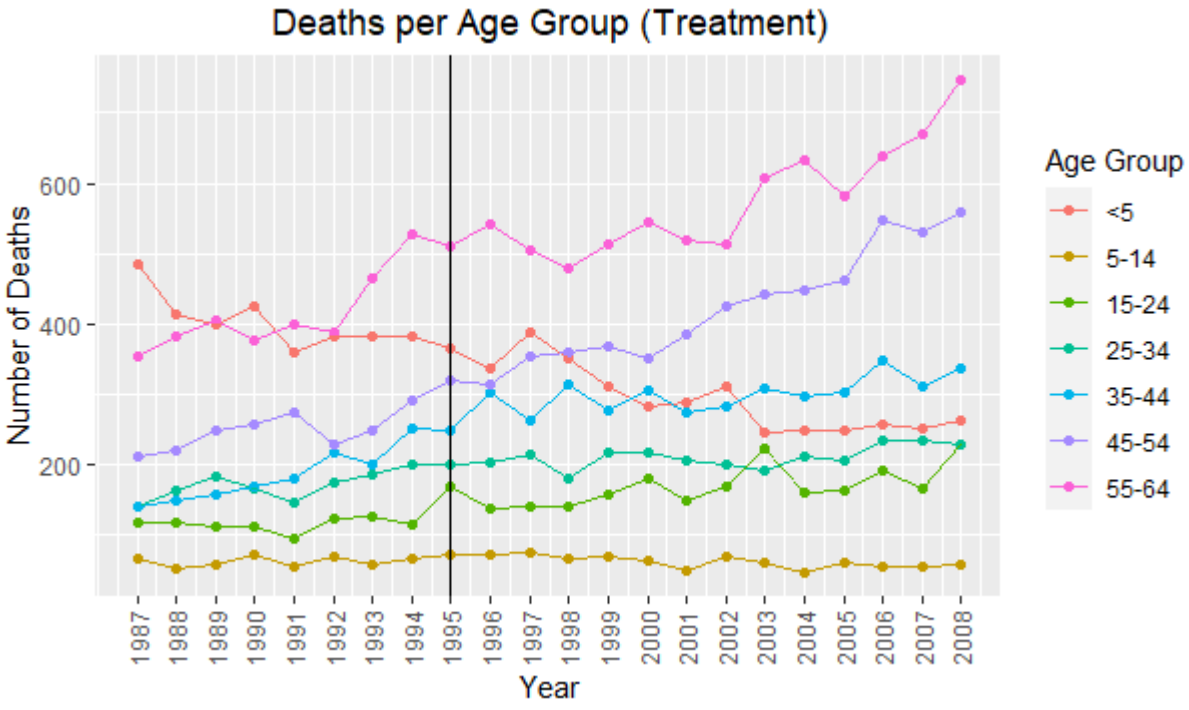

Figure 3.2: Total yearly deaths per age group in treatment group

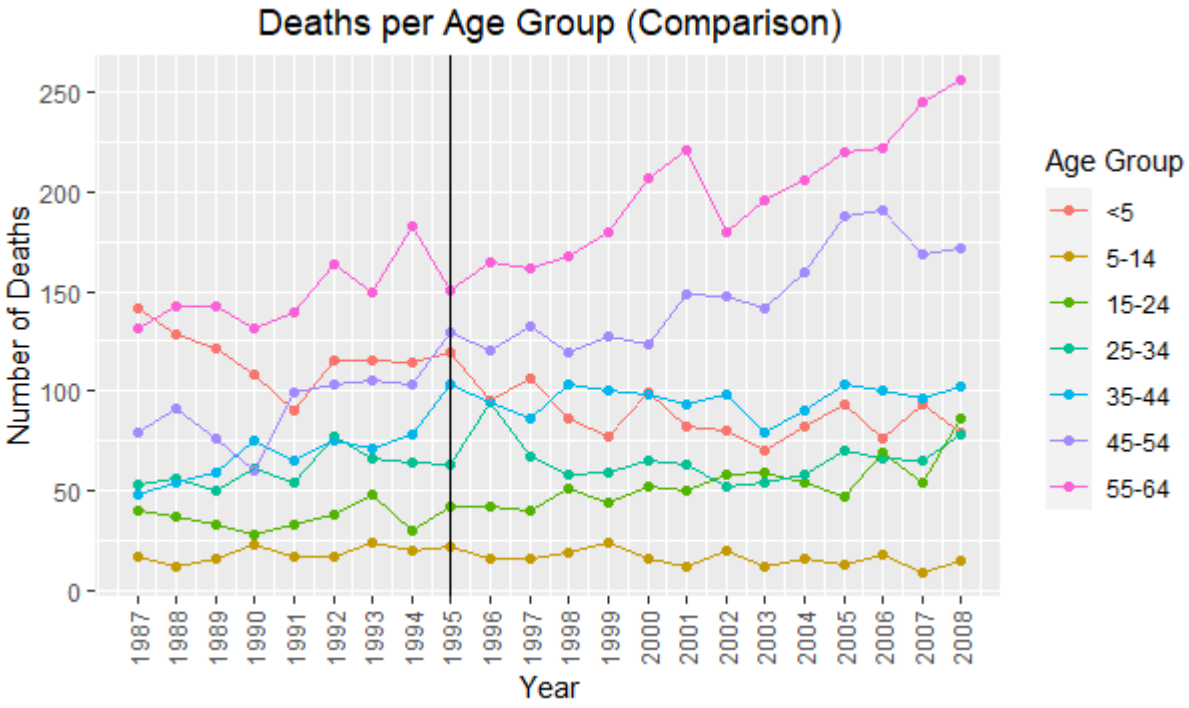

Figure 3.3: Total yearly deaths per age group in comparison group

3.4 Empirical Strategy

For close to twenty years, Rosero-Bixby's (2004) [67, 68] analyses of the healthcare reform that created the *EBAIS* in Costa Rica have been held as evidence that the program worked. Since then, both new data and new methods have become available. Taking advantage of these, I plan to revisit this reform and update Rosero-Bixby's results. To that end, I begin by running an analysis similar to his, this time with almost a decade's-worth more data.

The dependent variable is Number of yearly deaths per district [1], measured in levels and in changes [2] as a function of sex and age group, in addition to district and year fixed effects. The intervention is measured through a combination of two treatment variables: $Clinic_{dt}$, an indicator equal to 1 if district d had a clinic at year t, and *Years since treatment*$_{dt}$, an indicator that measures time since a district got treated, normalizing the value for the comparison group to -30. As Figure 3.4 indicates, there is a deceleration, followed by a decrease, in the number of deaths as time goes on after the reform went into effect. This provides some heuristic support for Rosero-Bixby's decision to include *Years since Treatment*, in addition to *Clinic*, as a variable measuring the intervention.

---

[1] Notice I am using total deaths, and not deaths per thousand. This decision was made due to limitations of the data. Consequently, in interpreting my results on changes in mortality, one must take into account that development and demographic changes can also cause a reduction in mortality for younger people (and an increase in the one for older people).

[2] Because for some districts deaths are 0, at which logarithms would be undefined, percent changes are calculated using the inverse hyperbolic sine transformation.

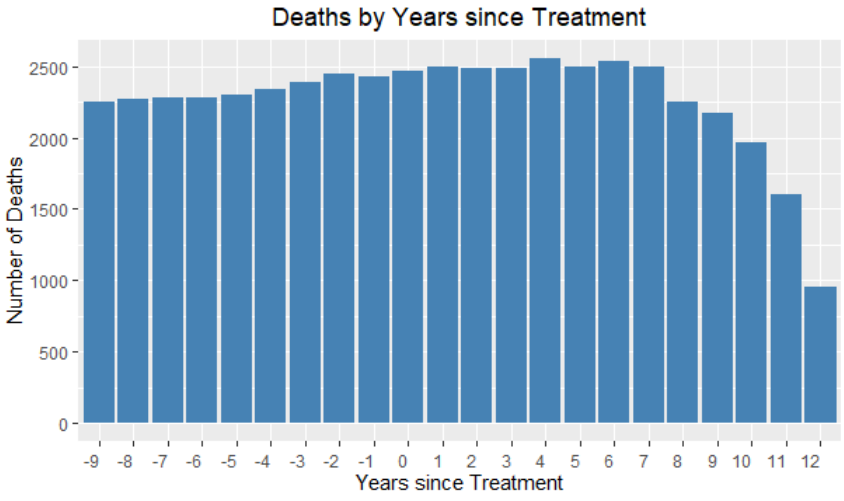

Figure 3.4: Deaths by years since treatment among the treated

With those variables, to calculate general mortality and mortality by age, respectively, the models to estimate become:

$$ihs(Deaths_{dt}) = X^t_{dt}\beta + \delta Clinic_{dt} + \sum_{t=-21}^{12} \eta_t YearsSinceTreatment_{dt} + \lambda_t + \gamma_d + \epsilon_{dt} \quad (3.1)$$

and

$$ihs(Deaths)_{dt} = X^t_{dt}\beta + \delta Clinic_{dt} + \theta^t AgeGroup \cdot Clinic + \sum_{t=-21}^{12} \eta_t YearsSinceTreatment_{dt} + \lambda_t + \gamma_d + \epsilon_{dt} \quad (3.2)$$

where ihs refers to the inverse hyperbolic sine transformation, and λ_t and γ_d are year and district fixed effects, respectively. Also, X is a matrix of controls that includes $AgeGroup$,

a categorical variable with values 0-8 corresponding to ages < 5, $5-14$, $25-34$, $35-44$, $45-54$, $55-64$, $65-74$, and ≥ 75, respectively. Other controls, used by Rosero-Bixby, include percentage of Nicaraguan mothers, general birth rate, proportion of teen mothers, proportion of adults with completed secondary education, proportion of people with health insurance, and an income index.

Heteroskedasticity-robust standard errors are clustered at the district level [11].

A final contribution will be analyzing the reform through the lens of an event study; that is, estimating the model

$$ihs(Deaths_{dt}) = X_{dt}^{t}\beta + \sum_{t=-21}^{12} \eta_t \, YearsSinceTreatment_{dt} + \lambda_t + \gamma_d + \epsilon_{dt} \tag{3.3}$$

3.5 Mechanism

In order to explain how having increased access to outpatient care would reduce mortality, I begin with Rosero-Bixby's [67, 68] original argument. He starts by grouping causes of death into three big categories: transmissible diseases (diarrhea, tuberculosis, respiratory infections, other infectious or parasitic diseases, HIV/AIDS, malnutrition, maternal ailments, and cervical-uterine cancer); social origin reasons (alcoholism, accidents, homicides, and suicides); and other chronic and degenerative issues. His contention is that the reform would cause a decrease in mortality through decreases in deaths within these categories as a result of having prompt access to healthcare.

He finds that deaths by infectious diseases decreased by 14% as a result of the healthcare reform. Similarly, deaths by chronic diseases, the most prevalent cause of death in Costa Rica, decreased by 2%. Finally, deaths by social origin reasons were not shown to decrease. All together, it is the decrease in deaths in the first two categories that drives the decrease in mortality, and which also helps explain my results on general mortality.

This, however, does not help explain the behavior of mortality by age. For that, I rely on the observation that transmissible diseases are the main source of illness in children and young people (see *Memorias Institucionales de la CCSS* [18] for more details). Similarly, degenerative and chronic diseases are most prevalent among older adults. Putting both together, I get a potential mechanism for decreases in mortality by age.

3.6 Results

Table 3.2 presents the regression results of the healthcare reform that created the *EBAIS* clinics on the general mortality rate and the mortality by age group. There are five specifications, with and without accounting for *AgeGroup*, the categorical variable whose values correspond to different age ranges, as specified in Section 3.4. They also analyze deaths in levels and in percentages, the latter calculated through the inverse hyperbolic sine transformation, as some districts report 0 deaths, so their logarithm would not be defined.

To get general mortality, column (3) provides my estimated coefficient to Rosero-Bixby's regression 3.1. Unlike his significant 2% decrease in mortality, I find an even bigger highly significant decrease of 5.62%. Column (2) shows how it is necessary to account for *AgeGroup*, as without it, although there is still a decrease in mortality of 1.74%, it is insignificant. Relatedly, one relationship that goes unexplored in his original paper is that of deaths in levels. For that, column (1) reveals highly statistically significant decrease in mortality of about 0.30 fewer deaths per district per year as a result of getting an *EBAIS* clinic due to the healthcare reform.

Next, column (4) shows the results of regression 3.2, which incorporates an interaction term between the treatment variable *Clinic* and *AgeGroup*. Doing this uncovers the result, if any, that the healthcare reform that created the *EBAIS* had on specific age groups. I expect, given the observed behavior of the group mortality rates, as seen on Figure 3.2, to find a decrease in mortality for the under-5 and $5-14$ groups.

Table 3.2: Rosero-Bixby Extension

	Number of Deaths		ihs (Number of Deaths)	
	(1)	(2)	(3)	(4)
Clinic	−0.2980***	−0.0174	−0.0562***	−0.2275***
	(0.0470)	(0.0120)	(0.0105)	(0.0143)
Clinic · Ages 5-14				0.1682***
				(0.0145)
Clinic · Ages 15-24				0.1935***
				(0.0145)
Clinic · Ages 25-34				0.1614***
				(0.0145)
Clinic · Ages 34-44				0.2133***
				(0.0145)
Clinic · Ages 45-54				0.2062***
				(0.0145)
Clinic · Ages 55-64				0.1616***
				(0.0145)
Clinic · Ages 65-74				0.1843***
				(0.0145)
Clinic · Over 75				0.2531***
				(0.0145)
Age group	No	No	Yes	Yes
Clinic · Age group	No	No	No	Yes
Observations	109,701	109,701	109,701	109,701
Adjusted R^2	0.2872	0.3280	0.5638	0.5653

Notes: Results of regressions 3.1,3.2, and 3.3. Heteroskedasticity-robust errors are clustered at the district level. All regressions include year and district fixed effects. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Because the reference category in *AgeGroup* is children under 5, the coefficient for the treatment variable *Clinic* provides the effect on them. According to my estimation, they saw a highly statistically significant decrease of almost 23% in their mortality. This is more than double the 8% initially found by Rosero-Bixby. In addition, the remaining coefficients for the interaction terms state the extra effect beyond the 23% decrease for each group. Although all these coefficients are positive, most are smaller in absolute value, except for those over 75. Consequently, once I also add the coefficient for *AgeGroup* (not shown), I find small decreases in mortality for all groups but the last three, those 55 and older, which coincides with the heuristic evidence from Figure B.1.

Finally, looking at the event study, estimating regression 3.3, I find puzzling but helpful results: effects indistinguishable from 0 both before and after the reform. Nonetheless, this provides suggestive evidence for parallel trends in mortality between the treatment and comparison groups. This is especially relevant in light of the newest developments in the literature of two-way fixed effects. For instance, Goodman-Bacon [35] proves that two-way estimators in which earlier treated groups are used as controls for later treated ones are in fact a weighed sum of all possible two-way estimators. In this case, my comparison group includes districts that were never treated, partially dealing with the issue.

3.7 Conclusions

Starting in the mid-1990s, Costa Rica underwent a major healthcare reform that greatly expanded access to healthcare. Through the creation of a set of new local clinics called *EBAIS*, Spanish for "Basic Integral Healthcare Teams", access to outpatient care increased from 30% to upwards of 90% of the population in the first decade of the reform alone [58]. This was expected to help improve health indicators like life expectancy, maternal mortality, and under-five mortality, among others.

While chapters 1 and 2 [36] look at the effects of the reform on household income and

education, respectively, this chapter consists of an analysis of its public health impact. In particular, it attempts to update the results of the reform on mortality, both general and by age group, originally studied in a series of papers by Rosero-Bixby (2004) [67, 68], this time with close to a decade's-worth of extra data.

I am able to find a highly statistically significant 5.62% decrease in general mortality, corresponding to 0.30 fewer yearly deaths per district, along with 22.75% decrease in under-five infant mortality. These numbers are highly promising, as they provide evidence that the reform has continued to reduce mortality even after Rosero-Bixby's original study, where he found a 2% reduction in general mortality and an 8% decrease in infant mortality.

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Appendix A

GROSSMAN MODEL

Begin with a one-person household that maximizes utility over one period. Furthermore, suppose that productivity is perfectly observed by firms and that labor markets are competitive. Let H be an individual's health vector-valued production function,

$$H = H(N, L; A, B, D, \mu, e_h) \tag{A.1}$$

where N represents health inputs (such as exercise, nutrition, or the healthcare reform under study) and L is labor supply. Assume that health is increasing in all inputs except perhaps labor supply. The technology behind the production of health is given by A; family background, like parental health, is in B; and environmental factors, D, incorporate aspects like sanitation infrastructure or the disease environment. Unobservables include inherent individual healthiness μ, known (at least partly) only to the individual, and an unknown measurement error e_h.

Assume the individual's real wage equals their marginal product, which is costlessly observable. Real log wage depends on health outputs H, schooling S, family background B, local community infrastructure I, unobserved factors α (like school quality), and random fluctuations e_w, which include measurement error:

$$w = w(H; A, S, B, I, \alpha, e_w) \tag{A.2}$$

Finally, assume that individual utility depends on labor supply L and consumption C,

and that it can be conditioned on health outputs H, schooling S, family background B, and unobserved characteristics ξ, which include tastes:

$$U = U(C, L; H, S, A, B, \xi) \tag{A.3}$$

If the individual has non-labor income V, the budget constraint is

$$p_c C^* + p_n N = wL + V \tag{A.4}$$

where consumption C has been decomposed into a vector of health inputs N with price p_n and non-health consumption C^*.

Under conditions that guarantee an interior solution, the first-order condition with respect to the j-th health input N_j (see the derivation below) establishes that

$$\frac{\partial U}{\partial H}\frac{\partial H}{\partial N_j} = \lambda\left(p_n - L\left[\frac{\partial w}{\partial H}\frac{\partial H}{\partial N_j}\right]\right) \tag{A.5}$$

where λ is the marginal utility of income.

Proof. This comes from solving the utility maximization problem

$$\max_{C,L} U(C, L; H, S, A, B, \xi) \text{ subject to } p_c C^* + p_n N = wL + V$$

where consumption C has been decomposed into a vector of health inputs N with price p_n and non-health consumption C^*. We will therefore also write both sources of consumption as different arguments in the utility function.

We proceed by forming the Lagrangian

$$\mathcal{L} = U(C^*, N, L; H, S, A, B, \xi) + \lambda\left[wL + V - p_n N - p_c C^*\right]$$

and differentiating it with respect to the j-th health input N_j:

$$\frac{\partial \mathcal{L}}{\partial N_j} = \frac{\partial U}{\partial N_j} + \frac{\partial U}{\partial H}\frac{\partial H}{\partial N_j} + \lambda\left(L\frac{\partial w}{\partial H}\frac{\partial H}{\partial N_j} - p_n\right) = 0$$

Following Strauss and Thomas [71], we also ignore the direct impact of N_j on U in order to focus on the indirect effects through health and wages. After this, if we add the term with λ on both sides, we get the desired result.

□

By assumption, health H and wages w are increasing in N and H, respectively. So, if there were a positive change in the j-th health input N_j —as would happen if a government increased access to primary healthcare, as was the case in Costa Rica—, then both health and wages would increase. Moreover, investments in health would not only make individuals wealthier, but also incentivize the use of more that health input. As Strauss and Thomas [71] put it:

> First, if health inputs raise wages through improving health outcomes, then the shadow price of health-augmenting inputs declines, inducing greater use of those inputs. Second, the degree of decline of the health input shadow price is likely to be greater for those in worse health.

Therefore, under this theoretical framework, I would expect for incomes to go up as a result of the healthcare reform.

Appendix B

ADDITIONAL FIGURES AND TABLES

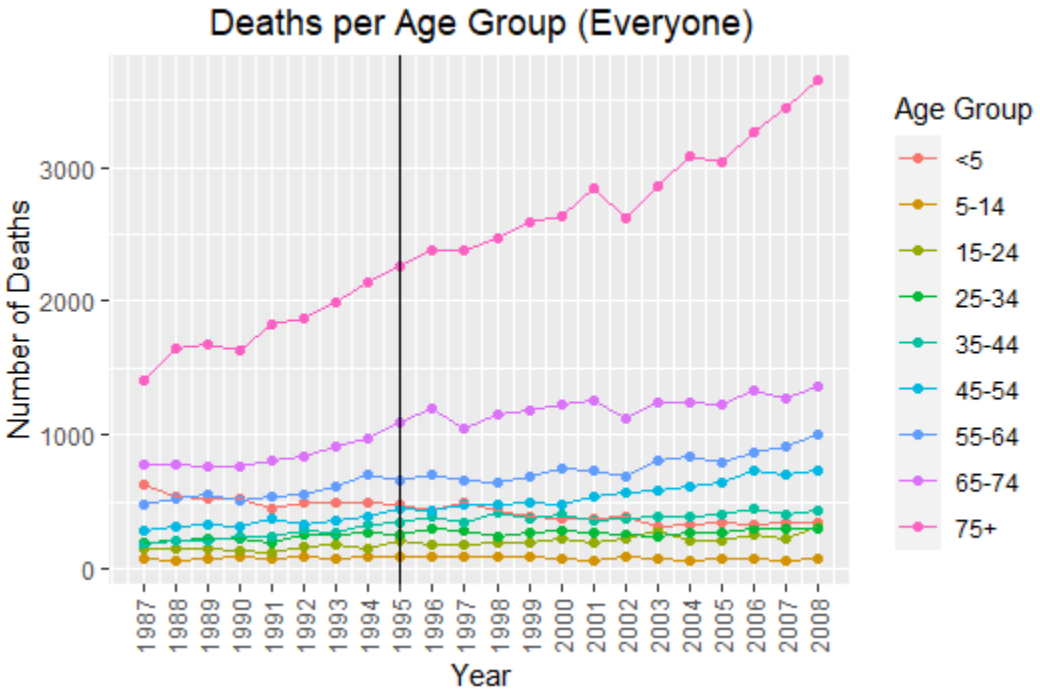

Figure B.1: Total yearly deaths per age group

Table B.1: Leads and lags in treatment for personal income

	Personal income	ihs(Personal income)
	(1)	(2)
Clinic · Year = 1987	−9,537.81*** (3,284.37)	−0.24* (0.13)
Clinic · Year = 1988	−4,405.56 (3,393.17)	−0.01 (0.13)
Clinic · Year = 1989	−7,483.28** (3,451.83)	−0.25* (0.13)
Clinic · Year = 1990	−7,295.32** (3,335.07)	−0.17 (0.13)
Clinic · Year = 1991	−2,770.23 (3,511.06)	−0.15 (0.14)
Clinic · Year = 1992	−1,204.71 (3,295.69)	−0.09 (0.13)
Clinic · Year = 1993	−207.84 (3,363.33)	0.12 (0.13)
Clinic · Year = 1995	−739.42 (3,216.32)	−0.09 (0.12)
Clinic · Year = 1996	−1,242.82 (3,251.37)	0.04 (0.13)
Clinic · Year = 1997	−5,213.12 (3,227.56)	−0.08 (0.13)
Clinic · Year = 1998	−5,329.68* (3,201.89)	−0.17 (0.12)
Clinic · Year = 1999	−6,541.08** (3,062.00)	−0.17 (0.12)
Clinic · Year = 2000	−3,029.59 (3,098.81)	−0.16 (0.12)
Clinic · Year = 2001	−7,360.11** (3,156.64)	−0.24* (0.12)
Clinic · Year = 2002	−5,348.39* (3,077.51)	−0.05 (0.12)
Clinic · Year = 2003	−3,963.22 (3,053.86)	−0.01 (0.12)
Clinic · Year = 2004	−3,348.49 (3,051.92)	−0.10 (0.12)
Clinic · Year = 2005	−6,650.53** (3,017.89)	−0.25** (0.12)
Clinic · Year = 2006	−7,061.74** (3,036.49)	−0.16 (0.12)
Clinic · Year = 2007	−5,690.54* (3,001.19)	−0.12 (0.12)
Clinic · Year = 2008	−7,609.69 (3,041.51)	−0.23** (0.12)
N	311,553	311,553
Adjusted R^2	0.64	0.62

Notes:

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Table B.2: Event study on Household Income

	Levels	Percentages
	(1)	(2)
Years since treatment = -10	−27,237.37*** (10,252.59)	−0.02 (0.02)
Years since treatment = -9	−39,320.32*** (9,811.79)	−0.08*** (0.02)
Years since treatment = -8	−40,300.53*** (9,768.55)	−0.09*** (0.02)
Years since treatment = -7	−12,572.15 (9,635.70)	−0.02 (0.02)
Years since treatment = -6	−5,978.89 (9,512.25)	−0.05** (0.02)
Years since treatment = -5	13,652.79 (9,471.34)	−0.01 (0.02)
Years since treatment = -4	−1,641.41 (9,350.43)	−0.01 (0.02)
Years since treatment = -3	−6,375.87 (9,205.84)	−0.03* (0.02)
Years since treatment = -2	2,067.47 (9,157.32)	−0.01 (0.02)
Years since treatment = -1	3,363.16 (9,078.79)	0.01 (0.02)
Years since treatment = 0	26,582.86*** (8,940.07)	0.02 (0.02)
Years since treatment = 1	26,832.45*** (8,973.41)	0.04** (0.02)
Years since treatment = 2	32,827.17*** (8,846.95)	0.03* (0.02)
Years since treatment = 3	38,254.24*** (8,650.24)	0.05*** (0.02)
Years since treatment = 4	19,630.15** (8,614.37)	0.01 (0.02)
Years since treatment = 5	10,775.00 (8,604.16)	0.02 (0.02)
Years since treatment = 6	25,996.87*** (8,495.19)	0.03 (0.02)
Years since treatment = 7	22,847.50*** (8,366.68)	0.01 (0.02)
Years since treatment = 8	48,845.35*** (8,424.75)	0.07*** (0.02)
Years since treatment = 9	54,300.70*** (8,398.00)	0.07*** (0.02)
Years since treatment = 10	41,681.79*** (8,241.27)	0.04** (0.02)
Years since treatment = 11	20,840.45 (8,436.18)	0.03** (0.02)
Years since treatment = 12		
N	311,553	311,553
Adjusted R^2	0.41	0.43

Notes:

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Table B.3: Event study on Personal Income

	Levels	Percentages
	(1)	(2)
Years since treatment = -10	5,760.05* (3,118.47)	0.28** (0.12)
Years since treatment = -9	732.92 (2,984.40)	0.04 (0.12)
Years since treatment = -8	−1,191.26 (2,971.24)	−0.05 (0.12)
Years since treatment = -7	5,169.72* (2,930.84)	0.16 (0.11)
Years since treatment = -6	4,275.94 (2,893.29)	0.07 (0.11)
Years since treatment = -5	7,628.25*** (2,880.84)	0.25** (0.11)
Years since treatment = -4	9,133.61*** (2,844.07)	0.30*** (0.11)
Years since treatment = -3	7,208.35*** (2,800.09)	0.15 (0.11)
Years since treatment = -2	7,306.48*** (2,785.33)	0.11 (0.11)
Years since treatment = -1	8,577.77*** (2,761.44)	0.20* (0.11)
Years since treatment = 0	12,247.28*** (2,719.25)	0.16 (0.11)
Years since treatment = 1	8,802.78*** (2,729.39)	0.17* (0.11)
Years since treatment = 2	9,290.99*** (2,690.93)	0.17* (0.10)
Years since treatment = 3	11,615.35*** (2,631.09)	0.31*** (0.10)
Years since treatment = 4	7,745.63*** (2,620.18)	0.20* (0.10)
Years since treatment = 5	6,509.76** (2,617.08)	0.16 (0.10)
Years since treatment = 6	8,031.94*** (2,583.93)	0.14 (0.10)
Years since treatment = 7	6,620.32*** (2,544.85)	0.18* (0.10)
Years since treatment = 8	11,786.21*** (2,562.51)	0.25** (0.10)
Years since treatment = 9	12,363.38*** (2,554.37)	0.33*** (0.10)
Years since treatment = 10	9,432.86*** (2,506.70)	0.12 (0.10)
Years since treatment = 11	6,466.10 (2,565.99)	0.02 (0.10)
N	311,553	311,553
Adjusted R^2	0.64	0.62

Notes:

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Table B.4: Leads and lags in education

	Years of Education
Clinic · Year = 1987	−0.18* (0.10)
Clinic · Year = 1988	−0.15 (0.10)
Clinic · Year = 1989	−0.24** (0.10)
Clinic · Year = 1990	−0.28*** (0.10)
Clinic · Year = 1991	−0.19* (0.10)
Clinic · Year = 1992	−0.28*** (0.10)
Clinic · Year = 1993	−0.17* (0.10)
Clinic · Year = 1995	−0.11 (0.09)
Clinic · Year = 1996	−0.05 (0.10)
Clinic · Year = 1997	−0.27*** (0.09)
Clinic · Year = 1998	−0.26*** (0.09)
Clinic · Year = 1999	−0.04 (0.09)
Clinic · Year = 2000	−0.02 (0.09)
Clinic · Year = 2001	−0.02 (0.09)
Clinic · Year = 2002	0.01 (0.09)
Clinic · Year = 2003	−0.01 (0.09)
Clinic · Year = 2004	0.11 (0.09)
Clinic · Year = 2005	0.04 (0.09)
Clinic · Year = 2006	−0.03 (0.09)
Clinic · Year = 2007	−0.05 (0.09)
Clinic · Year = 2008	0.02 (0.09)
N	311,553
Adjusted R^2	0.50

Notes: ***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Table B.5: Effects on those 25 or older

	Change in Years of Education
Clinic · YearsElapsed = −13	0.03(0.29)
Clinic · YearsElapsed = −12	0.01(0.30)
Clinic · YearsElapsed = −11	0.01(0.30)
Clinic · YearsElapsed = −10	−0.07(0.29)
Clinic · YearsElapsed = −9	0.19(0.29)
Clinic · YearsElapsed = −8	−0.02(0.26)
Clinic · YearsElapsed = −7	0.29**(0.26)
Clinic · YearsElapsed = −6	0.24*(0.21)
Clinic · YearsElapsed = −5	0.25**(0.20)
Clinic · YearsElapsed = −4	0.07(0.19)
Clinic · YearsElapsed = −3	0.25**(0.18)
Clinic · YearsElapsed = −2	0.12(0.17)
Clinic · YearsElapsed = −1	−0.07(0.16)
Clinic · YearsElapsed = 0	−0.10(0.16)
Clinic · YearsElapsed = 1	−0.03(0.16)
Clinic · YearsElapsed = 2	−0.11(0.15)
Clinic · YearsElapsed = 3	0.04(0.14)
Clinic · YearsElapsed = 4	0.19(0.14)
Clinic · YearsElapsed = 5	0.11(0.13)
Clinic · YearsElapsed = 6	0.18(0.12)
Clinic · YearsElapsed = 7	0.26(0.12)
Clinic · YearsElapsed = 8	0.23(0.12)
Clinic · YearsElapsed = 9	0.23(0.11)
Clinic · YearsElapsed = 10	0.20(0.11)
Clinic · YearsElapsed = 11	0.10(0.11)
N	101,814
Adjusted R^2	0.53

Notes: ***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.