Essays on the effects of the Homestead Act on Land Inequality and Human Capital, the effects of Land Redistribution on Crop Choice, and the effects of Earthquakes on Birth Outcomes

by

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Thesis

Submitted to the University of Warwick

for the degree of

Doctor of Philosophy

Department of Economics

July 2017

THE UNIVERSITY OF WARWICK

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Acknowledgments

I am grateful to my supervisors Sascha Becker and Fabian Waldinger for their encouragement and invaluable advice. I would also like to thank Clément de Chaisemartin, Luigi Pascali, James Fenske, Rocco Macchiavello, Bishnu Gupta, and seminar participants at Warwick for their helpful comments on the various stages of the research in this thesis.

I would also like to thank Daron Acemoğlu, James Robinson, and Francisco Gallego for providing the data on the land reform expropriations. Additionally, I would like to thank Felipe Gonzalez with his help dealing with this data.

I would like to thank my parents, Alejandro Lillo and Marta Bustos, my sisters Alejandra and Marta Lillo, and my partner Luiza Bandeira for their unconditional support and love. It would have been impossible without you.

The support and encouragement of my friends cannot be understated. I would specially like to thank Gonzalo Gaete, Catalina Bravo, Conrado Cuevas, Stephanie Langdon, Mario Sanclemente, María Teresa Plaza, Harold Contreras, Andrea Cortés, Nicolás Navarrete, Matteo Gamalerio, Francesca Noli, Guillem Ordóñez, Paula López, Alex Wisch, Gilda Rivero, Ghassan Asbool, Dana Kassem, Arthur Seibold, and Zakia Shiraz.

Finally, I would like to thank my fellow classmates of the Warwick Economics PhD programme. Their support and feedback was crucial for the development of the ideas in this thesis.

Declarations

This thesis is submitted to the University of Warwick in support of my application for the degree of Doctor of Philosophy. It has been composed by myself and has not been submitted in any previous application for any degree.

The work presented (including data generated and data analysis) was carried out by the author.

All data sources are publicly available except for the Land Reform expropriation records, which were provided by Daron Acemoğlu, James Robinson, and Francisco Gallego.

Abstract

Chapter 1: Land Inequality and Human Capital: Evidence for the United States from the Homestead Act. This chapter uses historical records of land patents and county level census data to estimate the impact of the Homestead Act of 1862 —an egalitarian land distribution policy implemented in the United States— on land inequality, school enrolment, and literacy during the late nineteenth and early twentieth centuries. The results show that the Homestead Act reduced land inequality and increased school enrolment and literacy, and that there is not heterogeneous effects on school enrolment by sex, but the effect *is* driven by the impact on children of primary school age. Using the Homestead Act as an instrument for land inequality, the results show that land inequality had a strong negative impact on school enrolment. This result is relevant to the literature because identification does not rely on variation across geographic, climatic, or soil characteristics. These results are robust to the inclusion of state specific year fixed effects and are not driven by convergence. On the contrary, I argue that convergence in school enrolment was a consequence of the Homestead Act.

Chapter 2: Land Redistribution and Crop Choice: Evidence from Reform and Counter-Reform in Chile. This chapter uses unique historical data on the Chilean land reform of the 1960s and 1970s to estimate the impact that redistribution had on land inequality and crop choice. The results show that land redistribution had a persistent negative effect on land inequality, and that areas that were treated with more reform increased their share of land cultivated with fruits, vegetables, and vineyards, and lowered the share of land destined to forest plantations. The fact that a military coup interrupted the reform process allows for the comparison of the effects of reform and counter-reform, which sheds light on the mechanisms through which redistribution operated. I find that land that was transferred to new owners drive the results for crop choice, but not those for land inequality.

Chapter 3: Earthquakes and Birth Outcomes in Chile. This chapter estimates the effects of earthquakes on birth weight and length of gestation. I use administrative data on the universe of live births in Chile between 1994 and 2011. I combine that data with GIS raster information from USGS *ShakeMaps* to assign a detailed measure of earthquake intensity for each birth during each trimester of pregnancy. I find that, although the baseline estimates suggest a weak negative effect, these results are not robust to the exclusion of births from a strong 8.8 magnitude earthquake that struck off the coast of south-central Chile, which caused approximately 500 casualties, heavy infrastructure damage, and significant disruption to the government's logistics.

Chapter 1

Land Inequality and Human Capital: Evidence for the United States from the Homestead Act

1.1 Introduction

Did egalitarian land policies in the United States lead to higher human capital accumulation? The influential Engerman and Sokoloff hypothesis (Engerman and Sokoloff, 1997; Sokoloff and Engerman, 2000) claims that factor endowments such as climate, geographic, and soil characteristics determine the types of crops that are more profitable in a given area, and thus these characteristics determine whether crops with economies of scale are grown. Places that are more suitable for crops with economies of scale had higher land inequality, which in turn led to institutions that are detrimental for economic development. This hypothesis has been tested and used by other authors to attempt to identify the effects of inequality on human capital (Galor et al., 2009; Easterly, 2007; Ramcharan, 2010). Unlike this literature, I use variation coming from the implementation of an egalitarian land distribution policy in the late nineteenth century called the Homestead Act of 1862.

The main contribution of this paper is that I show that the Homestead Act had a causal effect on land inequality and human capital. These results suggest that the determinism implied by the Engerman and Sokoloff hypothesis was addressed with an egalitarian land policy in the United States. I also contribute to the literature by showing that the estimate of the effect of land inequality on school enrolment is negative, using variation that does not come from geographic or climate heterogeneity, but rather by policy. To my knowledge, only one other study has used a change in policy to determine the effect of inequality on human capital in a historical setting: Cinnirella and Hornung (2016) use the abolition of serfdom in Prussia as a source of variation.

The Homestead Act established that any US citizen or immigrant with the intention of becoming a citizen could obtain a limited amount of federal land (no more than 160 acres) essentially free (only a small registration fee had to be paid), with the requirement that the homesteader had to reside and improve his or her claim for five years before being granted the corresponding land patent.[1] A previous version had already been approved but was vetoed by President James Buchanan. The bill could only be approved during President Abraham Lincoln's term because southern members of congress had withdrawn from Washington earlier in the Civil War. Thus, the timing of the policy was unexpected.

In this paper I use variation across counties in the implementation of the Homestead Act of 1862 to estimate a series of reduced form equations that measure

[1] Other stipulations were that the claimant had to be 21 years old or the head of a household, and had never taken up arms against the United States. This latter condition thus excluded former confederate soldiers.

the impact of the Homestead Act on land inequality, school enrolment, and literacy. I then employ an instrumental variables estimation strategy to estimate the effect that land inequality had on school enrolment.

To identify the reduced form impact of the Homestead Act, I employ a differences-in-differences estimation strategy. I define treatment based on terciles of the stock of Homestead Act acres distributed between 1862 and 1930. The control group includes all counties in states that did not belong to the public domain, and counties that were in the public domain but received zero Homestead Act acres. The post-treatment period is defined as the decades starting in 1870 until 1930. The outcomes of interest are land inequality measured by the Gini coefficient (imputing landless households, as in Vollrath (2013)), school enrolment measured by the enrolment rate of white children aged seven to nineteen, and literacy of adults of twenty years or older. For the human capital variables I restrict the measure to individuals who were born in the same state that they reside. This mobility restriction is to avoid the bias coming from sorting.

An important aspect of my methodological choices is that I include state-specific year fixed effects to all estimations. This methodological choice absorbs a considerable amount of the variation used for identification, but is a conservative choice so as to strengthen the robustness of the results.

The first set of results correspond to reduced form estimates of the effect of the Homestead Act on land inequality, school enrolment, and literacy. I find that land inequality was reduced by approximately 5 Gini points in counties treated with medium intensity of Homestead Act acres (approximately 62.5 percent of the standard deviation of the land Gini in 1860). In high intensity counties, the reduction in inequality was approximately seven Gini points (87.5 percent of a standard deviation in 1860). For school enrolment, the results show that counties treated with intermediate levels of the Homestead Act increased school enrolment by 15 percentage points, and counties treated with high levels by almost 20 percentage points. I also show that the effect is mostly due to increases in the school enrolment of young children aged seven to thirteen. In the case of literacy I show that the Homestead Act increased the literacy rate of adults by five percentage points for counties with low levels of treatment, eight to 9 percentage points for counties with medium levels of treatment, and approximately 18 percentage points for counties with high levels of treatment.

IV estimation of the effect of land inequality on school enrolment shows that land inequality decreases school enrolment. The point estimates are statistically significant at the 5 percent significance level, and imply that a 10 point increase

in the Gini of land inequality reduces school enrolment by 18 percentage points. However, these results should be taken with caution because the diagnostics tests for the first stage of the IV estimation suggest that the model is under-identified. I show that the inclusion of an specific control —prevalence of slavery in 1860— serves to strengthen the first stage and increases the magnitude and statistical significance of land inequality. The prevalence of slavery in 1860 acts as a proxy for the presence of plantations. I show that the effect of the Homestead Act on land inequality is strengthened once I account for slavery in 1860, because the gradual breakdown of plantations biases the estimated effect of the Homestead Act towards zero.

The rest of the paper is structured as follows. In the next section, I discuss the literature regarding inequality and the development of human capital. Then in section 1.3 I describe the historical background of the Homestead Act and federal land policy during the nineteenth century. Next, in section 1.4 I describe the data sources and the methodology I employ to construct my dataset. I describe the sample and provide summary statistics in section 1.5. The empirical framework is discussed in section 1.6, and the estimation results are presented and discussed in section 1.7. Finally, I conclude in section 1.8.

1.2 Inequality and Human Capital

In this section I discuss the relevant literature regarding the relationship between inequality and human capital accumulation. I start by locating the Homestead Act in the general discussion about the role of institutions on development. I then discuss the theory that links inequality and human capital accumulation through imperfections in capital markets. I then compare that literature to the one that focuses on the political economy of the provision of public goods. I then turn to the empirical literature and discuss their identification strategies. Finally I conclude this section by establishing why this paper is a contribution to the existing literature.

In this paper I focus on human capital as an outcome, and in particular on school enrolment and literacy. Education, to paraphrase North and Thomas (1973)[2], is not a factor of growth, but it *is* growth. In this section I describe the literature that attempts to quantify the impact that inequality has on education. This is relevant to estimate the impact of the Homestead Act, because one of its objectives was to avoid the high concentrations of wealth that had characterised the southern United States, and thus bring about a relatively more egalitarian society in the western states (Shanks, 2005).

[2]See North (1991) for a summary of this early research.

In a pair of highly influential articles, Stanley Engerman and Kenneth Sokoloff argued that land inequality, shaped by factor endowments such as climate and native population density, led to bad institutions in the Americas (Engerman and Sokoloff, 1997; Sokoloff and Engerman, 2000). They stress the role of geographical and population endowments, paying particular attention to the effect of geographic and climate characteristics on crop choice. One mechanism they highlight is that tropical climates, which were more suitable for cash crops that enjoyed returns to scale such as tobacco, sugar, and cotton, led to the concentration of land. This high concentration of land led to inequality and the concentration of political power, which in turn led to bad economic institutions such as slavery (Nunn and Wantchekon, 2011).

The effect that various colonial institutions had on growth and other outcomes were the subject of a very influential research agenda that began with Acemoglu et al. (2001). More recently, Bruhn and Gallego (2012) used historical sources to classify colonial practices based on population density and economies of scale, and look at the long term impact that these activities had on economic development across the Americas.

Theoretically, the connection between land inequality and education can flow through various mechanisms. In a very influential article, Galor and Zeira (1993) develop a model in which initial wealth inequality leads to persistent inequality because capital market imperfection and the indivisibility of human capital led to multiple stationary equilibria. Agents below a certain threshold of wealth optimally converge to a low level of human capital, and those above the threshold converge to the high human capital steady state. However, in a contrasting theory Mookherjee and Ray (2003, 2010) highlight that indivisibility of human capital is not a requirement for persistent inequality (at least in utility and consumption), and a richer set of occupations will led to increasing returns to scale in human capital. They show that a sufficiently dense distribution of occupations leads to multiple steady states. This literature thus emphasizes the role of imperfect capital markets that led agents to, in equilibrium, sort into occupations and human capital levels.

The models discussed above contrast with the political economy explanation of the institutional literature, in which education is not widespread because there is an elite that considers it against its interests, and thus decides to block its funding. Adamopoulos (2008) is explicit about this mechanism in the model that he develops while Galor and Moav (2006) develop a model in which the elite is not monolithic, and thus because the industrial revolution required a more educated labour force, capitalists broke ranks with the landed agricultural elites and allied with workers

to redistribute wealth through education. A later article, discussed in greater detail below, expands this model and tries to empirically estimate the effect of inequality on public funding of education (Galor et al., 2009).

One issue that could bias the estimation of the effects of the Homestead Act on human capital is the possible sorting of individuals according to their preference for education. Ethnic, cultural, or religious differences could drive sorting of individuals and bias the results. Although the differences-in-differences methodology described in section 1.6 controls for selection based on county characteristics that are fixed across time periods, one mechanism by which the Homestead Act could affect outcomes can be the sorting of individuals according to their preferences. A strand of literature uses the theory of public choice to determine the allocation and distribution of local public goods, of which the seminal theory is provided in Tiebout (1956). This is relevant for inequality research in the sense that the theory predicts that individuals will sort into homogeneous groups with similar preferences. Hoxby (2000), Rhode and Strumpf (2003) and Alesina et al. (2004) are examples of the empirical testing of this theory. The main message of this literature is that to estimate the impact of the Homestead Act, sorting of individuals with different preferences has to be taken into account. However, in the case of religion, Becker and Woessmann (2009) show that the channel through which Protestantism increased growth in Germany was increased literacy, and not through other cultural characteristics.

Another aspect of the literature is the research into the role of property rights in development. This is one of the key institutional factors that affect growth according to North and Thomas (1973). In the context of western expansion, the issue of property rights in the frontier was imperative. The proponents of the Homestead Act argued that giving the land away for free, but with the residency requirement, encouraged the strengthening of property rights. Anderson and Hill (1990) develop a model that makes the timing of settlement endogenous, and shows how Homesteading can delay this decision. Hornbeck (2010) looks directly at the effect of property rights on agricultural growth by exploiting the introduction of barbed wire as a new technology that allows better enforcement of property rights. Alston et al. (2012) highlight the role that norms and political arrangements have on property rights and how they are enforced in the frontier.

The empirical historical literature has shown a significant negative correlation between inequality and education. Goldin and Katz (1998) show that, at the state level, a "high level of wealth broadly distributed" was an important factor that encouraged support for Higher Education in the early twentieth century. Goldin and Katz's extensive research on the subject is compiled exhaustively in Goldin and

Katz (2008). Goldin and Katz (1999) show that in the context of the "high school revolution" in the United States during the early twentieth century, the size of the middle class had a positive correlation with educational attainment. To measure this correlation they proxy the size of the middle class with the number of automobiles per capita in 1930, and they use as the dependent variable the graduation rates across states in 1928. They stress the mechanism by which inequality adversely affects cooperation and social cohesion, which they highlight because education is a public good that requires cooperation to be publicly provided.

Using census data for 1890, Vollrath (2013) looks directly at the correlation between inequality and education in a cross-sectional setting. His focus is on the correlation of inequality and school funding at the county level, and shows that while there is a significant negative correlation across the full sample, the correlation is only significant in the northern states. Moreover, he shows that redistributing land in the south to match inequality levels in the north would not close the gap in education funding between these broad geographical areas.[3]

Identification of the effects of inequality on educational attainment or enrolment is difficult because of a series of confounding factors. Easterly (2007) directly tests the Engerman and Sokoloff hypothesis using cross-country data for the latter part of the twentieth century. Easterly instruments income inequality (measured as the average Gini from 1960 to 1998) with the log of the ratio of land suitable for wheat to land suitable for sugar. He finds a strong negative effect of inequality on income in 2002, a negative effect on institutions, and a negative effect on school enrolment.

For the United States, Galor et al. (2009) estimate a differences-in-differences model where they show that the share of land held by the top land owners lowers public expenditures in education at the state level between 1890 and 1920. They achieve identification using the interaction of land suitability for cotton and the relative price of cotton and wheat as an instrument for the share of land held by the top land owners. Ramcharan (2010) uses data for US counties and states in 1890 and 1930 to estimate the effect of inequality on educational spending. However, unlike Galor et al., Ramcharan estimates a cross-sectional instrumental variable regression, where the instruments are different measures of weather risk and inequality is measured using the Gini index.

[3]In this paper, I proxy inequality with a similar approach to that used by Vollrath (2013), which is explicit in incorporating landless workers into the distribution of land. Other papers proxy agricultural inequality by calculating a farm Gini, which is inequality amongst *landowners* (Deininger and Squire, 1998).

Cinnirella and Hornung (2016) look at the effects of land ownership concentration in Prussia during the nineteenth century using county level data. They use the level of inequality at the beginning of the period, right after the abolition of serfdom, to proxy for the level of inequality produced by the old regime and show that there was considerable convergence. They attempt to estimate an instrumental variables model where soil characteristics are used as instruments, but they caution against their own IV estimates because the emancipation of serfs could be related to soil quality through other mechanisms and not only through land inequality.

To summarise the literature described in this section, to the extent that the empirical studies have attempted a causal identification of the effects of inequality on human capital, they have done so heavily relying on the Engerman and Sokoloff hypothesis. Most attempts have relied on cross-sectional analysis, and the few that attempt some kind of longitudinal analysis have used price variations interacted with soil characteristics to instrument inequality, or regressed changes on levels. Thus, to the best of my knowledge no attempts have been made to use a narrowly defined (quasi) natural experiment in a longitudinal framework to identify the effect of inequality on human capital.

1.3 History of the US westward expansion and Federal Land Policy in the nineteenth century

The end of the Seven Years War culminated with the Treaty of Paris signed in 1763. Most territorial gains during the war were reverted in the treaty, but Great Britain expanded its colonial authority in North America westward to the Mississippi river. It also gained Florida to the south, which was then under Spanish control. In a previous secret agreement, France ceded Louisiana and all other lands west of the Mississippi to Spain in the Treaty of Fontainebleu in 1762 (Hoffman, 1996).

The existing Thirteen Colonies made various claims to the newly acquired lands between the Appalachians and the Mississippi river. These claims sometimes overlapped and led to conflict amongst the colonists. However, King George III denied these claims through the Royal Proclamation of 1763, effectively invalidating any land patents and claims made by British colonies in the lands west of the Proclamation Line. This boundary ran north from Georgia to Maine, roughly following the line created by the Appalachian Mountains. This constraint was a great source of discontent for the colonists against the crown, and is considered by some as one of the causes of the War of Independence (Holton, 1994).

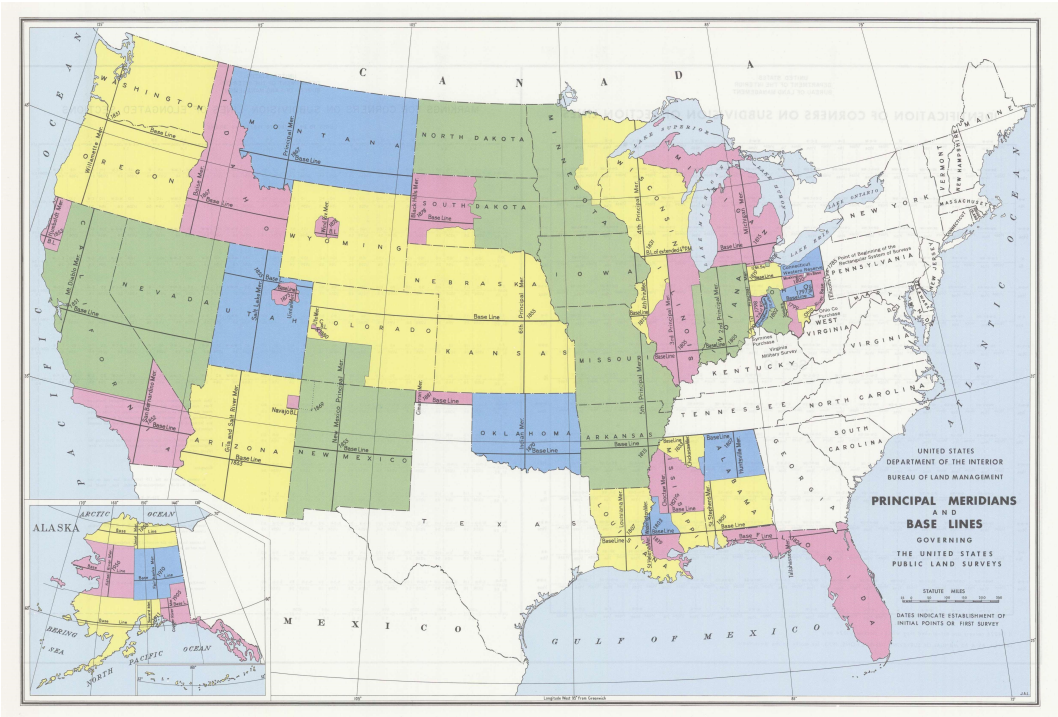
After independence, the newly formed Federal Government of the United States had no assets and enormous amounts of debt incurred by the Continental Congress to pay for the war. The issues of raising money to pay for these debts and the overlapping claims by the former colonies of the territory between the Appalachians and the Mississippi river were linked. The debate about what to do with this land and how to distribute it was to be ongoing throughout the nineteenth century (Anderson and Hill, 1990; Gates, 1941; Shanks, 2005; Shannon, 1936). There was the Jeffersonian ideal of the "yeoman farmer" which favoured a more equal distribution of land, and at the same time the Hamiltonean view that what was required was to use the land as a source of revenue to pay down the national debt (Gates, 1940, 1941, 1976).

In 1781, New York ceded its claims to the Federal Government and during the ensuing decade the rest of the states followed suit. By 1802, the Public Domain included what was known as the Northwest Territory, which contained the lands of the current states of Ohio, Indiana, Illinois, Michigan, Wisconsin, and parts of Minnesota. The Public Domain also included in 1802 what is now Alabama, Mississippi, and Tennessee. However, the latter was ceded back from the Federal Government to form the state of Tennessee in 1806 (and a further cession in 1846). All in all, excluding Tennessee, the states ceded to the Federal Government 236,825,600 acres between 1781 and 1802, which represents 10.4 percent of total US territory today (U.S. Department of the Interior, 2016).

The Louisiana Purchase in 1803 almost doubled the territory of the United States, adding 529,911,680 acres to the Public Domain (U.S. Department of the Interior, 2016). In 1819, the Adams-Onís treaty established that Florida would be ceded to the United States (which had been regained by Spain from Great Britain during the American Revolutionary War). This treaty also established the border between the United States and New Spain. The next large land acquisitions by the United States were Oregon Compromise with Great Britain in 1846, the Annexation of Texas in 1845, the Mexican Cession following the Mexican-American War in 1848, and the Gadsen Purchase from Mexico in 1853 (U.S. Department of the Interior, 2016). Although Texas was not included in the Public Domain, in 1850 the United States purchased a territory of almost 79 million acres from Texas which contains parts of what is now New Mexico, Oklahoma, Kansas, Colorado, and Wyoming.

The Public Lands Survey System was created by the Land Ordinance of 1785. The first areas to be surveyed were in what now is the state of Ohio, although the system was slightly different to the rest of the country (U.S. Department of the Interior, 2016). As the United States expanded westward this system was used to

Figure 1.1: Map of the Public Lands Survey System



Source: Bureau of Land Management - General Land Office Records Website

survey the newly acquired lands, and thus the states that were once part of the Public Domain are called PLSS states.[4] Non-PLSS states generally use the metes-and-bounds system to survey lands, which is a system based on land marks and not rectangular. Figure 1.1 shows the different PLSS areas according to the name of their *principal meridian*.

The PLSS is a rectangular survey system. The first step when surveying a certain territory is establishing a *principal meridian* and a *base line* at a given marker called an initial point. Guiding meridians and standard parallels are then drawn every twenty four miles. For meridians these are twenty-four miles apart east and west of the principal meridian and standard parallels are every twenty-four miles north and south of the base line. The guiding meridians and standard parallels form rectangles called *quadrangles*, which are then divided into *townships* of 36 square miles. These in turn are divided into thirty-six *sections*, one mile squared, or 640 acres. Within each section there are *aliquots* which are described as a fraction of a

[4]PLSS states include: Alabama, Alaska, Arkansas, Arizona, California, Colorado, Florida, Iowa, Idaho, Illinois, Indiana, Kansas, Louisiana, Michigan, Minnesota, Missouri, Mississippi, Montana, North Dakota, Nebraska, New Mexico, Nevada, Ohio, Oklahoma, Oregon, South Dakota, Utah, Washington, Wisconsin, and Wyoming.

section (e.g. north-west quarter of a section).

Because the Earth is a sphere, the meridians converge at the north pole, and thus the east-west distance in the northern boundary of a township is going to be slightly shorter than the southern boundary. The PLSS introduced two corrections to ensure a certain degree of homogeneity. First, within a township, sections are measured from the south-east corner to be exactly one mile squared. Thus the eleven sections along the western and northern boundaries of the township generally have an area less than one mile squared. Second, at the quadrangle level, the meridian lines are resurveyed to be exactly 24 miles from the principal meridian (Gay, 2015; U.S. Department of the Interior, 2009).

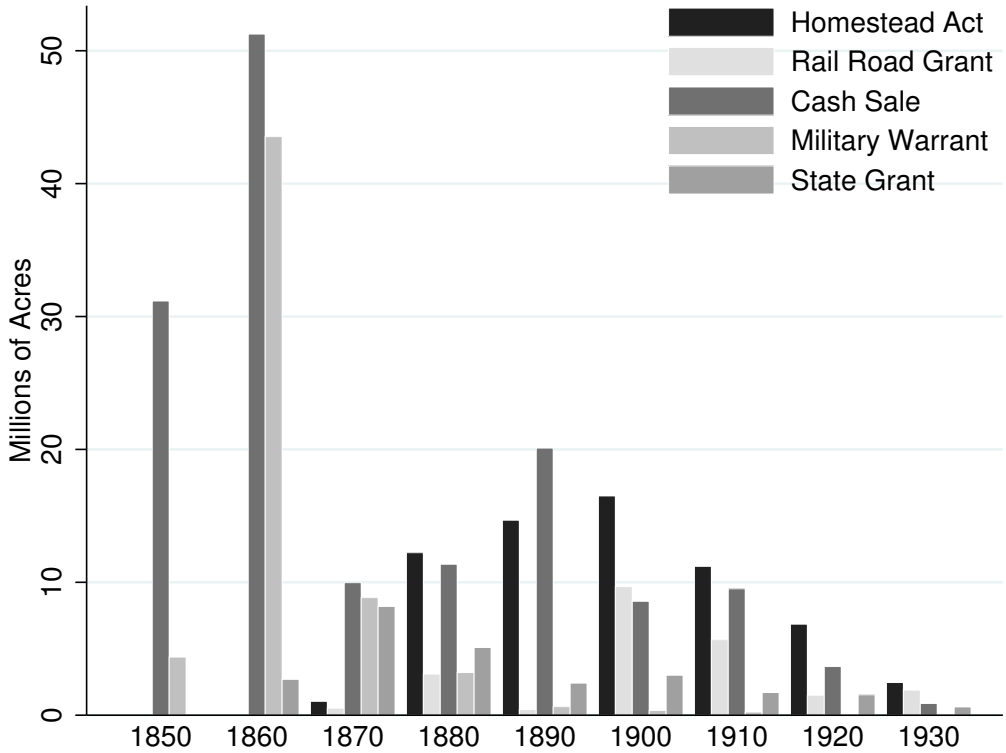
The Land Ordinance of 1785 was followed by the Northwest Ordinance of 1787 which established the Norhtwest Territory (Shanks, 2005). At first with the Land Ordinance of 1785, individuals that wished to purchase land from the Federal Government had to participate in an auction with a minimum price of $ 1.00 per acre, and had to acquire a minimum of 640 acres, which was the size of a section in the terminology of the PLSS. The Northwest Ordinance allowed payment to be split into one third cash and the remainder payable in three months (Anderson and Hill, 1990), effectively introducing a credit system.

Subsequent land acts (1800 and 1804) raised the price per acre to $2.00 minimum and lowered the minimum purchase to 320 acres (half-section) and then 160 acres (quarter-section). These acts lengthened the terms of the credit system and introduced incentives for upfront cash payment (by reducing the price per acre to $1.64 if payment was done in full with cash). However, the many difficulties in establishing successful farms led to default, and eventually to the Land Act of 1820. This act ended the credit system, but it also lowered the price per acre to $1.25 and lowered the minimum size requirement to a half-quarter-section (80 acres) (Anderson and Hill, 1990, see Table A1.).

The price reductions were not able to stem the pressure from illegal settlers and squatters. A series of acts beginning in 1830, which had to be renewed biannually, allowed squatters to purchase their tracts of land at the minimum price ($1.25 per acre) with a cap on the amount of land that these squatters could buy (160 acres, a quarter-section). The Preemption Act of 1841 formally established the right of preemption by removing the requirement that the act had to be renewed every two years. The terms were similar to the renewal of 1832, in which a minimum of 40 acres was required for the preemption right to be exercised, but the 160 acre cap remained in place. Importantly, preemption gave squatters priority in buying a tract of land, since they did not need to bid and compete with other prospective

buyers (Gates, 1941).

Figure 1.2: Acres granted in Land Patents by Patent Type and Decade



Notes: The graph shows the flow of acres granted by patent type and year. Other patent types are excluded from the graph for clarity, as they represent less than 5 percent of total land granted. The sample has been restricted to patents granted in counties that already existed by 1860 (i.e. it excludes large territories that were to subdivide into an "extreme" amount of subdivisions). For a definition of what "extreme" means and how the sample is constructed, see section 1.5.

Finally, in 1854, the Graduation Act modified the terms of sale of land that had not been preempted or sold up to that point. It lowered the price per acre to be in proportion to the time that the land had been on the market but unsold. For land that had been available for sale for ten years, the price per acre was $1.00, while land unsold for 30 years came to be as low as 12.5 cents per acre.

Another way of allocating public land was through military warrants. In its early years, the United States had few financial resources but huge quantities of land. Thus, military pensions would be paid in terms of land. This mechanism was used from the early days of the republic until well into the mid-nineteenth century. The first of these acts, the Scrip Warrant Act of 1790, allocated 160 acres of land to veterans of the Revolutionary War, part of the Virginia Line, in Ohio. Other acts followed for subsequent wars: in 1812 for the war against the British, and in

1847 for the Mexican War. Other acts in 1850, 1852, and 1855 granted land to veterans of the Indian wars and to veterans of the Mexican War which served less than 12 months (Gates, 1941). All these acts had a limit to the amount of land any given veteran could receive: 160 acres. However, Gates (1941) argues that records show that few veterans actually exercised their right to land, and instead sold the warrants to land speculators and brokers.

Figure 1.2 shows the sum of acres granted in land patents by patent type and decade the patent was granted (i.e. the quantities for 1850 corresponds to the sum of acres granted in the decade starting in 1 January 1841 to 31 December 1850). It is clear from the picture that on the eve of the Civil War nearly all land granted by the federal government was through cash sales or military warrants. Starting in the 1860s, the Homestead Act became an important tool by which the federal government transferred away the land in the public domain. Cash sales continued after the Homestead Act but their prevalence was halved. Military warrants dwindle by the 1880s, as soon as rail road grants became the second most important mechanism the federal government used to transfer away land. The Homestead Act peaked in the 1890s and by the end of the 1920s it was no longer significant.[5]

This section has shown that the history of land policy in the United States during the nineteenth century was dominated by the fundamental debate of how to organise society in the western states. Settlement was encouraged, but certain principles held back full scale expansion such as the revenue principle and the issue of removing Native American tribes from federal land. The federal government was also concerned with using land to pay for the pensions of veterans from the several expansionist wars the United States incurred in during this period. The Homestead Act was the culmination of a series of attempts to establish a long term coherent policy of settlement in the west, which had been debated and contested up to the eve of the Civil War.

1.4 Sources and Dataset Construction

In this section I describe the sources and methods used to construct the dataset I use to estimate the impact of the Homestead Act. First I describe the data sources and then I explain various methodological issues. After, I explain the procedure to homogenise county borders across time. Then, I show how I construct the land in-

[5]Not included in the graph because of the lack of data of other covariates used in the subsequent analysis are the acres granted in counties that did not exist in 1860. This is a major limitation of this study because it is in those counties that most of the Homestead Acres were distributed. See section 1.5 for more details.

equality and human capital variables. Finally I explain how I identify the treatment of the Homestead Act.

1.4.1 Sources

In this paper I combine three sources to construct a panel dataset for US counties between 1850 and 1930. US Census data are obtained from both the IPUMS-USA[6] (Ruggles et al., 2015) and NHGIS[7] (Minnesota Population Center, 2016) projects at the Minnesota Population Center. The NHGIS data also contains aggregates from the US Agricultural Census between 1850 and 1950, as well as most demographic aggregates. However, variables that are age group specific, such as school attendance by age, can only be obtained from the individual level data in the IPUMS-USA project. The IPUMS-USA data are drawn from a sample of the microfilm records for the different US Censuses (I use the 1% sample for the years 1850 to 1930). I aggregate the necessary measures to the county level using the person-specific weights provided by IPUMS.

Data on the Homestead Act are obtained from the General Land Office records at the Bureau of Land Management (Bureau of Land Management, 2017). This source contains the universe of land patents granted or recognised by the Federal government of the United States. The data contains information regarding the location, size, and the names of the beneficiaries of a land patent, as well as a codification of the authority under which the patent was granted. Thus, land granted through the authority of the Homestead Act is directly observable.

1.4.2 Dataset Construction

County Boundaries

County boundaries change over time, specially for the western states during the nineteenth century. To construct a stable unit of analysis, I use ESRI's ArcGIS 10.3 software to calculate the intersection between counties arising from boundary changes or the creation of new counties. I then obtain the percent of land in the intersection corresponding to the new county boundaries, discarding fragments less than 1 km^2. Using this information, I re-weight all the count data in years other than 1860 to the 1860 boundaries using this procedure. This is a similar procedure used in other papers that use data from the same time period (see Hornbeck (2010); Donaldson and Hornbeck (2016); Hornbeck and Naidu (2012)).

[6] Integrated Public Use Microdata Series for the United States of America.
[7] National Historical Geographic Information System

To illustrate the above procedure, consider the county of Desha, in Arkansas. Between 1860 and 1870, this county ceded an area of 423 square kilometres to the county of Drew (also in Arkansas). The surface of Drew county increased to a total of 2,890 square kilometres, so the new addition represented approximately 14% of Drew county in 1870. The population in Drew county in 1870 is recorded at 9,960, and in Desha county it was recorded as 6,125. Thus, following the above procedure, the population of Drew county in 1870 using 1860 boundaries is reweighted to be 86% of 9,960 (8565.6 persons), while in Desha county it is reweighted to be 14% of 9,960 plus the 6,125 recorded in 1870 (7519.4 persons). Desha county did not transfer or receive land from any other county between 1860 and 1870.

The implicit assumption with this re-weighting procedure is that population is uniformly distributed within a county. Even though this assumption can seem to be very strong, we should be concerned if the re-weighting procedure is somehow correlated with the Homestead Act. One cause for concern is that counties exposed to the Homestead Act are, in general, relatively younger counties with less stable borders (i.e. frontier counties). I thus control for this re-weighting procedure by introducing a variable in all regressions with the number of *pieces* that make up the re-weighting procedure (e.g., in the example above, the value in 1870 for Desha county would be two and for Drew county one).

Land Inequality

I measure land inequality in a given county in an specific census year by calculating a land Gini coefficient. To do this I use the land distribution variables in the NHGIS data. These contain the frequency of farms that belong to a specific size category. These categories vary by census year so I standardise by aggregating to a common definition across time, in a procedure illustrated in figure 1.3.

The 1860 census truncates the distribution at 3 acres, and the lowest size category in the 1890 census aggregates the intervals 0 to 2 and 3 to 9. Moreover, the 1900 census has as its lowest category the interval 1 to 2 acres. Given that 1860 is the earliest census year for which there is data available on the distribution of farm sizes, and that 1860 is the only *pre-treatment* year, I decided to standardise the size categories starting from three acres. There are thus seven homogenised size categories: 3 to 9 acres, ten tonineteen 20 to 49, 50 to 99, 100 to 499, 500 to 999, and 1000 and more.

The land Gini coefficient is calculated in the standard way, constructing a Lorentz curve from the land size distribution. Following Cowell (1991) and Vollrath (2007, 2013), let f_i and a_i denote the share of farms and land in farms in category i,

Figure 1.3: Homogenisation of Farm Size categories.

Year			I	II	III	IV	V			VI	VII	
1860			3 9	10 19	20 49	50 99	100 499			500 999	1000+	
1870	0 2		3 9	10 19	20 49	50 99	100 499			500 999	1000+	
1880	0 2		3 9	10 19	20 49	50 99	100 499			500 999	1000+	
1890	0 9			10 19	20 49	50 99	100 499			500 999	1000+	
1900	1 2		3 9	10 19	20 49	50 99	100 174	175 259	260 499	500 999	1000+	
1910	0 2		3 9	10 19	20 49	50 99	100 174	175 259	260 499	500 999	1000+	
1920	0 2		3 9	10 19	20 49	50 99	100 174	175 259	260 499	500 999	1000+	
1930	0 2		3 9	10 19	20 49	50 99	100 174	175 259	260 499	500 999	1000 4999	5000+

Note: This diagram illustrates the standardisation procedure for the farm size categories in the various censuses. The columns labelled I-VII denote the standardised categories: 3 to 9 acres, 10 to 19, 20 to 49, 50 to 99, 100 to 499, 500 to 999, and 1000 and more.

respectively. Also, denote $F_i = \sum_{j=0}^{i} f_i$ the cumulative share of farms in categories up to size category i, and $A_i = \sum_{j=0}^{i} a_i$ the same for land in farms. It can be shown that the Gini coefficient corresponds to:

$$G = 1 - \sum_{i=1}^{N-1} (F_{i+1} - F_i) \cdot (A_{i+1} + A_i)$$

The census tables do not contain data on the number of acres in each size category. I thus use the mid point value for categories I to VI, and for category VII, I top code it at 1000. This clearly censors the distribution and thus inequality is underestimated using this procedure. This could potentially be an issue if the allocation of Homestead Act patents was correlated with extent of the censoring of the distribution.

Another issue with using the land Gini coefficient as a measure of inequality is that it measures land inequality amongst farms only. This is problematic for two reasons. First, it assumes that each farm has only one owner. Second, it does not include the landless population. Thus, farmland Gini coefficients will understate true wealth inequality. To address this issue I proxy the number of landless households by taking the number of adult males and subtracting the total number of farms, in a similar way to Vollrath (2013). This becomes the category with "zero" land in the land distribution (i.e. $a_i = 0$).

Human Capital Variables

The IPUMS-USA project contains data at the individual level for a 1% sample of the censuses between 1850 and 1940.[8] I use data on whether a person is currently

[8]For some years (e.g., 1850 and 1880), the full count of data is available. However, to maintain consistency I use the 1% sample for these years as well. For the census years 1940 to the present

attending school and if she/he is literate.

The literacy rate variable is constrained to adults of twenty years or older. This is because the literacy question was not asked to people younger that 20 in the censuses of 1850 and 1860. I also break down the school enrolment variable by age groups: seven to thirteen year olds (a proxy for primary school years) and fourteen to nineteen year olds (a proxy for secondary school years). Breakdowns by age are possible for any age group, but due to low population density and sampling some county/year/age combinations can be empty and that would drop the county out of the sample. By aggregating to these broad age categories I can mitigate the sampling issue.

A further breakdown of the human capital variables is by place of birth. The IPUMS samples include a variable that identifies the state or country of birth, but not the county. I thus create a dichotomous variable that equals one if the person was born in the same state of residence, and zero otherwise. Naturally, this variable does not capture intra-state migration such as rural/urban or high-density to low-density agricultural flows, but it is a good proxy to capture inter-state and inter-country flows.

Homestead Act and other Land Patents

The Bureau of Land Management of the United States holds digitised images and data on land titles transferred from the Federal government to individuals. These records correspond to the title transfers previously under the General Land Office. The data is structured into several files for each state. One file is the Patent database, which identifies the documents, and holds the total acreage granted by a particular patent. The patent documents contain land descriptions in terms of the Public Lands Survey System (PLSS). Each piece of land described in a Patent is an observation in the Land Description database, which can then be joined with a county using GIS methods.

Each land patent was granted by some title authority, and this is coded in the data. I group the title authorities based on their broad definitions and the total share of land granted by the Federal Government. These categories are: (1) Homestead Act of 1862, (2) other homestead acts,[9] (3) cash sales (Act of 1820), (4) land granted to rail road companies, (5) scrip warrants for military service, (6) grants to states, (7) indian allotments, (8) mineral patents, and (9) a miscellaneous

the county of residence of an individual is not available.

[9]See section 1.4.2 for details. However, the important thing to note at this point is that the Homestead Act of 1862 can be clearly identified in the BLM-GLO data with the authority code identifier `251101`.

category. Categories (2) and (4) to (9) include various acts and treaties within each category, while categories (1) and (3) are specific acts.

Due to the fact that the Homestead Act required individuals to reside in their claim for five years before obtaining the land patent, I subtract five years from a Homestead Act patent date to merge it with the census data. For instance, a patent granted in 1875 corresponds to a homesteader that moved to the location of the patent in 1870. Let c index counties and let d index census years. I denote $h_{c,d}$ as the stock of acres distributed through the Homestead Act up to census year d, in county c. That is, I add the land in land patents corresponding to Homestead Act up to year $d+5$.[10] Therefore, the stock of Homestead acres as a percent of county area in county c in decade d is given by equation (1.1).

$$h_{c,d} = \frac{\sum_{\tau=1862}^{d+5} \text{Homestead Acres}_{c,\tau}}{\text{County Area}_c}. \tag{1.1}$$

Similarly, the flow of Homestead Act acres as a percent of county area distributed in decade d in county c is defined as $\Delta h_{c,d}$, which is given explicitly in equation (1.2).

$$\begin{aligned} \Delta h_{c,d} &= \frac{\sum_{\tau=1862}^{d+5} \text{Homestead Acres}_{c,\tau} - \sum_{\tau=1862}^{d-5} \text{Homestead Acres}_{c,\tau}}{\text{County Area}_c} \\ &= h_{c,d} - h_{c,d-10} \end{aligned} \tag{1.2}$$

An important issue to point out is that the definition of stock that I use corresponds to the cumulative amount of acres distributed through a particular patent authority up to a certain year. This means that the stock variable does not account for land sales after the patent has been granted. The immediate implication is that the stock variables can only increase, and thus the flow variables are also always positive.

To take into account possible non-linear effects without over-parametrising the models, and to simplify the interpretation of coefficients, I split the continuous treatment into four categories: zero, low, medium, and high. The low, medium, and high categories correspond to the three terciles for which $h_{c,d}$ is strictly positive. Henceforth, denote $T_{c,d}$ as the treatment level category of county c for decade d, and denote $h_{c,d}^{(p)}$ the p-th percentile of $h_{c,d}$ for $h_{c,d} > 0$. Then,

[10] Given that the Homestead Act was signed into law in 1862, the first land patents began to be granted no earlier than 1867. Thus, the stock of land *distributed* by the Homestead Act in 1870 —the first census year of the post-treatment period— is equal to the flow for 1870, and contains the amount of land in land patents *granted* up to year 1875.

$$T_{c,d} = \begin{cases} 0 & \text{if } h_{c,d} = 0 \\ 1 & \text{if } h_{c,d} > 0 \text{ and } h_{c,d} \leq h_{c,d}^{(33)} \\ 2 & \text{if } h_{c,d} > h_{c,d}^{(33)} \text{ and } h_{c,d} \leq h_{c,d}^{(66)} \\ 3 & \text{if } h_{c,d} > h_{c,d}^{(66)}. \end{cases} \tag{1.3}$$

Thus, $T_{c,d} = 0$ corresponds to the control group (i.e. no Homestead Act acres distributed up to decade d), $T_{c,d} = 1$ is the first tercile of treatment, $T_{c,d} = 2$ is the second, and $T_{c,d} = 3$ is the third. By construction, the values of the percentiles that are the cut-off in the categorization of $h_{c,d}$ vary with time as the stock of Homestead acres grows, so it is possible that some counties move *down* in the ranking as the treatment window progresses.

Throughout the estimated models, except for those in section 1.7.4, the treatment categories are going to be based on the stocks up to the decade of 1930 (i.e. 1926 to 1935, according to the formula in (1.1)). So the main treatment variable, $T_{c,1930}$, will be given by:

$$T_{c,1930} = \begin{cases} 0 & \text{if } h_{c,1930} = 0 \\ 1 & \text{if } h_{c,1930} > 0 \text{ and } h_{c,1930} \leq h_{c,1930}^{(33)} \\ 2 & \text{if } h_{c,d} > h_{c,1930}^{(33)} \text{ and } h_{c,1930} \leq h_{c,1930}^{(66)} \\ 3 & \text{if } h_{c,1930} > h_{c,1930}^{(66)}. \end{cases} \tag{1.4}$$

where the stock of Homestead Act acres in decade 1930, $h_{c,1930}$, is defined as:

$$h_{c,1930} = \frac{\sum_{\tau=1862}^{1935} \text{Homestead Acres}_{c,\tau}}{\text{County Area}_c}. \tag{1.5}$$

1.5 Sample and Summary Statistics

1.5.1 Sample Definition

In 1860 large swaths of the continental United States were still very sparsely populated. As such, the censuses did not cover all areas that were later to become states of the Union. There were both organised and un-organised territories in these parts, and as such some territories had a few large counties or jurisdictions that were later to break up into smaller states or counties. County boundaries between existing counties also changed significantly throughout this period, so I standardise the data

using 1860 boundaries. I thus drop from my estimation sample jurisdictions that eventually were to break up into an extreme number of pieces by 1930.[11]

Table 1.1: Summary Statistics of Dependent Variables, by Year and Sample

	Land Inequality			School Enrolment			Literacy		
	Un-balanced	Balanced	T-Test	Un-balanced	Balanced	T-Test	Un-balanced	Balanced	T-Test
1850				1697	1557		1534	1324	
				0.502	0.505	-0.003	0.796	0.821	-0.025
				(0.292)	(0.286)	(0.010)	(0.271)	(0.232)	(0.009)
1860	1944	1935		1730	1557		1555	1324	
	0.832	0.832	0.000	0.561	0.567	-0.006	0.875	0.870	0.005
	(0.079)	(0.079)	(0.003)	(0.273)	(0.255)	(0.009)	(0.184)	(0.178)	(0.007)
1870	2058	1935		1925	1557		1728	1324	
	0.803	0.800	0.003	0.496	0.471	0.025	0.829	0.810	0.019
	(0.098)	(0.096)	(0.003)	(0.293)	(0.274)	(0.010)	(0.210)	(0.200)	(0.007)
1880	2097	1935		2049	1557		1996	1324	
	0.751	0.752	-0.001	0.539	0.526	0.013	0.867	0.841	0.026
	(0.112)	(0.106)	(0.003)	(0.228)	(0.210)	(0.007)	(0.164)	(0.157)	(0.006)
1900	2100	1935		2095	1557		2095	1324	
	0.764	0.764	-0.001	0.607	0.591	0.016	0.912	0.888	0.024
	(0.106)	(0.104)	(0.003)	(0.152)	(0.142)	(0.005)	(0.110)	(0.112)	(0.004)
1910	2100	1935		2097	1557		2096	1324	
	0.775	0.776	-0.001	0.789	0.782	0.007	0.938	0.921	0.017
	(0.109)	(0.107)	(0.003)	(0.106)	(0.103)	(0.004)	(0.080)	(0.085)	(0.003)
1920	2100	1935		2097	1557		2100	1324	
	0.782	0.783	-0.001	0.803	0.798	0.004	0.955	0.943	0.013
	(0.110)	(0.108)	(0.003)	(0.097)	(0.092)	(0.003)	(0.062)	(0.067)	(0.002)
1930	2098	1935		2098	1557		2100	1324	
	0.800	0.801	-0.001	0.778	0.774	0.004	0.966	0.958	0.008
	(0.110)	(0.108)	(0.003)	(0.096)	(0.090)	(0.003)	(0.052)	(0.055)	(0.002)

Notes: For each variable, the first two columns show the number of observations, the mean, and standard deviation (in parenthesis) by year. The third column labelled "T-Test" shows the difference in means and the standard error (in parenthesis) of that difference between the unbalanced and balanced samples.

Besides dropping the "extremely" subdivided jurisdictions, in the reduced form estimation of the effect of the Homestead Act I estimate the models using balanced panels. In the case of the land inequality, given that the land distribution variables are only available from 1860 onwards, this means that a county is included in the sample if it has valid data for all decades from 1860 to 1930, excluding 1890 because the distribution categories are not conformable (see figure 1.3). For school enrolment and literacy, an observation is included if there is data for all years between 1850 and 1930, excluding 1890.

Table 1.1 shows the number of observations, means, and standard deviations (in parenthesis) of the dependent variables by census year and sample type (unbalanced and balanced), as well as t-tests of the difference in means by year. There are 2,126 jurisdictions available in the NHGIS shapefiles for 1860, of which 26 are dropped because they split into too many subdivisions. Under the "Unbalanced"

[11] I define "extreme" based on the number of subdivisions by 1930 of an 1860 jurisdiction. The threshold I use is the 99th percentile, which in the data corresponds to ten subdivisions. That is, any jurisdiction in 1860 that split into ten or more subdivisions by 1930 is dropped from the sample.

column for each variable in table 1.1, the first number for each year corresponds to the number of observations for which there is non-missing data. Land inequality is the variable that is least affected by missing values, with less than 8 percent missing values in the worst year (1860). Land inequality is the least affected by the balancing procedure as well, with a loss of only eleven counties.

School enrolment is significantly more affected by missing values. This is because I restrict the denominator in the school enrolment rate to white children, ages seven to nineteen, born in the same state of residence. Given that the second half of the nineteenth century saw large migration flows towards the western states, many frontier counties had few children born in the same state. This in turn leads to a considerable loss of observations. Moreover, the sampling procedure used by IPUMS-USA means that it is not uncommon for counties with low population density to not register children in the desired age range, and thus the denominator of the school enrolment rate is zero.

Literacy rates are the most affected by both sources of sample attrition. First, the unbalanced sample is considerably smaller than the corresponding one for land inequality (approximately a quarter less observations in 1860). This is mostly due to the fact that the literacy rate is calculated for individuals 20 years or older and born in the same state of residence. In the early decades of the sample period the population density of the frontier counties was very low, so the number of adults born in the same state of residence was virtually nil for those states and counties.

The t-tests in table 1.1 show that the panel balancing has no significant effect on land inequality in any year. Similarly, school enrolment is mostly unaffected except for the years 1870, 1880, and 1900, when the unbalanced mean was significantly greater than the balanced mean. Literacy, however, is significantly different between the unbalanced and balanced samples, with most years showing a statistically significant difference in means. This difference is positive between the unbalanced and balanced samples for all years except 1850.

The bounds by category of treatment for 1930 and other summary statistics are shown in table 1.2. The low treatment category is quite similar to the control, with a maximum treatment of only 1.4 percent of county area, and an average of 0.3 percent. Medium treatment averages at 6.6 percent of county area with a maximum of 12.3 percent. The high treatment category averages a considerable 25.3 percent of county area with a maximum of roughly three quarters of county area.

Table 1.2: Summary Statistics of the Stock of Homestead Acres in 1930, by Treatment Level in 1930

	Obs.	Mean	Std. Dev.	Min.	Max.
Control	1291	0.000	0.000	0.000	0.000
Low	279	0.003	0.003	0.000	0.014
Med.	278	0.066	0.031	0.014	0.123
High	278	0.253	0.120	0.124	0.764
Total	2126	0.042	0.096	0.000	0.764

Note: This table shows summary statistics of the stock of acres distributed through the Homestead Act by level of treatment in 1930. The observation unit is a county using 1860 borders.

1.5.2 Spatial Distribution of Treatment and Dependent Variables

Figure 1.4 shows the spatial distribution of the different treatment intensity categories for the stock of Homestead acres by 1930. It is clear from the figure that the Homestead Act was mainly used in frontier states and in the Pacific states. However, there are some exceptions: there is a large concentration of Homestead Act usage in northern Alabama and southern Mississippi and Alabama, as well as north-western Arkansas.[12]

Figure 1.5 shows the change in land inequality between 1860 and 1930. The strongest increases of land inequality are concentrated in the north-eastern and eastern-mid-western states, as well as along the Appalachian counties. In contrast, the strongest decreases in land inequality are observed in south-eastern Arkansas, north-eastern Louisiana, and northern Mississippi.

Figure 1.6 shows that school enrolment increased in most of the country, but the increases were mostly in the south. Literacy increases are also concentrated in the south as shown in figure 1.7. However, certain parts of the south experienced relatively large drops in literacy rates, such as southern Arkansas.

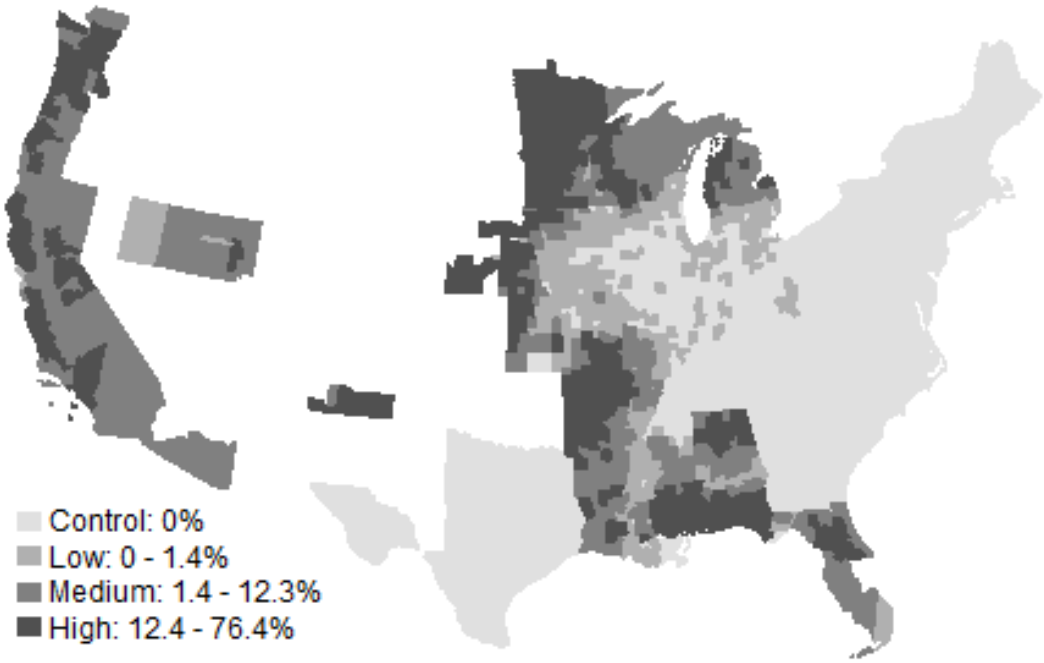
The maps of this section show that there is clearly convergence between the north and the south in terms of the two human capital variables. I will explore the effect that convergence has on the estimates of the effects of the Homestead Act in section 1.7.2.

1.6 Empirical Framework

This section presents the empirical framework of this paper. First, I describe the baseline model used to estimate the impact of the Homestead Act on the outcome

[12]The map also shows which countries pass the "extreme" subdivision filter described in footnote 11. Most of the Great Plains and Rocky Mountain states are dropped due to this filter.

Figure 1.4: Percent of County Area Distributed through the Homestead Act up to 1930, categorised



variables of land inequality, school enrolment, and literacy. Then I describe the model with added controls and the series of robustness checks I carry out to ensure the causal interpretation of the results. Finally I describe the instrumental variables procedure aimed at estimating the effect of land inequality on school enrolment.

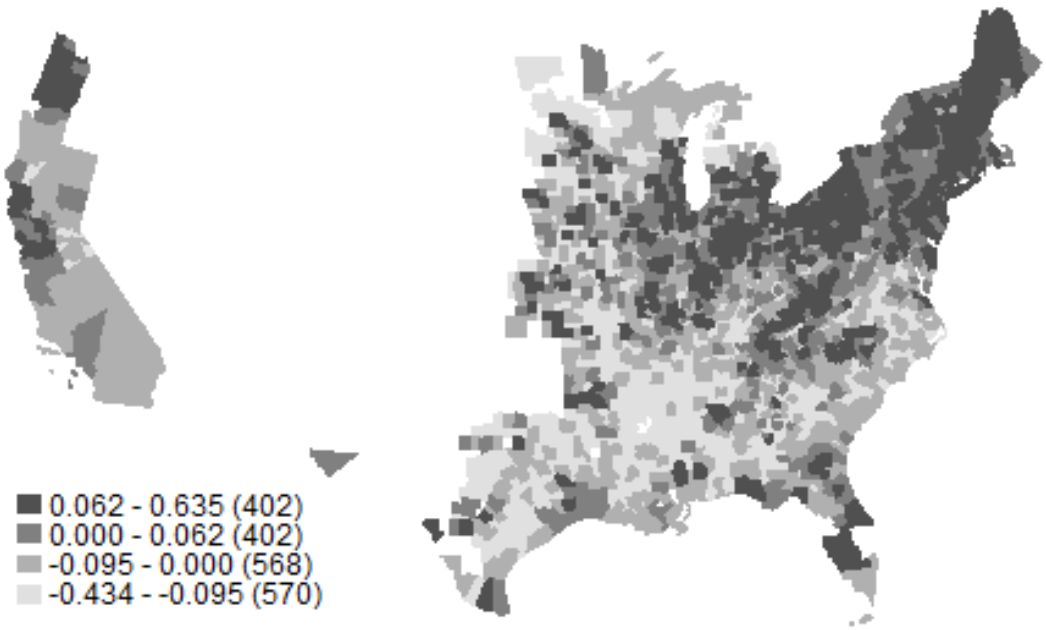
1.6.1 Baseline Model

Let $y_{c,s,t}$ denote outcome y in county c, state s, in year t, then the baseline model in equation (1.6) regresses an outcome against county fixed effects α_c, state-specific year fixed effects $\lambda_{s,t}$, and the interaction of the treatment level in 1930 ($T_{c,1930}$) with year fixed effects.

$$y_{c,s,t} = \alpha_c + \lambda_{s,t} + \sum_{j=1}^{3} \beta_{j,t,1930} \cdot I\left[T_{c,1930} = j\right] + \varepsilon_{c,s,t} \tag{1.6}$$

where $\varepsilon_{c,s,t}$ is an error term that is allowed have arbitrary heteroskedasticity and autocorrelation within states, but is distributed independently across states. For

Figure 1.5: Change in Land Inequality, 1860 - 1930

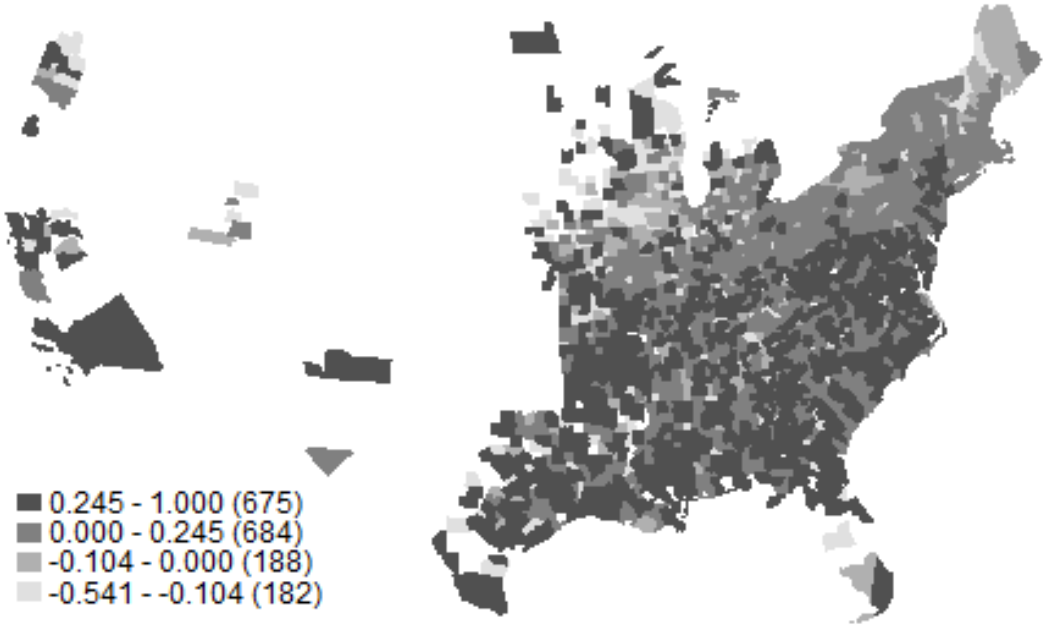


land inequality the range of t is from 1860 to 1930, excluding 1890, and for school enrolment and literacy the range of t is 1850 to 1930, excluding 1890.

Estimation of equation (1.6) is carried out with standard panel fixed effects regression clustering standard errors at the state level. The coefficients of interest are the $\beta_{j,t,1930}$'s which denote the difference-in-difference estimates of treatment with respect to control (i.e. the excluded category is $T_{c,1930} = 0$). j corresponds to a given tercile of treatment, hence $j \in \{1, 2, 3\}$. For instance, $\beta_{3,1880,1930}$ corresponds to the effect of a high intensity of treatment ($j = 3$) on the outcome y in 1880, when intensity of treatment is defined by the stock of land distributed through the Homestead Act up to the census year 1930.

The pre-treatment and post-treatment periods are defined as follows. For land inequality, the pre-treatment period only includes 1860, while for the human capital variables, the pre-treatment periods are 1850 and 1860. The post-treatment years are 1870, 1880, 1900, 1910, 1920, and 1930, for all outcome variables. The year 1890 is not included because the census data is not available.

Figure 1.6: Change in School Enrolment of White Young People (ages seven to 19), born in the Same State of Residence, 1860 - 1930



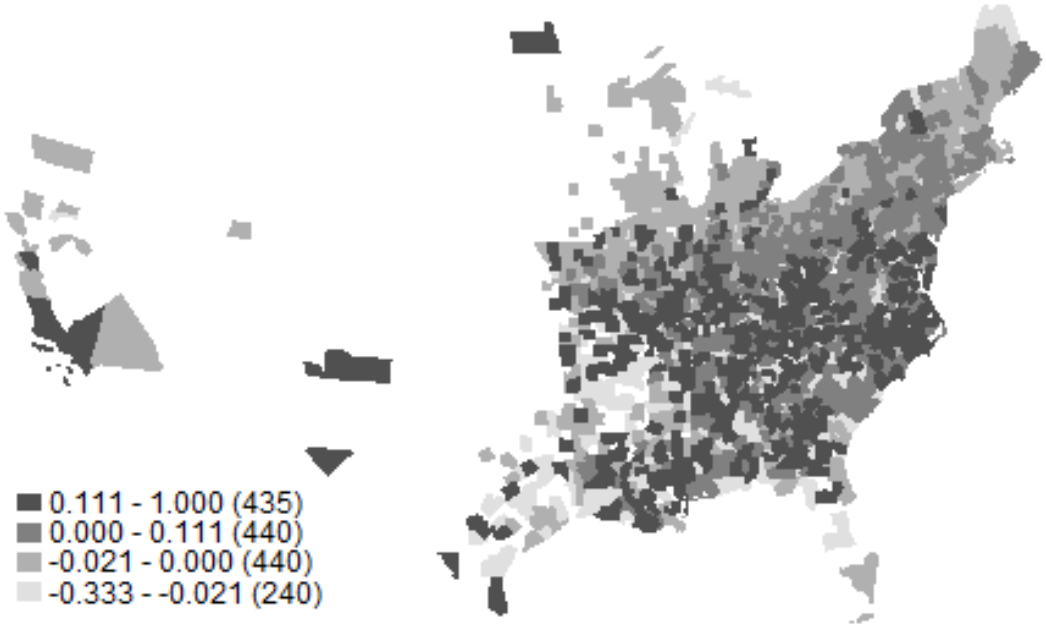
1.6.2 Additional Controls and Robustness Checks

To check the robustness of the baseline model, I add a series of controls measured at their level in 1860 and interacting that with the time fixed effects. Thus, the estimating equation in these expanded models can be expressed as equation (1.7).

$$y_{c,s,t} = \alpha_c + \lambda_{s,t} + \sum_{j=1}^{3} \beta_{j,t,1930} \cdot I\left[T_{c,1930} = j\right] + \sum_{k=1}^{K} \gamma_t \cdot z_{c,1860} + \varepsilon_{c,s,t} \tag{1.7}$$

The intuition behind estimating this series of models is that even though the baseline model in (1.6) is identified when the parallel trends assumption is met, there could be a confounding effect of shocks during the post-treatment period that interact with baseline characteristics that are correlated with the use of the Homestead Act. For instance, if price shocks to certain crops affect land distribution as is argued in Galor et al. (2009), and land suitability for that crop is correlated with the use of the Homestead Act, then the effect of these price shocks during the 1860 - 1930 period could be misattributed to the Homestead Act in the baseline model.

Figure 1.7: Change in Literacy of White Adults (age 20+), born in the Same State of Residence, 1860 - 1930



The treatment in the baseline model in equation (1.6) includes the stock of Homestead acres up to 1930, but I include the outcomes for *intermediate years* within the treatment window 1862 to 1930. Thus, the estimated effect of the treatment for an intermediate year could be the results of reverse causality. To rule out intermediate effects I estimate a series of differences-in-differences models with a moving treatment window. In each regression the pre-treatment period includes all census years with available data up to 1860. The post-treatment period includes only a given census year after 1860.

1.6.3 IV model

To obtain an estimate of the causal effect of land inequality on school enrolment, I estimate an instrumental variables model where the instruments are the interaction of the intensity categories of the Homestead Act with indicator variables for the post-treatment years. The baseline model, without additional controls, is given by

the following two equations,[13]

$$G_{c,s,t} = \theta_c + \tau_{s,t} + \sum_{j=1}^{3} \gamma_{j,t,1930} \cdot I\left[T_{c,1930} = j\right] + \eta_{c,s,t} \tag{1.8}$$

$$S_{c,s,t} = \mu_c + \varphi_{s,t} + \beta \cdot G_{c,s,t} + \epsilon_{c,s,t} \tag{1.9}$$

where $G_{c,s,t}$ and $S_{c,s,t}$ respectively denote land inequality (Gini coefficient) and the school enrolment rate in county c, in state s, in year t. θ_c and μ_c are county-specific fixed effects; $\tau_{s,t}$ and $\varphi_{s,t}$ denote state-specific year fixed effects; $T_{c,1930} \in \{1, 2, 3\}$ denotes the intensity level of treatment (low, medium, or high) measured from the stock of Homestead Act acres distributed up to 1930, and $\gamma_{j,t,1930}$ denotes the differences-in-differences effect of treatment intensity on land inequality. The coefficient of interest is β, which denotes the effect of land inequality on school enrolment. Finally, $\eta_{c,s,t}$ and $\epsilon_{c,s,t}$ are the error terms, which are assumed to be independent across states (dimension s) and as such the estimation procedure clusters standard errors at the state level. The endogeneity in equation (1.9) arises because land inequality $G_{c,s,t}$ can be correlated with the unobserved component $\varepsilon_{c,s,t}$ even after controlling for county fixed effects μ_c and state-specific time fixed effects $\varphi_{s,t}$.

Identification rests on the assumption of parallel trends in the differences-in-differences estimations described in the previous sub-sections, and on the IV assumptions of relevance and exogeneity. The relevance assumption is graphically tested in section 1.7.1 and it can be formally tested with a joint test of the excluded instruments in the first stage.[14]

The exogeneity assumption cannot be directly tested and requires that the Homestead Act did not affect school enrolment in any other way than through affecting land inequality. This assumption is strong in the context of the second half of the nineteenth century and early decades of the twentieth, but before dismissing the IV estimates it is important to understand how it could be violated.

One issue raised in the previous sections of the paper is that of migration and sorting. The Homestead Act could have attracted a certain type of settler, perhaps with a higher preference for education. However, sorting based on county characteristics that are fixed throughout the time period is controlled for by the inclusion of county fixed effects. Moreover, I restrict the sample in the calculation

[13] The model with controls is analogous to the ones in equations (1.8) and (1.9).

[14] In this setting, the small number of clusters (32) combined with the large number of instruments (3 levels $\times$ 6 census years = 18 instruments) leads to very few degrees of freedom. Moreover, the high correlation of treatment with state indicators (non PLSS states are always in the control group) leads to a first stage where the joint significance of the 18 instruments cannot be tested in the baseline model. See section 1.7.5 for more details on this issue.

of the school enrolment rate to white children *born in the same state of residence*, and thus avoid inter-state migration as a possible confounding factor.

Another issue is that the Homestead Act could have had an effect on property rights, and through that channel school enrolment increased. Although I do not test it directly given the lack of a good measure of enforcement of property rights, I include state-specific year fixed effects. These trends account for much of the variation in policies that could be affected by the Homestead Act.

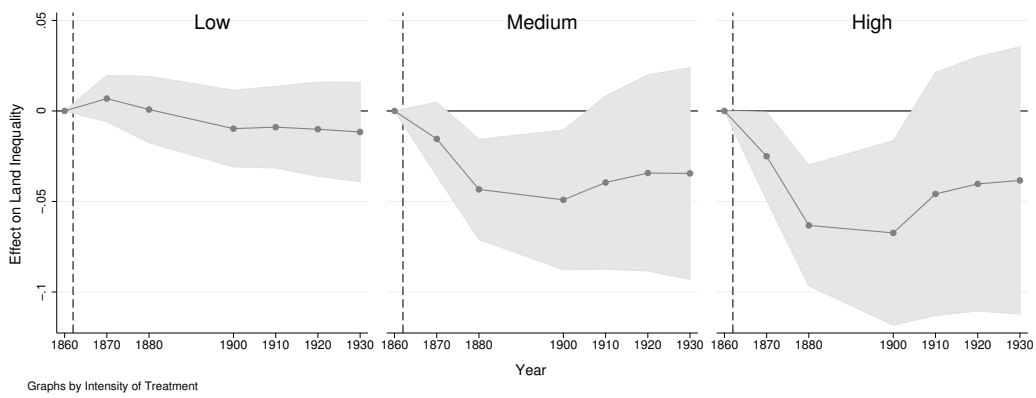
1.7 Results

This section presents the results of estimating the models described in the previous section 1.6. First, in sub-section 1.7.1, I present the reduced for results of the estimation of the effect of the Homestead Act on land inequality. Then, in sections 1.7.2 and 1.7.3, show the effects of the Homestead Act on school enrolment and literacy, respectively. In each sub-section I carry out robustness checks appropriate for each outcome.

1.7.1 The effect of the Homestead Act on Land Inequality

Baseline Model

Figure 1.8: Effect of the Homestead Act on Land Inequality, baseline model



Notes: The results come from the estimation of model (1.6) with a sample size of 1,935 counties. The dependent variable is the land Gini described in section 1.4.2. The three panels refer to the three categories of treatment, of which the summary statistics are shown in table 1.2. Shaded areas represent a 95% confidence interval based on standard errors clustered at the state level.

The results of applying the model in equation (1.6) to land inequality are shown in figure 1.8. The figure has three panels, with each panel plotting the coefficients representing the differences-in-differences estimates of the effect of the three

levels of intensity of treatment on the land Gini coefficient (defined in section 1.4.2). The grey areas in the plots represent 95% confidence intervals.

The first panel in figure 1.8 shows that there was no effect of low levels of intensity of the Homestead Act on land inequality. The second and third panels, for medium and high levels of intensity of treatment respectively, show that the Homestead Act reduced land inequality. The effect is strongest (and statistically significant) up until the turn of the century, and then the effects die out statistically. However, the effect is considerably negative even in 1930, albeit standard errors are larger as well. The magnitudes of the effects at their peak in 1900 are five Gini points for medium intensity treatment and approximately seven Gini points for high intensity treatment. These magnitudes are considerable given that the mean and standard deviation of the land Gini in 1860 were .83 and .08 respectively.

Land Inequality and Slavery

Table 1A.1 shows the results of estimating the model in equation (1.7) with the land Gini as the dependent variable, for different sets of controls. Column one in table 1A.1 is included for reference and is a model that only includes county and year fixed effects (i.e. it does not include state specific year fixed effects). Column two corresponds to the baseline model in equation (1.6), the coefficients of which are plotted in figure 1.8. Column three includes a battery of geographic controls: mean annual precipitation and temperature, natural logarithm of latitude and longitude, altitude, slope index, and thermal zones (percent of county area). Column four includes only economic and demographic controls: distance to largest population centre in the state in 1860, population density in 1860, percent of farm households in 1860, prevalence of slavery in 1860, stock of distributed land by the federal government in 1860, and presence of rail and water transport dichotomous variables from the 1860 census. Column five only includes the prevalence of slavery in 1860 as a control. Finally, column six includes all controls.

The first thing to note about the results in table 1A.1 is the stability of estimates before 1900 across specifications. The point estimates for all intensity levels are not statistically different from each other across specifications, and in the case of medium and high intensity treatments, the effects are all negative and statistically different from zero (with the exception of the 1880 coefficient in the no controls model for medium intensity treatment, which is negative but not statistically different from zero).

In the baseline model, the effect of the medium and high intensity Homestead Act becomes statistically insignificant after 1900, although the point estimates are

not statistically different from the estimates for 1880 and 1900. Qualitatively, nothing changes when I include geographic controls.[15] However, introducing economic controls, including slavery, increases the magnitude of the point estimates across years and decreases standard errors, and thus the treatment effects for high intensity treatment becomes statistically significant for the years 1910 to 1930. Column five shows that this change in magnitude and statistical significance is dominated by the inclusion of the slavery percent control (interacted with time fixed effects).

A coherent explanation for this result is that the percent of the population in slavery in 1860 is acting as a proxy for the presence of large plantations. In fact, a cross section regression of the percent of the county area distributed through the Homestead Act by 1930 on the percent of the population in slavery in 1860, controlling for state fixed effects, yields a point estimate of -0.117 and standard error of 0.060 (clustered at the state level: 34 clusters). Moreover, the point estimates for the slavery variable are strongly negative and significant beginning in the year 1900. Thus, a plausible conjecture is that the breakup of large plantations during the sample period confounds the effect of the Homestead Act.

To explore this hypothesis I estimate the effect of the Homestead Act and the slavery percent in 1860 on the share of farms in each farm size category of figure 1.3. That is, I estimate equations (1.6) and (1.7) with the share of farms in each size category as the dependent variable, and in the latter controlling for the slavery percent in 1860. The results of this exercise are shown in figure 1A.1. The first thing to note is that sub-figures 1A.1a to 1A.1e show that one channel of the effect on land distribution of the Homestead Act was through the consolidation of small farms. This result is robust to the inclusion of slavery in 1860 as a control. However, sub-figures 1A.1f and 1A.1g show that the effect of the Homestead Act on the top share of the distribution is not robust to the inclusion of slavery in 1860 as a control. This is consistent with the explanation that the secular breakup of the large plantations post Civil War and in particular in the early twentieth century confounds the effects of the Homestead Act in the baseline model.

1.7.2 The effect of the Homestead Act on School Enrolment

Baseline Model

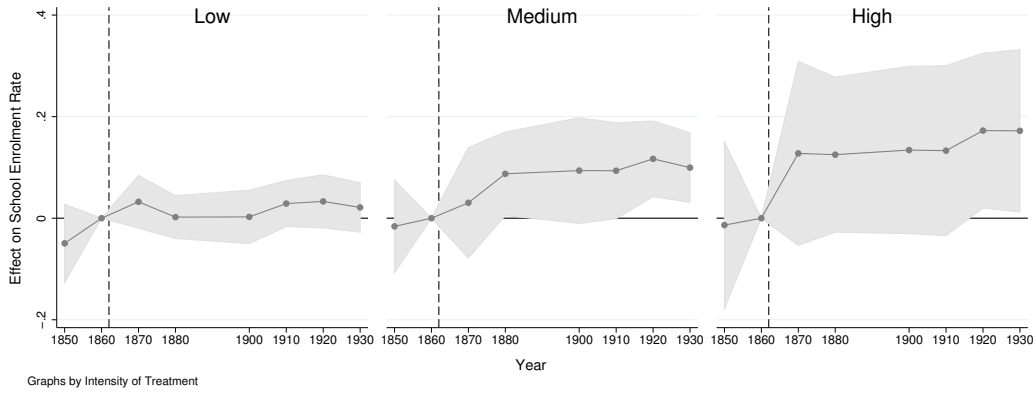
The effects of the Homestead Act on school enrolment, by intensity of treatment, are shown in figure 1.9. The school enrolment rate is calculated for white persons, ages

[15] I have also run the regressions introducing one control at a time and the conclusion is the same. I have grouped the specifications here for ease of exposition.

seven to nineteen, living in the same state they were born. This latter constraint on the sample is to indirectly control for sorting and migration across states.

The panels clearly show that the higher the intensity of treatment, the stronger the effect of the Homestead Act on school enrolment. For medium and high intensity of treatment, the effects are strong but become statistically significant only in the latter part of the study period, after the turn of the century. This coincides with the period known as the "high school movement" in the United States. It is interesting to note that the effects are relatively stable between 15 and 20 percentage points (the vertical axis units are shares). This is a considerable effect given that the mean and standard deviation of the school enrolment rate for this sub-population in 1860 were .57 and .25 respectively.

Figure 1.9: Effect of the Homestead Act on School Enrolment, baseline model



Notes: The results come from the estimation of model (1.6) with a sample size of 1,557 counties. The dependent variable is the share of white children ages seven to nineteen living in the same state that they were born in that are enrolled in school, described in section 1.4.2. The three panels refer to the three categories of treatment, of which the summary statistics are shown in table 1.2. Shaded areas represent a 95% confidence interval based on standard errors clustered at the state level.

Heterogeneous effects of the Homestead Act on School Enrolment

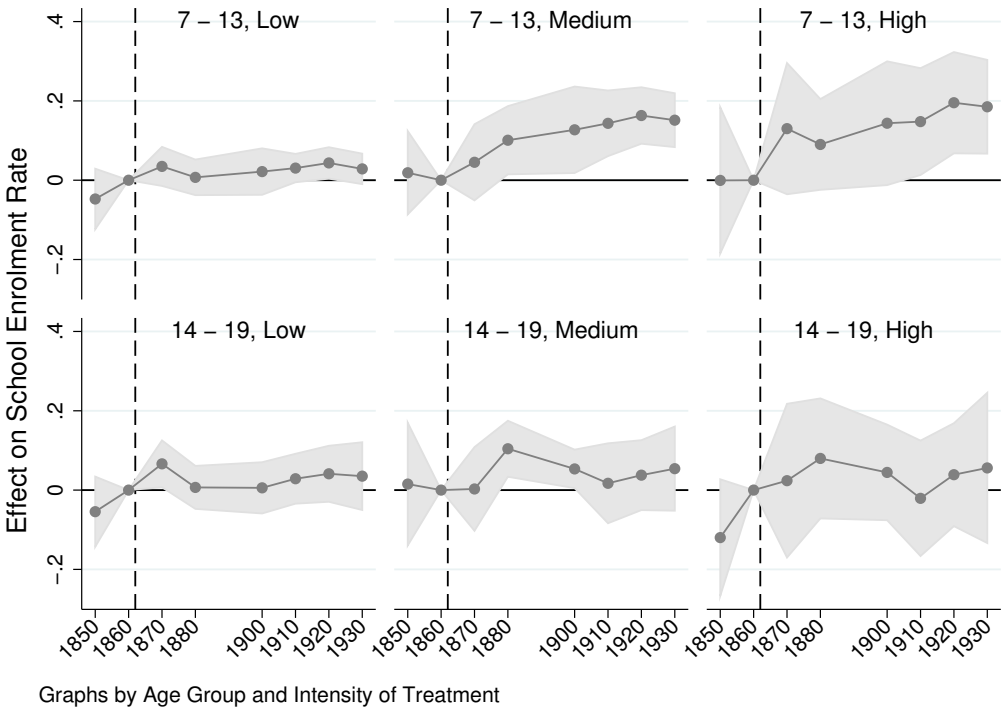
In this section I re-estimate the model in equation (1.6), but for different sub-populations depending on sex and age. The objective of this exercise is to determine heterogeneous treatment effects, which can shed light on the mechanisms by which the Homestead Act affected school enrolment.

Looking at sub-populations can drastically reduce sample size because of the sampling procedure in the digitisation of the census records by IPUMS. Thus, when breaking down the sample by sex, I maintain a constant age range (seven to nineteen years old), and when breaking down by age group I pool across sexes. The aim of

this exercise is to detect whether the Homestead Act had differential impacts across these dimensions. In the case of age group, I find that the bulk of the effect comes from the higher enrolment of children in primary school age, while in the case of sex I find no differences in effect between boys and girls.

Figure 1.10 shows the results of applying the same procedure as in figure 1.9, but splitting the sample by age groups seven to thirteen and fourteen to nineteen. The first row of panels contains the results of estimating the baseline model in equation (1.6) for the sample of white children ages seven to thirteen. The results are very similar to those in the pooled sample, but the point estimates are slightly higher and statistical significance is achieved earlier for the medium intensity treatment (first row, second column). In contrast, the second row shows that the Homestead Act had no significant effect on the school enrolment of children of secondary school age.

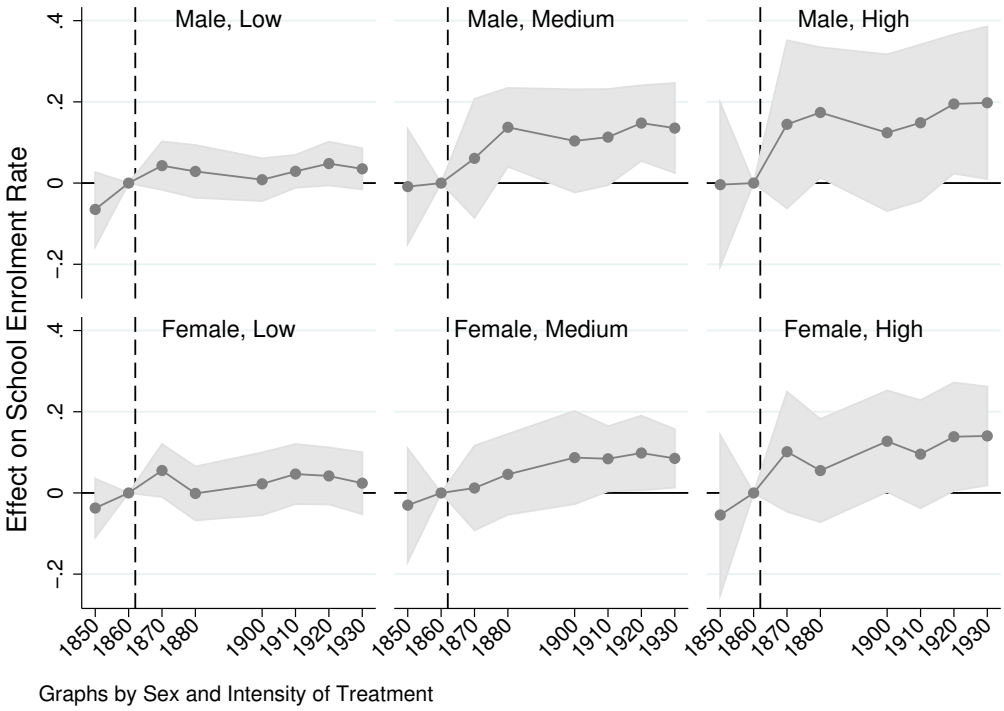
Figure 1.10: Effect of the Homestead Act on School Enrolment of White Children ages seven to 19, born in the same state of residence, by Age Group



Notes: The results come from the estimation of model (1.6) separately for the enrolment rate within children ages seven to thirteen and fourteen to nineteen, born in the same state of residence. The three panels refer to the three categories of treatment, of which the summary statistics are shown in table 1.2. Shaded areas represent a 95% confidence interval based on standard errors clustered at the state level.

I then look at the differential effects that the Homestead Act had on the school enrolment rate of girls and boys separately. Figure 1.11 shows the estimation of equation (1.6) by sex and intensity of treatment. The first row corresponds to the boys and the second row to girls. Although the point estimates suggest that the effect on boys was stronger than the effect on girls, the large standard errors indicate that this difference is not statistically significant.

Figure 1.11: Effect of the Homestead Act (stocks by 1930) on School Enrolment, by Sex



Notes: The results come from the estimation of model (1.6) separately by gender. The dependent variable is the share of white boys or girls ages seven to nineteen living in the same state that they were born in that are enrolled in school. The first three panels refer to the three categories of treatment applied to boys, while the second three panels are for girls. Shaded areas represent a 95% confidence interval based on standard errors clustered at the state level.

While there seems to be no significant differences of the effect of the Homestead Act on school enrolment by sex, the results when splitting the sample by age tell us that the reduced form effect of the Homestead Act on school enrolment is mainly given by the effect on primary school age children. This is relevant because, as Goldin and Katz (1999) argue, secondary education is much more specialised in contents that primary education. In that sense, one can interpret these results as the Homestead Act having an effect on the provision of a purely public good.

If, as Galor et al. (2009) argue, land inequality proxies the agricultural elite's bargaining power, and the industrial revolution pitted capitalists against agricultural elites, then the lower land inequality produced by the Homestead Act should unlock higher secondary school enrolment as well. However, Galor et al. (2009) do not use the land Gini as a the measure of inequality/agricultural elite's bargaining power. Instead they use a measure derived from the Gini that represents the percentage of land owned by the top twentieth percentile of landowners in 1880. Thus, my results do not necessarily contradict Galor et al. (2009), specially since, once controlling for the prevalence of slavery in 1860, the Homestead Act had no effect on the share of farms of 1000 acres or more (see figure 1A.1g). Nevertheless, if the Homestead Act did not affect the agricultural elite's bargaining power, then the effects that I do find on primary school enrolment would then be consistent with the mechanisms suggested by Goldin and Katz (1999) of homogeneity and cooperation.

School Enrolment and Convergence

Identification in a differences-in-differences setting relies on the assumption of parallel trends. With data from at least two census years before the Homestead Act was enacted, I have been able to test this assumption. Figure 1.9 clearly shows that for all levels of intensity of treatment, the coefficient for 1850 is not statistically significant. In particular, the break in trend between 1860 and 1870 of the point estimates for the high intensity treatment is a sign that the parallel trends assumption holds. However, for the lower intensity treatments this break in trend is not immediately clear. Specifically, for the low intensity treatment, even though the coefficient for 1850 is not statistically significant the point estimates suggest a pre-treatment trend is present.[16] Figures 1.10 and 1.11 also show that there seems to be a weak pre-treatment trend for low intensity treatment counties irrespective of age and sex. Moreover, figure 1.10 also shows a pre-treatment trend for high intensity treatment counties for secondary school age children, albeit not statistically significant.

These pre-treatment trends are important to understand because the most plausible alternative theory that could explain these effects is that the Homestead Act is acting as a proxy for a convergence effect. Homestead counties could be less developed (less land in private hands) and thus when privatisation occurs through the Homestead Act then convergence in human capital occurs. The state specific

[16] Unfortunately, due to the lack of data before 1850, it is impossible to have a clearer idea of the pre-treatment trend.

time fixed effects in the baseline model in equation (1.6) take away the cross-state convergence of human capital, but there could still be within-state convergence.

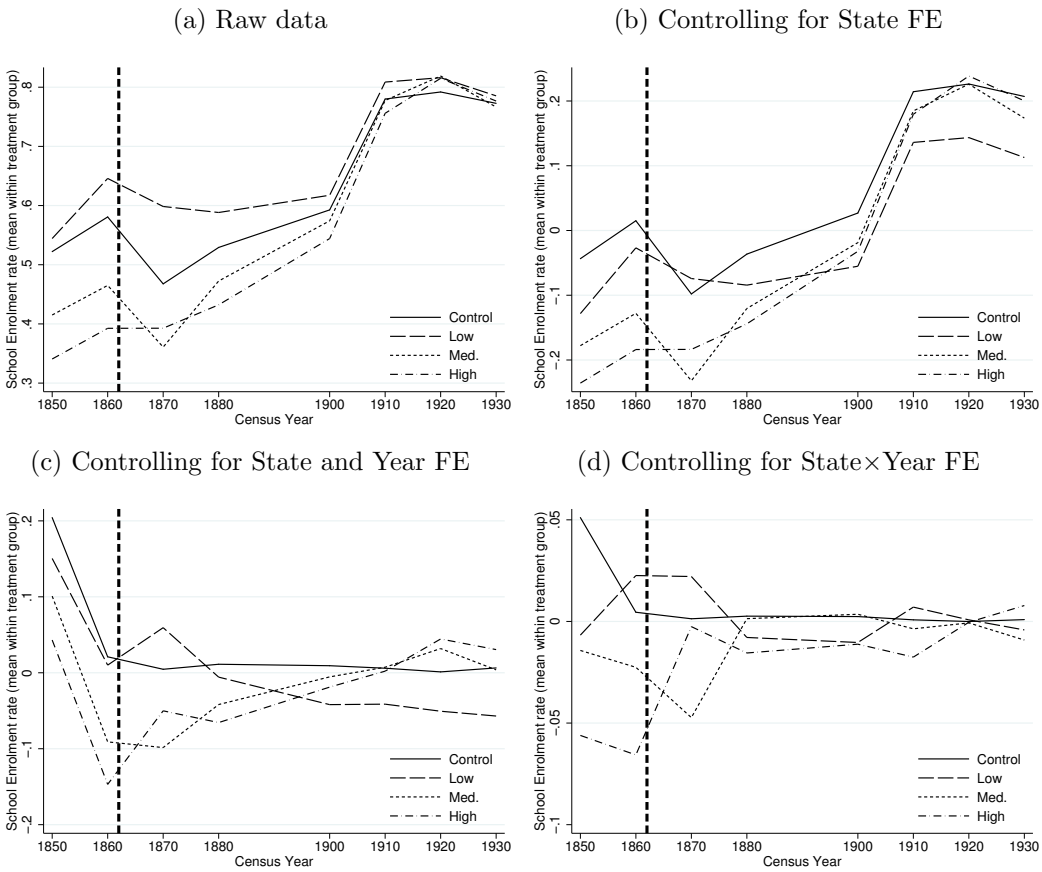
To account for within state convergence effects, I carry out four exercises. First, I compare the trends between treatment and control units before and after the implementation of the Homestead Act to show that the inclusion of the state-specific year fixed effects account for a sizeable amount of the convergence, but not all. Second, I estimate the baseline model including the level of school enrolment in 1850 and 1860 to show that even though the effect of the Homestead Act is indistinguishable from convergence after 1860, that is not the case when controlling for the level in 1850. Third, I estimate the model in equation (1.7) for two different sets of controls: geographic and economic (measured at base year 1860), to account for different characteristics that are correlated with the Homestead Act and that could drive convergence as well. Fourth, I re-estimate the baseline model replacing the state-specific year fixed effects with principal meridian-specific year fixed effects[17] and splitting the effect between northern and southern states, to determine what is driving the overall effect shown in figure 1.9.

Figure 1.12 shows the results of the first exercise. The means of school enrolment by treatment level and year are shown in figure 1.12a, and it is clear from the figure that prior to the enactment of the Homestead Act, there was no convergence between control counties and medium and high intensity counties, and actually divergence between control and low intensity counties. However, it is clear that after 1870 there is convergence across county groups. In figure 1.12b the vertical distance explained by differences across states fixed across time is controlled for, and convergence is present for medium and high intensity counties, but not for low intensity counties. Figures 1.12c and 1.12d show what happens when I control for state and year fixed effects in the former and state interacted with year fixed effects in the latter. As expected the overall increase in school enrolment is filtered out and convergence is still observed after 1860 but not in the pre-treatment period. Thus, to the extent that convergence is a confounding factor, these results imply that the Homestead Act either triggered the convergence in school enrolment or accelerated it.

In the second approach I re-estimate the model in equation (1.7), using the level of school enrolment en 1850 and 1860 as controls (first separately and then jointly). The results are shown in figure 1B.1. The first row of graphs corresponds to the treatment effects by intensity of treatment of the baseline model, which are

[17] Recall that the PLSS divides the public domain intro areas defined by the corresponding principal meridian. For reference, see the map in figure 1.1.

Figure 1.12: Means of School Enrolment by Treatment Level and Year



Note: Each graph in this figure plots the mean of school enrolment by treatment level and year, where school enrolment is the percentage of white children ages seven to nineteen in school that were born in the same state of residence. Panel 1.12a shows the means with the raw data, and the subsequent panels plot the mean of the residual of a regression on the corresponding fixed effects.

included as reference. The second row of graphs is the baseline model including the standardised level of the school enrolment in 1850 as a control interacted with year fixed effects. The treatment estimates are not affected by the inclusion of this control. The third row shows the results of including the standardised level of the 1860 school enrolment rate as a control interacted with year fixed effects. Clearly it can be seen that the treatment estimates of the Homestead Act all go to zero. Nevertheless, the fact that convergence nullifies the effects of the Homestead Act only after 1860 suggests that the Homestead Act acted as a sufficient condition for convergence.

Controlling for levels of the dependent variable in a differences-in-differences model may not present a clear picture when trying to disentangle convergence from treatment effects. For that reason I control for other covariates that might predict

convergence. Table 1B.1 shows the results of adding controls to the estimation of the effect of the Homestead Act on school enrolment. Just like table 1A.1, the results are split into three panels 1B.1a, 1B.1b, and 1B.1c, each with the results for the three levels of intensity of treatment. In each panel there are five columns which represent five different specifications. Column one is included for reference and is a regression with no controls and just county and year fixed effects (i.e. without state specific year fixed effects). Column two has the results for the baseline model in equation (1.6), and as such the results shown in this column correspond to those plotted in figure 1.9. Column three shows the results of including geographic controls interacted with year fixed effects and column four shows the results of including economic controls interacted with year fixed effects.[18] Column five is the baseline regression but including only the crop suitability indices interacted with year fixed effects as controls. Finally, column six includes all controls interacted with year fixed effects.

The added controls do not have an impact on the baseline estimates. There is remarkable coefficient stability across specifications. For all levels of treatment there is qualitatively and statistically no difference between coefficient estimates in the baseline model and in the other models. However, the sign of the 1850 coefficient for medium and high treatment counties changes when controlling for geographic characteristics. Column five shows that the reason for this change in sign is the inclusion of the crop suitability indices as controls. A cross-section regression of the percent of the county distributed through the Homestead Act by 1930 on the mean suitability index of each crop, controlling for state fixed effects, shows a weak positive correlation between the incidence of the Homestead Act and suitability of tobacco (p-value = .071), and a somewhat stronger negative correlation of the former with suitability for cotton (p-value = 0.041). No significant correlation was found with cotton suitability (p-value = 0.404).[19]

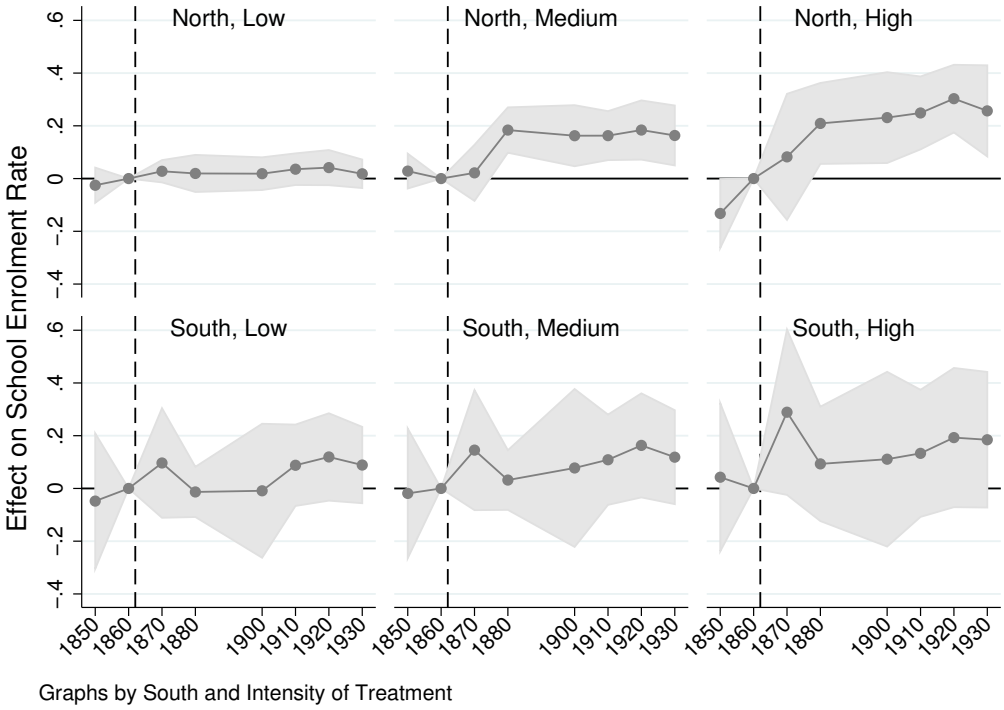
The final approach I take to disentangle convergence from the effects of the Homestead Act is to split the sample between northern and souther states.[20] However, to do this, I cannot include state-specific year fixed effects because the set of

[18] As in table 1A.1, the geographic controls included are: mean annual precipitation and temperature; altitude and slope index; land suitability indices of cereals, cotton, and tobacco; the natural logarithm of latitude and longitude; and thermal zones as a percent of county area. The economic controls included are: distance to largest population centre in the state, population density in 1860, percent slavery in 1860, percent farm households in 1860, stock of land distributed by the federal government by 1860, and two indicators of rail and water transport availability in 1860.

[19] The regression uses the same 1,557 counties of the baseline results in table 1B.1, column two.

[20] I define northern states as states north of the Ohio-Potomac rivers for those states east of the Mississippi, and the states north of the Arkansas/Missouri border on the west bank of the Mississippi. The entire Pacific coast is labelled as northern states.

Figure 1.13: Differential by North/South Effect of the Homestead Act (stocks by 1930) on School Enrolment



variables corresponding to the levels and interaction of the north/south indicator and the PLSS status indicator is perfectly collinear with the set of state indicator variables. Thus, I include year fixed effects that vary according to the interaction of north/south and PLSS/non-PLSS indicator variables. Thus, for outcome y, in county c, in state s, at time t, the equation to be estimated is:

$$y_{c,s,t} = \alpha_c + \lambda_{z,p,t} + \sum_{j=1}^{3} \beta_{z,j,t,1930} \cdot I\left[T_{c,1930} = j\right] + \varepsilon_{c,s,t} \tag{1.10}$$

where z indexes geographic zones (north or south) and p indexes whether the state is in the PLSS or not.[21] The results of estimating this model are shown in figure 1.13. The first row of graphs contain the estimated effects of the Homestead Act for counties in the north, and the second row for the south. The parallel trends assumption is satisfied for all panels except between control and high intensity of treatment counties in the north. The point estimates in the long run are of similar magni-

[21] Given that the treatment only occurred in PLSS areas, there is not point in interacting the treatment with a PLSS dummy variable.

tudes between the north and the south, but the estimates for southern counties are less precise. Figure 1.13 shows that does seem to be a process of convergence in the north between high intensity and control counties. However, that convergence is not present in the other intensity counties in the north, and in between any intensity group and the control counties in the south.

The four exercises that I have carried out in this section show that to the extent that convergence is confounding the effect of the Homestead Act, the evidence suggests that the Homestead Act either accelerated convergence or triggered it. In the absence of a policy such as the Homestead Act, it is quite possible that the gaps present in the pre-treatment period could have persisted.

1.7.3 The effect of the Homestead Act on Literacy

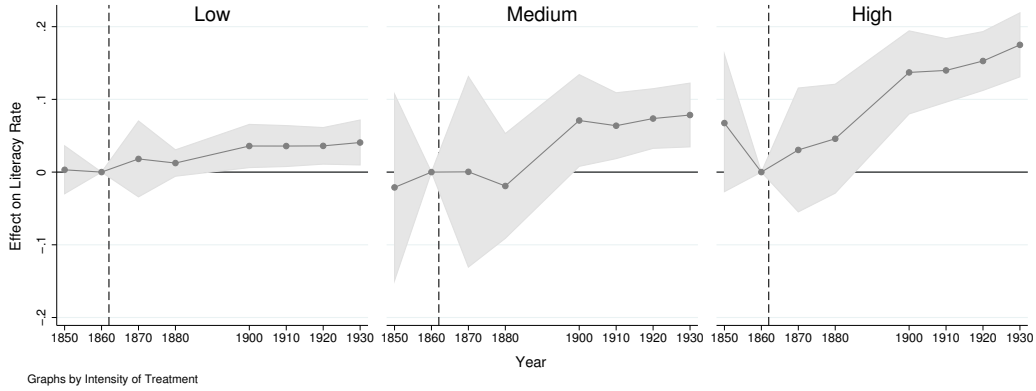
Baseline Model

Figure 1.14 shows the estimated differences-in-differences effects of model (1.6) applied to literacy rates amongst white adults (ages twenty or more) living in the same state they were born in. As in the case for school enrolment, I restrict the sample I calculate the literacy rate to non-movers as an indirect control for sorting and migration.

Analogously to figures 1.8 and 1.9, the figure plots the effects of the Homestead Act for three different levels of treatment. The first panel shows that even low levels of the Homestead Act had statistically significant effects on the literacy rate, of approximately five percentage points starting in 1900. The effects increase in magnitude for higher levels of intensity of treatment. Medium level intensity of treatment had effects of approximately eight percentage points starting in 1900, and high levels of intensity of treatment had effects greater than ten percentage points (although the medium intensity effects are not statistically different from the low intensity effects).

Figures 1.9 and 1.14 also show that the differences-in-differences identification strategy is appropriate. In both figures, the estimated effect of the treatment in the pre-treatment period (year 1850) is not statistically significant. This means that, once state level time trends have been taken into account, there are no statistically significant differences in trends within states between treated and control counties.

Figure 1.14: Effect of the Homestead Act on Literacy, baseline model



Notes: The results come from the estimation of model (1.6) with a sample size of 1,324 counties. The dependent variable is the share of white children ages seven to nineteen living in the same state that they were born in that are enrolled in school, described in section 1.4.2. The three panels refer to the three categories of treatment, of which the summary statistics are shown in table 1.2. Shaded areas represent a 95% confidence interval based on standard errors clustered at the state level.

1.7.4 Robustness Check: Separating the Flows between 1860 and 1930

The previous analysis has defined treatment intensity based on the stock of Homestead Act acres distributed up to 1930. A concern with using this measure of the Homestead Act is that there could be some anticipatory effects during the treatment period. That is, in the estimation of the model, flows of acres that occurred during the latter part of the treatment period are allowed to influence the outcomes in the early stages of the treatment period. This does not invalidate the differences-in-differences identification strategy, but the interpretation of the effects would be altered.

The interpretation of the effects is altered in the following way. If the news of the Homestead Act shaped the migration patterns to frontier counties, then anticipatory effects would mean that flows of Homestead acres in the latter parts of the treatment period could affect the outcomes in earlier years. As long as these anticipatory effects do not extend back to the proper pre-treatment period, then identification is not threatened. The different migration patterns shaped by the Homestead Act would be a mechanism of the treatment.

To investigate if there are anticipatory effects, I carry out two exercises. First, I estimate the baseline model in equation (1.6) using only the pre-treatment years and a given year in the post-treatment period. Second, I split the stock of Homestead acres up to 1930 into six flows, and jointly estimate the impact that each

had on the outcomes.

For the first exercise, the treatment intensities are then defined by the stock of Homestead acres up to that specific year in the post-treatment period. For school enrolment and literacy, there are three census years: 1850, 1860 (base year), and a given year in the post-treatment period. For land inequality there are only two waves: 1860 and the given year in the post-treatment period. Thus, for each outcome I estimate six times the baseline model in equation (1.6), but changing the treatment to the corresponding stock by year.

Figures 1C.1, 1C.2, and 1C.3 show the results of the first exercise. The columns represent intensity of treatment and each row a separate regression where the treatment is the stock of Homestead acres up to a certain year in the post-treatment period. The pattern of coefficients is similar to the results in section 1.7.1. There seems to be no anticipatory effects, but for land inequality, there is actually a dampening of the treatment effects for medium and high intensity treatment, which was also present in the baseline results shown in figure 1.8.

The results of the second exercise are shown in appendix figures 1C.4, 1C.5, and 1C.6. In these figures I show the result of estimating the baseline model but with continuous treatment, and in each panel I post the estimated coefficients of each flow/year multiplied by the average flow (for positive flows), using the latest census year as the base year. The estimated equation can be expressed as:

$$y_{c,s,t} = \alpha_c + \lambda_{s,t} + \sum_{d=1870}^{1930} \delta_{d,t} \cdot \Delta h_{c,d} + \varepsilon_{c,s,t} \tag{1.11}$$

where $\Delta h_{c,d}$ is the flow of Homestead acres as a percent of county area defined in equation (1.2). It represents the flow of acres (as a percent of county area) distributed between census years $d-10$ and d. That means that it corresponds to the Homestead Act acres *granted* between the years $d-5$ and $d+5$.[22] For example $\Delta h_{c,1880}$ is the flow of acres (as a percent of county area) distributed between 1871 and 1880 (i.e. the patents were granted between 1876 and 1885).

In figures 1C.4 to 1C.6 the estimated $\delta_{d,t}$'s are first normalised so that the base year is the year prior to the start of that flow, and then multiplied by the mean of $\Delta h_{c,d}$ for $\Delta h_{c,d} > 0$. For instance, in figure 1C.4, for the graph labelled 1880, the coefficient for the year 1880 is the mean of the flow between 1871 and 1880 ($\overline{\Delta h_{1880}} = 0.025$) multiplied by $\delta_{1880,1880} - \delta_{1880,1870}$ (because $\delta_{1880,1870}$ corresponds

[22]Recall that the Homestead Act required that the homesteader reside in his or her claim for five years before being granted the land patent. Thus, the date the land is *distributed* is five years before the patent is *granted*.

to the effect relative to 1860 of the flow between 1871 and 1880, in the year 1870).

Figure 1C.4 shows that flows before and including 1880 had a negative and long lasting effect on land inequality, while flows after 1881 had either positive or negligible effects. The figure also shows that there seems not to be pre-trends in flows. Figure 1C.5 shows that most flows had positive and statistically significant effects on school enrolment, except for the 1871 - 1880 flow (although the negative effects of that flow comes many years later). There seems to be a negative pre-trend for the flows in 1910 and 1920, i.e., education enrolment was falling in counties that received more Homestead Act acres in those years, but the post-treatment effects of those flows are positive or zero. Figure 1C.6 shows that in terms of literacy, each independent flow did not have significant effects. It can also be observed in the figure that there are no significant pre-trends present.

Overall, these two exercises have shown that although the effects of the different flows might vary across time, there is no evidence of anticipatory effects, both within the post-treatment period or in the pre-treatment period.

1.7.5 IV estimation of the effect of Inequality on School Enrolment

In previous sections of this paper I have shown that the Homestead Act had a reduced form effect on land inequality and school enrolment. I now attempt to estimate the implied effect of land inequality on school enrolment. In this section I discuss the results of the IV estimation of the model described in section 1.6.3. I begin by showing how the estimate of the effect of land inequality on school enrolment changes when adding different sets of fixed effects to a baseline OLS regression. Then I discuss four IV models which vary by the inclusion and exclusion of state-specific year fixed effects and the prevalence of slavery in 1860.

Table 1.3 shows the results of four different panel fixed effects OLS specifications. column one corresponds to a simple panel fixed effects model with one regressor (land inequality), county specific fixed effects, and year specific fixed effects. column two shows the results of estimating the same model as in column one, but now controlling for state-specific year fixed effects. Columns three and four are analogous to columns one and two, but additionally controlling for the interaction of the ratio of slaves to total population in 1860 with the year fixed effects.

The main message of this benchmark exercise is that state-level trends absorb much of the effect that could be attributed to land inequality or slavery. Comparing columns one and three, it is clear that the relatively strong negative effect of land inequality on school enrolment disappears once the long term effects of slavery are

Table 1.3: Panel Fixed Effects Benchmark Estimations

	(1) School Enrolment	(2) School Enrolment	(3) School Enrolment	(4) School Enrolment
Land Inequality	−0.460***	−0.0857	−0.0168	−0.0170
	(0.0830)	(0.0752)	(0.0566)	(0.0689)
1870 × Slave % 1860			−0.284***	−0.0815
			(0.0607)	(0.0864)
1880 × Slave % 1860			−0.111***	−0.155**
			(0.0401)	(0.0648)
1900 × Slave % 1860			0.162***	−0.0621
			(0.0441)	(0.0445)
1910 × Slave % 1860			0.318***	0.0268
			(0.0438)	(0.0468)
1920 × Slave % 1860			0.391***	0.0721**
			(0.0429)	(0.0333)
1930 × Slave % 1860			0.310***	0.0345
			(0.0434)	(0.0406)
State × Year FE		Y		Y
Obs.	10843	10843	10843	10843
Counties	1549	1549	1549	1549
Clusters (States)	32	32	32	32
R^2-Overall	0.255	0.243	0.288	0.240
R^2-Between	0.063	0.226	0.355	0.239
R^2-Within	0.443	0.557	0.498	0.560

Standard errors clustered at the state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Note: This table shows the results of running panel fixed effects regressions of school enrolment on land inequality under different assumptions. Columns one and three include year fixed effects common to all observations. Columns two and four relax this assumption and include *state-specific* year fixed effects.

controlled for. However, the inclusion of state-specific year fixed effects drastically reduces the point estimates of both land inequality and slavery (columns two and four respectively).

Table 1.4 shows the result of estimating the model in equations (1.8) and (1.9) under different specifications. Column one corresponds to the IV estimation of the baseline model, without including any controls besides the state-specific time fixed effects. The IV estimate for the effect of land inequality on school enrolment is approximately −0.6, which means that a ten point increase in inequality (i.e. an increase of 0.1 in the Gini index) leads to a six percentage point decrease in school enrolment. However, this effect is not statistically significant, possibly due to a weak first stage.[23]

[23]The under-identification tests for the baseline model without controls is not possible because the available Stata routine that calculates these tests (`ranktest`) finds the rank of the variance-

Table 1.4: IV Estimation of the Effect of Land Inequality on School Enrolment

	(1) School Enrolment	(2) School Enrolment	(3) School Enrolment	(4) School Enrolment
Land Inequality	−0.604	−0.612	−1.822**	−1.825**
	(0.435)	(0.439)	(0.854)	(0.872)
State × Year FE	Y	Y	Y	Y
Aux. Random Var. × Year FE		Y		Y
Slave % 1860 × Year FE			Y	Y
Observations	10843	10843	10843	10843
Counties	1549	1549	1549	1549
Clusters (States)	32	32	32	32
K-P rk LM stat.		16.151	15.935	15.927
K-P rk LM p-value		0.582	0.597	0.598
C-D Wald F stat.		5.954	5.980	5.974

Standard errors clustered at the state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Column two in table 1.4 shows the results of estimating the baseline model but adding a user-generated random variable as a control.[24] I do this so as to add noise to the first stage and avoid the issue of collinearity explained in footnote 23. When adding a user generated random variable, the near-perfect collinearity issue is resolved because the noise introduced by the auxiliary variable allows the diagonal elements of the covariance matrix to be slightly larger, bypassing `Stata's` tolerance threshold. This problem I have encountered is not present if I increase the number of clusters by allowing the panel to be unbalanced. However, changing the criteria for the sample makes comparability with the results in the previous sections difficult. Nevertheless, column two shows that the IV estimate for land inequality is approximately the same as for column one. The low values of the Kleibergen and Paap rk LM statistic and its corresponding high $p - value$ cast doubt on the strength of the first stage. That is, the reduction in land inequality caused by the Homestead Act seems not strong enough to allow the IV estimation to identify the effect of land inequality on school enrolment.

Columns three and four are analogous to columns one and two, but additionally controlling for the prevalence of slavery in 1860. It can be seen that the IV

covariance matrix of excluded instruments to be equal to 14, which is less than the number of excluded instruments (18). However, I have calculated the rank directly using a smaller numerical tolerance threshold and have correctly obtained a rank of 18. The issue is the high correlation of the treatment category with PLSS status (all counties in Non-PLSS states are in the control group). The inclusion of state-specific time fixed effects is the source of the problem: `ranktest` "partials-out" these fixed-effects from the instruments, leaving very little variation, and thus Assumption 2 in Kleibergen and Paap (2006), page 104, is not satisfied.

[24] Uniformly distributed, with seed set equal to `10101` (for replicability).

estimate increases in magnitude to approximately -1.8, and it is now statistically significant at the 5 percent significance level. This means that a ten point increase in land inequality (.1 increase in the Gini index) leads to a 18 percentage point decrease in school enrolment. This corresponds to approximately 2 standard deviations of school enrolment in 1930 (one standard deviation over the pooled sample). This is consistent with the results presented in section 1.7.1, where I showed that the omission of slavery biased the effects of the Homestead Act downwards. Thus, the inclusion of slavery as a control strengthens the first stage, although not enough to alter the result of the Kleibergen and Paap (2006) under-identification test.

1.7.6 Discussion

In section 1.7.5 I have shown that the effect of land inequality on school enrolment is negative by using the Homestead Act interacted with time as an instrument. This result is conditional on controlling for the prevalence of slavery in 1860. One reason for this is that slavery in 1860 acts as a proxy for the prevalence of plantations, which gradually subdivided in the post-treatment period. Since slavery in 1860 and the stock of Homestead Act acres are negatively correlated, controlling for slavery yields a stronger effect of the Homestead Act on land inequality, which yields a stronger effect of land inequality on school enrolment.

The exogeneity assumption or exclusion restriction when using IV methods requires that the Homestead Act have no other effect on school enrolment besides through the reduction in land inequality. I have argued previously that by including state-specific year fixed effects, the role of sorting and migration is reduced, and the possible effect of the Homestead Act on property rights is controlled for (i.e. changes in policies with regard to property rights at the state level are captured by the state-year fixed effects). However, there is one channel which I have not excluded: wealth effects. The Homestead Act was in essence a large wealth transfer from the federal government to private citizens. Nevertheless, Bleakley and Ferrie (2016) show how an earlier free land transfer experiment (the Georgia Cherokee Land Lottery of 1832) did not lead to better outcomes (wealth, income, and literacy) for the descendants of the winners relative to the non-winners.

Taking the evidence from this paper together with the results of Bleakley and Ferrie, I hypothesise that the accumulation of basic human capital during the nineteenth century was not particularly expensive, and thus wealth was not the main constraint. Also, the technology to provide literacy and basic primary education was not prohibitive (Goldin and Katz, 2008). Thus the main constraints to basic human capital accumulation were on the *supply* side. In section 1.7.2 I

argue that the convergence observed after the implementation of the Homestead Act is a consequence of the latter. This is consistent with the conjecture that by distributing land more equally, the Homestead Act increased the *density* of children in primary school age in rural areas. A higher density of children —brought about by a more egalitarian distribution of land— could have led to economies of scale in the provision of schooling, and hence a higher enrolment rate.

Thus, the results of this paper suggest another mechanism by which land inequality negatively affects school enrolment: it increases the costs of the provision of schooling. This adds to the mechanisms discussed in the previous literature, in which land inequality could be either acting as a proxy for the political power of landed elites (Galor et al., 2009), or as a measure of cultural heterogeneity Alesina et al. (2004); Goldin (1998). In either case, the Homestead Act reduced land inequality, and through that channel increased school enrolment. Further research should focus on disentangling these various channels.

1.8 Conclusion

This paper estimates the impact that an egalitarian policy such as the Homestead Act had on land inequality, school enrolment, and literacy. The results show that the Homestead Act lowered land inequality, increased school enrolment, and increased literacy. To estimate these effects I used variation in the implementation of the Homestead Act across counties in the United States during the late nineteenth and early twentieth centuries, combined with the unexpected enactment of the Homestead Act, which had been prevented from being signed into law until the Civil War. Thus, I implement a differences-in-differences identification strategy which is supported by the fact that, taking into account state-specific year fixed effects, the human capital variables follow parallel trends between treatment and control groups in the pre-treatment period.

I also use an instrumental variables approach to estimate the impact of land inequality on school enrolment. I show that, once I account for the legacy of the prevalence of slavery in 1860, the Homestead Act generates enough variation in land inequality to estimate a negative effect of land inequality on school enrolment. Both the reduced form results and the IV estimation are robust to the inclusion of state-specific time fixed effects, which control for changes in policies at the state level, as well as cross-state sorting and migration. Moreover, I employ a series of robustness checks to rule out that the effects of the Homestead Act on school enrolment is simply due to convergence.

The contribution of this paper is twofold. First, to my knowledge I am the first to quantify the impact of the Homestead Act on land inequality and human capital. Second, I show that the negative effect of land inequality on human capital that has been estimated by others using the Engerman and Sokoloff hypothesis (Galor et al., 2009; Easterly, 2007; Ramcharan, 2010) can also be estimated using a policy induced change in land inequality, rather than one determined by geographic and climate characteristics. Thus, I show that an egalitarian policy such as the Homestead Act had a significant role in the expansion of school enrolment in the United States.

Further research on this topic should be aimed at exploiting the rich biographical information in the land patent records that I use to construct the county level aggregates. The main limitation of this paper is that the areas that the analysis is circumscribed to contain only the counties of the United States that *already* existed and had data available since 1850. The Homestead Act was mostly used in the states belonging to the region called "The Great Plains", which was settled *because* of the Homestead Act, and thus did not have data during the pre-treatment period. I exclude these counties because the longitudinal analysis would have been affected by the unbalanced panel. Using data on individuals rather than counties would allow me to bypass these data limitations, as well as significantly increase the causal interpretation of the results. Moreover, individual level data can help to disentangle the channels by which lower land inequality increased school enrolment.

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1.A Land Inequality, the Homestead Act, and Slavery

Figure 1A.1: Effect of the Homestead Act (stocks by 1930) on the Share of Farms per Size Category

(a) 3 to 9 acres

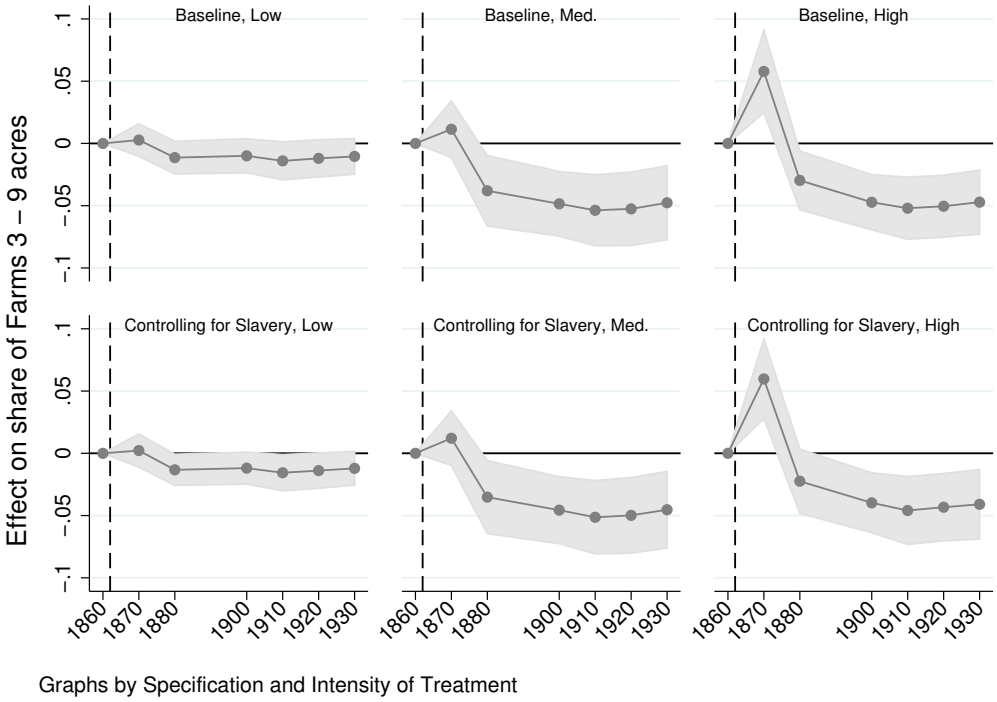


Figure 1A.1: Effect of the Homestead Act (stocks by 1930) on the Share of Farms per Size Category (continued)

(b) 10 to nineteen acres

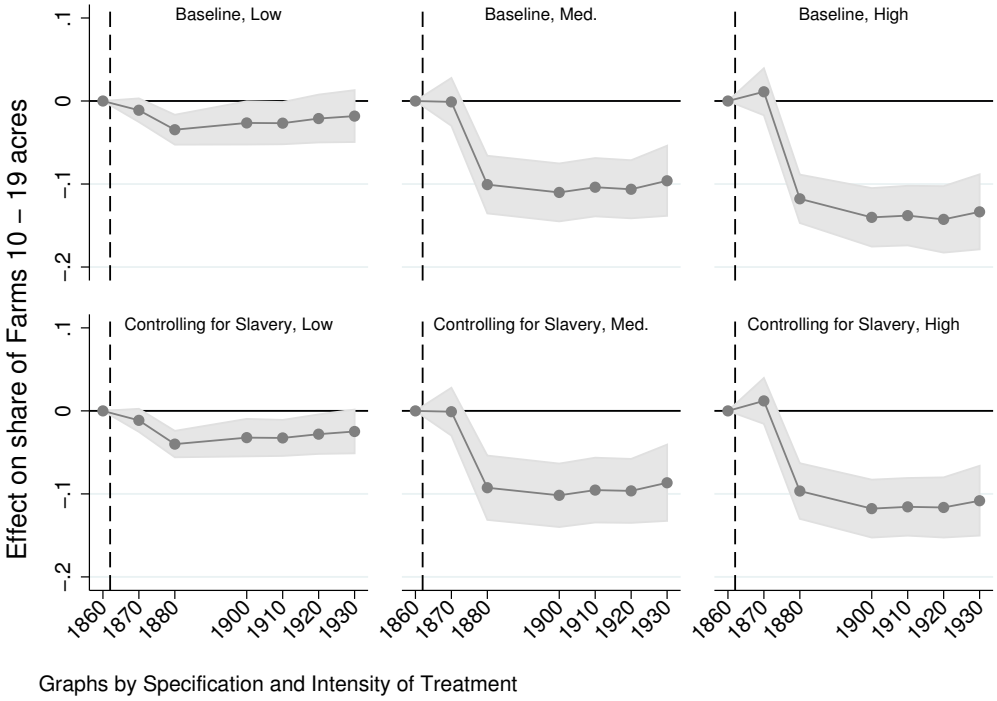


Figure 1A.1: Effect of the Homestead Act (stocks by 1930) on the Share of Farms per Size Category (continued)

(c) 20 to 49 acres

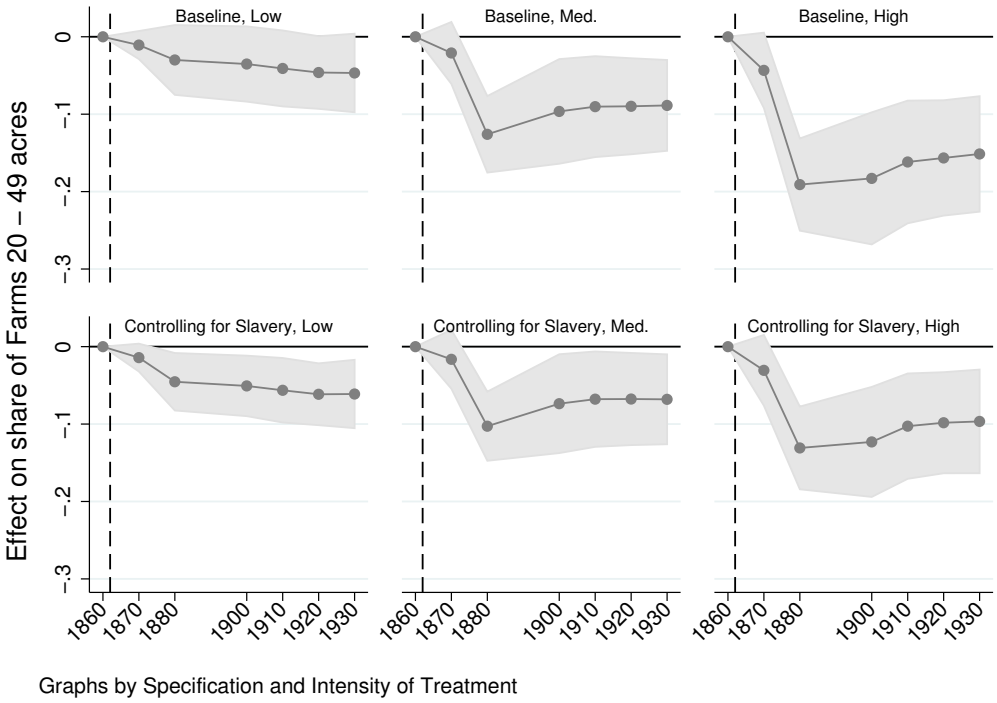


Figure 1A.1: Effect of the Homestead Act (stocks by 1930) on the Share of Farms per Size Category (continued)

(d) 50 to 99 acres

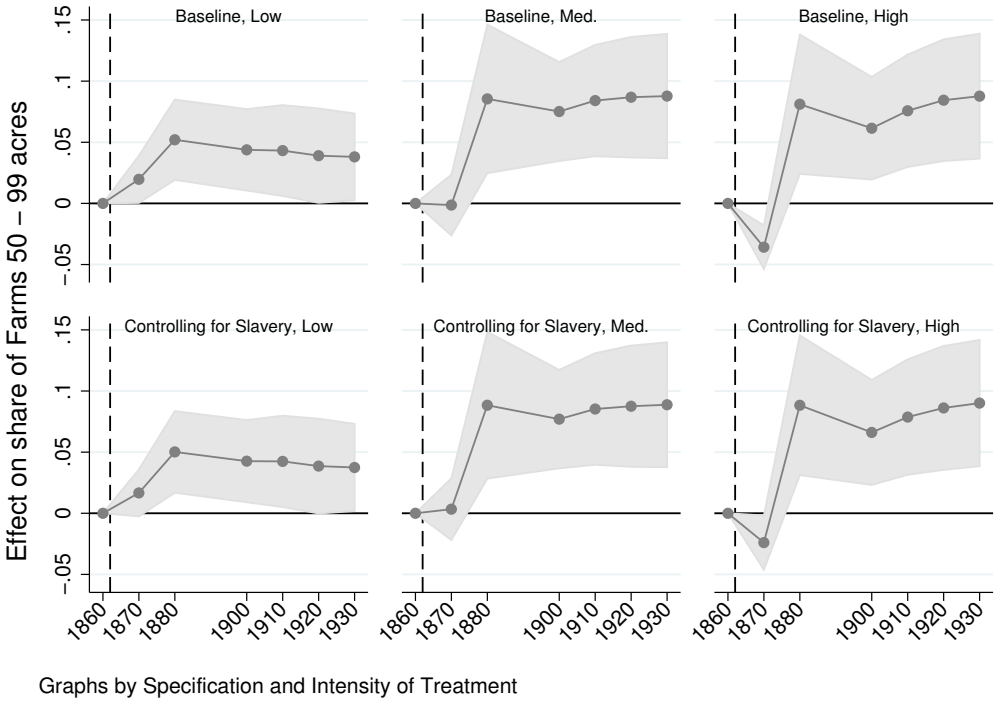


Figure 1A.1: Effect of the Homestead Act (stocks by 1930) on the Share of Farms per Size Category (continued)

(e) 100 to 499 acres

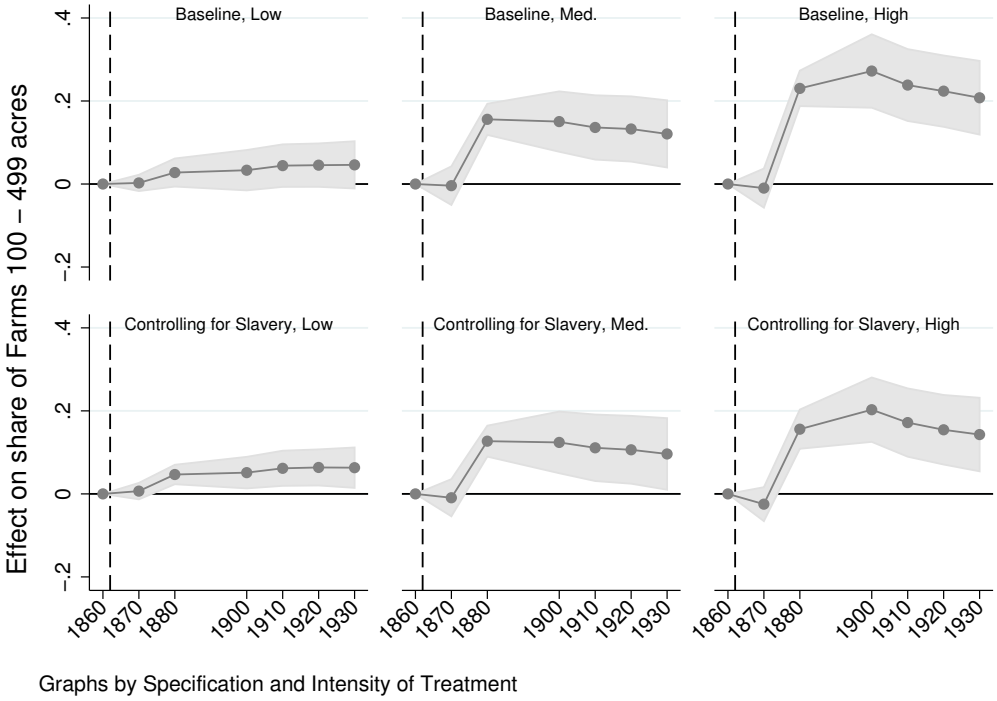


Figure 1A.1: Effect of the Homestead Act (stocks by 1930) on the Share of Farms per Size Category (continued)

(f) 500 to 999 acres

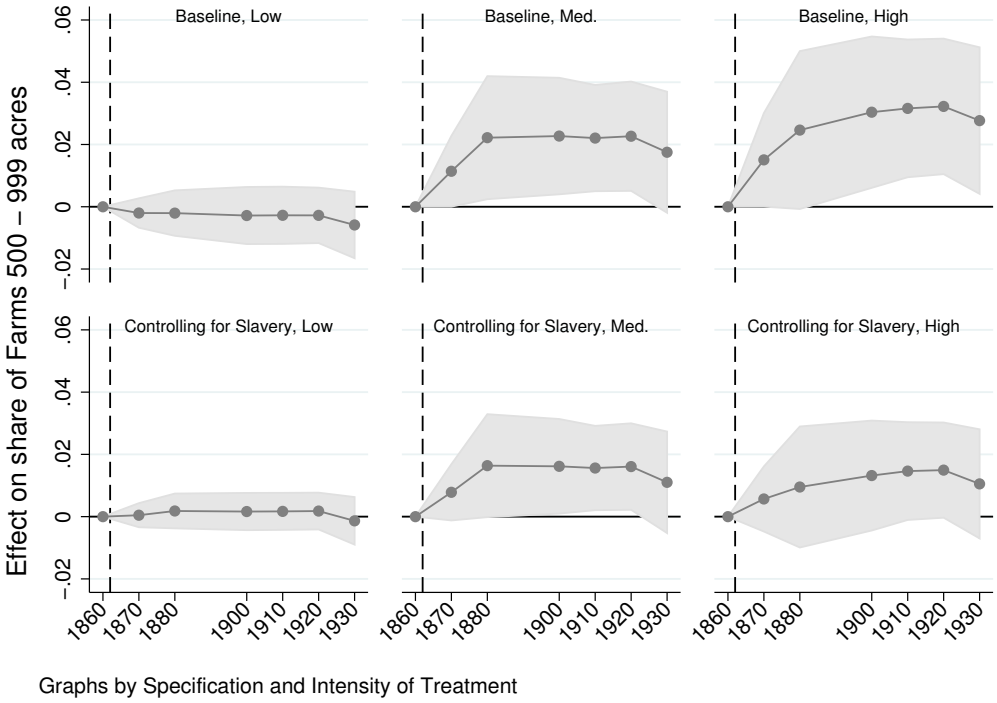


Figure 1A.1: Effect of the Homestead Act (stocks by 1930) on the Share of Farms per Size Category (continued)

(g) 1000+ acres

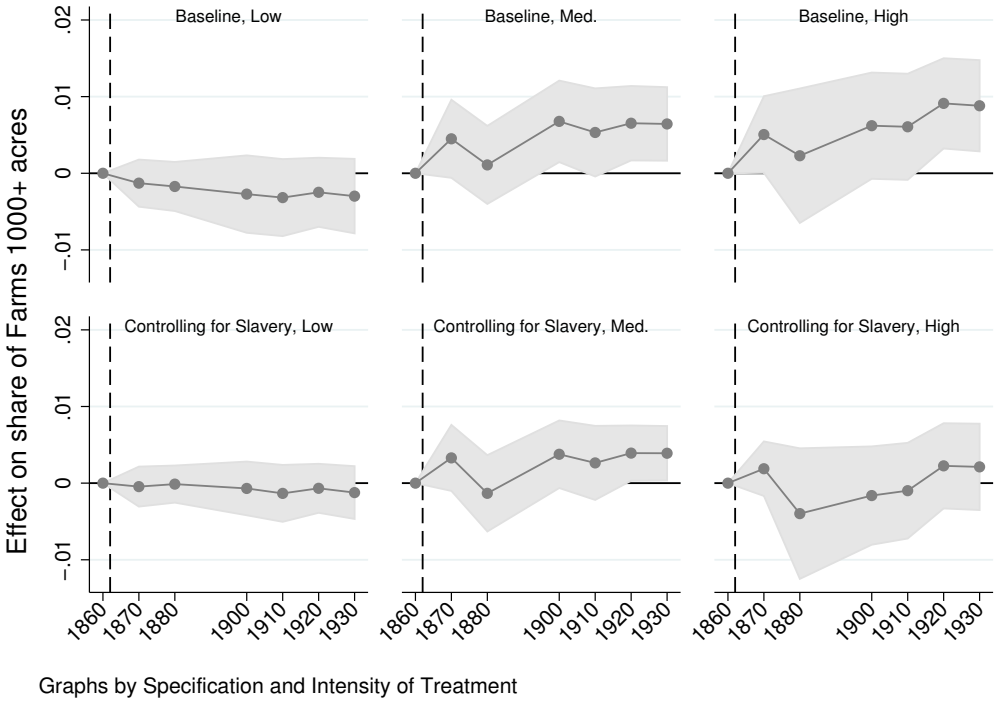


Table 1A.1: Land Inequality, Homestead Act, and Slavery in 1860

(a) Low Intensity Treatment

	(1) Land Inequality	(2) Land Inequality	(3) Land Inequality	(4) Land Inequality	(5) Land Inequality	(6) Land Inequality
1870 × Low	0.0107	0.00683	0.00669	0.00571	0.00532	0.00547
	(0.00849)	(0.00650)	(0.00572)	(0.00674)	(0.00622)	(0.00593)
1880 × Low	0.00738	0.000803	0.00218	−0.00122	−0.000381	0.000229
	(0.0133)	(0.00934)	(0.00881)	(0.00862)	(0.00920)	(0.00817)
1900 × Low	−0.00206	−0.00976	−0.0126	−0.00851	−0.00611	−0.00976
	(0.0173)	(0.0108)	(0.00960)	(0.0102)	(0.0105)	(0.00836)
1910 × Low	−0.00714	−0.00896	−0.0128	−0.00485	−0.00404	−0.00737
	(0.0191)	(0.0115)	(0.00953)	(0.0109)	(0.0115)	(0.00792)
1920 × Low	−0.00926	−0.0101	−0.0149	−0.00488	−0.00439	−0.00882
	(0.0196)	(0.0133)	(0.0105)	(0.0130)	(0.0133)	(0.00896)
1930 × Low	−0.0193	−0.0116	−0.0159	−0.00515	−0.00556	−0.00900
	(0.0204)	(0.0140)	(0.0115)	(0.0132)	(0.0136)	(0.00910)
1870 × Slave %, 1860				0.0485**	0.0560**	0.0428**
				(0.0235)	(0.0265)	(0.0183)
1880 × Slave %, 1860				0.0170	0.0383	0.000114
				(0.0206)	(0.0227)	(0.0177)
1900 × Slave %, 1860				−0.160***	−0.138***	−0.192***
				(0.0245)	(0.0232)	(0.0257)
1910 × Slave %, 1860				−0.206***	−0.184***	−0.234***
				(0.0260)	(0.0252)	(0.0302)
1920 × Slave %, 1860				−0.231***	−0.212***	−0.243***
				(0.0262)	(0.0261)	(0.0304)
State × Year FE		Y	Y	Y	Y	Y
Geo. Ctrls. × Year			Y			Y
Econ. Ctrls. × Year				Y		Y
Slavery % × Year				Y	Y	Y
Observations	13545	13545	13545	13216	13545	13216
Counties	1935	1935	1935	1888	1935	1888
Clusters	34	34	34	34	34	34
Within R^2	0.202	0.374	0.425	0.463	0.440	0.496

Standard errors clustered at the state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 1A.1: Land Inequality, Homestead Act, and Slavery in 1860 (continued)

(b) Medium Intensity Treatment

	(1) Land Inequality	(2) Land Inequality	(3) Land Inequality	(4) Land Inequality	(5) Land Inequality	(6) Land Inequality
1870 × Med.	−0.0105	−0.0154	−0.0139	−0.00355	−0.0127	−0.00504
	(0.0102)	(0.0104)	(0.0110)	(0.0113)	(0.0110)	(0.0125)
1880 × Med.	−0.0255	−0.0433***	−0.0327**	−0.0351**	−0.0412***	−0.0276*
	(0.0158)	(0.0141)	(0.0151)	(0.0146)	(0.0144)	(0.0152)
1900 × Med.	−0.0407*	−0.0491**	−0.0420**	−0.0529***	−0.0542***	−0.0432**
	(0.0202)	(0.0197)	(0.0191)	(0.0193)	(0.0183)	(0.0179)
1910 × Med.	−0.0345	−0.0395	−0.0349	−0.0428*	−0.0466**	−0.0357**
	(0.0207)	(0.0244)	(0.0213)	(0.0216)	(0.0222)	(0.0174)
1920 × Med.	−0.0328	−0.0343	−0.0317	−0.0374	−0.0425	−0.0318*
	(0.0199)	(0.0276)	(0.0222)	(0.0237)	(0.0252)	(0.0178)
1930 × Med.	−0.0378*	−0.0345	−0.0329	−0.0353	−0.0433	−0.0320*
	(0.0195)	(0.0298)	(0.0229)	(0.0248)	(0.0272)	(0.0180)
1870 × Slave %, 1860				0.0485**	0.0560**	0.0428**
				(0.0235)	(0.0265)	(0.0183)
1880 × Slave %, 1860				0.0170	0.0383	0.000114
				(0.0206)	(0.0227)	(0.0177)
1900 × Slave %, 1860				−0.160***	−0.138***	−0.192***
				(0.0245)	(0.0232)	(0.0257)
1910 × Slave %, 1860				−0.206***	−0.184***	−0.234***
				(0.0260)	(0.0252)	(0.0302)
1920 × Slave %, 1860				−0.231***	−0.212***	−0.243***
				(0.0262)	(0.0261)	(0.0304)
State × Year FE		Y	Y	Y	Y	Y
Geo. Ctrls. × Year			Y			Y
Econ. Ctrls. × Year				Y		Y
Slavery % × Year				Y	Y	Y
Observations	13545	13545	13545	13216	13545	13216
Counties	1935	1935	1935	1888	1935	1888
Clusters	34	34	34	34	34	34
Within R^2	0.202	0.374	0.425	0.463	0.440	0.496

Standard errors clustered at the state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 1A.1: Land Inequality, Homestead Act, and Slavery in 1860 (continued)

(c) High Intensity Treatment

	(1) Land Inequality	(2) Land Inequality	(3) Land Inequality	(4) Land Inequality	(5) Land Inequality	(6) Land Inequality
1870 × High	−0.0228**	−0.0250*	−0.0207*	−0.00679	−0.0189	−0.00802
	(0.00999)	(0.0124)	(0.0115)	(0.0141)	(0.0138)	(0.0141)
1880 × High	−0.0512***	−0.0632***	−0.0499**	−0.0556***	−0.0589***	−0.0467**
	(0.0178)	(0.0170)	(0.0183)	(0.0166)	(0.0179)	(0.0179)
1900 × High	−0.0620***	−0.0674**	−0.0609**	−0.0870***	−0.0814***	−0.0726***
	(0.0181)	(0.0260)	(0.0256)	(0.0219)	(0.0224)	(0.0209)
1910 × High	−0.0456**	−0.0459	−0.0438	−0.0667**	−0.0646**	−0.0555**
	(0.0201)	(0.0342)	(0.0312)	(0.0260)	(0.0293)	(0.0224)
1920 × High	−0.0413**	−0.0403	−0.0429	−0.0611**	−0.0618*	−0.0540**
	(0.0195)	(0.0358)	(0.0304)	(0.0269)	(0.0306)	(0.0221)
1930 × High	−0.0442**	−0.0384	−0.0438	−0.0554*	−0.0614*	−0.0531**
	(0.0190)	(0.0377)	(0.0311)	(0.0275)	(0.0314)	(0.0225)
1870 × Slave %, 1860				0.0485**	0.0560**	0.0428**
				(0.0235)	(0.0265)	(0.0183)
1880 × Slave %, 1860				0.0170	0.0383	0.000114
				(0.0206)	(0.0227)	(0.0177)
1900 × Slave %, 1860				−0.160***	−0.138***	−0.192***
				(0.0245)	(0.0232)	(0.0257)
1910 × Slave %, 1860				−0.206***	−0.184***	−0.234***
				(0.0260)	(0.0252)	(0.0302)
1920 × Slave %, 1860				−0.231***	−0.212***	−0.243***
				(0.0262)	(0.0261)	(0.0304)
State × Year FE		Y	Y	Y	Y	Y
Geo. Ctrls. × Year			Y			Y
Econ. Ctrls. × Year				Y		Y
Slavery % × Year				Y	Y	Y
Observations	13545	13545	13545	13216	13545	13216
Counties	1935	1935	1935	1888	1935	1888
Clusters	34	34	34	34	34	34
Within R^2	0.202	0.374	0.425	0.463	0.440	0.496

Standard errors clustered at the state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

1.B School Enrolment and Convergence

Table 1B.1: Robustness checks for the Effect of the Homestead Act (stocks by 1930) on School Enrolment

(a) Low Intensity Treatment

	(1) School Enrolment	(2) School Enrolment	(3) School Enrolment	(4) School Enrolment	(5) School Enrolment	(6) School Enrolment
1850 × Low	−0.0402	−0.0495	−0.0515	−0.0519	−0.0523	−0.0500
	(0.0260)	(0.0392)	(0.0330)	(0.0343)	(0.0356)	(0.0305)
1870 × Low	0.0638*	0.0323	0.0299	0.0369	0.0347	0.0343
	(0.0347)	(0.0265)	(0.0221)	(0.0254)	(0.0242)	(0.0211)
1880 × Low	−0.00626	0.00215	0.00156	0.00805	0.00161	0.00563
	(0.0412)	(0.0215)	(0.0189)	(0.0195)	(0.0207)	(0.0177)
1900 × Low	−0.0384	0.00259	−0.00179	0.00443	0.00132	0.00272
	(0.0376)	(0.0268)	(0.0233)	(0.0232)	(0.0271)	(0.0215)
1910 × Low	−0.0330	0.0289	0.0203	0.0272	0.0261	0.0222
	(0.0401)	(0.0231)	(0.0190)	(0.0182)	(0.0227)	(0.0165)
1920 × Low	−0.0362	0.0331	0.0224	0.0300	0.0295	0.0234
	(0.0426)	(0.0267)	(0.0215)	(0.0206)	(0.0260)	(0.0191)
1930 × Low	−0.0469	0.0212	0.0134	0.0190	0.0184	0.0150
	(0.0365)	(0.0248)	(0.0205)	(0.0195)	(0.0243)	(0.0189)
State × Year FE		Y	Y	Y	Y	Y
Geo. Ctrls. × Year			Y			Y
Econ. Ctrls. × Year				Y		Y
Crop Ctrls. × Year			Y		Y	Y
Observations	12456	12456	12456	12456	12456	12456
Counties	1557	1557	1557	1557	1557	1557
Clusters	33	33	33	33	33	33
Within R^2	0.397	0.505	0.519	0.512	0.510	0.522

Standard errors clustered at the state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 1B.1: Robustness checks for the Effect of the Homestead Act (stocks by 1930) on School Enrolment (continued)

(b) Medium Intensity Treatment

	(1) School Enrolment	(2) School Enrolment	(3) School Enrolment	(4) School Enrolment	(5) School Enrolment	(6) School Enrolment
1850 × Med.	0.00102	−0.0160	0.0117	−0.0105	0.0208	0.0138
	(0.0208)	(0.0465)	(0.0377)	(0.0402)	(0.0446)	(0.0369)
1870 × Med.	−0.00379	0.0304	0.0417	0.0466	0.0482	0.0504
	(0.0511)	(0.0555)	(0.0507)	(0.0510)	(0.0498)	(0.0453)
1880 × Med.	0.0364	0.0875**	0.0912*	0.0817**	0.0904**	0.0858*
	(0.0510)	(0.0420)	(0.0453)	(0.0391)	(0.0429)	(0.0429)
1900 × Med.	0.0717	0.0937*	0.0922*	0.0980*	0.101*	0.0885*
	(0.0442)	(0.0531)	(0.0478)	(0.0516)	(0.0555)	(0.0478)
1910 × Med.	0.0876*	0.0936*	0.0849*	0.0931**	0.0967**	0.0771*
	(0.0449)	(0.0481)	(0.0459)	(0.0410)	(0.0453)	(0.0412)
1920 × Med.	0.118**	0.117***	0.119***	0.120***	0.125***	0.113***
	(0.0467)	(0.0380)	(0.0346)	(0.0316)	(0.0361)	(0.0301)
1930 × Med.	0.0845**	0.0997***	0.103***	0.105***	0.107***	0.0992***
	(0.0394)	(0.0350)	(0.0325)	(0.0284)	(0.0317)	(0.0290)
State × Year FE		Y	Y	Y	Y	Y
Geo. Ctrls. × Year			Y			Y
Econ. Ctrls. × Year				Y		Y
Crop Ctrls. × Year			Y		Y	Y
Observations	12456	12456	12456	12456	12456	12456
Counties	1557	1557	1557	1557	1557	1557
Clusters	33	33	33	33	33	33
Within R^2	0.397	0.505	0.519	0.512	0.510	0.522

Standard errors clustered at the state level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

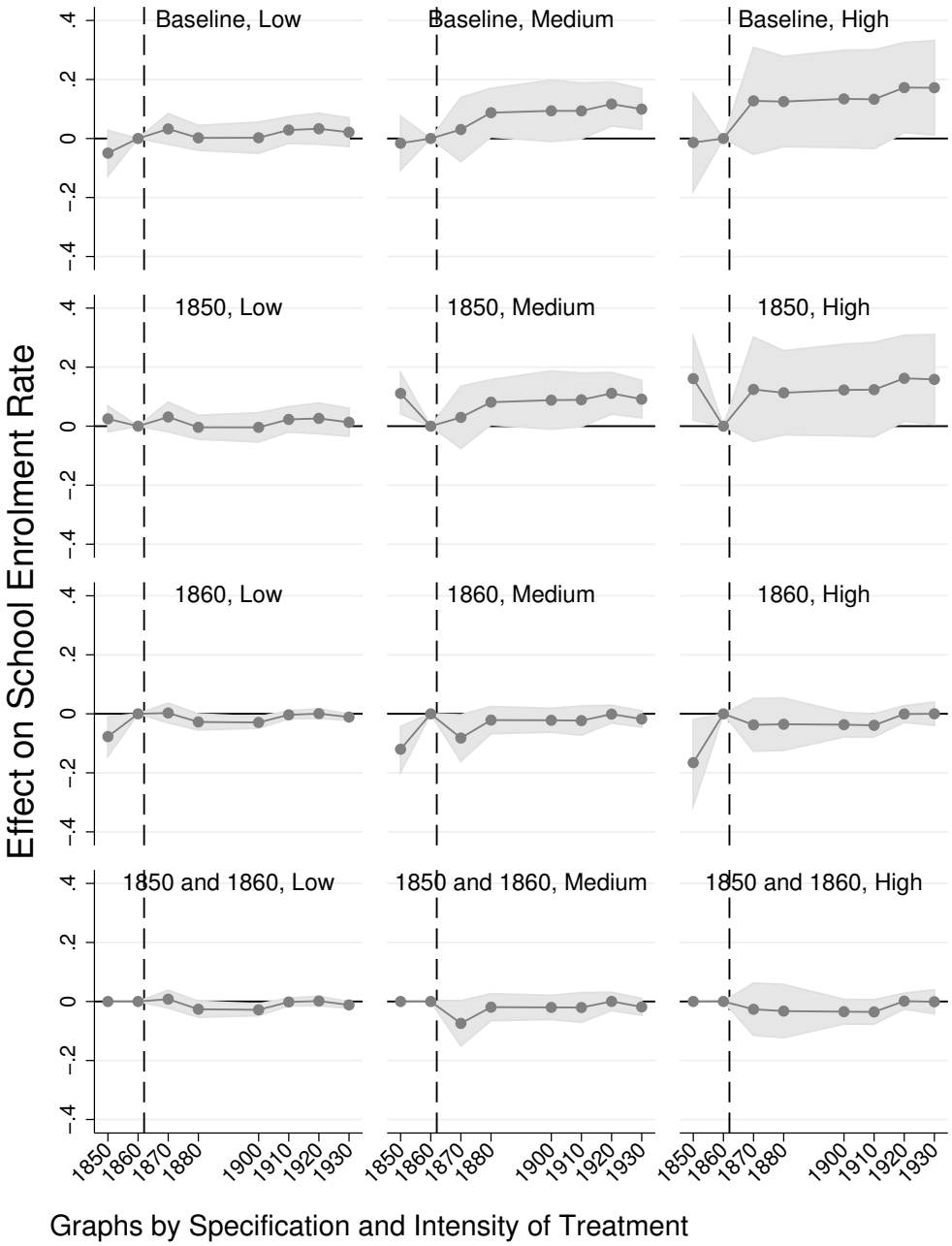
Table 1B.1: Robustness checks for the Effect of the Homestead Act (stocks by 1930) on School Enrolment (continued)

(c) High Intensity Treatment

	(1) School Enrolment	(2) School Enrolment	(3) School Enrolment	(4) School Enrolment	(5) School Enrolment	(6) School Enrolment
1850 × High	−0.00227	−0.0136	0.0380	−0.0234	0.0431	0.0256
	(0.0555)	(0.0837)	(0.0727)	(0.0815)	(0.0801)	(0.0721)
1870 × High	0.102	0.128	0.141	0.140*	0.153*	0.146*
	(0.0801)	(0.0924)	(0.0846)	(0.0806)	(0.0875)	(0.0732)
1880 × High	0.0676	0.125	0.121	0.101	0.131	0.106
	(0.0732)	(0.0779)	(0.0829)	(0.0748)	(0.0811)	(0.0790)
1900 × High	0.112*	0.134	0.139*	0.124	0.149*	0.120
	(0.0628)	(0.0841)	(0.0795)	(0.0794)	(0.0852)	(0.0768)
1910 × High	0.134*	0.133	0.135*	0.121	0.143*	0.111
	(0.0693)	(0.0853)	(0.0788)	(0.0756)	(0.0837)	(0.0740)
1920 × High	0.180***	0.173**	0.183**	0.166**	0.192**	0.164**
	(0.0600)	(0.0778)	(0.0696)	(0.0649)	(0.0750)	(0.0626)
1930 × High	0.161**	0.172**	0.182**	0.167**	0.189**	0.167**
	(0.0653)	(0.0815)	(0.0768)	(0.0714)	(0.0777)	(0.0720)
State × Year FE		Y	Y	Y	Y	Y
Geo. Ctrls. × Year			Y			Y
Econ. Ctrls. × Year				Y		Y
Crop Ctrls. × Year			Y		Y	Y
Observations	12456	12456	12456	12456	12456	12456
Counties	1557	1557	1557	1557	1557	1557
Clusters	33	33	33	33	33	33
Within R^2	0.397	0.505	0.519	0.512	0.510	0.522

Standard errors clustered at the state level in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Figure 1B.1: Robustness: Effect of the Homestead Act (stocks by 1930) on School Enrolment



1.C Separating the Flows between 1860 and 1930

1.C.1 Estimating the effect of the stock of Homestead acres up to a certain year on outcome on that specific year

Figure 1C.1: Effect of the Homestead Act on Land Inequality, one post-treatment year at a time

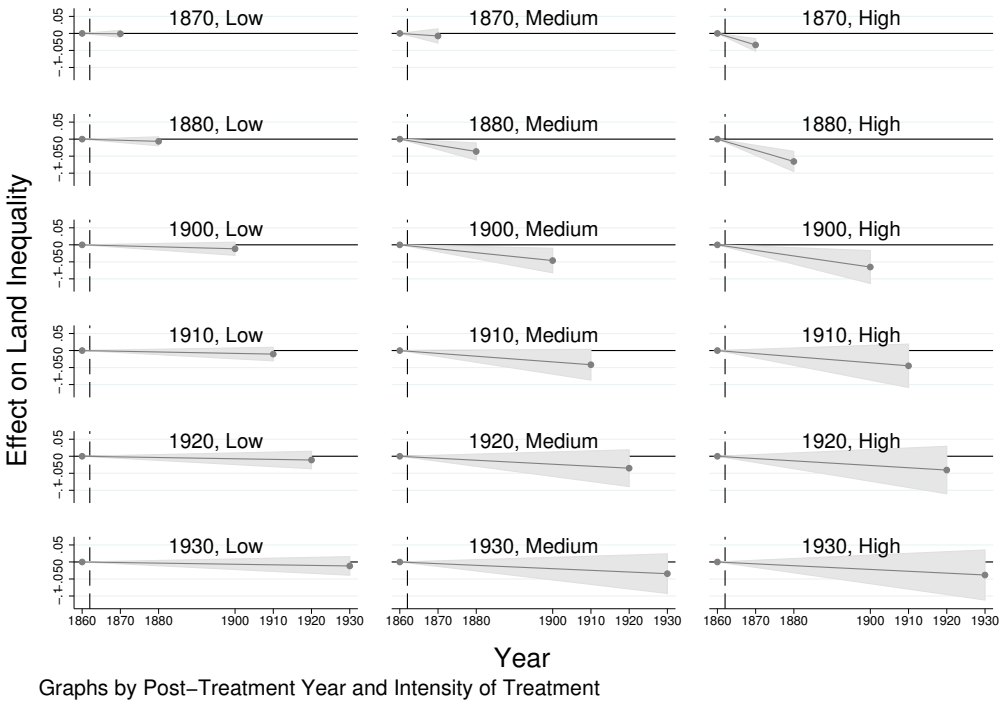


Figure 1C.2: Effect of the Homestead Act on School Enrolment, one post-treatment year at a time

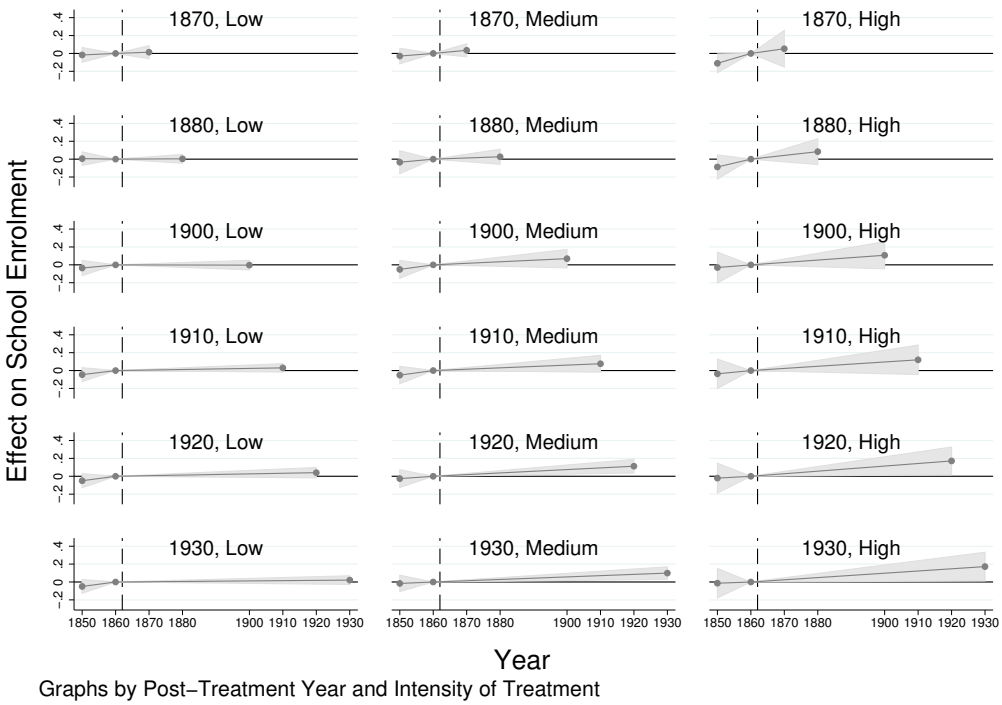
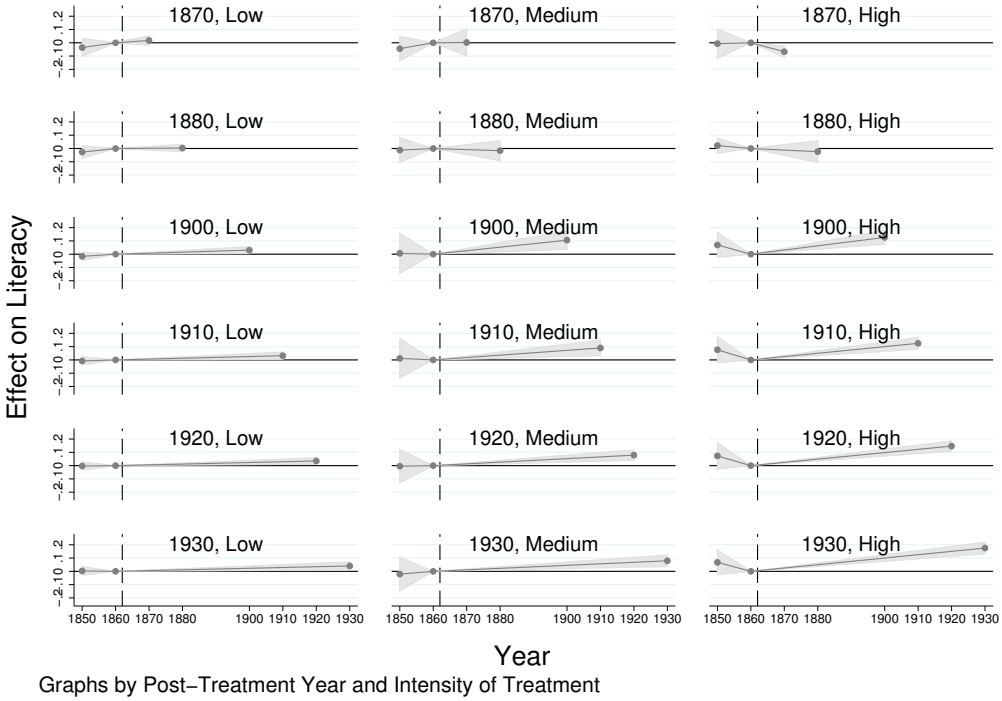


Figure 1C.3: Effect of the Homestead Act on Literacy, one post-treatment year at a time



1.C.2 Jointly estimating the effects of different flows

Figure 1C.4: Effect of the Homestead Act on Land Inequality, joint estimation of the effect of each flow

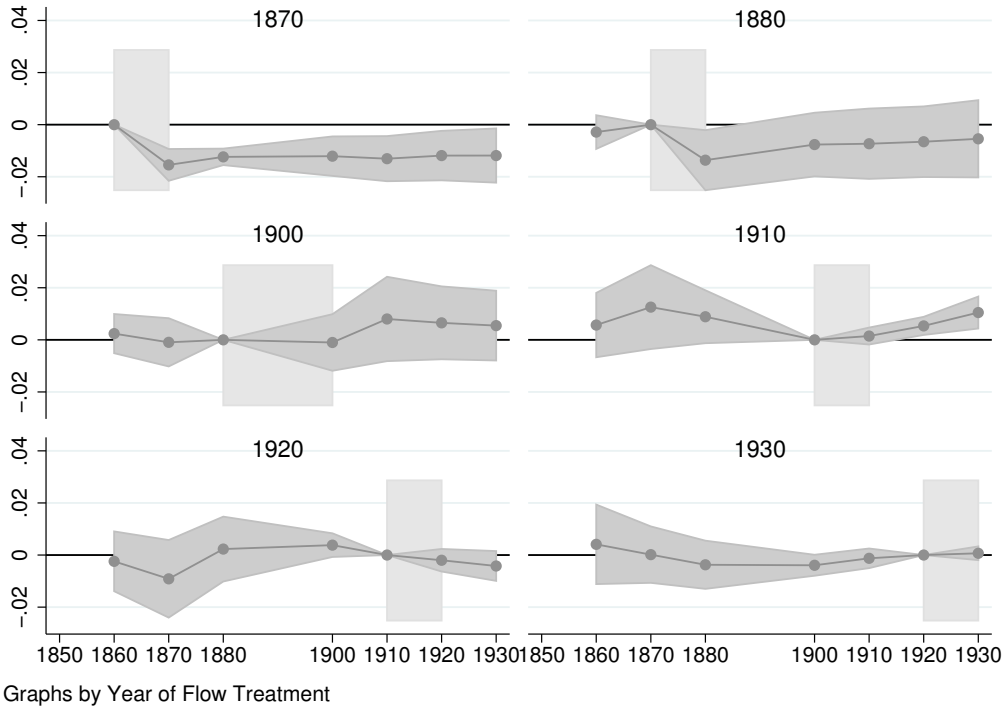


Figure 1C.5: Effect of the Homestead Act on School Enrolment, joint estimation of the effect of each flow

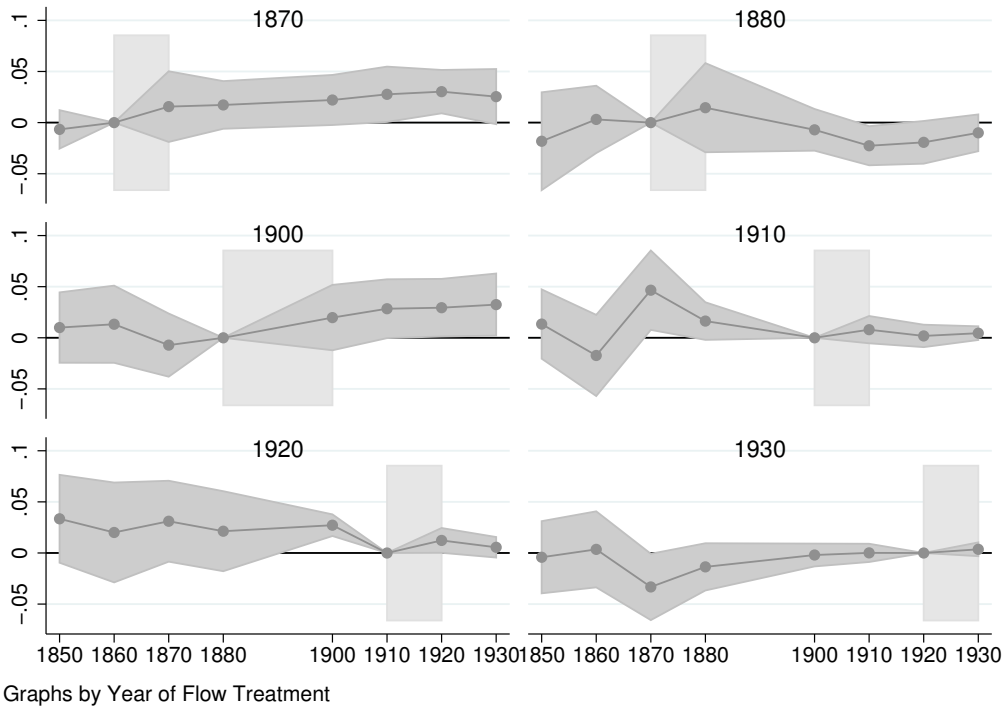
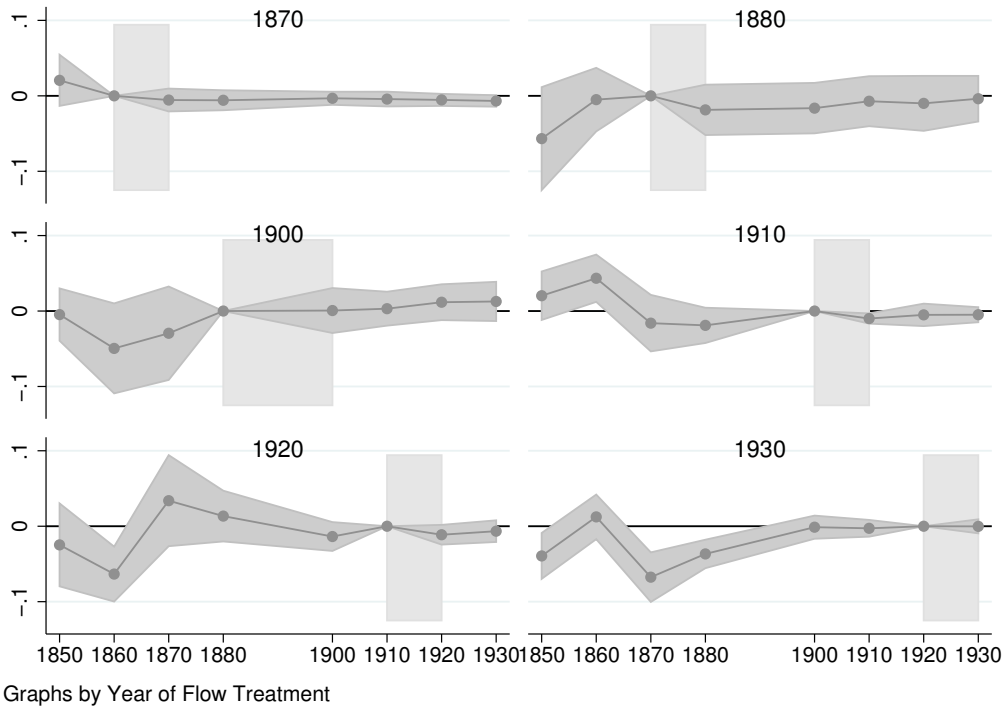


Figure 1C.6: Effect of the Homestead Act on Literacy, joint estimation of the effect of each flow



Chapter 2

Land Redistribution and Crop Choice: Evidence from Reform and Counter-Reform in Chile

2.1 Introduction

Can land redistribution lower land inequality? How does land redistribution affect crop choice? This paper uses county level data regarding the Chilean land reform of the 1960s and 1970s to analyse the impact that redistribution had on land inequality and crop choice. I show that redistribution had a persistent effect on land inequality and crop choice through shaping the spatial distribution of forestry plantations. A particular feature of the Chilean land reform was that due to a military coup there was also a counter-reform process, and I show that it is effects roughly mirror the effects of the reform process.

These are important results because they provide evidence to support the claim that initial distributive conditions can affect paths of development. In particular, had there not been land redistribution, the share of land used for forestry plantations would be significantly higher and the share of land used for fruits, vegetables and vineyards would be significantly lower than they currently are. Given that the cultivation of crops such as fruits, vegetables and vineyards have higher value added than forestry plantations, it is important to understand why certain areas of Chile concentrated in forestry as opposed to other crops.

The historical literature on institutions and growth has linked factor endowments as key determinants of an economy's institutional roots (Engerman and Sokoloff, 1997; Sokoloff and Engerman, 2000; Frankema, 2010). The literature also claims that geographic and climatic heterogeneity determine land suitability for certain crops in a given area, and whether these crops enjoy economies of scale leads to varying levels of land inequality. Thus, the causality claimed in the literature goes from crop choice to land inequality, and hence to institutions through the concentration of political power (Easterly, 2007).

Galor and Moav (2006) develop a model to show that landed elites have the incentive to block investment in human capital promoting institutions such as public education. In Galor et al. (2009), this model is expanded and tested with state level data for the United States in the early twentieth century. Their findings are in line with their predictions but the source of variation comes from the interaction of relative prices and the suitability of wheat versus cotton. Thus, the variation in land inequality is an outcome of crop choice based on geographic and climatic characteristics. Fiszbein (2016) looks at how crop diversity can lead to different paths of development, and again the source of variation is land suitability for certain crops. Ramcharan (2010) uses geographic variation and suitability of crops to identify the impact of land inequality on the demand for redistribution,

in a cross-sectional framework. Cinnirella and Hornung (2016) employ the closest identifying strategy to this paper since they use panel fixed effects estimation paired with a political reform in the shape of the emancipation of serfs in Prussia.

Although I cannot test for reverse causality in the first stage of the relationships described in the previous paragraph, this paper clearly shows that land inequality can be persistently shocked by a human intervention such as land redistribution. The land reform did not shock crop choice directly (it was not designed as such), but I cannot claim that land redistribution affected crop choice only through land inequality. I explore other mechanisms by exploiting the multiple reform treatments generated by the military coup of 1973.

The literature on the effects of other land reforms is vast. The case of the Indian land reforms is studied in Besley and Burgess (2000) and Besley et al. (2016). The former argues that legislated land reform led to lower poverty levels, and the latter uses variations coming from the linguistic differences of India's states to estimate heterogeneous effects of land reform by caste.

Our data comes mainly from two sources. Agricultural outcomes and land distribution are obtained from the digitalisation of historical agricultural censuses. Data on the land reform come from the digitalised expropriation records of the government agency in charge of the reform process. The agricultural censuses contain detailed information on crop choice and land distribution at the county level. The expropriation records provide information on the nearly 5700 expropriated plots and whether they were subject to the counter-reform process or not. I aggregate the expropriation data to the county level for our analysis.

Identification is achieved using panel fixed effects estimation. I only have two pre-reform periods, but constraints on data comparability only allow us to test parallel trends in the pre-reform period for land inequality.

The rest of this article is organised as follows. Section 2.2 describes and discusses the history and process of land reform in Chile. Section 2.3 presents the data and section 2.4 discusses the empirical strategy. Section 2.5 presents and analyses the results and finally section 2.6 concludes.

2.2 Historical Background of The Chilean Land Reform

In this section, I discuss the historical period surrounding the implementation of the land reform and I describe its operative details. An important aspect that I discuss below is how the different policy changes led to different reform outcomes

for a given plot of land. This is crucial for the different results that I will present in section 2.5.

The Cuban Revolution of 1959 was a shock to US foreign policy in Latin America.[1] The Kennedy administration pushed for a treaty that aimed at modernising the backward and repressive societies in Latin America. The working theory was that, like the Marshall Plan in Europe a decade and a half earlier, foreign aid tied to political and economic reforms would block Marxism from taking root in Latin America (see Welch (1985)). In 1961 the Charter of the Punta del Este Conference was signed by twenty Latin American countries, creating the "Alliance for Progress". The charter called for higher growth and better income distribution in a number of well specified goals, including per capita growth targets (2.5 percent per year), increases in adult literacy, increases in the legal requirements of education attainment, increases in life expectancy at birth, price stability, and democratisation across the region. In exchange, the United States would provide aid to the countries that undertook these reforms, although no explicit timing is mentioned in the charter.

Relevant for this paper is the explicit call for "comprehensive agrarian reform" that would lead to the "[...] effective transformation, where required, of unjust structures and systems of land tenure and use, with a view to replacing latifundia and dwarf holdings by an equitable system of land tenure[...]." The United States would provide funding and aid for the technical assistance projects for the purpose of "[...] field investigations and studies, including those relating to development problems, the organization of national planning agencies and the preparation of development programs, agrarian reform and rural development, health, cooperatives, housing, education and professional training, and taxation and tax administration; [...]" (Charter, 1961).

In Chile the conservative President Jorge Alessandri (1958-1964) pushed law 15,020 through Congress, which entered into force in late November, 1962. This law set the legal stage for future land reform, by introducing the legal instruments for expropriation and establishing the legal entity that would carry out the land reform from expropriation to redistribution: the Corporation for Land Reform (CORA). It defined an "economic unit," which was the size of a farm that, depending on soil quality and other characteristics, would have a size that would constitute the upper bound for farms. Anything above that size would be considered a *latifundio*. However, Alessandri's administration would not aggressively pursue land reform,

[1] For a detailed history of the Chilean Land Reform see Rojas et al. (1988).

and used the law mainly to reallocate land in Chile's extreme regions as a means to colonise and reassert sovereignty over under-populated areas.

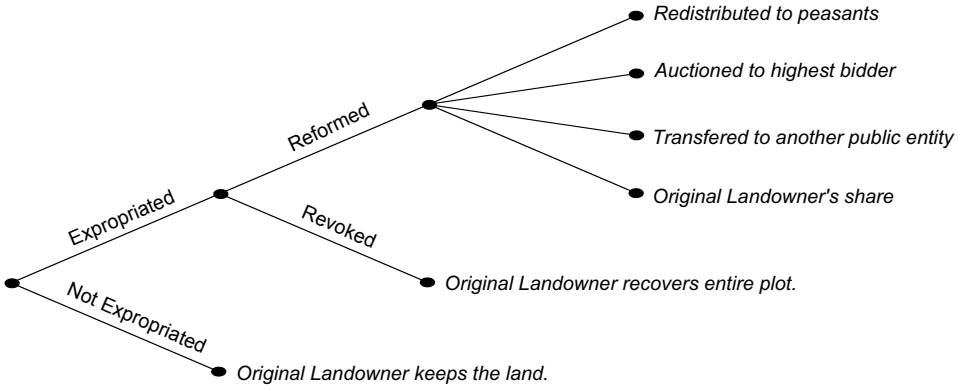
In November 1964, a reformist Christian Democrat, President Eduardo Frei Montalva took office. Frei (1964 - 1970) had campaigned on accelerating the land reform process and immediately started using law 15,020 towards that end, but found that the expropriation clauses were too vague, which made expropriation more difficult. Thus in 1965 a new law was sent to congress, law 16,640, which was passed and entered into effect in late July 1967. This law was the definitive framework by which the land reform process would be carried out during the ensuing years. It established the exclusive legal causal for expropriation, and established the procedure by which expropriated land would first remain whole and administered by the former peasants organised in an *asentamiento*. This entity functioned much like a cooperative. Indeed, after a period of three years during which the *asentamiento* would receive technical and financial help from CORA and INDAP, the peasants had the choice of becoming a cooperative or subdividing the land into private plots. The law also allowed the landowner to keep a share of the land, as long as that share did not exceed a certain threshold established in the expropriation articles (see below). The land that the original landowner kept was called the *reserva*.

Law 16,640 clearly stated the reasons by which a plot could be expropriated in relatively unambiguous terms. Title I, Chapter I contained the articles which were explicit about the conditions for expropriation and left very little room to manoeuvre for both the landowners and the government. In particular, articles 3 and 6 stated clearly that plots which exceeded 80 Basic Irrigated Hectares (BIH)[2] were subject to expropriation whether they were owned by a natural person (article 3) or by a juridical person such as a firm (article 6). There was some ambiguity still left in article 4, which allowed CORA to expropriate abandoned or inefficient plots, without defining what inefficiency means. However, article 4 could not be used for expropriation until after three years from the date of entry into force of the law. Another space for manoeuvre was created by article 10, which allowed landowners to offer their land to CORA voluntarily. The rest of the articles in Title I, Chapter I deal with subdivisions, swamplands, and extreme zones of Chile. More importantly, article 15 explicitly prohibits the expropriation of plots owned by a single natural person if the sum of the surface of the plots is less than 80 BIH.

By the end of Frei's administration, the leftist coalition led by Salvador Allende was critical of the slow pace of the reform. Allende had come close to winning

[2] A BIH is an irrigation weighted average of physical hectares. The conversion coefficients from physical hectare to BIH was also defined in law 16,640, article 172, and varied across regions.

Figure 2.1: Simplified diagram of possible Land Reform outcomes.



Note: This diagram illustrates the different possible outcomes of the land reform process. Given that a plot was expropriated, it could be reformed or the expropriation could have been revoked by Pinochet after the military coup of September, 1973. If it was not revoked, the original landowner could keep a share that did not exceed 80 BIH, and the rest of the land could be redistributed to peasants, auctioned off to the highest bidder, or transferred to another public entity.

the election of 1958, in which the right wing candidate, Jorge Alessandri, won due to the electoral rule that said that when no candidate reached 50 percent of the popular vote it was Congress that chose from the two candidates with most votes. Baland and Robinson (2008) show that the introduction of the secret ballot for the 1958 elections had a negative effect on the conservative parties because it allowed tenant peasants to vote freely without worrying about retribution from their landowners. However, this effect was not strong enough to prevent Alessandri's election. In Baland and Robinson (2012), they show that land prices reacted negatively to the introduction of the secret ballot.

Allende had been a force in Chilean politics for thirty years, first as a member of the Chamber of Deputies and later as a senator. In the 1970 election the pace of land reform was a major issue, and Allende narrowly won the popular vote obtaining 36.6 percent of votes, against 35.3 percent obtained by Alessandri and 28.1 percent obtained by Radomiro Tomic, the Christian Democrat candidate (Frei was barred from participating because consecutive re-elections were not allowed). The Christian Democrats supported Allende when congress had to decide between Alessandri and him and thus the first democratically elected Marxist became president of a country in the Western Hemisphere.

US foreign policy was not slow to react and in the context of the Cold War, a Marxist government in Latin America was unacceptable to Washington. In September 1973, General Augusto Pinochet led a military coup backed by the CIA that toppled Allende a seventeen year dictatorship ensued. Pinochet could not reverse

land reform because it had been sanctioned by the judiciary which implicitly had supported the overthrow of Allende. Thus, the dictatorship would simply revoke the expropriations of the plots that had not been expropriated correctly or that were not far along the reform process. However, this counter-reform of sorts did not completely roll back all expropriations. In fact, the land reform could be considered as continuing during the dictatorship. Even though expropriations were stopped under Pinochet, the distribution of land continued. The *asentamientos* were subdivided into small 10 BIH plots called *parcelas* and granted to peasants that met a series of conditions (male, married, and not a member of a leftist group or agricultural trade union). The aim of revocations was not only to counter-reform but also to get land back into the hands of farmers as quickly as possible since political instability had brought agricultural production to a halt.

Thus, given that a hectare of land was expropriated, there were five possible outcomes: redistribution, auction, transfer to another public entity, reserve of the original owner, or the expropriation could be revoked. Figure 2.1 shows the paths to the different reform outcomes. A more detailed descriptive analysis of the different reform outcomes is provided in section 2.3.1. Section 2.4.2 explains in more detail how I exploit the different paths of reform to look into the mechanisms through which the land reform and counter-reform had an impact.

It could be said that given that Pinochet revoked some expropriations, this negated the effects of the land reform. Moreover, the land reform process has been criticised because there is some anecdotal evidence that the beneficiaries of the reform sold their land after the sale embargoes were lifted. Bowles (2012) argues that the reform was not ex-post optimal and thus the intended effects were not persistent, which meant that the land reform only managed to transfer wealth to the landless peasants at a great cost. On the other hand, one argument in favour of the reform is that the expropriation process led to a process modernisation of the Chilean agricultural sector. Bellisario (2006, 2007a,b, 2013) argues that the expropriations accelerated the transition towards a more capitalist agricultural sector.

Bowles's argument, taken to the extreme, would imply that areas where there was relatively more reform should not experience a long term reduction in land inequality. However, given the lumpiness of land markets, the splitting of the large plots into smaller fractions should lead to a more liquid land market. Even if all beneficiaries sold their land, unless the buyers were the previous large land owners one would expect that land inequality would fall. Bellisario's claim that land reform led to a modernisation of the agricultural sector through a change in the entrepreneural characteristics of land owners is not inconsistent with the land

market liquidity mechanism. In this paper I will present some suggestive evidence that the type of owner changed as a consequence of reform.

2.3 Data

In this section, I describe the data, its sources, and how I construct my final dataset that I will use to carry out the empirical analysis. In section 2.3.1, I describe the expropriations data, and how the land reform was implemented. Section 2.3.2 presents the agricultural censuses and how I construct the dependent variables in the analysis, which are land inequality and the share of a county's planted area corresponding to each type of crop. Finally, section 2.3.3 describes the geographic control variables included in the analysis.

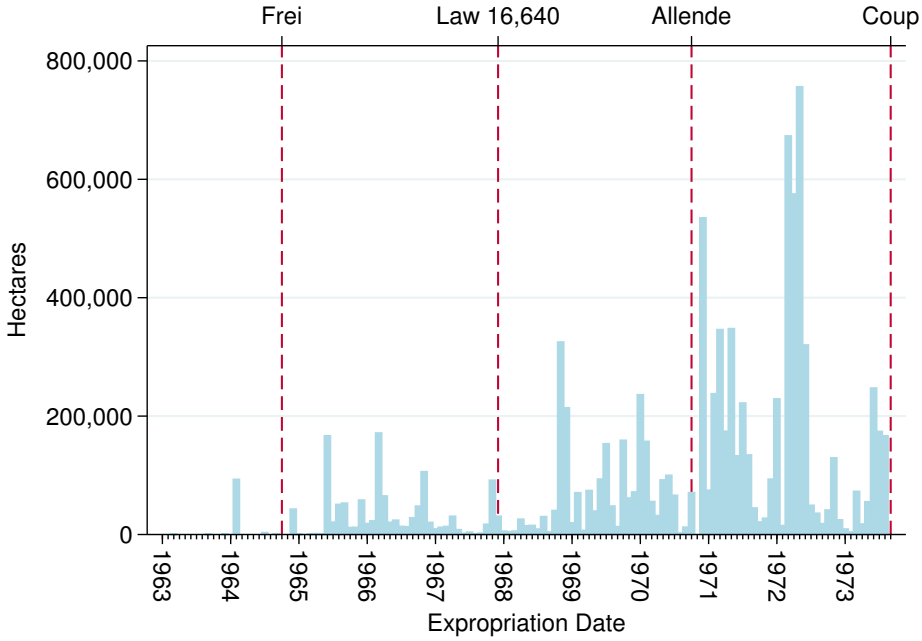
2.3.1 Land Reform Expropriation Cards

In this paper I use a unique dataset on expropriations carried out during the land reform. This dataset was obtained from the digitisation of the "Summary Expropriation Cards" that CORA holds in microfilm. The "Summary Expropriation Card" of each plot contains identifying information such as plot name, plot owner, the tax number of each plot, and the county and province where the plot was located. Moreover, each card contains information on the expropriation process itself: the size of the plot according to the land registry, how much was expropriated by CORA in physical hectares and in BIH, the reason for expropriation, the date of expropriation and the number of the council agreement that determined the expropriation. In addition, the card also has details on whether the land owner kept a reserve, the size of the reserve, the council agreement determining this and the corresponding date.

In some cases the top right corner of the card is crossed out which means that the expropriation was revoked. In such cases the card clearly indicates that the land was returned to the original owner and also provides the council agreement and corresponding date of the revocation.

The cards have other information which has not been digitised at the moment. In particular, the front of the card also contains information on the date and the number of the council agreement that determined the compensation that the owner obtained for the expropriation. Also, there is information on the *asentamiento* that received the land, if the reform process reached that stage for this particular plot. The card provides the name of the *asentamiento* and the date it was constituted. In the back side of the card there is information on the allocation

Figure 2.2: Hectares Expropriated by Month



Note: This graph shows the number of hectares expropriated by month, as well as markers for important events during the land reform process.

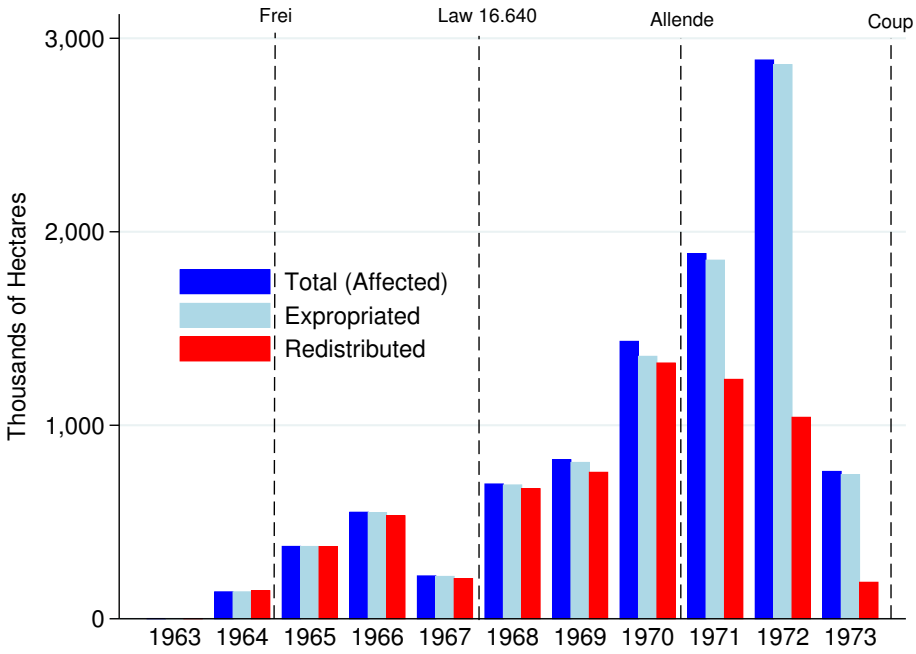
Source: CORA data

of land to the *asentamiento*, any direct transfers to any other public entity such as municipalities, ministries, the armed forces, etc., if the land was auctioned off, and details on the winners of the auction.

The expropriation data contains information on 5,702 expropriations of which 5,655 have non-missing information on the key variables. The total area expropriated equalled up to approximately 9.6 million hectares, while the total surface area of Chile is around 74.5 million hectares, which means that approximately 13 percent of Chile's surface area entered the reform process. Figures 2.2, 2.3, and 2.4 tell the broad story of the land reform process. Figure 2.2 shows how the pace of reform accelerated during the Allende years, specially in 1971 and 1972. The area expropriated by Frei totalled 3.5 million hectares in six years. The Allende government managed to almost double that in just three years, with total hectares expropriated reaching 6 million hectares by September 1973. However, the size of the plots that were expropriated diminished during the Allende years. The average plot size during the Frei period was approximately 2,800 hectares, while during the Allende administration that number fell to approximately 1,400 hectares.

Figure 2.3 also shows the acceleration of reform by the UP government, and

Figure 2.3: Hectares Affected, Expropriated, and Redistributed by Year



Note: This graph shows the number of hectares expropriated and distributed, by year, during the land reform process. Relevant events are marked with vertical dashed lines. Redistributed land is a subset of expropriated land. The total is not equal to the expropriated because of differences in measurement.

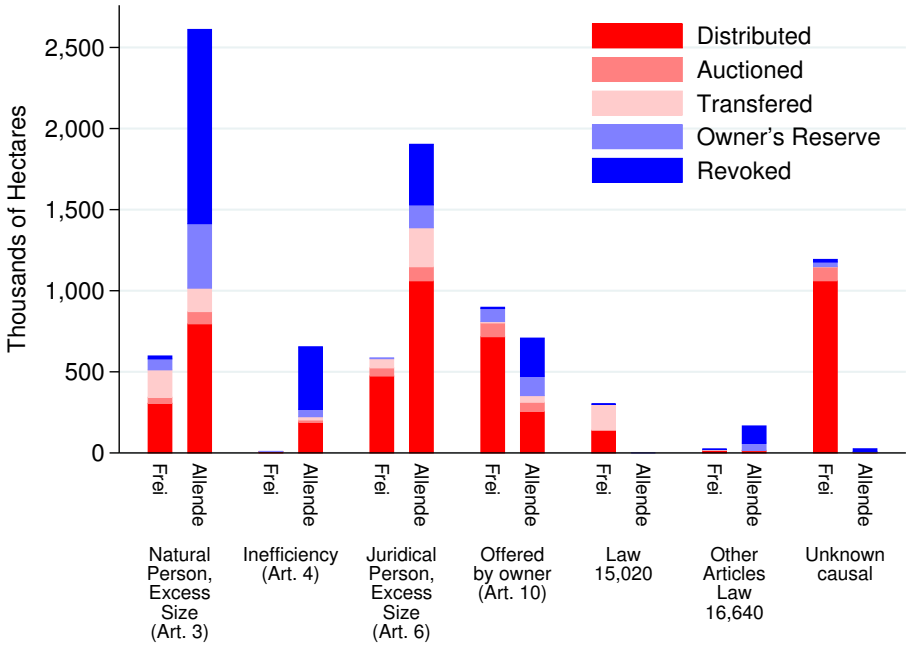
Source: CORA data

it also shows how the ratio of expropriations to redistribution drops significantly for expropriations after Allende became president.

The legal basis for expropriation also varied between the two governments. The Frei government favoured voluntary expropriations (Article 10) which represented 41.9 percent of the total number of expropriations under law 16,640, while expropriations from natural persons due to excess size (Article 3) represented 36.4 percent and from juridical persons due to excess size (Article 6) represented 7.4 percent. Allende's administration reversed this relationship, reducing the voluntary expropriations to 22.3 percent and increasing expropriations from natural persons due to excess size to 46.4 percent and from juridical persons to 7 percent. Allende's government also started expropriating plots based on the inefficiency causal (Article 4), which ended up representing 22.1 percent of expropriations during the Allende period.

In terms of hectares, the share of voluntary expropriations under the Frei government was 40.5 percent, which declined to 11 percent during Allende. For the

Figure 2.4: Hectares Distributed, Auctioned, Transferred, Reserved and Revoked by Reason of Expropriation and Expropriating Government



Note: The graph shows the number of hectares by outcome of the reform process, article of expropriation, and who was president when the plot was expropriated. Frei was president between 1964 and 1970, and Allende was president between 1970 and 1973.

Source: CORA data

expropriations from natural persons due to excess size, that share increased from 29.3 percent during Frei to 43.5 percent during Allende. The share of expropriated hectares due to inefficiency increased from 0.5 percent during Frei to 11 percent during Allende. A significant percent of hectares were expropriated using the excess size owned by juridical entities causal (Article 6). While as a percent of the number of expropriations these only represented 7.3 and 6.9 percent during the Frei and Allende periods respectively, in terms of hectares these figures increase significantly to 28.7 and 31.8 percent.

The different legal bases used to expropriate also led to different outcomes. The raw data can be grouped into four major outcomes of the reform process: Redistribution, auction, transfer to another public entity, or revocation. Of the 8.1 million hectares expropriated under law 16,640, 28.7 percent were returned to their original owner when Pinochet revoked the original expropriation. Of the main reasons for expropriation, the most affected by revocations were expropriations due to inefficiency, which saw 58.4 percent of expropriated hectares returned to the

original owner. Voluntary expropriations were less affected by revocations as only 16.6 percent of expropriated hectares were revoked. For expropriations from natural persons due to excess size, revocations returned 37.6 percent of expropriated hectares to their original landowners, and for expropriations from juridical persons due to excess size that number is just 14.9 percent.

2.3.2 Agricultural Censuses

All agricultural data comes from the Agricultural Censuses published by the National Institute of Statistics of Chile (INE). There have been seven agricultural censuses in Chile, but only the last five have followed the guidelines of the FAO. These five censuses correspond to the years 1955, 1965, 1976, 1997 and 2007 (censuses V to VII).[3] The field work for these censuses was carried out in April, coinciding with the autumn in the southern hemisphere and the end of the agricultural calendar.

The censuses define an agricultural operation as any piece of land that is used wholly or partially for agricultural activities by a producer, regardless of the legal ownership, size or location. Thus, an agricultural operation does not correspond one to one with a plot defined in the legal sense. Moreover, a producer is any entity, human or legal, that has the technical and economic initiative to exploit the agricultural operation.

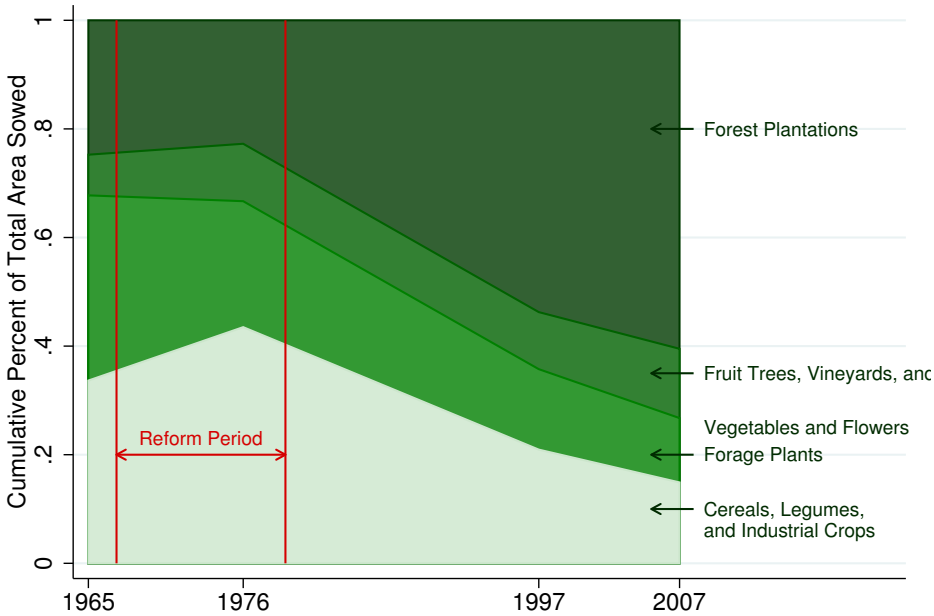
The INE only provides microdata at the agricultural operation level for the sixth and seventh censuses. The III, IV, and V censuses are available at the county level and report aggregates of each variable, not averages (although it is straightforward to construct them for some variables). The main variables of interest are the land size distribution, which for censuses III-V are reported in a table which has the number of plots for each size category, and for censuses VI and VII which can be obtained from the microdata. The definition of the size categories varies for each of the censuses for which I do not have microdata, and as such an standardization is carried out.[4]

Land inequality is measured using the land size distribution tables from the agricultural censuses. The distribution tables are collapsed into eleven size categories to construct a Land Gini coefficient. Each land size distribution table contains the number of agricultural operations that fit within each size category. Unfortunately, the amount of land in each size category is not available at the municipality level for the III, IV and V censuses. Moreover, the top size category does not have an explicit

[3] There were no censuses during the 1940s or during the 1980s.

[4] I collapse the different size categories into 11: 0 to less than 1 hectares, 1 to less than 5, 5 to less than 10, 10 to less than 20, 20 to less than 50, 50 to less than 100, 100 to less than 200, 200 to less than 500, 500 to less than 1000, 1000 to less than 2000, and 2000 an more hectares.

Figure 2.5: Share of Area Planted by (Grouped) Census Crop Category, 1965-2007



Note: The graph shows the share of the area planted for each crop category, after grouping smaller categories: fruit trees, vineyards, and vegetables and flowers are three sub-categories that have been grouped. Cereals, legumes, and industrial crops are also three subcategories that have been grouped.

Source: Agricultural Censuses

upper bound. The first issue has been dealt by the previous literature by imputing the midpoint of each size category as the average size category. However, in this paper I benefit from the fact that this information is available at the province level, and thus I input for each county the province level average size for the corresponding size category. Thus the second issue of an open ended interval for the top size category is dealt in a similar way.[5]

The agricultural censuses have detailed information on surface planted with dozens of crops, and in the case of cereals there is also information on yields. Crops are grouped into seven broad categories: Cereals and Legumes, Industrial Crops, Vegetables and Flowers, Forage Plants, Fruit Trees, Vineyards, and Forest Plantations. Starting with the census of 1965, a summary table is published with the

[5]To check the robustness of this approach, two exercises were carried out. First, all the results were replicated using the mid-point of a size category bin in line with literature, and the results were very similar. Second, the VI and VII censuses *do* contain the surface of farms per size category, and a comparison of the imputed values and the real values was made using standard t-tests. The null hypothesis that both measures were the same could not be rejected at the 1% level.

surface planted with crops in each crop category, and from that I can calculate the percentage of area planted with a certain crop category. The seven different crop categories are grouped into four major categories according to export potential and scale requirements.

Figure 2.5 shows the share of land planted with a certain crop group between 1965 and 2007, aggregating across the entire country. As can be seen in the figure the share of area planted with Forest Plantations has increased significantly and displayed other crops except the crop group formed of Fruits, Vegetables, and Vineyards. Crops in which Chile does not have a special comparative advantage such as cereals and forage plants have yielded their initial dominance to these two new categories. The main source for this change has been attributed to Chile's increasing trade openness, which began with unilateral import tariff reduction during the 1980s and continued into the 21st century with signing of a series of Free Trade Agreements with the United States, the European Union, and a series of APEC countries.

2.3.3 Geographic and Weather Controls

Even though all estimations are carried out controlling for region specific time trends, there is still a great deal of within region geographical and weather variation. Most geographical and climate characteristics of counties in Chile are obtained by geoprocessing raster data from the FAO GAEZ database. Weather data are obtained from the Climate Research Unit at the University of East Anglia, which provides monthly averages of temperature, precipitation, and cloud cover since the early twentieth century. A measure of *ruggedness* of the terrain is obtained from Nunn and Puga (2012).

The geographic variables included can be categorised into two types: distances to relevant locations and raster data on soil and climate characteristics. The distances category includes distance to major population centres (Santiago and provincial capitals), distance to the main highway that crosses Chile (Pan-American route 5), distances to ports, and distance to the coast. Soil characteristics obtained from the FAO GAEZ database categorise soils into 36 different groups. In this paper, the predominant soil categories are individually identified and the remainder are collapsed into an "other" category. The FAO GAEZ database also classifies locations according to Thermal Zones: Tropics, Subtropics, and Temperate with varying degrees of temperature subcategories (all in all 5 categories). The soil and thermal zone data are available in 30 arc-second grids, and thus the prevalent type is calculated for each county (i.e. the mode within the boundaries of the county).

2.4 Empirical Strategy

This section describes the empirical strategy used to identify and estimate the effects of the land reform on land inequality and crop choice. I begin by describing the econometric model in section 2.4.1, and then in section 2.4.2 I discuss how using different measures of reform and counter-reform —which depend on whether the land came to be owned by new individuals or firms— allows the identification of a turnover effect.

2.4.1 Empirical Framework and Identification

Our estimation strategy relies on panel fixed effects estimation of the effect of the reform. Thus our estimating equation is

$$Y_{it} = \alpha_i + \lambda_t + \beta_t \cdot R_i + \gamma_t \cdot C_i + \sum_{k=1}^{K} \delta_t^{(k)} \cdot X_i^{(k)} + \mathbf{Z}'_{it}\delta + \varepsilon_{it} \tag{2.1}$$

where Y_{it} is an outcome variable for municipality i at time t, R_i is a measure of effective reform for municipality i, C_i is a measure of counter reform for municipality i (revocations), the $X_i^{(k)}$s are time invariant control variables $k = 1, \ldots, K$ for municipality i, $\mathbf{Z}_{it}$ is a vector of time variant control variables for municipality i at time t, α_i is a municipality specific fixed effect, and λ_t is a year specific time fixed effect common to all municipalities. The effect of reform on the outcome β_t is allowed to vary across time, as well as the effect of the time invariant control variables, $\delta_t^{(k)}$. The error term ε_{it} is allowed to be autocorrelated within each municipality so standard errors are clustered in the estimation.

This specification allows the identification of the causal impact of reform in two ways. First, the panel structure of the data allows us to determine causality by removing the time invariant variation across counties that could confound our estimates. By testing the significance of the β_t's in the pre-treatment period I can determine whether the post-treatment effect is causal or whether it is simply the product of a pre-existing trend between treatment and control counties.

Second, given that the amount of land that was counter-reformed is observable, I can estimate the impact of intended expropriation. That is, I can interpret counter-reform as a separate treatment. By comparing the coefficients and statistical significance of the β_t's and γ_t's, not only can the reform areas be compared to the non-expropriated areas, but the latter can also be compared to counter-reform areas. Counter-reform can be interpreted as a separate treatment due to a number

of reasons, and in particular, as a threat to property rights. A large landowner might behave differently after been expropriated relative to a large landowner that was never expropriated even if the expropriation was revoked.

One issue that must be dealt with is the correlation of reform with geographical characteristics. The identification strategy in this paper is based on panel estimation, and thus all time-invariant characteristics such as geography are captured by the county specific fixed-effect. Nevertheless, given that the treatment variable is also time-invariant, the estimation of the effects of the reform amount to an event study that interacts treatment with time dummies. Thus, if reform is correlated with geographic characteristics, post reform shocks that interacted with geography might explain the perceived effect of the reform.

This is a particularly sensitive issue since the post-reform period is marked by a series of free-market reforms that began during the dictatorship and continued when democracy was restored in March of 1990. The fact that Chile unilaterally reduced import tariffs in the late 1980s is of special consideration, which significantly reduced the costs of importing machinery and equipment. Moreover, in 2003-2004 Chile signed significant Free Trade Agreements with both the European Union and the United States. To control for the possible confounding effects of trade liberalisation, geographic characteristics are interacted with the time fixed effects. To the extent that trade liberalisation had an impact on crop choice, the main channel by which it operated was through comparative advantage and access to ports. Thus, interacting geographic characteristics with the time fixed effects captures this source of bias.

2.4.2 Definitions of Reform

As described in section 2.3.1 and in figure 2.1, conditional on the expropriation not being revoked, the land expropriated could be redistributed to landless peasants, auctioned off to the highest bidder, or transferred to another public entity. All reform variables have been normalised by the surface area of the county.[6]

Even accounting for the amount of counter-reform, there is no reason to expect that the effect of reform would be the same if the final beneficiary is a landless peasant or the winner of a land auction. It is reasonable to expect that landless peasants did not have the financial resources to participate in auctions. However, I cannot a priori predict that a land auction will reinforce or subtract from the effects of expropriation and redistribution to landless peasants.

[6]The standardisation of county boundaries was carried out by backtracking the legislative record with respect to county splits since the 1950s.

I can, however, look into the role of new versus old owners. Non-revoked expropriations (i.e., reform areas) can be split between land expropriated and given to new owners (landless peasant or auction winner) and the amount of land that remained in the hands of the previous landowner as a reserve. This is an interesting distinction to make since a new land distribution is composed of new owners and old owners with less land. Even though I do not have outcomes for each of these groups, I can look into the effects that different combinations of redistribution and reserve had on crop choice and land inequality.

2.5 Results

In this section, I present the results of employing the empirical strategies discussed in section 2.4. I begin with section 2.5.1, which presents the results of estimating a reduced form model where the treatment variables are the share of land expropriated under different laws and/or articles. Then, in section 2.5.2, I describe the results of estimating the effects of reform and counter-reform separately. Finally, due to the fact that non-revoked expropriations allowed the original landowner to keep a reserve of land, in section 2.5.3 I estimate the effects of owner turnover by splitting the reform treatment into two: reformed with new owners and reformed with the original owners (reserve).

2.5.1 Overall Expropriations

This section takes a look at the effects of overall land expropriated on land inequality and crop choice. Table 2.1 shows the result of estimating a constrained version of equation 2.1 in which reformed land and counter-reform land are pooled together. The resulting equation takes the following form:

$$Y_{it} = \alpha_i + \lambda_t + \beta_t \cdot E_i + \sum_{k=1}^{K} \delta_t^{(k)} \cdot X_i^{(k)} + \mathbf{Z}'_{it}\delta + \varepsilon_{it} \tag{2.2}$$

where E_i corresponds to total land expropriated as a fraction of total county area, that is, the sum of reformed and counter-reformed land.

Table 2.1 contains panel fixed effect regression estimates for three outcome variables under two specifications each. Columns (1), (3), and (5) show the results when only controlling for region specific time fixed effects and columns (2), (4), and (6) show the results when additionally controlling for a series of geographic and weather controls. An important feature of the results shown in table 2.1 is that controlling for geographic and weather characteristics results in point estimates that

Table 2.1: Overall Effect of Expropriation on Land Inequality and Crop Choice

	Land Gini		% Fruits, Vegetables, and Vineyards		% Forest Plantations	
	(1)	(2)	(3)	(4)	(5)	(6)
Exprop. Total:						
1955	-0.027**	-0.021				
	(0.013)	(0.013)				
1976	-0.021*	-0.017	0.035**	0.028*	-0.079***	-0.037
	(0.013)	(0.013)	(0.014)	(0.016)	(0.025)	(0.023)
1997	-0.056***	-0.060***	0.117***	0.064	-0.152***	-0.085*
	(0.019)	(0.017)	(0.041)	(0.043)	(0.050)	(0.045)
2007	-0.079***	-0.090***	0.178***	0.119***	-0.207***	-0.130***
	(0.026)	(0.023)	(0.047)	(0.042)	(0.060)	(0.049)
Region × Year	Yes	Yes	Yes	Yes	Yes	Yes
Geo. Controls	No	Yes	No	Yes	No	Yes
R^2	.4	.55	.6	.73	.53	.68
Counties	192	192	192	192	192	192
Obs.	960	960	768	768	768	768

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$
Note: The table shows fixed effects panel regressions where the dependent variables are shown at the top. For each dependent variable there are two specifications: first without, and then with geographic controls interacted with year fixed effects. All regressions have standard errors clustered at the county level.

are slightly larger in absolute value for the effect of expropriations on land inequality, though these differences are not statistically significant. Meanwhile, the inclusion of the geographic and weather controls reduce the size of the effects when looking at crop choice. Even though these geographic characteristics are fixed across time, their inclusion is aimed at capturing the potential interaction of Chile's opening to international trade with local characteristics. I see that even though there is a slight reduction in the absolute value of the point estimates the general effect is robust to the inclusion of these variables.

As can be observed in table 2.1, total expropriations had a negative impact on land inequality, a positive impact on the share of land planted with Fruits, Vegetables, and Vineyards, and a negative impact on the share of land planted with Forest Plantations. The results suggest that reform areas became more equal in their land distribution, and more importantly, this effect persisted across time. The results also show that reform areas were more likely to switch crops towards Fruits, Vegetables, and Vineyards, and were less likely to switch to Forest Plantations. It is important to remember that both of these categories of crops were increasing their share over time, so a differential impact of reform should be interpreted as

Table 2.2: Overall Effect of Expropriation on Land Inequality and Crop Choice, by Cause for Expropriation

	Land Gini		% Fruits, Vegetables, and Vineyards		% Forest Plantations	
	(1)	(2)	(3)	(4)	(5)	(6)
Exprop. Excess Size						
1955	-0.050***	-0.040**				
	(0.019)	(0.020)				
1976	-0.037*	-0.021	0.039**	0.041*	-0.087**	-0.023
	(0.022)	(0.020)	(0.018)	(0.022)	(0.041)	(0.043)
1997	-0.071*	-0.085**	0.120*	0.110*	-0.097	-0.050
	(0.038)	(0.034)	(0.063)	(0.056)	(0.085)	(0.077)
2007	-0.077**	-0.105***	0.189***	0.170**	-0.157*	-0.093
	(0.035)	(0.035)	(0.069)	(0.068)	(0.092)	(0.090)
Exprop. Voluntary						
1955	-0.044	-0.036				
	(0.039)	(0.036)				
1976	-0.007	-0.011	-0.013	-0.017	-0.082**	-0.029
	(0.026)	(0.027)	(0.038)	(0.037)	(0.040)	(0.047)
1997	0.001	-0.004	0.212	0.086	-0.287***	-0.145
	(0.031)	(0.030)	(0.129)	(0.111)	(0.109)	(0.097)
2007	-0.007	-0.021	0.275**	0.181*	-0.352***	-0.236***
	(0.041)	(0.040)	(0.120)	(0.106)	(0.127)	(0.087)
Exprop. Law 15020						
1955	0.068**	0.056*				
	(0.031)	(0.034)				
1976	0.024	-0.003	0.065**	0.003	-0.009	-0.026
	(0.053)	(0.049)	(0.031)	(0.052)	(0.058)	(0.062)
1997	-0.062	-0.041	0.062	-0.095	0.002	0.094
	(0.071)	(0.057)	(0.167)	(0.196)	(0.251)	(0.192)
2007	-0.185**	-0.135**	0.041	-0.128	-0.156	-0.020
	(0.086)	(0.064)	(0.131)	(0.169)	(0.158)	(0.145)
Region × Year	Yes	Yes	Yes	Yes	Yes	Yes
Geo. Controls	No	Yes	No	Yes	No	Yes
R^2	.41	.55	.61	.74	.54	.69
Counties	192	192	192	192	192	192
Obs.	960	960	768	768	768	768

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Note: The table shows fixed effects panel regressions where the dependent variables are shown at the top. For each dependent variable there are two specifications: first without, and then with geographic controls interacted with year fixed effects. All regressions have standard errors clustered at the county level. The expropriations total has been broken down into three separate variables: expropriations due to excess size, voluntary expropriations, and expropriation by Law 15,020.

changes *relative* to the trend. However, given that the coefficients are increasing in magnitude it can be interpreted that reform and non-reform areas are diverging.

Table 2.2 shows the results of estimating equation 2.2 while splitting the

treatment according to the target of expropriation. The table shows the coefficient estimates for expropriations due to excess size, voluntary expropriations (under art. 10 of law 16,640), and expropriations from Law 15,020. It is clear that the targeting of the reform towards large landowners is what's driving the results for the effects on land inequality and the choice to plant Fruits, Vegetables, and Vineyards, while it is the voluntary expropriations that drive the results on Forest Plantations. However, the magnitude of the coefficients is similar for the excess size expropriations in table 2.2 and the aggregate measure of expropriations in table 2.1, with larger standard errors.

2.5.2 Reform and Counter-Reform

Since not all expropriated land was eventually distributed to peasants because of Pinochet's counter-reform, the coefficients estimates in table 2.1 and table 2.2 should be interpreted as an *intent to treat*. The richness of the expropriation data allows for the disaggregation of the expropriated hectares into two different treatments: reform land and counter-reform land. Thus any given county is split into three categories of land: never expropriated, expropriated and revoked, and expropriated and not revoked. This split corresponds to the estimation of the main specification in equation 2.1, where reformed land has been denoted with the variable R_i and the counter-reform land has been denoted with the variable C_i.

The results of this estimation are presented in table 2.3. The estimates show that revocation had no effect in the short and medium run on inequality, but there are signs that by the year 2007, areas where there was relatively more counter-reform begun to catch up in terms on land inequality to areas with relatively more effective reform. The most significant difference of the results in table 2.3 compared to table 2.1 is the differential impact that reform and counter-reform had on the percent of land planted allocated to Forest Plantations. It can be seen that reform and counter-reform areas diverged in opposite directions with respect to the base category of no expropriation at all.

Table 2.4 singles out the expropriations due to excess size from the overall expropriation effect. It can be seen that the magnitude of the effects of reform on land inequality is larger, and that counter-reform does not significantly affect land inequality. The results for crop choice are also stronger and more precisely estimated in the case of reform, but precision is lost in the case of counter-reform (although the point estimates are similar to table 2.3). The results for land inequality in table 2.4 can be observed graphically in figures 2.6a and 2.6b. It is clear from the figures that

Table 2.3: Effects of Reform and Counter-Reform on Land Inequality and Crop Choice

	Land Gini		% Fruits, Vegetables, and Vineyards		% Forest Plantations	
	(1)	(2)	(3)	(4)	(5)	(6)
Exprop. Reform						
1955	-0.031**	-0.022				
	(0.014)	(0.016)				
1976	-0.037**	-0.023	0.027	0.017	-0.124***	-0.072**
	(0.016)	(0.017)	(0.017)	(0.019)	(0.033)	(0.031)
1997	-0.075***	-0.072***	0.129**	0.078	-0.271***	-0.210***
	(0.025)	(0.023)	(0.051)	(0.049)	(0.058)	(0.054)
2007	-0.092***	-0.094***	0.221***	0.167***	-0.327***	-0.262***
	(0.027)	(0.027)	(0.055)	(0.049)	(0.075)	(0.058)
Exprop. Counter-Reform						
1955	-0.017	-0.019				
	(0.024)	(0.024)				
1976	0.026	-0.000	0.060**	0.059*	0.057	0.060
	(0.016)	(0.017)	(0.029)	(0.033)	(0.061)	(0.057)
1997	0.002	-0.026	0.082	0.025	0.203*	0.262**
	(0.036)	(0.029)	(0.091)	(0.092)	(0.112)	(0.101)
2007	-0.038	-0.079*	0.046	-0.014	0.152	0.239**
	(0.059)	(0.047)	(0.086)	(0.085)	(0.096)	(0.103)
Region × Year	Yes	Yes	Yes	Yes	Yes	Yes
Geo. Controls	No	Yes	No	Yes	No	Yes
R^2	.4	.55	.6	.74	.54	.7
Counties	192	192	192	192	192	192
Obs.	960	960	768	768	768	768

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Note: The table shows fixed effects panel regressions where the dependent variables are shown at the top. For each dependent variable there are two specifications: first without, and then with geographic controls interacted with year fixed effects. All regressions have standard errors clustered at the county level. The expropriations total has been broken down by outcome of the reform process: expropriations that were not revoked (reform) and expropriations that were revoked (counter-reform).

the counter-reform did not have an impact on land inequality while the effective reform did, and this effect is persistent.

Figures 2.7a and 2.7b show the persistence of the effects of reform and counter-reform on the percent of land cultivated with Fruits, Vegetables, and Vineyards. As in the case of land inequality, the differential impacts of reform and counter-reform can be easily observed, with reform areas relatively increasing the share of land cultivated with Fruits, Vegetables, and Vineyards. In contrast, figures 2.8a and 2.8b show how reform and counter-reform affected the share of land dedicated to Forest Plantations. These four figures roughly mirror each other, highlighting how the two divergent paths were determined by how much effective reform

Table 2.4: Effects of Reform and Counter-Reform on Land Inequality and Crop Choice, focusing on Expropriations due to Excess Size

	Land Gini		% Fruits, Vegetables, and Vineyards		% Forest Plantations	
	(1)	(2)	(3)	(4)	(5)	(6)
Exprop. Excess Size Reformed						
1955	-0.051**	-0.033				
	(0.023)	(0.024)				
1976	-0.060**	-0.020	0.029	0.024	-0.160***	-0.074
	(0.028)	(0.025)	(0.020)	(0.025)	(0.052)	(0.048)
1997	-0.118**	-0.126***	0.120*	0.119**	-0.233***	-0.190**
	(0.049)	(0.047)	(0.067)	(0.058)	(0.088)	(0.079)
2007	-0.130***	-0.141***	0.218***	0.209**	-0.298***	-0.239**
	(0.044)	(0.050)	(0.073)	(0.083)	(0.100)	(0.102)
Exprop. Excess Size Counter-Reformed						
1955	-0.048**	-0.056**				
	(0.024)	(0.026)				
1976	0.022	-0.024	0.064	0.083*	0.102	0.088
	(0.027)	(0.028)	(0.041)	(0.046)	(0.109)	(0.106)
1997	0.051	0.011	0.120	0.089	0.254	0.269*
	(0.043)	(0.044)	(0.133)	(0.118)	(0.158)	(0.158)
2007	0.058	-0.022	0.115	0.082	0.205	0.242
	(0.047)	(0.054)	(0.132)	(0.117)	(0.152)	(0.180)
Other Expropriations	Yes	Yes	Yes	Yes	Yes	Yes
Region × Year	Yes	Yes	Yes	Yes	Yes	Yes
Geo. Controls	No	Yes	No	Yes	No	Yes
R^2	.42	.56	.61	.74	.55	.7
Counties	192	192	192	192	192	192
Obs.	960	960	768	768	768	768

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Note: The table shows fixed effects panel regressions where the dependent variables are shown at the top. For each dependent variable there are two specifications: first without, and then with geographic controls interacted with year fixed effects. All regressions have standard errors clustered at the county level. The expropriations have been broken down by cause of expropriation and reform process outcome, although only the coefficients for expropriations due to excess size are shown.

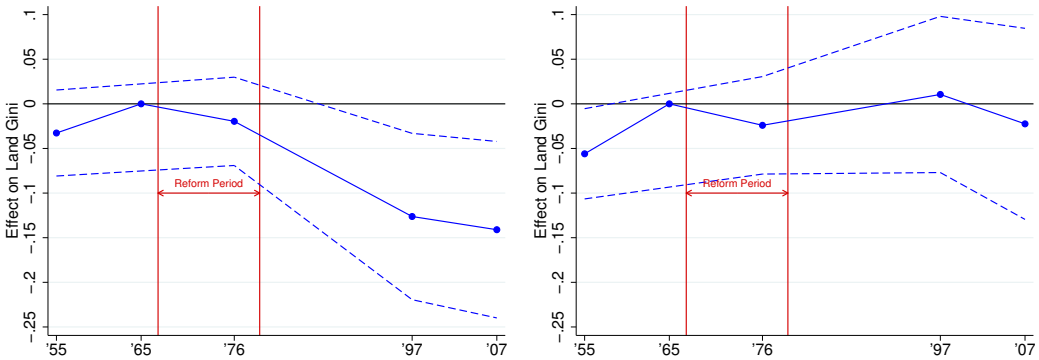
an area had.

2.5.3 New Owners and Old Owners

Up to this point this paper has discussed the effect of reform and counter-reform on land inequality and crop choice. However, as discussed earlier, the reform was such that the original landowners that were expropriated had the right to keep a

Figure 2.6: Effect of Land Reform and Counter-Reform on Land Inequality

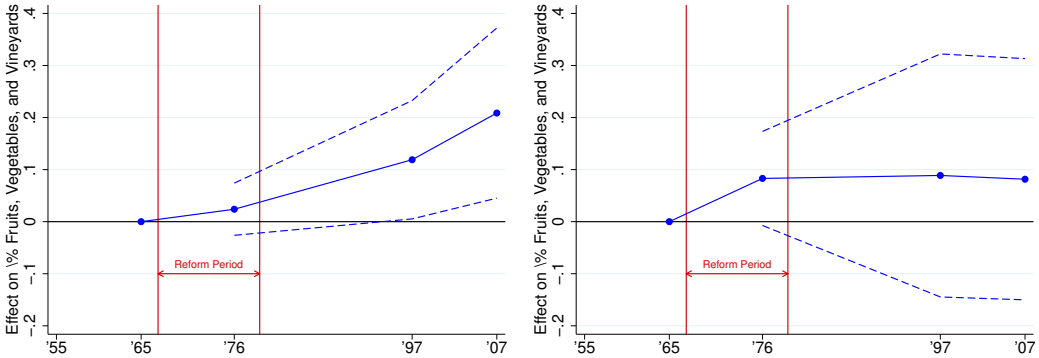
(a) Reform (b) Counter-Reform



Note: Solid blue lines correspond to point estimates and dashed lines correspond to the 95% confidence interval calculated with clustered standard errors. The plots show the coefficients of column (2) in table 2.4, for reformed areas in panel 2.6a and for counter-reform areas in panel 2.6b.

Figure 2.7: Effect of Land Reform and Counter Reform on the share of area planted with Fruits, Vegetables, and Vineyards.

(a) Reform (b) Counter-Reform



Note: Solid blue lines correspond to point estimates and dashed lines correspond to the 95% confidence interval calculated with clustered standard errors. The plots show the coefficients of column (4) in table 2.4, for reformed areas in panel 2.7a and for counter-reform areas in panel 2.7b.

reserve of land. This provides a unique setting whereby it is possible to exploit this heterogeneity to understand the role of owner turnover on crop choice.

Given the right of the former landowners to keep a reserve, the effect of reform on land inequality operates via two channels: first by reducing the size of the land of large land owners, and second by subdividing that land into smaller parcels. The expropriated land can thus be split into two categories: land that ended up in the hands of new owners and the land that remained in the hands of the original landowners as a reserve.

In the case of crop choice, if scale dominates that decision, then there should be no qualitative difference in the effects of land that was expropriated and redistributed, and land that was expropriated and returned as a reserve. If, on the other hand, land that was kept as a reserve has a different effect than land that was redistributed, then that would be evidence that owner turnover plays a role in crop choice.

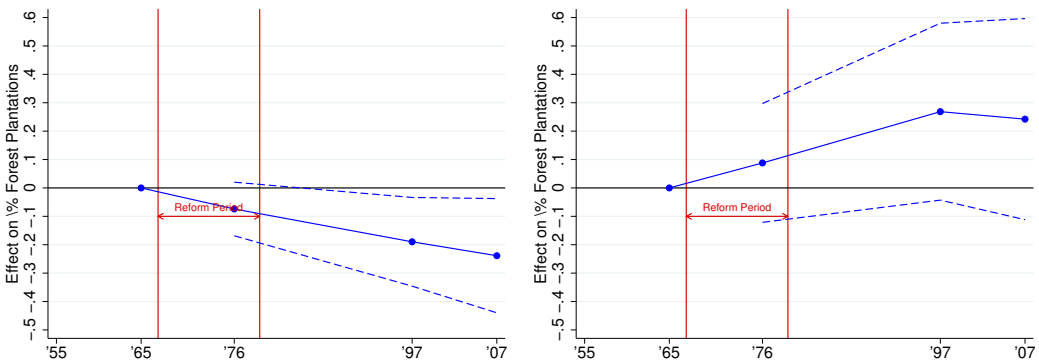
Tables 2.5 and 2.6 show the results of splitting the reform variable into land with new owners and land kept as a reserve by the original land owner. Table 2.5 shows the results when all causes for expropriation are pooled, and table 2.6 is the case when splitting the treatment by the cause for expropriation, but it shows only the coefficients for the excess size cause.

Focusing on column (2) in both tables, the case of land inequality which includes all geographic controls, it can be observed that the point estimates of the reserve variable show that a relatively large part of the original drop in land inequality is due to the effects of lower land size of large land owners. In both tables the coefficients for the reserve variable are relatively greater in magnitude than the coefficients for the new owner variable. However, the coefficients for the former are not estimated as precisely as the ones for the latter.

In the case of crop choice, while controlling for geographic variables in columns (4) and (6) of each table, a similar pattern emerges in which point estimates of the reserve variable are high in magnitude relative to the coefficients on reform with new owners, but the standard errors also increase. The point estimates in Table 2.5

Figure 2.8: Effect of Reform and Counter-Reform on the share of area planted with Forest Plantations

(a) Reform (b) Counter-Reform



Note: Solid blue lines correspond to point estimates and dashed lines correspond to the 95% confidence interval calculated with clustered standard errors. The plots show the coefficients of column (6) in table 2.4, for reformed areas in panel 2.8a and for counter-reform areas in panel 2.8b.

Table 2.5: Channels of the effects of Reform and Counter-Reform on Land Inequality and Crop Choice

	Land Gini		% Fruits, Vegetables, and Vineyards		% Forest Plantations	
	(1)	(2)	(3)	(4)	(5)	(6)
Reform, New Owner						
1955	-0.012	-0.009				
	(0.018)	(0.017)				
1976	-0.020	-0.013	0.029	0.041*	-0.080***	-0.070*
	(0.016)	(0.016)	(0.020)	(0.024)	(0.030)	(0.037)
1997	-0.066**	-0.061**	0.135*	0.083	-0.260***	-0.230***
	(0.026)	(0.026)	(0.071)	(0.063)	(0.068)	(0.064)
2007	-0.089***	-0.079**	0.220***	0.177***	-0.292***	-0.262***
	(0.033)	(0.033)	(0.070)	(0.058)	(0.077)	(0.067)
Reform, Reserve						
1955	-0.098**	-0.065				
	(0.045)	(0.056)				
1976	-0.174**	-0.123*	0.010	-0.112	-0.381***	-0.082
	(0.075)	(0.066)	(0.065)	(0.081)	(0.143)	(0.130)
1997	-0.162	-0.161*	0.237	0.206	-0.420	-0.170
	(0.110)	(0.083)	(0.225)	(0.159)	(0.276)	(0.266)
2007	-0.147	-0.137	0.335	0.221	-0.627**	-0.252
	(0.124)	(0.104)	(0.229)	(0.175)	(0.275)	(0.253)
Exprop. Counter-Reform						
1955	-0.024	-0.024				
	(0.022)	(0.023)				
1976	0.017	-0.007	0.065**	0.061*	0.028	0.044
	(0.015)	(0.016)	(0.028)	(0.033)	(0.058)	(0.055)
1997	-0.009	-0.037	0.102	0.044	0.160	0.228**
	(0.036)	(0.030)	(0.089)	(0.090)	(0.109)	(0.096)
2007	-0.049	-0.090*	0.076	0.010	0.099	0.199*
	(0.058)	(0.049)	(0.086)	(0.084)	(0.094)	(0.101)
Region × Year	Yes	Yes	Yes	Yes	Yes	Yes
Geo. Controls	No	Yes	No	Yes	No	Yes
R^2	.41	.55	.61	.74	.55	.7
Counties	192	192	192	192	192	192
Obs.	960	960	768	768	768	768

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Note: The table shows fixed effects panel regressions where the dependent variables are shown at the top. For each dependent variable there are two specifications: first without, and then with geographic controls interacted with year fixed effects. All regressions have standard errors clustered at the county level. Expropriated areas have been broken down by outcome (reform or counter-reform) and by type of owner at the reform process.

Table 2.6: Channels of the effects of Reform and Counter-Reform on Land Inequality and Crop Choice, by Cause for Expropriation

	Land Gini		% Fruits, Vegetables, and Vineyards		% Forest Plantations	
	(1)	(2)	(3)	(4)	(5)	(6)
Exprop. Excess Size Reformed, New Owner						
1955	-0.015	-0.005				
	(0.027)	(0.027)				
1976	-0.050	-0.009	0.029	0.058*	-0.122**	-0.088
	(0.037)	(0.033)	(0.028)	(0.031)	(0.059)	(0.070)
1997	-0.128*	-0.148**	0.164	0.164**	-0.185	-0.204*
	(0.067)	(0.066)	(0.099)	(0.077)	(0.125)	(0.108)
2007	-0.125**	-0.119*	0.263**	0.272**	-0.306**	-0.298**
	(0.059)	(0.067)	(0.107)	(0.110)	(0.121)	(0.133)
Exprop. Excess Size Reformed, Reserve						
1955	-0.120*	-0.064				
	(0.064)	(0.088)				
1976	-0.209**	-0.147	0.035	-0.072	-0.451***	-0.094
	(0.098)	(0.093)	(0.075)	(0.094)	(0.160)	(0.183)
1997	-0.206	-0.145	0.065	0.087	-0.511	-0.140
	(0.128)	(0.118)	(0.255)	(0.221)	(0.322)	(0.301)
2007	-0.301**	-0.276**	0.165	0.110	-0.520	-0.051
	(0.142)	(0.134)	(0.311)	(0.239)	(0.383)	(0.368)
Exprop. Excess Size Counter-Reformed						
1955	-0.048*	-0.054**				
	(0.025)	(0.026)				
1976	0.019	-0.024	0.065	0.086*	0.097	0.086
	(0.027)	(0.028)	(0.041)	(0.046)	(0.106)	(0.105)
1997	0.044	0.007	0.138	0.104	0.242	0.264*
	(0.042)	(0.044)	(0.135)	(0.120)	(0.158)	(0.158)
2007	0.053	-0.022	0.131	0.093	0.192	0.236
	(0.046)	(0.053)	(0.136)	(0.121)	(0.150)	(0.180)
Other Expropriations	Yes	Yes	Yes	Yes	Yes	Yes
Region × Year	Yes	Yes	Yes	Yes	Yes	Yes
Geo. Controls	No	Yes	No	Yes	No	Yes
R^2	.43	.56	.61	.74	.55	.7
Counties	192	192	192	192	192	192
Obs.	960	960	768	768	768	768

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Note: The table shows fixed effects panel regressions where the dependent variables are shown at the top. For each dependent variable there are two specifications: first without, and then with geographic controls interacted with year fixed effects. All regressions have standard errors clustered at the county level. Expropriated areas have been broken down by cause for expropriation, outcome of reform process (reform or counter-reform) and type of owner at the end of the reform process. Only the coefficients for the expropriations by excess size are shown.

for the reserve variable are larger than for the new owner variable, but imprecise. However, when looking at the corresponding results for expropriations due to excess size in table 2.6, it is clear that the new owners are the ones that are driving the change in crop choice towards fruits, vegetables, and vineyards. Indeed, the point estimates for the share of land kept in a reserve by the old owners are approximately half those for the new owners. In all cases, land subject to counter-reform maintains the pattern observed in section 2.5.2.

Due to the imprecise estimates, it is difficult to definitely draw a conclusion on whether it is the change in scale of the plots or the change in the identity of the owners that drives the changes in crop choice. However, this evidence is very suggestive for future research.

2.6 Conclusion

This paper analyses the effects of land redistribution on land inequality and crop choice, using the Chilean land reform of 1967-1979 as the main source of exogenous variation. The effects of the reform are estimated for 194 counties in the central part of Chile using data from historical agricultural censuses and recently digitalised expropriation data. The data on expropriations have been obtained from the original expropriation records that summarised key information on the reform process such as the amount of land expropriated, the reason for expropriation, as well as the outcomes of the reform.

A particular feature of the Chilean reform process was that due to the military coup of 1973 a counter-reform process revoked the expropriation of nearly half of the hectares expropriated by the previous democratic governments. This feature lends itself for a comparison of the effects of reform and counter-reform across counties.

The results obtained show that effective reform was responsible for a sharp and persistent decrease in land inequality in the areas most affected by the reform. This persistent decrease in land inequality is also accompanied by an relative increase in the land cultivated with crops classified as Fruits, Vegetables, and Vineyards, as well as a sharp relative decrease in the land destined to Forest Plantations. These results are robust to regional trends and the inclusion of a variety of geographical, climatic, and weather controls.

An attempt was made to separate two mechanisms by which land redistribution led to these outcomes: a scale effect and changing the identity of the land

owners. The data suggests that both mechanisms play a role but unfortunately there is not enough statistical power to distinguish these two channels.

In the last quarter of the twentieth century Chile experienced a sharp increase in living standards and growth, which has been often credited to an increase in trade openness and the subsequent specialisation in the crops in which Chile has a comparative advantage. The results of this paper suggest that exactly which (comparatively advantageous) crop is chosen is not entirely geographically determined but depends significantly on past redistributive policies. Moreover, these results also show stable long term persistence of the effects of the reform on land inequality but divergent paths of crop choice between reformed, counter-reformed, and untreated areas.

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Chapter 3

Earthquakes and Birth Outcomes in Chile

3.1 Introduction

Health at birth has been reported to be a strong predictor of future well-being (Black et al., 2007; Bharadwaj et al., 2013, 2017). As such, a growing literature has taken a look at the effects that environmental factors have on measures of health for newborns. The mechanism suggested is that acute sources of stress for the mother lead to excess production of cortisol, which in turn leads to premature delivery (Glynn et al., 2001). One environmental source of stress that has been studied is earthquakes, but for the most part the literature has focused on a single event. In this paper, I exploit rich administrative and earthquake data covering sixteen years and the entire country of Chile in a multiple-event framework to determine the effects of earthquakes on birth weight and weeks of gestation.

The literature on external shocks and birth outcomes is varied, and the mechanism they highlight is that acute stress shortens the length of gestation. Various sources of stress are analysed in the literature. One of the most similar papers to the present one in methodological terms is written by Currie and Rossin-Slater (2013) who analyse the effect of hurricanes on birth outcomes in the United States. Carlson (2015) finds that mass layoffs have a negative effect on birth weight, and the effect is stronger when the news of the layoffs takes place during the late stages of pregnancy. Terrorism has been found to also cause lower birth weights through stress (Brown, 2012; Camacho, 2008; Quintana-Domeque and Ródenas-Serrano, 2017). Class et al. (2011) and Persson and Rossin-Slater (2017) look into the effects that a loss in the family has on birth outcomes for a large sample from Sweden.[1] The most relevant research to this paper is by Torche (2011), who looks at the effects of a single earthquake that occurred in the north of Chile on birth outcomes. I expand on Torche's research and attempt to identify the effects of several earthquakes over a longer time period. To my knowledge, this paper is the first attempt to identify the effects of earthquakes on perinatal outcomes in such a broad setting.

In the baseline specification I find that strong earthquakes have a negative effect on birth weight, while medium intensity earthquakes have a *positive* effect. However, these results are not robust to the exclusion of births exposed to the earthquake which took place on the 27th of February, 2010 off the coast of south-central Chile. Additionally, I find heterogeneity of effects depending on whether a birth is the mother's first child. Moreover, I do not find robust results for the effect of earthquakes on weeks of gestation, suggesting that to the extent the earthquake-

[1] Other literature that looks into the effects of natural disasters on health at birth, but do not claim that the mechanism is through stress include Almond (2006), and Lin and Liu (2014), which highlight the role of the 1918 influenza pandemic.

stress-weeks of gestation channel exists, it is not the only mechanism by which earthquakes affect birth outcomes.

To obtain these results I combine administrative data on live births from the Chilean Ministry of Health with GIS rasters of earthquakes produced by the US Geological Survey (USGS). The raster data are called *ShakeMaps* and provide the spatial distribution of the Modified Mercalli Intensity of an earthquake, which is a measure of the effects of an earthquake *as felt by the population.* The birth data is rich with information about the mother, including municipality of residence, and whether the address is in an urban or rural area (the specific address is not available). Using this information I use geoprocessing tools to assign an earthquake intensity measure to each birth, for each trimester of pregnancy. In section 3.2.2 I discuss the procedure in greater detail.

This paper is structured as follows. In section 3.2 I describe the data and the procedure to assign earthquake exposure to each birth (and in what stage of pregnancy they were exposed). Then in section 3.3 I describe the baseline model I will estimate, as well as the procedures I will carry out to analyse the robustness of the results. In section 3.4 I present and discuss the results. Finally, section 3.5 concludes.

3.2 Data and Summary Statistics

In this section I present the two types of data that I use in this paper. First, in section 3.2.1 I discuss the live births data and the procedures to calculate the week of conception, a key element in the analysis to follow. Also in section 3.2.1 I describe summary statistics on the sub-sample I use in the rest of the paper. Then in section 3.2.2 I present the data on earthquakes and describe the procedure I use to determine the intensity measures at the location level. I conclude this section by describing the impact that the February 2010 earthquake has on the distribution of earthquake intensities.

3.2.1 Birth Data

In this paper I use administrative data from the Ministry of Health of Chile on the universe of live births born between 1994 and 2013. The dataset is very rich with a battery of variables describing outcomes at birth which include birth weight, birth height, birth date, weeks of gestation, sex, type of birth (single, multiple), type of medical staff attending delivery, and location of birth (hospital, home, etc.). This data also includes a wide range of mothers' characteristics, and if applicable, fathers'

Table 3.1: Descriptive Statistics by Year Range

	1994 - 2010		1994 - 2008	
	mean/sd	min/max	mean/sd	min/max
Outcomes				
Birth weight (grams)	3354.5	90	3355.3	90
	523.8	6909	523.9	6909
Weeks of Gestation	38.76	16	38.78	16
	1.710	52	1.702	52
Sex				
Male	0.512	0	0.512	0
	0.500	1	0.500	1
Female	0.488	0	0.488	0
	0.500	1	0.500	1
Mother's age	26.61	15	26.59	15
	6.338	40	6.326	40
Mother's education				
None	0.00257	0	0.00279	0
	0.0506	1	0.0528	1
Primary	0.213	0	0.225	0
	0.410	1	0.418	1
Secondary	0.588	0	0.587	0
	0.492	1	0.492	1
Univ./College	0.195	0	0.184	0
	0.397	1	0.387	1
Unknown	0.000506	0	0.000563	0
	0.0225	1	0.0237	1
Mother's civil status				
Married	0.457	0	0.476	0
	0.498	1	0.499	1
Single	0.543	0	0.524	0
	0.498	1	0.499	1
Mother's labour market status				
Inactive	0.718	0	0.733	0
	0.450	1	0.443	1
Employed	0.282	0	0.267	0
	0.450	1	0.442	1
Unemployed	0.000641	0	0.000630	0
	0.0253	1	0.0251	1
Unknown	0.0000486	0	0.0000446	0
	0.00697	1	0.00668	1
Number of births, alive	1.928	0	1.938	0
	1.098	69	1.110	69
Number of births, died	0.0160	0	0.0171	0
	0.152	16	0.159	16
Number of births, stillborn	0.0147	0	0.0151	0
	0.161	9	0.166	9
Observations	4014205		3538795	

characteristics. These are age, education, and labour market status. Crucially, a mother's residence at the municipality/county level is included, as well as whether the residence is in an urban or rural area. This is a key piece of information as I use the earthquake data to calculate a precise measure of earthquake intensity at the county-urban/rural level, which I henceforth refer to simply as "location."

The earthquake data, of which I will talk more in depth in section 3.2.2, ranges from 1990 to 2011. I thus restrict the sample to births conceived between 1994 and 2010. The reason I do this is based on the way I calculate the week of conception, which is similar to the method used in the rest of the literature.[2] Namely, to calculate the week of conception, I obtain the birth week given by the birth date, and I subtract the weeks of gestation. I then define trimesters as thirteen week periods, and I study a window of 65 weeks: the thirteen weeks culminating on the week of conception, the first trimester (weeks 14 to 26), the second trimester (weeks 27 to 39), the third trimester (weeks 40 to 52), and a fourth post-birth trimester (weeks 53 to 65). The need to define trimesters from the week of conception stems from avoiding the possibility of a mechanical bias that is the result of the inverse relationship between length of gestation and exposure to shocks.

In February 2010, a strong 8.8 magnitude earthquake struck off the coast of south-central Chile. This earthquake caused a significant number of casualties and was highly disruptive. Highways were cut off, and there were frequent power outages (CEPAL, 2010; Contreras and Winckler, 2013; Duputel et al., 2012; Subsecretaría del Interior de Chile, 2011). In section 3.3, I describe the models that I estimate, and I do so for the full sample and for the sub-sample of births that were not affected by this earthquake during the 65 week window I described in the previous paragraph.

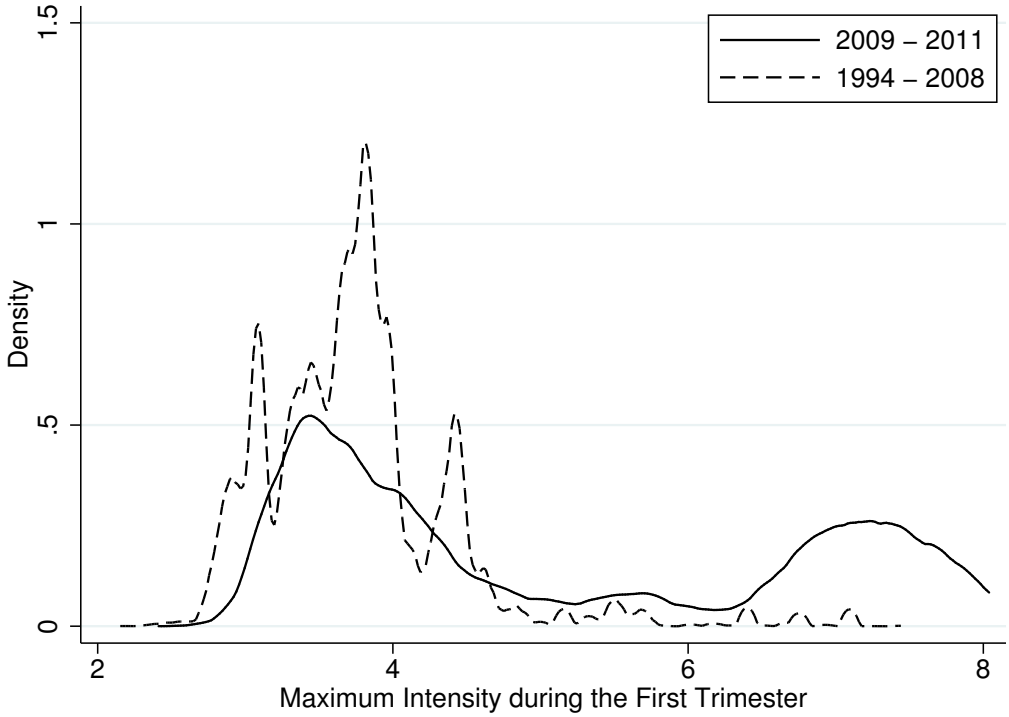
For the rest of the paper, I restrict the data to non-multiple births, for which there are non-missing values of the relevant variables. I also exclude births in non-continental Chile, and I also restrict births to mothers aged 15 to 40. Table 3.1 presents summary statistics for the full sample and for the restricted year sample. It is clear from the table that there are no significant differences in the key variables. However, as I will show in section 3.3.2, the inclusion or exclusion of the births exposed to the 2010 earthquake greatly influences the point estimates.

[2]See for example: Camacho (2008), Currie and Rossin-Slater (2013), Persson and Rossin-Slater (2017), and Quintana-Domeque and Ródenas-Serrano (2017).

3.2.2 Earthquake Data

The earthquake data come from the USGS[3] Atlas of ShakeMaps for Selected Earthquakes (Allen et al., 2008). A *ShakeMap* is a graphical representation of the intensity of an earthquake over a certain geographic area around the epicentre of an earthquake. The inputs into a *ShakeMap* are instrumental readings of Peak Ground Acceleration, Peak Ground Velocity, and reports of damage from various sources. These are combined to calculate a Modified Mercalli Intensity (MMI). The most important characteristic of the MMI is that it is a measure of *intensity* as felt by the population. The MMI is a measure defined in roman numerals, from I to X. Earthquakes of MMI greater than VI are considered strong according to the spectral scale developed by Wald et al. (1999).

Figure 3.1: Density of Maximum Earthquake Intensity during the First Trimester of Pregnancy



To assign an intensity measure for each location, I start by selecting any

[3]The US Geological Survey (USGS) is a government agency of the United States federal government which is part of the Department of the Interior. One of its roles is to monitor earthquake activity around the world. The data used in this paper is available at https://earthquake.usgs.gov/data/shakemap/.

earthquakes since the 1th of January, 1990 with an epicentre in a rectangle around the Chilean continental territory. This procedure yields 103 potential earthquakes, of which 5 are dropped because their *ShakeMaps* do not intersect with the Chilean territory. Using the remaining 97 earthquakes, I then calculate a set of summary statistics (mean, maximum, minimum, standard deviation, range) of the intensity perceived in each location in Chile, where the location is defined as either the urban area or the rural area of a county.[4]

I focus on the zonal mean of the MMI in a location. The underlying assumption is that the population is distributed uniformly within a location's polygon. Thus, each birth has a vector of earthquake intensities associated for each trimester. For instance, I consider the urban area of the municipality of Antofagasta, in the north of Chile. On the 14th of November, 2007, an earthquake of magnitude 7.7 struck 160km north of the city, but the intensity felt in the urban area of Antofagasta was approximately 6.4 in the MMI. This earthquake was followed by three aftershocks during that week (46th week of 2007) with intensities of 3.8, 4.2, and 3.8. One week later, another aftershock was felt with an intensity of 3.6, and then three more aftershocks were felt on week 50 of 2007, with intensities of 3.8, 4.0, and 4.4. Hence, for a birth conceived during week 45 of 2007 by a mother residing in the urban area of Antofagasta, the vector of earthquake intensities for the first trimester of pregnancy (ending on week six of 2008) would be given by $\mathbf{q}_{Antofagasta(Urban),1} = [3.8, 4.2, 3.8, 3.6, 3.8, 4.0, 4.4]$.

As mentioned in the previous section, the 2010 earthquake is a key event that affects the robustness of the point estimates. The fact that the earthquake affected the part of Chile with the highest population densities, and that the intensity was so strong, can affect estimates in two ways. First, the strength of the shock adds variation to the treatment, reducing noise. Second, the strength of the earthquake can trigger different mechanisms by which birth outcomes are affected. To illustrate the difference in the distribution of treatment caused by the 2010 earthquake, in figure 3.1 I plot the density of the maximum intensity felt in the mother's location of residence during the first trimester of pregnancy, by conception year range. The figure clearly shows that for births conceived between 2009 and 2011 the distribution of the maximum intensity during the first trimester is bimodal, with a large density around intensity level VII.

[4]The dimensions of one cell in a *ShakeMap* raster are approximately 2.6 by 2.6 km, and the smallest urban location has an area of approximately 0.05 km^2. To be able to calculate zonal statistics for the smallest polygons I adjust the `CellSize` environment setting in ArcGIS to the diameter of a circle with area equal to 0.05 km^2.

3.3 Empirical Framework

In this section, I describe the empirical framework that I will use to estimate the impact of earthquakes on birth outcomes in Chile. In sub-section 3.3.1 I describe the benchmark linear model, which is similar to the one used in Quintana-Domeque and Ródenas-Serrano (2017). Then, in section 3.3.2 I describe the series of robustness checks that I carry out: first, I show that varying the sample years greatly affects estimates; and second, I show that estimating semi-parametric and non-linear models does not reduce the sensibility of estimates caused by varying the sample years.

3.3.1 Baseline Linear Model

The point of departure is a linear relationship between birth outcomes and the number of earthquakes of different intensities during each trimester of pregnancy. Equation (3.1) shows this baseline linear model that will yield the benchmark estimates. $Y_{i,l,w}$ denotes an outcome for birth i, with mother's residence in location l, conceived in week w (weeks since 1960 week one). The two birth outcomes I focus on are birth weight measured in grams and weeks of gestation. The baseline model is thus given by:

$$Y_{i,l,w} = \lambda_l + \omega_w + \sum_{j=0}^{4} \sum_{m=0}^{3} \beta_j^{(m)} N_{l,j}^{(m)} + \mathbf{z}_i' \gamma + \varepsilon_{i,l,w} \tag{3.1}$$

where λ_l is a location fixed effect, ω_w is a week of conception fixed effect, $\mathbf{z}_i$ is a vector of the mother's characteristics, and $N_{l,j}^{(m)}$ denotes the number of earthquakes felt with intensity category m in mother's residence location l, during the trimester of pregnancy j.

The trimesters are indexed by $j = 0, 1, \ldots, 4$, where $j = 0$ denotes the thirteen weeks up to the week of conception (i.e. gestational weeks -12 to 0), $j = 1, 2, 3$ denote the trimester of pregnancy (i.e. weeks 1 to 13, 14 to 26, and 27 to 39, respectively), and $j = 4$ denotes the trimester after the *predicted* birth week (i.e. weeks 40 to 52). The intensity categories $m = 1, 2, 3$ correspond to Modified Mercalli Intensity categories I-III, IV-V, and VI or greater, while $m = 0$ denotes earthquakes that were not felt in location l. The error term in equation (3.1), $\varepsilon_{i,l,w}$, is allowed to be correlated freely within a location but are assumed to be independent across locations. Therefore, I estimate the standard errors clustered at the location level.

The coefficients of interest are the $\beta_j^{(m)}$. These indicate the effect of an additional earthquake of intensity category m if it was felt during trimester j. In

this paper, I do not transform the dependent variables, so the units of the $\beta_j^{(m)}$ correspond to the unit of measurement of the dependent variable (grams for birth weight and weeks for gestational length). It is expected that $\beta_4^{(m)}$ should have no effect on birth outcomes because earthquakes that occur after birth should have no effect. However, given that trimester four is calculated based on a theoretical 39 week pregnancy, it could be the case that for some children the actual gestational length is longer than 39 weeks. On the other hand, $\beta_0^{(m)}$ to $\beta_3^{(m)}$ are expected to be either not significant or negative and statistically significant, and the coefficients to have a stronger impact for higher levels of m. Even though $\beta_0^{(m)}$ is the coefficient for earthquakes during the trimester *before* conception, it is not expected to have no effect since it could be the case that earthquakes during this period have a lagged effect on birth outcomes through an effect on the mother.

I estimate the coefficients in equation (3.1) using ordinary least squares.[5] Identification relies on the assumption that, conditional on location and week of conception fixed effects, as well as the controls in $\mathbf{z}_i$, there is no correlation between the number of earthquakes and the error term $\varepsilon_{i,l,w}$. Given that earthquakes are unexpected both in their location and timing, it is highly unlikely that there are any anticipation effects that could bias results. However, estimation of equation (3.1) corresponds to a reduced form estimation, and to the extent there are any effects, I cannot isolate the acute stress mechanism.

Nevertheless, there is a major weakness of the method used in this paper, and it is caused by the fact that the database only contains data for *live* births. This is a serious source of sample selection, that is very likely to bias results. If, for instance, earthquakes cause higher rates of miscarriages, this is not identified in the dataset, and the effects of earthquakes on birth weight will be biased upwards.

In most specifications, I control for a mother's characteristics in $\mathbf{z}_i$. These controls are: mother's age and age squared, four indicator variables for four levels of education (the omitted category is no primary education), civil status of the mother (the omitted category is married), labour force status of the mother (default is inactive), and a set of three variables describing the birth experience of the mother: number of other children the mother has had that are still alive, number of other children the mother has had that are dead, and the number of stillborn children the mother has had.

This section has described a linear model that relates a mother's experience with earthquakes during pregnancy with birth outcomes. In sub-section 3.3.2, I

[5] All models are estimated in Stata using the custom command `reghdfe`, which allows for the fast estimation of models with high dimensions of fixed effects. See Correia (2016).

detail how I relax the linear assumption to include semi-parametric and non-linear parametric models.

3.3.2 Procedures for Robustness Checks

In this subsection, I detail the procedures I employ to check the robustness of the estimates I find using the benchmark model in equation (3.1). The robustness checks can be categorised into three groups: heterogeneous effects according to a birth's firstborn status, sensibility of results to the inclusion/exclusion of births affected by a 27th of February, 2010 earthquake of magnitude 8.8 off the coast of south-central Chile, and robustness to non-linear functional forms of the relationship between earthquakes and birth outcomes.

The first group of procedures entail re-estimating the benchmark model for different sub-samples, for which there is no need to detail a new estimation model. The first procedure is to estimate the model in equation (3.1) for different samples according to a birth's firstborn status. If a birth is a firstborn, then the total number of births by the mother is going to be equal to one. When I condition on this value the controls for the number of births alive, dead, and stillborn are automatically dropped from the vector $\mathbf{z}_i$. The second procedure excludes all births conceived after 2008. If a birth was affected during trimester four (i.e., $j = 4$, after the 39 weeks of gestation) by the 27th of February, 2010 earthquake, then the earliest it could be conceived was 52 weeks before, during February 2009. I cut-off at week 52 of 2008 for simplicity and straightforwardness. As I show in section 3.4, the results of estimating the benchmark model are very sensitive to the inclusion or exclusion of data for births that experienced the 2010 earthquake.

The second type of robustness checks entails re-estimating the model with a different functional form. In particular, I look at two new specifications: defining the treatment as indicator variables for experiencing *any* earthquake of intensity category m, and replacing the treatment variable using a continuous measure in a non-linear function. Equation (3.2) is a generalized version of equation (3.1), where the relationship between the outcome $Y_{i,l,w}$ and the magnitude of the earthquakes in location l during trimester j is given by a function $\Gamma_j[\cdot]$, which is allowed to vary by trimester. In its most general form, the input of this function is the vector of intensities felt in location l during trimester j, denoted by the vector $\mathbf{q}_{l,j}$.

$$Y_{i,l,w} = \lambda_l + \omega_w + \sum_{j=0}^{4} \Gamma_j[\mathbf{q}_{l,j}] + \mathbf{z}_i'\gamma + \varepsilon_{i,l,w} \tag{3.2}$$

It is clear that for the benchmark model in equation (3.1), the definition of the functional form $\Gamma[\cdot]$ is given by:

$$\Gamma_j[\mathbf{q}_{l,j}] = \sum_{m=0}^{3} \beta_j^{(m)} \cdot N_{l,j}^{(m)} \tag{3.3}$$

As a robustness check, I redefine $\Gamma[\cdot]$ to be an indicator variable that takes the value one when at least *one* earthquake of a given intensity m is experienced in a location l during trimester j. Hence, the term $\Gamma_j[\mathbf{q}_{l,j}]$ becomes:

$$\Gamma_j[\mathbf{q}_{l,j}] = \sum_{m=1}^{3} \beta_j^{(m)} \cdot I\left[N_{l,j}^{(m)} > 0\right] \tag{3.4}$$

The input into the indicator functions $I[\cdot]$ are the number of earthquakes felt in location l during trimester j of magnitude m. For example, $N_{l,j}^{(1)}$ is the number of earthquakes felt with intensity category I to III in location l during trimester j (i.e. low intensity earthquakes which are categorised as "not felt" to "moderate" in perceived shaking according to the scale in Allen et al. (2008) and Wald et al. (1999)).[6] The advantage of this definition of $\Gamma[\cdot]$ is that it allows for non-linearities in a semi-parametric way.

A second alternative functional form analysed is given by a non-linear function of the intensities felt by the mother during a given trimester. In this case, the functional form I use is a quartic polynomial of the maximum intensity felt during trimester j. Let $q_{l,j}^{max}$ denote the maximum intensity felt in location l during trimester j. Then, $\Gamma[\cdot]$ is given by:

$$\Gamma[\mathbf{q}_{l,j}] = \phi_{1,j} \cdot q_{l,j}^{max} + \phi_{2,j} \cdot (q_{l,j}^{max})^2 + \phi_{3,j} \cdot (q_{l,j}^{max})^3 + \phi_{4,j} \cdot (q_{l,j}^{max})^4 \tag{3.5}$$

The coefficients in equation (3.5) can be estimated using traditional OLS, and then these values can be used to estimate a marginal effect for different levels of $q_{l,j}^{max}$.

I use the maximum intensity felt during the trimester as an input in definition (3.5) because of two reasons. First, the maximum intensity has greater variation than the mean[7], and second, averaging across earthquakes during a time period can greatly affect the interpretation of the results. The main question of interest in this paper is whether perinatal outcomes are different for births of mothers who expe-

[6] Note however, that the summation in definition (3.4) starts at $m = 1$. This is because when including conception week fixed effects, the variable $N_{l,j}^{(0)}$ becomes collinear.

[7] For example, for women that felt an earthquake during the first trimester, the standard deviation of the maximum intensity during the first trimester was approximately 1.2, while the standard deviation of the mean intensity was 0.54.

rienced an earthquake versus those who did not. Averaging across a time period can make two very different experiences look the same although they are not: a mother who experiences four earthquakes of intensity 4 will have the same average as a mother who experiences 2 earthquakes of intensity 1 and 7. Clearly these two experiences are very different, and the mean hides those differences.

Comparing the first definition of $\Gamma_j[\cdot]$, given by equation (3.4), to the second definition in (3.5), it is clear that the former is more flexible. However, in terms of parameter saturation there is no advantage to either definition. Both entail the estimation of twenty coefficients: five trimesters, four parameters per trimester. Nevertheless, definition (3.5) makes use of the detailed raster data in a relatively smoother manner.

In this section I have detailed the procedures that I use to analyse the heterogeneity and robustness of the baseline model. In the next section, I show how heterogeneity across firstborn status plays a significant role, and how non-linear functional forms yield similar results to the baseline model.

3.4 Results

In this section, I present the results from estimating the models discussed in section 3.3. I begin by describing the results of the baseline linear model introduced in section 3.4.1. I then discuss how the effects of earthquakes on birth outcomes vary according to firstborn status.

3.4.1 Baseline Linear Model

In this section I describe the results of estimating the model in equation (3.1) by OLS, for the dependent variables birth weight and gestational length. As discussed earlier in section 3.3.1, if the channel by which earthquakes affect birth weight is through reduced gestational length because of stress, then the reduced form results should show that the number of earthquakes during pregnancy reduces birth weight and gestational length. However, I find that the effect of earthquakes on birth weight and gestational length only fits this pattern when excluding a the variable for low intensity earthquakes (i.e. $N^(m)_{l,j}$ when $m = 1$).

Table 3.2 shows the results of estimating the model in equation (3.1). The first three columns correspond to results when the dependent variable is birth weight, and the second three columns for when it is weeks of gestation. Columns (1) and (4) correspond to estimating equation (3.1) including only the variables corresponding to the number of high intensity earthquakes. Columns (2) and (5) show the results

Table 3.2: Effect of Earthquakes on Birth Outcomes, Baseline Linear Model (Equation (3.1))

	(1) Birth Weight	(2) Birth Weight	(3) Birth Weight	(4) Weeks of Gestation	(5) Weeks of Gestation	(6) Weeks of Gestation
$N_{l,0}^{(High)}$	5.219	0.539	9.709	0.0231	−0.00613	0.00779
	(4.980)	(5.121)	(5.909)	(0.0278)	(0.0256)	(0.0265)
$N_{l,1}^{(High)}$	−6.701	−13.47**	−7.161	0.0232	−0.00493	0.00346
	(6.131)	(6.502)	(8.049)	(0.0247)	(0.0227)	(0.0261)
$N_{l,2}^{(High)}$	−6.650	−12.68**	−6.403	0.0267	0.00292	−0.00269
	(4.715)	(5.068)	(5.445)	(0.0233)	(0.0238)	(0.0243)
$N_{l,3}^{(High)}$	−1.888	−7.166	−2.099	0.0239	−0.000472	0.00950
	(5.455)	(5.696)	(5.241)	(0.0263)	(0.0262)	(0.0243)
$N_{l,4}^{(High)}$	−3.384	−7.407	−7.527	0.0118	−0.00451	−0.00529
	(5.000)	(4.782)	(4.884)	(0.0236)	(0.0220)	(0.0197)
$N_{l,0}^{(Med.)}$		1.471	1.773		0.0132***	0.0135***
		(1.611)	(1.615)		(0.00443)	(0.00436)
$N_{l,1}^{(Med.)}$		3.780**	3.899***		0.0157***	0.0155***
		(1.475)	(1.489)		(0.00452)	(0.00453)
$N_{l,2}^{(Med.)}$		3.539***	3.723***		0.0133***	0.0133***
		(1.272)	(1.288)		(0.00492)	(0.00510)
$N_{l,3}^{(Med.)}$		3.276**	3.268**		0.0156***	0.0159***
		(1.382)	(1.424)		(0.00486)	(0.00487)
$N_{l,4}^{(Med.)}$		2.872*	2.378		0.0116**	0.0108**
		(1.468)	(1.555)		(0.00463)	(0.00467)
$N_{l,0}^{(Low)}$			−1.354*			−0.00207
			(0.785)			(0.00262)
$N_{l,1}^{(Low)}$			−0.902			−0.00145
			(0.601)			(0.00253)
$N_{l,2}^{(Low)}$			−0.943*			0.00112
			(0.567)			(0.00264)
$N_{l,3}^{(Low)}$			−0.829			−0.00169
			(0.606)			(0.00219)
$N_{l,4}^{(Low)}$			0.0994			0.000256
			(0.562)			(0.00201)
Observations	4014038	4014038	4014038	4014038	4014038	4014038
Counties	655	655	655	655	655	655
Weeks	884	884	884	884	884	884
Clusters	655	655	655	655	655	655

Standard errors clustered at the location level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

when I include the variables only for the medium and high intensity earthquakes. Lastly, columns (3) and (6) present the results for when variables for all intensity earthquakes are included.

Focusing first on column (1), although no point estimate is statistically different from zero, the pattern of coefficients matches the expectations laid out in section 3.3.1. I find negative coefficients for trimesters one, two, and three, with the third trimester being significantly smaller than the first two. The fourth trimester coefficient is -3.4, which although not statistically significant, is relatively still quite large compared to the other coefficients. Moreover, the sign for the coefficient during the pre-conception trimester is positive, and the magnitude is quite large, even though it is statistically insignificant. These results are at best weak evidence of a negative effect on birth weight. Moreover, column (4) shows point estimates that are all positive and of approximately the same order of magnitude. It is at odds with earthquake-stress-weeks of gestation mechanism that the coefficients in column (4) are positive.

Nevertheless, when controlling for additional measures of earthquakes, I find some evidence supporting at least a negative effect of earthquakes. Column (2) shows that when introducing variables for the medium intensity earthquakes, the coefficients for the high intensity earthquakes become statistically significant and large for the first and second trimesters of pregnancy. However, the coefficients for medium intensity earthquakes are *positive*, and statistically significant at least at the 10 percent significance level for all trimesters except the pre-conception trimester. Column (5) shows that some coefficients for the high intensity earthquakes become negative, but still not significant. Also in column (5), the coefficients for the medium intensity earthquakes are all statistically significant and *positive*. Columns (3) and (6) additionally include variables for all earthquakes, and the magnitude of coefficients for the high intensity earthquakes reverts to the sizes of column (1) for birth weight. The positive effect of medium intensity earthquakes on birth weight and weeks of gestation is robust.

To make sense of the results presented in table 3.2, it is important to note that there is a high correlation between the earthquake variables *across* intensities. For instance, when looking at the number of earthquakes during the first trimester, the variable for high intensity earthquakes is positively correlated with both medium and low intensity, with correlations of 0.57 and 0.81 respectively. I conjecture that including low intensity earthquakes is a case of the inclusion of "bad controls." My reasoning is that because earthquakes cause a series of relatively low intensity aftershocks, then the effects of the large earthquake is split between high and low

intensity earthquakes in column (3).

On the other hand, medium intensity earthquakes can be proper independent events *or* aftershocks of larger earthquakes. If independent medium intensity earthquakes also have a negative effect on birth weight, then their exclusion in column (1) is the case of an omitted relevant variable. However, this does not explain the positive coefficients. One possible explanation is that the channels by which medium and large earthquakes affect the biology of pregnancy are different. The fact that the effect is consistently positive across specifications for both birth weight and weeks of gestation suggests this is a robust result. Tables 3A.1 and 3A.2 show more detailed results, including regressions of the intensity of earthquakes one set at a time. Columns (2), (4), and (5) in both tables show that the positive effect of medium intensity earthquakes on birth weight and weeks of gestation is quite robust.

3.4.2 Inclusion/Exclusion of the February 2010 Earthquake

The 27th of February, 2010 earthquake off the coast of south-central Chile had a magnitude of 8.8 Mww (Duputel et al., 2012), which caused a tsunami that wreaked havoc on the coast (CEPAL, 2010; Contreras and Winckler, 2013; Subsecretaría del Interior de Chile, 2011). The particularly high magnitude and intensity of this earthquake disrupted logistics in a highly populated part of Chile. As such, it is important to determine if the results of the baseline model hold for births unaffected by this earthquake. I thus re-estimate the model for a restricted conception year range ending in 2008, as explained in section 3.3.2.

Table 3.3 compares the estimates for the full and restricted year ranges, when the dependent variable is birth weight, and table 3.4 is the analogous counterpart for when the dependent variable is weeks of gestation. The first three columns in each table are identical to the columns of tables 3.2, correspondingly. The second three columns contain the estimates for the baseline model restricted to conception years up to 2008. As is clear in the table, the results are not consistent across year ranges for either dependent variable. Nevertheless, the relationship between gestational length and birth weight is consistent. In table 3.3, for the 1994-2008 year range, the high intensity earthquakes have a strong positive effect on birth weight when they strike during the pre-conception trimester, and a strong negative effect when they strike during the post-birth trimester. This is consistent with the positive (albeit marginally significant) effect on weeks of gestation during the pre-conception trimester and the negative effect during the fourth trimester (see columns (4)-(6) in both tables).

Table 3.3: Effect of Earthquakes on Birth Weight, by Year Range, Baseline Linear Model (Equation (3.1), detailed)

	1994 - 2010			1994 - 2008		
	(1) Birth Weight	(2) Birth Weight	(3) Birth Weight	(4) Birth Weight	(5) Birth Weight	(6) Birth Weight
$N_{l,0}^{(High)}$	5.219	0.539	9.709	21.64***	21.30**	22.69**
	(4.980)	(5.121)	(5.909)	(7.784)	(8.926)	(9.042)
$N_{l,1}^{(High)}$	−6.701	−13.47**	−7.161	−11.01	−12.30	−11.21
	(6.131)	(6.502)	(8.049)	(7.240)	(9.573)	(9.155)
$N_{l,2}^{(High)}$	−6.650	−12.68**	−6.403	−2.408	−5.617	−1.951
	(4.715)	(5.068)	(5.445)	(7.605)	(8.158)	(7.956)
$N_{l,3}^{(High)}$	−1.888	−7.166	−2.099	−3.604	−4.691	−2.575
	(5.455)	(5.696)	(5.241)	(5.315)	(5.606)	(5.680)
$N_{l,4}^{(High)}$	−3.384	−7.407	−7.527	−11.13***	−13.95***	−14.33***
	(5.000)	(4.782)	(4.884)	(3.520)	(3.968)	(3.737)
$N_{l,0}^{(Med.)}$		1.471	1.773		0.354	0.224
		(1.611)	(1.615)		(1.980)	(2.007)
$N_{l,1}^{(Med.)}$		3.780**	3.899***		0.0504	0.0420
		(1.475)	(1.489)		(3.754)	(3.724)
$N_{l,2}^{(Med.)}$		3.539***	3.723***		2.769	2.987
		(1.272)	(1.288)		(2.701)	(2.520)
$N_{l,3}^{(Med.)}$		3.276**	3.268**		0.224	0.430
		(1.382)	(1.424)		(2.288)	(2.319)
$N_{l,4}^{(Med.)}$		2.872*	2.378		2.632	2.738
		(1.468)	(1.555)		(2.844)	(2.863)
$N_{l,0}^{(Low)}$			−1.354*			−1.091
			(0.785)			(2.711)
$N_{l,1}^{(Low)}$			−0.902			−0.725
			(0.601)			(1.405)
$N_{l,2}^{(Low)}$			−0.943*			−3.680*
			(0.567)			(2.145)
$N_{l,3}^{(Low)}$			−0.829			−2.065
			(0.606)			(1.606)
$N_{l,4}^{(Low)}$			0.0994			0.534
			(0.562)			(1.890)
Observations	4014038	4014038	4014038	3538628	3538628	3538628
Counties	655	655	655	655	655	655
Weeks	884	884	884	780	780	780
Clusters	655	655	655	655	655	655

Standard errors clustered at the location level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.4: Effect of Earthquakes on Weeks of Gestation, by Year Range, Baseline Linear Model (Equation (3.1), detailed)

	1994 - 2010			1994 - 2008		
	(1) Weeks of Gestation	(2) Weeks of Gestation	(3) Weeks of Gestation	(4) Weeks of Gestation	(5) Weeks of Gestation	(6) Weeks of Gestation
$N_{l,0}^{(High)}$	0.0231	−0.00613	0.00779	0.0310*	0.0242	0.0370
	(0.0278)	(0.0256)	(0.0265)	(0.0173)	(0.0211)	(0.0280)
$N_{l,1}^{(High)}$	0.0232	−0.00493	0.00346	−0.0108	−0.0293	−0.0156
	(0.0247)	(0.0227)	(0.0261)	(0.0375)	(0.0352)	(0.0394)
$N_{l,2}^{(High)}$	0.0267	0.00292	−0.00269	−0.0201	−0.0290	−0.0170
	(0.0233)	(0.0238)	(0.0243)	(0.0173)	(0.0217)	(0.0281)
$N_{l,3}^{(High)}$	0.0239	−0.000472	0.00950	−0.0114	−0.0127	−0.00102
	(0.0263)	(0.0262)	(0.0243)	(0.0228)	(0.0279)	(0.0370)
$N_{l,4}^{(High)}$	0.0118	−0.00451	−0.00529	−0.0307***	−0.0414**	−0.0399**
	(0.0236)	(0.0220)	(0.0197)	(0.00825)	(0.0176)	(0.0163)
$N_{l,0}^{(Med.)}$		0.0132***	0.0135***		0.00396	0.00423
		(0.00443)	(0.00436)		(0.00797)	(0.00736)
$N_{l,1}^{(Med.)}$		0.0157***	0.0155***		0.0129	0.0131
		(0.00452)	(0.00453)		(0.00949)	(0.00954)
$N_{l,2}^{(Med.)}$		0.0133***	0.0133***		0.00764	0.00843
		(0.00492)	(0.00510)		(0.0105)	(0.00999)
$N_{l,3}^{(Med.)}$		0.0156***	0.0159***		−0.00132	0.000331
		(0.00486)	(0.00487)		(0.0108)	(0.00956)
$N_{l,4}^{(Med.)}$		0.0116**	0.0108**		0.0107	0.0120
		(0.00463)	(0.00467)		(0.0118)	(0.0123)
$N_{l,0}^{(Low)}$			−0.00207			−0.0112
			(0.00262)			(0.00980)
$N_{l,1}^{(Low)}$			−0.00145			−0.0125
			(0.00253)			(0.00844)
$N_{l,2}^{(Low)}$			0.00112			−0.0109
			(0.00264)			(0.0100)
$N_{l,3}^{(Low)}$			−0.00169			−0.0114
			(0.00219)			(0.00859)
$N_{l,4}^{(Low)}$			0.000256			−0.000581
			(0.00201)			(0.0105)
Observations	4014038	4014038	4014038	3538628	3538628	3538628
Counties	655	655	655	655	655	655
Weeks	884	884	884	780	780	780
Clusters	655	655	655	655	655	655

Standard errors clustered at the location level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The main take-away of this exercise is to highlight the sensitivity of results to the inclusion or exclusion of the February 2010 earthquake. On the one hand, this event helps identification because the strong magnitude of this earthquake and the fact that is struck off the coast of the most highly populated part of Chile increases the treated population and lowers standard errors. However, the fact that it was so intense caused significant infrastructure damage, and the Chilean government devoted significant resources to aid the population. Thus, the births affected by this earthquake were not only affected by the channels discussed in other literature, but through changes in infrastructure, public health, and government aid efforts.

Tables 3A.3, 3A.4, and 3A.5 are analogous to the results shown in tables 3.2, 3A.1, and 3A.2, but for the 1994-2008 sample. A couple of relatively robust results arise: there is a positive effect of strong earthquakes on birth weight during the pre-conception trimester ($j = 4$) and a negative effect during the fourth trimester. Moreover, the point estimates during trimesters one to three, although statistically insignificant, exhibit a pattern that is consistent with the hypothesis that earthquakes have a negative impact on birth weight when they hit during the early stages of pregnancy. This pattern is consistent with the results that Torche (2011) found for the 2005 earthquake in Iquique, but the magnitudes of the coefficients that she obtains are significantly larger than those that I find.

3.4.3 Heterogeneity by Firstborn Status

Having discussed the role of the 27th of February, 2010 earthquake, I now show the results of estimating the baseline model by firstborn status. The objective of this exercise is to show how heterogeneous the coefficients estimated in the previous sections can be.

Table 3.5 shows the results of estimating the baseline model using the full range of conception years possible (1994 - 2010), by dependent variable and firstborn status. Table 3A.6 shows the analogous results for the case when I exclude births conceived after 2008. The most remarkable aspect of both tables is how the estimates for the high intensity earthquakes during the pre-conception trimester and the post-birth trimester change in sign, magnitude, and statistical significance when considering firstborns versus non-firstborns. These results suggest that, at least for high intensity earthquakes, it is the non-firstborns that are driving the change between including and excluding the births affected by the February 2010 earthquake.

Table 3.5: Effect of Earthquakes on Birth Outcomes, Baseline Linear Model (Equation (3.1)), Conception Years 1994 - 2010, by Outcome and Firstborn Status

	Birth Weight			Weeks of Gestation		
	(1) First	(2) Other	(3) All	(4) First	(5) Other	(6) All
$N_{l,0}^{(High)}$	−5.739	20.70**	9.709	−0.0204	0.0289	0.00779
	(8.498)	(8.685)	(5.909)	(0.0386)	(0.0333)	(0.0265)
$N_{l,1}^{(High)}$	−11.80	−2.828	−7.161	−0.00378	0.0113	0.00346
	(8.753)	(9.828)	(8.049)	(0.0431)	(0.0213)	(0.0261)
$N_{l,2}^{(High)}$	−3.388	−8.527	−6.403	0.0221	−0.0234	−0.00269
	(6.738)	(6.881)	(5.445)	(0.0329)	(0.0246)	(0.0243)
$N_{l,3}^{(High)}$	0.438	−3.648	−2.099	0.00813	0.00951	0.00950
	(8.293)	(6.637)	(5.241)	(0.0331)	(0.0285)	(0.0243)
$N_{l,4}^{(High)}$	−7.112	−8.657	−7.527	−0.00825	−0.00319	−0.00529
	(8.005)	(6.374)	(4.884)	(0.0307)	(0.0212)	(0.0197)
$N_{l,0}^{(Med.)}$	4.144*	−0.0235	1.773	0.0116*	0.0148**	0.0135***
	(2.132)	(2.075)	(1.615)	(0.00659)	(0.00618)	(0.00436)
$N_{l,1}^{(Med.)}$	4.181**	3.534**	3.899***	0.00939	0.0198***	0.0155***
	(2.051)	(1.686)	(1.489)	(0.00767)	(0.00415)	(0.00453)
$N_{l,2}^{(Med.)}$	2.391	4.685***	3.723***	0.00135	0.0232***	0.0133***
	(1.503)	(1.664)	(1.288)	(0.00643)	(0.00585)	(0.00510)
$N_{l,3}^{(Med.)}$	0.0539	5.635***	3.268**	0.00640	0.0236***	0.0159***
	(1.675)	(1.837)	(1.424)	(0.00607)	(0.00618)	(0.00487)
$N_{l,4}^{(Med.)}$	0.555	3.773**	2.378	0.00740	0.0135***	0.0108**
	(1.971)	(1.651)	(1.555)	(0.00662)	(0.00496)	(0.00467)
$N_{l,0}^{(Low)}$	−0.699	−1.866*	−1.354*	0.000394	−0.00397	−0.00207
	(0.892)	(0.978)	(0.785)	(0.00332)	(0.00324)	(0.00262)
$N_{l,1}^{(Low)}$	−1.169	−0.757	−0.902	−0.00108	−0.00191	−0.00145
	(0.715)	(0.719)	(0.601)	(0.00352)	(0.00234)	(0.00253)
$N_{l,2}^{(Low)}$	−0.255	−1.559**	−0.943*	0.00555*	−0.00273	0.00112
	(0.779)	(0.674)	(0.567)	(0.00304)	(0.00321)	(0.00264)
$N_{l,3}^{(Low)}$	−0.356	−1.384*	−0.829	0.00283	−0.00555**	−0.00169
	(0.729)	(0.744)	(0.606)	(0.00288)	(0.00251)	(0.00219)
$N_{l,4}^{(Low)}$	0.702	−0.391	0.0994	0.00293	−0.00185	0.000256
	(0.905)	(0.684)	(0.562)	(0.00273)	(0.00248)	(0.00201)
Observations	1733052	2280966	4014038	1733052	2280966	4014038
Counties	637	648	655	637	648	655
Weeks	884	884	884	884	884	884
Clusters	637	648	655	637	648	655

Standard errors clustered at the location level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

3.4.4 Semi-Parametric and Non-Linear Functional Forms

Having established the sensitivity of the estimates to different sub-samples, I now discuss the effects that changing the assumption about the functional form of $\Gamma_j[\cdot]$ has on the treatment effects. The objective of using a semi-parametric model is to allow for a non-linear effect when estimating the effect of *any* earthquake in each size category. Thus, I abstract from the issue of aftershocks, since the absolute number of earthquakes does not affect the value of the treatment (i.e. ten aftershocks of medium intensity count the same as one aftershock or earthquake of medium intensity).

Table 3.6 shows the results when estimating equation (3.2) with definition 3.4, for the full sample with years from 1994 to 2010, by dependent variable. The results overall are very similar to those in table 3.2, but it is clear that the semi-parametric model has yielded stronger negative effects of high intensity earthquakes on birth weight during terms one and two: the corresponding estimates in column (3) are -11.3 and 10.8 respectively, which suggests that experiencing at least one high intensity earthquake causes birth weight to drop by approximately 11 grams if the earthquake is felt during the first or second trimester. However, these results are not accompanied by negative effect on weeks of gestation, which suggest that the mechanism is not through stress-lower weeks of gestation. The results for medium intensity earthquakes show robust positive effects for both birth weight and weeks of gestation. Low intensity earthquakes have no impact on either birth weight or weeks of gestation.

Table 3A.7 shows results analogous to table 3.6, but when restricting the conception years to 1994-2008, which excludes the February 2010 earthquake. The results are very similar to table 3A.3, where the estimate for the high intensity earthquakes hitting during the pre-conception trimester are strongly positive, and strongly negative if they hit during the fourth trimester. As was the case in table 3A.3, the pattern of point estimates for high intensity earthquakes during the pregnancy trimesters is consistent with the estimates in the literature, but not statistically significant. When it comes to the sensibility of results regarding whether the child is a firstborn or not, the results are qualitatively the same between tables 3A.6 and tables 3A.8.

The final exercise that I carry out is to redefine the functional form of the relationship between earthquakes and birth outcomes as a quartic polynomial on the strongest intensity that a mother experiences during a trimester of pregnancy. Figure 3.2 shows the results of this approach when using data for the entire sample and the dependent variable is birth weight, while figure 3.3 shows the results for

Table 3.6: Effect of Earthquakes on Birth Outcomes, Semi-Parametric Model (Equation (3.2), Definition (3.4)), Conception Years 1994-2010

	(1) Birth Weight	(2) Birth Weight	(3) Birth Weight	(4) Weeks of Gestation	(5) Weeks of Gestation	(6) Weeks of Gestation
$I\left[N_{l,0}^{(High)} > 0\right] = 1$	4.327	−0.253	−1.342	0.0228	−0.00546	−0.0127
	(5.153)	(4.626)	(4.477)	(0.0285)	(0.0226)	(0.0239)
$I\left[N_{l,1}^{(High)} > 0\right] = 1$	−6.470	−10.81	−11.31*	0.0229	−0.00284	−0.00485
	(6.271)	(6.653)	(6.863)	(0.0252)	(0.0224)	(0.0224)
$I\left[N_{l,2}^{(High)} > 0\right] = 1$	−5.741	−10.72**	−10.80**	0.0293	0.00965	0.00392
	(4.709)	(4.671)	(4.661)	(0.0235)	(0.0232)	(0.0226)
$I\left[N_{l,3}^{(High)} > 0\right] = 1$	−1.135	−5.921	−7.074	0.0262	0.00658	−0.00200
	(5.525)	(5.394)	(5.395)	(0.0264)	(0.0236)	(0.0233)
$I\left[N_{l,4}^{(High)} > 0\right] = 1$	−3.338	−6.489	−7.703*	0.0108	−0.00614	−0.0130
	(5.109)	(4.689)	(4.621)	(0.0242)	(0.0204)	(0.0199)
$I\left[N_{l,0}^{(Med.)} > 0\right] = 1$		3.757	3.508		0.0293***	0.0281***
		(2.310)	(2.294)		(0.0102)	(0.0103)
$I\left[N_{l,1}^{(Med.)} > 0\right] = 1$		4.013	4.036		0.0313***	0.0304***
		(3.094)	(3.150)		(0.0111)	(0.0115)
$I\left[N_{l,2}^{(Med.)} > 0\right] = 1$		6.529***	6.489***		0.0232**	0.0228**
		(2.178)	(2.316)		(0.00914)	(0.00948)
$I\left[N_{l,3}^{(Med.)} > 0\right] = 1$		7.425***	7.164***		0.0275***	0.0260***
		(2.051)	(2.093)		(0.00740)	(0.00792)
$I\left[N_{l,4}^{(Med.)} > 0\right] = 1$		5.687**	5.713**		0.0319***	0.0312***
		(2.461)	(2.476)		(0.0114)	(0.0119)
$I\left[N_{l,0}^{(Low)} > 0\right] = 1$			−0.234			0.00139
			(1.962)			(0.00669)
$I\left[N_{l,1}^{(Low)} > 0\right] = 1$			0.763			−0.000601
			(1.519)			(0.00613)
$I\left[N_{l,2}^{(Low)} > 0\right] = 1$			−0.446			0.00358
			(1.557)			(0.00639)
$I\left[N_{l,3}^{(Low)} > 0\right] = 1$			0.663			0.00661
			(1.445)			(0.00575)
$I\left[N_{l,4}^{(Low)} > 0\right] = 1$			1.992			0.0129**
			(1.609)			(0.00584)
Observations	4014038	4014038	4014038	4014038	4014038	4014038
Counties	655	655	655	655	655	655
Weeks	884	884	884	884	884	884
Clusters	655	655	655	655	655	655

Standard errors clustered at the location level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 3.2: Effect of Earthquakes on Birth Weight, Quartic Polynomial on Maximum Intensity during Trimester (Equation (3.2), Definition (3.5)), Conception Years 1994 - 2010, by Trimester

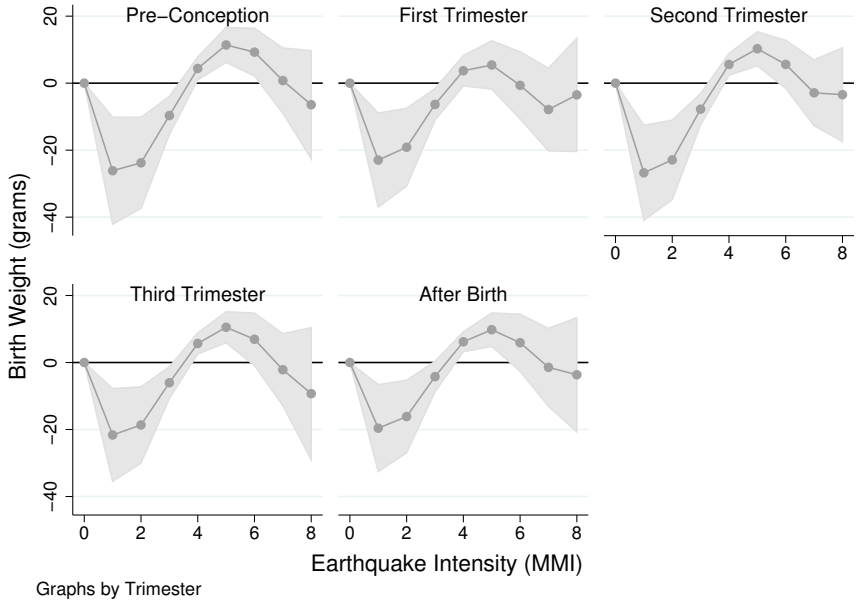
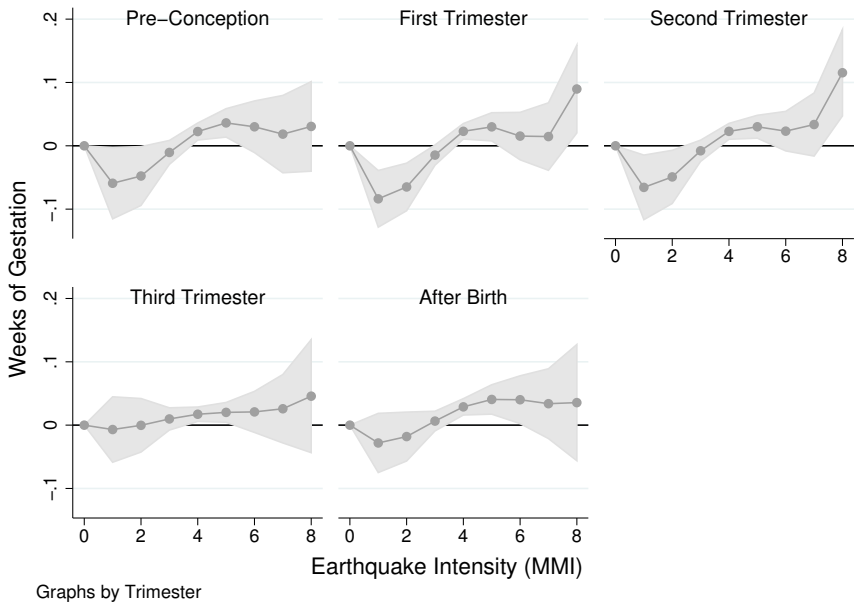


Figure 3.3: Effect of Earthquakes on Weeks of Gestation, Quartic Polynomial on Maximum Intensity during Trimester (Equation (3.2), Definition (3.5)), Conception Years 1994 - 2010, by Trimester



weeks of gestation. The figure plots the $\Gamma_j[\cdot]$ function against its argument, the maximum intensity felt in the mother's residence during trimester j. The graphs have been plotted by trimester of pregnancy. As is clear in the figures, the effect of earthquake intensity is not monotonic. In fact, it is shown that for all trimesters, the effect is most negative on birth outcomes when the maximum intensity is 1, which in the Modified Mercalli Intensity scale corresponds to earthquakes which are not felt. Observing the shape of the polynomials it is clear that given the results in the previous sections, a quartic polynomial is over-fitting the data. A linear model, not shown, resulted in a slightly downward slopping line that is never statistically different from zero.

The main takeaway of this section is that the results obtained with the baseline model in section 3.4.1 are not sensitive to functional form, but they are sensitive to the inclusion or exclusion of births affected by the February 2010 earthquake. Moreover, there is considerable heterogeneity of effects when splitting the sample by firstborn status.

3.5 Conclusion

The study of the effects of natural disasters on birth outcomes exploits variation across space, time, and stages of pregnancy for identification. The literature often uses a single event to carry out differences-in-differences estimation of the effects of a natural disaster or other environmental factors. The mechanism that has been advanced by the literature is that natural disasters are sources of acute stress, which in turn has physiological impacts on a pregnancy. Thus, the channel by which natural disasters are supposed to affect birth outcomes is through reducing gestational length.

In this paper, I focus on the effects of earthquakes. Unlike most of the literature on earthquakes and birth outcomes, I use more than a decade of data and analyse multiple earthquakes. I combine administrative data on more than four million live births in Chile for births conceived between 1994 and 2010, with earthquake data coming from the USGS in the form of *ShakeMaps*, which provide a detailed measure of the intensity of an earthquake in a given location. This earthquake data is superior to traditional measures of exposure based on distance to the epicentre because it combines geophysical data as well as intensity measures based on the Modified Mercalli Intensity scale.

Using this large dataset, I find that after controlling for location and week of conception fixed effects, the effect of earthquakes on birth outcomes is for the

most part insignificant or non-robust. In the baseline models, although I find statistically significant negative effects of high intensity earthquakes on birth weight in a particular specification, medium intensity earthquakes are shown to *increase* birth weight, although the point estimates are low. However, the baseline results are not robust to the exclusion of births affected by the 27th of February, 2010 earthquake off the coast of south-central Chile which claimed the lives of approximately 500 people and caused significant logistical disruption. I also showed that effects are heterogeneous across a birth's firstborn status. Using different functional forms to model the relationship between birth outcomes and earthquakes does not affect these conclusions.

The results in this paper do not support the mechanism most often discussed in the literature: earthquakes cause stress, which shortens gestational length and through that channel birth weight is reduced. To the extent that this mechanism is present, I have shown that it is not the dominant mechanism. Therefore, these results suggest that earthquakes might operate through multiple mechanisms so they do not satisfy the conditions necessary to identify the effects of stress on birth outcomes.

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3.A Additional Results

Table 3A.1: Effect of Earthquakes on Birth Weight, Baseline Linear Model (Equation (3.1), detailed)

	(1) Birth Weight	(2) Birth Weight	(3) Birth Weight	(4) Birth Weight	(5) Birth Weight
$N_{l,0}^{(High)}$	5.219			0.539	9.709
	(4.980)			(5.121)	(5.909)
$N_{l,1}^{(High)}$	−6.701			−13.47**	−7.161
	(6.131)			(6.502)	(8.049)
$N_{l,2}^{(High)}$	−6.650			−12.68**	−6.403
	(4.715)			(5.068)	(5.445)
$N_{l,3}^{(High)}$	−1.888			−7.166	−2.099
	(5.455)			(5.696)	(5.241)
$N_{l,4}^{(High)}$	−3.384			−7.407	−7.527
	(5.000)			(4.782)	(4.884)
$N_{l,0}^{(Med.)}$		1.924		1.471	1.773
		(1.411)		(1.611)	(1.615)
$N_{l,1}^{(Med.)}$		2.816**		3.780**	3.899***
		(1.194)		(1.475)	(1.489)
$N_{l,2}^{(Med.)}$		2.496**		3.539***	3.723***
		(1.176)		(1.272)	(1.288)
$N_{l,3}^{(Med.)}$		2.690**		3.276**	3.268**
		(1.235)		(1.382)	(1.424)
$N_{l,4}^{(Med.)}$		2.213		2.872*	2.378
		(1.363)		(1.468)	(1.555)
$N_{l,0}^{(Low)}$			−0.641		−1.354*
			(0.581)		(0.785)
$N_{l,1}^{(Low)}$			−0.805		−0.902
			(0.510)		(0.601)
$N_{l,2}^{(Low)}$			−0.818*		−0.943*
			(0.474)		(0.567)
$N_{l,3}^{(Low)}$			−0.490		−0.829
			(0.599)		(0.606)
$N_{l,4}^{(Low)}$			0.153		0.0994
			(0.517)		(0.562)
Observations	4014038	4014038	4014038	4014038	4014038
Counties	655	655	655	655	655
Weeks	884	884	884	884	884
Clusters	655	655	655	655	655

Standard errors clustered at the location level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3A.2: Effect of Earthquakes on Weeks of Gestation, Baseline Linear Model (Equation (3.1), detailed)

	(1) Weeks of Gestation	(2) Weeks of Gestation	(3) Weeks of Gestation	(4) Weeks of Gestation	(5) Weeks of Gestation
$N_{l,0}^{(High)}$	0.0231			−0.00613	0.00779
	(0.0278)			(0.0256)	(0.0265)
$N_{l,1}^{(High)}$	0.0232			−0.00493	0.00346
	(0.0247)			(0.0227)	(0.0261)
$N_{l,2}^{(High)}$	0.0267			0.00292	−0.00269
	(0.0233)			(0.0238)	(0.0243)
$N_{l,3}^{(High)}$	0.0239			−0.000472	0.00950
	(0.0263)			(0.0262)	(0.0243)
$N_{l,4}^{(High)}$	0.0118			−0.00451	−0.00529
	(0.0236)			(0.0220)	(0.0197)
$N_{l,0}^{(Med.)}$		0.0127***		0.0132***	0.0135***
		(0.00447)		(0.00443)	(0.00436)
$N_{l,1}^{(Med.)}$		0.0152***		0.0157***	0.0155***
		(0.00457)		(0.00452)	(0.00453)
$N_{l,2}^{(Med.)}$		0.0136***		0.0133***	0.0133***
		(0.00457)		(0.00492)	(0.00510)
$N_{l,3}^{(Med.)}$		0.0156***		0.0156***	0.0159***
		(0.00455)		(0.00486)	(0.00487)
$N_{l,4}^{(Med.)}$		0.0112**		0.0116**	0.0108**
		(0.00507)		(0.00463)	(0.00467)
$N_{l,0}^{(Low)}$			−0.000776		−0.00207
			(0.00236)		(0.00262)
$N_{l,1}^{(Low)}$			0.000674		−0.00145
			(0.00218)		(0.00253)
$N_{l,2}^{(Low)}$			0.00297		0.00112
			(0.00240)		(0.00264)
$N_{l,3}^{(Low)}$			0.00112		−0.00169
			(0.00247)		(0.00219)
$N_{l,4}^{(Low)}$			0.00215		0.000256
			(0.00244)		(0.00201)
Observations	4014038	4014038	4014038	4014038	4014038
Counties	655	655	655	655	655
Weeks	884	884	884	884	884
Clusters	655	655	655	655	655

Standard errors clustered at the location level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3A.3: Effect of Earthquakes on Birth Outcomes, Baseline Linear Model (Equation (3.1)), Conception Years 1994 - 2008

	(1) Birth Weight	(2) Birth Weight	(3) Birth Weight	(4) Weeks of Gestation	(5) Weeks of Gestation	(6) Weeks of Gestation
$N_{l,0}^{(High)}$	21.64***	21.30**	22.69**	0.0310*	0.0242	0.0370
	(7.784)	(8.926)	(9.042)	(0.0173)	(0.0211)	(0.0280)
$N_{l,1}^{(High)}$	−11.01	−12.30	−11.21	−0.0108	−0.0293	−0.0156
	(7.240)	(9.573)	(9.155)	(0.0375)	(0.0352)	(0.0394)
$N_{l,2}^{(High)}$	−2.408	−5.617	−1.951	−0.0201	−0.0290	−0.0170
	(7.605)	(8.158)	(7.956)	(0.0173)	(0.0217)	(0.0281)
$N_{l,3}^{(High)}$	−3.604	−4.691	−2.575	−0.0114	−0.0127	−0.00102
	(5.315)	(5.606)	(5.680)	(0.0228)	(0.0279)	(0.0370)
$N_{l,4}^{(High)}$	−11.13***	−13.95***	−14.33***	−0.0307***	−0.0414**	−0.0399**
	(3.520)	(3.968)	(3.737)	(0.00825)	(0.0176)	(0.0163)
$N_{l,0}^{(Med.)}$		0.354	0.224		0.00396	0.00423
		(1.980)	(2.007)		(0.00797)	(0.00736)
$N_{l,1}^{(Med.)}$		0.0504	0.0420		0.0129	0.0131
		(3.754)	(3.724)		(0.00949)	(0.00954)
$N_{l,2}^{(Med.)}$		2.769	2.987		0.00764	0.00843
		(2.701)	(2.520)		(0.0105)	(0.00999)
$N_{l,3}^{(Med.)}$		0.224	0.430		−0.00132	0.000331
		(2.288)	(2.319)		(0.0108)	(0.00956)
$N_{l,4}^{(Med.)}$		2.632	2.738		0.0107	0.0120
		(2.844)	(2.863)		(0.0118)	(0.0123)
$N_{l,0}^{(Low)}$			−1.091			−0.0112
			(2.711)			(0.00980)
$N_{l,1}^{(Low)}$			−0.725			−0.0125
			(1.405)			(0.00844)
$N_{l,2}^{(Low)}$			−3.680*			−0.0109
			(2.145)			(0.0100)
$N_{l,3}^{(Low)}$			−2.065			−0.0114
			(1.606)			(0.00859)
$N_{l,4}^{(Low)}$			0.534			−0.000581
			(1.890)			(0.0105)
Observations	3538628	3538628	3538628	3538628	3538628	3538628
Counties	655	655	655	655	655	655
Weeks	780	780	780	780	780	780
Clusters	655	655	655	655	655	655

Standard errors clustered at the location level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3A.4: Effect of Earthquakes on Birth Weight, Baseline Linear Model (Equation (3.1), detailed), Conception Years 1994 - 2008

	(1) Birth Weight	(2) Birth Weight	(3) Birth Weight	(4) Birth Weight	(5) Birth Weight
$N_{l,0}^{(High)}$	21.64***			21.30**	22.69**
	(7.784)			(8.926)	(9.042)
$N_{l,1}^{(High)}$	−11.01			−12.30	−11.21
	(7.240)			(9.573)	(9.155)
$N_{l,2}^{(High)}$	−2.408			−5.617	−1.951
	(7.605)			(8.158)	(7.956)
$N_{l,3}^{(High)}$	−3.604			−4.691	−2.575
	(5.315)			(5.606)	(5.680)
$N_{l,4}^{(High)}$	−11.13***			−13.95***	−14.33***
	(3.520)			(3.968)	(3.737)
$N_{l,0}^{(Med.)}$		1.995		0.354	0.224
		(1.803)		(1.980)	(2.007)
$N_{l,1}^{(Med.)}$		−0.493		0.0504	0.0420
		(2.976)		(3.754)	(3.724)
$N_{l,2}^{(Med.)}$		2.193		2.769	2.987
		(2.697)		(2.701)	(2.520)
$N_{l,3}^{(Med.)}$		0.0871		0.224	0.430
		(2.271)		(2.288)	(2.319)
$N_{l,4}^{(Med.)}$		1.351		2.632	2.738
		(2.330)		(2.844)	(2.863)
$N_{l,0}^{(Low)}$			−0.473		−1.091
			(2.459)		(2.711)
$N_{l,1}^{(Low)}$			−0.875		−0.725
			(1.374)		(1.405)
$N_{l,2}^{(Low)}$			−3.578*		−3.680*
			(1.996)		(2.145)
$N_{l,3}^{(Low)}$			−2.065		−2.065
			(1.548)		(1.606)
$N_{l,4}^{(Low)}$			0.392		0.534
			(1.835)		(1.890)
Observations	3538628	3538628	3538628	3538628	3538628
Counties	655	655	655	655	655
Weeks	780	780	780	780	780
Clusters	655	655	655	655	655

Standard errors clustered at the location level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3A.5: Effect of Earthquakes on Birth Weight, Baseline Linear Model (Equation (3.1), detailed), Conception Years 1994 - 2008

	(1) Weeks of Gestation	(2) Weeks of Gestation	(3) Weeks of Gestation	(4) Weeks of Gestation	(5) Weeks of Gestation
$N_{l,0}^{(High)}$	0.0310*			0.0242	0.0370
	(0.0173)			(0.0211)	(0.0280)
$N_{l,1}^{(High)}$	−0.0108			−0.0293	−0.0156
	(0.0375)			(0.0352)	(0.0394)
$N_{l,2}^{(High)}$	−0.0201			−0.0290	−0.0170
	(0.0173)			(0.0217)	(0.0281)
$N_{l,3}^{(High)}$	−0.0114			−0.0127	−0.00102
	(0.0228)			(0.0279)	(0.0370)
$N_{l,4}^{(High)}$	−0.0307***			−0.0414**	−0.0399**
	(0.00825)			(0.0176)	(0.0163)
$N_{l,0}^{(Med.)}$		0.00604		0.00396	0.00423
		(0.00722)		(0.00797)	(0.00736)
$N_{l,1}^{(Med.)}$		0.0113		0.0129	0.0131
		(0.00837)		(0.00949)	(0.00954)
$N_{l,2}^{(Med.)}$		0.00525		0.00764	0.00843
		(0.00937)		(0.0105)	(0.00999)
$N_{l,3}^{(Med.)}$		−0.00224		−0.00132	0.000331
		(0.00968)		(0.0108)	(0.00956)
$N_{l,4}^{(Med.)}$		0.00718		0.0107	0.0120
		(0.00986)		(0.0118)	(0.0123)
$N_{l,0}^{(Low)}$			−0.00978		−0.0112
			(0.00825)		(0.00980)
$N_{l,1}^{(Low)}$			−0.0119*		−0.0125
			(0.00724)		(0.00844)
$N_{l,2}^{(Low)}$			−0.0108		−0.0109
			(0.00926)		(0.0100)
$N_{l,3}^{(Low)}$			−0.0115		−0.0114
			(0.00793)		(0.00859)
$N_{l,4}^{(Low)}$			−0.000912		−0.000581
			(0.00996)		(0.0105)
Observations	3538628	3538628	3538628	3538628	3538628
Counties	655	655	655	655	655
Weeks	780	780	780	780	780
Clusters	655	655	655	655	655

Standard errors clustered at the location level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3A.6: Effect of Earthquakes on Birth Outcomes, Baseline Linear Model (Equation (3.1)), Conception Years 1994 - 2008, by Outcome and Firstborn Status

	Birth Weight			Weeks of Gestation		
	(1) First	(2) Other	(3) All	(4) First	(5) Other	(6) All
$N_{l,0}^{(High)}$	−3.037	40.70**	22.69**	−0.0406	0.0925**	0.0370
	(10.10)	(16.78)	(9.042)	(0.0707)	(0.0401)	(0.0280)
$N_{l,1}^{(High)}$	−8.690	−13.12	−11.21	−0.0297	−0.00854	−0.0156
	(11.89)	(9.408)	(9.155)	(0.0765)	(0.0232)	(0.0394)
$N_{l,2}^{(High)}$	1.095	−4.623	−1.951	0.0290	−0.0538**	−0.0170
	(8.033)	(9.407)	(7.956)	(0.0413)	(0.0239)	(0.0281)
$N_{l,3}^{(High)}$	−6.123	−0.771	−2.575	−0.00510	0.00264	−0.00102
	(9.739)	(9.792)	(5.680)	(0.0576)	(0.0456)	(0.0370)
$N_{l,4}^{(High)}$	5.768	−29.54***	−14.33***	−0.00244	−0.0679***	−0.0399**
	(5.289)	(5.425)	(3.737)	(0.0205)	(0.0200)	(0.0163)
$N_{l,0}^{(Med.)}$	3.239	−2.120	0.224	−0.00676	0.0116	0.00423
	(3.483)	(3.129)	(2.007)	(0.0119)	(0.0106)	(0.00736)
$N_{l,1}^{(Med.)}$	0.865	−0.239	0.0420	0.0117	0.0146**	0.0131
	(5.491)	(3.554)	(3.724)	(0.0167)	(0.00738)	(0.00954)
$N_{l,2}^{(Med.)}$	2.059	3.786	2.987	−0.00650	0.0186	0.00843
	(3.366)	(2.980)	(2.520)	(0.0118)	(0.0115)	(0.00999)
$N_{l,3}^{(Med.)}$	−1.876	2.055	0.430	−0.0150	0.0113	0.000331
	(3.785)	(2.604)	(2.319)	(0.0161)	(0.00837)	(0.00956)
$N_{l,4}^{(Med.)}$	−1.966	6.216**	2.738	0.00355	0.0177	0.0120
	(3.813)	(3.094)	(2.863)	(0.0144)	(0.0134)	(0.0123)
$N_{l,0}^{(Low)}$	−2.225	−0.226	−1.091	−0.0155	−0.00834	−0.0112
	(2.949)	(3.121)	(2.711)	(0.0112)	(0.0103)	(0.00980)
$N_{l,1}^{(Low)}$	0.299	−1.479	−0.725	−0.0161*	−0.00997	−0.0125
	(1.761)	(1.843)	(1.405)	(0.00971)	(0.00879)	(0.00844)
$N_{l,2}^{(Low)}$	−4.782*	−3.084	−3.680*	−0.0120	−0.0105	−0.0109
	(2.664)	(2.291)	(2.145)	(0.0127)	(0.00942)	(0.0100)
$N_{l,3}^{(Low)}$	−0.0348	−3.947**	−2.065	−0.00636	−0.0149	−0.0114
	(2.130)	(1.855)	(1.606)	(0.00955)	(0.00936)	(0.00859)
$N_{l,4}^{(Low)}$	−1.352	1.884	0.534	−0.00409	0.00214	−0.000581
	(2.382)	(2.041)	(1.890)	(0.0135)	(0.00970)	(0.0105)
Observations	1519454	2019152	3538628	1519454	2019152	3538628
Counties	636	647	655	636	647	655
Weeks	780	780	780	780	780	780
Clusters	636	647	655	636	647	655

Standard errors clustered at the location level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3A.7: Effect of Earthquakes on Birth Outcomes, Semi-Parametric Model (Equation (3.2), Definition (3.4)) Conception Years 1994-2008

	(1) Birth Weight	(2) Birth Weight	(3) Birth Weight	(4) Weeks of Gestation	(5) Weeks of Gestation	(6) Weeks of Gestation
$I\left[N_{l,0}^{(High)} > 0\right] = 1$	21.46***	20.48**	21.00**	0.0317*	0.0161	0.0159
	(7.800)	(9.269)	(9.363)	(0.0174)	(0.0188)	(0.0201)
$I\left[N_{l,1}^{(High)} > 0\right] = 1$	−11.76*	−12.77	−12.32	−0.0112	−0.0335	−0.0327
	(7.052)	(8.372)	(8.139)	(0.0382)	(0.0306)	(0.0306)
$I\left[N_{l,2}^{(High)} > 0\right] = 1$	−1.878	−5.781	−4.917	−0.0192	−0.0344*	−0.0336*
	(7.646)	(8.520)	(8.151)	(0.0171)	(0.0188)	(0.0186)
$I\left[N_{l,3}^{(High)} > 0\right] = 1$	−2.900	−4.726	−4.144	−0.00800	−0.0191	−0.0191
	(5.127)	(5.432)	(5.184)	(0.0204)	(0.0215)	(0.0231)
$I\left[N_{l,4}^{(High)} > 0\right] = 1$	−11.49***	−13.06***	−13.41***	−0.0317***	−0.0443***	−0.0467***
	(3.698)	(3.696)	(3.700)	(0.00817)	(0.0163)	(0.0149)
$I\left[N_{l,0}^{(Med.)} > 0\right] = 1$		1.214	0.703		0.0166	0.0152
		(2.363)	(2.282)		(0.0108)	(0.0103)
$I\left[N_{l,1}^{(Med.)} > 0\right] = 1$		−0.581	−0.708		0.0222	0.0212
		(4.197)	(4.178)		(0.0146)	(0.0138)
$I\left[N_{l,2}^{(Med.)} > 0\right] = 1$		5.641*	5.285*		0.0200*	0.0201*
		(3.059)	(2.950)		(0.0117)	(0.0111)
$I\left[N_{l,3}^{(Med.)} > 0\right] = 1$		1.855	1.691		0.0122	0.0118
		(2.600)	(2.655)		(0.00926)	(0.00912)
$I\left[N_{l,4}^{(Med.)} > 0\right] = 1$		2.491	2.910		0.0219	0.0244
		(3.469)	(3.250)		(0.0164)	(0.0159)
$I\left[N_{l,0}^{(Low)} > 0\right] = 1$			−1.450			−0.00254
			(2.204)			(0.00792)
$I\left[N_{l,1}^{(Low)} > 0\right] = 1$			−0.819			−0.00642
			(1.737)			(0.00646)
$I\left[N_{l,2}^{(Low)} > 0\right] = 1$			−2.609			−0.00268
			(2.009)			(0.00830)
$I\left[N_{l,3}^{(Low)} > 0\right] = 1$			−1.069			−0.00204
			(1.957)			(0.00738)
$I\left[N_{l,4}^{(Low)} > 0\right] = 1$			1.728			0.0117
			(1.990)			(0.00761)
Observations	3538628	3538628	3538628	3538628	3538628	3538628
Counties	655	655	655	655	655	655
Weeks	780	780	780	780	780	780
Clusters	655	655	655	655	655	655

Standard errors clustered at the location level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3A.8: Effect of Earthquakes on Birth Outcomes, Semi-Parametric Model (Equation (3.2), Definition (3.4)) Conception Years 1994-2008, by Outcome and Firstborn Status

	Birth Weight			Weeks of Gestation		
	(1) First	(2) Other	(3) All	(4) First	(5) Other	(6) All
$I\left[N_{l,0}^{(High)} > 0\right] = 1$	−4.030	38.58**	21.00**	−0.0678	0.0750*	0.0159
	(9.001)	(16.47)	(9.363)	(0.0500)	(0.0440)	(0.0201)
$I\left[N_{l,1}^{(High)} > 0\right] = 1$	−9.177	−14.58*	−12.32	−0.0489	−0.0241	−0.0327
	(10.77)	(8.158)	(8.139)	(0.0616)	(0.0258)	(0.0306)
$I\left[N_{l,2}^{(High)} > 0\right] = 1$	−3.871	−6.242	−4.917	0.00642	−0.0666***	−0.0336*
	(9.104)	(8.927)	(8.151)	(0.0249)	(0.0188)	(0.0186)
$I\left[N_{l,3}^{(High)} > 0\right] = 1$	−8.690	−1.812	−4.144	−0.0271	−0.0123	−0.0191
	(8.239)	(9.759)	(5.184)	(0.0386)	(0.0398)	(0.0231)
$I\left[N_{l,4}^{(High)} > 0\right] = 1$	3.838	−26.46***	−13.41***	−0.0138	−0.0711***	−0.0467***
	(5.293)	(6.225)	(3.700)	(0.0233)	(0.0143)	(0.0149)
$I\left[N_{l,0}^{(Med.)} > 0\right] = 1$	3.170	−1.371	0.703	0.00120	0.0243*	0.0152
	(4.144)	(3.701)	(2.282)	(0.0130)	(0.0147)	(0.0103)
$I\left[N_{l,1}^{(Med.)} > 0\right] = 1$	0.841	−1.367	−0.708	0.0193	0.0233**	0.0212
	(6.355)	(3.882)	(4.178)	(0.0219)	(0.0108)	(0.0138)
$I\left[N_{l,2}^{(Med.)} > 0\right] = 1$	4.707	5.805*	5.285*	0.00764	0.0284**	0.0201*
	(3.880)	(3.503)	(2.950)	(0.0124)	(0.0134)	(0.0111)
$I\left[N_{l,3}^{(Med.)} > 0\right] = 1$	−0.373	2.995	1.691	−0.00510	0.0240***	0.0118
	(4.117)	(3.262)	(2.655)	(0.0157)	(0.00890)	(0.00912)
$I\left[N_{l,4}^{(Med.)} > 0\right] = 1$	−2.640	6.895*	2.910	0.0103	0.0337**	0.0244
	(4.190)	(3.665)	(3.250)	(0.0192)	(0.0158)	(0.0159)
$I\left[N_{l,0}^{(Low)} > 0\right] = 1$	−2.401	−0.726	−1.450	−0.00819	0.000988	−0.00254
	(2.937)	(2.601)	(2.204)	(0.0103)	(0.00850)	(0.00792)
$I\left[N_{l,1}^{(Low)} > 0\right] = 1$	1.168	−2.146	−0.819	−0.0101	−0.00358	−0.00642
	(2.164)	(2.207)	(1.737)	(0.00827)	(0.00724)	(0.00646)
$I\left[N_{l,2}^{(Low)} > 0\right] = 1$	−3.252	−2.409	−2.609	−0.00214	−0.00363	−0.00268
	(2.598)	(2.371)	(2.009)	(0.0111)	(0.00857)	(0.00830)
$I\left[N_{l,3}^{(Low)} > 0\right] = 1$	1.321	−3.387	−1.069	0.00221	−0.00504	−0.00204
	(2.427)	(2.382)	(1.957)	(0.00880)	(0.00864)	(0.00738)
$I\left[N_{l,4}^{(Low)} > 0\right] = 1$	−0.0131	2.940	1.728	0.00663	0.0152*	0.0117
	(2.257)	(2.296)	(1.990)	(0.0102)	(0.00791)	(0.00761)
Observations	1519454	2019152	3538628	1519454	2019152	3538628
Counties	636	647	655	636	647	655
Weeks	780	780	780	780	780	780
Clusters	636	647	655	636	647	655

Standard errors clustered at the location level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.