Essays in U.S. Financial History

By

Lillian R. Gaeto

Dissertation

Submitted to the Faculty of the

Graduate School of Vanderbilt University

in partial fulfillment of the requirements

for the degree of

DOCTOR OF PHILOSOPHY

in

Economics

May 12, 2023

Nashville, Tennessee

Approved:

Peter L. Rousseau, Ph.D.

William Collins, Ph.D.

Kevin Huang, Ph.D.

Sarah Quincy, Ph.D.

David Parsley, Ph.D.

To my parents who encouraged, supported, and loved me when I needed it most.

ACKNOWLEDGMENTS

First, I want to thank my advisor, Peter Rousseau. It was a pleasure to be his student and learn from him throughout my graduate school career. Peter provided the appropriate support and encouragement as I developed my research ideas. He also instilled confidence in me throughout the tumultuous job market process. I look forward to working with him more in the future.

I additionally thank the other members of my dissertation committee. Bill Collins provided detailed feedback on my early drafts that proved extremely helpful. From my independent study, Sarah Quincy instilled an interest and passion for the Great Depression era of American history. I am grateful for her depth of economic and historical knowledge of this time period and the way she pushed my research to be the best it could be. I also thank Kevin Huang who showed a dedication to me in his roles as a professor and researcher.

This dissertation is dedicated to my parents, Paul and Patricia Gaeto, who have exemplified unconditional love throughout my PhD program. They never ceased to encourage me either in person or from afar, reminding me that their love and pride does not stem from any specific accomplishment. I can confidently say I would not have completed this degree without their sacrifices and emotional investment in me. I am also fortunate to have had my fiancé, Mark Trotter, and his family as additional sources of encouragement. Mark rode the highs and lows of the research and job market process with me and adapted plans to my ever-changing graduate school timeline. His encouragement and strong faith always pointed me back to Christ and instilled me with peace and confidence. I am also grateful to Mark's parents, Kathy and Paul Trotter, who encouraged me and prayed for me over the past year.

My friends, both Vanderbilt-affiliated and otherwise, have provided me with a wonderful balance of hard work and social activities over the past five years. Thu Tran pursued a friendship with me from one of my first days on campus and has served as a mentor and dear friend throughout my time at Vanderbilt. I will cherish our spontaneous visits to Chick-fil-A and walks around Nashville. Elijah Coleman additionally sacrificed time to help me prepare for the Microeconomics Preliminary exam and often provided transportation to said fast food restaurant. I am grateful to my friend Kayleigh McCrary who set an example of how to balance social engagements and productivity in graduate school. I am thankful for the stability of our lunches together throughout the years and the insight she provided. Finally, the camaraderie of my cohort and their supportive nature provided additional encouragement and brightness throughout the more stressful times of the program.

Lastly, I want to thank my church community and the firm foundation which it provided throughout the ups and downs of the PhD program. Lexie Spence encouraged me in my passion for economics and teaching. Anna Pollard was one of my biggest cheerleaders sending me faith-filled reminders during stressful weeks and always making time to hang out. Hannah Sills, a proud Vanderbilt alumna, shared her experiences and her love for Vanderbilt athletics with me, which greatly increased my school spirit and enjoyment of graduate school.

INTRODUCTION

In this dissertation, I incorporate my interests in history and macroeconomics by studying financial economics in a historical setting. Specifically, I analyze the effects of expanding access to financial markets and securities during the early twentieth century and study how the expansion was affected by the Great Depression and vice versa. The mechanisms for increasing access to financial markets in the dissertation take three main forms: employee stock ownership programs, government bonds, and stock brokerage offices.

In the first chapter, I use the unanticipated crash of the stock market to study the effects of employee stock ownership programs on productivity during a financial crisis. The Great Depression era provides an interesting natural experiment in which to study employee stock ownership programs (ESOPs) due to their popularity throughout the 1920s and the long-term nature of company contracts in this period. The Census of Manufactures provides the establishment-level outcome and control variables and allows me to study these effects at a micro-level. I collect data on the duration of ESOPs and the institutional details of the programs from reports by the National Industrial Conference Board, annual company reports, and other primary sources.

The results show that branches of companies with active, broad-based programs had significantly lower real output and real wages than firms with inactive programs. The results suggest that price appreciation is a key mechanism that incentivizes employees and thus, corresponds with the efficiency-wage literature. Yet, when isolating the effects in smaller firms, the results support the existing theory on ESOPs leading to stronger group identity and incentive-compatibility when able to overcome the free-rider problem. I show that ESOPs disincentivize productivity less in smaller firms. By exposing the negative consequences of these programs in an economic downturn, this chapter suggests that the effects of employee stock ownership are possibly more nuanced than previously thought.

My second chapter analyzes the often-overlooked Liberty Loan Program of World War I which was the first large-scale government-sponsored saving initiative in which middle- and working-class households were encouraged to participate. Using the exogenous shock of the U.S. joining World War I and the extraordinary government spending that ensued, a vector autoregression framework is developed and initial conditions are met which allow me to empirically study the short-run effects of issuing public debt versus increasing taxation. The results imply war bonds were successful at channeling capital into the productive war industries, such as iron and steel. The theoretical motivation presented suggests that increased productivity through bond issuance comes from households foregoing spending on consumption goods and reallocating savings from cash and deposits to investments in the war sectors. Thus, liberty bonds functioned as productive assets and were an essential component of public finance during the unprecedented upheaval of World War I.

The third chapter documents the rapid expansion of New York Stock Exchange (NYSE) member firms during the 1920s and sheds light on possible effects of accessibility to the stock market as well as the competing roles of banks and brokerages. A major contribution of this project is the NYSE directory data which had never been fully digitized or studied empirically prior to this project. These data show patterns of member firm expansion concentrated in the Midwestern and Southern United States up until the Great Depression. The cross-sectional data suggest that banks and brokerages were complements during the early phase of office expansion before shifting to substitute financial services. Overall, this paper lays the groundwork for future research about the significance of stock market access in the 1920s and its role in the economic depression that followed.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

CHAPTER 1

Employee Stock Ownership and the Great Depression

1.1 Introduction

Employee stock ownership programs (ESOPs) have expanded to become a conventional component of employee compensation in the modern context, and subsequently research on their effects in terms of worker behavior and firm motivations has similarly burgeoned.[1] Today, there are estimated to be around 6,500 employee stock ownership programs with almost 14 million participants in the U.S. (National Center for Employee Ownership, 2021). Over 10 percent of individuals who directly own stocks do so through an ESOP (Saad and Jones, 2021). Yet, even as these programs are increasingly adopted, the key questions in the employee stock ownership literature such as how and why these programs can increase productivity remain largely unanswered.

These programs are often adopted under the assumption that they align incentives between employees and shareholders, which is known as the incentive-compatibility mechanism (Brickley and Hevert, 1991; Hochberg and Lindsey, 2010). However, the usefulness of these programs from a firm productivity perspective, their direct effects under various conditions, and the mechanisms behind these effects are not generally agreed upon. The literature tends to rely on case studies and narrative evidence to draw conclusions since robust, empirical results about the causal effects of these programs are difficult to obtain due to a lack of exogenous variation in timing of adoption. Throughout history, the adoption of ESOPs has been shown to be pro-cyclical and highly correlated to rising stock prices and firm profits (Ott, 2011). Thus, little is known about their direct effects in an expansionary economy, let alone under less favorable economic conditions.

This paper seeks to fill a gap in the research on ESOPs by answering the question of how continuing an employee stock ownership program during a financial crisis affects productivity. I look to the past for insight and study employee stock ownership in a setting where companies were ending their programs but only when contractually allowed to do so. Specifically, I analyze the effects of ESOPs during the Great Depression as stock prices fall, but firms remain locked into their programs.

[1] Throughout the paper, ESOP is used in the general sense for an employee stock ownership program instead of a formal succession plan which is believed to have originated with Louis O. Kelso in 1956 (Menke and Buxton, 2010).

In doing so, I can study the role of price appreciation in aligning worker effort.

My results are consistent with workers' utility from holding and buying company stocks falling during the Depression. Employees became disincentivized by the plunging stock prices which in effect reflect their return to labor. This resulted in lower output growth at companies where ESOPs operated. With these programs, workers substitute wages for deteriorating stock, and their low return further discouraged workers. Thus, price appreciation is an important mechanism for understanding how stocks incentivize worker effort and when these programs are profitable from a firm perspective.

In Section 1.2, I begin with an overview of the historical setting and the details of the natural experiment which suggests the timing of program expiration is plausibly exogenous. Employee stock ownership has been present in the United States for over 100 years, which makes the extent of the uncertainty about their real effects more striking.[2] Large companies adopted ESOPs starting in the early 1900s, and adoption increased in the 1920s following the success of the Liberty Loan Program (Ott, 2011). For management, giving employees the opportunity to have ownership in the company was seen as a way to appease workers, discourage unionization, and increase loyalty. The number of programs reached a peak in the mid-1920s, and on the eve of the Great Depression hundreds of thousands of employees owned stock in their employers (National Industrial Conference Board, Inc., 1928).

Instead of following the modern literature and using the choice of adoption by company executives to determine the timing of treatment, which is likely related to firm characteristics such as hiring decisions and profits, I leverage the pre-determined expiration of the programs. I present data that suggests the timing of expiration, according to the format of company contracts, was plausibly exogenous to the company's underlying characteristics. The long-term nature of these programs which began in the mid-to-late 1920s means that both companies and employees were more or less locked into their contracts after the largely unanticipated stock market crash of 1929. Following the strategy developed by Almeida et al. (2012) and Benmelech et al. (2019) more recently, the pre-determined length of the contract between firm management and rank-and-file employees is used to identify the effects of employee stock ownership in the turmoil of the poor stock performance

[2]Proctor and Gamble is believed to be one of the first companies to adopt a program for employees to purchase stock, dating back to 1886 (National Industrial Conference Board, Inc., 1928).

during the Depression.

Given the lack of research on employee stock ownership in this period, this project required an extensive data collection process. The basis for the employee stock ownership data comes from a comprehensive study by the National Industrial Conference Board, Inc. (1928) conducted prior to the Depression in 1928. To this, I hand-collected information from various primary sources to establish the details of these programs, especially the origination and expiration of the stock contracts. The establishment-level data utilized in this paper is from the United States Census of Manufactures, 1929-1935 digitized by Vickers and Ziebarth (2018) and is manually matched to the novel, employee stock ownership data. Establishments are defined as all the individual plants of manufacturing companies in the sample. Only establishments whose parent firm ever had a stock ownership program remain in the dataset. The advantage of using establishment-level schedules over aggregated firm-level data is that the former gives a more precise understanding of how employee monitoring and the free-rider problem affect productivity.[3]

Treatment and control groups are then assigned based on the quasi-random timing of plan expiration after the 1929 stock market crash. Establishments are treated as long as their program, which originated prior to the crash and was set for a pre-determined duration, remains active.[4] Once the parent firm's program becomes inactive, these establishments move to the control group.

The baseline results show these first employee stock ownership programs had a direct, negative effect on productivity. Having an active program during the Great Depression caused lower establishment-level output growth and decreased the hours worked by employees. These results suggest employee stock ownership programs can have negative incentive effects for employees during times of extreme economic turmoil. I present a table that suggests workers were further discouraged by poor stock performance and conclude that the real monetary benefit to employees does matter when it comes to aligning incentives. Employee stock ownership requires more than simply bonding individual workers to each other and to the firm as has been suggested (Lazear, 1981; Kim and Ouimet, 2014).

[3]The free-rider problem exists when it is believed that one worker's individual productivity decisions will not affect overall output, and they choose to "free ride" off the hard work of co-workers. This incentive to shirk grows with the size of the firm.

[4]In this paper, an ESOP is considered active if employees are still able to purchase new stock at the employee rate. A program is inactive once new employees do not have the option to buy stock since the formal plan has ended, even if some workers already have stock in their portfolio.

I also further break down the productivity results based on firm size to better understand the competing roles of incentive-compatibility and free-riding. The results suggest that the free-rider problem can be mitigated when individual employees feel they can affect the stock price which accords with the existing ESOP literature (Blasi et al., 1996). Smaller firms, those in the first tercile based on number of employees, with active programs saw establishment-level output decrease by less than larger firms. Finally, I show that there is a direct tradeoff between wages and stock purchases as anticipated since employees pay for company stocks through wage deductions in active programs. Employees, in effect, saw their real benefit of working fall through this substitution. I interpret these results as consistent with a model where the value of stocks to employees is a function of the asset's return and the worker's ability to alter the price with their effort level. Additionally, employees paying into these programs were likely disincentivized by their falling return to working. Thus, the firm's marginal costs of implementing the program outweighed the marginal benefit of increased worker effort in this setting.

These results are robust to various data alterations. I exclude the largest outliers and show the results are not dependent on any single firm being present in the dataset. The last year of the sample period, 1935, is also excluded as a robustness test. The active programs in this last period are the longest programs, and while I show they are not different in terms of observables, I test whether they drive the baseline results. The main results indicating these programs caused workers to behave less productively still hold when this smaller sample period is used.

My empirical strategy contributes to the literature by providing convincing causal estimates on the effect of employee ownership on productivity in a historical setting. To my knowledge, this paper is the first to empirically study this early period of employee stock ownership.[5] The majority of previous empirical studies on the topic of employee stock ownership and the functioning of a firm are confined to analyzing the past few decades and highlight the expansion of employee stock ownership programs in times of economic prosperity (Jones and Kato, 1995; Blasi et al., 1996; Kim and Ouimet, 2014). In doing so, these studies often perform cross-sectional analyses or event studies to document how ESOPs are related to firm-level characteristics. While these relationships are

[5] Two previous studies of the beginnings of employee stock ownership which were extremely helpful during the data collection process are National Industrial Conference Board, Inc. (1928) and National Industrial Conference Board, Inc. (1930). These primary sources outline specific dates when programs were adopted, but they provide no empirical tests or analyses.

important, the previous literature cannot provide causal conclusions due to the lack of any plausibly exogenous variation in the timing of employee stock programs in the modern setting. In the end, the empirical results are generally mixed in terms of the strength of an ESOP's effect on productivity and firm profitability in an expansionary economy (Livingston and Henry, 1980; Jones and Kato, 1995; Blasi et al., 1996; Kim and Ouimet, 2014).

My results also contribute to the broad literature on the impact of the Great Depression in terms of the stock market, output, and employment. The crash of the stock market in October 1929 precipitated the severe economic contraction and led to a persistent change in stock market volatility (White, 1990; Rappoport and White, 1994; Cortes and Weidenmier, 2018). Overall real output fell by 26% between 1929 and 1933, and the Great Depression remains the worst economic downturn in U.S. history to date (Margo, 1993). At its peak, unemployment reached 25%. Several explanations have been suggested for the economic fallout, such as widespread bank failures, financial frictions, and inappropriate policies by the Federal Reserve (Lee and Mezzanotti, 2015; Bernanke, 1983; Richardson and Troost, 2009; Benmelech et al., 2019). Various effects from this economic contraction have been documented in terms of reduced manufacturing output but also a foundation for significant technological progress in the following decades (Lee and Mezzanotti, 2015; Field, 2003). My results support an additional mechanism through which the stock market affected workers and firms during the Great Depression. ESOPs reduced productivity at the companies hardest hit by the stock crash and likely made workers worse off in this period.

By studying ESOPs when stock prices are falling across industries, this paper also addresses a key point of disagreement in the theoretical literature about how and when these programs increase worker effort. Social identity theory posits these programs bond employees together and reduce turnover regardless of the individual's influence on the stock price (Kim and Ouimet, 2014). The overall decrease in productivity from employee stock ownership rules out the social cohesion theory and instead points to the real return of ownership as being an important driver of worker effort.

The empirical results in this paper remain consistent with aspects of both theories on efficiency wages and incentive-compatibility. Efficiency wage theory predicts firms pay above market wages to attract high-quality workers and this should increase productivity over time (Shapiro and Stiglitz, 1984). While eager workers may have been attracted to firms because of these programs initially, the fall in prices reversed the predicted effect of efficiency wages and instead decreased productivity.

The real output results for the smaller firms show that when the free-rider problem is reduced, the fall in productivity may be attenuated by more directly aligning the workers' effort levels with firm profits even in a struggling economy (Jensen and Meckling, 1976; Blasi et al., 1996). The relevance of these theories suggests that with an active ESOP, labor effort is dependent on both the asset's return and the individual's perception of how they can affect the return over time.

1.2 Historical Background

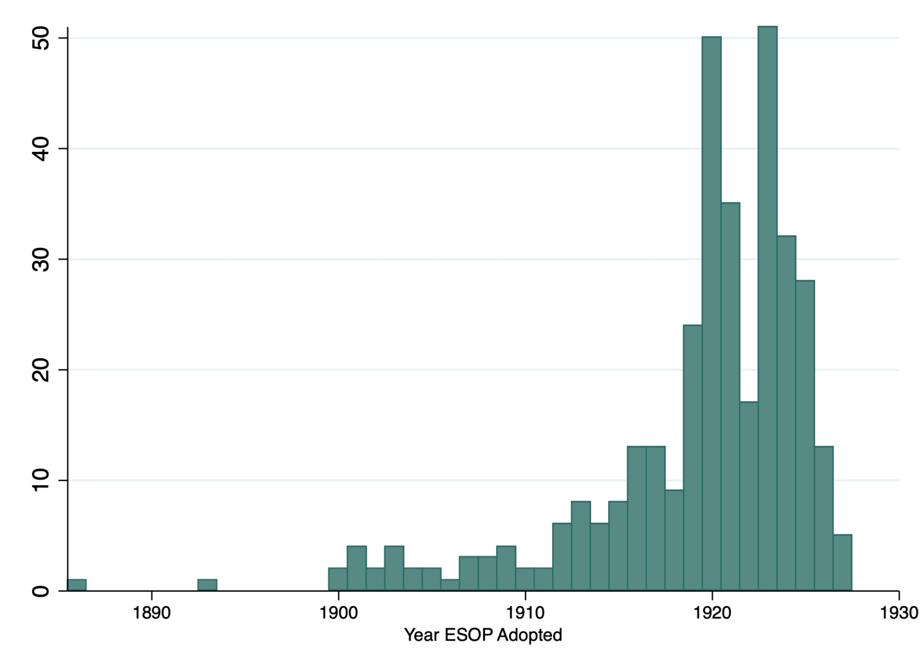
The first employee stock ownership programs emerged in the United States at the turn of the twentieth century (Ott, 2011). Companies increasingly adopted these novel programs in the aftermath of World War I following the emergence of a new class of modest-income investors during the Liberty Loan Campaigns. In 1924, Economist H.T. Warshow used income tax returns to estimate the number of stock holders in the U.S. (Warshow, 1924). While acknowledging the lack of data on employee stock ownership, he posits the expansion of stockholders was due, at least in part, to the growth of ownership programs throughout the country.

At the time, company surveys highlighted various motivations behind the introduction of these programs. Continuing the rhetoric introduced by the war bond drives, many employers stated these programs were a way to encourage thrift (National Industrial Conference Board, Inc., 1928). Additional reasons acknowledged in surveys include rewarding employee contributions to the company's success and garnering public interest in the company. Historian Julia Ott emphasizes that ESOPs also effectively discouraged discussions of unionization among lower-level employees (Ott, 2011).

The initial stock programs and their contracts introduced in the 1920s can be roughly categorized into two main types based on who was eligible to participate. Programs that were open to all employees or, more specifically, lower level employees are referred to as "rank and file" plans. These plans may have had additional requirements for eligibility, such as length of time with the company and a lack of any previous work offenses. Alternatively, programs that only allowed employees to participate once they had achieved a certain rank in the company are termed selective programs. In this period, the broad-based, rank and file plans were more prevalent and are the primary focus of this paper.

By 1928, the programs had gained enough attention to warrant an extensive study by the National Industrial Conference Board. They documented the trends and timing of plan adoption by

Figure 1.1: Years Companies Adopted Programs until 1928



Source: National Industrial Conference Board, Inc. (1928)

sending surveys to companies across the U.S. Figure 1.1 shows the number of companies that adopted their first program in each year until 1928. The graph shows the peak of adoption occurred in the early 1920s. Table 1.14 in Appendix 1.6.4 shows these data in table form. The five year period which corresponds to the most companies establishing their first program was 1921-1925.

It should be noted that the U.S. economy, and particularly, the stock market boomed during the 1920s and participating employees were positioned to benefit from ownership. Even so, the growing adoption of these programs was not without controversy. Even the National Industrial Conference board report acknowledged the impact of these programs likely depended on the state of the economy. It states,

> It should always be remembered, however, that more than half of the companies whose experience with selling stock to their employees is considered here offered their securities for the first time in 1921 or later. Steady employment, high wages and rising prices of securities have marked this entire period. How conclusive evidence is, therefore, regarding experience with stock purchase plans for employees during periods of

depression as well as prosperity is not quite certain" (National Industrial Conference Board, Inc. (1928), pg. 17).

New York University economist, Willard Fischer, emphasized the need for employees to diversify and not "put all of his eggs in one basket." He went as far as condemning the practice of employee stock ownership (New York Herald Tribune, 1927).

Stock ownership programs were also viewed with deep suspicion by organized labor. The American Federation of Labor feared stock ownership would lead to workers losing their identity, and it would never effectively redistribute power to the working class (Ott, 2011). Despite the various concerns raised publicly the programs continued to expand, and on the eve of the Great Depression, over 800,000 employees at over 300 companies owned stock in their employers (National Industrial Conference Board, Inc., 1928).

1.2.1 Stock Offerings and Installment Plans

The specific details of the programs and offerings varied significantly across companies in terms of eligibility, payment options, resale, and so on. Nevertheless, this section draws heavily on the National Industrial Conference Board reports and two Princeton University studies to summarize their main characteristics as well as outline their key differences (National Industrial Conference Board, Inc., 1928, 1930; Baker, 1932; Davis, 1933).[6]

Most companies had plans that lasted from two to five years, and their duration was set from the outset or until a certain number of shares had been subscribed for. For example, the Standard Oil Company of New Jersey had four "long-term" plans from 1920 to 1935 that ranged from three to five years (National Industrial Conference Board, Inc., 1930). Each plan specified an amount that the company would match of the employees' contribution, eligibility requirements for participation, and a rule to determine the employee price (Davis, 1933). The vast majority of companies, including the Standard Oil Company of New Jersey, allowed employees to purchase shares through installments. Often, employees could have a percentage of their wages taken out each month to be directly deposited into their stock accounts.

Table 1.1 summarizes the number of companies in the National Industrial Conference Board's

[6]Table 1.16 in Appendix 1.6.4 provides an overview of the ESOPs for every company in the main dataset which is described more fully in Section 1.3.

Table 1.1: Installment Periods and Prices of Securities by Employees

	Rank and File	Selective	Total
Installment Period			
No deferred payments	12	2	14
One year or less	44	2	46
Over one year	151	13	164
No information	144	21	165
Price			
Par	121	15	136
Percentage below par	20	1	21
Flat reduction from par	3	0	3
Market	115	16	131
Percentage below market	15	4	19
Flat Reduction from market	63	3	66
Not specified	15	1	16

Source: National Industrial Conference Board, Inc. (1928) pg. 72, 83

extensive sample that allow various lengths of employee payment plans and the price-level at which employees can purchase stock. Most companies allow deferred payments for some length of time and offer their stock to employees at either market or par value.

Taken together, this information implies the greatest benefit to employees of ownership in most cases was the long-term installment plan. An example of a typical statement of stocks is provided as Figure 1.8 in Appendix 1.6.3. As the statement shows, it will take several years for this employee to fully own the stock she has subscribed for, but the shares are waiting with her name on them when she does. This effectively broadened access to stock ownership because most workers earning modest hourly wages could not afford the face market value of stocks at a single time. While New York Stock Exchange brokerage offices also expanded across the country throughout the 1920s, these firms could not offer the working class this kind of subscription program (Ott, 2011).

1.2.2 ESOPs and the Great Depression

The first major test of the companies' commitment to employee stock ownership came in 1929 with the stock market crash and the beginning of the Great Depression. As the Industrial Conference Board had noted, most employee stock owners had no experience with market volatility at this time. Some employees sought to sell their stock while others chose to no longer make their subscription payments and to instead receive their investment back in the form of cash. Still others held onto their assets and even subscribed to additional shares at low prices with the hope that their value would bounce back quickly. While the market prices for stocks plummeted, many companies reported that on net, the cost to employees was still less than the prevailing market price at any given time (National Industrial Conference Board, Inc., 1930).

Unfortunately, detailed data on employee participation, the exact amount of cancelled subscriptions, and stock resales are unavailable at the aggregate level, but studies of a sample of representative companies provide an overview of the situation. Similar to the way ownership plans varied across companies, there were a range of responses to the downturn and allowances for employees. Out of 99 companies surveyed, a majority of them would purchase the stock back from their employees at the cost the employee incurred (usually with a low interest rate). However, a large number of companies had no plan in place should employees want to sell their stock quickly. This highlights the unanticipated nature of the crash to the public (Davis, 1933). In the midst of the overall enthusiasm for broad-based stock ownership, most employers could not fathom a scenario where workers would want to discard their securities.

The National Industrial Board also surveyed the sample of companies on how subscription cancellations and resales in 1930 compare to previous years. The majority of companies reported they either had no cancellations or resales or a normal level of them. The selective ESOPs exhibit more stability in terms of the long-term holding of stocks since most companies had no cancellations or resales. Some companies took this as evidence that the working class should not have had the opportunity to own stock in the first place (Davis, 1933). This is represented by a trend of companies switching to offering only selective ESOPs when they are contractually able to do so following the crash.

Additionally, many companies decided to indefinitely postpone their regularly scheduled offerings during the aftermath of the crash. Because most of the companies had the length of the plan or installment period determined from the outset, there is substantial variation in the timing of discontinuance across companies.

Still, a few companies persevered through the market turmoil and continued to renew their programs. From a sample of 50 companies studied by Baker (1932), nineteen continued their programs uninterrupted throughout the Great Depression. The Standard Oil Company of New Jersey was one such company that made a new offering to employees in 1932 instead of postponing it. A timeline

Table 1.2: Standard Oil Co. of New Jersey Timeline of Plans

Plan	Start Date	End Date	Duration
First Stock Acquisition Plan	Dec. 30, 1920	Dec. 30, 1925	5 Years
Second Stock Acquisition Plan	Jan. 1, 1926	Dec. 30, 1928	3 Years
Third Stock Acquisition Plan	Jan. 1, 1929	Dec. 31, 1931	3 Years
Fourth Stock Acquisition Plan	Jan. 1, 1932	Dec. 31, 1935	4 Years

Source: Baker (1932), pg. 17

of the four stock acquisition plans offered by the Standard Oil Company of New Jersey is provided as Table 1.2. With each plan, the length of the program was determined when it went into effect. For example, the company committed to continuing the third plan for three years on January 1, 1929 before the stock market crash.

When it was time to decide whether to implement the fourth plan, the company executives surveyed the current participants about their stance (Baker, 1932). The rank and file employees involved in the third plan responded positively with 88% in favor of renewing the plan. This shows how the choice to make an additional offering was endogenous since it arose from the workers' attitude toward the company and the stock market more broadly.

Ultimately, the ideal of employees owning stock in their employers would outlast the Great Depression and recovery. Even amidst the market volatility, a survey taken in 1930 suggests that a majority of employees would choose to subscribe to a plan again in the future (National Industrial Conference Board, Inc., 1930). While there was a trend away from rank and file programs in the initial aftermath of the crash, broad-based stock ownership would gain popularity again following World War II when organized labor also rebounded (Ott, 2011).

More recently, the 1970s and 1980s were also periods of rapid growth in ESOPs, and represent a time when employee stock ownership became more of the rule rather than the exception. A survey by the National Center for Employee Ownership conducted in 1989 reported that 36 percent of large public firms had an ESOP and an additional 33 percent of companies without a program suggested they would likely start one (Blasi and Kruse, 1991). A full analysis and summary of the modern state of employee stock ownership is beyond the scope of this paper, but this section provides a background on the genesis of the programs. It seems reasonable to assert that the Great Depression has been the greatest test of employee ownership to date, and understanding the motivations behind them and their effects in this period of economic turmoil can help inform the puzzle of their modern-day prominence.

1.3 Data and Empirical Strategy

This section introduces the data, provides an overview of the empirical strategy, and presents summary tables suggesting that the baseline sample of firms and their establishments are balanced prior to the stock market crash in 1929.

1.3.1 Data

The data utilized in this project come from a variety of sources, including novel, hand-collected data on employee stock ownership programs throughout the late 1920s and early 1930s.

The establishment panel data come from the U.S. Census of Manufactures (CoM), which contains detailed production and compensation information for individual manufacturing establishments producing a variety of goods every two years from 1929 to 1935 (Vickers and Ziebarth, 2018). The data are coded at the establishment level, and in many cases, there are multiple establishments for each firm. To further diminish the possibility of confounding variables affecting the estimation of the ESOPs' direct effect, only the establishments which are separate from a company's headquarters will be analyzed (henceforth, referred to as branches). As mentioned above, all establishments are equally affected by the decision to offer a rank and file ESOP, and company executives cannot pick and choose which establishments are eligible. Recent research further suggests that multi-location and single-location firms may have behaved quite differently during the Great Depression (Loualiche et al., 2019). Thus, to mitigate systematic differences in companies, the main analysis seeks to compare multi-location firms with active ESOPs to other multi-location firms with inactive programs. Figure 1.2 presents the locations of all the branches which ever had ESOPs by 1929 in the CoM data where the size of the bubble reflects the number of branches at that location.

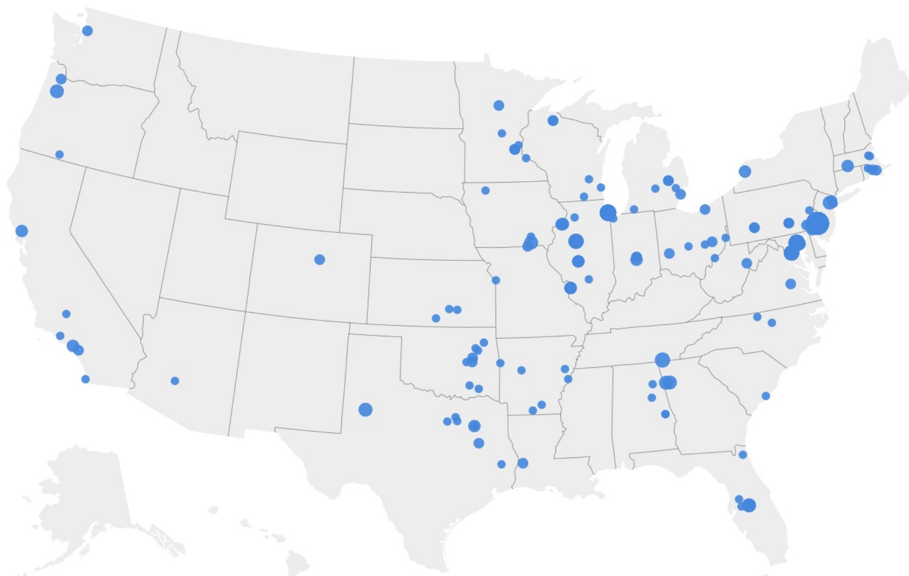
For the key information about the various stock programs, the extensive report from the National Industrial Conference Board (NICB) again proves useful for summarizing the state of employee stock ownership at the beginning of the period of interest. In 1928, the NICB published a detailed analysis using firm surveys to document the rapid growth in the number of ESOPs throughout the 1920s. This document contains appendices which categorize the companies by their level of activity and include the address of the company's headquarters, the year the plan was adopted, and the approximate number of employees for all U.S.-based firms ever having programs. To complete the ESOP data collection, this information is supplemented by two later studies published by the Industrial Relations Section at Princeton University in 1932 and 1933 (Baker, 1932; Davis, 1933).

These sources essentially cover the largest firms in the data until 1933. Additional information on smaller companies and changes in ESOPs through 1935 was collected by hand from various historical annual company reports and newspaper articles. The reports were obtained from ProQuest Historical Annual Reports database and various additions and volumes of Moody's Manual of Investments (Moody, 1935). These sources contain statements to stockholders and typically include a detailed balance sheet of the companies' operating expenses and outstanding stock. Once these statements were collected, the level of activity of the employee stock ownership programs as well as the length of the contract and subscription period could be determined. A sample of a balance sheet from the Firestone Tire & Rubber Company's annual report is provided as Figure 1.9 in Appendix 1.6.3. Local newspapers also usually highlighted news of a company in their community offering an opportunity for employees to purchase stock.

The final panel dataset of establishments was created by merging the establishment panel data with the novel employee stock ownership data. These were matched by hand according to the owner name, firm industry, and location of firm headquarters. Of the full sample of companies which instituted an ESOP at some point in their history collected by the NICB study in 1928, about 23 percent of these companies appear in the full Census of Manufactures data. When focusing on just the manufacturing companies which appear in the NICB report, the percent of these firms which are in the Census of Manufactures increases to 40 percent (Vickers and Ziebarth, 2018). Because the CoM data is the population of establishments for only specific manufacturing industries that have been digitized, this match rate seems reasonable. Appendix 1.6.5 provides the precise details of the data cleaning and merging processes.

An important feature of the data is that not all companies that had employee stock programs were listed on a major stock exchange (and vice versa). In many cases, the companies offered private assets to their employees (National Industrial Conference Board, Inc., 1928). Data on which firms in the Census of Manufactures dataset were listed and when come from Rousseau and Watchel (1998). Approximately 20 percent of the companies with ESOPs were listed according to their data

Figure 1.2: Locations of all the branches with ESOPs in Census of Manufactures, 1929



Source: ESOP data come from National Industrial Conference Board, Inc. (1928) and various sources collected by the author. Establishment locations are from Vickers and Ziebarth (2018).

from the Center for Research in Securities Prices (CRSP) (2022). This information is crucial because whether a firm is public is an important control variable in order to isolate the effect of the ESOP in the analysis. With this variable included in the regression models, the stock market financing channel can be ruled out when examining the effects of employee stock ownership. CRSP data were also collected to study the stock price and return trends for listed firms in the main sample. Figures 1.10 and 1.11 present the monthly average prices and returns for these firms.

It should be noted that while I primarily use the granular, establishment-level data, I am also able to aggregate the data to the firm-level to investigate specific firm-level outcomes such as survival and the number of branches within the firm. The identification strategy outlined in the next subsection similarly applies to both the firm and establishment-level data and its validity will be tested at both levels of aggregation.

1.3.2 Identification

The historical background section summarized the specifics of how these early employee stock ownership programs were often set for a pre-determined amount of time when they went into effect. For example, Table 1.2 shows that the length of time for Standard Oil Co. of New Jersey's programs varied from three to five years throughout the 1920s and 1930s. The length of the contracts in this example are fairly standard for the manufacturing sector in this period.[7]

This means that in October of 1929 when the Dow Jones Industrial Average fell 23 percent over two days, companies and their employees had varying lengths of commitment to their ESOP depending on when it was initially instituted and/or when the program was last renewed (Rappoport and White, 1994). Since many firms adopted programs in the 1920s, some plans had already expired before the crash and were waiting to be renewed, but others were instituted shortly before the unanticipated fall in stock prices. The present study circumvents the endogenous choice to begin or discontinue an ESOP, which restricts much of the previous literature from establishing causal estimates, because of this variation in plan timing and commitment device.

In the following analysis, when the stock market crash occurs, I focus solely on the ESOP that is in place at that point in time. This is referred to as the "pre-Depression" plan. The program is considered "active" until this contract expires. Then, the firm and its establishments are denoted as having an inactive ESOP in the dataset going forward, regardless of whether it chooses to renew its program or not. The control group is composed of the firms and establishments whose plans had already expired prior to 1929 and those whose pre-Depression plans have ended. The main identifying assumption is that, in the absence of treatment, establishments whose ESOPs were active throughout the 1930s would have evolved identically to establishments that had expired ESOPs when the stock market crashed.

Because the stock market crash was largely unanticipated, there is no reason to believe that firms would have targeted 1929 as the year for their plan to expire.[8] For example, Procter and Gamble renewed their program in early 1929 for six years, meaning they were locked into the contract until the end of 1934 (National Industrial Conference Board, Inc., 1930). Therefore, the causal effects of employee stock ownership are identified by exploiting this preexisting variation in the timing of plan expiration.

The identification strategy outlined in this section can be related to Almeida et al. (2012) and Benmelech et al. (2019) who identify financial frictions using preexisting levels of long-term cor-

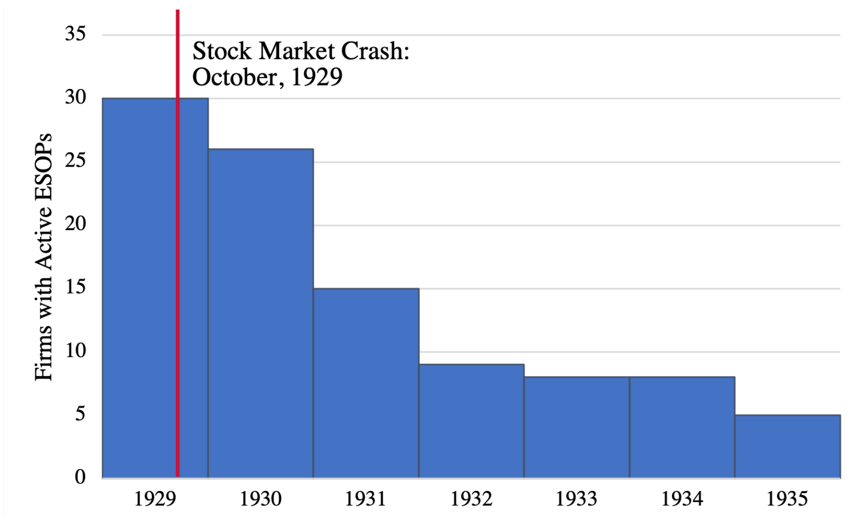
[7]For the baseline manufacturing sample, the average length of a contract is 3.95 years and the median is 4 years.

[8]In fact, in the data I find that many companies actually decide to renew their programs early in 1929 which provides excellent variation in the timing of plan expiration.

porate bonds. Almeida et al. (2012) first used ex-ante variation in the debt-maturity structure of long-term bonds to identify financial frictions during the 2007 crisis. Following this identification strategy, Benmelech et al. (2019) study the effect of financing frictions on firm changes in employment during the Great Depression. Instead of using the ex ante debt-maturity structure of long-term bonds, I use the pre-existing variation in employee stock ownership program expiration dates across firms to identify the productivity effects of the programs.

Figure 1.3 provides an overview of the baseline sample and shows the variation in timing of

Figure 1.3: Timing of ESOPs Becoming Inactive in Main Sample



Note: ESOP data collected from National Industrial Conference Board, Inc. (1928), and plan length data collected by author from various sources.

plan expiration. By construction, the most firms have active programs in 1929 since I study the programs that expire post stock market crash. The red line reflects the ”event” of the stock market crash in 1929 that rocked the securities markets (White, 1990). In each year of the CoM data, the active group of firms or their establishments are compared to the analogous group whose plan had already expired (either before or after 1929). Table 1.16 in Appendix 1.6.4 provides a more detailed overview of the length and type of active, pre-Depression plans that make up the treated establishments in the main sample. This table shows that plans range from one and a half years to seven years in length. The next subsection provides balance tables to support the identifying assumption outlined here.

1.3.3 Summary Statistics and Balance

Table 1.15 breaks down the main dataset by industry and summarizes the number of firms (not establishments) which have ESOPs in each industry and their rate of survival through the Depression. Only firms that are multi-location companies appear in this aggregated sample. Ice and petroleum refining are the industries which have the most firms with employee stock programs. The various industries which have been collected to form the Census of Manufactures are clearly not evenly distributed within the dataset (Vickers and Ziebarth, 2018). This is expected because within each industry, the full population of establishments, and thus firms, were collected and not merely a random sample. Thus, it seems logical that the linoleum industry has many fewer firms than the beverages industry when we think in terms of the entire population of firms. The varying size of the industries and exposure to ESOPs provide strong reasoning for why controlling for industry-specific factors through fixed-effects is important and will be discussed in the next subsection.

The last column of Table 1.15 shows that the survival rate differs greatly across industries as well. Surviving firms are those that have at least one establishment identified in the Census of Manufactures data by 1935, which is unfortunately subject to measurement error. At first glance, there does not seem to be a strong connection between having and an ESOP in place and surviving, but this will be tested empirically in the next section.

Even though employee stock ownership programs were gaining popularity on the eve of the Great Depression, they were still more the exception than the rule at the time (National Industrial Conference Board, Inc., 1928). While there are over 1,000 firms in the dataset, only about forty had ever had ESOPs by 1928. An additional advantage of studying the effects of the programs at the establishment level, is the increase in the number of observations, especially for "treated" establishments. ESOP firms have around seven branches on average with the median number being three in the main sample. Clearly, there are some large outliers which will be discussed in the robustness section.

To further understand the validity of the identification strategy, the timing of plan expiration is broken down in Table 1.3. Each active, pre-Depression plan is grouped based on when it expires and their 1929 firm statistics are shown. The groups correspond to the timing of the Census of Manufactures data as they are grouped into two-year bins. While the categories are not even in terms of the number of firms whose programs expire in each window, the data do not suggest there is any clear trend. In terms of the mean, no one group dominates a majority of the variables.

With this background and summary of the main sample, the results of more rigorous tests for balance are presented with particular focus on establishment-level variables. Unfortunately, due to data limitations with the sample of CoM data, testing pre-trends for all observables is not possible. I am able to test for differences in stock prices and returns in the 1920s, however, using the data extracted from Center for Research in Securities Prices (CRSP) (2022). Figures 1.12 and 1.13 present the difference-in-difference results before and after the stock market crash. The treated group is firms with active programs in 1929, and the coefficients of the treated indicator interacted with the year fixed effects are shown on the graphs. There appear to be no differences in prices or annual stock returns leading up to the crash between the treated and control groups.

For the variables where pre-trend data is unavailable, there is still one period before the crash that is analyzed. Table 1.4 summarizes the establishment-level 1929 data and compares establishments of firms with active versus inactive programs before the stock market crash. Once industry and Federal Reserve district fixed effects are included in the regression, the conditional t-statistics show the active and inactive establishments are similar before the stock market crash.

With the inactive bins at the establishment level, Table 1.5 shows the establishments are generally more balanced than firms based on 1929 observables. Once the average number of wage earners is included as a control variable, only one t-statistic is significant and then only at the 10 percent level with an incredibly small coefficient estimate. It is expected that at least one test returns a significant result in a random sample of 24 t-statistics. Even so, the average hours a plant operates is included as a control variable in the main estimations where possible.[9] With these tables suggesting the sample is balanced before the crash and consistent with the identification assumption, the next section presents the precise empirical model used to estimate the effects of employee stock ownership on firm and establishment outcomes.

1.3.4 Regression Models

Based on the identification strategy defined above, the causal effect of employee stock ownership can be estimated using a straight-forward difference-in-difference regression. In the main specifica-

[9]Average hours a plant operates is missing in some years and causes the sample size to be reduced significantly in a few estimations.

Table 1.3: 1929 Company Summary Statistics By Timing of Rank-and-File ESOP Expiration

	Mean	Median	Obs.
	(1)	(2)	(3)
Inactive Before 1929			
Average No. Salaried Employees	17	0	8
Average No. Wage Earners	2,302	1,020	8
Average Value of Production	10,400,000	6,673,458	8
Average Hours Plant Operation (per week)	109.05	123.44	8
Average Total Wages	1,303,766	1,144,396	8
Inactive Between 1929 and 1931			
Average No. Salaried Employees	24.067	0	15
Average No. Wage Earners	3,360	51	15
Average Value of Production	13,700,000	550,969	15
Average Hours Plant Operation (per week)	124.44	168	15
Average Total Wages	599,456	23,778	15
Inactive Between 1931 and 1933			
Average No. Salaried Employees	69.25	51	4
Average No. Wage Earners	10,812.65	1,300.42	4
Average Value of Production	52,100,000	61,800,000	4
Average Hours Plant Operation (per week)	130.68	151.69	4
Average Total Wages	3,277,753	1,887,352	4
Inactive Between 1933 and 1935			
Average No. Salaried Employees	216.75	12	4
Average No. Wage Earners	30,910	4,349	4
Average Value of Production	23,600,000	29,300,000	4
Average Hours Plant Operation (per week)	126.74	140.08	4
Average Total Wages	2,297,674	2,285,306	4
Plans Still Active by 1935			
Average No. Salaried Employees	199.29	0	7
Average No. Wage Earners	5,830	3,784	7
Average Value of Production	54,600,000	28,100,000	7
Average Hours Plant Operation (per week)	92.71	50	7
Average Total Wages	2,340,620	2,050,284	7
Total			38

Note: Firm-level data aggregated up from establishment-level data digitized by Vickers and Ziebarth (2018). ESOP data collected from National Industrial Conference Board, Inc. (1928), and plan length data collected by author from various sources.

Table 1.4: 1929 Establishment Summary Statistics

	Mean	SD	$\overline{Active}-\overline{Inactive}$	Uncond. T	Cond. T	N
Value of Production	32.6	3.86	23.3	1.82*	0.56	277
Avg. Hrs. Plant Operation (Per Week)	125	2.94	27.9	2.89***	-0.87	277
Avg. Ind. Days Worked (Per Week)	6.22	0.13	0.28	0.66	-0.02	275
Avg. Ind. Hours Worked (Per Week)	54.4	0.60	1.64	0.82	1.37	277
Total Wages	2.74	0.32	1.41	1.34	0.97	276
Total Salaries	0.93	0.23	0.44	0.57	0.00	277
Avg. No. Wage Earners	998	109	379	1.05	1.35	277

Note: Establishment-level data from Vickers and Ziebarth (2018). ESOP and plan activity data from National Industrial Conference Board, Inc. (1928) and various sources collected by author. All establishments of firms ever having ESOPs in CoM data included. Value of production, total wages, and total salaries shown in millions of 1929 USD. T-statistics for equality of means based on whether ESOP active in 1929. Conditional t refers to outcome on active 1929 dummy with industry and Federal Reserve fixed effects as controls. *, **, and *** denote significance at the 10, 5 , and 1 percent levels, respectively.

tions, the dependent variable is the growth rate of various establishment-level outcomes of interest, where $g_{i,t} = \ln(y_{i,t}) - \ln(y_{i,t-1})$. The baseline regression specification is provided as equation 1.1 below.

$$\begin{aligned} g_{i,t} = \alpha_0 + \beta \mathbb{1}_{\text{Active}_{f,t}} + \alpha_2 \mathbb{1}_{\text{Listed}_f} + \alpha_3 \ln(y_{i,t-1}) \\ + \Gamma X_{i,t} + \omega_I \times \gamma_t + \delta_{FED} \times \gamma_t + \nu_i + \varepsilon_{i,t} \end{aligned} \tag{1.1}$$

The coefficient of interest which captures the growth effects of having an active rank and file ESOP during the Great Depression is represented by β. To accurately summarize this effect, the omitted category in the estimation is the group of rank-and-file ESOPs that are not active at time t. That is, the regression compares the establishments with ESOPs still active based on the pre-determined length of the firm's contract and timing of renewal before the crash to establishments with programs that are already inactive.

The preferred regression also includes a dummy variable equal to one if the firm is listed on a

Table 1.5: 1929 Establishment Conditional T-Statistics

	Inactive b/t 1929 and 1931	Inactive b/t 1931 and 1933	Inactive b/t 1933 and 1935	Still Active post-1935	N
Value of Production	1.25	0.57	1.04	2.59**	272
Avg. Hrs. Plant Operates (Per Week)	-0.97	0.65	-0.72	-0.52	272
Avg. Ind. Days Worked (Per Week)	-0.23	0.03	0.68	0.21	270
Avg. Ind. Hours Worked (Per Week)	1.38	1.54	1.50	0.34	272
Total Wages	0.73	1.57	1.45	1.42	271
Total Salaries	-0.08	0.26	0.09	0.35	272
Avg. No. Wage Earners	1.07	1.82*	1.62	1.53	277
With Additional Controls					
Value of Production	0.91	-0.86	-0.74	1.54	272
Avg. Hrs. Plant Operates (Per Week)	-1.29	-0.17	-1.79*	-1.31	272
Avg. Ind. Days Worked (Per Week)	-0.24	0.02	0.65	0.20	270
Avg. Ind. Hours Worked (Per Week)	1.37	1.51	1.45	0.33	272
Total Wages	0.16	-0.28	-1.04	-0.27	271
Total Salaries	-0.39	-0.63	-1.04	-0.46	272

Note: Establishment-level data from Vickers and Ziebarth (2018). ESOP and plan activity data from National Industrial Conference Board, Inc. (1928) and various sources collected by author. All establishments of firms ever having ESOPs in CoM data included. Conditional t refers to outcome on inactive dummy for specific window with industry and Federal Reserve fixed effects as controls. Bottom panel also includes average no. wage earners as control. *, **, and *** denote significance at the 10, 5 , and 1 percent levels, respectively.

major stock exchange. Listed firms may have differing access to financial capital and have more sophisticated financial reporting than private firms, and employees may more easily resell their shares if their employer is listed.

Other control variables include the initial level of the growth outcome variable, $y_{i,t-1}$, and a vector of establishment-level controls suggested by the balance tables, X. This includes the number of wage earning employees and the average hours of operation for the establishment at time t.[10] Finally, the preferred regression structure includes three sets of fixed-effects: year by industry, year by Federal Reserve district, and establishment denoted by $\gamma_t \times \omega_I$, $\gamma_t \times \delta_{FED}$, and ν_i, respectively. These fixed effects control for growth factors that may affect specific sectors and regions of the country in certain years.

One important policy enacted during this period of study is the National Industrial Recovery Act (NIRA) which differentially affected economic sectors starting in 1933 (Roos, 1937). It was a short-lived program that sought to stabilize employment in the economy during the Great Depression. President Roosevelt had a particular interest in the the automobile industry, which was prone to dramatic shifts in employment and production because of the demand for new car models (Cooper and Haltiwanger, 1993). The year by industry fixed effects in the preferred regression will control for these shocks for companies in the automobile industry in my sample and should alleviate concerns about the NIRA affecting the coefficient estimates.

The Federal Reserve district fixed effects control for the different policies and approaches enacted by the districts in response to the Great Depression which could have impacted businesses operating in certain districts. Richardson and Troost (2009) show that rates of bank failure differed along the 6th and 8th Federal Reserve District border in Mississippi because of the different approaches to monetary policy by each District bank. Firms having establishments primarily in communities with high rates of bank failure could affect business completely unrelated to employee stock ownership, and the fixed effects control for this additional volatility. As a robustness check, state fixed effects were included instead of Federal Reserve District fixed effects, but they were found to explain very little variation.

For a concrete example of how the identification of employee stock ownership works in prac-

[10]In the preferred regression, these control variables are not interacted with the year fixed-effects. However, these additional interactions are something I consider in the robustness section.

tice, consider two similar tire manufacturers in the Census of Manufactures data, Kelly-Springfield and Firestone. Kelly-Springfield was founded in Springfield, Ohio in 1894 and listed on the New York Stock Exchange in 1916 (Kelly Tires: Commercial Truck Tires, 2022; Rousseau and Watchel, 1998). In December 1917, a rank-and-file employee stock ownership program was adopted, and by that point, the company had moved its headquarters to New York City (National Industrial Conference Board, Inc., 1928). Responding to the query by the National Industrial Conference Board, Inc. (1928), they state that they were no longer actively selling stock to employees by 1928.

Founded in 1900, Firestone Tire & Rubber was headquartered in Akron, Ohio (Sull, 1999). In 1902, it adopted its first rank-and-file employee stock ownership program and was still actively selling stock to employees in 1928 (National Industrial Conference Board, Inc., 1928). This plan was renewed early in 1929 and was set to run for the following five years. Also in 1929, it was listed on the New York Stock Exchange (Rousseau and Watchel, 1998).

In terms of the regression, all of the Kelly-Springfield establishments would always be in the inactive control group and the active indicator in equation 1.1 is always equal to zero. The establishments of Firestone Tire & Rubber, however, are all treated until their pre-Depression plan expires in 1934, when they move to the control group. Both companies are listed throughout the sample period, meaning the listed indicator variable is turned on, and the differences in their establishment-level outcome variables captured by the active indicator represent the causal effect of being locked into a program in 1929 through 1934. It should be noted that the identification strategy outlined above only applies to the structure of rank and file ESOPs, and selective programs are not studied here. Firms that only have selective ESOPs are exluded from the main dataset and thus are not included in the control group.[11]

I use an additional regression model to estimate the effect of the ESOP becoming inactive. The bins in equation 1.2 are identical to the categories in the balance tables. Instead of forcing the effects to be the same for each level of treatment, this specification allows the coefficient estimates to vary based on the duration of activity after 1929. The control variables and fixed effects are the same as in the baseline model.

[11]There were only a few selective ESOPs identified in the CoM sample to begin with. For robustness, specifications with selective ESOPs included in the main sample were estimated with an indicator for having a selective program, but in most cases the coefficients were not significant and did not alter the remaining estimates.

$$g_{i,t} = \alpha_0 + \beta_1 \mathbb{1}_{\text{Inactive}_{f,1929-31}} + \beta_2 \mathbb{1}_{\text{Inactive}_{f,1931-33}} + \beta_3 \mathbb{1}_{\text{Inactive}_{f,1933-35}} + \beta_4 \mathbb{1}_{\text{Active}_{f,1935}} + \alpha_2 \mathbb{1}_{\text{Listed}_f} + \alpha_3 \ln(y_{i,t-1}) + \Gamma X_{i,t} + \omega_I \times \gamma_t + \delta_{FED} \times \gamma_t + \nu_i + \varepsilon_{i,t} \quad (1.2)$$

The advantage of estimating the effects of these different treatment levels is it allows me to see how the duration of the program matters and specifically, whether there is a linear relationship between duration and growth. With the four β_j estimates above, the omitted category is the group of establishments or firms that never had an active program. Therefore, each coefficient should be interpreted as the effect of becoming inactive in the specified window as opposed to being inactive for the entire post-1929 sample.

1.4 Results

1.4.1 Establishment-Level Productivity and Worker Utilization

The most often cited motivation for a firm to offer an ESOP is to increase worker productivity, and this holds for this early period of employee stock ownership as mention in Section 1.2.[12] This increases firm profits if the increase in productivity outweighs the cost of the program which consists of administrative fees and stock discounts. To test this main motivator, results of ESOPs' effects on a proxy for productivity, value of product, and for worker utilization, hours worked for individual worker per week, are presented first. Beyond the productivity effects, we might expect management at companies with ESOPs to push their employees harder since the programs should encourage the workers' interest in the company's welfare. Estimating the effect of having an active program on a measure of worker utilization is a way to capture this.

Table 1.6 presents the results with the real value of output growth and average days worked per worker as the dependent variables. The size of the firm in terms of the number of wage earners and the hours of operation are included as control variables and versions of equation 1.1 are estimated as discussed in Section 1.3. With these controls, the active indicator variable captures worker output and hours worked driven by having an active, employee stock ownership program that employees are paying into at time, t.

[12]For narrative evidence, see this quote from a representative of New York Trust Company in 1926. "The chief purpose of a company's efforts to enroll its employees as stockholders is to engage the interest of the rank and file of the workers. For the company itself there is no immediate financial gain. On the contrary, the stock is usually sold at less than its market price. The indirect profit to be realized by the company will depend upon the effect of stock ownership upon the workers" *The Los Angeles Times*, Sept. 1, 1926

Table 1.6: Effects of ESOP Activity on Establishment Productivity

Dependent Variable:	$g_{\text{RealValueProduct}_{i,t}}$			$g_{\text{AveDaysWorkedPerWorker}_{i,t}}$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Active	-1.038	-1.038*	-1.112*	-0.188***	-0.197***	-0.193***	-0.190***
	(0.650)	(0.650)	(0.593)	(0.038)	(0.051)	(0.050)	(0.048)
Initial Value	-1.009***	-1.009***	-1.015***	-0.267	-0.234	-0.227	-0.173
	(0.152)	(0.152)	(0.155)	(0.241)	(0.189)	(0.191)	(0.189)
Listed		1.379***	-0.076		-0.012	-0.009	-0.004
		(0.342)	(0.791)		(0.038)	(0.036)	(0.033)
Num Wage Earners			0.664*			-0.005	-0.004
			(0.354)			(0.009)	(0.008)
Hours of Operation							-0.037
							(0.031)
Establishment FE?	Yes	Yes	Yes	No	No	No	No
Year×Industry FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year×Fed District FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	336	336	335	102	102	102	102
R-Squared	0.816	0.925	0.927	0.615	0.616	0.617	0.630

Note: ESOP data collected from National Industrial Conference Board, Inc. (1928) and various sources collected by the author. Establishment-level data from (Vickers and Ziebarth, 2018). Due to data availability, data on wage earners is the average of monthly data on wage earners. Only establishments which are branches of a larger company ever having an ESOP are included in the sample. Columns (5) through (7) only present a cross-section due to missing data and do not include establishment or year fixed effects. Robust standard errors are given in parentheses. *, ** and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table 1.6 shows that active programs have a negative effect on the real value of the output produced at the establishment level. This estimate is quite large, but recall that this is the average effect over a two-year period based on the establishment-level data structure. Columns (4) through (7) show ESOPs also have a statistically significant negative effect on average days worked per worker. Given the setting in the midst of the Great Depression, it seems unlikely that employees are the ones choosing to work less. Instead, it seems reasonable that the managers at the establishment are cutting back their hours for various reasons. Nevertheless, these results suggest establishments with active ESOPs could have better utilized their workforce, especially if these programs were intended to attract high-quality, loyal employees.

Instead of forcing the effect of having an active program to be the same in each period, Table 1.7 estimates equation 1.2 which allows the effect to vary depending on how long it is in place. The control variables are unchanged from the previous table. Column (3) is the preferred regression which controls for establishment size, and Figure 1.4 is a visual representation of the coefficients

Table 1.7: Effects of Intensity of ESOP Activity on Establishment Output Growth

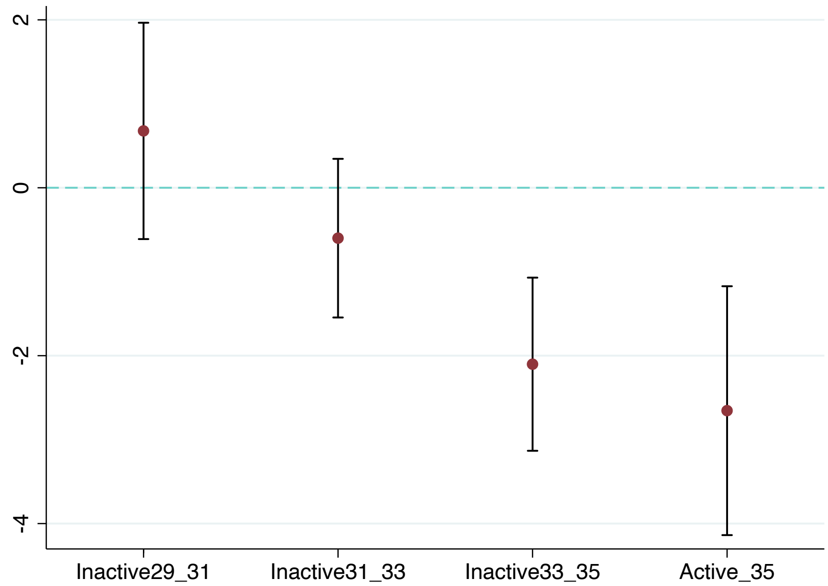
Dependent Variable: $g_{\text{RealValueProduct}_{i,t}}$			
	(1)	(2)	(3)
$\text{Inactive}_{1929-1931}$	0.890*	-0.251	0.677
	(0.476)	(0.467)	(0.650)
$\text{Inactive}_{1931-1933}$	0.325	-0.817	-0.601
	(0.326)	(0.517)	(0.477)
$\text{Inactive}_{1933-1935}$	-1.403***	-2.545***	-2.102***
	(0.342)	(0.547)	(0.521)
Active 1935	-0.540**	-1.681***	-2.655***
	(0.249)	(0.526)	(0.748)
Initial Value	-1.020***	-1.020***	-1.026***
	(0.152)	(0.152)	(0.156)
Listed		-1.141**	-2.067***
		(0.549)	(0.719)
Total Wage Earners			0.625*
			(0.355)
Establishment FE?	Yes	Yes	Yes
Year×Industry FE?	Yes	Yes	Yes
Year×Fed District FE?	Yes	Yes	Yes
R-Squared	0.922	0.922	0.923
Obs.	336	336	335

Note: ESOP data collected from National Industrial Conference Board, Inc. (1928) and various sources collected by author. Firm-level data aggregated up from establishment-level CoM data digitized by Vickers and Ziebarth (2018). Due to data availability, data on wage earners is the average of monthly data on wage earners. All firms ever having ESOPs in CoM data are included. The omitted category is establishments which had an inactive ESOP in 1929. Robust standard errors are given in parentheses. *, ** and *** denote significance at the 10, 5, and 1 percent levels, respectively.

which capture these differing effects.

Column (3) shows that there is an increasingly negative effect the longer the program is active following the 1929 stock market crash. Further, Figure 1.4 suggests the effect is strongly linear. This provides additional evidence that I am capturing the causal effects of these programs as it suggests the longer employees are paying into these programs, the lower their effort and in turn their real output.

Figure 1.4: Intensity of Treatment on Output Over Time



Note: Graphical representation of coefficients from column (3) of Table 1.7. The omitted category is establishments which had an inactive ESOP in 1929. Black error bars represent 95% confidence intervals.

1.4.2 Potential Mechanisms: Returns, Firm Size, and Real Wages

With these baseline results of reduced productivity and worker utilization established, this section presents possible mechanisms driving worker behavior. In doing so, it explains what factors and employee characteristics a firm should account for when deciding whether to implement such a program.

To more directly understand the role of the stock market in discouraging workers and leading to decreased establishment-level output growth, Table 1.8 adds in stock price and return data from Center for Research in Securities Prices (CRSP) (2022) for listed firms ever having ESOPs in the CoM data. Due to many ESOP firms not being listed, the sample size falls substantially for these results. Nevertheless, they provide suggestive evidence that stock prices and returns are significantly related to productivity. Column (3) includes an interaction between having a negative return and an active program. Since having a non-negative return and an active program is the omitted category, the interaction coefficient shows that the overall negative productivity effect from having an active program becomes stronger for companies with worse returns.

Table 1.8: Effect of ESOPs and Stock Performance on Establishment Output Growth

Dependent Variable: $g_{\text{RealValueProduct}_{i,t}}$			
	(1)	(2)	(3)
Active	-1.00***	-0.300	-0.513
	(0.247)	(0.192)	(0.295)
Stock Price	1.725***		
	(0.374)		
Total Return		0.248***	
		(0.054)	
LowReturn × Active			-4.217**
			(1.492)
Initial Value	-0.860***	-0.860**	-0.860**
	(0.285)	(0.285)	(0.285)
Establishment FE?	Yes	Yes	Yes
Year×Industry FE?	Yes	Yes	Yes
Year×Fed District FE?	Yes	Yes	Yes
Observations	67	67	67
R-Squared	0.952	0.952	0.952

Note: ESOP data collected from National Industrial Conference Board, Inc. (1928) and various sources collected by author. Establishment-level data from CoM data digitized by Vickers and Ziebarth (2018). Due to data availability, data on wage earners is the average of monthly data on wage earners. Stock price and return data extracted from Center for Research in Securities Prices (CRSP) (2022). All establishments ever having ESOPs in CoM data are included. Robust standard errors are given in parentheses. *, ** and *** denote significance at the 10, 5, and 1 percent levels, respectively.

To get a better understanding of the varying effects of the free-rider problem, I next study real output while directly controlling for the size of the firm. A small firm is defined as a company having less than 2,770 total employees, which is the first tercile, or the 33rd percentile, for firms ever having ESOPs in 1929. The free-rider problem is believed to be mitigated in smaller companies where one employee's effort level can more directly affect firm profit (Blasi et al., 1996). If employees believe their individual choice to work harder can have non-negligible effects on the stock price, the firm is more likely to see increased productivity.

In Table 1.9, the coefficient for the small firm, active interaction in column (2) is positive and statistically significant consistent with the narrative that incentive effects of ESOPs are more powerful in smaller firms. For comparison, columns (3) and (4) of Table 1.9 include an indicator for being a small establishment, which is defined as the first tercile of the average number of wage earners in the establishment sample. In contrast to firm size, the size of the establishment has no statistically significant relationship with output growth. While the results thus far suggest that there is a significant negative effect of having an active ESOP during the Great Depression, this response dissipates when the free-rider problem is at least partially mitigated. The fact that the size of the establishment does not seem to matter suggests employees may have accounted for how their individual effort affects the stock price which depends on the size of the firm and not the size of the establishment.

Next, I turn to compensation to understand how employees may have been discouraged by a falling return to labor. The Census of Manufactures dataset has detailed information on total wages at the establishment level. Wage growth might be expected to fall when a company has an active ESOP for two reasons. Mechanically, most companies allowed employees to extract a certain percentage of their wages each month to go directly to purchasing company stock. Unfortunately, I am unable to discern if total wages in the United States Census of Manufactures data is net wages, after this deduction has been taken, or gross real wages. If it is net, then a strong, causal relationship is expected. Otherwise, a negative effect would reflect a substitution between real wages and stock benefits by the firm. This second mechanism allows firms to defer payments to employees until a future period when it may pay dividends and likely represents a key advantage of these programs for firms struggling to pay employees during the depths of the Great Depression.

The first two columns of Table 1.10 suggest that there is not a strong effect overall before the timing of becoming inactive is taken into account. The final two columns of Table 1.10, however, suggest there is indeed a strong, negative causal effect of having an active pre-Depression plan on wage growth, specifically for establishments with plans expiring after 1931. The omitted category remains establishments whose programs were inactive prior to 1929. The coefficient estimate for the group of establishments with active programs in 1935 is still negative, but it breaks the linear pattern seen before and is only significant at the 10 percent level as shown by Figure 1.5. It is not obvious why this would be the case, but it suggests that those firms whose plans are active the longest do not see their wages fall quite as much as other firms with programs.

One potential explanation is that these plans do not require employees to deposit as high of a percentage of their wage income into their employee stock account for these longer plans. Or rather, it may reflect employees finishing their subscription payments earlier in the sample before the expiration of the plan and starting to see their wages rebound. Even without statistically significant

Table 1.9: Effects of ESOP Activity on Output Growth in Small Establishments

Dependent Variable: $g_{\text{RealValueProduct}_{i,t}}$				
	(1)	(2)	(3)	(4)
Active	-0.421**	-0.477**	-0.530***	-0.601***
	(0.197)	(0.206)	(0.189)	(0.054)
Small Firm	0.164			
	(0.245)			
Small Firm × Active		0.422*		
		(0.237)		
Small Estab.			-0.303	
			(0.290)	
Small Estab. × Active				-0.312
				(0.321)
Initial Value Product	-0.605***	-0.604***	-0.601***	-0.601***
	(0.053)	(0.053)	(0.054)	(0.054)
Num Wage Earners	0.731***	0.730***	0.684***	0.684***
	(0.090)	(0.090)	(0.103)	(0.103)
Listed	-0.300	-0.323	-0.368*	-0.366*
	(0.207)	(0.212)	(0.199)	(0.198)
Year×Industry FE?	Yes	Yes	Yes	Yes
Year×Fed District FE?	Yes	Yes	Yes	Yes
Observations	335	335	335	335
R-Squared	0.776	0.777	0.777	0.777

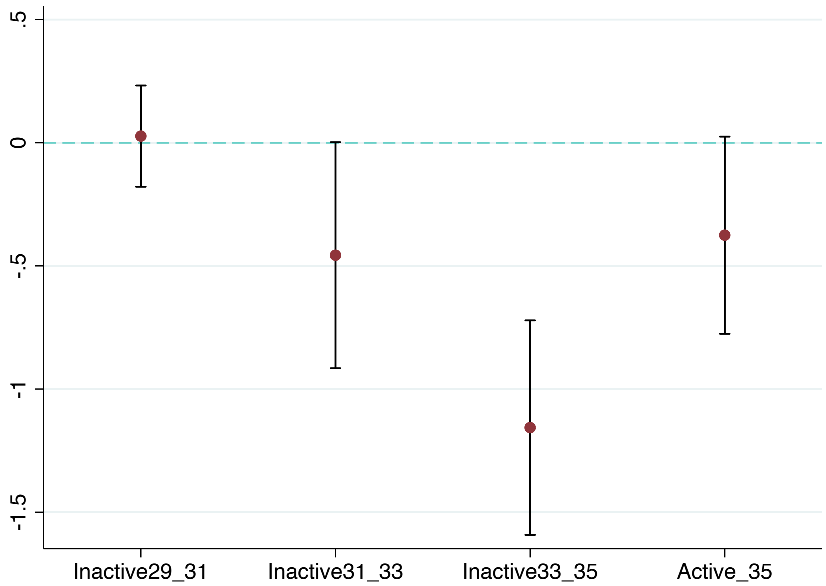
Note: ESOP data collected from National Industrial Conference Board, Inc. (1928) and various sources collected by the author. Establishment-level data from Census of Manufactures digitized by Vickers and Ziebarth (2018). Due to data availability, data on wage earners is the average of monthly data on wage earners. Only establishments which are branches of a larger company ever having an ESOP are included in the sample. Small is defined as being below the 1st tercile for either establishments or firms based on the distribution of the average number of wage earners. Robust standard errors are given in parentheses. *, ** and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table 1.10: Effect of ESOPs on Real Wage Growth

Dependent Variable: $g_{\text{RealWagesPerWorker}_{i,t}}$				
	(1)	(2)	(3)	(4)
Active	0.043	0.043		
	(0.087)	(0.087)		
Inactive$_{1929-1931}$			0.510**	0.027
			(0.201)	(0.104)
Inactive$_{1931-1933}$			0.026	-0.457*
			(0.150)	(0.232)
Inactive$_{1933-1935}$			-0.673***	-1.157***
			(0.030)	(0.220)
Active 1935			0.108***	-0.375*
			(0.011)	(0.203)
Initial Wages	-1.321***	-1.321***	-1.323***	-1.323***
(Per Worker)	(0.112)	(0.112)	(0.111)	(0.111)
Listed		0.673***		-0.483**
		(0.030)		(0.207)
Establishment FE?	Yes	Yes	Yes	Yes
Year×Industry FE?	Yes	Yes	Yes	Yes
Year×Fed District FE?	Yes	Yes	Yes	Yes
Observations	385	385	385	385
R-Squared	0.863	0.863	0.863	0.863

Note: ESOP data collected from National Industrial Conference Board, Inc. (1928). Establishment-level data from Census of Manufactures digitized by Vickers and Ziebarth (2018). Due to data availability, data on wage earners is the average of monthly data on wage earners. Only establishments which are branches of a larger company ever having an ESOP are included in the sample. Robust standard errors clustered a the firm level are given in parentheses. *, ** and *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

Figure 1.5: Intensity of Treatment on Wage Growth Over Time



Note: Graphical representation of coefficients from column (4) of Table 1.10. The omitted category is establishments which had an inactive ESOP in 1929. Black error bars represent 90% confidence intervals.

lower wages, Figure 1.4 still shows that output is significantly decreased for establishments with active plans in 1935. This might suggest current lower real wages are not be the only mechanism leading to reduced output, or periods of lower wages may have a more permanent productivity effect.

The next subsection discusses various robustness tests which further validate the results presented.

1.4.3 Extensions and Robustness

The establishment results drive the main conclusions of this paper, but I also estimate specific outcomes at the firm level. Specifically, I study the implications of the baseline fall in productivity on the number of company branches and firm survival. In the aggregate, it is not clear that employee stock ownership had any significant effects on the economy during the Great Depression even though they had clear negative productivity effects at the establishment level. While there is a slight negative effect on the number of branches throughout the Great Depression, the results are not statistically significant. Similarly, the survival results suggest there may have been a positive relationship between surviving the Great Depression and having a program that expired early in the downturn.

The outcome variables for the firm-level regressions are likely imperfect measures which may attenuate the results. Survival is defined as having at least one establishment present in the 1935 panel. Thus, if a firm is missing establishments it may be incorrectly denoted as not surviving. This is also true for the number of branches, which clearly depends on the response rate to the Census of Manufactures survey. The firm balance tables as well as these regression results are included in the Appendix. The remainder of the paper focuses on the establishment data and results.

To ensure the main, establishment-level results are not contingent on the specific regression or driven by any individual firm, the above results are re-run with various alterations. First, given the wide-range in the number of branches across firms, the largest company in terms of number of establishments in the dataset is excluded and the results re-estimated. The largest company overall is United States Steel having 92 establishments. U.S. Steel had an active pre-Depression plan that expired in 1934 putting it in the treated category until the 1935 panel. With this firm excluded, the sample size becomes smaller, of course, but the qualitative results remain consistent. The signs of the coefficients and their significance levels generally hold with this sub-sample. For the sake of space, these tables and the others mentioned in this section are relegated to Appendix 1.6.4.

I additionally altered the main sample and re-ran the results after excluding all 1935 observations. This is to ensure that the year when there are the fewest treated firms and establishments is not driving the results. The key results remain consistent with this 1929 to 1933 subsample, and this should alleviate concerns that the firms with plans expiring post-1935 are greatly affecting the results.

Going back to the full sample of establishments, I also adjust the main regression by interacting the control variables with year fixed effects. This ensures that changes in the work force or number of branches over time are not driving the effects of employee stock ownership. The main results are again robust to this more rigorous specification as shown in Table 1.24.

Previous iterations of this project have also studied the entire, unbalanced sample of firms and establishments. That is, all branches in the Census of Manufactures data regardless of their history with employee stock ownership programs were included, and the coefficients were measured by interacting ESOP and active indicators. For the sake of space, these results are not presented but are available upon request. Except for a few differences in the level of significance, they are consistent with the results discussed in the previous subsections, albeit these results cannot be interpreted causally due to the unbalanced nature of the entire CoM sample.

With these robustness tests completed, the reader should feel confident about the reliability of the main results on productivity. The following subsection delves into how the robust, causal results on lower productivity and the potential mechanisms affecting worker behavior discussed above relate to the firm's profit maximization problem.

1.4.4 Discussion

Arguably the most prominent question in the theoretical literature on employee stock ownership is if and how these programs can increase worker effort for the firm. Tables 1.6 and 1.7 present results counter to principal-agent theory. Instead of aligning workers' payoffs with firm profitability, the results suggest employees were less productive as a result of having part ownership of the firm during the Great Depression. Overall real output and average days worked controlling for firm size and hours of operation fell because of these active programs.

Any firm profit model seeking to reflect worker behavior when participating in this type of program should account for the worker's personal value of the stock. This includes the real return on the investment and the employee's standing within the company. Table 1.8 suggests a negative return on investment can disincentivize workers. Table 1.9 shows, however, that workers may value stock more in smaller firms where they feel they can have more of an effect on its price movements. Additionally, when employees give up their wage to purchase stock that rapidly loses its value, this further discourages them. These results provide a cautionary tale for profit-maximizing firms that offering stock to incentivize employees can have negative ramifications when aspects like returns, firm size, and wage growth are not taken into account.

The introduction suggests another theory beyond solving the principal agent problem that could explain a firm's motivation to offer an employee stock ownership program: efficiency wage theory. In the traditional sense, efficiency wages, that is wage rates above market-clearing, should increase worker effort since the opportunity cost of being fired is higher (Shapiro and Stiglitz, 1984).

The historical setting analyzed here allows me to understand if this relationship changes when the real benefit to working falls below the market level. Recall the "efficiency wages" here consist of a worker's real wage plus their payoff from stock ownership. Instead of this package surpassing the value of market wages as intended, the drastic fall in stock prices often led to a cut in total benefits in real terms (National Industrial Conference Board, Inc., 1930). This is precisely what Table 1.8 reflects, specifically column (3). In column (3), the effect of having an active program on its own is not statistically significant, but its interaction with having a negative annual return is negative and statistically significant. The unexpected, low realized payoff thus served to disincentivize employee effort. This empirical study of employee stock ownership during the Great Depression thus provides an example where uncertainty in the realized employee benefits package can have negative consequences.

Furthermore, there is reason to believe my results may be attenuated for the later sample years. A few of these firms did renew their programs after their pre-Depression plan expired, but based on the identification discussed, these firms and their establishments are still considered inactive (Davis, 1933). Therefore, firms that actually have ongoing employee stock ownership programs would be included in the control group and could be influencing those results. While plan renewals often had different prices and installment schedules that may have made the programs more favorable to employees, this suggests that the true negative effects of employee stock ownership on production could be greater.

At first, these productivity results seem to contradict the well-documented claim that the 1930s were the most productive decade in the twentieth century (Field, 2003). Field's paper, however, emphasizes the second half of the decade and notes high productivity being concentrated in specific industries, particularly novel sectors for the 1930s. Recall that the majority of employee stock ownership firms in this period had a long history and were concentrated in developed industries. Further, the data coverage for my productivity results are for the early period of the Great Depression, not the entire decade. The results in this paper thus provide a more nuanced perspective on how the first employee stock ownership programs may have stunted productivity early in the decade.

The puzzle of employee stock ownership thus persists since ESOPs have rebounded in popularity in different waves throughout U.S. history despite their poor performance during the Great Depression (Ott, 2011; National Center for Employee Ownership, 2021).

1.5 Conclusion

The novel data collected and the results discussed in this paper have shed additional light on the complexities of employee stock ownership. During the Great Depression, employee stock purchase programs did have an effect on production. Using the long-term structure of stock contracts and the quasi-random variation in their expiration for identification, I document a negative effect on real output and hours worked that has not been reported in the previous literature in any context. As a whole, and for large firms especially, the Great Depression results suggest employee stock ownership does work through price appreciation and efficiency-wage-type incentives and not primarily through group cohesion or pride in ownership.

A profit-maximizing firm should, thus, take into account their employees' value of stock ownership when deciding to implement a program. This setting shows workers may have various responses to these programs depending on the asset's return, their ability to effect the price, and what they are sacrificing to become shareholders. The Great Depression provides a unique environment where the real payoff to working continued to fall and suggests most employees were disincentivized to work their hardest. Employees at smaller firms may value their individual stocks more since they feel they can alter the price through increased productivity going forward. However, this mechanism only mitigates the negative productivity results seen in this period.

The results and accompanying discussion suggest that one model or theory alone cannot fully explain the various effects of employee stock ownership across firms. This paper makes strides at applying an alternative efficiency-wage theory to ESOPs during the Great Depression and shows the theory is consistent with the results for large companies, but incentive-compatibility more accurately explains some of the differing effects exhibited by smaller firms. Further narrowing down cases where the effects of employee stock ownership align with specific theories in the historical or modern setting is a promising area for future research.

In many ways, this research introduces additional questions regarding the popularity of these types of programs today. It is unclear if any lessons were learned from this early experience and why these programs continue to gain popularity. While I have mentioned some parallels to the early employee stock programs during the Great Depression and modern ESOPs, investigating these precise relationships and their evolution is left to future research.

Additional study should also be given to the various motives that led companies to reintroduce employee stock programs later in the twentieth century. The role of labor unions has been mentioned as driving these programs, but to my knowledge no empirical analysis has been conducted (Ott, 2011). Further, this paper focuses solely on manufacturing establishments, but employee purchase plans were prevalent in other industries, such as banking and financial services. Comparing the manufacturing results to other sectors of the economy could highlight key differences in employee stock programs, in practice.

1.6 Appendix

1.6.1 Firm-Level Balance Tables and Results

Table 1.11: 1929 Firm Summary Statistics

	Mean	SD	$\overline{Active}-\overline{Inactive}$	Uncond. T	Cond. T	N
Total Wage Earners	7,090	3,241	6,557	0.85	0.38	38
No. Branches	7.29	2.28	4.80	0.85	0.93	38
Yrs Since Initial Program	10.36	1.42	3.62	1.04	-0.06	38
Listed	0.32	0.08	0.08	0.44	-0.46	38

Note: Firm-level data aggregated up from establishment-level data digitized by Vickers and Ziebarth (2018). ESOP and plan activity data from National Industrial Conference Board, Inc. (1928) and various sources collected by author. All firms ever having ESOPs in CoM data included. T-statistics for equality of means based on whether ESOP active in 1929. Conditional t refers to outcome on active 1929 dummy with industry and Federal Reserve fixed effects as controls. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table 1.12: 1929 Firm Conditional T-Statistics

	Inactive b/t 1929 and 1931	Inactive b/t 1931 and 1933	Inactive b/t 1933 and 1935	Still Active post-1935	N
Total Wage Earners	-0.16	1.89*	2.16**	0.69	38
No. Branches	0.67	1.00	0.88	0.21	38
Yrs Since Initial Program	-0.40	-1.01	3.05***	1.47	38
Listed	0.38	0.63	-0.45	-0.29	38
With Additional Controls					
No. Branches	0.77	-0.21	-0.19	-0.05	38
Yrs Since Initial Program	-0.41	-1.16	2.59**	1.39	38
Listed	0.37	0.62	-0.36	-0.27	38

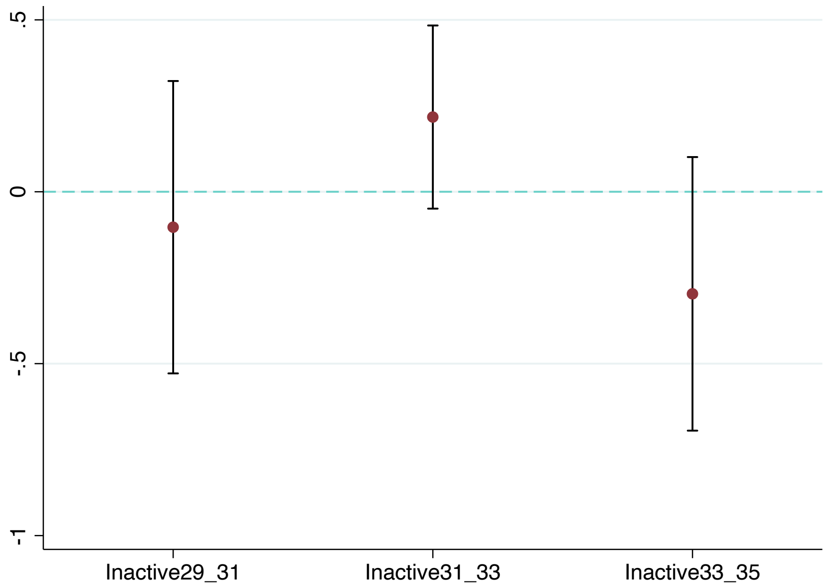
Note: Firm-level data aggregated up from establishment-level data digitized by Vickers and Ziebarth (2018). ESOP and plan activity data from National Industrial Conference Board, Inc. (1928) and various sources collected by author. All firms ever having ESOPs in CoM data included. Conditional t refers to outcome on inactive dummy for specific window with industry fixed effects as controls. Bottom panel also includes total no. wage earners as control. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table 1.13: Effects of Intensity of ESOP Activity on Firm Outcomes

Dependent Variable:		$\mathbb{1}_{\text{Survive}_{f,1935}}$				$\ln(\text{Branches}_{f,t})$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\text{Inactive}_{1929-1931}$	-0.135	-0.100	-0.103	-0.103	-0.152	-0.064	-0.044
	(0.258)	(0.210)	(0.211)	(0.212)	(0.403)	(0.166)	(0.173)
$\text{Inactive}_{1931-1933}$	0.376**	0.230*	0.218*	0.217	0.318	-0.262*	-0.266*
	(0.150)	(0.128)	(0.128)	(0.133)	(0.264)	(0.157)	(0.159)
$\text{Inactive}_{1933-1935}$	-0.240	-0.287	-0.297	-0.297	0.181	-0.162	-0.146
	(0.219)	(0.192)	(0.195)	(0.199)	(0.418)	(0.183)	(0.193)
Active 1935					-0.021	-0.325	-0.258
					(0.415)	(0.246)	(0.273)
Listed	-0.115	-0.079	-0.091	-0.091	-0.122	-0.177	-0.158
	(0.158)	(0.152)	(0.154)	(0.154)	(0.270)	(0.179)	(0.177)
Total Wage Earners		0.129***	0.154***	0.154***		0.494***	0.491***
		(0.036)	(0.052)	(0.053)		(0.044)	(0.044)
Branches			-0.054	-0.054			
			(0.115)	(0.118)			
Yrs Since Initial Program				-0.002			-0.049
				(0.056)			(0.094)
Industry×Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.437	0.559	0.561	0.561	0.582	0.873	0.874
Obs.	107	107	107	107	145	145	145

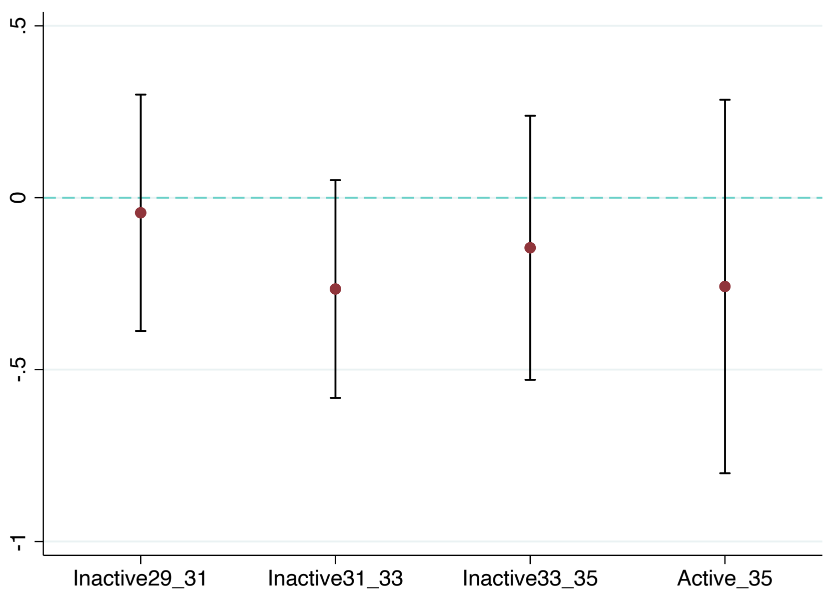
Note: ESOP data collected from National Industrial Conference Board, Inc. (1928) and various sources collected by author. Firm-level data aggregated up from establishment-level data digitized by Vickers and Ziebarth (2018). Due to data availability, data on wage earners is the average of monthly data on wage earners. All firms that have ever had ESOPs in CoM data included. The omitted category is firms which had an inactive ESOP in 1929. Robust standard errors are given in parentheses. *, ** and *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

Figure 1.6: Intensity of Treatment on Firm Survival Over Time



Note: Graphical representation of coefficients from column (4) of Table 1.13. The omitted category is establishments which had an inactive ESOP in 1929. Black error bars represent 95% confidence intervals.

Figure 1.7: Intensity of Treatment on Branches Over Time



Note: Graphical representation of coefficients from column (7) of Table 1.13. The omitted category is establishments which had an inactive ESOP in 1929. Black error bars represent 95% confidence intervals.

1.6.2 Theory: Outline of Firm Problem with an ESOP

This section provides a theoretical overview of the firm's profit maximization problem which explains why a firm would adopt a program to begin with. It also informs the structure of the results in terms of studying how firm size and stock performance relate to the effects of the ESOPs on productivity.

Before the program, we have the following basic firm profit maximization problem:

$$\max_{N} \pi = zF(N) - wN \tag{1.3}$$

After the introduction of the program, the firm still wants to maximize profits.

$$\max_{N} \pi = zF(N) - (w-x)N(\delta(p,N)x) - x(p-s) \tag{1.4}$$

- Where w is set by market but x chosen by employee
- $\delta(p,N)$ reflects value of stock to worker in terms of individual stake (in investment terms and/or personal value of holding stock)
  - Theory suggests value of stock is likely a function of firm size, N and market value of stock, p
- Net cost of program to the firm is $x(p-s)$ where p is the market price of the stock and s is employee price
- Theory suggests x can affect worker effort, but depends on firm size, N, and value of stock to worker (i.e. $N(\delta(p,N)x)$)
- For firm to decide to offer program, firm must believe increase in worker effort from x is larger than cost of implementing program
- **Marginal benefit**: $N'(\delta(p,N)x)\delta(p,N)$
- **Marginal cost**: $(p-s)$

After active program:

$$\max_{N} \pi = zF(N) - wN\beta(\delta(p,e)x) \tag{1.5}$$

- β reflects discount in relationship between x and N
  - β falls longer program has been inactive
  - Problem looks more and more like pre-program firm problem

1.6.3 Additional Figures

Figure 1.8: Sample Employee Stock Ownership Form

Form S. N. 518-I

STATEMENT OF ACCOUNT UNDER
SUBSCRIPTIONS FOR CAPITAL STOCK OF THE
AMERICAN TELEPHONE AND TELEGRAPH COMPANY
UNDER EMPLOYEES' STOCK PLAN DATED MAY 1, 1921

Name Alice Gray   Date December 31, 1925.

Month in Which Deductions from Pay Began	Number of Shares	Price	Amount Deducted to Date	Interest Credit to Date at 7% (See Note)	Total Credit to Date
April, 1924	1	118.00	63.00	3.98	66.98
March, 1925	2	121.00	60.00	1.78	61.78
August, 1925	3	125.00	45.00	.66	45.66
This amount represents your accumulated savings to date on the above subcriptions					$174.42

NOTE: If subscriptions are canceled before completion the interest credit will be computed at 6% instead of 7%.

Source: National Industrial Conference Board, Inc. (1928)

Figure 1.9: Sample of Consolidated Balance Sheet for Firestone Tire & Rubber Company

The Firestone Tire & Rubber Co

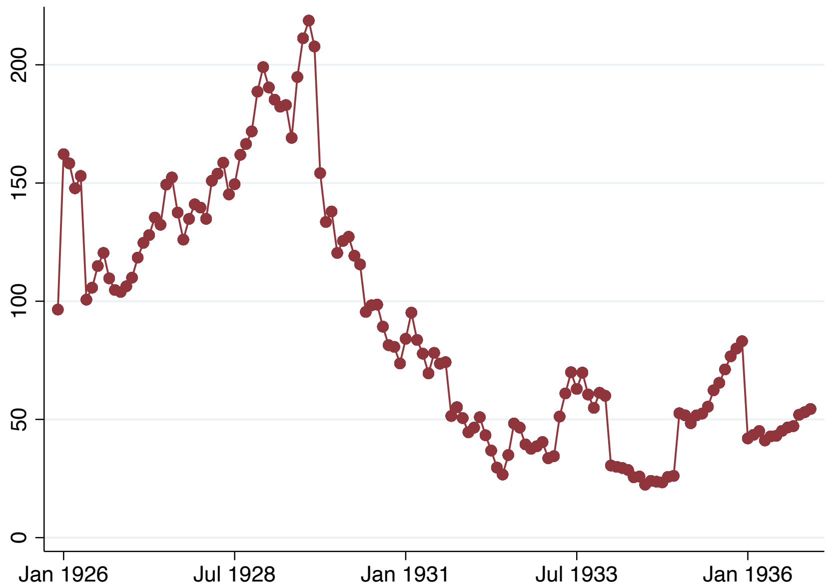
CONSOLIDATED BALANC

ASSETS

Current		
Cash	$11,613,286.89	
Customers' Notes and Accounts Receivable, less Reserve	19,134,779.73	
Inventories of Finished and In Process Products at cost, Materials and Supplies at the lower of cost or market	35,995,720.15	$ 66,743,786.77
Other Assets		
Employees' Stock Contracts secured by 236,515 shares of Common Stock of The Firestone Tire & Rubber Company	$ 3,346,880.07	
Securities owned—at cost ($391,750 deposited in escrow)	2,624,140.77	
Miscellaneous Accounts and Advances, less Reserve	1,828,172.04	7,799,192.88

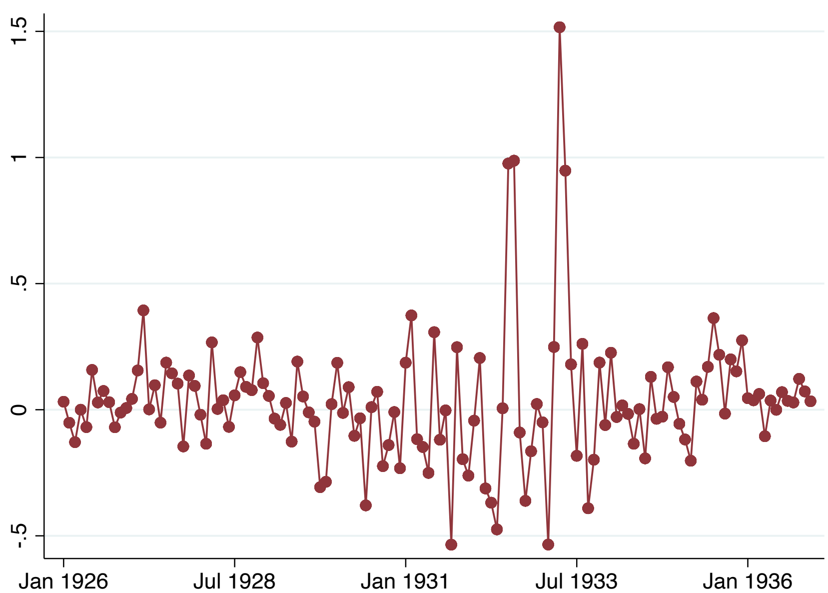
Source: The Firestone Tire & Rubber Company Annual Report, 1930

Figure 1.10: Stock Prices in Listed Sample Over Period of Interest



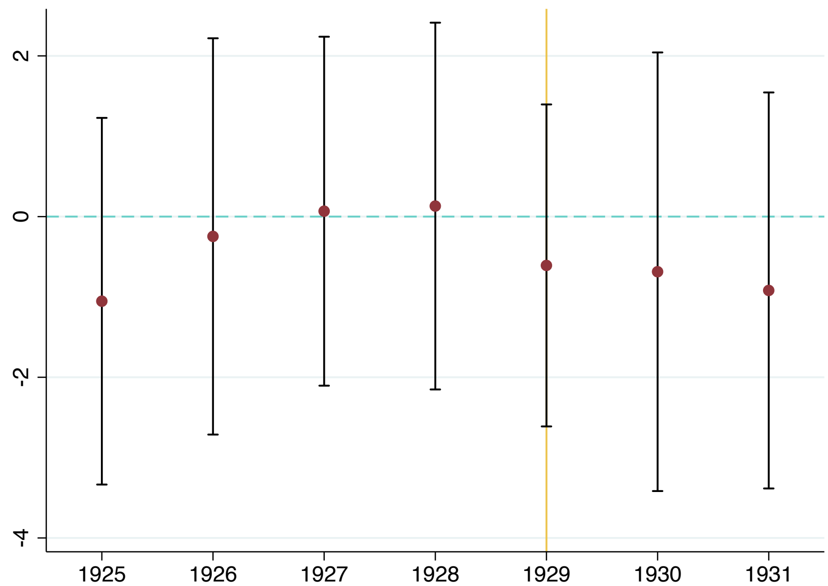
Notes: Monthly data extracted from Center for Research in Securities Prices (CRSP) (2022) and averaged to get annual data. Prices deflated to 1929 USD.

Figure 1.11: Stock Returns in Listed Sample Over Period of Interest



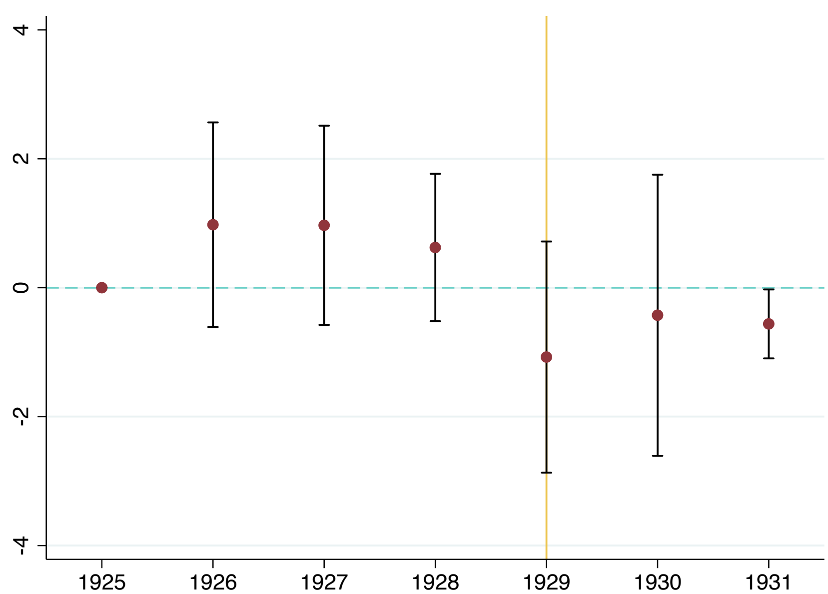
Notes: Monthly data extracted from Center for Research in Securities Prices (CRSP) (2022) and averaged to get annual data. Returns deflated to 1929 USD.

Figure 1.12: Parallel Trends in Stock Prices between Active and Inactive Firms



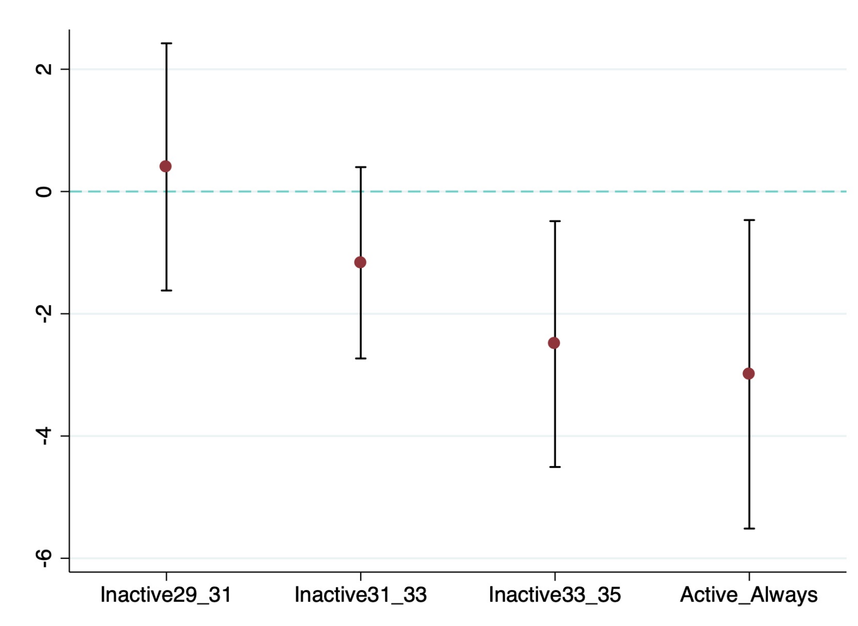
Notes: Monthly data extracted from Center for Research in Securities Prices (CRSP) (2022) and averaged to get annual data. Prices deflated to 1929 USD. Coefficients are from a difference-in-difference regression, and Inactive1929 firms in 1926 is omitted category. Black error bars represent 95% confidence intervals.

Figure 1.13: Parallel Trends in Stock Returns between Active and Inactive Firms



Notes: Monthly data extracted from Center for Research in Securities Prices (CRSP) (2022) and averaged to get annual data. Returns deflated to 1929 USD. Coefficients are from a difference-in-difference regression, and Inactive1929 firms in 1926 is omitted category. Black error bars represent 95% confidence intervals.

Figure 1.14: Intensity of Treatment on Output Growth Over Time, Excluding Largest Firm



Notes: Graphical representation from column (3) of Table 1.19. The omitted category is establishments which had an inactive ESOP in 1929. Black error bars represent 95% confidence intervals.

1.6.4 Additional Tables

Table 1.14: Years Companies Adopted Employee Stock Purchase Programs

Year	**No. of Companies**
1900 or earlier	3
1901-1905	14
1906-1910	13
1911-1915	30
1916-1920	111
1921-1925	162
1926-1927	17
No information	39

Source: National Industrial Conference Board, Inc. (1928) pg. 2

Table 1.15: ESOPs and Firm Survival by Industry

Industry	**Firms**	**ESOPs**	**Year Earliest ESOP**	**Survive**	**Percent Surviving (%)**
Beverages	67	0	NA	11	16
Ice cream	149	1	1914	36	24
Ice, manufactured	240	10	1919	67	28
Macaroni	4	0	NA	2	50
Malt	3	1	1923	3	100
Sugar, cane	2	0	NA	1	50
Sugar, refining	3	1	1918	2	67
Cotton goods	99	3	1900	37	37
Linoleum	1	0	NA	1	0
Planing mills	181	0	NA	27	15
Bone black	4	0	NA	2	50
Soap	17	4	1886	5	29
Petroleum refining	43	6	1920	12	30
Rubber tires	5	1	1924	1	20
Cement	14	1	1903	5	36
Concrete products	87	1	1923	17	20
Glass	20	1	1924	10	50
Blast furnaces	18	2	1918	7	39
Steel works	44	2	1920	18	41
Agricultural implements	16	3	1909	10	63
Aircraft and parts	18	1	1917	5	28
Motor Vehicles	15	1	1923	4	27
Cigars and cigarettes	5	1	1919	4	80
Radio equipment	25	1	NA	2	8
Total	1,080	41		289	

Note: Firm-level data aggregated up from establishment-level data digitized by Vickers and Ziebarth (2018). ESOP data collected from National Industrial Conference Board, Inc. (1928) and various sources collected by author.

Table 1.16: Summary of Rank and File Employee Stock Ownership Programs

Company Name	Stock Type	Start Date	End Date	Program Duration
Allegheny Steel Co.	Both	1927	1934	7 Years
American Ice Co.	Common	1927	1930	4 Years
American Rolling Mill Co.	Common	1929	1931	1.5 Years
American Sugar Refining Co.	Both	1929	1931	2.5 Years
American Tobacco Co.	Preferred	1929	1935	7 Years
Arkansas Power	N/A	1926	1929	3 Years
Bethlehem Steel Co.	Preferred	1929	1932	3 Years
Bemis Brothers	N/A	1928	1930	2 Years
Colgate-Palmolive-Peet Co.	Both	1928	1934	6 Years
Firestone Tire and Rubber Co.	Common	1929	1935	6 Years
General Motors Corp.	Preferred	1928	1929	2 Years
H.P. Hood & Sons	Common	1926	1930	4 Years
International Harvester	Preferred	1929	1935	6 Years
Kansas City Power	N/A	1928	1930	2 Years
Long-Bell Lumber	Common	1925	1930	5 Years
Middle West Utilities	Common	1928	1930	2 Years
Monongahela West Penn	N/A	1925	1929	4 Years
Procter & Gamble Co.	Common	1929	January 1, 1935	6 Years
Public Service Co. of Oklahoma	N/A	1928	1930	2 Years
Simmons Company	N/A	1925	1929	4 Years
Sears, Roebuck & Co.	Common	1926	1931	5 Years
Standard Oil Co. (Indiana)	Both	March 31, 1929	March 31, 1932	3 Years
Standard Oil Co. (New Jersey)	Both	January 1, 1929	December 31, 1931	3 Years
Standard Oil Co. (New York)*	Both	1927	July 31, 1931	4 Years
Standard Oil Co. (California)	Both	1926	1930	4 Years
Swift & Co.	Both	1928	1935	7 Years
Tampa Electric	Both	1926	1930	4 Years
Tide Water Oil Co.	Both	1929	1931	1.5 Years
Union Oil Co. of California	Both	1928	1930	2 Years
United States Steel Corp.	Common	1928	1934	6 Years

Note: Company data on employee stock ownership for firms with active programs that appear in main dataset. Various sources used including National Industrial Conference Board, Inc. (1928, 1930); Baker (1932); Davis (1933); and various company annual reports for 1929-1935.

Table 1.17: Comparison of Employee Purchase Price to Market Price of Company Stock

Price Comparison	**No. of Companies**
Market Price	
Never fell below employee purchase price	51
Dropped below employee purchase price	19
No information	20
Total	90

Source: National Industrial Conference Board, Inc. (1930)

Table 1.18: Effects of ESOP Activity on Establishment Productivity, Excluding Largest Firm

Dependent Variable:	$g_{\text{RealValueProduct}_{i,t}}$			$g_{\text{AveDaysWorkedPerWorker}_{i,t}}$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Active	-0.832	-0.832	-0.436	-0.233***	-0.215***	-0.196***	-0.197***
	(1.084)	(1.084)	(0.781)	(0.054)	(0.064)	(0.065)	(0.058)
Initial Value	-0.935***	-0.935***	-0.921***	-0.305	-0.444	-0.572	-0.322
	(0.268)	(0.268)	(0.267)	(0.719)	(0.657)	(0.672)	(0.551)
Listed		1.211**	0.025		0.030	0.044	0.035
		(0.604)	(1.326)		(0.052)	(0.046)	(0.038)
Num Wage Earners			0.522			-0.019	-0.017
			(0.490)			(0.027)	(0.026)
Hours of Operation							-0.105
							(0.083)
Establishment FE?	Yes	Yes	Yes	No	No	No	No
Year×Industry FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year×Fed District FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	249	249	248	62	62	62	62
R-Squared	0.961	0.961	0.961	0.590	0.593	0.602	0.620

Note: ESOP data collected from National Industrial Conference Board, Inc. (1928) and various sources collected by the author. Establishment-level data from Census of Manufactures digitized by Vickers and Ziebarth (2018). Due to data availability, data on wage earners is the average of monthly data on wage earners. Only establishments which are branches of a larger company ever having an ESOP are included in the sample. U.S. Steel establishments are excluded for robustness. Columns (4) through (7) only present a cross-section due to missing data and do not include establishment or year fixed effects. Robust standard errors are given in parentheses. *, ** and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table 1.19: Effects of Intensity of ESOP Activity on Establishment Output Growth, Excluding Largest Firm

Dependent Variable: $g_{\text{RealValueProduct}_{i,t}}$			
	(1)	(2)	(3)
$\text{Inactive}_{1929-1931}$	1.440	-0.246	0.402
	(1.136)	(1.039)	(1.006)
$\text{Inactive}_{1931-1933}$	0.400	-1.286	-1.166
	(0.589)	(0.960)	(0.779)
$\text{Inactive}_{1933-1935}$	-1.188**	-2.874**	-2.496**
	(0.581)	(1.174)	(1.000)
Active 1935	-0.384	-2.069*	-2.991**
	(0.422)	(1.174)	(1.256)
Initial Value	-0.924***	-0.924***	-0.914***
	(0.257)	(0.257)	(0.258)
Listed		-1.685	-2.588*
		(1.272)	(1.357)
Total Wage Earners			0.569
			(0.474)
Establishment FE?	Yes	Yes	Yes
Year×Industry FE?	Yes	Yes	Yes
Year×Fed District FE?	Yes	Yes	Yes
R-Squared	0.961	0.961	0.961
Obs.	249	249	248

Note: ESOP data collected from National Industrial Conference Board, Inc. (1928) and various sources collected by author. Firm-level data aggregated up from establishment-level CoM data digitized by Vickers and Ziebarth (2018). Due to data availability, data on wage earners is the average of monthly data on wage earners. All firms ever having ESOPs in CoM data are included. U.S. Steel establishments are excluded for robustness. The omitted category is establishments which had an inactive ESOP in 1929. Robust standard errors are given in parentheses. *, ** and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table 1.20: Effect of ESOPs on Real Wage Growth, Excluding Largest Firm

Dependent Variable: $g_{\text{RealWagesPerWorker}_{i,t}}$				
	(1)	(2)	(3)	(4)
Active	0.305	0.305		
	(0.200)	(0.200)		
$\text{Inactive}_{1929-1931}$			0.656**	-0.023
			(0.263)	(0.160)
$\text{Inactive}_{1931-1933}$			-0.025	-0.703**
			(0.121)	(0.285)
$\text{Inactive}_{1933-1935}$			-0.695***	-1.373***
			(0.057)	(0.332)
Active 1935			0.116***	-0.563**
			(0.022)	(0.303)
Initial Wages	-1.393***	-1.393***	-1.401***	-1.401***
(Per Worker)	(0.217)	(0.217)	(0.215)	(0.215)
Listed		0.692***		-0.679**
		(0.058)		(0.303)
Establishment FE?	Yes	Yes	Yes	Yes
Year×Industry FE?	Yes	Yes	Yes	Yes
Year×Fed District FE?	Yes	Yes	Yes	Yes
Observations	267	267	267	267
R-Squared	0.910	0.910	0.907	0.907

Note: ESOP data collected from National Industrial Conference Board, Inc. (1928). Establishment-level data from Census of Manufactures digitized by Vickers and Ziebarth (2018). Due to data availability, data on wage earners is the average of monthly data on wage earners. Only establishments which are branches of a larger company ever having an ESOP are included in the sample. U.S. Steel establishments are excluded for robustness. Robust standard errors clustered a the firm level are given in parentheses. *, ** and *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

Table 1.21: Effects of ESOP Activity on Establishment Productivity, Excluding 1935

Dependent Variable:	$g_{\text{RealValueProduct}_{i,t}}$			$g_{\text{AveDaysWorkedPerWorker}_{i,t}}$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Active	-1.420***	-1.420***	-1.007	-0.228***	-0.238***	-0.233***	-0.229***
	(0.328)	(0.328)	(0.647)	(0.047)	(0.062)	(0.061)	(0.058)
Initial Value	-0.784***	-0.784***	-0.807***	-0.263	-0.231	-0.223	-0.166
	(0.234)	(0.234)	(0.263)	(0.248)	(0.194)	(0.196)	(0.193)
Listed		-0.828	-2.299*		-0.014	-0.010	-0.004
		(0.711)	(1.260)		(0.046)	(0.044)	(0.040)
Num Wage Earners			0.865			-0.006	-0.005
			(0.586)			(0.011)	(0.010)
Hours of Operation							-0.045
							(0.038)
Establishment FE?	Yes	Yes	Yes	No	No	No	No
Year×Industry FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year×Fed District FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	222	222	222	102	102	102	102
R-Squared	0.943	0.943	0.949	0.605	0.606	0.608	0.621

Note: ESOP data collected from National Industrial Conference Board, Inc. (1928) and various sources collected by the author. Establishment-level data from Census of Manufactures digitized by Vickers and Ziebarth (2018). Due to data availability, data on wage earners is the average of monthly data on wage earners. Only establishments which are branches of a larger company ever having an ESOP are included in the sample. The 1935 panel is excluded for robustness. Columns (4) through (7) only present a cross-section due to missing data and do not include establishment or year fixed effects. Robust standard errors are given in parentheses. *, ** and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table 1.22: Effects of Intensity of ESOP Activity on Establishment Output Growth, Excluding 1935

Dependent Variable: $g_{\text{RealValueProduct}_{i,t}}$			
	(1)	(2)	(3)
$\text{Inactive}_{1929-1931}$	0.796	0.055	1.450
	(0.558)	(0.241)	(0.891)
$\text{Inactive}_{1931-1933}$	0.032	-0.709	-0.242
	(0.488)	(0.547)	(0.637)
$\text{Inactive}_{1933-1935}$	-0.613	-1.355***	-0.527
	(0.583)	(0.457)	(0.554)
Active 1935	-0.167	-0.909**	-2.298***
	(0.395)	(0.380)	(0.824)
Initial Value	-0.792***	-0.792***	-0.816***
	(0.241)	(0.241)	(0.271)
Listed		-0.741	-2.032**
		(0.485)	(0.795)
Total Wage Earners			0.970*
			(0.536)
Establishment FE?	Yes	Yes	Yes
Year×Industry FE?	Yes	Yes	Yes
Year×Fed District FE?	Yes	Yes	Yes
R-Squared	0.939	0.939	0.947
Obs.	222	222	222

Note: ESOP data collected from National Industrial Conference Board, Inc. (1928) and various sources collected by author. Firm-level data aggregated up from establishment-level CoM data digitized by Vickers and Ziebarth (2018). Due to data availability, data on wage earners is the average of monthly data on wage earners. All firms ever having ESOPs in CoM data are included. 1935 panel is excluded for robustness. The omitted category is establishments which had an inactive ESOP in 1929. Robust standard errors are given in parentheses. *, ** and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table 1.23: Effect of ESOPs on Real Wage Growth, Excluding 1935

Dependent Variable: $g_{\text{RealWagesPerWorker}_{i,t}}$				
	(1)	(2)	(3)	(4)
Active	0.041	0.041		
	(0.157)	(0.157)		
$\text{Inactive}_{1929-1931}$			0.883**	0.041
			(0.375)	(0.126)
$\text{Inactive}_{1931-1933}$			0.452***	-0.390
			(0.165)	(0.401)
$\text{Inactive}_{1933-1935}$			-0.424***	-1.266***
			(0.052)	(0.372)
Active 1935			0.130***	-0.712**
			(0.023)	(0.362)
Initial Wages	-1.538***	-1.538***	-1.544***	-1.544***
(Per Worker)	(0.228)	(0.228)	(0.222)	(0.222)
Listed		0.464***		-0.842**
		(0.149)		(0.364)
Establishment FE?	Yes	Yes	Yes	Yes
Year×Industry FE?	Yes	Yes	Yes	Yes
Year×Fed District FE?	Yes	Yes	Yes	Yes
Observations	258	258	258	258
R-Squared	0.911	0.911	0.911	0.911

Note: ESOP data collected from National Industrial Conference Board, Inc. (1928). Establishment-level data from Census of Manufactures digitized by Vickers and Ziebarth (2018). Due to data availability, data on wage earners is the average of monthly data on wage earners. Only establishments which are branches of a larger company ever having an ESOP are included in the sample. 1935 panel is excluded for robustness. Robust standard errors clustered a the firm level are given in parentheses. *, ** and *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

Table 1.24: Effects ESOP Activity on Establishment Output Growth, Robust

Dependent Variable: $g_{\text{RealValueProduct}_{i,t}}$			
	(1)	(2)	(3)
Active	-1.026*	-1.026*	-1.167**
	(0.556)	(0.556)	(0.522)
Initial Value	-1.018***	-1.018***	-0.998***
	(0.156)	(0.156)	(0.155)
Small Firm		2.629***	
		(0.652)	
Small Estab.			-1.020
			(0.815)
(Total Wage Earners) × 1931	0.697**	0.697**	0.566
	(0.344)	(0.346)	(0.398)
(Total Wage Earners) × 1933	0.665*	0.665*	0.499
	(0.374)	(0.374)	(0.448)
(Total Wage Earners) × 1935	0.799**	0.799**	0.698
	(0.387)	(0.387)	(0.428)
Listed	0.543	0.543	0.150
	(1.664)	(1.664)	(1.538)
Establishment FE?	Yes	No	No
Year×Industry FE?	Yes	Yes	Yes
Year×Fed District FE?	Yes	Yes	Yes
R-Squared	0.927	0.927	0.929
Obs.	335	335	335

Note: ESOP data collected from National Industrial Conference Board, Inc. (1928) and various sources collected by author. Firm-level data aggregated up from establishment-level CoM data digitized by Vickers and Ziebarth (2018). Due to data availability, data on wage earners is the average of monthly data on wage earners. All firms ever having ESOPs in CoM data are included. The omitted category is establishments which had an inactive ESOP in 1929. Robust standard errors are given in parentheses. *, ** and *** denote significance at the 10, 5, and 1 percent levels, respectively.

1.6.5 Data Collection Process

The two main datasets which form the foundation for this empirical study are the "United States Census of Manufactures, 1929-1935" and "Employee Stock Purchase Plans in the United States" (Vickers and Ziebarth, 2018; National Industrial Conference Board, Inc., 1928). For this paper, it was important to correctly group establishments to other establishments in the same company to correctly assign the treatment of ESOP to the whole company, where appropriate. Due to the hand-written nature of the original survey data, typos and inconsistencies were present throughout the original digitized dataset.

First, only establishments labeled as branches/subsidiaries of a larger company were kept in the main sample. Any establishment not identified as a subsidiary of another company was dropped. Then, I used basic cleaning codes in Stata to fix the straight-forward issues in the dataset, such as varying capitalization, extra spacing, and punctuation in the owner name. Then using the plant name, owner name, and parent firm information, I was able to go through the branched sample by hand and correct any typos, which were most commonly misspellings.

An additional issue to deal with is the large number of mergers and acquisitions throughout the 1920s. Therefore, I had to determine which subsidiaries were owned by parent firms in the dataset, which was not always obvious. It was also necessary to understand whether these subsidiaries had access to the ESOP at the time of acquisition. Historical annual company reports which I accessed through ProQuest Historical Annual Reports and various additions of Moody's Investment Manual provided information on subsidiaries and often had a line item which allowed me to determine if employees of subsidiaries had the same access to the employee stock ownership program.

The National Industrial Conference Board, Inc. (1928) data provides extensive information on who had active or inactive ESOPs in 1928, when it was published. However, I had to extend these data to match the period covered by the Census of Manufactures. An additional study by the National Industrial Conference Board as well as other contemporary sources on ESOPs were used to fill in these data as well as understand when exactly these programs were active. Historical newspaper articles also were utilized as companies often advertised new stock offerings for their employees in the local publications. The published studies and books were the first stage of looking to fill in these data on ESOPs in specific companies. The next stage was reading through historical annual company reports and letters to stockholders. Finally, newspapers were the final stage to understand the length and the terms of the various ESOPs.

Once each separate dataset was cleaned and expanded to address the main research question about firm productivity, the final major step was to merge the two data sources together. This was done by hand by searching through the Census of Manufactures data set for specific company names. To confirm the ESOP company was the same firm present in Census of Manufactures data, I made sure the company's main industry and the headquarter location match across both datasets. Similarly, I matched the data on which firms were listed on a major stock exchange from Rousseau and

Watchel (1998) manually by searching the company names in the cleaned Census of Manufactures data.

CHAPTER 2

The Real Effects of the Liberty Loan Program and Financial Intermediation During World War I

2.1 Introduction

The World War I period offers an interesting natural experiment for studying government finance, especially through the lens of financial intermediation. Out of a dire need for government funds and to divert U.S. production to war industries, the U.S. Treasury and newly formed Federal Reserve launched an unprecedented saving initiative through the sale of "liberty bonds" in 1917. This was not the first time war bonds had been issued to the public,[1] but it was by far the most successful in terms of subscriptions and overall participation rates (Kang and Rockoff, 2015).

This paper shows that given the special circumstances surrounding the U.S. economy in the early twentieth century, namely a novel central bank working in coordination with the government, an economy producing at capacity, and a large segment of the population not participating in financial markets, it was essential to finance government spending primarily through war bonds instead of relying fully on tax revenue. This idea is motivated by a prominent model of financial intermediation that reflects the role of bonds in this historical period. Using the exogenous shock of the U.S. joining World War I and the extraordinary government spending that ensued, a vector autoregressive (VAR) model is employed to test empirically how production responds to both tax and debt shocks. In this analysis, liberty bonds are characterized both as a major component of government debt and as a type of intermediated asset to show that these bonds had a strong positive effect on production, especially in the war industries.

Before the Liberty Loan Program, which consisted of multiple campaigns to sell government debt to ordinary citizens in the form of fixed-denomination bonds, most American households had no experience with purchasing stocks or bonds and were skeptical of these types of intangible assets.[2] It is estimated that on the eve of the first Liberty Loan Campaign, only about 0.5% of the U.S.

[1]Union bonds were issued during the Civil War, but these assets were viewed suspiciously by many and failed to permanently expand the number of American investors (Ott, 2011)

[2]Throughout the paper, the terms "liberty loan" and "liberty bonds" are used synonymously as both refer to this government bond program.

population or approximately 2.5% of households owned either stocks or bonds (Ott, 2011). When compared to a survey of urban, working-class households conducted near the end of World War I by the Bureau of Labor Statistics, which suggests that around 67% of this population purchased liberty bonds in some form, the impact of the program is striking (Olney, 2006).

The theoretical motivation presented suggests that increased productivity from bond issuance comes from foregoing spending on consumption goods and reallocating savings from cash and deposits to investments in the productive, war sectors. During World War I, the Liberty Loan Program effectively increased the U.S. savings rate and diverted capital from the less useful consumer goods sector to war production. Unlike the powers granted by today's Defense Production Act, in this period the government had little authority over private production, even during wartime (Sutch, 2015). Thus, purchasing bonds out of a patriotic duty to finance the war was essential to driving this shift in production.

While war bonds were marketed to all citizens, including children, the government sought to extract funds from primarily wealthy individuals through taxation. This period of financial diversification for average Americans also coincides with the beginning of the modern tax system. The income tax that exists today originated from the passing of the 16th Amendment in 1913, and the War Revenue Act of 1917 increased these progressive initial tax rates to unprecedented levels (Terrell, 2018; Blakely, 1917). Throughout this paper, it is argued that extracting taxes from the public would not have sufficiently financed government debt or shifted production to the necessary war industries, emphasizing the essential role of liberty bonds.

2.1.1 Related Literature

Recent studies of World War I and the Liberty Loan Program can be grouped into two broad categories: analysis of participation incentives and local economic effects.

Kang and Rockoff (2015) study the patriotic motive and financial incentives for holding bonds by analyzing the interest rates for the Fourth Liberty Loan and the Victory Loan. Using a cost-benefit analysis approach, they argue that patriotism only had a small effect on the motive for buying these bonds. They attribute their success mostly to wealthy individuals purchasing bonds for tax incentives.

To analyze factors related to the success of the liberty loans at the county level, Hilt and Rahn

(2016) use a novel dataset which contains county-level data on liberty loan subscriptions. Their results emphasize the role county civil and social organizations played in achieving significantly higher liberty bond subscriptions even after controlling for financial factors. Comparing the rates of bond ownership during World War I to modern rates of stock ownership by income level, they also show that modern households hold stock at much lower rates, reflecting the relative success of the earlier bond drives.

Focusing on the effect of the bond program and financial literacy, Hilt et al. (2020), find that counties with high liberty bond subscription rates had more competition between financial institutions in the 1920s. Using panel data on U.S. counties, they conclude that participation in the Liberty Loan Program led to an increase in the number of investment banks and more diversity in the asset-holdings of American households.

While the Liberty Loan Program has been studied in a variety of contexts, to my knowledge, this paper is the first to compare the effects of the bond program and taxes in terms of production during the war. Regarding war finance more generally, Ohanian (1997) has studied the debt versus taxation question by comparing the different fiscal policies used during World War II and the Korean War. He constructs a general equilibrium model and simulates the U.S. economy under both increased borrowing and balanced-budget-type policies. His results emphasize the positive welfare effects of a tax-smoothing policy, which consists primarily of financing war spending through debt, over a balanced budget achieved through increased taxation.

I address the same overarching war finance question in this paper by going back to World War I, when both fiscal policies were being thoughtfully considered. Instead of emphasizing the tax-smoothing role of debt, this paper primarily focuses on war bonds as a novel asset that reached many households not previously involved in financial markets (Kang and Rockoff, 2015). In the next section, I summarize the specifics of the fiscal policy debates during World War I and the history of the Liberty Loan Program.

2.2 World War I and the Liberty Loan Program

When World War I officially began in Europe in 1914, the majority of Americans opposed the United States getting involved. President Woodrow Wilson even centered his 1916 Presidential re-election campaign on a pro-neutrality stance. As President, he was determined to keep the United

States out of the war and made few preparations in the event that the nation would join the conflict. After several attacks on American ships by German submarines, however, Wilson gave in to the shift in U.S. sentiment toward war. The United States officially entered World War I on April 2, 1917 (Gay and Gay, 1995).

Due to Wilson's anti-war posture, the United States was grossly unprepared to fight in the Great War, especially in terms of economic capacity. Furthermore, before joining the war effort, the American economy was already producing at full capacity with no underutilized resources. Thus, there needed to be a clear and direct shift in U.S. production to manufacture needed wartime materials (Sutch, 2015).

As one of the first modern wars, industrialism and mass production on the home front played a central role in preparing for battle. Specifically, the steel and iron industries, which helped produce guns, ships, helmets, and even shoes, were essential to the war effort (The National Iron & Steel Heritage Museum, 2012). Two agencies were created under President Wilson to assist in preparing the U.S. economy for wartime. The War Industries Board was created to coordinate procurement of materials between the Army and the Navy, and the Council of National Defense was established to inventory the nation's resources (Bradsher, 2017; Baruch, 1921). There was no WWI equivalent, however, to the War Powers Act, which would come during World War II and gave the President substantial powers to organize production (77th Congress, First Session, 1941). Thus, achieving a shift in private production had to be a coordinated public effort.

Early on, it became clear that the costs associated with the Great War were going to be unprecedented, and the government would require a well-grounded plan to finance the inevitable debt. In the end, government spending in this period is estimated to have increased to twenty-five times its pre-war level (Hilt and Rahn, 2016). Richard Sutch explains that the government had several tactics to choose from for covering its costs. The Treasury, led by Secretary William McAdoo, could print more money, which would subsequently lead to higher inflation, raise taxes, or borrow from the public (Sutch, 2015).

Learning from the U.S. experience with greenbacks during the Civil War,[3] Secretary McAdoo knew that printing money to finance government spending would not be advantageous for the U.S. economy in the long-run (Kang and Rockoff, 2015). There were several arguments in favor of war

[3]For a detailed history of paper currency during the Civil War, see Mitchell (1903).

bonds over higher taxes circulating in political and economic spheres at this time. First, at this point, the income tax was primarily levied on the rich (Tax Foundation, 2013).[4] Government officials feared wealthy citizens would be frustrated by multiple, substantial tax increases and in turn they would lose support for the war (Kang and Rockoff, 2015). Also, the cost of the war was far from certain at the outset, and increasing tax rates required drawn-out political debates and Congressional approval (Sutch, 2015). Additionally, there was the argument that war bonds allowed citizens to show their support for the war and a patriotic sacrifice for their country (Ott, 2011). Political economist and wartime propaganda writer, Thomas Nixon Carver, made another fundamental point that buying a bond directly allocated production away from consumption goods and toward war industries (Carver, 1919). He writes,

> . . . Every time any one spends a dollar for an unnecessary thing he is furnishing an inducement to capital and labor to continue in the production of unnecessary things and to stay out of the war industries. He should turn that dollar over to the government to be spent for the necessary things. Then the government can use it to induce capital and labor to go into the necessary industries. That is very much better in every way than conscription.[5]

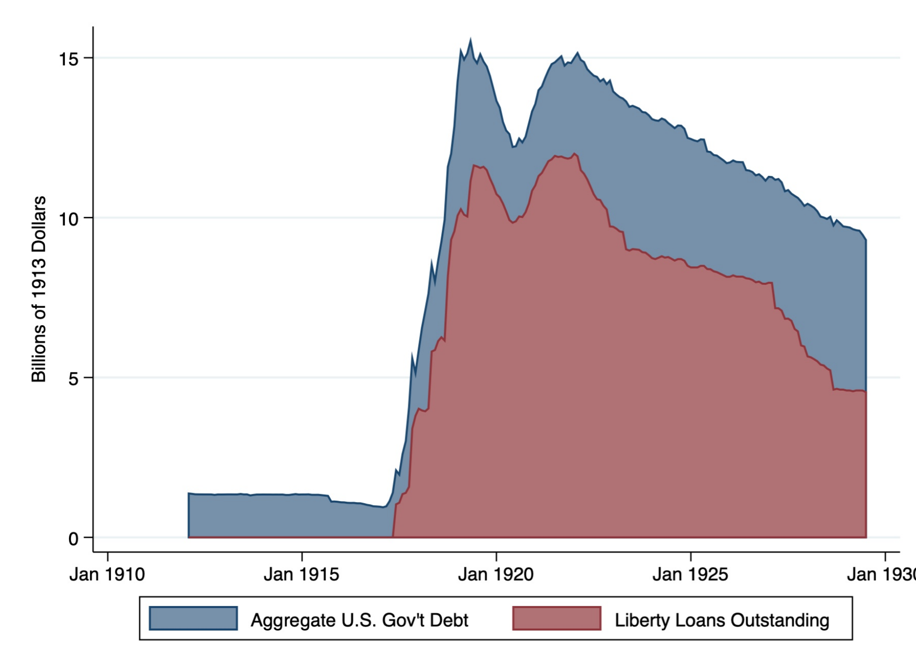
Ultimately, based on these arguments, Secretary McAdoo believed that government bonds should play a significant role in financing the war, and he developed a plan which included both increased taxation and borrowing from the public (Sutch, 2015).

The name of this unprecedented borrowing campaign that introduced war bonds into the economy was the Liberty Loan. Secretary McAdoo set the interest rate on these bonds to be comparable to bank savings accounts so that financially sophisticated Americans would not simply divert their savings from banks to bonds. Secretary McAdoo knew that merely substituting funds from a bank would not increase the private savings rate or decrease consumption of unnecessary goods (Sutch, 2015). Interestingly, the bonds from the First Liberty Loan were tax exempt. However, there was some opposition to this policy as many felt that it simply created a way for the rich to avoid paying taxes. In the following bond drives, only the interest on the first $30,000 worth of bonds was ex-

[4]Households making less than $20,000 (approximately $420,000 today) only faced a 2% marginal income tax rate. Rates increased consistently from there with the highest tax bracket paying a 15% rate.

[5]Carver (1919), pg. 37

Figure 2.1: Liberty Loans as a Component of Government Debt



Source: U.S. Treasury Department, Monthly Statement of Public Debt

empt from taxation (Kang and Rockoff, 2015). Still, for the wealthiest Americans, it was possible to some degree to substitute between the two types of war finance.

In addition to raising funds for the government and increasing savings, the Liberty Loan Program sought to educate the public about the goals of the war and about savings bonds in general. The smallest denomination of liberty bond was $50 or about two weeks of wages for the average factory worker (Sutch, 2015). The government also marketed 25 cent "War Thrift Stamps," which could be collected over time and exchanged for a $5 stamp and eventually a $50 liberty bond.

Figure 2.1 shows an area graph of the aggregate government debt as well as the portion of the debt coming from outstanding liberty loans in billions of constant 1913 dollars. The dip in the series in the early 1920s reflects a period of post-war deflation from June 1920 to March 1922 before the price level picked back up. The nominal series shows a smooth curve with the same extraordinary growth of debt from 1917 to 1919.

In total, there were four separate liberty loan drives with precise opening and closing dates during the war and one additional drive after the war concluded. Table 2.1 below outlines the dates, quantities of subscriptions, and number of subscribers for each loan. The first campaign began in the

Spring of 1917, just twenty-two days after the U.S. declared war on Germany, and the final "Victory Liberty Loan" occurred after the conclusion of the war in the Spring of 1919 (Sutch, 2015).

The newly formed Federal Reserve System was instrumental to the success of the Liberty Loan

Table 2.1: Liberty Loan Subscriptions for Each Drive

	First May-June 1917	**Second** Oct. 1917	**Third** April 1918	**Fourth** Sept.-Oct. 1918	**Victory** April-May 1919
Subscriptions ($ billions)	3.04	4.62	4.18	6.96	5.25
Number of subscribers (millions)	4.0	9.4	18.4	22.8	11.8

Source: Kang and Rockoff (2015)

Program.[6] Reserve member banks, which consisted primarily of converted national banks from around the country, sold liberty bonds directly to individuals. Each Federal Reserve district also had a specific quota of subscriptions it was required to meet, which fostered fierce, regional competition (Kang and Rockoff, 2015).

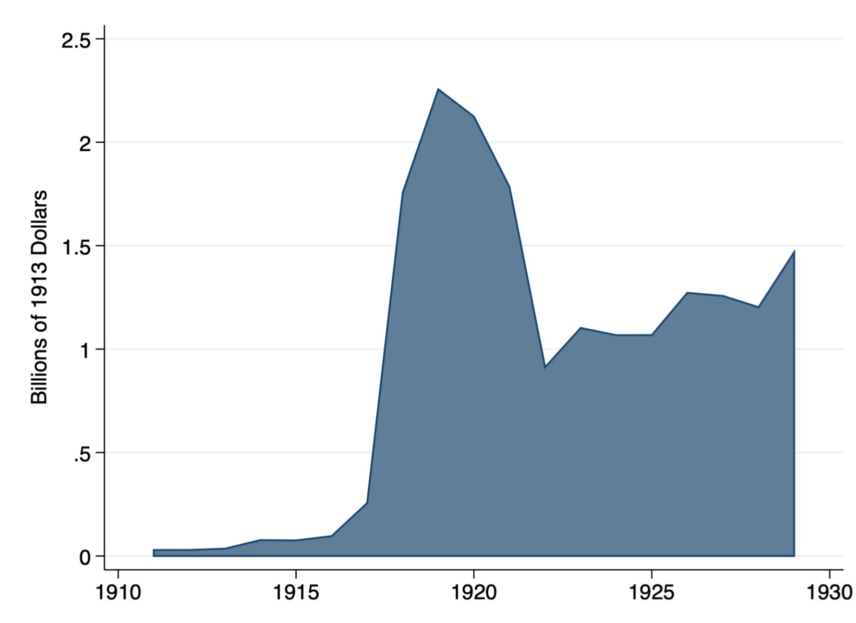
Largely deemed a success, the liberty loan campaigns surpassed their quotas. While Secretary McAdoo had planned for bonds and taxes to split the burden of financing the war evenly, in the end, approximately two-thirds of the war effort was financed by these bonds. Moreover, government surveys suggest that the majority of the bonds sold were bought by the American public, especially households with modest incomes, rather than banks and other financial institutions (Olney, 2006; Kang and Rockoff, 2015).

Tax increases financed roughly the remaining one-third of government spending (Sutch, 2015). The War Revenue Act of 1917 was signed into law in October and effectively increased income taxes to unprecedented rates. This expanded income tax revenue to about five times its prewar level. As before, these increases were levied primarily on the richest in the population, resulting in a powerfully progressive tax structure (Blakely, 1917).[7] Figure 2.2 shows the government's annual income tax revenue during the war and into the 1920s. The large spike in tax receipts following the 1917 legislation is striking. Yet, simply comparing the magnitudes of the government debt and cumulative tax revenue in Figure 2.3 shows that taxes alone could not offset government spending

[6]The Federal Reserve Act was signed into law by President Wilson at the end of 1913, and the central bank officially began operations in 1914. Until 1936, the Secretary of the Treasury was also the Chairman of the Federal Reserve board, which highlights the government's strong influence on the reserve banks in this period (Sutch, 2015).

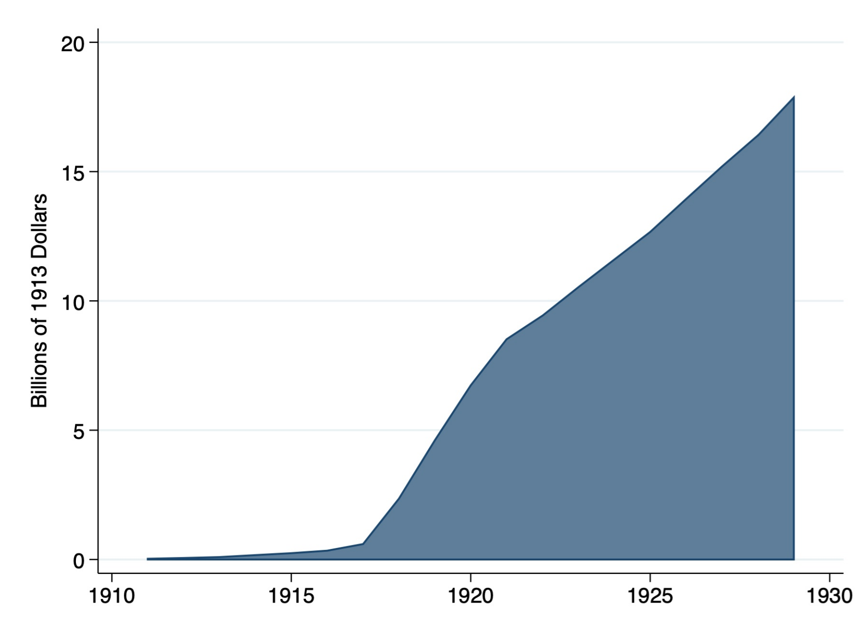
[7]For the highest income tax bracket, those households making over $2 million, their tax rates increased from 15% in 1916 to 67% after the War Revenue Act of 1917 went into effect (Tax Foundation, 2013).

Figure 2.2: Annual Federal Income Tax Receipts



Source: NBER Macrohistory Database

Figure 2.3: Cumulative Federal Income Tax Receipts



Source: NBER Macrohistory Database

during the war. While the tax base did grow, on a cumulative basis, tax receipts did not surpass the government's peak war expenditure until the late 1920s. Even so, this tax revenue also needed to finance normal government operations and could not solely fund the war effort.

To balance the budget, politicians would have needed to rapidly expand the tax base, which was politically difficult. Given that the personal income tax was relatively new at this point, tax revenue could not simply increase overnight to meet the government's demand. Instead, it was determined that financing a significant portion of government spending through the voluntary purchase of war bonds, under the aegis of patriotism, was a better method.

2.3 Motivation and Related Theory

As discussed in Section 2.1, the Liberty Loan Program has been linked to the expansion of the investor class and the diversification of asset portfolios (Hilt et al., 2020; Ott, 2011). Some have even ventured to characterize this period as the "birth of the modern Treasury market."[8] Therefore, it seems instructive to ground the impact of these government assets in the theory on financial intermediation before turning to the empirical analysis.

A large theoretical and empirical literature exists on the role that financial intermediation plays in economic growth and industrialization (McKinnon, 1973; Greenwood and Jovanovic, 1990; Bencivenga and Smith, 1991; Rousseau and Watchel, 1998). This research tends to focus on banks as the primary financial intermediary in an economy and generally shows that their establishment can lead to growth. The role of allocating resources, however, could be extended to the government during extraordinary times. While legislation has evolved since World War I which allows the government to coordinate production pursuant to national defense directly, this power was unavailable to the executive branch in 1917 (Congressional Research Service, 2020). The Liberty Loan Program, subsequently, provided the government a mechanism to allocate the factors of production. Given the mass scale of production required during World War I and the small initial class of investors, the existing intermediating sector was likely less productive than the government sector in overcoming large indivisibilities in investment, providing a clear justification for government intervention.

The two-sector, two-asset model developed by Bencivenga and Smith (1991) which includes a simple financial intermediary allocating resources closely relates to the economic environment during World War I. Their single intermediary, which represents the Federal Reserve System working with the Treasury Department, allocates resources by accepting funds from savers and lending them to entrepreneurs who invest in productive capital. The two alternative assets, which are central to

[8]The Joe I. Herbstman Memorial Collection of American Finance (2021)

their model structure, are a liquid, unproductive asset and an illiquid, productive asset.

During the war, the unproductive asset could be represented by inventories of nonessential consumption goods or simply money held as cash or in checking accounts. Naturally, liberty bonds constitute the illiquid investment in the productive sector: the war industries. Bonds are inherently more illiquid than cash or bank deposits. Additionally, during the war, it was seen as unpatriotic to sell a bond purchased for the war effort. Secretary McAdoo even went as far as calling people who resell liberty bonds "quitters and unpatriotic citizens," making these assets highly illiquid in the short term.[9]

While it is unclear exactly how much of this debt went into capital production (as opposed to food or wages), these bonds were also marketed as productive assets. Much of the rhetoric of the Treasury Department's War Loan Organization suggested the purchase of liberty bonds was essentially a direct investment in capital goods for the war effort (Ott, 2011).[10]

Beyond simply financing the government's debt, a fundamental purpose of these bonds was to allocate resources away from consumption. Secretary McAdoo believed government borrowing need not be inflationary if private saving also increased (Sutch, 2015). This reallocation channel is unique to bond finance and is a crucial differentiator between borrowing and taxation.[11] When a household pays taxes, it channels funds to the government, but it does not necessarily increase a household's savings rate or directly force them to substitute between saving and the consumption of "unnecessary goods" like investing in war bonds does (Carver, 1919). For example, the War Loan Organization urged households to invest in liberty bonds to the extent possible and likened thriftiness to having a high moral character (Ott, 2011). Whereas paying income taxes was done out of necessity, foregoing certain luxuries to buy bonds was a way to show one's virtuous nature.

In the end, Bencivenga and Smith (1991) show that the establishment of financial intermediaries can increase an economy's growth rate by channeling savings into the productive sector, which can be used to produce capital goods by entrepreneurs and away from the unproductive sector. Similarly, the newly formed Federal Reserve System and the Department of the Treasury acted as a type of

[9]"McAdoo Classes All Who Resell Liberty Bonds as Quitters", *New York Tribune*, August 23, 1918

[10]For example, a popular poster advertising the Second Liberty Loan read "Every Liberty Bond is a shot at a U Boat. Fire your shot today. Buy a Liberty Bond." An image of the poster is shown as Figure 2.13 in the Appendix (The Library of Congress, 1917).

[11]Barro (1979) shows that governments vary their debt in order to maintain a relatively stable tax rate over time. While this is clearly a key distinction between taxation and borrowing, it is a long run phenomenon that is beyond the scope of this paper and does not explain how taxation and debt could be used to allocate capital during wartime.

government-run financial intermediary and worked together to ensure funds from citizen-investors were allocated to the vital war industries (Ott, 2011).[12] Industrial companies could then effectively accumulate the factors of production needed to produce war materials at the required scale.

One might argue that from the banks' perspective, selling bonds over marketing deposit accounts to customers was not in their profit-maximizing interest. The Liberty Loan quota system, however, at least temporarily incentivized banks to encourage the purchase of bonds and aligned their interests with the government's wartime production goals.

Based on the theoretical arguments and the asset properties of liberty bonds emphasized above, this public debt program should lead to a strong increase in production during wartime. Because it allowed the government to direct discretionary funds into the productive sector, the sale of liberty bonds should have led to a greater increase in war production than a tax hike. The fact that the U.S. went into the war with its economy already producing at capacity and had to reallocate production through market forces is key to this interpretation. In the next sections, I test these general theoretical conclusions empirically.

2.4 Data and Empirical Strategy

In this section, I summarize the various data series used in the analysis, investigate their time series properties, and describe the empirical approach.

2.4.1 Data

The data series consist of monthly measures of production, the money stock, government tax revenue, and a broad measure of the size of the Liberty Loan Program. Table 2.8 in the Appendix summarizes the data and sources. Where necessary, nominal data are converted into real terms using the U.S. Index of the General Price Level from the NBER Macrohistory database. All variables are seasonally adjusted. The variables are also transformed by dividing by the population and then taking the natural logarithm. While the production, money stock, and tax data are relatively straightforward, the government debt and liberty bonds data require further discussion.

To estimate the effects of taxation and debt financing, it was important to create a dataset that allows for the study of a long sample period. Because liberty bonds were first introduced in May

[12] In the context of Bencivenga and Smith (1991), these war industries were run by qualified entrepreneurs.

1917 after the U.S. had declared war, alternative, broader measures are considered that include these war bonds but also permit study of a longer time frame.

As shown in Figure 2.1, outstanding liberty bonds were a major component of aggregate government debt. Both series were collected from the U.S. Treasury Department's Monthly Statement of the Public Debt. Because the bonds are such a significant driver of government debt and the data on this debt goes back several years before the war, this series is included in the analysis instead of the actual liberty bonds measure.

Additionally, to address the intermediation channel emphasized in Section 2.3 more effectively, bank data on holdings of liberty bonds were obtained from the *Annual Reports of the Comptroller of the Currency* (1910-1929) and the monthly *Federal Reserve Bulletin*. The data are aggregated from these two sources to estimate the share of bonds held by banks in the Federal Reserve System, including the Federal Reserve district banks themselves. It should be emphasized that this measure is only an estimate because reporting of bond holdings by national banks was less than ideal after the war.

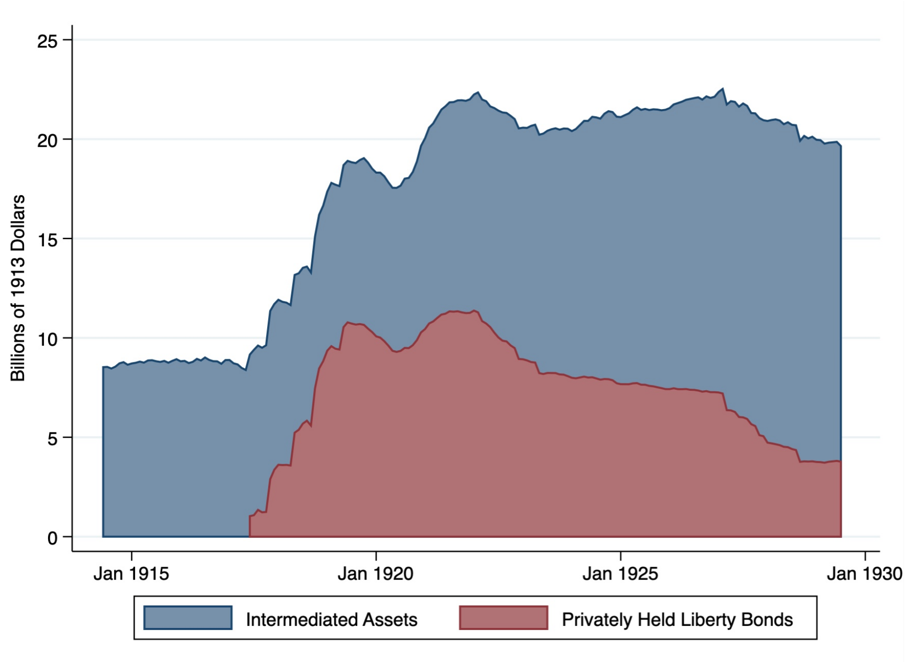
Starting in September 1919, liberty bonds were no longer reported as a separate line item on banks' balance sheets. Rather, liberty bonds were combined into a total U.S. government securities statistic. To continue the liberty bond series for the remainder of the sample, the average ratio of liberty bond holdings to total U.S. government securities is calculated throughout the war period and then used to estimate the series forward. Because there is precise monthly data during each of the liberty bond campaigns and there were no new issues of war bonds after September 1919, this estimate is a reasonable proxy for the amount of bonds held by banks.

Finally, with this newly constructed data series, which is defined as "bank holdings," the face value of privately held liberty bonds can be estimated. This is simply the difference between liberty bonds outstanding and bank holdings. While participation in these bond drives has been the subject of recent research, I know of no earlier attempt to measure the private holdings of liberty bonds. To make these data more comparable to the debt measure, a broad measure of intermediated assets is defined in equation 2.1 below.

$$\text{Intermediated Assets}_t \equiv \text{M2}_t - \text{M1}_t + \underbrace{\text{Liberty Bonds Outstanding}_t - \text{Bank holdings}_t}_{\text{Privately held Liberty Bonds}_t} \qquad (2.1)$$

This series again allows for extending the sample backward to before the war when the last two terms of equation 2.1 are simply zero. Figure 2.4 is similar in nature to Figure 2.1 in that it shows the component of the intermediated assets measure that comes from liberty bonds. Similarly, liberty bonds indeed appear to represent a shock to this series in May 1917.

Figure 2.4: Liberty Bonds as a Component of Intermediated Assets



Source: Author's construction based on M2 and M1 from Friedman and Schwartz (1963), *Annual Reports of Comptroller of the Currency* (1910-1929), and the *Federal Reserve Bulletin*.

2.4.2 Vector Autoregression

To study the effects of these two types of government finance schemes, taxation and borrowing from the public, the vector autoregressive (VAR) model is employed. This is a type of multi-variate time-series analysis where each dependent variable is regressed on its own lags as well as lags of the other dependent variables in the system. Identification is achieved because the optimal lag structure reduces the endogeneity between variables and allows one to estimate relationships in the data over time (Sims, 1980). To determine this optimal lag length, a series of nested likelihood ratio tests are estimated that select a VAR with 9 lags.

Equations (2.2a) through (2.2d) below show the general form of the VAR model. The baseline four-variable model includes a measure of production as x_1, tax revenue as x_2, money stock as

x_3, and a broad measure reflecting the Liberty Loan Program as x_4. As discussed above, this last variable is either government debt or intermediated assets, both of which are driven by liberty bonds during the war. The sample period covers nearly twenty years of monthly data from May 1910 to July 1929. This time frame provides over 200 observation periods and ample degrees of freedom to run the model.

$$x_{1,t} = a_{1,0} + \sum_{i=1}^{k} a_{1,i}x_{1,t-i} + \sum_{i=1}^{k} b_{1,i}x_{2,t-i} + \sum_{i=1}^{k} c_{1,i}x_{3,t-i} + \sum_{i=1}^{k} d_{1,i}x_{4,t-i} + u_{1,t} \tag{2.2a}$$

$$x_{2,t} = a_{2,0} + \sum_{i=1}^{k} a_{2,i}x_{1,t-i} + \sum_{i=1}^{k} b_{2,i}x_{2,t-i} + \sum_{i=1}^{k} c_{2,i}x_{3,t-i} + \sum_{i=1}^{k} d_{2,i}x_{4,t-i} + u_{2,t} \tag{2.2b}$$

$$x_{3,t} = a_{3,0} + \sum_{i=1}^{k} a_{3,i}x_{1,t-i} + \sum_{i=1}^{k} b_{3,i}x_{2,t-i} + \sum_{i=1}^{k} c_{3,i}x_{3,t-i} + \sum_{i=1}^{k} d_{3,i}x_{4,t-i} + u_{3,t} \tag{2.2c}$$

$$x_{4,t} = a_{4,0} + \sum_{i=1}^{k} a_{4,i}x_{1,t-i} + \sum_{i=1}^{k} b_{4,i}x_{2,t-i} + \sum_{i=1}^{k} c_{4,i}x_{3,t-i} + \sum_{i=1}^{k} d_{4,i}x_{4,t-i} + u_{4,t} \tag{2.2d}$$

To calculate impulse response functions (IRFs) from the VARs, the Cholesky decomposition is used with the variables ordered from most to least exogenous. This allows for the graphical analysis of the separate effects of a 1% shock to debt or taxes on production. The orderings used and their justifications are provided in Section 2.5 below.

2.4.3 Properties of Data Series

Before the results from the four-variable system are estimated, the stationarity properties of each data series must be investigated to determine the appropriate model structure. Results from Augmented Dickey-Fuller (ADF) tests for each series in levels and first differences are presented in Table 2.2 below. Both a constant and a trend are included when computing these tests based on the series characteristics after taking the log transformation.

Excluding the three production variables, industrial production, iron, and steel, the levels data series fail to reject the null hypothesis of a unit root. Industrial production rejects the presence of a unit root at the 5% level but not the 1% significance level. The results suggest that iron and steel production may have unit roots. The null hypothesis is strongly rejected, however, in first-differences

Table 2.2: Augmented Dickey-Fuller (ADF) Test Statistics for Per Capita Macroeconomic and Asset Variables

	Level	1st Difference
Industrial Production (IP)	-3.668*	-7.530**
Iron	-4.400**	-6.523**
Steel	-4.142**	-6.662**
Debt	-1.998	-4.452**
Intermediated Assets	-1.132	-4.133**
M2	-2.191	-6.160**
M1	-2.029	-4.914**
Tax	-2.230	-9.018**

Note: The ADF statistics were generated by a model with constant, trend, and nine lags. * and ** denote rejection of the unit root hypothesis at the 5 percent and 1 percent levels respectively, using critical values from Fuller (1996).

for every series. While the production variables are not as definitive, the analysis proceeds by treating all variables as first-difference stationary.

Additionally, the Granger causality statistics (i.e., Wald tests) from the VAR estimation need to follow the standard distributions to determine the significance of the results. Sims et al. (1990) show that for a three-variable system with one cointegrating vector, the levels VAR specification is valid for calculating Wald statistics and testing for Granger-causality. Toda and Yamamoto (1995) go further to state that as long as the order of integration is less than the true lag length of the model, the VAR can be estimated in levels with minor alterations, and the Wald statistics will conform to the χ^2 distribution.[13]

To this end, a series of Johansen (1991) tests are computed to determine the number of cointegrating relationships in each system. Table 2.3 shows that all of the series strongly reject the null hypothesis of no cointegrating relationships, and most fail to reject the null of one or fewer cointegrating relationships. Therefore, the Toda and Yamamoto (1995) result is invoked, and the model is appropriately estimated by over-fitting the VARs with one additional lag of all endogenous variables in the system.

[13]Specifically, even though the model is estimated with $k+1$ lags, the Wald statistics are calculated based on the original k lags determined by the Likelihood ratio test.

Table 2.3: Johansen Test Statistics for Cointegration between Real Per Capita Log Levels of Production, the Monetary Base, Tax Revenue, and Measures of Liberty Loans

	Trace			Max. Eigen.			
	$r = 0$	$r \leq 1$	$r \leq 2$	$r = 0$	$r \leq 1$	$r \leq 2$	$r \leq 3$
IP, Debt, M2	59.78**	25.96	11.04	33.81**	14.92	9.45	1.60
Iron, Debt, M2	61.92**	28.98	12.11	32.94**	16.87	10.66	1.45
Steel, Debt, M2	67.59**	29.27	12.52	38.31**	16.75	11.07	1.45
IP, Intermed. Assets, M1	65.20**	32.35*	12.79	32.85**	19.55	8.07	4.72*
Iron, Intermed. Assets, M1	68.02**	36.25**	13.91	31.77**	22.33*	9.06	4.85*
Steel, Intermed. Assets, M1	76.55**	39.36**	14.36	37.19**	25.01*	9.61	4.75*
IP, M2-M1, M1	64.84**	14.42	4.77	50.42**	9.64	4.64	0.14
Iron, M2-M1, M1	64.62**	16.12	6.32	48.50**	9.81	5.76	0.55
Steel, M2-M1, M1	75.38**	16.16	6.51	59.22**	9.64	5.84	0.67

Note: Each system includes logs of either real per capita M2 or M1, tax revenue, and the production and Liberty Loan variables at the left. The lag length for each system is nine and was determined by nested likelihood ratio tests. The columns labeled $r = 0$ test a null of no cointegration, while the $r \leq 1$ ($r \leq 2$) columns test a null of at most one (two) cointegrating vectors. * and ** denote rejections of the null at the 5 percent and 1 percent levels respectively, with critical values from Osterwald-Lenum (1992).

2.5 Results

To understand the relationships between liberty bonds, taxation, and production in World War I, a VAR model is used to perform a series of Granger non-causality tests. The idea of Granger causality is often used to indicate which variables have leading roles in a system. To understand these dynamics following Rousseau (1998), the tables below report the sum of the coefficients for all k lags of the respective regression as shown in equations (2a) through (2d), with the p-value of the F-statistic for the Granger non-causality restriction in parentheses. Specifically, this restriction is defined as

$$\hat{g}_{j,i} = \hat{g}_{j,i+1} = \cdots = \hat{g}_{j,k} = 0; \quad g = a,b,c,d; \quad j = 1,2,3,4 \tag{2.3}$$

Orthogonalized impulse response functions (IRFs) are then calculated and plotted in the figures below. As mentioned above, the responses show the effects on production over time of a 1% shock to either the broad liberty bonds measure or taxation. The Choleski decomposition is ordered with the liberty bonds measure first (either debt or intermediated assets), followed by tax revenue, a measure of the money stock (either M1 or M2), and then the production measure.

This ordering is based on both historical data and economic growth theory. Given that President

Wilson did not signal his intent to go to war and Secretary McAdoo gave little prior notice of the liberty bond drives, these war bonds exemplify an exogenous shock to the economy quite early on in the war effort for the U.S. A sudden increase in tax rates came later, near the end of 1917. The money stock acts fundamentally as a control for monetary policy in this period and is believed to be less exogenous than the previous policy variables. Finally, the production measure comes last due to the well-established lag in production time, which is emphasized by Bencivenga and Smith (1991).

One may argue that both taxation and the bond program were being debated and publicized at the same time, when the U.S. declared war. And while the tax hike did not go into effect until later, the public knew these tax increases were coming. In the Appendix, this ordering is altered by placing taxes first and re-estimating two of the key IRF graphs. In Figures 2.18 and 2.19, the results are similar with this order and the main conclusions unchanged.

2.5.1 Government Debt and Production

First, the baseline VAR model is presented which includes aggregate government debt as the measure of the Liberty Loan Program and a measure of overall industrial production.

The top panel of Table 2.4 shows the results of this four variable system with a time trend as the only exogenous variable. The coefficient on debt in equation (2a) shows that debt has a positive and significant aggregate effect on production. The aggregate effect of the tax, however, is positive but insignificant. According to equation (2d) in the top panel, there is some feedback from production that negatively affects debt. While the relationships between the variables appear to have the correct signs, based on these results, it cannot be determined that debt Granger causes production.

In the bottom panel, this same model is re-estimated with an exogenous dummy variable. This *LL* dummy variable represents the periods when liberty bonds are present in the economy. Specifically, this variable is defined as

$$LL = \begin{cases} 1, & \text{if } t \geq \text{May 1917} \\ 0, & \text{otherwise} \end{cases} \tag{2.4}$$

This allows for the separation of the effect of the bonds from the overall effect of government debt. With this definition, more than simply the wartime period is being controlled for. The dummy variable also captures some of the longer run production effects of the bonds discussed in Hilt et al. (2020). In the bottom panel, this dummy variable is positive and significant. In fact, the coefficient on the dummy variable is larger than the sum of the coefficients for all the debt lags. Furthermore, once these bonds are controlled for, the results from equation (2d) show that government debt becomes more endogenous according to the p-values from the F-tests.

Comparing the effects of taxation and liberty bonds on production graphically through impulse

Table 2.4: VAR Estimates for Systems with Income Tax Receipts, the Money Stock, Debt, and Production

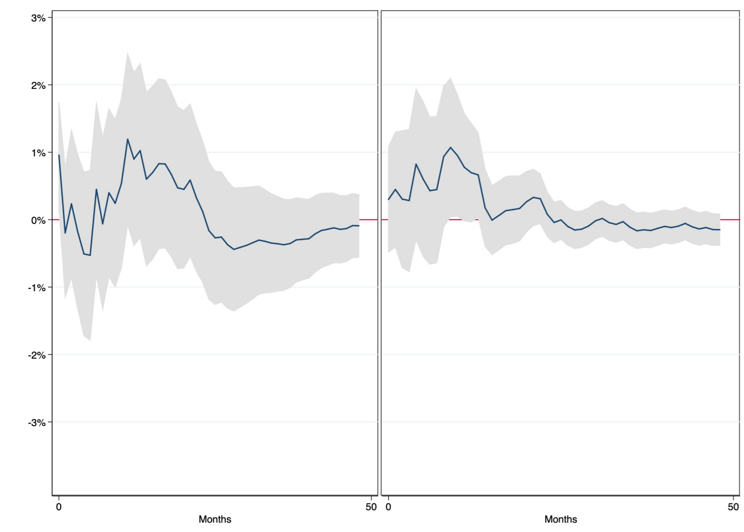
	Levels VAR Granger Tests				Exogenous	
Eq. #	IP	Tax	M2	Debt	LL	$R^2/(DW)$
(2a)	0.757	0.122	-0.040	0.157		0.879
	(0.000)	(0.548)	(0.000)	(0.001)		(2.000)
(2b)	0.356	-0.497	0.519	-1.786		0.875
	(0.516)	(0.000)	(0.099)	(0.000)		(1.800)
(2c)	-0.025	-0.010	0.951	-0.050		0.994
	(0.001)	(0.003)	(0.000)	(0.000)		(2.034)
(2d)	-0.070	-0.015	-0.101	1.187		0.999
	(0.051)	(0.141)	(0.095)	(0.000)		(1.945)
(2a)	0.718	0.119	-0.034	0.054	0.179	0.883
	(0.000)	(0.507)	(0.001)	(0.000)	(0.029)	(2.001)
(2b)	0.365	-0.496	-1.788	0.542	-0.032	0.875
	(0.527)	(0.000)	(0.099)	(0.000)	(0.873)	(1.802)
(2c)	-0.017	-0.009	0.949	-0.029	-0.042	0.995
	(0.013)	(0.006)	(0.000)	(0.000)	(0.000)	(2.080)
(2d)	-0.149	-0.021	-0.089	0.977	0.358	0.999
	(0.000)	(0.006)	(0.000)	(0.000)	(0.000)	(2.195)

Note: The columns report the sum of the regression coefficients in a levels VAR, with the significance level of the F-test for Granger causality in parentheses. The next columns includes Durbin-Watson and R^2 statistics for each VAR equation.

responses, there is a stronger initial positive effect from the debt shock in Figure 2.5. Cumulatively, the effect of the tax shock is smaller than the effect of the debt shock. A more striking difference occurs when the effect of the Liberty Loan Program is controlled for. The left-hand side of Figure 2.6, shows a strong negative effect of production in response to a debt shock. Production then recovers, and the effect returns to zero. This is evidence of government debt crowding out private investment under normal circumstances (i.e. when the country is not at war and the government has no justification to allocate resources).[14] On the right, the graph of the tax shock looks quite similar to Figure 2.5. Taken together, these two figures show that the positive effect of debt is driven by the period when liberty bonds are circulating in the economy.

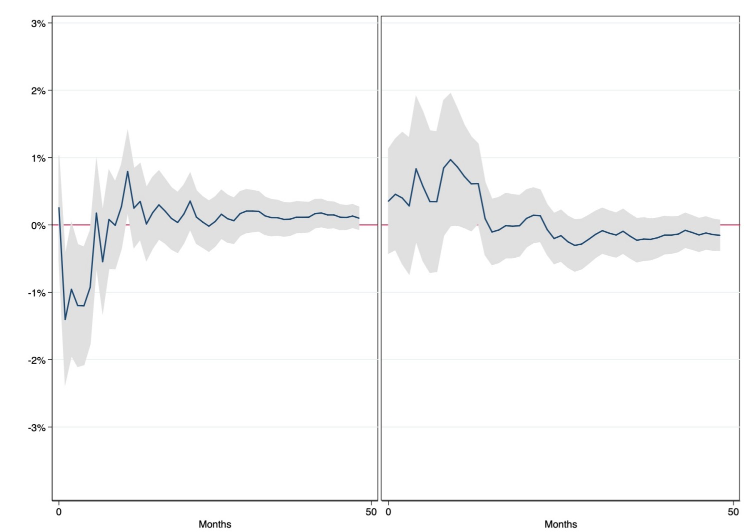
Now, the above analysis is repeated, but instead of using overall industrial production, the fo-

Figure 2.5: IP Responses to Debt and Tax Shocks



Left: Response to Debt **Right:** Response to Tax

Figure 2.6: IP Responses to Debt and Tax Shocks, Controlling for Liberty Loan



Left: Response to Debt **Right:** Response to Tax

Note: 90% confidence intervals shown in light gray. Ordering: Debt, Taxes, M2, IP

[14]Reinhard and Rogoff (2010) began a large literature on the crowding out effects of government borrowing in light of the 2007-08 financial crisis.

cus is on the iron industry which was central to the war effort (The National Iron & Steel Heritage Museum, 2012). Thus, industrial production (IP) is replaced with pig iron production while keeping the remaining variables in the VAR unchanged. Now, in the top panel of Table 2.5, debt Granger causes iron production and has a relatively strong positive aggregate effect. The low significance of the F-tests in equation (2d) coincides with the historical narrative that the debt shock from the Liberty Loan Program was exogenous. The effect of taxes in equation (2a) is again small and not statistically significant.

In the bottom panel of Table 2.5, the liberty loan dummy is positive but not significant. This result can be reconciled by the fact that iron production was highly targeted during the war effort but not afterward. It is likely that after the war, other industries besides iron experienced higher output. This explains why the dummy variable is significant for overall production in Table 2.4 but not significant when only iron production is the focus. In the bottom row, the p-values from the F-tests are all lower than in the top panel. This further shows that the exogenous debt shock comes from the Liberty Loan Program, and when it is controlled for, government debt is no longer exogenous.

The debt impulse response function when controlling for the influence of the liberty bonds differs dramatically from the graph without the exogenous variable. Figure 2.7 shows a strong positive effect of the debt shock of about 2.4% after 18 months. For the response of iron production to a tax shock, there is a much smaller effect, and zero is always within the 90% confidence bands. In Figure 2.8, there is initially a strong, significant negative response of iron to a debt shock. The response does eventually become positive after about 11 months, but the magnitude is much smaller. On the right, the response of iron production to a tax shock is again virtually identical to Figure 2.7.

2.5.2 Role of Intermediated Assets

To better understand the effect of the liberty bonds on production, an alternative measure of the bond program, which highlights the intermediary role of the government asset, is used to continue the analysis. The top panels of Tables 2.6 and 2.7 use the constructed intermediated assets data as defined by equation 1. The bottom panel uses a more traditional measure of intermediated assets, the difference between M2 and M1. This variable is referred to as restricted assets, since it is a more narrow definition of assets which excludes liberty bonds.

Starting with the effect of the assets on overall production, Table 2.6 shows that intermediated

Table 2.5: VAR Estimates for Systems with Income Tax Receipts, the Money Stock, Debt, and Iron Production

	Levels VAR Granger Tests				Exogenous	
Eq. #	Iron	Tax	M2	Debt	LL	$R^2/(DW)$
(2a)	0.971	0.025	-0.434	0.272		0.941
	(0.000)	(0.650)	(0.000)	(0.019)		(1.974)
(2b)	0.025	-0.412	-0.013	-0.024		0.878
	(0.239)	(0.000)	(0.048)	(0.000)		(1.768)
(2c)	-0.004	-0.013	0.986	-0.047		0.994
	(0.362)	(0.002)	(0.000)	(0.000)		(2.056)
(2d)	-0.012	-0.024	-0.115	1.185		0.999
	(0.766)	(0.293)	(0.248)	(0.000)		(1.945)
(2a)	0.966	0.021	-0.419	0.245	0.070	0.941
	(0.000)	(0.641)	(0.000)	(0.016)	(0.306)	(1.985)
(2b)	0.021	-0.416	-0.010	-0.048	0.008	0.878
	(0.244)	(0.000)	(0.047)	(0.000)	(0.967)	(1.768)
(2c)	0.000	-0.010	0.974	-0.025	-0.046	0.995
	(0.453)	(0.007)	(0.000)	(0.000)	(0.000)	(2.134)
(2d)	-0.050	-0.048	-0.016	1.003	0.353	0.999
	(0.151)	(0.041)	(0.001)	(0.000)	(0.000)	(2.144)

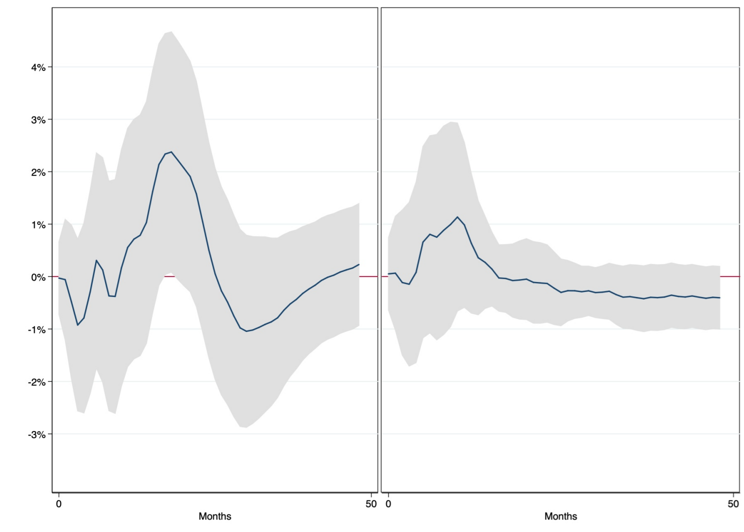
Note: The columns report the sum of the regression coefficients in a levels VAR, with the significance level of the F-test for Granger causality in parentheses. The next columns includes Durbin-Watson and R^2 statistics for each VAR equation.

assets Granger cause industrial production, and their aggregate effect on production is quite large. The effect of the restricted assets on production in the bottom panel is also large and significant. This should not be unexpected as M2 less M1 is essentially a savings measure, and savings is generally expected to have a positive effect on production. Also, while taxes have no significant effect on production in the top panel, they have a small, significant, positive effect in the lower panel. In this case, the impulse response functions are key to understanding the differences between these two systems.

On the left side of Figure 2.9, the asset shock leads to a strong initial negative response before quickly becoming positive. This likely reflects the lag between the initial investment and the completed physical product mentioned in Bencivenga and Smith (1991). The maximum effect of the intermediated asset shock on production occurs at 11 months. The effect of the tax shock, shown on the right side of Figure 2.9, has a slight initial positive effect, but then remains fairly close to zero.

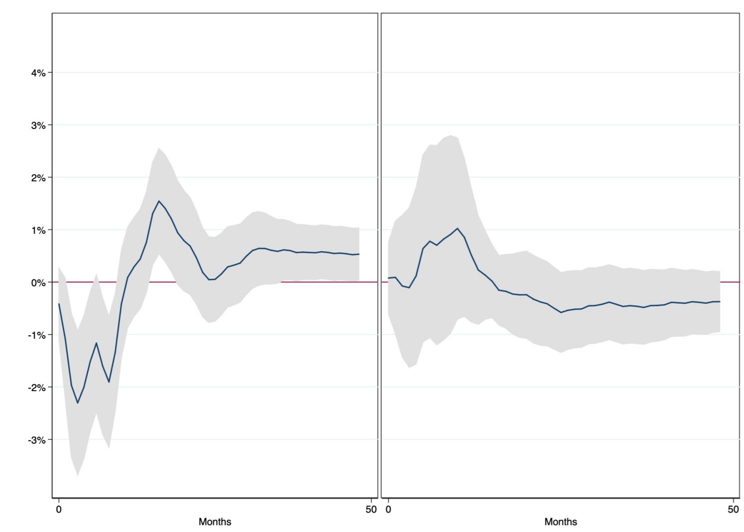
When the IRF is estimated without including privately held liberty bonds, the overall shape of the response to an asset shock is fairly similar. However, as Figure 2.10 shows, the graph appears to

Figure 2.7: Iron Responses to Debt and Tax Shocks



Left: Response to Debt **Right:** Response to Tax

Figure 2.8: Iron Responses to Debt and Tax Shocks, Controlling for Liberty Loan



Left: Response to Debt **Right:** Response to Tax

Note: 90% confidence intervals shown in light gray. Ordering: Debt, Taxes, M2, Iron Production

be shifted down and to the right. The initial negative response of production is stronger, and it takes more time for the effect of the restricted assets shock to become positive. The maximum effect is only reached after 27 months. Further, this effect is still smaller than Figure 2.9 at just about 1%. The effect of the tax shock is fairly similar to before, but there is a slightly stronger initial positive effect.

Table 2.6: VAR Estimates for Systems with Income Tax Receipts, the Money Stock, Intermediated Assets or Restricted Assets, and Production

Levels VAR Granger Tests					
Eq. #	IP	Tax	M1	Intermed. Assets*	$R^2/(DW)$
(2a)	0.775	-0.092	-0.230	0.841	0.790
	(0.000)	(0.665)	(0.001)	(0.000)	(2.016)
(2b)	-0.092	0.178	-0.022	0.003	0.839
	(0.126)	(0.000)	(0.000)	(0.000)	(1.742)
(2c)	-0.025	-0.022	1.072	0.039	0.928
	(0.066)	(0.091)	(0.000)	(0.000)	(1.996)
(2d)	-0.007	0.003	-0.460	0.818	0.996
	(0.464)	(0.005)	(0.006)	(0.000)	(2.097)
(2a)	0.603	0.086	-0.429	1.667	0.802
	(0.000)	(0.039)	(0.000)	(0.000)	(2.007)
(2b)	0.086	0.691	0.014	0.016	0.761
	(0.020)	(0.000)	(0.000)	(0.011)	(1.815)
(2c)	-0.048	0.014	1.119	-0.249	0.922
	(0.000)	(0.038)	(0.000)	(0.000)	(2.001)
(2d)	-0.017	0.016	-0.092	0.971	0.998
	(0.352)	(0.027)	(0.369)	(0.000)	(2.017)

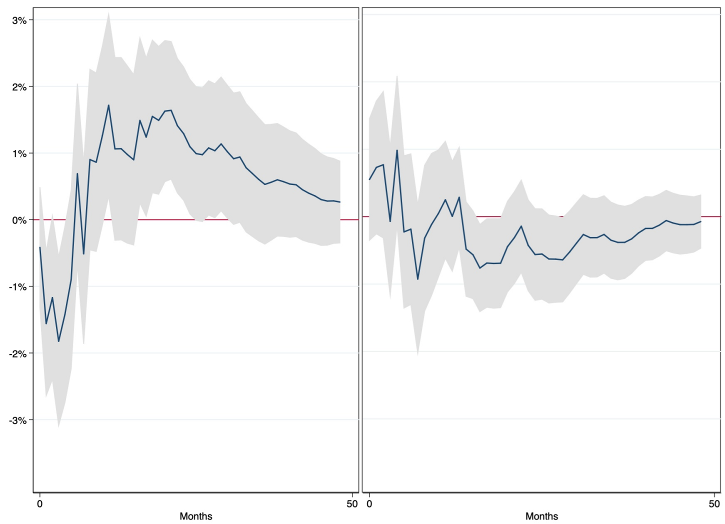
Note: The columns report the sum of the regression coefficients in a levels VAR, with the significance level of the F-test for Granger causality in parentheses. The next columns includes Durbin-Watson and R^2 statistics for each VAR equation. *The bottom panel shows the results from replacing intermediated assets with restricted assets.

When intermediation and the iron industry is analyzed, the top panel of Table 2.7 shows that intermediated assets also Granger cause iron production. The aggregate effect of intermediated assets, which includes privately held liberty bonds, on iron production is actually six times larger than the effect of restricted assets. In both systems, the aggregate effect of taxes on production is negative but insignificant.

Comparing the IRFs for the two systems, the responses to the intermediated asset shock and restricted asset shock again have a similar shape. Figures 2.11 and 2.12 both show a strong initial negative response to the asset shock. With the restricted assets, the negative response is strong at almost -5% compared to -3% when liberty bonds are included. With the liberty bonds in Figure 2.11, the positive effect occurs more quickly, reaching its maximum positive effect of 2.3% at 15 months. In Figure 2.12, this maximum effect is not reached until 17 months have passed.

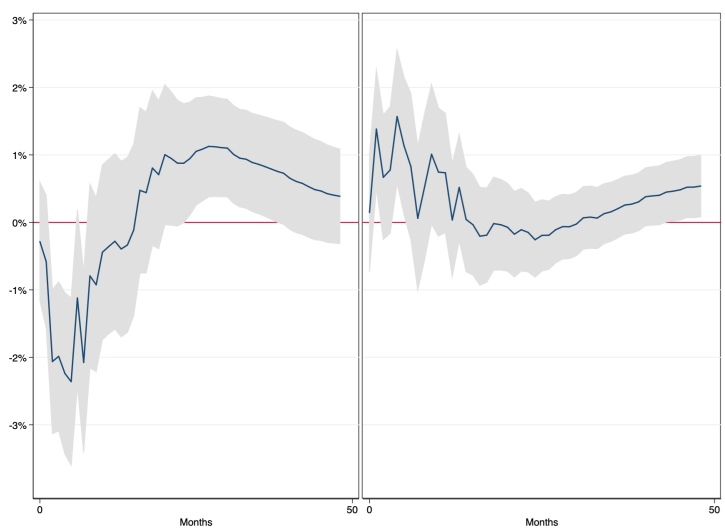
The Appendix shows the related tables and IRF results when pig iron production is replaced with steel ingot production for both the government debt and intermediated asset systems. As the

Figure 2.9: IP Responses to Asset and Tax Shocks



Left: Response to Asset **Right:** Response to Tax

Figure 2.10: IP Responses to Restricted Asset and Tax Shocks



Left: Response to Asset **Right:** Response to Tax

Note: 90% confidence intervals shown in light gray. Ordering: Intermed. Assets, Taxes, M1, IP

iron and steel industries are closely related, especially in terms of their uses in war production, it is reassuring that the results for steel are quite similar to the ones that have been presented for iron above.[15]

[15] In fact, pig iron is the main raw material used in the manufacturing of steel (Schaetzl, 2021).

Table 2.7: VAR Estimates for Systems with Income Tax Receipts, the Money Stock, Intermediated Assets or Restricted Assets, and Iron Production

	Levels VAR Granger Tests				
Eq. #	Iron	Tax	M1	Intermed. Assets*	$R^2/(DW)$
(2a)	0.959	-0.150	-0.369	0.618	0.940
	(0.000)	(0.188)	(0.016)	(0.008)	(2.035)
(2b)	-0.150	0.178	-0.022	-0.003	0.842
	(0.042)	(0.000)	(0.010)	(0.000)	(1.746)
(2c)	-0.004	-0.022	1.065	0.052	0.923
	(0.509)	(0.029)	(0.000)	(0.000)	(2.004)
(2d)	-0.017	-0.003	-0.403	0.873	0.995
	(0.924)	(0.002)	(0.023)	(0.000)	(2.102)
(2a)	0.866	-0.071	-0.070	0.103	0.945
	(0.000)	(0.396)	(0.088)	(0.000)	(1.995)
(2b)	-0.071	0.827	-0.007	0.014	0.753
	(0.242)	(0.000)	(0.000)	(0.037)	(1.834)
(2c)	-0.008	-0.007	1.053	-0.212	0.913
	(0.069)	(0.025)	(0.000)	(0.000)	(2.018)
(2d)	0.004	0.014	-0.118	1.039	0.998
	(0.492)	(0.018)	(0.106)	(0.000)	(2.007)

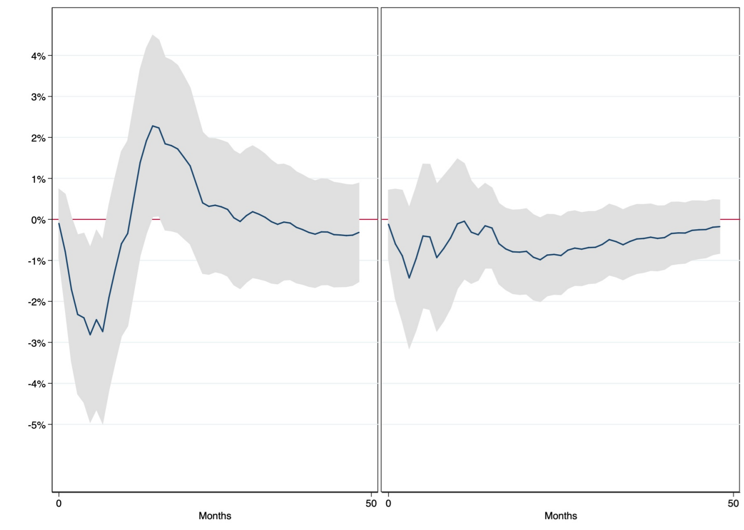
Note: The columns report the sum of the regression coefficients in a levels VAR, with the significance level of the F-test for Granger causality in parentheses. The next columns includes Durbin-Watson and R^2 statistics for each VAR equation. *The bottom panel shows the results from replacing intermediated assets with restricted assets.

2.6 Conclusion

The preceding analysis demonstrates that in addition to the well-known high participation rates for the Liberty Loan Program, the initiative was successful in promoting war production. Specifically, war bonds and a modest increase in tax revenue combined to be an effective war finance plan for the U.S. economy during World War I. While it is unlikely that Secretary McAdoo fully understood the intermediation mechanism at play when he developed the Liberty Loan Program, it nevertheless, appears to correspond closely to modern growth theory regarding the positive effects of financial development.

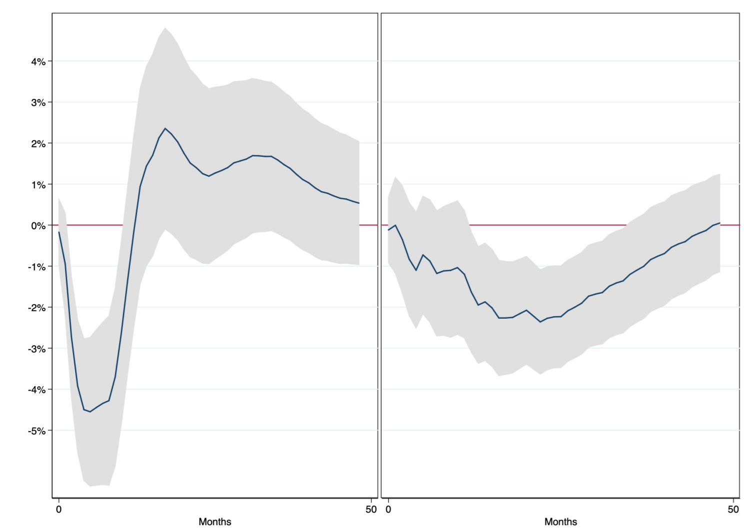
When studying the impact on industrial production broadly, government debt is moderately more effective than taxation. Additionally, controlling for the period when liberty bonds circulated in the economy confirms that the bonds themselves drive the positive effects on production. In the peacetime period before war bonds were issued, government debt had a negative effect on production, which highlights the standard result of government spending crowding out private investment.

Figure 2.11: Iron Responses to Asset and Tax Shocks



Left: Response to Asset **Right:** Response to Tax

Figure 2.12: Iron Responses to Restricted Asset and Tax Shocks



Left: Response to Asset **Right:** Response to Tax

Note: 90% confidence intervals shown in light gray. Ordering: Intermed. Assets, Taxes, M1, Iron Production

When studying the industries directly involved in war production, and iron and steel in particular, the role of debt issuance becomes more apparent. As theory predicts, the bonds successfully increase production, and this growth is specifically driven by the war industries. This provides evidence for the reallocation of the factors of production from consumption goods ("unnecessary goods" in the words of Carver (1919)), to the manufacturing of necessary war materials.

The results from comparing the two systems with different measures of intermediated assets further underscore the government's wartime role as a productive intermediator. The liberty bonds channeled saving into production with a shorter lag time than savings accounts and other similar types of financial resources. This shows that the government played an active role, superior to the banks alone, in allocating the factors of production to the key war industries identified by the Council of National Defense (Baruch, 1921).

Turning to the impact of tax shocks, it is striking how similar the tax IRFs appear with and without controlling for the liberty loan period (Figures 2.5 and 2.6; Figures 2.7 and 2.8). This suggests the Liberty Loan Program had little effect on the government's ability to levy taxes during and after the war. The progressive tax structure meant that the average American did not have to pay a significant amount of taxes. Additionally, the biggest potential tax loophole was only available during the First Liberty Loan, which also raised the least amount of money as shown in Table 2.1. Thus, it seems unlikely that most wealthy Americans simply invested in war bonds to avoid taxes as Kang and Rockoff (2015) suggest.

While there is strong evidence that liberty bonds positively affected production through financial intermediation during the war, it should be emphasized that these results do not take into account the long-run effects of the program or the welfare implications. At present, that question is beyond the scope of this paper and is a potential avenue for future research. Also, this paper does not broadly conclude that issuing government debt leads to a short-run increase in production. The results presented here hold during a never-before experienced worldwide conflict when most Americans had minimal financial education, and thus, the country's financial intermediaries were largely underutilized. Nevertheless, these results about the U.S. experience during World War I should be thoughtfully analyzed by policymakers when discussing various forms of government finance in the future.

2.7 Appendix

Figure 2.13: Poster for Second Liberty Loan

Source: Library of Congress

Table 2.8: Data Sources

Data	Source
Index of Industrial Production (IP)	Miron and Romer (1990)
Pig Iron Production	NBER Macrohistory Database
Steel Ingot Production	NBER Macrohistory Database
M1	Friedman and Schwartz (1963)
M2	Friedman and Schwartz (1963)
Federal Income Tax Receipts	NBER Macrohistory Database
Aggregate Government Debt	U.S. Treasury Department
Liberty Bonds Outstanding	U.S. Treasury Department
Aggregate holding of Liberty Bonds in Nat'l Banks	Comptroller of the Currency
Aggregate holding of Liberty Bonds by Federal Reserve	Federal Reserve Bulletin
U.S. Index of General Price Level	NBER Macrohistory Database

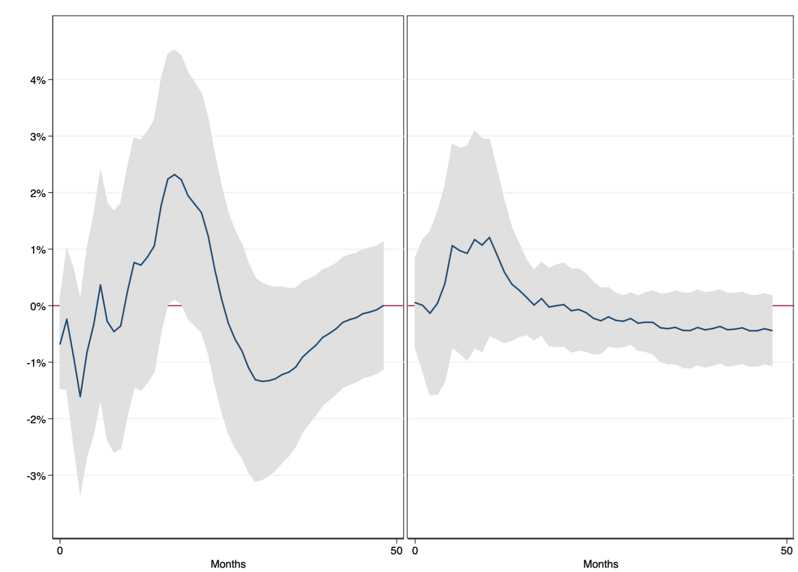
Note: Money stock data already seasonally adjusted. All production data and tax data seasonally adjusted by author.

Table 2.9: VAR Estimates for Systems with Income Tax Receipts, the Money Stock, Debt, and Steel Production

	Levels VAR Granger Tests				Exogenous	
Eq. #	Steel	Tax	M2	Debt	LL	$R^2/(DW)$
(2a)	0.952	0.039	0.408	0.220		0.930
	(0.000)	(0.522)	(0.000)	(0.021)		(1.992)
(2b)	0.039	-0.387	-0.012	-0.023		0.879
	(0.119)	(0.000)	(0.072)	(0.000)		(1.771)
(2c)	-0.003	-0.012	0.948	-0.048		0.994
	(0.050)	(0.002)	(0.000)	(0.000)		(2.058)
(2d)	-0.007	-0.023	-0.028	1.193		0.999
	(0.657)	(0.295)	(0.264)	(0.000)		(1.949)
(2a)	0.951	0.038	0.411	0.210	0.035	0.930
	(0.000)	(0.524)	(0.000)	(0.023)	(0.660)	(1.994)
(2b)	0.038	-0.391	-0.009	-0.045	0.025	0.879
	(0.119)	(0.000)	(0.069)	(0.000)	(0.898)	(1.770)
(2c)	0.000	-0.009	0.941	-0.027	-0.044	0.995
	(0.155)	(0.005)	(0.000)	(0.000)	(0.000)	(2.129)
(2d)	-0.039	-0.0450	0.033	1.007	0.357	0.999
	(0.109)	(0.053)	(0.001)	(0.000)	(0.000)	(2.135)

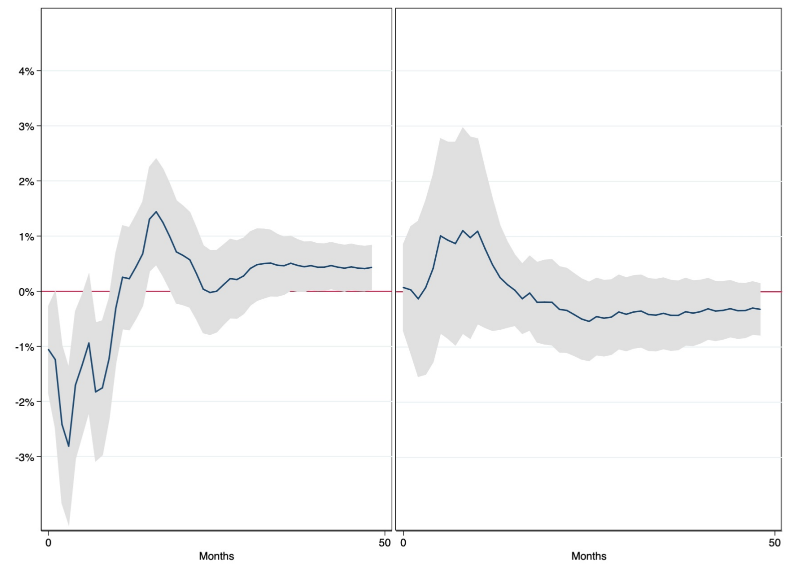
Note: The columns report the sum of the regression coefficients in a levels VAR, with the significance level of the F-test for Granger causality in parentheses. The next columns includes Durbin-Watson and R^2 statistics for each VAR equation.

Figure 2.14: Steel Responses to Debt and Tax Shocks



Left: Response to Debt **Right:** Response to Tax

Figure 2.15: Steel Responses to Debt and Tax Shocks, Controlling for Liberty Loan



Left: Response to Debt **Right:** Response to Tax

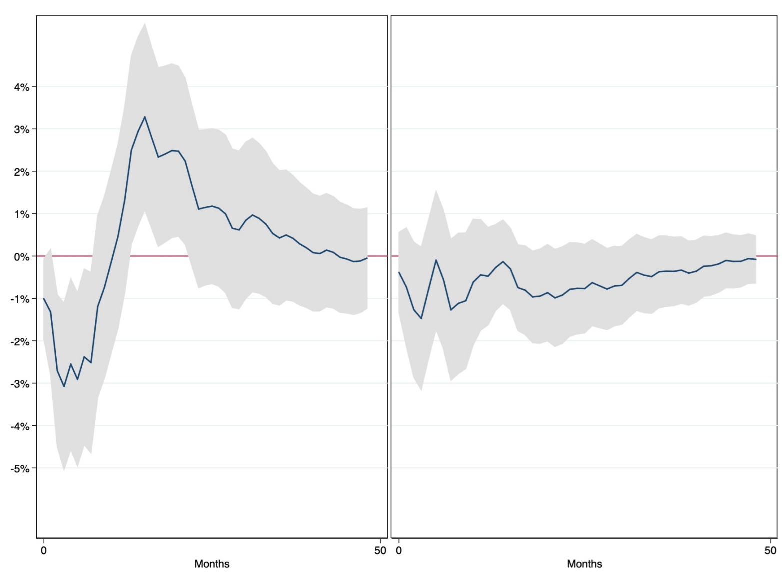
Note: 90% confidence intervals shown in light gray. Ordering: Debt, Taxes, M2, Steel Production

Table 2.10: VAR Estimates for Systems with Income Tax Receipts, the Money Stock, Intermediated Assets or Restricted Assets, and Steel Production

	Levels VAR Granger Tests				
Eq. #	Steel	Tax	M1	Intermed. Assets*	$R^2/(DW)$
(2a)	0.899	-0.191	0.591	0.530	0.918
	(0.000)	(0.152)	(0.001)	(0.005)	(2.020)
(2b)	-0.191	0.234	-0.018	-0.002	0.844
	(0.035)	(0.000)	(0.032)	(0.000)	(1.755)
(2c)	-0.006	-0.018	1.066	0.032	0.927
	(0.159)	(0.042)	(0.000)	(0.000)	(2.016)
(2d)	-0.013	-0.002	-0.401	0.866	0.996
	(0.793)	(0.003)	(0.035)	(0.000)	(2.086)
(2a)	0.783	-0.045	0.441	0.190	0.924
	(0.000)	(0.387)	(0.005)	(0.000)	(2.001)
(2b)	-0.045	0.776	-0.001	0.014	0.756
	(0.185)	(0.000)	(0.000)	(0.023)	(1.853)
(2c)	-0.011	-0.001	1.042	-0.214	0.916
	(0.020)	(0.082)	(0.000)	(0.000)	(2.042)
(2d)	0.004	0.014	-0.116	1.021	0.998
	(0.963)	(0.045)	(0.212)	(0.000)	(2.015)

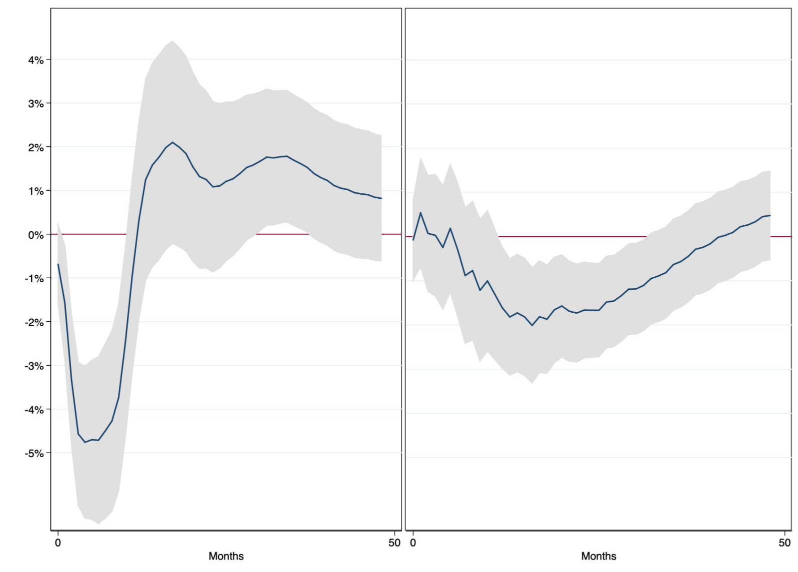
Note: The columns report the sum of the regression coefficients in a levels VAR, with the significance level of the F-test for Granger causality in parentheses. The next columns includes Durbin-Watson and R^2 statistics for each VAR equation. *The bottom panel shows the results from replacing intermediated assets with restricted assets.

Figure 2.16: Steel Responses to Asset and Tax Shocks



Left: Response to Asset **Right:** Response to Tax

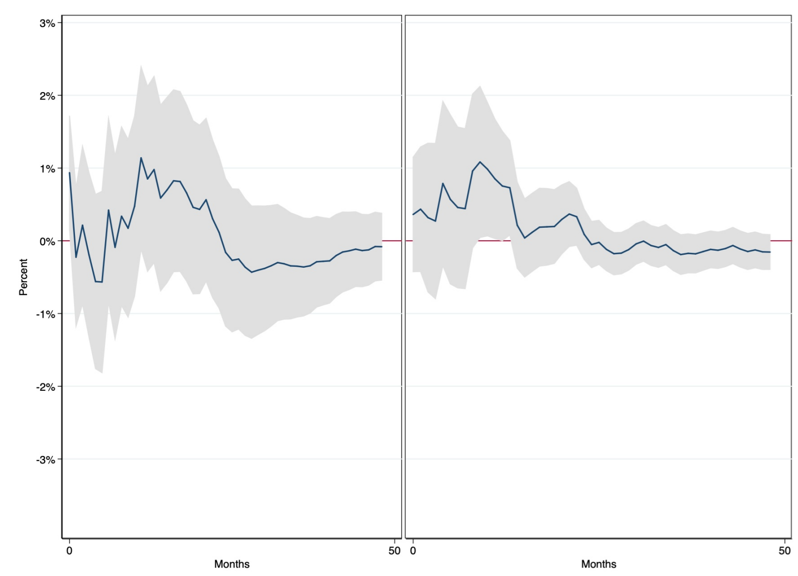
Figure 2.17: Steel Responses to Restricted Asset and Tax Shocks



Left: Response to Asset **Right:** Response to Tax

Note: 90% confidence intervals shown in light gray. Ordering: Intermed. Assets, M1, Steel Production

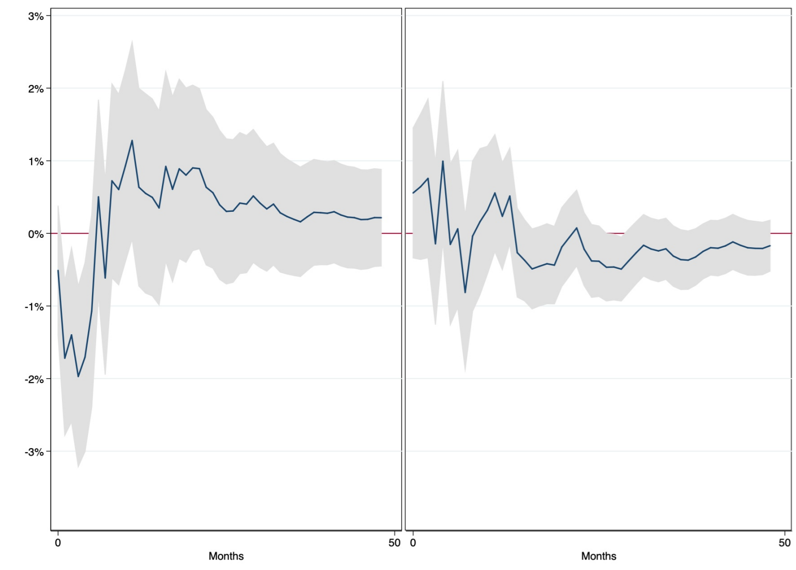
Figure 2.18: IRF from Figure 2.5 with Alternative Ordering



Left: Response to Debt **Right:** Response to Tax

Note: 90% confidence intervals shown in light gray. Ordering: Taxes, Debt, M2, IP

Figure 2.19: IRF from Figure 2.9 with Alternative Ordering



Left: Response to Asset **Right:** Response to Tax

Note: 90% confidence intervals shown in light gray. Ordering: Taxes, Intermed. Assets, M1, IP

CHAPTER 3

New York Stock Exchange Member Firms and Financial Sophistication in the 1920s and Beyond

3.1 Introduction

While relatively low stock market participation in the United States has remained a puzzle for some time, the 1920s was actually a period of expanding participation in financial markets (Haliassos and Bertaut, 1995). This growth has been attributed to the financial education from the Liberty Loan Program, rising incomes, popularity of employee stock ownership programs (ESOPs), and reduced barriers to entry (Warshow, 1924; Hilt et al., 2020; Ott, 2011). This increased interest in markets led brokerage firms, which had a director on the floor of the New York Stock Exchange (NYSE), to establish new branches across the country throughout the 1920s, particularly in the South and West.[1] For the most part, the stock market soared, and investing middle- and working-class households gained. This extended period of growth, however, ended with the crash of the stock market in October of 1929 and the Great Depression (White, 1990). Stock market participation rates fell throughout the Depression and recovery, and they did not start rising again until the 1950s (Maxey, 2002). The main goal of this chapter is to understand the patterns behind member-firm expansion and address the relationships between stock market participation and the local economy leading up to and following the Great Depression.

Beyond participation, stock market accessibility is a topic of current interest as exhibited by the popularity and controversy surrounding commission-free investing companies like Robinhood (Fitzgerald, 2021). By studying this historical period, the 1920s, when the perception of the stock market changed and many communities were connected to Wall Street for the first time, I can address questions about the implications of expanding access to the stock market (White, 1990; Ott, 2011). If these communities with local, member firms experienced increased savings and capital accumulation and even better endured the Great Depression, it could have clear policy implications for the importance of financial education and accessibility.

Additionally, this research can add to the discussion on how the banking sector and financial

[1] Throughout this chapter, brokerage firms which had a director or partner on the floor of the NYSE are referred to as "member firms."

markets coexist and the role of financial deepening and economic growth (see McKinnon (1973)). Closely related to this is the idea of financial sophistication. At the local level, banks are primarily used as a proxy for financial development in the finance-growth literature (Jayaratne and Strahan, 1996). However, brokerage offices could add another layer to reflect the level of local financial sophistication beyond what banks provide.

This historical period also coincides with new regulations affecting financial markets, specifically a shadow market, which can inform our understanding of the effects of intervention on financial development and economic growth (Ott, 2011). Successful campaigns to close bucket shops originated in New York City but later spread throughout the country during the early 1920s. Bucket shops were a type of "shadow market" because they were places where people could essentially bet on the price movements of stocks and bonds without actually purchasing the asset (Hochfelder, 2006). Often the owners of bucket shops would intentionally mislead customers about their bets. Nevertheless, these establishments garnered interest in the stock market for middle- and working-class people that were formerly completely excluded from financial markets.

This paper makes strides toward addressing these significant questions about the effects of stock market accessibility in the 1920s, outlining potential mechanisms driving these relationships, and understanding their implications. With this goal, data on the locations of NYSE member brokerage firms are collected throughout the 1910s, 1920s, and early 1930s. To my knowledge, this is the first time these office locations have been studied empirically. These data are aggregated to the county level and combined with decadal U.S. census data.

First, probit and logit regressions are used to study the entry of these member firms throughout the early twentieth century to understand the characteristics associated with counties gaining their first office. Then, a growth regression strategy is developed to study the relationships between member firm and growth outcomes. The outcomes of interest include the firms' relationship with output, capital accumulation, and the farming industry.

The data on member-firm expansion in the 1920s suggests that there were at least two waves of growth in the U.S. Local banks seem to attract member firms in the late 1910s and early 1920s, but this relationship wanes in the later wave from 1922 to 1930. This suggests banks and brokerages were compliments initially but later became competitors for local savings.

Furthermore, the finance-growth regressions suggest that a county gaining its first office is positively related to manufacturing output growth and negatively related to farm growth at the county level. Additional offices, however, are negatively related to output growth, highlighting the potentially extractive local effects of the stock market.

The paper is organized as follows: the next section provides background information on the stock market in the 1920s. Section 3.3 describes the data and provides some summary statistics. Sections 3.4 and 3.5 present results on brokerage firm entry and their relationship with county-level growth, respectively. And, the final section discusses avenues for future research and concludes.

3.2 Background on the Stock Market in the 1920s

The 1920s are generally depicted as a decade of economic prosperity and growth. This idea holds especially true for the New York Stock Exchange (NYSE), which experienced extraordinary growth in the number of trades and market participation by the general public.

At the turn of the 20th century, however, people who owned securities were viewed as a dangerous, non-producing class (Ott, 2011). Initially, brokerage firms only sought out wealthy individuals as customers. Barriers to entry for working- and middle-class people included a $1,000 minimum investment value and a standard $100 per share cost.

The Liberty Loan Program of World War I is credited in part for changing this view of securities and for making them more accessible to "every man" (Ott, 2011). Recent research has provided empirical evidence for this view.[2] Thus, after the financial education due to the campaigns for war bonds during the Great War, there emerged a new class of potential investors.

Warshow (1924) estimates the number of stockholders in the immediate post-war period (1917-1921) using data on income tax returns and finds evidence suggesting stock ownership not only expanded, but also that there was a shift in ownership from wealthier classes to middle- and working-classes. In addition to the Liberty bonds program, he cites the break up of the standard $100 stock into smaller denominations as leading to increased accessibility.

Extending Warshow's work, Means (1930) shows that while there appeared to have been a demographic shift in ownership immediately following the war, after 1921, the proportion of rich and middle class households owning stock remained fairly constant. He credits the popularity of em-

[2] Using panel data on U.S. counties, Hilt et al. (2020) conclude that participation in the Liberty Loan Program led to an increase in the number of investment banks and more diversity in the asset-holdings of American households in the 1920s.

ployee stock ownership programs (ESOPs) from 1919 to 1923 with promoting the initial shift in stockholders. After 1923, the number of companies adopting these types of programs stalled. Nevertheless, Means estimates there were more than 800,000 employee stockholders by 1928.

Another reason the stock market had a nefarious reputation at the turn of the century was due to bucket shops. These underground gambling operations were places where people could essentially bet on the price movement of stocks without actually purchasing the asset. Thus, they fostered a type of early "shadow market." Often, the owners of bucket shops would intentionally mislead customers and abscond with their funds (Ott, 2011). Legal campaigns to identify and shut down bucket shops grew throughout the 1920s, and the certified brokerages that remained had much stronger reputations due to distinguishing themselves from bucket shops (New York Times, 1923).

With this transition underway, reputations repaired, and a new class of investors emerging, NYSE member firms expanded to take advantage of the untapped market of middle-class customers (Ott, 2011). Many certified brokerages even moved into communities that had recently closed bucket shops. From 1920 to 1929, the number of NYSE branch offices almost tripled. An estimated 1.5 million Americans had set up accounts at these brokerage firms across the country by 1929.

What drove brokerages to establish these firms in specific cities over others? Unfortunately, data on bucket shop locations and activities do not exist because of their illicit nature, but narrative evidence suggests NYSE member firms moved in and replaced them. While some of the branch locations seem sporadic, an interview with a partner at the New York-based brokerage firm, Hinck Brothers & Co, about the opening of a new branch of the firm in Hartford, Connecticut provides some additional background. In the article, resident partner Russell A. Spalding states,

> The great growth in investment business in recent years and the consequent expanding interests of the company have brought it directly in contact with the Hartford financial world. Hartford itself has grown to be a financial center of considerable size and we have felt that our company should locate here to better carry on the business we formerly conducted by wire and mail with brokers, investors, and financial institutions of Hartford."[3]

[3] Hartford Courant (1928), June 28, 1928

Thus, the firm believes proximity is important for business dealings and for more efficiently working with clients in the Hartford area. This clearly exhibits that at least some firm owners selected cities on the basis of certain criteria. This creates an endogeneity issue making it difficult to estimate a direct causal effect of these brokerage firms. It is less clear that this selection based on financial sophistication occurs for some rural areas, such as Kewanee, Illinois and Newberry, South Carolina. Specific factors that influence location decisions of these firms are still largely unknown. This is an area for future research and could lead to the development of an empirical design to measure causality. Possible future work to estimate a causal effect is discussed more thoroughly in Section 3.6.

Nevertheless, narrative evidence does suggest these local firms were quite successful at increasing participation (Ott, 2011). A surge in trading is clearly seen by the mid-1920s in Figure 3.1 which shows the volume of shares sold on the NYSE in millions. This period was also accompanied by soaring stock prices until the Great Crash of 1929 when share trading also declined. While a full analysis of the stock market crash is beyond the scope of this paper, some attribute it in part to new investors lacking experience in both buying stock and monitoring firms (White, 1990).

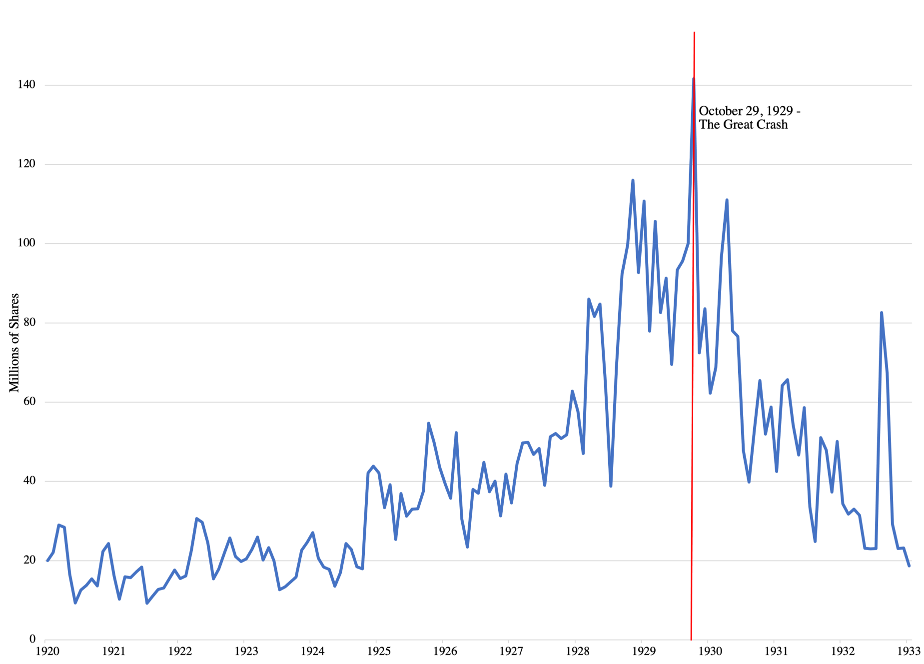
In the next section, the precise locations are presented and the expansion of member-firm offices throughout the early twentieth century is analyzed.

3.3 Data

A major contribution of this project is the county-level data on locations of NYSE member firms and companies with employee stock ownership programs. The brokerage firm locations come from the various NYSE Directories which were found in the NYSE archives (New York Stock Exchange, 1933). These documents list the members of various committees on the exchange as well as the firm names and office locations within New York City and outside of the city. The directory also lists the partners at each firm and indicates whether they are members of the Exchange. By definition, all of the member firms have at least one partner who is a member on the New York Stock Exchange. For all the offices located in New York City (specifically, Manhattan) the directory also lists their street addresses. However, for the "out of town" firms, only their city is provided.

Maps of the 1914, 1922, 1929, and 1930 data are shown in Figures 3.2 through 3.5. The darker circles indicate cities where more than one firm was located.

Figure 3.1: Stock Shares Traded on NYSE

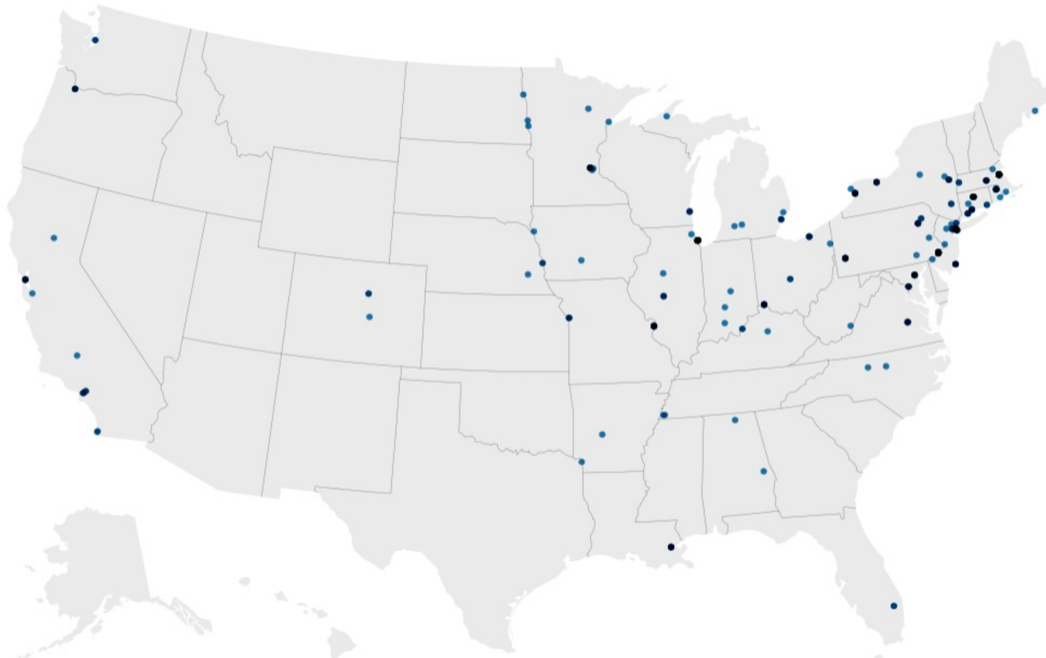


Source: Commercial and Financial Chronicle

In the earliest location data presented, NYSE member firms are clearly clustered in the Northeast region of the United States. Figure 3.2 shows that New Jersey, New York, Connecticut, and Massachusetts have the most cities with brokerage firms according to the 1914 data. In 1922, the vast majority of offices are still concentrated in the Northeast with the Midwest having the next most locations. By contrast, the South and West regions had only a few offices. In 1929, Figure 3.4 shows that the South saw rapid growth in member-firm offices, particularly in the states of North Carolina, Mississippi, and Texas. Nevertheless, the Northeast, particularly New York City, remained the epicenter of financial activity and was highly exposed to the volatility of Wall Street.

From 1929 onward, the locations of the brokerage offices are fairly stable. After the stock market crash, there was less expansion into new areas. The 1929 and 1930s maps are almost identical. The 1933 data is included in the Appendix as Figure 3.9 and again appears quite similar to the previous year's data. For the remainder of this section, the 1929 data is analyzed as the conclusions drawn from it also apply to the later data, but it has the advantage of not being affected by the stock market crash and is more easily compared to the earlier location data.

Figure 3.2: Locations of NYSE Member Firms in 1914



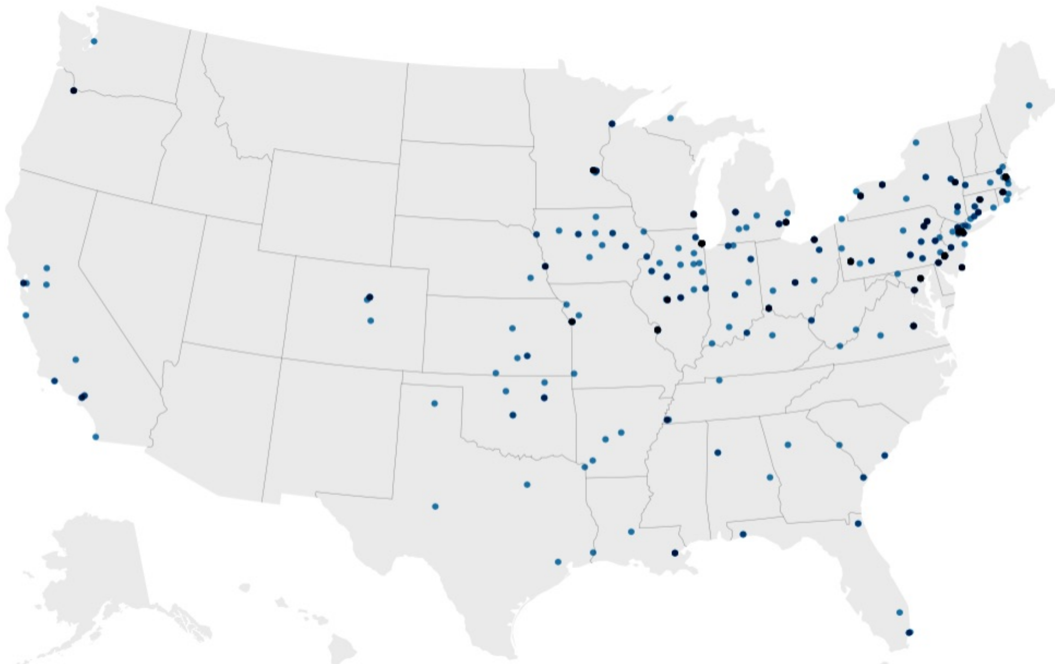
Source: NYSE Archives

To better distinguish the locations of member firms in the Northeast, Figure 3.6 provides the locations of offices in New York, New Jersey, Massachusetts, Pennsylvania, Rhode Island, and Connecticut. With over 800 offices in Manhattan alone, New York county is certainly an outlier. Almost all firms had at least one office in New York City, and most were located in close proximity to the NYSE. After New York, Philadelphia has 90 firms and Boston has 67. For the remainder of the paper, New York county is dropped from the main sample in order to not bias the sample characteristics. Often, other major financial centers are also excluded as detailed in the notes which accompany the figures and tables.

Table 3.1 provides a numerical counterpart to the figures which breaks the location data down by region. Consistent with what the maps showed, the Northeast has by far the most offices and the largest percent of counties with member firms. The South and West regions experienced the greatest growth, with the amount of member firms located in the Midwest remaining steady throughout the decade.

To illustrate the extensive branching of the member firms, Figure 3.7 shows the locations of

Figure 3.3: Locations of NYSE Member Firms in 1922



Source: NYSE Archives

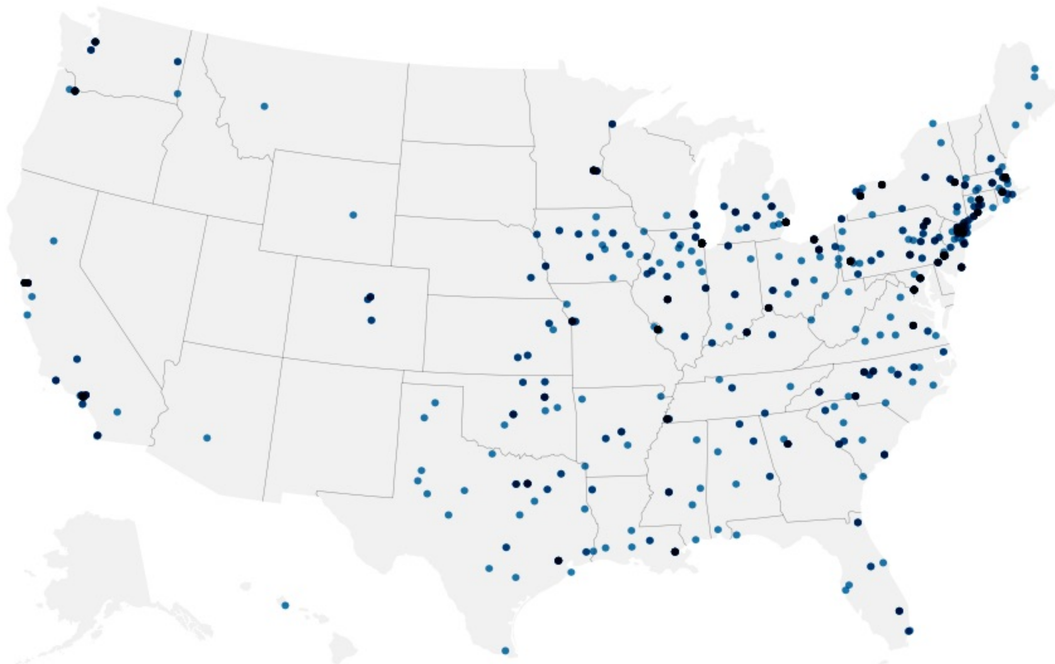
the 10 brokerage firms with the most offices. All firms included on this map have over 20 offices throughout the U.S.[4] While several of the branched firms appear to be regional in nature, such as Lamson Bros. & Co, Fenner & Beane, and H.B. Beer, other firms are fairly dispersed throughout the country.[5] Some of this regional branching suggests that certain communities may only have gained an office due to their proximity to a regional firm. Consider the state of Iowa which has several offices of a Chicago-based brokerage firm. While these locations may still be far from random, the inherent differences between a town in Missouri that never gained an office and a small town in Iowa that did may not be as great as previously thought.

The remaining data comes from standard sources for this time period, but I will briefly summarize them. The majority of the demographic, control variables come from the 1920, 1930, and 1940 U.S. Censuses (Haines, 2010). Jaremski and Wheelock (2020) provide data on the number of banks

[4]Several firms also had offices outside of the United States, such as in Montreal, Calgary, London, and Paris. However, these locations are not included in the sample.

[5]Figure 3.8 in the Appendix shows an advertisement for the Nashville office of Fenner & Beane, which is a regional, New Orleans-based brokerage firm. The ad also notes that they have an office in New York City, which is standard even for NYSE member firms based outside of New York.

Figure 3.4: Locations of NYSE Member Firm Offices in 1929



Source: NYSE Archives

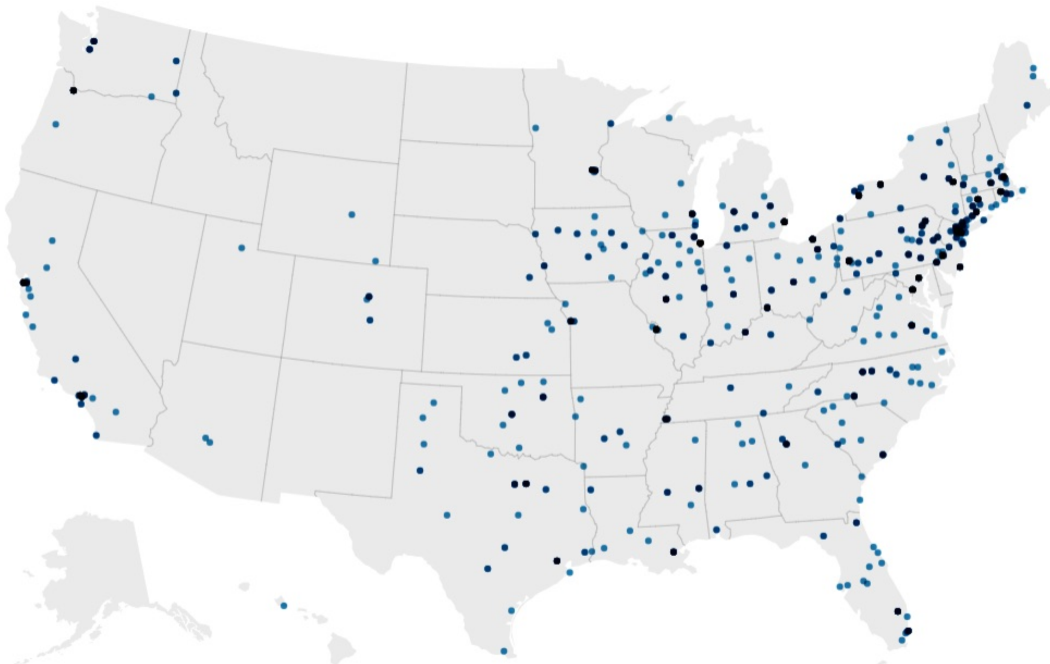
at the county level. To these data, I added data on clearinghouse locations from *Rand McNally's Bank Directory*, January 1920 which could act as another proxy for financial sophistication.

3.3.1 Summary Statistics

To understand initial differences between counties with and without member-firm offices, this section reports summary statistics of the main sample as well as selected regional samples. Since the firms arguably had the widest reach in 1929 as depicted in Figure 3.4, the summary statistics of these data are reported. In the Appendix, the treatment-control balance for the full 1922 sample is provided. As expected by the expansion of the offices between 1922 and 1929, the differences in demographic characteristics between the treated and untreated samples are generally more significant for the 1922 data. This suggests that counties with and without offices became more similar over time.

In the full 1929 sample, Table 3.2 shows that counties with offices tended to be more densely populated, more urban, and more industrial. Further, on average they had a larger share of women,

Figure 3.5: Locations of NYSE Member Firms in 1930



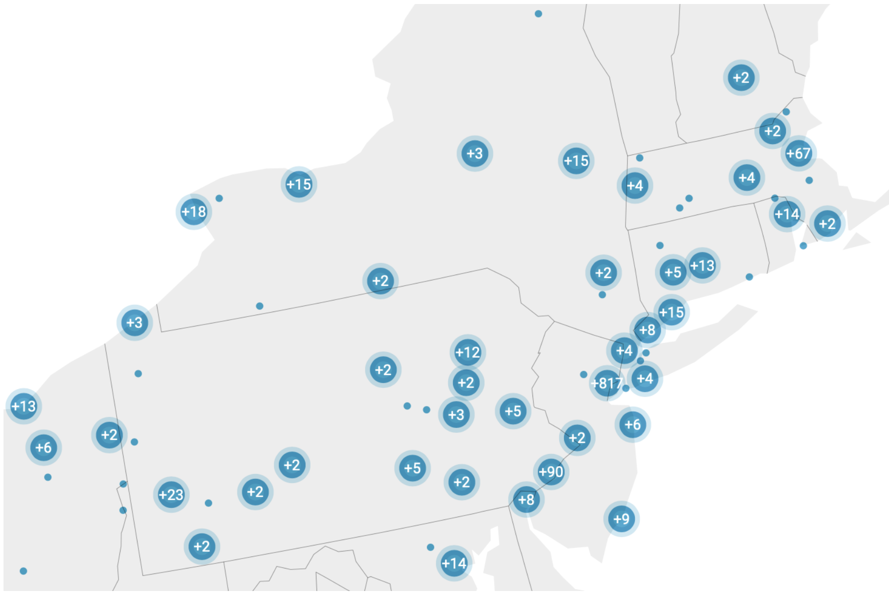
Source: NYSE Archives

more bank deposits, and were far more likely to have a clearinghouse. Interestingly, they have less banks per 1,000 people, but they have more deposits per capita and a larger fraction of national banks. This implies banks were larger in communities with NYSE member firms. These characteristics tend to align with cities having firms being more financially sophisticated.

While the average differences between counties are significant, cutting the sample in various ways reduces these differences. Tables 3.14 and 3.15 in the Appendix provide the balance tables for various sub-samples. The differences between treated and control counties become less significant when more restrictive criteria are used to select the sample, such as a county having no more than one member firm. There is clearly a trade-off between sample size and similarity of the two groups of counties.

Given the stark regional variation in the number of member firms, the main sample is also broken up by each of the four regions listed in Table 3.1. First, Table 3.9 provides statistics on the counties in the Northeast. Since member firms are most concentrated in this region, it makes sense that the two categories are significantly different in terms of demographics and industrialization.

Figure 3.6: Locations of NYSE Member Firm Offices in the Northeast, 1929



Source: NYSE Archives

Similar to the overall sample, counties with member firms in the Northeast are generally more populous, more urban, more industrial, and have less farms.

Moving to counties in the Midwest region, I find that some of these major differences in the overall and Northeast samples are no longer significant. The differences in the number of manufacturing establishments and the share of national banks are not significant. Also, the difference in the percent of the population that is illiterate is less significant in the Midwest than in the Northeast.

For the South, I find that the difference in the population density becomes less significant, and the number of manufacturing establishments is also not significant.

While the summary statistics are fairly consistent, Table 3.12 shows that the counties with and without member-firm offices in the Western region are most similar. The illiterate share, number of manufacturing establishments, and share of national banks are all not statistically different between the two groups.

To summarize, the tables show that there were indeed significant differences between counties with access to the stock market through NYSE member brokerage firms and those without. However, the South and West regions appear to have treated and untreated counties that are most similar to each other initially. This highlights the expansion of the brokerage firms across the South and

Table 3.1: Regional Distribution and Density of NYSE Branch Offices in 1920s

		1922	
Region	Counties	Number of Offices	Percent of Counties with Offices (%)
Northeast	216	294	29.2
South	1,347	79	2.30
Midwest	1,034	218	6.67
West	377	21	2.65
		1929	
Region	Counties	Number of Offices	Percent of Counties with Offices (%)
Northeast	216	529	35.6
South	1,347	252	6.98
Midwest	1,034	275	6.67
West	377	89	4.51

Note: New York county is excluded from main sample.

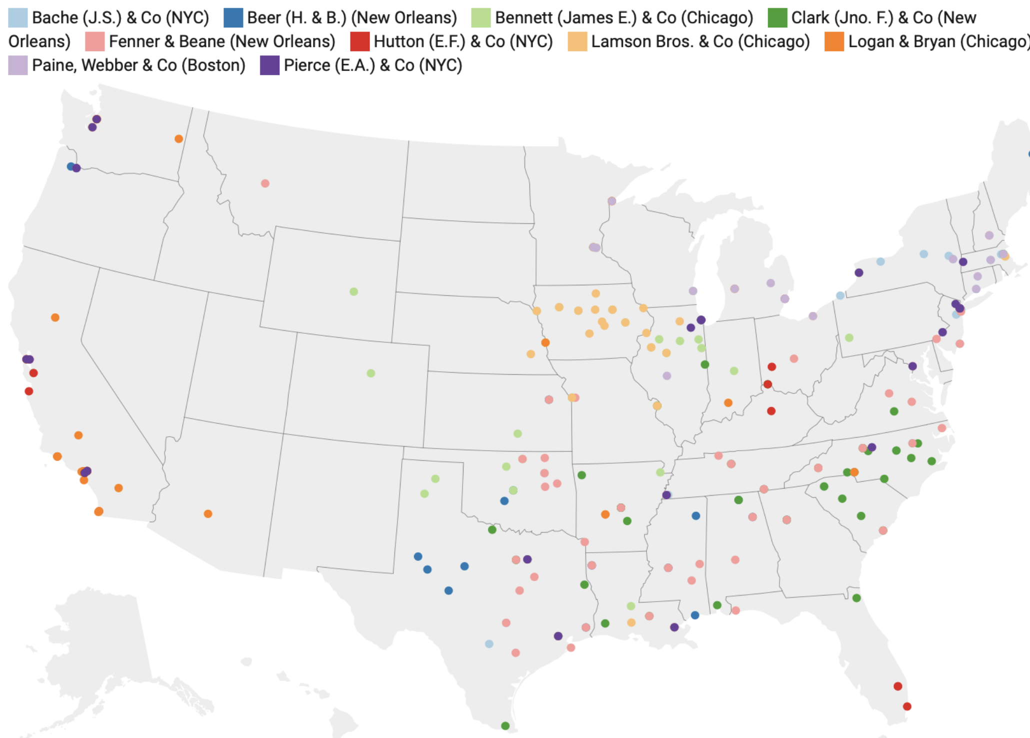
West during the 1920s and implies there may have been room for more growth.

Instead of claiming that member-firm offices were randomly distributed across the country in the 1920s, which these tables clearly fail to show, I will control for these significant demographic variables in the empirical analysis that follows. The significance of many of the characteristics in the balance tables additionally shows that the expansion of firms is likely driven by selection, and I do not claim the results accurately reflect causality. Further, the regional variation, which Tables 3.9 through 3.12 in the Appendix illustrate, confirms that including fixed-effects based on location within the U.S. will be important.

3.4 Member-Firm Entry and Financial Sophistication

Before getting into the relationships between member-firm offices and local outcomes, it is informative to study the characteristics that can explain locations of new offices throughout the 1920s. As mentioned in the previous section, a complete understanding of the motivation behind all of the locations of firms is beyond the scope of this paper. However, this section seeks to summarize the key features associated with gaining a brokerage office.

Figure 3.7: Locations of Branched NYSE Member Firms with 20+ Offices in 1929



Source: NYSE Archives

3.4.1 Empirical Strategy

For this section, the sample is restricted to counties which did not have a brokerage firm at the beginning of the period of study. Equation 3.1 shows the baseline regression.

$$\mathbb{1}_{\text{Office}_{c,t}} = \alpha + \Gamma X_{c,t-1} + \delta_D + \varepsilon_{c,t} \tag{3.1}$$

The dependent variable is then a dummy equal to one if a county gets their first member-firm office in the specific time interval. A vector of pre-determined controls and Federal Reserve level fixed effects are included in the regression. The two periods in which I study member-firm entry correspond to the two waves of expansion for which I have data available. The first period of interest is 1914-1922, when office expansion was primarily in the Midwest and to a lesser degree in the Northeast. The second time period is 1922 through 1930, which corresponds with the fastest growth of members firms, specifically in the South and West.

Table 3.2: Treatment-Control Balance for Full Sample, 1929

	(1) Total	(2) NYSE=1	(3) NYSE=0	(4) Difference
Population	33,870	168,888	21,153	147,735**
	(1852)	(18876)	(517)	(6024)
Density	102.9	714.8	45.30	670**
	(16.05)	(168)	(6.69)	(55.8)
Female Share	0.482	0.493	0.481	0.013**
	(0.000)	(0.001)	(0.000)	(0.002)
Urban Share	0.184	0.595	0.146	0.45**
	(0.004)	(0.016)	(0.004)	(0.013)
Illiterate Share	0.019	0.0010	0.020	-0.010**
	(0.002)	(0.001)	(0.001)	(0.002)
Farms	0.109	0.045	0.115	-0.07**
	(0.001)	(0.002)	(0.001)	(0.003)
Manufacturing Est. (per 1,000 people)	2.19	2.67	2.14	0.53**
	(0.026)	(0.064)	(0.027)	(0.089)
Manufacturing Output	232.81	668.0	189.95	478.09**
	(7.25)	(32.20)	(6.74)	(23.73)
Crop Value	255.38	122.55	267.89	-145.34**
	(3.63)	(8.38)	(3.80)	(12.66)
Total Banks (per 1,000 people)	0.509	0.243	0.534	-0.29**
	(0.007)	(0.012)	(0.008)	(0.025)
Nat'l Bank Share	0.259	0.355	0.250	0.105**
	(0.004)	(0.013)	(0.005)	(0.015)
Deposits	199.26	320.64	187.82	132.82**
	(3.14)	(9.83)	(3.23)	(10.95)
Clearing House	0.080	0.510	0.039	0.471**
	(0.005)	(0.031)	(0.004)	(0.015)
ESOP	0.037	0.293	0.013	0.280**
	(0.003)	(0.029)	(0.002)	(0.011)
Observations	2,974	256	2,718	

Note: New York county is excluded from main sample. Besides the population and density variables, all data are in per capita terms unless otherwise noted. The variables are measured in 1920. * and ** denote significance at the 5% and 1% levels, respectively.

Equation 3.1 is initially estimated according to a linear probability model. It is also estimated with a probit model and logit model to better reflect the bounded nature of the dependent variable.

3.4.2 Results

In Table 3.3 the outcome variable is a dummy equal to one if a county opened their first office between 1914 and 1922. The significance of the estimated coefficients is fairly consistent across the different specifications represented by columns (1) through (3). The magnitudes differ, however, because the probit and logit models are bounded between zero and one. Overall, the results show that banks, county urbanization, and population are all positively related to a county gaining their first office. The concentration of farms in a county is negatively related to gaining a brokerage firm. These results suggest that small, rural, farming areas were initially unlikely to gain a brokerage firm in their community. Finally, the positive and significant coefficient on banks suggests that banks and brokerage offices may compliment each other in terms of financial development in this period. It is possible that bank density was viewed as a signal of financial sophistication by brokerage firm partners when deciding where to expand and attracted member firms in the late 1910s and early 1920s.

Table 3.3: Member-Firm Office Entry, 1914-1922

Dependent Variable: $\mathbb{1}_{\text{Office}_{c,1922}}$			
	OLS	Logit	Probit
	(1)	(2)	(3)
Banks	0.016***	1.262***	0.541***
	(0.005)	(0.420)	(0.198)
Urbanshr	0.557***	3.173***	1.648***
	(0.105)	(1.04)	(0.538)
Totpop	0.024**	1.166**	0.654***
	(0.011)	(0.457)	(0.221)
Literateshr	0.032	3.662	0.465
	(0.069)	(10.375)	(4.399)
Farms	-0.019**	-0.639*	-0.356**
	(0.009)	(0.336)	(0.159)
District Fixed Effects?	Yes	Yes	Yes
New Office Counties	60	60	60
Observations	2,249	2,249	2,249
R-Squared	0.189	0.385	0.392

Note: Office indicator data collected from 1922 and 1914 NYSE Directories. Bank data from Jaremski and Wheelock (2019). Remaining control variables are from U.S. Census. All variables are log transformed and measured in 1910. Robust standard errors are given in parentheses. ** and *** denote significance at the 5 percent and 1 percent levels, respectively.

Table 3.4: Member-Firm Office Entry, 1922-1930

Dependent Variable: $\mathbb{1}_{\text{Office}_{c,1930}}$			
	OLS	Logit	Probit
	(1)	(2)	(3)
Banks	0.002	-0.523	-0.220
	(0.010)	(0.329)	(0.170)
Urbanshr	0.802***	0.833	0.634
	(0.119)	(0.865)	(0.479)
Totpop	0.104***	2.578***	1.332***
	(0.018)	(0.452)	(0.222)
Literateshr	0.083	-2.193	-0.005
	(0.196)	(5.316)	(2.790)
Farms	-0.044***	-0.396*	-0.272***
	(0.012)	(0.206)	(0.106)
Mfgestab	0.012	0.922***	0.404***
	(0.008)	(0.285)	(0.148)
District Fixed Effects?	Yes	Yes	Yes
New Office Counties	147	147	147
Observations	2,223	2,223	2,223
R-Squared	0.300	0.450	0.449

Note: Office indicator data collected from 1922 and 1930 NYSE Directories. Bank data from Jaremski and Wheelock (2019). Remaining control variables from U.S. Census. All variables are log transformed and measured in 1910. Robust standard errors are given in parentheses. *, ** and *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

On the other hand, Table 3.4 shows that banks are no longer significant for counties gaining an office between 1922 and 1930. Again, this wave represents when the largest expansion of member firms occurred, and the sample is restricted to counties that did not have NYSE member firms by 1922. Most strikingly, the coefficient estimates on banks are negative for the logit and probit specifications. Banks and offices appear to no longer be compliments by this point. Member firms may have even become substitutes for banks, but at the very least they did not attract offices like they did in the prior period. Instead, it is the total population and the presence of farming that significantly predicts a county getting their first office. This could be explained by a possible over-expansion of offices in the booming 1920s.

3.5 Member Firms and County Growth

With the background of what county characteristics are associated with gaining brokerage firms discussed, I now focus on my principal results of understanding how these NYSE member firms are related to county-level growth.

3.5.1 Possible Mechanisms

My main research question regarding how NYSE member firms and increased accessibility to the stock market may have affected communities and local industries during the 1920s and 1930s is intentionally broad because there are many possible mechanisms that could account for the strength and direction of this relationship.

The previous section studies the relationship between banks and brokerage firms and discusses the possibility that brokerage offices represent a broad measure of financial sophistication and education. These firms could act as an impetus for financial education and learning about the stock market in the 1920s simply by their presence in a community. This mechanism could be summarized as a type of education spillover effect. For example, suppose Harry lives in a mid-sized town that has recently opened a brokerage firm in the mid-1920s. While this person does not have an interest in investing in the stock market initially, his neighbor decides to open an account at the brokerage. Over the next few months, conversations between Harry and his neighbor about how his portfolio has grown pique his interest. He begins reading about the stock market in the newspaper and discusses his investment options with his banker. This simple story illustrates that while Harry has no direct relationship with the brokerage firm, he still learns about the stock market from a community member due to this local office.

Another mechanism to consider is the potential that brokerage firms actually could extract local savings from the community. When NYSE member firms open in a county, they compete with the local banks for investment funds. Instead of keeping savings in the local bank which could be loaned to start a new business in the community, someone with access to a brokerage firm could deposit this money in an investment account which would be wired to New York City. Thus, NYSE member firms could crowd out local investment opportunities.

The advantage of a brokerage account versus a standard savings account in a bank, of course, is the opportunity for higher returns on investment and dividends. If these gains are spent locally, this could increase spending in the community and boost the local economy. Re-investing these returns into stocks, however, would have no effect on local businesses.

The last mechanism that will be considered in this chapter is the role of bucket shops. Positive local growth effects might be expected from these NYSE brokerage firms simply by displacing dubious bucket shops and improving the reputation of stock brokers. There is narrative evidence of this replacement occurring, but to my knowledge there has been no attempt to study this empirically due to the lack of data. An empirical analysis of this mechanism is an extension of this chapter that I plan to develop in the future.

3.5.2 Empirical Strategy

To estimate the relationships between these member firms and county-level outcomes, I use a straight-forward growth regression strategy, which is standard in the finance-growth literature (Barro, 1979). The variation over time in the number of NYSE member firms can be leveraged to estimate the overall growth relationship between the brokerage offices and the outcome variables of interest. Equation 3.2 is the preferred growth regression.

$$g_{c,t} = \alpha + \beta_1 \Delta \ln(\text{Offices}_{c,t-1}) + \beta_2 \ln(\text{Offices}_{c,t-2}) + \beta_3 \mathbb{1}_{\text{FirstOff}_{c,t-1}} + \beta_4 y_{c,t-1} + \Gamma X_{c,t-1} + \delta_D + \varepsilon_{c,t} \tag{3.2}$$

In the model, $g_{c,t}$ is the per capita growth rate of either manufacturing output or the number of farms per capita over the 1920s or the 1930s. The variable $\Delta \ln(\text{Offices}_{c,t-1})$ is the log change in the number of member-firm offices per 1,000 people in county c, where $t-1$ refers to the year from which the member-firm data is obtained, either 1922 or 1930 depending on the specification. Futhermore, $\text{Offices}_{c,t-2}$ is the number of member-firm offices two periods prior to the growth outcome variable. This controls for the level effect of the number of offices. The final variable of interest is $\mathbb{1}_{\text{FirstOff}_{c,t-1}}$. This is an indicator variable which turns on for those counties which receive their first ever office between $t-2$ and $t-1$. This captures any initial effects of stock market access and allows for the differentiation of the relationship between the first office in a county and additional brokerages offices beyond that. The initial level of the outcome variable is included in the regression as $y_{c,t-1}$. Because the initial level of a variable is negatively related to its growth rate, this coefficient is expected to be negative and significant. $X_{c,t-1}$ is a vector of county-level control variables such as the proportion of the population that is literate and that lives in an urban area. Finally, the regression also includes fixed effects at the Federal Reserve district level. The Federal Reserve district dummies control for regional differences as well as differences in monetary policy during this time period.

3.5.3 Results

The growth regression estimates, which are the central results of this project, are shown for manufacturing output growth leading up to the Great Depression, specifically the period 1920-1929, in Table 3.5. The data used in this regression are all adjusted for population and log transformed. Column (1) is the baseline regression before the office-related variables are included. Banks alone turn out to be insignificant in this regression. Column (2) shows the results with the first office indicator which is also insignificant on its own. This indicator takes a value of one if a county gained their first office between 1914 and 1922 and zero otherwise. Next, columns (3) and (4) differentiate between the effect of the first office and the number of offices overall. The first office indicator is positive and highly significant and the coefficient on the growth rate of offices is negative and significant at the 10% level. This is suggestive evidence of the first office reflecting financial sophistication and educational spillover and additional offices possibly having extractive effects on local capital.

Next, in the post-crash decade, 1930-1939, Table 3.6 shows that there is again a strong positive and significant effect of gaining a first office in the county as seen in columns (2) and (4). In column (4), the coefficient on the lagged number of offices, which is meant to capture the level effect of brokerage firms is positive and highly significant. This could be explained by the lag between opening offices and attracting customers which then leads to the educational spillover about ten years later. A final coefficient to note is that the coefficient on overall office growth is negative but insignificant. This contrasts the coefficient in Table 3.5, which is large and significant at the 10 percent level. This might be explained through the less rapid expansion of offices in the 1930s, and thus, a decreased extractive effect.

The next set of results focuses on the farming industry, specifically how the number of farms changes after a NYSE member firm opens in the county. Unlike the majority of sectors of the U.S. economy, the farming industry struggled throughout the 1920s when the high demand for agricultural goods fell after World War I (Federico, 2005). Table 3.7 shows that brokerage offices had a universally negative relationship with the amount of farms in a community. In column (2), the first office indicator is negative and highly significant. Column (4) additionally shows that the lagged number of offices is also negative and significant. This could be evidence of communities with member-firm offices providing farmers with more opportunities to get out of the farming industry.

In the next decade, 1930-1939, however, Table 3.8 shows that the negative relationship between

Table 3.5: Growth Effects on Manufacturing Output, 1920-1929

Dependent Variable: Manufacturing Output Growth Per Capita				
	(1)	(2)	(3)	(4)
First Office Indicator		0.057	0.098**	0.146***
		(0.047)	(0.042)	(0.047)
1922 Offices			-1.703	
			(1.514)	
1914 Offices				0.273
				(1.551)
Office Growth				-3.592*
				(2.148)
Banks	-0.090	-0.093	-0.093	-0.091
	(0.134)	(0.134)	(0.135)	(0.135)
Initial Mfgout	-0.109***	-0.109***	-0.110***	-0.109***
	(0.016)	(0.016)	(0.016)	(0.016)
Urbanshr	0.378***	0.360***	0.334***	0.551***
	(0.074)	(0.076)	(0.080)	(0.080)
Literateshr	1.100	1.095	1.001	0.990
	(0.787)	(0.788)	(0.787)	(0.789)
District Fixed Effects?	Yes	Yes	Yes	Yes
Observations	2,062	2,062	2,062	2,062
F-test			2.94*	3.27**
R-Squared	0.067	0.067	0.068	0.069

Note: Office data collected from 1914 and 1922 NYSE Directories. Bank data come from Jaremski and Wheelock (2019). Value of manufacturing output and remaining control variables from U.S. Census. All control variables have been log transformed and adjusted for population. Control variables are measured in 1920 unless otherwise specified. Robust standard errors are given in parentheses. *, **, and *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively. The F-statistic reflects the test of the null hypothesis that all coefficients on NYSE office-related variables are jointly zero.

the number of farms and brokerages dissipates. The only office-related coefficient that is significant in Table 3.8 is the lagged number of brokerage firms in column (4). This again could reflect a lagged educational spillover that takes time to manifest into real effects. This period also coincides with when New Deal policies sought to help farmers by propping up the prices of agricultural goods (Federico, 2005). Taken together, these two tables suggest that the more financially sophisticated farmers may have left the farming industry in the 1920s when there were other opportunities in the booming economy. By the 1930s, however, this exit strategy was unavailable to them as the economy bottomed out in the depths of the Great Depression.

Table 3.6: Growth Effects on Manufacturing Output, 1930-1939

Dependent Variable: Manufacturing Output Growth Per Capita				
	(1)	(2)	(3)	(4)
First Office Indicator		0.094***	0.058	0.150***
		(0.033)	(0.038)	(0.044)
1930 Offices			2.282*	
			(1.332)	
1922 Offices				5.500***
				(1.922)
Office Growth				-0.896
				(1.247)
Banks	0.194	0.204	0.237*	0.225*
	(0.136)	(0.136)	(0.136)	(0.136)
Initial Mfgout	-0.084***	-0.086***	-0.085***	-0.083***
	(0.015)	(0.015)	(0.015)	(0.015)
Urbanshr	0.229***	0.193**	0.160***	0.070
	(0.071)	(0.069)	(0.075)	(0.079)
Literateshr	2.321**	2.273**	2.707**	2.179**
	(1.009)	(1.010)	(1.011)	(1.007)
District Fixed Effects?	Yes	Yes	Yes	Yes
Observations	1,813	1,813	1,813	1,813
F-test			5.53***	5.98***
R-Squared	0.054	0.055	0.056	0.057

Note: Office data collected from 1922 and 1930 NYSE Directories. Bank data come from Jaremski and Wheelock (2019). Value of manufacturing output and remaining control variables from U.S. Census. All control variables have been log transformed and adjusted for population. Control variables are measured in 1930 unless otherwise specified. Robust standard errors are given in parentheses. *, **, and *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively. The F-statistic reflects the test of the null hypothesis that all coefficients on NYSE office-related variables are jointly zero.

3.5.4 Discussion

It should be noted that there is no tight identification strategy presented here, and thus, unfortunately, these results do not lend themselves to a causal interpretation. Partners at these stock brokerage firms likely selected cities to expand their branches based on specific criteria. While these results are only suggestive about the possible effects of NYSE member firms, they do show that these brokerage offices are important at the local level. Whether they are a proxy for a broader measure of financial connectedness in a community (such as Hartford) or directly take away savings that would otherwise be bound for a bank savings account, they illustrate that firm offices are important in the community.

It would be helpful to analyze these counties before they were affected by the establishment of

Table 3.7: Growth Effects on Farms, 1920-1929

Dependent Variable: Farm Growth Per Capita				
	(1)	(2)	(3)	(4)
First Office Indicator		-0.073***	-0.052*	-0.081**
		(0.026)	(0.028)	(0.034)
1922 Offices			-1.137	
			(0.796)	
1914 Offices				-2.343**
				(0.960)
Office Growth				0.022
				(1.121)
Banks	-0.002	-0.001	-0.002	-0.003
	(0.043)	(0.014)	(0.043)	(0.043)
Initial Farms	-0.009	-0.010	-0.013	-0.012
	(0.014)	(0.014)	(0.014)	(0.014)
Urbanshr	-0.311***	-0.291***	-0.301***	-0.288***
	(0.053)	(0.054)	(0.056)	(0.057)
Literateshr	-0.818***	-0.808***	-0.790***	-0.786***
	(0.292)	(0.292)	(0.292)	(0.292)
District Fixed Effects?	Yes	Yes	Yes	Yes
Observations	2,464	2,464	2,464	2,464
F-test			4.42**	4.15***
R-Squared	0.160	0.160	0.160	0.160

Note: Office data collected from 1914 and 1922 NYSE Directories. Bank data come from Jaremski and Wheelock (2019). Number of farms and remaining control variables from U.S. Census. All control variables have been log transformed and adjusted for population. Control variables measured in 1920 unless otherwise specified. Robust standard errors are given in parentheses. *, **, and *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively. The F-statistic reflects the test of the null hypothesis that all coefficients on NYSE office-related variables are jointly zero.

a brokerage firm to better understand which mechanisms might be most prevalent. Unfortunately, the decadal frequency of the U.S. Census data makes this difficult to pin down. Table 3.16 in the Appendix makes an attempt at studying whether gaining a future office has an effect in the decade prior to its establishment. A significant coefficient would suggest that the brokerage firms select certain cities based on their output growth potential, and the brokerages are not causing this effect. However, columns (1) and (2) show that gaining an office in the next period is not significantly related to current manufacturing output growth. It only becomes significant in the period when the brokerage is established. This provides further evidence that the establishment of the offices themselves are having a direct effect and not simply capturing something unique about the counties selected.

Table 3.8: Growth Effects on Farms, 1930-1939

Dependent Variable: Farm Growth Per Capita				
	(1)	(2)	(3)	(4)
First Office Indicator		-0.021	-0.020	-0.046
		(0.018)	(0.019)	(0.028)
1930 Offices			-0.528	
			(0.534)	
1922 Offices				-1.271**
				(0.624)
Office Growth				0.238
				(0.940)
Banks	0.017	0.015	0.018	0.020
	(0.028)	(0.028)	(0.029)	(0.029)
Initial Farms	-0.094***	-0.095***	-0.099***	-0.098***
	(0.020)	(0.020)	(0.021)	(0.020)
Urbanshr	-0.159**	-0.151**	-0.130**	-0.114*
	(0.064)	(0.063)	(0.065)	(0.065)
Literateshr	0.727	0.734	0.713	0.690
	(0.462)	(0.463)	(0.464)	(0.463)
District Fixed Effects?	Yes	Yes	Yes	Yes
Observations	2,474	2,474	2,474	2,474
F-test			1.42	2.04
R-Squared	0.158	0.158	0.159	0.159

Note: Office data collected from 1922 and 1930 NYSE Directories. Bank data come from Jaremski and Wheelock (2019). Number of farms and remaining control variables from U.S. Census. All control variables have been log transformed and adjusted for population. Control variables are measured in 1930 unless otherwise specified. Robust standard errors are given in parentheses. *, **, and *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively. The F-statistic reflects the test of the null hypothesis that all coefficients on NYSE office-related variables are jointly zero.

3.6 Conclusion

This chapter outlines several avenues through which local, NYSE member firms could affect the surrounding community. The empirical analysis suggests that the relationship between banks and brokerage firms evolved over the 1910s and 1920s from compliment services to competing entities for local funds. The growth results are consistent with member firms having an initial positive effect on output that eventually becomes extractive through over-expansion. Lastly, the farm results suggest brokerage firms may have opened up new opportunities outside of agriculture in the farm depression of the 1920s.

Employee stock purchase plan data which I described in Section 2.4 could prove useful in terms of determining whether brokerage firms are directly affecting local output or whether the timing of expansion coincides with the adoption of an ESOP in a local branch of a manufacturing company. In the future, the number of local manufacturing firm branches with ESOPs can be controlled for to rule out this channel. In terms of the educational spillover, ESOPs and brokerage firms may work together to increase the public's knowledge of the stock market and access to investment, and both of these mechanisms could be driving the results.

Another extension to consider is expanding the sample period into the 1940s and 1950s to understand the longer-term relationships between stock market access and the local economy as participation falls. I plan to continue collecting NYSE directories throughout the 1900s to further understand the patterns of expansion and additionally address the fall in participation following the Great Depression. Eventually, I also want to study the relationships between member firms and ESOPs on a more micro-level. That is, my goal is to address how households specifically are affected by the establishment of member firms in their community and the adoption of ESOPs by companies in their industry.

As mentioned previously, the results presented in this chapter are not intended to have a causal interpretation. Several potential avenues have been mentioned to construct a balanced sample that could lend itself to a causal analysis, such as studying regional branching and a subset of smaller counties. Currently, these various sub-samples have shown significant pre-trends and are still unbalanced. This is an aspect of the analysis I will continue to improve upon moving forward. I am hopeful that additional years of NYSE member firm data post-Great Depression may lend itself to a causal analysis as the motivators for a branch in the 1920s may not necessarily hold for the 1940s and so on. This degree of persistence may prove more suitable for a direct study of the effects of stock market access given the upheaval of the Great Depression.

At its current stage, this chapter brings up more questions than answers about the various possible ways stock market access might matter on a local level. However, the novel NYSE member firm location data along with the employee stock ownership information lay the groundwork for future research about the significance of stock market access in the 1920s and its role in the economic depression that followed. Future work expanding on the mechanisms described (and others) could paint a more complete picture of the role of these undoubtedly important financial institutions historically and potentially provide implications for current policies.

3.7 Appendix

Table 3.9: Treatment-Control Balance for Counties in the Northeast Region, 1929

	(1) Total	(2) NYSE=1	(3) NYSE=0	(4) Difference
Population	126,750	238,899	62,080	176,819**
	(15995)	(36887)	(10179)	(31015)
Density	646.52	1174.92	341.84	833.08*
	(194.48)	(437.66)	(170.46)	(400.70)
Female Share	0.495	0.498	0.493	0.005**
	(0.001)	(0.001)	(0.001)	(0.002)
Urban Share	0.454	0.647	0.343	0.303**
	(0.019)	(0.025)	(0.022)	(0.034)
Illiterate Share	0.007	0.005	0.008	-0.003**
	(0.000)	(0.001)	(0.001)	(0.001)
Farms	0.053	0.053	0.027	-0.041**
	(0.003)	(0.003)	(0.003)	(0.004)
Manufacturing Est. (per 1,000 people)	3.59	3.23	3.80	-0.57**
	(0.09)	(0.12)	(0.13)	(0.19)
Manufacturing Output	658.55	857.67	543.73	313.94**
	(29.65)	(53.32)	(31.41)	(57.86)
Crop Value	100.59	61.45	123.16	-61.71**
	(5.81)	(9.27)	(6.74)	(11.33)
Total Banks (per 1,000 people)	0.214	0.157	0.246	-0.214**
	(0.007)	(0.009)	(0.009)	(0.014)
Nat'l Bank Share	0.620	0.548	0.662	0.113**
	(0.015)	(0.020)	(0.019)	(0.029)
Deposits	247.69	290.86	222.79	68.06**
	(8.05)	(12.74)	(9.76)	(16.09)
Clearing House	0.185	0.430	0.044	0.185**
	(0.026)	(0.056)	(0.018)	(0.048)
ESOP	0.176	0.380	0.058	0.321**
	(0.026)	(0.055)	(0.020)	(0.049)
Observations	216	79	137	

Note: New York county is excluded from the main sample. Besides the population and density variables, all data are in per capita terms unless otherwise noted. The variables are measured in 1920. * and ** denote significance at the 5% and 1% levels, respectively.

Table 3.10: Treatment-Control Balance for Counties in the Midwest Region, 1929

	(1) Total	(2) NYSE=1	(3) NYSE=0	(4) Difference
Population	32,040	194,557	20,600	173,957**
	(3545)	(49618)	(17176)	(13245)
Density	69.55	527.45	37.32	490.13*
	(13.36)	(195.14)	(1.43)	(51.72)
Female Share	0.481	0.489	0.481	0.008**
	(0.000)	(0.002)	(0.000)	(0.002)
Urban Share	0.211	0.666	0.179	0.487**
	(0.005)	(0.027)	(0.004)	(0.017)
Illiterate Share	0.007	0.004	0.008	-0.015*
	(0.000)	(0.000)	(0.000)	(0.001)
Farms	0.110	0.043	0.115	-0.072**
	(0.001)	(0.004)	(0.001)	(0.004)
Manufacturing Est. (per 1,000 people)	2.193	2.756	2.152	0.605
	(0.03)	(0.09)	(0.03)	(0.13)
Manufacturing Output	236.73	853.30	191.88	661.42**
	(9.62)	(45.19)	(9.47)	(36.09)
Crop Value	339.08	157.85	351.83	-193.98**
	(6.23)	(18.43)	(6.35)	(24.4)
Total Banks (per 1,000 people)	0.773	0.338	0.804	-0.466**
	(0.013)	(0.032)	(0.013)	(0.050)
Nat'l Bank Share	0.201	0.211	0.200	0.011
	(0.005)	(0.014)	(0.005)	(0.020)
Deposits	268.90	365.34	262.11	268.90**
	(3.73)	(13.20)	(3.79)	(14.69)
Clearing House	0.083	0.647	0.043	0.604**
	(0.009)	(0.058)	(0.007)	(0.029)
ESOP	0.042	0.338	0.021	0.318**
	(0.006)	(0.058)	(0.005)	(0.023)
Observations	1,034	68	966	

Note: Besides the population and density variables, all data are in per capita terms unless otherwise noted. The variables are measured in 1920. * and ** denote significance at the 5% and 1% levels, respectively.

Table 3.11: Treatment-Control Balance for Counties in the Southern Region, 1929

	(1) Total	(2) NYSE=1	(3) NYSE=0	(4) Difference
Population	23,422	74,202	19,656	54,546**
	(871)	(9420)	(477)	(3097)
Density	53.97	237.11	40.38	196.73*
	(7.26)	(102)	(1.44)	(28.15)
Female Share	0.489	0.496	0.488	0.008**
	(0.000)	(0.002)	(0.000)	(0.002)
Urban Share	0.119	0.476	0.092	0.384**
	(0.005)	(0.027)	(0.004)	(0.017)
Illiterate Share	0.032	0.018	0.033	-0.015**
	(0.001)	(0.002)	(0.001)	(0.003)
Farms	0.121	0.063	0.125	-0.062**
	(0.121)	(0.063)	(0.125)	(0.004)
Manufacturing Est. (per 1,000 people)	1.79	2.016	1.77	-0.24
	(0.04)	(0.09)	(0.04)	(0.15)
Manufacturing Output	153.88	426.36	132.51	293.86**
	(9.62)	(45.19)	(9.47)	(36.09)
Crop Value	216.34	142.37	221.83	-79.46**
	(4.72)	(13.83)	(4.93)	(18.50)
Total Banks (per 1,000 people)	0.340	0.253	0.346	-0.093**
	(0.008)	(0.015)	(0.008)	(0.031)
Nat'l Bank Share	0.226	0.308	0.220	0.088**
	(0.006)	(0.017)	(0.007)	(0.025)
Deposits	125.59	280.18	114.13	166.06**
	(5.08)	(14.81)	(5.20)	(19.51)
Clearing House	0.059	0.441	0.031	0.409**
	(0.006)	(0.052)	(0.005)	(0.023)
ESOP	0.013	0.118	0.006	0.113**
	(0.003)	(0.034)	(0.002)	(0.012)
Observations	1,347	93	1,254	

Note: Besides the population and density variables, all data are in per capita terms unless otherwise noted. The variables are measured in 1920. * and ** denote significance at the 5% and 1% levels, respectively.

Table 3.12: Treatment-Control Balance for Counties in the Western Region, 1929

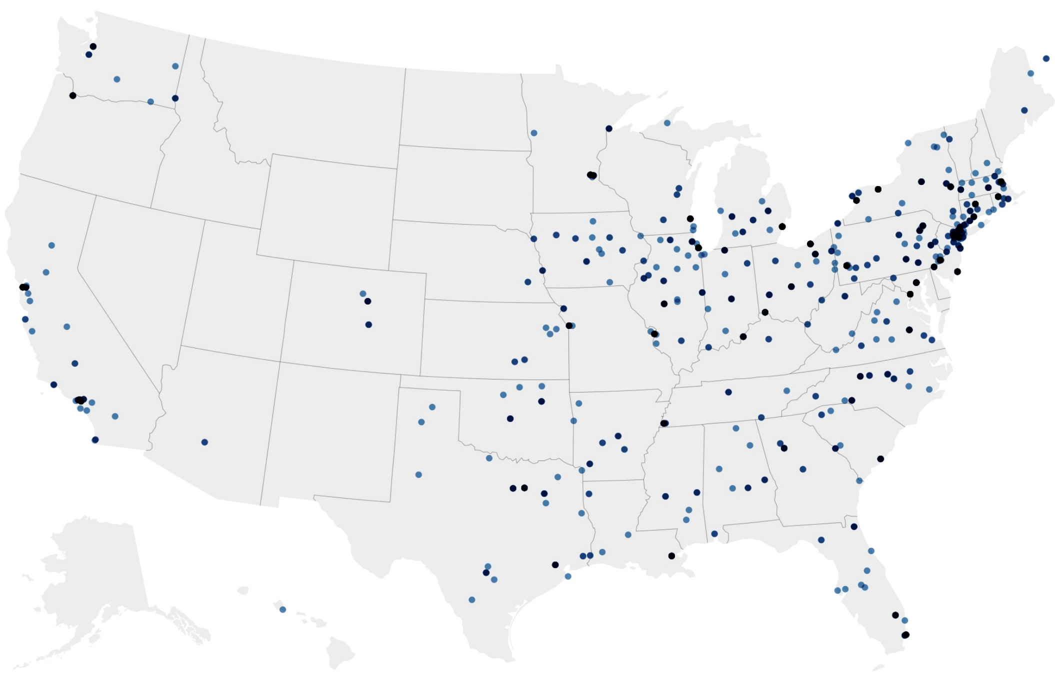
	(1) Total	(2) NYSE=1	(3) NYSE=0	(4) Difference
Population	23,007	210,567	14,150	196,416**
	(65319)	(56795)	(16737)	(12675)
Density	57.99	1084,01	9.54	1074.47**
	(34.09)	(732.27)	(0.91)	(154.84)
Female Share	0.449	0.476	0.447	0.029**
	(0.002)	(0.005)	(0.002)	(0.008)
Urban Share	0.189	0.699	0.165	0.533**
	(0.013)	(0.051)	(0.012)	(0.055)
Illiterate Share	0.009	0.003	0.009	-0.006
	(0.001)	(0.001)	(0.001)	(0.006)
Farms	0.095	0.032	0.098	-0.066**
	(0.003)	(0.003)	(0.007)	(0.003)
Manufacturing Est. (per 1,000 people)	2.674	3.216	2.647	0.569
	(0.08)	(0.19)	(0.08)	(0.36)
Manufacturing Output	234.86	470.48	223.08	247.41**
	(19.87)	(70.41)	(20.37)	(92.51)
Crop Value	254.03	142.24	259/30	-117.06*
	(11.41)	(39.09)	(11.74)	(54.73)
Total Banks (per 1,000 people)	0.556	0.202	0.573	-0.371**
	(0.018)	(0.029)	(0.019)	(0.086)
Nat'l Bank Share	0.329	0.346	0.328	0.019
	(0.012)	(0.033)	(0.013)	(0.059)
Deposits	243.68	465.99	233.19	232.80**
	(8.08)	(72.15)	(7.33)	(37.08)
Clearing House	0.082	0.647	0.056	0.592**
	(0.014)	(0.119)	(0.012)	(0.061)
ESOP	0.029	0.471	0.008	0.462**
	(0.009)	(0.125)	(0.005)	(0.034)
Observations	377	17	360	

Note: Besides the population and density variables, all data are in per capita terms unless otherwise noted. The variables are measured in 1920. * and ** denote significance at the 5% and 1% levels, respectively.

Figure 3.8: Ad for Brokerage Firm in *Nashville-Tennessean*, April 15, 1929

Source: Ten (1929)

Figure 3.9: Locations of NYSE Member Firms in 1933



Source: NYSE Archives

Table 3.13: Treatment-Control Balance for Full Sample, 1922

	(1) Total	(2) NYSE=1	(3) NYSE=0	(4) Difference
Population	33,870	226,551	22,842	203,709**
	(1852)	(28879)	(567)	(7286)
Density	102.9	1020.20	50.43	969.76**
	(16.05)	(261.2)	(6.89)	(68.7)
Female Share	0.482	0.494	0.481	0.013**
	(0.000)	(0.001)	(0.000)	(0.002)
Urban Share	0.184	0.664	0.157	0.507**
	(0.004)	(0.019)	(0.004)	(0.017)
Illiterate Share	0.019	0.006	0.019	-0.014**
	(0.000)	(0.001)	(0.000)	(0.002)
Farms	0.109	0.045	0.113	-0.078**
	(0.001)	(0.002)	(0.001)	(0.003)
Manufacturing Est. (per 1,000 people)	2.19	2.94	2.14	0.80**
	(0.026)	(0.072)	(0.027)	(0.109)
Manufacturing Output	232.81	719.06	203.15	515.92**
	(7.25)	(36.53)	(6.96)	(29.760)
Crop Value	255.38	111.97	263.59	-151.62**
	(3.63)	(10.90)	(3.73)	(15.79)
Banks (per 1,000 people)	0.509	0.256	0.523	-0.268**
	(0.007)	(0.018)	(0.008)	(0.032)
Nat'l Bank Share	0.259	0.344	0.254	0.090**
	(0.004)	(0.016)	(0.004)	(0.019)
Deposits	199.26	358.62	190.13	168.48**
	(3.14)	(13.70)	(3.14)	(13.55)
Clearing House	0.080	0.584	0.051	0.533**
	(0.005)	(0.039)	(0.004)	(0.020)
ESOP	0.037	0.398	0.016	0.381**
	(0.003)	(0.039)	(0.002)	(0.014)
Observations	2,974	161	2,813	

Note: New York county is excluded from main sample. Besides the population and density variables, all data are in per capita terms unless otherwise noted. The variables are measured in 1920. * and ** denote significance at the 5% and 1% levels, respectively.

Table 3.14: Treatment-Control Balance for Sub-Sample of Counties with less than 5 NYSE Branch Offices, 1929

	(1) Total	(2) NYSE=1	(3) NYSE=0	(4) Difference
Population	25,179	81,012	21,153	59,859**
	(628)	(4332)	(517)	(2249)
Density	53.84	172.33	45.30	127.03**
	(6.52)	(26.4)	(6.70)	(25.91)
Female Share	0.481	0.492	0.481	0.012**
	(0.000)	(0.001)	(0.000)	(0.001)
Urban Share	0.172	0.531	0.146	0.386**
	(0.004)	(0.016)	(0.004)	(0.015)
Illiterate Share	0.019	0.011	0.020	-0.009**
	(0.000)	(0.001)	(0.001)	(0.002)
Farms	0.111	0.053	0.115	-0.061**
	(0.001)	(0.003)	(0.001)	(0.003)
Manufacturing Est. (per 1,000 people)	2.17	2.52	2.14	0.38**
	(0.026)	(0.070)	(0.027)	(0.101)
Manufacturing Output	218.50	597.44	189.95	407.48**
	(7.05)	(36.22)	(6.74)	(26.49)
Crop Value	259.87	148.55	267.89	-119.34**
	(3.65)	(10.07)	(3.80)	(14.42)
Total Banks (per 1,000 people)	0.517	0.282	0.534	-0.252**
	(0.007)	(0.014)	(0.008)	(0.029)
Nat'l Bank Share	0.257	0.353	0.250	0.104**
	(0.004)	(0.014)	(0.005)	(0.017)
Deposits	195.26	298.36	187.82	110.54**
	(3.11)	(8.76)	(3.23)	(12.25)
Clearing House	0.066	0.437	0.039	0.398**
	(0.005)	(0.035)	(0.004)	(0.017)
ESOP	0.023	0.163	0.013	0.150**
	(0.003)	(0.026)	(0.002)	(0.011)
Observations	2,914	196	2,718	

Note: Besides the population and density variables, all data are in per capita terms unless otherwise noted. The variables are measured in 1920. * and ** denote significance at the 5% and 1% levels, respectively.

Table 3.15: Treatment-Control Balance for Sub-Sample of Counties with Only 1 NYSE Branch Office, 1929

	(1) Total	(2) NYSE=1	(3) NYSE=0	(4) Difference
Population	22,619	60,561	21,153	39,407**
	(542)	(4392)	(517)	(2768)
Density	45.50	104.61	45.30	59.31
	(6.70)	(13.40)	(6.47)	(35.16)
Female Share	0.481	0.492	0.481	0.011**
	(0.000)	(0.002)	(0.000)	(0.002)
Urban Share	0.157	0.452	0.146	0.306**
	(0.004)	(0.021)	(0.004)	(0.020)
Illiterate Share	0.019	0.013	0.020	-0.006*
	(0.000)	(0.002)	(0.001)	(0.003)
Farms	0.113	0.065	0.115	-0.050**
	(0.001)	(0.004)	(0.001)	(0.004)
Manufacturing Est. (per 1,000 people)	2.15	2.34	2.14	0.205
	(0.027)	(0.094)	(0.027)	(0.139)
Manufacturing Output	202.07	507.88	189.95	317.93**
	(6.86)	(49.79)	(6.74)	(35.28)
Crop Value	264.80	184.59	267.89	-83.31**
	(3.71)	(14.69)	(3.80)	(19.57)
Total Banks (per 1,000 people)	0.526	0.313	0.534	-0.22**
	(0.007)	(0.018)	(0.008)	(0.039)
Nat'l Bank Share	0.253	0.348	0.250	0.098**
	(0.004)	(0.019)	(0.005)	(0.023)
Deposits	190.98	272.62	187.82	84.80**
	(3.15)	(11.47)	(3.23)	(16.58)
Clearing House	0.016	0.085	0.013	0.072**
	(0.002)	(0.027)	(0.002)	(0.012)
ESOP	0.016	0.086	0.013	0.073**
	(0.002)	(0.027)	(0.002)	(0.012)
Observations	2,823	105	2,718	

Note: Besides the population and density variables, all data are in per capita terms unless otherwise noted. The variables are measured in 1920. * and ** denote significance at the 5% and 1% levels, respectively.

Table 3.16: Manufacturing Output Growth Robustness

Dependent Variable: Manufacturing Output Growth Per Capita				
	1920-1929		1930-1939	
	(1)	(2)	(3)	(4)
First Office Indicator	0.050		0.147*	
	(0.111)		(0.090)	
1930 Offices		-0.911		6.479***
		(6.146)		(2.515)
Banks	-0.074	-0.049	0.261*	0.263*
	(0.141)	(0.144)	(0.144)	(0.144)
Initial Mfgout	-0.116***	-0.064***	-0.087***	-0.086***
	(0.017)	(0.013)	(0.016)	(0.016)
Urbanshr	0.416***	0.020***	0.264**	0.281*
	(0.144)	(0.006)	(0.123)	(0.125)
Literateshr	0.590	0.744	2.014*	1.982*
	(0.826)	(0.841)	(1.073)	(1.074)
District Fixed Effects?	Yes	Yes	Yes	Yes
Observations	1,826	1,826	1,582	1,582
R-Squared	0.068	0.050	0.053	0.054

Note: Counties which had office in 1914, 1922, or 1929 are dropped from this sample. Office data collected from 1922 and 1930 NYSE Directories. Bank data come from Jaremski and Wheelock (2019). Value of manufacturing output and remaining control variables from U.S. Census. All control variables have been log transformed and adjusted for population. Control variables for columns (1) and (2) are measured in 1920 and columns (3) and (4) are measured in 1930 unless otherwise specified. Robust standard errors are given in parentheses. *, **, and *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

REFERENCES

(1929). Fenner & beane. *The Nashville Tennessean*.

77th Congress, First Session (1941). A bill to expedite the prosecution of the war effort. H.R. 6223(1507).

Almeida, H., Campello, M., Laranjeira, B., and Weisbenner, S. (2012). Corporate debt maturity and the real effects of the 2007 credit crisis. *Critical Finance Review*, 1(1):3–58.

Baker, H. (1932). *Statistical Analysis of Twenty Stock Purchase Plans, 1925-1932*. Edwards Brothers, Inc.

Barro, R. J. (1979). On the determination of the public debt. *Journal of Political Economy*, 87(5):940–971.

Baruch, B. M. (1921). *American Industry in the War: A Report of the War Industries Board*, chapter 1. Govt. Print. Off.

Bencivenga, V. R. and Smith, B. D. (1991). Financial intermediation and endogenous growth. *The Review of Economic Studies*, 58(2):195–209.

Benmelech, E., Frydman, C., and Papanikolaou, D. (2019). Financial frictions and employment during the great depression. 133(3):541–563.

Bernanke, B. S. (1983). Non-monetary effects of the financial crisis in the propagation of the great depression. *The American Economic Review*, 73(2):257–276.

Blakely, R. G. (1917). The war revenue act of 1917. *The American Economic Review*, 7(4):791–815.

Blasi, J., Conte, M., and Kruse, D. (1996). Employee stock ownership and corporate performance among public companies. *Industrial and Labor Relations Review*, 50(1):60–79.

Blasi, J. R. and Kruse, D. L. (1991). *The New Owners: The Mass Emergence of Employee Ownership in Public Companies and What It Means to American Business*. HarperCollins.

Bradsher, G. (2017). The council of national defense: Now a little known or appreciated world war i federal agency.

Brickley, J. A. and Hevert, K. T. (1991). Direct employee stock ownership: An empirical investigation. *Financial Management*, 20(2):70–84.

Carver, T. N. (1919). *War Thrift*, chapter 3. Oxford University Press.

Center for Research in Securities Prices (CRSP) (2022). Historical monthly price index. [Online; accessed 22-September-2022].

Congress of the United States (1910-1929). Annual report of the comptroller of the currency. *Government Printing Office*.

Congressional Research Service (2020). The defense production act of 1950: History, authorities, and considerations for congress. *CRS Reports*.

Cooper, R. and Haltiwanger, J. (1993). Automobiles and the national industrial recovery act: Evidence on industry complimentarities. *The Quarterly Journal of Economics*, pages 1043–1071.

Cortes, G. S. and Weidenmier, M. D. (2018). Stock volatility and the great depression. *The Review of Financial Studies*, 32(9):3544–3570.

Davis, E. (1933). *Employee Stock Ownership and the Depression*. Edwards Brothers, Inc.

Federal Reserve Board (1917-1929). Federal reserve bulletin. *Government Printing Office*.

Federico, G. (2005). Not guilty? agriculture in the 1920s and the great depression. *The Journal of Economic History*, 65(4):949–976.

Feenberg, D. and Miron, J. (2021). Nber macrohistory database.

Field, A. J. (2003). The most technologically progressive decade of the century. *The American Economic Review*, 93(4):1399–1413.

Fitzgerald, M. (2021). Robinhood appears to be benefiting from the trading controversy, seeing record app downloads. [Online; posted 1-February-2021].

Friedman, M. and Schwartz, A. (1993). *A Monetary History of the United States*. Princeton University Press.

Gay, K. and Gay, M. (1995). *World War I: Voices from the Past*, chapter 1 and 2. Twenty-First Century Books.

Greenwood, J. and Jovanovic, B. (1990). Financial development, growth, and the distribution of income. *The Journal of Political Economy*, 98(5):1076–1107.

Haines, M. R. (2010). Historical, demographic, economic, and social data: The united states, 1790-2002. *Inter-university Consortium for Political and Social Research*.

Haliassos, M. and Bertaut, C. C. (1995). Why do so few hold stocks? *The Economic Journal*, 105(432):1110–1129.

Hartford Courant (1928). Hincks bros. opening here next monday. *Hartford Courant*.

Hilt, E., Rahn, W., and Jaremski, M. S. (2020). When uncle sam introduced main street to wall street: Liberty bonds and the transformation of american finance. *NBER Working Paper Series*, (27703).

Hilt, E. and Rahn, W. M. (2016). Turning citizens into investors: Promoting savings with liberty bonds during world war i. *RSF: The Russell Sage Foundation Journal of Social Sciences*, 2(6):264–281.

Hochberg, Y. V. and Lindsey, L. (2010). Incentives, targeting, and firm performance: An analysis of non-executive stock options. *The Review of Financial Studies*, 23(11):4148–4186.

Hochfelder, D. (2006). ‘where the common people could speculate’: The ticker, bucket shops, and the origins of popular participation in financial market, 1800-1920. *Journal of American History*, 93(2):335–358.

Jaremski, M. and Wheelock, D. (2020). The founding of the federal reserve, the great depression, and the evolution of the u.s. interbank network. *Journal of Economic History*, 80:69–99.

Jayaratne, J. and Strahan, P. E. (1996). The finance-growth nexus: Evidence from bank branch deregulation. *The Quarterly Journal of Economics*, 111(3):639–670.

Jensen, M. C. and Meckling, W. H. (1976). Theory of the firm: Managerial behavior, agency costs and ownership structure. *Journal of Financial Economics*, 3(4):305–360.

Johansen, S. (1991). Estimation and hypothesis testing of cointegration vectors in gaussian vector autoregressive models. *Econometrica*, 59(6):1551–1580.

Jones, D. C. and Kato, T. (1995). The productivity effects of employee stock-ownership plans and bonuses: Evidence from japanese panel data. *The American Economic Review*, 85(3):391–414.

Kang, S. W. and Rockoff, H. (2015). Capitalizing patriotism: The liberty loans of world war i. *Financial History Review*, 22(1):45–78.

Kelly Tires: Commercial Truck Tires (2022). About kelly tires. [Online; accessed 15-August-2022].

Kim, E. H. and Ouimet, P. (2014). Broad-based employee stock ownership: Motives and outcomes. *The Journal of Finance*, 69(3):1273–1319.

Lazear, E. P. (1981). Agency, earnings profiles, productivity, and hours restrictions. *The American Economic Review*, 4(71):606–620.

Lee, J. and Mezzanotti, F. (2015). Bank distress and manufacturing: Evidence from the great depression.

Livingston, D. and Henry, J. B. (1980). The effect of employee stock ownership plans on corporate profits. *The Journal of Risk and Insurance*, 47(3):491–505.

Loualiche, E., Vickers, C., and Ziebarth, N. L. (2019). Firm networks in the great depression.

Margo, R. A. (1993). Employment and unemployment in the 1930s. *Journal of Economic Perspectives*, 7:41–59.

Maxey, D. (2002). Should history be my guide? *The Wall Street Journal*.

McKinnon, R. I. (1973). *Money & Capital in Economic Development*. The Brookings Institution.

Means, G. C. (1930). The diffusion of stock ownership in the united states. *The Quarterly Journal of Economics*, 44:561–600.

Menke, J. D. and Buxton, D. C. (2010). The origin and history of the esop and its future role as business succession tool. *The Menke Group*.

Miron, J. A. and Romer, C. D. (1990). A new monthly index of industrial production, 1884-1940. *The Journal of Economic History*, 50(2):321–337.

Mitchell, W. C. (1903). *A History of Greenbacks: With Special Reference to the Economic Consequences of Their Issue: 1862-65*. The University of Chicago Press.

Moody, J. (1929-1935). *Moody's Manual of Investments, American and Foreign: Industrials*. Analyses Publishing Co.

National Center for Employee Ownership (2021). Employee ownership by the numbers. [Online; accessed 15-March-2022].

National Industrial Conference Board, Inc. (1928). *Employee Stock Purchase Plans in the United States*. National Industrial Conference Board, Inc.

National Industrial Conference Board, Inc. (1930). *Employee Stock Purchase Plans and the Stock Market Crisis of 1929: A Supplement to 'Employee Stock Purchase Plans in the United States'*. National Industrial Conference Board, Inc.

New York Herald Tribune (1927). Fisher condemns workers investing in own companies. *New York Herald Tribune*.

New York Stock Exchange (1914, 1922, 1929, 1930, 1933). New york stock exchange directory. *NYSE Archives*.

New York Times (1923). Banton starts war, with federal help, on 116 bucket shops. *The New York Times*.

Ohanian, L. E. (1997). The macroeconomic effects of war finance in the united states: World war ii and the korean war. *The American Economic Review*, 87(1):23–40.

Olney, M. L. (2006). Saving and dissaving by 12,817 american households, 1917-1919.

Ott, J. C. (2011). *When Wall Street Met Main Street: The Quest for an Investors' Democracy*, chapter 3. Harvard University Press.

Rand McNally and Company (1920). January 1920: Introductory material. *Rand McNally Bankers Directory*.

Rappoport, P. and White, E. N. (1994). Was the crash of 1929 expected? *The American Economic Review*, 84(1):271–281.

Reinhard, C. M. and Rogoff, K. S. (2010). Growth in a time of debt. *American Economic Review P&P*, 100(2):573–78.

Richardson, G. and Troost, W. (2009). Monetary intervention mitigated banking panics during the great depression: Quasi-experimental evidence from a federal reserve district border, 1929-1933. *Journal of Political Economy*, 117(6):1031–1073.

Roos, C. F. (1937). *NRA Economic Planning*. The Principia Press, Inc.

Rousseau, P. L. (1998). The permanent effects of innovation on financial depth: Theory and us historical evidence from unobservable components models. *Journal of Monetary Economics*, 42:387–425.

Rousseau, P. L. and Watchel, P. (1998). Financial intermediation and economic performance: Historical evidence form five industrial countries. *Journal of Money, Credit, and Banking*, 30:657–678.

Saad, L. and Jones, J. M. (2021). What percentage of americans own stock? [Online; accessed 15-March-2022].

Schaetzl, R. (2021). Iron and steel: A trip inside a steel mill.

Shapiro, C. and Stiglitz, J. E. (1984). Equilibrium unemployment as a worker discipline device. *American Economic Review*, 74(3):433–444.

Sims, C. A. (1980). Macroeconomics and reality. *Econometrica*, 48(1):1–48.

Sims, C. A., Stock, J. H., and Watson, M. W. (1990). Inference in linear time series models with some unit roots. *Econometrica*, 58(1):113–144.

Sull, D. N. (1999). The dynamics of standing still: Firestone tire & rubber and the radial revolution. *The Business History Review*, 73(3):430–464.

Sutch, R. (2015). Financing the great war: A class tax for the wealthy, liberty bonds for all. *Berkeley Economic History Laboratory Working Paper 2015-09*.

Tax Foundation (2013). Historical income tax rates and brackets, 1862-2013.

Terrell, E. (2018). History of the us income tax.

The Joe I. Herbstman Memorial Collection of American Finance (2021). The collection.

The Library of Congress (1917). Every liberty bond is a shot at a u boat–fire your shot today–buy a liberty bond.

The National Iron & Steel Heritage Museum (2012). Use of iron and steel in war.

Toda, H. Y. and Yamamoto, T. (1995). Statistical inference in vector autoregression with possibly integrated processes. *Journal of Econometrics*, 66(1-2):225–250.

United States Treasury (1910-1929). Annual report of the secretary of the treasury on the state of the finances. *Government Printing Office*.

Vickers, C. and Ziebarth, N. L. (2018). United states census of manufactures, 1929-1935. *Inter-university Consortium for Political and Social Research*.

Warshow, H. T. (1924). The distribution of corporate ownership in the united states. *The Quarterly Journal of Economics*, 39:15–38.

White, E. N. (1990). The stock market boom and crash of 1929 revisted. *Journal of Economic Perspectives*, 4(2):67–83.