ESSAY IN APPLIED MICROECONOMETRICS

A Dissertation
Submitted to
the Temple University Graduate Board

In Partial Fulfillment
of the Requirements for the Degree
DOCTOR OF PHILOSOPHY

by

Nathan Blascak
May 2017

Examining Committee Members:

Dr. Douglas Webber, Advisory Chair, Department of Economics
Dr. Michael Leeds, Department of Economics
Dr. Dai Zusai, Department of Economics
Dr. Andrew Kish, External Member, Federal Reserve Bank of Philadelphia

ABSTRACT

This dissertation contains three essays applying microeconometric methods to unique panel datasets to answer economically-motivated research questions at the individual or firm level. The first essay uses microeconometric methods to analyze firm-level data on patenting. The second and third essays use anonymized individual-level credit bureau data to investigate the effects of a national health care policy on financial well-being and borrowing behavior.

The first chapter examines the effect of being the target of patent litigation on the efficiency of patenting-intense firms' knowledge production. To test the hypothesis, I construct a new, unique data set by linking firm-level patent, financial, and patent litigation data for any firm with more than 1000 lifetime U.S. patents from 2000-2010. Estimating a dynamic count data model via generalized method of moments (GMM), I find that a 1% increase in patent litigation leads to a small, but statistically significant 0.214% decrease in patent production for small firms relative to large firms.

The second chapter analyzes if the passage of the Affordable Care Act's (ACA) dependent coverage mandate in 2010 reduced financial distress for young adults. To test if increased health insurance coverage leads to improvements in financial well-being, I use a large, nationally representative database of anonymized consumer credit report information from the years 2009-2013. I employ a difference-in-differences research design to examine financial outcomes for young adults that were born in 1982-1983 and 1985-1986, with the latter cohort serving as a treatment group. I find that the mandate reduced debt in third-party collections by 3% and bankruptcies by 1.2 per 1000 people for young adults covered by the mandate. These effects are

stronger in counties that experienced higher rates of uninsurance and states that experienced higher rates of unemployment at the time the mandate was passed. The estimates also show that these reductions are transitory, as they diminish after an individual ages out of the mandate at age 26. These results are consistent with other recent research showing that the implications of health care policy extend beyond measures of physical health.

The third chapter, using similar data and empirical methodology employed by the second chapter, examines the effect of providing health insurance on consumption and borrowing choices of young adults. Using the ACA's 2010 dependent coverage mandate as an exogenous change in medical expenditure risk for those young adults gaining health insurance coverage, empirical estimates show that individuals affected by the mandate increased their use of credit cards, auto loans, and student loans, while also maintaining higher balances on these loans. Coefficient estimates also show that lenders provided affected individuals with higher credit limits and larger loans. These findings suggest that a reduction in medical expenditure risk due to insurance coverage may allow young adults to expand their consumption and borrowing and accept additional financial risks.

ACKNOWLEDGEMENTS

First, sincere thanks to my advisor Doug Webber, and my committee members Mike Leeds and Dai Zusai. Without their help, support, and advice, I certainly would not have made it to the end. I extend sincere gratitude to Andy Kish for agreeing to be my external reader.

I am especially grateful to Slava Mikhed, for his time, energy, dedication, patience, and feedback. Much of this research would not be complete without him.

Thanks to Bob Hunt, and the rest of the staff at the Payment Cards Center at the Federal Reserve Bank of Philadelphia. Your feedback and time were invaluable.

Thanks to Jeff Coons for funding the summer research grants. I am honored and grateful to be the initial recipient of the award.

Special thanks to James Bailey, for the help and inspiration to keep on going and staying interested in economics, and for pushing me forward on doing research.

Thanks to the staff in the Temple Economics Department, Temple Dissertation Seminar members, my friends, and other faculty members at Temple. The experience would have not been the same without you all.

Thanks to Thanh, for everything.

Finally, infinite thanks to my family, for supporting me through these years and for all of the love and support they have given me. None of this would have been possible without them.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

CHAPTER 1

DOES PATENT LITIGATION HAVE AN ADVERSE EFFECT ON PATENT GRANTS AT THE FIRM LEVEL?

1.1 Introduction

Patent litigation in the United States has increased significantly over the past twenty years. The number of cases filed annually has increased 337 percent, from 1187 cases filed in 1990 to 5189 in 2012, with average annual growth of 6.5 percent from 2000 to 2012.[1] Much focus has been placed on the utilization of patent rights and patent assertion entities (PAEs), commonly known as "patent trolls,"[2] and the effect they can have on innovating behavior of firms and overall social costs (Bessen & Meurer, 2013, 2014; Kiebzak, Rafert, & Tucker, 2016). A report by the Council of Economic Advisers in 2013 stated that over 60 percent of all infringement suits in the U.S. were brought by PAEs, triple the number from 2011(*Patent Assertion and U.S. Innovation*, 2013). However, while understanding the role of PAEs in the current

[1]As disclosed in the *Judicial Facts and Figures* report, which is published by the Administrative Office of the U.S. Courts system

[2]A patent assertion entity is an organization that owns a patent, but instead of using it to design or produce goods or services, engages in aggressive litigation to collect legal damanges or other royalties or fees.

patent litigation environment is important, there has been little focus on the effect of being targeted by a lawsuit more generally.

Theoretical research has shown that patent litigation can have negative impacts on firm value and productivity for innovating firms (Bessen & Meurer, 2006; Choi, 1988; Crampes & Langinier, 2002), empirical research on patent litigation has primarily focused on identifying the determinants of litigation, estimating the probabilities that certain patents will be litigated (Harhoff & Reitzig, 2004; Lanjouw & Schankerman, 2001, 2004), and how the costs of litigation affect the probability that a firm will patent in certain technology areas (Lerner, 1995). While understanding which patents are most at risk for litigation is of great interest, especially for innovating firms, little research has looked at litigation's effect on firm patenting output. I addresses this gap by utilizing the literatures on knowledge production to study how patent litigation affects a firm's innovation behavior, as measured by patent grants.

My empirical analysis is based on a unique data set comprised of the top 194 patenting-intensive public U.S. firms from 2000 to 2010. I merge data from three sources: patent data from the U.S. Patent and Trademark Office (USPTO), firm-level data from Compustat, and patent litigation data from Lex Machina, an intellectual property litigation database. I combine these three data sources to estimate a standard model of firm-level knowledge production as a function of current and past R&D expenditure, patent litigation, and firm characteristics.

To identify the causal impact of litigation on firm-level patent production, I exploit the cross-sectional and time variation in patent litigation for firms to estimate a dynamic, distributed lag count-data model. I account for endogeneity due to time-invariant, unobserved firm characteristics by using a standard, quasi-differencing transformation. To control for endogeneity from the lags of the dependent and independent variables, I estimate an instrumental variables (IV) model via generalized method of moments (GMM) using Arellano-Bond-styled instruments.

Estimation of the model presents a key result that supports the hypothesis that patent litigation is a source of inefficiency in patent production. For smaller firms, patent litigation has a negative long run effect, with an estimated patent elasticity of litigation of -0.214. Litigation has no significant effect on patent production by large firms. The asymmetry suggests that litigation costs are significant enough that large firms with more resources are better able to sustain them than small firms.

1.2 Institutional Details and Literature Review

1.2.1 Background on Patents

A patent is a document that gives an inventor the legal right to exclude others from using or selling an invention for a specified period. In the United States and Europe, this legal protection lasts up to 20 years, depending on the patent type.[3] This right grants the patentee a temporary government-sanctioned monopoly for the invention - which traditionally has been seen as an effective financial incentive for firms and inventors to innovate and spend money on R&D. The benefits of a patent are not restricted to the patenting entity, as detailed information regarding the invention must be disclosed (and subsequently made available to the public) in the application for the patent. This disclosure allows others to learn of the new invention and facilitates the diffusion of this new knowledge throughout the economy.

In the United States, patents can be broken down into three distinct categories, based on the type of innovation: a utility patent, which is given to the inventor of a new good, process, or improvement to an existing good or process; a design patent,

[3]The term for a utility patent is 20 years in the United States while the term for a *design* patent is 15 years

which is given to an inventor of a new and original design for a good/service; and a plant patent, which is given to an inventor or discoverer of a new and distinct type of plant. Utility patents are the most commonly issued, making up 92 percent of all issued patents in 2012. Both domestic and foreign entities are typically eligible to apply for patents of all types in a given country. In the United States, foreign-based entities had a 52 percent share of total patents granted in 2012.

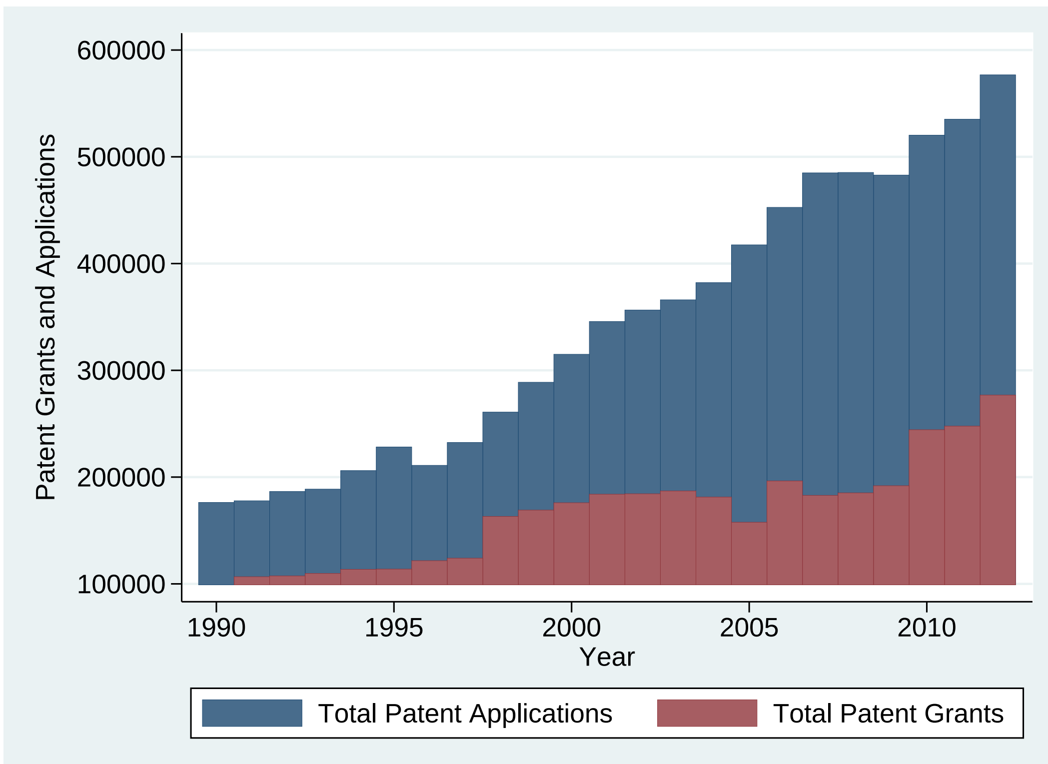


Figure 1.1: U.S. Patent Applications and Grants, 1963-2012. Source: USPTO

Since 1963, the first year that yearly patent data are available from the USPTO's Patent Technology Monitoring Team, both the number of patent applications and the number of patents granted have been increasing steadily, with patent applications growth averaging 5.5% since 1990. Numerous studies have attempted to explain this trend. Kortum and Lerner (1999) analyzed the surge in U.S. patents from 1985 to 1995 and determined that changes in the management of R&D activities and innovation was a primary determinant. Hall and Ziedonis (2001) analyzed patenting behavior in the semiconductor industry and found that the patenting surge there was in response to legislative changes. Other research has noted that changes in the patenting environment due to increases in the types of patentable 'inventions' (software, genetic material) and the increases of patenting by universities, due to the Bayh-Dole Act[4], have contributed to this trend.

1.2.2 Patent Litigation

If another inventor uses or engages in an activity that utilizes a patented invention without permission from the patentee, that inventor is guilty of patent infringement. In most countries, patent infringement is a civil matter, implying that patents are enforced through civil lawsuits. There are relatively few countries in the developed world where patent infringement is considered a criminal matter.[5]

The patent litigation process is lengthy and typically expensive. The process begins when the patent holder (the plaintiff) files a complaint in a U.S. district court. The alleged infringer (the defendant) has up to three months to answer the patentee's complaint, responding to each allegation. Typically, the infringing firm will utilize affirmative defenses, such as alleging non-infringement or patent invalidity and will assert any counterclaims.[6] The plaintiff has a maximum of three weeks to respond to

[4]The Bayh-Dole Act, also known as the Patent and Trademark Law Amendments Act, was passed in 1980. It permitted organizations (such as universities and for-profit institutions) that produced inventions with government funds to retain the title to those inventions. Prior to the act, there was no uniform Federal policy on how to assign patent rights when non-government institutions patented inventions with government funds (U.S. General Accounting Office, 2003).

[5]France and Austria are two prominent examples.

[6]For example, the defendant could seek a declaratory judgement that the patent in question is invalid.

any counterclaims by the defendant. After complaints and counterclaims have been filed, both parties meet to discuss the case schedule. The court then conducts a case management conference, where the judge officially sets the schedule for the case.

Once the schedule for the case has been set, both parties enter the discovery stage, which is the most lengthy and expensive stage of the patent litigation process. The discovery stage can be divided into two parts: fact discovery and expert discovery. Fact discovery consists of gathering and exchanging documents, interrogatories,[7] admissions, depositions, and subpoenas. Firms commonly exchange tens of thousands of pages of documents during fact discovery. Expert discovery consists of depositions of opinions and reports from expert witnesses on the infringement and damages. As the value of the patent at risk increases in value, so does the cost of litigation. According to the AIPLA's 2013 annual survey, litigation costs averaged $350,000 by the end of the discovery stage when less than $1 million was at risk and $3 million when more than $25 million was at risk.

If found guilty of patent infringement, the infringer is liable for the larger of either a reasonable royalty[8] or compensation of lost profits that result from the infringement. If it is determined that the guilty party deliberately infringed (known as willful infringement), the infringed party is entitled to recover lost profits and punitive damages, which can be up to three times the size of actual damages. According to the study done by PricewaterhouseCoopers in 2012, median damage awards have ranged from $1.9 million to $16.5 million from 1995 to 2012.

[7]Interrogatories are a set of written questions that a party must answer.

[8]Also known as RAND (reasonable and non-discriminatory) or FRAND (fair, reasonable, and non-discriminatory) royalties. U.S. law does not explicitly state the definition of a reasonable royalty.

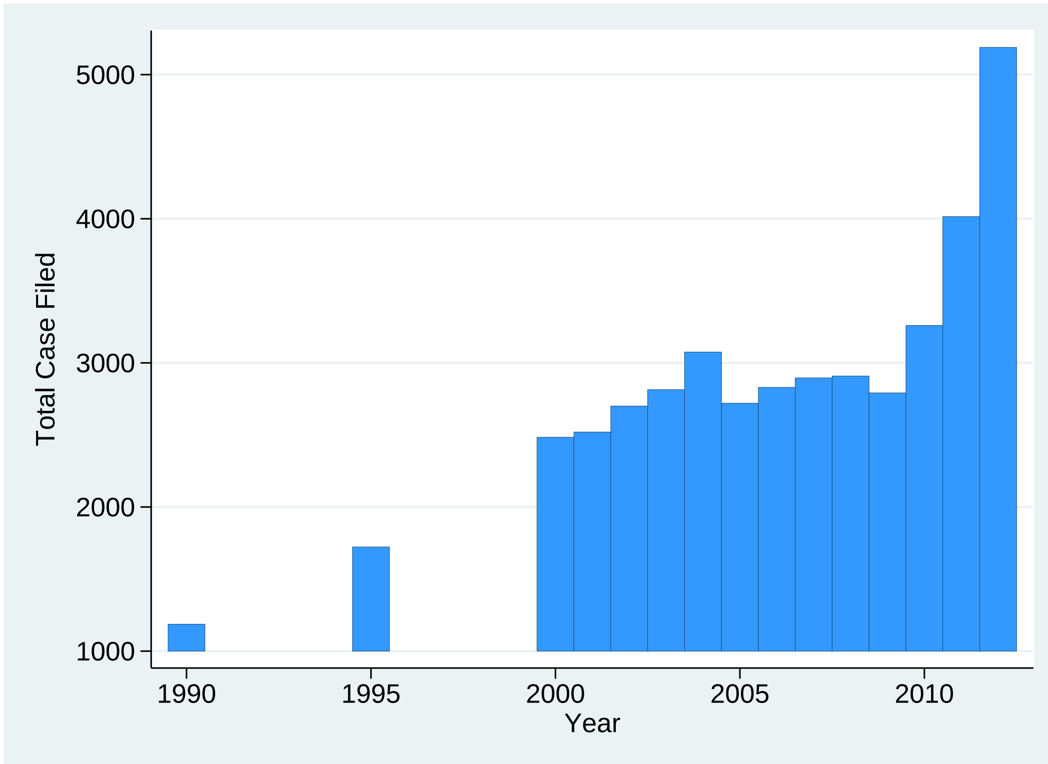


Figure 1.2: U.S. Patent Litigation Cases Filed, 1990-2012. Source: Judicial Facts and Figures

As can be seen in Figure 1.2, the trend in the number of patent cases filed, as measured by the Judicial Facts and Figures has increased from 1187 cases filed in 1990 to 5189 in 2012, coinciding with the increase patent applications and grants over the same time period.

1.2.3 Previous Literature

Lerner (1995) combined data from the United States Patent and Trademark Office (USPTO) and Venture Economics to analyze the propensity to patent for 419 biotechnology firms from 1970 to 1992. Using past litigation experience and capital expenditures as proxies for the cost of patent litigation, he estimated probit regressions and found that increases in the cost of litigation led firms to reduce their patenting in technology subclasses occupied by rival firms. This was due in part to the substantial indirect costs incurred by firms when entering the litigation process.

Lanjouw and Schankerman (2001) used case filing data from the USPTO and U.S. districts courts and analyzed the effect of patent and firm characteristics on litigation rates and probabilities of lawsuits. Estimating probit regressions, they found a significant statistical relationship between patent characteristics and the levels of exposure to the risk of getting involved in litigation. Their results suggested these probabilities differ greatly across different firm and patent characteristics. In particular, they identified that litigation probabilities differ for "low-value" and "high-value" patents.

Lanjouw and Schankerman (2004) studied the determinants of patent litigation and settlements from 1978-1999 using data from LitAlert and the USPTO. Also estimating probit regressions, they identified key firm attributes that have a significant effect on the probability that a firm's patent becomes involved in litigation. These attributes include the size of the firm, the nationality of ownership, and the size of the firm's patent portfolio. They showed that patents of smaller firms and firms with small patent portfolios are at a higher risk of litigation compared to larger firms.

Harhoff and Reitzig (2004) also studied the determinants of patent opposition for European patents in biotechnology and pharmaceutical fields. Using European Patent Office data from 1978-2001, they found that variables correlated to patent value are important predictors of the probability of opposition.

Hall and Ziedonis (2007) conducted a firm-level analysis and examined factors that affect the probability that firms in the semiconductor industry would be involved in patent litigation. Using data on 136 firms from 1973-2001, they estimated a number of different probit models and found that the risk of becoming a target of patent litigation has increased over the period of their sample. Specifically, they found that the probability that a firm will be sued by a non-rival firm increased in the final decade of their sample period.

1.3 Data

1.3.1 Constructing the Dataset

The data used in this paper combine three different data sets: data on patent applications and patent grants were obtained from the United States Patent and Trademark Office (USPTO); firm level data obtained from Compustat via the University of Pennsylvania's Wharton Research Data Services; and data on patent litigation come from Lex Machina, an intellectual property litigation data service.[9] Because firms can obtain patents under their own name or under the names of their subsidiaries, the USPTO data is highly disaggregated. This fact results in there being over 200,000 different patenting entities for utility patents alone. The USPTO's data also do not consider firm mergers or transference of patent ownership.

While the combination of USPTO and Compustat data is standard in the patent literature regarding patents.What makes the dataset used in the paper unique is the inclusion of data from the Lex Machina website, an intellectual property litigation database. Lex Machina uses a proprietary language processing and machine learning engine that extracts patent information and documents from the Public Access to Court Electronic Records (PACER) website, the websites of all U.S. District Court websites, the website of the US International Trade Commission (USITC), and the USPTO's website. For each firm in the dataset, I attempted to collect the number of patent litigation cases listing the firm as a defendant. Aggregating the data from Lex

[9]Lex Machina was originally a joint operation between Standford University's Computer Science Department and Law School (http://lexmachina.com)

Machina proved labor intensive. Because case data from the Lex Machina website can be retrieved only one firm at a time, proper corporate hierarchy, along with the timing of mergers and acquisitions, must be known prior to retrieving the relevant cases for each firm, complicating the process of matching litigation to firms.

To exploit both the cross-sectional and time series variation present in the three data sources, I created a panel dataset. Since I am interested in firms who are active in the innovation and patenting process, the initial sample consisted of the 487 patenting entities in the Extended Year Set - All Technologies Report issued by the Patent Technology Monitoring Team (PTMT) of the USPTO. The report contains the patent count for any patenting entity that has been granted at least 1000 patents from 1969-2012. Universities, government institutions, and privately held firms were dropped from the sample, along with foreign firms that did not have matching firm-level data from Compustat.[10] Yearly patent grant and patent litigation totals were summed to the appropriate parent firm in the proper year. To accurately reflect patenting performance and quantity of patent litigation of firms, subsidiaries were matched with their respective parent firms using the LexisNexis Corporate Affiliations database. The timing of mergers and acquisitions were obtained from Bloomberg Businessweek and Reuters, so firms that had less than two full years of data and firms that did not have any corresponding Compustat data, were dropped from the sample.

Merging the USPTO, Compustat, and Lex Machina data yielded an unbalanced panel dataset of 194 firms from 2000-2012. Because Compustat data goes back to 1950 and USPTO data goes back to 1970, pre-sample data on firm-level variables and patents granted is available, allowing for minimal data loss when using lags. The

[10]Compustat has a separate database for international firms called Compustat Global. However, reported variable values are not in dollars. It currently beyond the scope of this paper to calculate and apply the proper exchange rates and price deflators for international firms in multiple countries.

specific range of years was chosen because the Lex Machina database currently has case data that goes back only to 2000 and USPTO data goes only to 2012. The lack of pre-sample data for patent litigation is problematic as it becomes necessary to remove observations from the sample as lagged values of patent litigation are included in the model specifications.

1.3.2 Data Truncation

A standard problem with the use patent data is that patent counts are truncated. This is due to the lag between the time a patent application is received by the USPTO and is eventually granted. Since the lag, also known as patent pendency, is fairly lengthy (averaging 2.67 years from 2004-2010), this implies that patent counts at the end of the dataset will necessarily be biased downwards. This is because reported patent counts for each firm are only a fraction of the patents they will eventually be granted over time. To correct the data for truncation bias, I employ the same method used by Hall et al. (2005) and calculated weight factors to correct for this truncation using the empirical application-grant lag distribution from the NBER patent data project.[11]

1.3.3 Data Summary

Summary statistics for the dataset appear in Table 1.1 below. All data are yearly. As can be seen in the table, the mean number of patents granted is approximately 298 per year. This number is relatively high due to the subsample of data available from the USPTO website. The minimum and maximum number of patents in the

[11]The NBER's patent data project contains individual U.S. patent data from 1976-2006, including information on the initial assignee, number of citations, the year the patent was applied for, and the year the patent was granted.

dataset highlight the variability in patenting behavior by firms over the time period. For example, IBM applied for, and was eventually granted, 7099 patents in 2008. The existence of firm-year observations of zero patents can be attributed to either new firms entering the sample or to older firms who no longer patent.

Table 1.1: Summary Statistics

Variable	Mean	Median	Std Dev	Min	Max
Patents Granted*	352.73	154	565.77	0	8435
Patent Cases Filed Against	2.25	1	4.12	0	52
R&D Expenditure**	1544.05	618.76	2046.65	2.12	14745.72
Employment***	65.89	38.52	80.46	.257	484
No. obs = 2021					

*by year of application, **in millions, ***in hundreds

R&D data was deflated using the GDP deflator indexed to 2012 prices.

Data for both patent grants and patent litigation cases are heavily skewed to the right, with the mean greater than the median by more than 100% for each. This is to be expected, as both are count variables with a large number of zero values.

1.4 Empirical Methodology

To identify the causal effect of patent litigation on firm patenting performance, I propose a model that accounts for the count-data nature of patents while addressing endogeneity issues that arise from including patent litigation as a regressor.

1.4.1 Modeling Patent Data

The starting point for empirically modeling patent production is based on work by Pakes and Griliches (1984) and Hausman, Hall, and Griliches (1984), which typically specifies a Poisson regression model with mean specification:

$$y_{it} = exp(X_{it}\beta) + \epsilon_{it} \tag{1.1}$$

The specification of a non-linear mean in the standard model ensures the non-negativity of the fitted count variable, $\hat{y}_{it}$. It is also common that these specifications use a long distributed lag of R&D expenditure to model how firm inputs over time affect firm innovation. A more parsimonious specification of patenting performance can be modeled by specifying current patent production a function of both R&D expenditure and past patenting performance. It is reasonable to assume that success in patenting in the past affect patenting in the future. As shown in Pakes and Griliches (1984) and Hausman et al. (1984), including multiple lags of R&D expenditure induces multicollinearity and/or truncation issues, leading to a U-shaped distributed lag structure,[12] which makes estimating such models problematic. By specifying a dynamic equation of patent production, I avoid this problem, while being able to specify a more parsimonious model.

For the dynamic model, I specify an exponential feedback model with multiplicative fixed effect α_i:

$$y_{it} = \alpha_i \exp(\gamma y_{it-1} + X_{it}\beta) + \epsilon_{it} \tag{1.2}$$

where y_{it} is the number of patents granted to firm i that were applied for in year t and y_{it-1} is the lagged patent term. X includes the following variables: logged R&D spending in the current period and its first lag,[13] a measure of firm size, as measured by the log of number of employees, the log of the number of patent lawsuits

[12]This U-shape occurs when the coefficients on the initial lagged terms increase in magnitude, but become smaller as the lags go farther back in time.

[13]R&D expenditure is deflated by the GDP deflator in 2012 dollars.

and two subsequent lags,[14] and year fixed effects. Specifying a multiplicative fixed effect, instead of an additive one, still yields an interpretation of the fixed effect being an intercept shift. However, this specification relies upon the assumption that conditional mean is exponential in nature.

1.4.2 Modeling Patent Litigation

As previously mentioned, Lerner (1995) details the "substantial indirect costs" associated with being involved in patent litigation. These indirect costs may be associated with the discovery process, where researchers and managers may spend significant time giving depositions or answering interrogatories. Firms may decide to invent around a certain innovation or change the technology subclass they patenting in order to settle a patent litigation suit or to avoid future suits. Firms may also receive adverse publicity in the press or additional monitoring from rival firms after a patent infringement suit has been raised. These types of indirect costs typically may not cause distortions in the accounting cost of performing innovative activities, but instead cause resources to be allocated or used inefficiently.

Since these indirect costs are not measurable, I use the number of patent litigation suits as a proxy for the indirect costs the firm experiences. While there might exist learning-by-doing in the patent litigation process, leading to more efficient handling of additional cases, increases in the amount of patent infringement suits should lead to higher total indirect litigation costs. To represent this, I assume that the inefficiency enters the knowledge production function additively:

$$y_{it} = \alpha_i \exp(\gamma y_{it-1} + \psi_1 ln(cases_{it}) + \psi_2 ln(cases_{it-1}) + ... + X_{it}\beta) + \epsilon_{it} \tag{1.3}$$

[14] Because the number of cases for a firm in a given year can be zero, I added '1' to the value before taking the log.

It is easily seen in Equation (1.3) that if firm i experiences no litigation, there is no effect on patent production, and as a firm experience more litigation, the greater the impact the inefficiency has on reducing patent production.

1.4.3 Identification and Estimation

There are two primary challenges in obtaining causal estimates of the effect of patent litigation on patent production. First, it is highly likely that there is a simultaneous relationship between patent grants and patent litigation. By patenting more, firms may increase the probability that other firms will infringe on their patents. Second, unobservable firm-level factors may be correlated with both patents and litigation. For example, effort or propensity to file cases, differences in research effort, differences in firms ability to convert R&D expenditures into useful innovations, or differences in managerial ability may affect both patent production and litigation.

Additionally, the assumption that the regressors are strictly exogenous (i.e., current period shocks affect both past and future values of the independent variables) almost certainly does not hold in this context. Therefore, a more flexible assumption must be made regarding the correlation of shocks to the independent variables in the model. Specifically, I relax the strict exogeneity assumption and instead assume that the independent variables are predetermined (i.e., only past shocks affect the independent variables). While this assumption is more realistic, it does not allow for Equation (1.3) to be estimated via standard Poisson or negative binomial regression models. This is because these models require strict exogeneity of the independent variables for consistent estimation (Cameron & Trivedi, 2013; Salomon & Shaver, 2005).[15]

[15]This problem also rules out the generalized estimating equations (GEE) approach introduced by Liang and Zeger (1986).

To properly estimate the effect of patent litigation on patent production, I use the non-linear GMM methodology outlined in Wooldridge (1997). The relaxation of the strict exogeneity assumption of the independent variables (i.e., that the independent variables are predetermined) assumes that for variables in X_{it}: $E[X_{it}\epsilon_{it-s}] \neq 0, s \geq 0$ and $E[X_{it}\epsilon_{it-s}] \neq 0, s \geq 0$.[16] Taken together, these assumptions imply the following:

$$E[\epsilon_{it}|X_{it}, X_{it-1}, \dots X_{i1}] = 0, \quad t = 1, ..., T \tag{1.4}$$

Equation (1.4) is commonly referred to as the weak exogeneity assumption, as it assumes that after conditioning on present and past values of the independent variables x_{it}, the expected value of the error term is zero.[17]

Addressing the potential endogeneity issues listed above, I first transform Equation (1.3) using a quasi-differencing transformation initially suggested by Chamberlain (1992) to control for any time-invariant firm-level fixed effects:[18]

$$g_{it}(\beta) \equiv \frac{\lambda_{it-1}}{\lambda_{it}} y_{it} - y_{it-1} \tag{1.5}$$

where $\lambda_{it} = \exp(\gamma y_{it-1} + \psi_1 ln(cases_{it}) + \psi_2 ln(cases_{it-1}) + ... + X_{it}\beta)$. This type of transformation is necessary because the specification is assume to have multiplicative fixed effects. To address the simultaneity of patent production and litigation, I estimate Equation (1.5) via an instrumental variables approach. Since the regression equation is quasi-differenced, the pool of available instrumental variables consists of

[16] Additionally, the model assumes $E[\alpha_i \epsilon_{it}] = 0$ and $E[\epsilon_{is}\epsilon_{it}] = 0, s \neq t$.

[17] This assumption generates the *sequential moment restrictions* as defined by Chamberlain (1992).

[18] A random effects specifications would be inappropriate in this case because the sample is non-random collection of patent-intensive firms and there is some concern that the unobserved idiosyncratic effects may be correlated with some of the regressors.

any of the level values of the right hand side variables since:

$$E[g_{it}(\beta)|\alpha_i, x_{it}, ..., x_{i1}] = 0 \tag{1.6}$$

$$E[x_{it} g_{it}(\beta)] = 0, \quad t = 1, ..., T-1 \tag{1.7}$$

where $g_{it}(\beta)$ is as defined in Equation (1.5). Equation (1.6) describes the moment condition based on the quasi-differencing. Equation (1.7) describes the orthogonality condition that results from the moment condition. The vector of instrumental variables $\mathbf{z_{it}}$ that satisfy the orthogonality conditions in Equation (1.7) can consist of current and lagged level values of the independent variables in X_{it}. Due to the weak exogeneity assumption of Equation (1.4), the list of available instrumental variables grows as the time dimension t increases.[19]

1.5 Results

Table 1.2 presents results from estimating the model of patent production. Columns (1) and (2) contain baseline pooled and fixed effect Poisson estimates. Coefficient estimates for the patent litigation variables are problematic, as they are all positive with the only the first lag being statistically significant at the 95% level. These results suggest that increases in patent litigation are positively correlated with increases in patent production. This result is likely due to the inconsistent estimation due to the dynamic nature of the model (Cameron & Trivedi, 2013).

Columns (3) and (4) of Table 1.2 report results from the just-identified and over-identified estimation via GMM. Robust standard errors corrected for clustering along the firm level are reported in parentheses for the two GMM estimations. The just-

[19] This is analogous to the linear model of Arellano and Bond (1991).

Table 1.2: Estimation Results

Coefficient	(1) Pooled-Poisson	(2) FE Poisson	(3) Just-Identified	(4) Over-Identified
$patents_{t-1}$	0.0002	0.0002	0.0002*	0.0001***
	(0.0002)	(0.0002)	(0.0001)	(0.0000)
$ln(R\&D)_t$	0.3393***	0.3368***	-0.3679	0.1768
	(0.0917)	(0.0782)	(1.4048)	(0.2018)
$ln(R\&D)_{t-1}$	0.0272	0.0263	0.1060	0.1826***
	(0.0750)	(0.0685)	(0.0841)	(0.0609)
$ln(cases)_t$	0.0081	0.0077	-0.1027	-0.0288
	(0.0241)	(0.0265)	(0.2334)	(0.0553)
$ln(cases)_{t-1}$	0.0488**	0.0486**	-0.0356	-0.0062
	(0.0213)	(0.0239)	(0.1600)	(0.0370)
$ln(cases)_{t-2}$	0.0016	0.0015	-0.0419	-0.0180
	(0.0181)	(0.0193)	(0.0674)	(0.0219)
$ln(employees)$	0.1895	0.1931*	0.5090	0.0771
	(0.1199)	(0.0996)	(1.4787)	(0.1345)
$constant$	2.4404***			
	(0.5593)			
$ln(alpha)$	-0.0986			
	(0.1736)			
Year Dummies	Yes	Yes	Yes	Yes
No. of Observations	1577	1546	1383	1343

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.01

identified case was estimated using the one-step GMM estimator. Because two lags of patent litigation are included in the specification and three lags are included as instruments, the sample is trimmed by three years. Overall, coefficients from the just-identified case are imprecisely estimated. Contemporaneous and lagged patent litigation coefficients are of the expected sign, but none are significantly different from zero. While the coefficient value on the lagged patent term is in line with those found in the literature, the values of the coefficients on the $ln(R\&D)$ and $ln(employees)$ variables differ significantly. These results from the just-identified case may be due to the underutilization of the information from available moment conditions. Blundell, Griffith, and Windmeijer (2002) have also shown that insignificant GMM results could be due to the weakness of the instruments.

The GMM estimation for the over-identified case is presented in column (4) of Table 1.2. Estimation used the two-step GMM estimator. The over-identified model uses 22 instruments against 14 regressors, so there are eight over-identifying restrictions. Because there are two lags of the patent litigation variable, two additional years of data are lost and seven year dummies are included. To test for model adequacy, Hansen's J statistic is calculated and returns a value of 6.31362 with 8 degrees of freedom. Thus, I fail to reject the null hypothesis of model adequacy and determine that model's moment conditions match the data well. The lagged patent term is positive and statistically significant at the 99% level. Though the value is small, its magnitude is consistent with results from Cameron and Trivedi (2013). Coefficients on both contemporaneous and lagged logged R&D expenditures are positive and the lagged value is statistically significant. Though these coefficient values are inconsistent with the findings of Blundell et al. (2002), who obtain values of -0.215 for $patents_{t-1}$ and 0.173 on $ln(R\&D)$, and Cameron and Trivedi (2013), they are in line with Montalvo (1997). The inconsistency with the results of Blundell et al. (2002) is likely due to the difference in methods of defining the quasi-differencing transformation to remove the fixed effects.

The coefficients on current and lagged patent litigation in the over-identified case are of the expected sign, though are estimated imprecisely.[20] A χ^2 Wald test fails to reject ($\chi^2(3) = 1.29; p = 0.7306$) the hypothesis that the coefficients on all three patent litigation terms are jointly zero. These results imply that contemporaneous

[20]Given that litigation is logged and the conditional mean of the model is exponential, the coefficients on $ln(cases)_t$, $ln(cases)_{t-1}$, and $ln(cases)_{t-2}$ can be interpreted as elasticities.

and lagged patent litigation have minimal, if any, effect on patent production at the firm level. This may indicate that litigation costs incurred by firms do not severely impact firms' abilities to efficiently use R&D dollars. Alternately, if firms have in-house patent law expertise, this may mitigate any impact that litigation has on firms' patenting performance (Somaya, Williamson, & Zhang, 2007).

1.5.1 Robustness Checks

To check the robustness of the results from the main specification, I estimate a number of other models. Results are presented in Tables 1.3 and 1.4. Table 1.3 includes results from re-estimating the over-identified model with one and two leads of patent litigation. If the model is correctly specified, the inclusion of leads of the logged patent litigation should not greatly affect the results. Similar to the main specification, the coefficients on the contemporaneous and lagged patent litigation variables are estimated with low precision in both specifications, but the magnitudes and signs are consistent with the main empirical results. Since the lagged dependent variable is not logged in the original specification, the model may be explosive since the count variable, γy_{it-1}, is non-negative. I control for potential explosiveness in the lagged dependent variable by re-estimating the main over-identified specification by taking the log of the dependent variable. Results are presented in the third column of Table 1.3. The estimates are less precise than main empirical results, but magnitudes and signs are both consistent with the main results and with the other robustness tests.

Since the mean of the count variable is large, estimation via a linear model may be satisfactory. Thus, as a further robustness tests, I estimate Equation 1.3 via GMM. To provide results that are consistent across the two models, I estimate the linear dynamic model with the log of patents as the dependent variable. The more efficient

Table 1.3: Robustness Checks Using Leads

Coefficient	(1) One Lead	(2) Two Leads	(3) Explosiveness
$Patents_{t-1}$	0.0001***	0.0001***	
	(0.0000)	(0.0000)	
$ln(patents)_{t-1}$			0.4645***
			(0.1664)
$ln(R\&D)_t$	0.2031	0.2841	0.0416
	(0.1951)	(0.2423)	(0.2616)
$ln(R\&D)_{t-1}$	0.1517**	0.0784	0.0594
	(0.0668)	(0.0731)	(0.1043)
$ln(cases)_t$	-0.0427	-0.0549	-0.0438
	(0.0485)	(0.0745)	(0.0489)
$ln(cases)_{t-1}$	-0.0137	-0.0147	-0.0136
	(0.0352)	(0.0432)	(0.0338)
$ln(cases)_{t-2}$	-0.0211	-0.0253	-0.0294
	(0.0216)	(0.0277)	(0.0212)
$ln(employees)_t$	0.0816	0.0837	-0.0632
	(0.1269)	(0.1166)	(0.3935)
$ln(cases)_{t+1}$	0.0178	0.0369	
	(0.0337)	(0.1002)	
$ln(cases)_{t+2}$		0.0134	
		(0.1319)	
Year Dummies	Yes	Yes	Yes
No. of Observations	1319	1293	1349

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.01

two step estimator is calculated with the bias-corrected robust standard errors. As in the original specification, I am able to control for unobserved heterogeneity and for the weakly exogenous nature of the regressors. Results are presented in Table 1.4.

Coefficients on contemporaneous and the second lag of patent litigation are negative all patent litigation variables are estimated imprecisely. Tests for serial correlation in the first differenced errors of the Arellano-Bond estimator at orders higher than one are indicate no serial correlation, implying that the moment conditions used in

Table 1.4: Linear Robustness Check

	(1)
Coefficient	Linear Dynamic GMM
$ln(Patents)_{t-1}$	0.6923***
	(0.0659)
$ln(R\&D)_t$	0.3406***
	(0.0937)
$ln(R\&D)_{t-1}$	-0.1896***
	(0.0592)
$ln(cases)_t$	-0.0136
	(0.0315)
$ln(cases)_{t-1}$	0.0054
	(0.0226)
$ln(cases)_{t-2}$	-0.0164
	(0.0203)
$ln(Employees)_t$	0.0316
	(0.1477)
Year Dummies	Yes
No. of Instruments	463
No. of Observations	1383

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.01

estimation are valid.

As a final robustness check, I re-estimated both the just-identified and over-identified GMM specifications with one additional lag of patent litigation as a regressor. Results are presented in Table 1.5. The coefficient on the third lag of patent litigation becomes is positive in both cases and a U-shaped distributed lag appears with contemporaneous and lagged patent litigation being negative and the third lag being positive in the over-identified case. This indicates that the addition of the extra lag may have induced multicollinearity or, as suggested by Pakes and Griliches (1984), that there is truncation problem where the third lag is proxying for additional effects of earlier litigation.

Table 1.5: Additional Lag

Coefficient	(1) Just-Identified	(2) Over-Identified
$Patents_{t-1}$	0.0001**	0.0001***
	(0.0001)	(0.0000)
$ln(R\&D)_t$	0.0910	0.2713
	(1.0697)	(0.2320)
$ln(R\&D)_{t-1}$	0.1295	0.0694
	(0.1031)	(0.0729)
$ln(cases)_t$	-0.0485	-0.0744
	(0.2632)	(0.0578)
$ln(cases)_{t-1}$	0.0114	-0.0181
	(0.1701)	(0.0393)
$ln(cases)_{t-2}$	-0.0224	-0.0225
	(0.0711)	(0.0287)
$ln(cases)_{t-3}$	0.0102	0.0136
	(0.0227)	(0.0190)
$ln(employees)$	-0.1241	0.0437
	(1.1012)	(0.1395)
Year Dummies	Yes	Yes
No. of Observations	1201	1177

Standard errors in parentheses

* p<0.10, ** p<0.05, *** p<0.01

The over-identified case was re-estimated with two different specifications for the weight matrix: a weight matrix that account for correlation inside each panel and a heteroskedastic- and autocorrelation-consistent weight matrix. Results were very similar to those using the initial specification with very little change in either the coefficient estimates or the standard errors.

1.5.2 Estimation by Firm Size

A potential determinant of the effect of patent litigation on patent production may be the size of the firm receiving litigation. Lanjouw and Schankerman (2004) have shown that small patentees face significant disadvantages in utilizing and protecting their patent rights. They found that small firms[21] are involved in litigation more frequently than larger firms, indicating that smaller firms are unable to avoid litigation due to resource constraints. To test if there are asymmetric affects of patent litigation on patent production, I divide my sample into small and large firms. Like Lanjouw and Schankerman (2004), I define firm size by using the median of employment. The median number of employees in my sample is 3851 employees.

Table 1.6: Regressions by Firm Size

	Small Firm: <3800 Employees	
Coefficient	Small Firm	Large Firm
$patents_{t-1}$	0.0001	0.0001***
	(0.0001)	(0.0000)
$ln(R\&D)_t$	0.7626***	0.3494
	(0.1803)	(0.4348)
$ln(R\&D)_{t-1}$	0.2943***	-0.0027
	(0.0497)	(0.0893)
$ln(cases)_t$	-0.0910***	0.0351
	(0.0340)	(0.1006)
$ln(cases)_{t-1}$	-0.0545**	0.0065
	(0.0248)	(0.0759)
$ln(cases)_{t-2}$	-0.0683***	-0.0262
	(0.0238)	(0.0378)
$ln(employees)$	-0.1489	-0.0242
	(0.1534)	(0.1585)
Year Dummies	Yes	Yes
No. of Observations	905	438

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.01

Results are presented in Table 1.6. Results from a Chow test allow me to reject the null hypothesis of coefficient equality between small and large firms. Columns

[21] Defined as a firm with less than 5425 employees, the median of their sample.

(1) and (2) of Table 1.6 contain results from the over-identified GMM estimator. For small firms, the coefficients on contemporaneous and lagged patent litigation are negative and significant. A Wald test allows me to reject the hypothesis that all three coefficients are jointly zero ($\chi^2(3) = 11.61; p = 0.0088$). For large firms, coefficients on contemporaneous and the first lag of patent litigation are positive and the second lag is negative with all coefficients calculated with low precision. I am unable to reject hypotheses that any of the coefficients are different from zero. The coefficient on the second lag of patent litigation is -0.0683, indicating that a 1% increase in patent litigation leads to a 0.068% decrease in patent grants applied for two years later. Given that median time to trial for a patent lawsuit is approximately 2.5 years, the significance of the second lag is in line with the data.

These results present strong evidence that the effect of patent litigation on patent grants is asymmetric, with smaller firms more adversely effected. This is consistent with the conclusions from Lerner (1995) and Lanjouw and Schankerman (2004) that patent litigation will impact smaller firms to a greater degree than larger firms.

1.6 Discussion

The results displayed in Table 1.6 imply that patent litigation has a significant effect on small firms' ability to efficiently convert R&D expenditures into patents. These results have implications for policy makers. Starting in 2005, the U.S. Congress unsuccessfully attempted to pass a number of bills aimed at patent reform.[22] It proposed such reforms as switching to a "first-to-invent" system, increasing the ability to oppose/re-examine patents, refining language on damages, and placing limitations on patent litigation. The piece of legislation that was passed during this period

[22]These include the Patent Reform Acts of 2005, 2007, and 2009.

was the Patent Reform Act of 2011, or the Leahy-Smith America Invents Act (AIA). However, unlike the bills proposed before it, the AIA did not make significant changes to the existing system of litigation and damages.

Given the proliferation of non-patenting entities[23] and the high costs of patent litigation, these effects are likely to persist in the near future. Current pending legislation aimed to curb litigation and reduce costs would address the issues for small firms facing litigation. For example, the Innovation Act (H.R. 9) includes language that would increase the requirements needed to raise a complaint regarding infringement, reduce discovery costs, and include provisions for cost and fee shifting. These kinds of policies would help limit the costs incurred by smaller firms that do not have the resources to efficiently deal with litigation.

1.7 Conclusion

This paper utilizes a newly constructed data set combining data from the United States Patent and Trademark Office, Compustat, and Lex Machina. The combined data allows me to form a panel data set consisting of 194 firms across 11 years. The longer panel enables me to better model the time dimension while controlling for cross-sectional effects of patent litigation on patenting-intensive firms' abilities to produce patents.

Estimating a dynamic count data model of patent production via generalized method of moments, I find evidence that patent litigation has an asymmetric effect on firms depending on size: litigation disproportionately hurts small firms compared to larger firms. This result is consistent with other research in the patent litigation literature regarding firm size. The result also has important implications for policy

[23] Commonly known as patent 'trolls'.

makers in regards to currently proposed legislation regarding controlling the costs of litigation.

There are a number of dimensions that could be added to this research. Extending the dataset across more firms would provide a better cross-sectional analysis. Additional litigation data across time would provide the prehistory data needed to avoid trimming the sample when adding lags to the regression. If the findings of this paper could be extrapolated to a larger dataset, this could present stronger evidence to the growing opinion, both legally and economically, that stronger patent protection (or at least a legal environment that encourages more litigation) may reduce patenting, rather than be a driver of it, as is traditionally assumed. This is particularly pertinent for the United States, as the secular trends show continued growth in the number of patents being applied for and the number of infringement lawsuits being levied.

CHAPTER 2

DID THE ACA'S DEPENDENT COVERAGE MANDATE DECREASE FINANCIAL DISTRESS FOR YOUNG ADULTS?*

2.1 Introduction

Implemented in September 2010, the dependent coverage mandate of the Patient Protection and Affordable Care Act (ACA) allowed young adults to remain on their parents' health insurance plan until the age of 26. Prior to the passage of the ACA, young adults under 26 experienced the highest uninsured rates of any age group in the United States, with data from the Current Population Survey (CSP) indicating that the average annual uninsured rate from 2006-2009 for young adults ages 19-25 was 35.2 percent. Since implementation, the mandate has resulted in significant reductions in the uninsured rates for young adults, with a 30-43.6 percent increase in parental ESI coverage among young adults in the first year of the mandate (Akosa Antwi, Moriya, & Simon, 2013).

*This essay is based on a paper by Blascak (2017), written while an employee of the Federal Reserve Bank of Philadelphia. The views expressed here are soley those of the author and do not necessarily reflect the views of the Federal Reserve Bank of Philadelphia or the Federal Reserve System.

A large body of literature has examined the effect of the mandate on a number of other outcomes, including the effects on employment (Akosa Antwi et al., 2013), self-employment (Bailey & Chorniy, 2016), and health outcomes and health care utilization (Antwi, Moriya, & Simon, 2015; Barbaresco, Courtemanche, & Qi, 2015). However, relatively few studies have researched the one of the primary goals of the mandate: reducing medical expenditure risk for young adults. Chua and Sommers (2014) and Chen, Vargas-Bustamante, and Novak (2016) both showed that annual health care expenditures for individuals ages 19-25 decreased by 14 percent and overall out-of-pocket (OOP) expenditures decreased between 18-21 percent after the mandate was implemented.

This paper contributes to this literature by examining the financial effects of the dependent coverage mandate. I follow a number of recent studies that have utilized large, nationally representative data sets of credit bureau data, to examine the effect of health insurance policies on financial well-being (Hu, Kaestner, Mazumder, Miller, & Wong, 2016; Mazumder & Miller, 2016). These data provide a unique perspective for assessing financial distress, as they contain detailed credit and debt information on a 5 percent sample of U.S. adults with a credit report.

To estimate the effect of the mandate on measures of financial distress, I use the standard differences-in-differences (DID) framework and compare young adults born in either 1985 or 1986 (who were ages 24 or 25 at the passage of the mandate in 2010) with individuals born in 1982 or 1983 (who were ages 27 or 28 in 2010). I find that the mandate had an immediate effect of reducing the amount of money owed in third-party collections and lowering the probability that a young adult would file for bankruptcy during the pre-implementation period from March 2010-September 2010. During this period, the probability that a young adult would file for bankruptcy fell by 0.04 percentage points and the amount owed in third-party collections fell by 3.2 percent. I obtain similar estimates for the short-run effect of the mandate after its implementation. During the two years immediately following the passage of the mandate, from the fourth quarter of 2010 to the fourth quarter of 2012, the probability of bankruptcy also fell by an average of 0.04 percentage point and the amount owed in third-party collections fell by 3.1 percent. Placebo tests on cohorts of older individuals do not show comprehensive evidence that my results are driven by a secular trend in a reduction of financial distress for young adults.

Estimates of the long-run effect of the mandate indicate that improvements observed in the short-run disappear after an individual ages out of the mandates coverage at age 26. This result is consistent with Dahlen (2015), who found a 15.4 percent increase in the share of young adults with worse health insurance coverage after they aged out of the mandate.[1]

To test the robustness of my results, I estimate the effects of the mandate on financial distress along two different geographic dimensions. First, I investigate if individuals living in counties that experienced high young adult uninsured rates prior to the mandate experienced larger decreases than individuals living in counties that experienced lower uninsured rates. I then examine if individuals living in states with high young adult unemployment rates experienced relatively larger decreases than those living in states with lower rates of unemployment. I find evidence that decreases in financial distress were largest for individuals living in counties that experienced high rates of young adult uninsurance and states that experienced high rates of young adult unemployment. To directly test if individuals in these areas experienced greater improvements, I follow Mazumder and Miller (2016) and combined the rates of uninsured and unemployment and form an 'exposure' indicator and estimate

[1]This decrease in quality occurred even though uninsured rates did not increase at age 26. This is result was driven by the fact that purchases of non-ESI coverage went up at age 26, which typically more expensive and provides less generous benefits than ESI coverage.

a triple-difference model. Point estimates from the triple-difference specification indicate that young adults living in high exposure areas compared to those living in low exposure areas experienced greater improvements in financial distress, but most coefficients are statistically insignificant.

Overall, my results indicate that ACA's dependent coverage mandate improved the financial well-being of young adults. This is consistent with other recent literature that has shown that assessments of welfare effects of health care policy should account for the impact on individuals' personal finance as well as such factors as labor market outcomes.

It is important to note that, because I observe only if young adults are eligible to be covered by the mandate, not if they actually gained health insurance coverage, my estimates measure the intent-to-treat (ITT) effects of the dependent coverage mandate. This implies that my estimates of the effect of the mandate on the treated individuals are more conservative than the treatment effects for individuals that received health insurance through the mandate,[2] as I will be averaging effects across eligible individuals that actually received health insurance through their parents' plans and those that did not.

2.2 Institutional Details and Literature Review

2.2.1 The Affordable Care Act's Dependent Coverage Mandate

The dependent coverage mandate was passed as part of the Affordable Care Act in March of 2010. The mandate took effect for family health insurance plans issued

[2]These are known as treatment effects on the treated, or TOT.

after September of 2010, requiring plans to offer coverage to children of the plan-holder until their 26th birthday. Previously it was common for family plans to cover dependents only up to age 18, though many states had their own prior dependent coverage mandates which often extended coverage through age 23. The mandate led to a substantial increase in the proportion of 19-25 year olds with health insurance coverage. In addition to uninsured young adults gaining coverage, many young adults who previously had insurance in their own name switched to dependent coverage (Akosa Antwi et al., 2013).

A growing literature has examined the effect of the mandate on the health and labor market decisions of young adults. It has been shown that the mandate increased self-reported health and the probability of having a primary care doctor (Barbaresco et al., 2015), increased educational attainment and wages (Dillender, 2014), and increased the rate of self-employment, though only for young adults expected to have high health costs (Bailey, 2016).

Most relevant to the financial decisions we study, the mandate led to a substantial drop in the chance that young adults incurred large out-of-pocket charges for health care. For example, one study has found that the mandate reduced the chance of spending more than $1,500 per year out-of-pocket on health care by 57% (Busch, Golberstein, & Meara, 2014). While it is difficult to observe within-family financial transactions, it is likely that young adults pay little toward the cost of their coverage under the mandate. The marginal premium increase from adding a member to a family insurance plan tends to be lower than the cost of an individual plan; most plans are subsidized by employers, and the remaining costs may be borne by parents rather than children. While it has been shown that the mandate led to an increase in the average premiums of family plans, employers appear not to have passed on much of this cost to employees (Depew & Bailey, 2015). What costs were passed on were spread among employees in general, not only those with dependent children (Goda, Farid, & Bhattacharya, 2016). Overall, it seems clear that the mandate reduced the chance that young adults would incur large, unexpected health costs.

2.2.2 Literature Review

Mazumder and Miller (2016) study the effects of the 2006 Massachusetts health care reform on financial distress. Using credit bureau data, they use a "triple difference" identification strategy that exploits variation across time, across states, and across counties within Massachusetts for different age groups. Specifically, they take advantage of the fact that different counties within Massachusetts had different levels of uninsurance rates, or 'exposure rates,' prior to the health reform. Their empirical results show that the reform led to statistically significant reductions in total debt, total amount past due, percent of debt past due, and the probability of bankruptcy for age groups in counties in Massachusetts relative to other states.

Barcellos and Jacobson (2015) study the effect of Medicare eligibility on financial distress for older adults. Using data from the Medical Expenditure Panel Survey (MEPS), they use a regression discontinuity research design to exploit the age-based eligibility criteria for the Medicare program to compare medical expenditure risk between near-elderly individuals (ages 50-64) with young elderly individuals ages (65-80). The authors find that, as individuals age into Medicare, the distribution of out-of-pocket (OOP) spending shifts to the left, as expenditures drop 33 percent at the mean and 53 percent at the 95th percentile. There is little evidence of discrete changes in covariates at the discontinuity, indicating that change in medical expenditure risk is not due to changes in utilization or rationing of care.

Gross and Notowidigdo (2011) examine how the Medicaid expansions during the 1990s affected consumer bankruptcy filings. Combining data from the Current Population Survey (CPS) and the census of consumer and business bankruptcies published by the Administrative Office of the U.S. Courts, they use variation in state-level expansions of Medicaid eligibility to estimate the effect of Medicaid eligibility on bankruptcy. To address endogeneity issues due unobserved shocks that may lead to both more consumer bankruptcies and households eligible for Medicaid, the authors use simulated Medicaid eligibility as an instrumental variable (IV) for actual Medicaid eligibility. They find that a 10 percentage-point increase in Medicaid eligibility reduces bankruptcies by 8 percent. Additional within-state analyses indicate that reductions in bankruptcies were concentrated in zip codes with high percentages of youth under 17 and annual incomes under $40,000, the groups most affected by the Medicaid expansion. These findings reinforce the idea that health insurance reduces household's medical expenditure risk by limiting OOP costs.

Debbaut, Ghent, and Kudlyak (2016) analyze the impact of the passage of the Card Accountability, Responsibility and Disclosure (CARD) Act on credit card availability. The CARD Act, passed in 2009, implemented a number of restrictions and regulations on the credit card industry including Title 3, which prohibited credit card companies from recruiting on college campuses or sending preapproved credit card offers to individuals under 21. The authors detail a number of important stylized facts about the credit card behavior of young adults, including detailed information on how youth ownership of credit cards has changed over time. Using detailed individual-level credit bureau data, the authors exploit the policy change to estimate a differences-in-differences model to causally identify the impact of the CARD Act on credit card ownership. They find that the passage of the law decreased the likelihood that an individual under 21 would have a credit card by 8 percentage points.

Hu et al. (2016) also use credit bureau data to analyze the effect of the ACA's 2014 Medicaid expansion on financial distress. To compare the credit records of individuals that lived in states that chose to expand Medicaid under the ACA with those individuals who lived in states that did not, the authors use the synthetic control method to address the potential endogeneity of the decision to expand Medicaid. Their results indicate that individuals living in zip codes that were most likely to be affected by the Medicaid expansion experienced reductions in unpaid non-medical bills and the amount of non-medical debt sent to third party collections.

Brown, Grigsby, van der Klaauw, Wen, and Basit (2016) examine the effects of financial education on borrowing and debt behavior of young adults. Using individual-level credit bureau data, the authors exploit state-level variation in the timing of implementation of high school curriculum aimed at improving financial education, to causally identify the effect of exposure to financial and quantitative education on a number of debt outcomes. Results from their event study specification indicate that an additional year of math training has small, but statistically significant effect on several measures of debt performance, while mandatory financial education leads to declines in the percentage of debt that is delinquent. Surprisingly, the mandatory economics training is actually associated with small declines in debt performance.

Lee (2016a) uses data from the Survey of Income and Program Participation (SIPP) to investigate if households reduced their precautionary savings in response to the Affordable Care Act's dependent coverage mandate. He argues that the implementation of the mandate lowers the risk of consumption shocks due to medical expenditures, which in turn should lower the amount of precautionary savings held by these households. Using a 'triple difference' identification strategy, Lee examines a number of different measures of precautionary savings, including savings/bank accounts and retirement accounts. He finds that households with dependent children ages 19-25 reduced their precautionary savings by almost $900 a year after the mandate went into effect.

2.3 Conceptual Framework

The main hypothesis of this paper is that providing health insurance to young adults through their parents' plans should lessen financial distress. However, there are many mechanisms through which the provision of health insurance can reduce financial distress. For those individuals that were uninsured prior to the passage of the mandate, receiving coverage likely results in an 'income effect' that improves their financial standing. Receiving coverage through parental employer-sponsored insurance (ESI) reduces OOP medical expenditures on preventative medical services and bills incurred due to an adverse medical shock, resulting in higher income.

A sizeable number of young adults that had own-name ESI switched to a parent's plan in response to the mandate (Akosa Antwi et al., 2013). For these individuals, the decision to switch plans is likely driven by differences in OOP expenditures and/or plan generosity. For example, if I assume young adults derive utility from insurance coverage and there are non-zero switching costs for shifting to parental ESI from an own-name plan, then individuals must receive higher utility either through increased plan generosity or through lower OOP costs to incentivize a switch. Similar to the uninsured, individuals who switched insurance plans due to lower OOP costs also experienced an 'income effect' through this OOP reduction. Overall, I expect the 'income effect' for these individuals that switched from own-name to parental ESI, to be less than that of the uninsured group, since it includes individuals that switched due to both utility gains through increased benefits and lower costs.

Another mechanism through which the mandate may operate is a 'risk' mechanism where insurance coverage reduces the risk of incurring large medical expenditures. Newly covered individuals receive the largest reduction in this kind of risk, while young adults who switched from own-name to parental ESI likely receive smaller decreases in this kind of risk. However, given that younger people are healthier on average, I expect to see relatively smaller effects from this mechanism than previously seen for more general populations.

It is also possible that the mandate indirectly affected young adults' financial well-being in the short-run. For example, if individuals experience "job-lock"[3] due to health insurance, health insurance through parental ESI may remove the incentive to stay at a job. In the short-run, this may result in a temporary increase in unemployment and subsequent declines in income, but may also increase entrepreneurial or college-going opportunities.

If individuals operate within this basic conceptual framework, I hypothesize that the mandate should reduce financial distress. Given that severe health shocks are less likely for this population, I predict that improvements will come from extreme cases such as bankruptcy or accounts in collections, where the insurance protection is greatest.

2.4 Data

2.4.1 Consumer Credit Data

The consumer credit data used in the analysis come from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel (CCP). The CCP data set is an anonymized, nationally representative 5 percent random sample of individuals with credit bureau records from 1999 to the present. Consumers must have at least one public record or credit account and a social security number (SSN) to be included in the CCP. Individuals are followed at a quarterly frequency until they die, change their

[3] "Job lock" is the reluctance to change jobs due to the fear of losing ESI.

SSN, or drop off due to an extended period of credit market inactivity. While the CCP contains extensive information regarding credit information, it does not contain any demographic information besides year of birth and address. In a given quarter, the CCP contains approximately 78 million observations on 12 million different consumers.[4]

One concern with our data is that not all individuals, especially young adults, have a credit bureau file. Work by Lee and van der Klauw (2010) and Brown et al. (2016) compare the CCP with data from the Survey of Consumer Finances (SCF), the American Community Survey (ACS) and provide strong evidence that young adult population covered in the CCP is representative of other measures of this age population. Analysis by Brevoort, Grimm, and Kambara (2015) show that, while approximately 62 percent of consumers age 18-19 do not having a credit bureau file, that number drops to nearly 10 percent for consumers age 25-29.

2.4.2 Sample Selection

Because the mandate's effects are determined by age, I restrict the data to include the credit files of individuals who are born in the years of 1982-1983 and 1985-1986. The individuals born in 1985 and 1986 will serve as a treatment group, as they would have been 24 and 25, respectively, when the mandate took effect in 2010. Individuals born in 1982 and 1983 are never treated by the mandate, as they would have been 27 or 28 when it was implemented, and therefore serve as the control group.[5] Limiting the analysis to four birth-year cohorts also helps increase comparability within the

[4]For a more comprehensive overview of the CCP, see Lee and van der Klauw (2010).

[5]Because the CCP only contains information on birth year, the possible age range of individuals in the treated group could be from 23-25. I exclude individuals born in 1984, as it is possible they would have turned 26 prior to the passage of the mandate.

groups, as both levels and trends in credit data exhibit substantial time-varying cohort effects (Debbaut et al., 2016; Fulford & Schuh, 2016). Including more birth years in the treatment and control groups would likely cause the two groups to trend differently and cast doubt on the identifying assumption of the DID framework. Therefore, by limiting the analysis to four birth-years, I increase the likelihood that the control group properly accounts for other unobserved factors that affect the financial outcomes of young adults.

Table 2.1: Financial Distress Summary Statistics

	Treatment			Control		
Variable	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs
Amount in Third-Party Collections	739.14	2603.89	1,651,309	721.10	2865.04	1,730,304
Number of Accounts in Third-Party Collections	0.511	1.398	3,575,695	0.506	1.456	3,680,227
Total Amount Past Due, revolving accounts	914.75	4311.58	3,147,932	1322.73	14914.34	3,248,394
New Incidence of Third-Party Collections	0.086	0.281	3,539,760	0.094	0.277	3,652,047
New Incidence of Bankruptcy Filing	0.001	0.030	3,595,895	0.002	0.039	3,663,109
Number of individuals	435,765					
Number of new bankruptcy filings	10,597					

Note: Author's calculations using data from the FRBNY Consumer Credit Panel/Equifax. Data is for full sample period, Q2:2009-Q4:2013.

I take a 50 percent subsample from Q2:2009 to Q4:2013 of all consumers from the four birth-year cohorts and drop any consumers that have less than four total observations across the sample period. My final sample includes approximately 436,000 individuals and 7.8 million observations. The panel is unbalanced, since restricting the data to only include young consumers, who typically have thin credit files[6] and are less like to have continuously present credit bureau files, may introduce sampling bias.[7]

To analyze financial distress, I use a number of different measures as outcomes variables. I use the amount of revolving debt past due,[8] the number of accounts in third-party collections,[9] and the amount of debt in third-party collections. I also include an indicator variable for a new declaration of bankruptcy and an indicator for the presence of an account in third-party collections. These variables measure different levels of financial distress, with bankruptcy and third-party collections indicating the most extreme type of financial distress.[10]

2.4.3 Control Data

Although I cannot control for individual or household-level insurance status, I account for county-level differences in insurance status prior to the passage of the mandate using the Small Area Health Insurance Estimates (SAHIE) data from the U.S. Census Bureau. The SAHIE data are produced by a hierarchal Bayesian model that estimates health insurance coverage for every county in the United States. This

---

[6]Credit bureau records with only one or two trades, or accounts, are considered 'thin'.

[7]Young people have 'thin' credit files typically because they have little need or opportunity for credit activity (Lee & van der Klauw, 2010).

[8]Revolving debt includes debt from credit cards, charge cards, department store cards, and home equity lines of credit.

[9]An account is sent to third-party collections when the party that the debt is initially owed to is unable to collect it from the debtor and contracts an outside party to collect the debt.

[10]A large number of other variables were considered for this analysis, including number of accounts in severe delinquency, presence of bankruptcy declared within the past 24 months, other measures of total amount past due, and the presence of derogatory events (e.g., charge-offs, first-party collections), but the trends for the two age groups in these variables were not parallel.

model combines data from multiple sources, including the ACS, the Current Population Survey (CPS), and data from the Medicaid and SNAP programs.[11] Following Mazumder and Miller (2016), I include uninsured rates for two age groups, 18-39 year-olds and 18-64 year-olds.

To control for local labor market conditions, I use yearly data from the American Community Survey (ACS). I use state-level unemployment data for three different age groups: 20-24 year-olds, 25-34 year olds, and the total unemployment rate for the state. I also use county-level poverty data from the Small Area Income and Poverty Estimates (SAIPE). Similar to SAHIE, SAIPE model utilizes a large number of data sources to make regression-based predictions on the number of people living in poverty at the state, county, and school-district level.

2.5 Methodology

To identify the impact of the mandate on financial distress, I use the standard difference-in-differences (DID) framework to separate the impacts of the policy change from other contemporaneous changes in the financial distress of young adults.

The DID framework relies on the assumption that the trends in the financial variables would be the same for the treatment and control groups in the absence of the mandate. While I cannot test if the treatment and control groups would have trended similarly in the post-mandate period, I can evaluate if the two groups had similar trends in the pre-mandate period. To assess if the trends in my financial variables are comparable across the treated and control groups, I use a method similar

[11] For more information on the SAHIE data, see http://www.census.gov/did/www/sahie/index.html

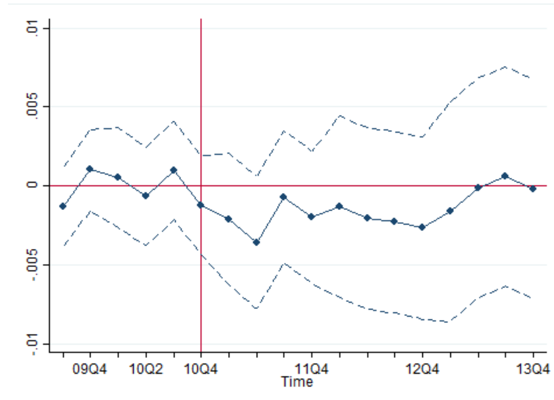
to Mazumder and Miller (2016) and estimate the following model:

$$y_{it} = \gamma_0 + T_t \times Treated_i\Gamma + T_t\Pi + \gamma_2 Treated_i + X_{it}\beta + \zeta_{it} \tag{2.1}$$

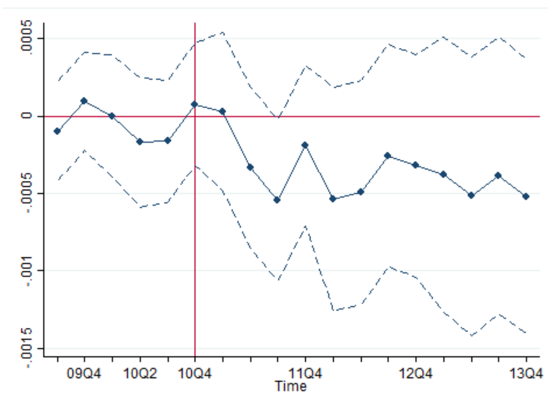
where T_t is a vector of calendar time dummy variables, $Treated_i$ is a dummy variable equal to one if an individual was born in 1985 or 1986, and X_{it} is a vector of control variables that includes state fixed effects, state linear time trends, county-level uninsured and poverty rates, state-level unemployment rates for the 19-25 year old age group and the 26-34 year old age group, state-level unemployment rate interacted with the treatment dummy variable, age and age $\times$ birth-year cohort fixed effects. The interaction between $Treated_{it}$ and the vector of calendar time dummy variables T_t provides estimates of the differences in the credit variable of interest between the treated and control groups for each quarter. Statistically significant coefficient estimates on the coefficients in the vector Γ indicate differential trends between the treatment and control groups in that period. The absence of significant coefficients implies no difference in trends between the two groups.

Figure 2.1 plots the coefficients from the Γ vector in Equation (2.1) for differences in the amount of debt owed in third-party collections, the number of accounts in third-party collections, the probability of having a new account in third party collections, the probability of a filing bankruptcy, and the amount of debt past due for all revolving accounts. Dotted lines represent 95% confidence intervals. Coefficients on interactions for each variable in Figure 2.1 are zero or close to zero in the pre-mandate period prior to the second quarter of 2010, indicating that the trends in the outcome variables are parallel for the treatment and control groups. Coefficient estimates in the post-mandate period are negative, indicating that the treatment group experienced a relative decline in the probability of incurring financial distress. However, Panel A: Amount in Third-Party Collections

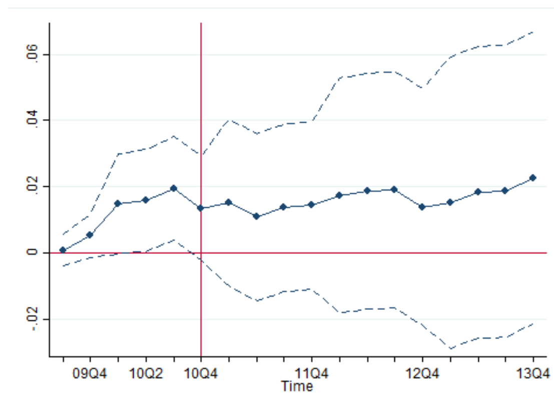
Panel B: Probability of New Incidence of an Account in 3rd Party Collections



Panel C: Incidence of New Bankruptcy Filing



Panel D: Number of Accounts in Third-Party Collections



Panel E: Total Amount Past Due, Revolving Accounts

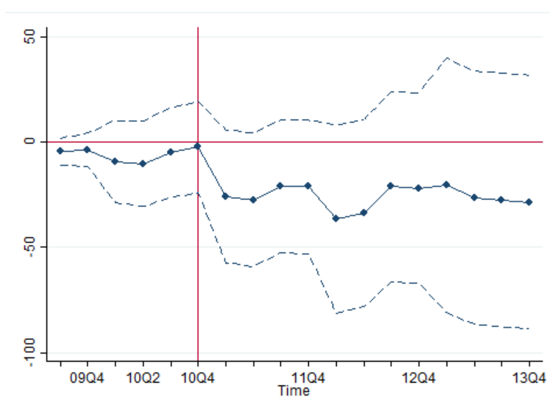


Figure 2.1: Differences in Trends. Note: Authors calculations using data from FRBNY Consumer Credit Panel / Equifax.

the coefficients are estimated with less precision in the later periods, as can be seen with the widening 95% confidence intervals. This may be a result of the event-study nature of the regression.

As an additional formal test of the equality of trends in the treatment and control groups before treatment, I estimate the following model on the pre-mandate data, from the first quarter of 2009 to the first quarter of 2010:

$$y_{it} = \delta_0 + \delta_1 Treated_i \times Time + \delta_2 Treated_i + \delta_3 Time + X_{it}\beta + \nu_{it} \tag{2.2}$$

The model is estimated with the same control variables as in Equation (2.1). The variable of interest in Equation (2.2) is the interaction between the treatment dummy variable $Treated_i$ and the linear time trend $Time$. If there is a difference in trends in the pre-mandate period for the treatment and control groups, we would expect δ_1 to be statistically significant and non-zero. A coefficient of zero indicates that that there is no difference in trends between the two groups. Results for the credit variables of interest are presented in Table 2.2. Based on the results, I conclude that are no significant differences in trends for the variables of interest prior to the passage of the mandate.

To estimate the causal impact of the passage of the mandate on financial distress for young adults, in principle, I would estimate the following DID model:

$$y_{it} = \alpha_0 + \alpha_1 Treated_t \times Post_t + \alpha_2 Treated_i + \alpha_5 Post_t + X_{it}\beta + \mu_i + T_t + \epsilon_{it} \tag{2.3}$$

where $Treated_i$, X_{it}, and T_t are as defined above. μ_i is an individual fixed effect and $Post_t$ is a dummy variable equal to one for observations starting in the fourth quarter of 2010, the first quarter that the mandate was officially implemented, to the end of the sample period in 2013. However, it is known that several health

Table 2.2: Difference in Linear Trends Between the Treatment and Control Groups Before Treatment (Q2:2009-Q1:2010)

Variable	Coefficient	Obs.
Number of Accounts in Third-Party Collections	0.003	1,510,718
	(0.002)	
Amount in Third-Party Collections	-1.864	700,841
	(8.467)	
Total Amount Past Due, revolving accounts	-1.910	1,063,320
	(4.657)	
New Incidence of Account in Third-Party Collections	0.002*	1,125,103
	(0.001)	
New Bankruptcy Filing	0.00004	1,513,497
	(0.000)	

Note: Standard errors clustered at the individual level. Author's calculations using data from the FRBNY Consumer Credit Panel / Equifax.

insurers announced their intention to implement the mandate prior to the required implementation date in September 2010.

To address the staggered nature of the implementation of the mandate, I follow the approach widely used in the previous literature and create a number of time period dummy variables to control for the effects of the mandate at different points in the timeline of the implementation. $Enact_t$ is a dummy variable equal to one for observations that span the enactment period of the mandate, from the second quarter to third quarter of 2010 (March 2010-September 2010). I divide the post-implementation period into two separate time periods to analyze the short-run and long-run effects of the mandate on financial distress. In particular, the long-run time period coincides with quarters where individuals in the treatment group have aged out of the mandate. The short-run treatment period spans the fourth quarter of 2010 to the fourth quarter of 2012 while the long-run period runs from the first quarter of

2013 to the fourth quarter of 2013. The model to be estimated is now:

$$\begin{aligned} y_{it} =& \alpha_0 + \alpha_1 Treated_i \times Enact_t + \alpha_2 Treated_i \times Implement_t + \\ & \alpha_3 Treated_i \times AgeOut_t + \alpha_4 Treated_i + \alpha_5 Enact_t + \alpha_6 Implement_t + \\ & \alpha_7 AgeOut_t + X_{it}\beta + \mu_i + T_t + \epsilon_{it} \end{aligned} \tag{2.4}$$

The coefficients of interest are α_1, α_2, and α_3, which indicate the immediate, short-run, and long-run effects of the mandate on financial distress for treated individuals. If access to health insurance only improves financial outcomes while young adults are covered, then we would expect the α_2 coefficient to be statistically significant. If health insurance companies began implementation of the mandate before it went into effect, α_1 can be negative and significant. The sign and significance of α_3 is ex-ante ambiguous, as there are many potential mechanisms that could drive certain effects after individuals have aged out of the mandate. For example, if young adults age out of the mandate and do not regain health insurance coverage, we may expect financial distress to increase again, or any improvements made while being insured may stop.

I considered two alternative methodologies, but ultimately decided they were inappropriate for this analysis. First, treatment and control groups were formed by age, instead of by birth year. This strategy results in a number of problems. Because I observe only year of birth and not the exact birth date, an individual's age would automatically change in the first quarter of a given year, inducing attenuation bias due to the measurement error in age. Also, use of age in the definition of the treatment and control groups introduced migration problems because of the length of the panel. Specifically, by using age instead of cohorts, individuals either entered or exited the treatment and control group each year after the implementation date. The combined problem of treatment/control migration along with the attenuation bias due to all individuals changing age in the first quarter of the year introduced unusual cyclicality into the analysis. I also tried applying a regression discontinuity (RD) approach at age 26. Unfortunately, none of the variables exhibited sharp enough discontinuities to warrant using an RD design. Also, I do not observe the precise birth date of an individual, only birth year, which limits the usefulness of an RD design.

2.6 Results

2.6.1 Main Results

The main DID results from Equation (2.4) are presented in Table 2.3. Each coefficient represents the average effect of the mandate on the measures of financial distress in each period. The first row of Table 2.3 provides evidence that the mandate had an immediate effect on financial distress during the pre-implementation period. The probability of a new bankruptcy declaration declined by a statistically significant 0.04 percentage points, which translates to an annual reduction of 1.2 bankruptcies per 1000 individuals.[12] The amount of debt that young adults have in third-party collections also declines by a small but statistically significant amount of $22, which is approximately a three-percent decline from the pre-implementation period mean. Although point estimates for the other measures of financial distress of the hypothesized sign, they are imprecisely estimated. Observing minor improvements in financial distress is not unexpected, given that there was a 10 percent increase in dependent health insurance coverage under parental ESI during this period (Akosa Antwi et al., 2013).

[12]Based on data from the CPS, the Administrative Office of the U.S. Courts, and from survey data produced by Thorne, Warren, and Sullivan (2009), the average number of bankruptcies per 1000 individuals for this age group was approximately 8 bankruptcies per 1000 people.

Table 2.3: The Effect of the Dependent Coverage Mandate on Financial Distress: DID Results (Q2:2009-Q4:2013)

Coefficient	Number of Accounts in Third-Party Collections	Amount in Third-Party Collections	Total Past Due, Revolving Accounts	New Incidence of Bankruptcy Filing	New Incidence of Third-Party Collection
Enactment	0.004	-22.184**	-1.9328	-0.0004***	-0.0002
(Q2:2010-Q3:2010)	(0.003)	(8.909)	(3.143)	(0.000)	(0.001)
Implementation	0.001	-21.964	6.9477	-0.0004***	-0.001
(Q4:2010-Q4:2012)	(0.004)	(14.968)	(4.803)	(0.000)	(0.001)
Age-Out	0.001	-33.803	17.083*	-0.0002	0.0004
(Q1:2013-Q4:2013)	(0.009)	(30.238)	(9.489)	(0.000)	(0.002)
Average in Enactment	0.498	704.62	1009.27	0.0014	0.084
Average in Treatment	0.510	733.56	1144.93	0.0012	0.085
Average in Age-Out	0.530	784.25	1387.19	0.0012	0.084
R^2	0.533	0.320	0.586	0.191	0.175
Num. of Obs.	7,255,544	3,381,503	5,091,669	7,258,621	7,191,476

Note: Standard errors clustered at the individual level. All regressions include individual, time, and state fixed effects. Author's calculations using data from the FRBNY Consumer Credit Panel / Equifax.

The short-run effects of the mandate are presented in the second row of Table 2.3. Similar to the enactment period, I observe a statistically significant decrease of 0.04 percentage points in the probability of declaring bankruptcy. The estimates also indicate that the mandate reduced the amount of debt in third-party collections by $21 and the probability of a new incidence of an individual having an account in third-party collections by 1.2 percent, although I cannot reject that these changes are different from zero. Because this time period coincides with the years that the treatment group is covered by the dependent coverage mandate (i.e., receives treatment), I would expect to find the strongest effects during this time period.

The third row of Table 2.3 shows the long-run effects of the mandate for the treated group, after they have aged out of the mandate. While point estimates for amount of debt in third-party collections and probability of new bankruptcy filing are negative, I observe no statistically significant improvements in any of the measures of financial distress in this period. The estimates indicate that positive effects of the mandate disappear after an individual ages out of the mandate.

Across all three time periods, I observe no statistically significant changes in the number of accounts in third-party collections or in the probability that an individual will have an account in third-party collections. While we observe no marked improvement in these measures of financial distress, it is important to note that these are ITT estimates. For treated individuals who become covered due to the mandate, these estimates provide a lower bound on the average treatment effect.

Overall, the results imply that the mandate had a modest, but economically significant effect on financial distress of young adults. As mentioned above, because these are ITT estimates, I interpret the results for all coefficients as a lower bound on the average treatment effect on the treated (TOT).

2.6.2 Heterogeneity Analysis

It is likely that the results displayed in Table 2.3 mask significant heterogeneity in the effects of the mandate. In Table 2.4, I divide the sample based on the average uninsurance rate in the county of residence in 2009. Specifically, I separate the sample based on whether a county was in the 75th percentile of young adult uninsurance rate in 2009,[13] the year before the mandate was enacted, and then estimate Equation (2.4) on each group separately. The top panel of Table 2.4 presents the results for counties that were in the 75th percentile or higher for young adult uninsured rate, and the lower panel presents results for counties below the 75th percentile. The number of new bankruptcy filings declined by 0.1 percentage points for individuals living in high uninsurance counties, a decrease that is approximately four times larger

[13]Estimates for young adult uninsured rate are generated from the SAHIE data.

than for those living in low uninsurance counties. Point estimates for reductions in amount of debt in third party collections are significantly larger for high uninsurance county individuals, but the estimates are not statistically significant. Estimates of the number and probability of having a new account in third-party collections are also statistically insignificant for both groups.

Table 2.4: The Effect of the Dependent Coverage Mandate on Financial Distress: Heterogeneity Effects by YA Uninsured Rate (Q2:2009-Q4:2013)

Coefficient	Number of Accounts in Third-Party Collections	Amount in Third-Party Collections	Total Past Due, Revolving Accounts	New Incidence of Bankruptcy Filing	New Incidence of Third-Party Collection
High Uninsurance Rate					
Enactment	0.001	-25.292	1.884	-0.001***	-0.001
(Q2:2010-Q3:2010)	(0.006)	(15.470)	(7.227)	(0.000)	(0.002)
Implementation	0.002	-32.784	1.422	-0.001***	-0.001
(Q4:2010-Q4:2012)	(0.008)	(28.537)	(8.225)	(0.000)	(0.003)
Age-Out	-0.001	-77.199	11.557	-0.001	0.004
(Q1:2013-Q4:2013)	(0.017)	(56.042)	(17.883)	(0.000)	(0.004)
R^2	0.537	0.298	0.562	0.189	0.164
Num. of Obs.	2,219,308	1,234,125	1,444,029	2,227,517	2,208,039
Low Uninsurance Rate					
Enactment	0.005	-20.447*	1.914	-0.0003*	-0.0001
(Q2:2010-Q3:2010)	(0.003)	(10.832)	(3.300)	(0.000)	(0.002)
Implementation	0.001	-15.766	9.117	-0.0003*	-0.002
(Q4:2010-Q4:2012)	(0.005)	(16.819)	(5.871)	(0.000)	(0.001)
Age-Out	0.002	-10.204	20.272*	-0.0001	-0.001
(Q1:2013-Q4:2013)	(0.010)	(35.058)	(11.278)	(0.000)	(0.002)
R^2	0.529	0.338	0.599	0.193	0.178
Num. of Obs.	5,036,236	2,147,378	3,647,640	5,031,104	4,983,437

Note: Standard errors clustered at the individual level. All regressions include individual, time, and statel fixed effects. Author's calculations using data from the FRBNY Consumer Credit Panel / Equifax.

Another dimension on which I expect to see significant differences is the level of unemployment young adults were experiencing prior to the passage of the mandate. It is likely that individuals living in states with high young adult unemployment were less likely to have health insurance. I employ the same method used for the uninsured rate and divide the sample based on the average young adult unemployment rate in the state of residence in 2009, using the 75th percentile of young adult unemployment as the cut-off. Results for the regressions on the unemployment samples are presented in Table 2.5, with states in the 75th percentile or higher for young adult unemployment in the top panel, and states below the 75th percentile in the bottom panel.

Table 2.5: The Effect of the Dependent Coverage Mandate on Financial Distress: Heterogeneity Effects by YA Unemployment Rate (Q2:2009-Q4:2013)

Coefficient	Number of Accounts in Third-Party Collections	Amount in Third-Party Collections	Total Past Due, Revolving Accounts	New Incidence of Bankruptcy Filing	New Incidence of Third-Party Collection
High Unemployment Rate					
Enactment	0.004	-14.696	1.741	-0.001***	-0.001
(Q2:2010-Q3:2010)	(0.004)	(11.791)	(4.395)	(0.000)	(0.001)
Implementation	0.006	-26.336	9.737	-0.001***	-0.001
(Q4:2010-Q4:2012)	(0.006)	(19.701)	(6.358)	(0.000)	(0.002)
Age-Out	0.019	-29.637	23.702*	-0.0001	0.003
(Q1:2013-Q4:2013)	(0.012)	(40.725)	(12.498)	(0.000)	(0.002)
R^2	0.529	0.306	0.580	0.191	0.171
Num. of Obs.	4,282,105	2,113,593	2,964,525	4,274,082	4,262,649
Low Unemployment Rate					
Enactment	0.004	-34.741***	1.768	-0.0001	-0.0004
(Q2:2010-Q3:2010)	(0.004)	(13.246)	(4.305)	(0.000)	(0.002)
Implementation	-0.004	-14.984	1.969	-0.00001	-0.002
(Q4:2010-Q4:2012)	(0.006)	(22.625)	(7.277)	(0.000)	(0.002)
Age-Out	-0.024*	-33.177	10.311	-0.0003	-0.003
(Q1:2013-Q4:2013)	(0.012)	(43.423)	(14.944)	(0.000)	(0.003)
R^2	0.539	0.354	0.601	0.196	0.179
Num. of Obs.	2,973,439	1,267,910	2,127,144	2,984,539	2,928,827

Note: Standard errors clustered at the individual level. All regressions include individual, time, and state fixed effects. Author's calculations using data from the FRBNY Consumer Credit Panel / Equifax.

The results are slightly more mixed than the results for the uninsured rate split. Similar to the previous results, reductions in bankruptcy filing probability are significantly larger in states that experienced higher rates of young adult unemployment prior to the passage of the mandate, compared with counties that experienced lower rates. Specifically, individuals living in high unemployment states experienced a statistically significant decrease in the probability of a new bankruptcy filing of 0.1 percent.

The lack of significant results for amounts of debt in third-party collections may be due to the presence of heterogeneity in the distribution of debt owed. To analyze the distribution, I follow Mazumder and Miller (2016) and construct a series of indicator variables equal to one if an individuals had a bracketed amount of debt in third-party collections: \$0, \$1-\$500, \$501-\$1000, \$1001-\$2000, and more than \$2000. Coefficients on the DID term produce probabilities that an individual has 3rd-party collections debt in one of the following categories. I estimate two models for these variables: an event study similar to Equation (2.1) and the DID analysis specified in Equation (2.4). Results from the event study are displayed graphically in Figure 2.2.

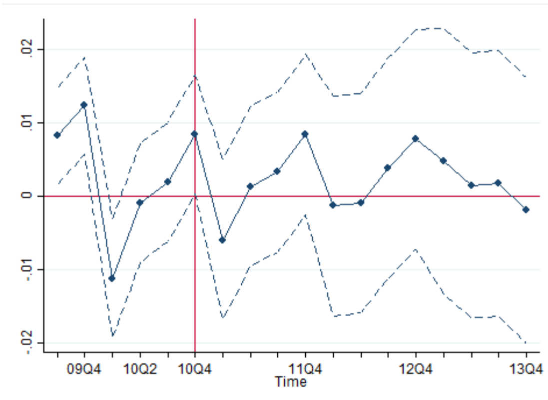
Table 2.6: The Effect of the Dependent Coverage Mandate on Financial Distress: Distribution of Amount in Third-Party Collections

	Amount in Third-Party Collections				
Coefficient	\$0	\$1-\$500	\$501-\$1000	\$1001-\$2000	\$2000+
Enactment	0.008***	-0.006*	0.001	0.005*	-0.0003
(Q2:2010-Q3:2010)	(0.002)	(0.004)	(0.003)	(0.005)	(0.002)
Implementation	0.013***	-0.001	-0.0007	0.0008	0.001
(Q4:2010-Q4:2012)	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)
Age-Out	0.016***	0.004	0.0004	-0.0006	-0.003
(Q1:2013-Q4:2013)	(0.005)	(0.007)	(0.007)	(0.006)	(0.006)
R^2	0.369	0.427	0.310	0.283	0.3818
Num. of Obs.	3,381,503	1,718,979	1,718,979	1,718,979	1,718,979

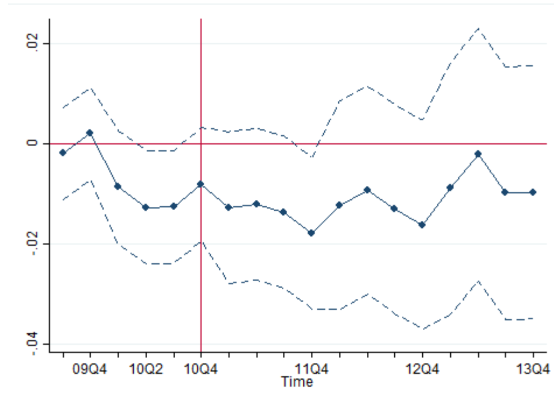
Note: Standard errors clustered at the individual level. All regressions include individual, time, and statel fixed effects. Author's calculations using data from the FRBNY Consumer Credit Panel / Equifax.

The probability of an individual having zero dollars in third-party collections show small positive increases, despite displaying significant seasonality, while the point estimates of the probabilities of having intermediate amounts in third-party

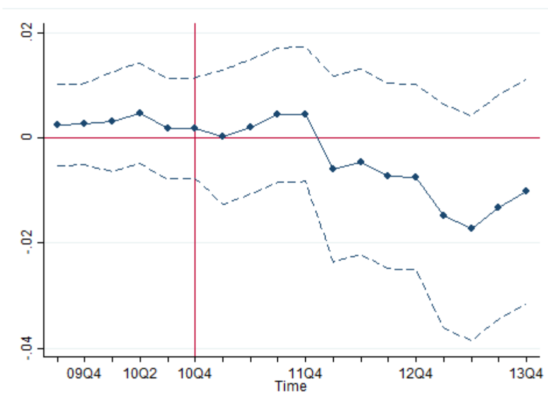
Panel A: Probability of $0 in Third-Party Collections



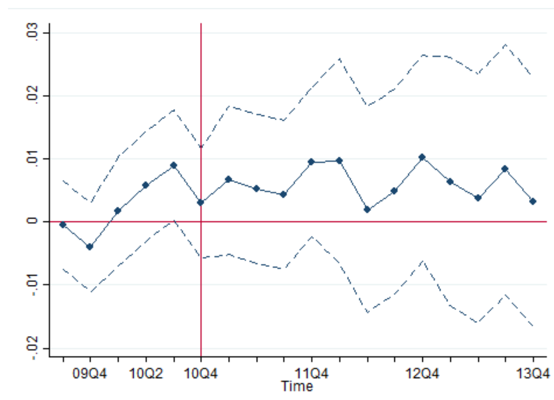
Panel B: Probability of $1-$500 in Third-Party Collections



Panel C: Probability of $501-$1000 in Third-Party Collections



Panel D: Probability of $1001-$2000 in Third-Party Collections



Panel E: Probability of $2000+ in Third-Party Collections

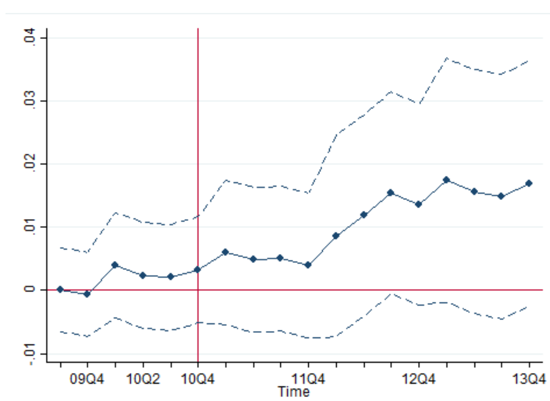


Figure 2.2: Distribution of Debt in Third-Party Collections. Note: Authors calculations using data from FRBNY Consumer Credit Panel/Equifax

collections fall in the post-period, and point estimates of large amounts ($2000+) trend positively. However, time coefficient estimates for each category are imprecise and I am unable to reject the null hypotheses that the results are different than zero. Estimates from the DID specification for the five categories of third-party collections debt are presented in Table 2.6. The results show that passage of the mandate increased the probability of an individual having $0 of debt in third-party collections in all periods after the enactment date. Most coefficients for other probabilities are statistically insignificant. Overall, the estimates from both models suggest that the mandate helped lower the amount of debt owned in third-party collections, though I cannot say with certainty that the mandate had an economically significant effect on the probability of having very large amounts in serious delinquency.

As a final robustness check, I combine county- and state-level information on uninsured and unemployment rates for young adults and form an 'exposure' variable, similar to Mazumder and Miller (2016). Using data from the CPS and SAHIE, I create an indicator variable equal to one if an individual was living in a county that was at or above the 75th percentile of the uninsured rate and a state that was at or above the 75th percentile of the unemployment rate for young adults 2009.[14] I then interact this dummy variable with the DID specification in Equation (2.4):

$$\begin{aligned} y_{itc} =& \lambda_0 + (Exposure_c \times Treated_i \times (Enact_t + Implement_t + AgeOut_t))\mathbf{\Phi} + \\ & \ldots \\ & + \mathbf{X_{it}}\mathcal{B} + \mu_i + T_t + \epsilon_{it} \end{aligned} \tag{2.5}$$

where all control variables are as defined previously and the remaining interaction

[14]According to the SAHIE data, the county-level young adult uninsured rate at the 75th percentile was 31.4% in 2009. The 75th percentile of the young adult unemployment rate in 2009 was 16%.

terms are omitted for brevity. The treatment group in the triple-difference specification is comprised of young adults born in 1985-1986 living in areas at or above the 75th percentile for both young adult unemployment and uninsured rates in 2009, while the control group for this estimation strategy consists of young adults born in 1982-1983, living in areas that were below the 75th percentile. The coefficients of interest are in the vector $\mathbf{\Phi}$, which are the triple-interactions for each of the time periods analyzed above. The DDD specification in Equation (2.5) uses variation in measures of financial distress over time, across groups, and across geography to identify the effect of the mandate.

Results from the triple-difference specification are presented in Table 2.7. Point estimates for all measures of financial distress are negative, though imprecisely estimated for all coefficients except for new bankruptcy filing. For bankruptcies, the results indicate that young adults living in areas with greater 'exposure' to the mandate experienced a decline of 0.05 percentage points in the probability of filing for bankruptcy, compared to those lived in counties with less exposure in the implementation period. This roughly translates to an annual decrease of two new bankruptcy filings per 1000 people.

Table 2.7: The Effect of the Dependent Coverage Mandate on Financial Distress: Triple-Difference Specification (Q2:2009-Q4:2013)

Coefficient	Number of Accounts in Third-Party Collections	Amount in Third-Pary Collections	Total Past Due, Revolving Accounts	New Incidence of Bankruptcy Filing	New Incidence of Third-Party Collections
Treated × Exposure × Enactment	-0.006	-28.177	-5.066	-0.0004	-0.0002
(Q2:2010-Q3:2010)	(0.009)	(27.216)	(16.678)	(0.000)	(0.002)
Treated × Exposure × Implementation	-0.010	-21.253	-8.644	-0.0005**	-0.001
(Q4:2010-Q4:2012)	(0.010)	(35.438)	(18.995)	(0.000)	(0.001)
Treated × Exposure × Age-Out	-0.014	-26.235	9.116	-0.0003	0.002
(Q1:2013-Q4:2013)	(0.013)	(41.732)	(22.778)	(0.000)	(0.002)
R^2	0.533	0.320	0.586	0.191	0.175
Num. of Obs.	7,255,544	3,381,503	5,091,669	7,258,621	7,191,476

Note: Standard errors clustered at the individual level. All regressions include individual, time, and statel fixed effects. Author's calculations using data from the FRBNY Consumer Credit Panel / Equifax.

2.6.3 Placebo Tests

If the results from the previous section are driven by a secular changes that affect all young adults, not just adults in the narrow band of birth years used in the treatment and control groups during this time period, then I would mistakenly attribute these improvement in financial distress to the mandate. To test if my results can be attributed to changes in the economic environment for young people unrelated to the passage of the mandate, I conduct a series of placebo tests by estimating Equation (2.4) for individuals that should not have been affected by the mandate. Specifically, I compare individuals born in 1982-1983 against those born in 1980-1981 and individuals born in 1980-1981 to those born in 1978-1979. If the above analysis is capturing the effect of the mandate on financial distress, and not simply a secular trend in improvement in financial distress for young adults, then estimates from Equation (2.4) using older cohorts should produce statistically insignificant results.

Results from the placebo regressions are reported in Appendix Table A1. Generally, the placebo results are statistically insignificant, with some results showing significance up to the 95% level. Given that 15 placebo regressions were run, probabilistically speaking I would expect to see a few results to have significant p-values. The notable exception is for the new bankruptcy indicator. In the placebo test comparing young adults born in 1982-1983 to those born in 1980-1981, regressions coefficients are significant for the anticipatory and short-run periods. However, for placebo tests for older young adults (birth years 1980-1981 and 1978-1979) and a narrower band of young adults (1982 and 1983), I find no statistically significant differences in the probability of new bankruptcy filings. In total, this evidence is suggestive that my regression results are not detecting an overall trend instead of an effect due to the mandate, though it should be interpreted with some caution.

2.7 Conclusion

The results from this analysis contribute to the growing body of studies that leverage large consumer credit data sets to analyze the effects of health insurance policy on financial distress. Utilizing these data for individuals affected by the dependent coverage mandate (born in 1985-1986) and not affected by the law (born in 1982-1983), I find that the ACA's dependent coverage mandate reduced financial distress for young adults across a long time period, starting in the pre-implementation period of Q2:2010-Q3:2010 and to the end of 2012. While results for some measures of financial distress do not appear to be significantly affected by the mandate, I find that debt in third-party collections and new bankruptcy filings fell in these two periods. Regressions results also suggest that once individuals aged out of the mandate after age 26, the improvements disappeared, indicating that young adults did not receive the same amount of financial protection when transitioning to individual health insurance plans.

Examining the heterogeneity in the effects of the mandate, I find that individuals living in counties with high rates of young adult uninsurance and unemployment prior to the enactment of the mandate experienced greater improvements in financial distress than persons living in counties with relatively better local economic conditions. These results suggests that mandate was effective in the geographical areas that had the most room for improvement and that providing health insurance to the young can have significant effect on their level of financial distress.

The results of the analysis have important policy implications. The provision of health insurance may generate important, welfare-enhancing benefits beyond providing access to health care. If policy makers are to properly assess the expansion or contraction of health insurance, they need to consider the impact of providing or removing health insurance on the financial distress of individuals, not just measures of physical health and access to health care.

CHAPTER 3

HEALTH INSURANCE, COMPETITION, AND BORROWING: EVIDENCE FROM THE AFFORDABLE CARE ACT'S DEPENDENT COVERAGE MANDATE**

3.1 Introduction

In this paper, we empirically investigate how health insurance can change an individuals consumption and borrowing decisions. Economic theory predicts that individuals adjust consumption and risk-taking choices following a shock to their overall level of risk. For example, the buffer stock model predicts that economic agents hold precautionary savings to insure against shocks to their incomes or expenses (Carroll, Hall, & Zeldes, 1992; Kimball, 1990; Starr-McCluer, 1996). These savings may prevent individuals from increasing consumption or making risky investment and borrowing decisions. In theory, insuring individuals against shocks to income or expenses may generate additional consumption and borrowing, thus spurring immediate economic

**This essay is based on a study by Bailey, Blascak and Mikhed (2017). The views expressed here are solely those of the authors and do not necessarily reflect the view of the Federal Reserve Bank of Philadelphia or the Federal Reserve System.

growth. We use the health insurance coverage expansion under the 2010 dependent coverage mandate of the Affordable Care Act (ACA) to test how reduced risk of health related financial expenses affects young adult household consumption and borrowing decisions.

While a large literature in medicine and public health has examined the effect of health insurance on health, economists have tended to emphasize the importance of insurance on financial distress. For example, the recent Oregon Health Insurance Experiment demonstrated that, while the health benefits of health insurance are hard to detect, providing insurance coverage alleviated individuals' financial distress. Finkelstein et al. (2012) find receiving Medicaid in Oregon led to a 25-percent decrease in the chance of having medical debt sent to collections and nearly eliminated large out-of-pocket medical expenditures. Hu et al. (2016) find a similar result for the 2014 ACA Medicaid expansions. Mazumder and Miller (2016) find that the Massachusetts health reform and health insurance expansion of 2006 reduced personal bankruptcy, third-party debt collections, and the amount of debt that was past-due while improving credit scores. However, there is limited research on the effects of an expansion in health insurance coverage on credit outcomes other than financial distress. Both economic theory and recent empirical studies suggest that health insurance may affect not only financial distress, but also financial risk taking, savings, and borrowing. Lee (2016a) finds that acquiring health insurance can lead households to reduce their precautionary savings, though this is generally considered to be a rational, efficient response to the reduction in tail risk. Goldman and Maestas (2013) and Ayyagari and He (2016) conclude that health insurance allows individuals to relocate their investment portfolios from less risky bonds to more risky stocks. A similar finding is documented by Lee (2016b) for household portfolio choices and financial risk taking after the dependent coverage mandate implementation. In addition, Bailey (2016), shows that increased risk-taking may be reflected in other, more socially positive ways, such as starting a small business.

While these studies look at risks related to investment portfolio choices or precautionary savings, other risk-taking behavior, such as borrowing, may be important in this context. In particular, young individuals may be borrowing-constrained and unable to finance their optimal levels of consumption or investment into human capital via student loans. Without insurance, young individuals may also need to search for a job earlier to get employer-sponsored insurance. All these effects combined may lead to lower consumption or investment into human capital by uninsured young adults. Indeed, Dillender (2014) finds this to be the case, estimating that the mandate led young men to acquire an additional 0.17 years of education on average. We examine whether an expansion of health insurance coverage changed consumption or borrowing of affected individuals.

To identify the effects of health insurance on individuals' consumption and borrowing choices, we use the variation in health insurance provided by the dependent coverage mandate of the ACA. This mandate was enacted in March 2010 and took effect in September 2010, requiring family health insurance plans to cover dependents of insured individuals until age 26. Previously, it was common for family plans to cover dependents only up to age 18, though many states had their own prior dependent coverage mandates which often extended coverage through age 23. The mandate increased the share of insured individuals aged 19-25 by 3.2 percentage points, and the share that received coverage as dependents by 7.0 percentage points (Akosa Antwi et al., 2013).

Based on the structure of the mandate, we define and use a simple difference-in-differences (DID) methodology to estimate the effect of the law change on borrowing and consumption by young adults. The treatment group consists of individuals aged

24-25 in 2010, when the law went into effect. We compare the outcomes of this group to the outcomes of a control group defined as individuals aged 27-28 at the time of law implementation. The data come from the Consumer Credit Panel (CCP) of the Federal Reserve Bank of New York / Equifax. This dataset is a representative panel of individuals in the U.S. with social security numbers and at least one credit or public record.

We look at four broad types of consumer credit: credit cards, auto loans, student loans, and first lien mortgages. These types of credit are most frequently used by individuals in the U.S. to finance their consumption and investment into cars, human capital, and real estate. For all these types of credit, we examine the effect of the mandate along both the extensive and intensive margins. We also consider credit applications to examine whether individuals applied for credit more after the ACA implementation. To investigate how individuals perform on new loans, we look at measures of default, such as 30 days past due occurrences, the number of loans current, and amounts current and past due. We also consider credit card line limits and loan sizes for auto and student loans and mortgages to examine if lenders supplied insured individuals with additional credit.

We find that after the health insurance expansion, covered young adults applied for more credit than individuals in the control group. Individuals affected by the law also increased their number of active credit cards and credit card balances. We also find that lenders provided these individuals with larger lines of credit. In terms of performance, we do not find any increase in the number of credit cards 30 days past due, but we find that the number of cards current and the amount on cards current grew after the mandate implementation.

The estimates suggest qualitatively similar effects in auto loans, with increases in the number of auto loans and balances. There is no deterioration in the performance of auto loans, with the number of loans current and amount on loans current both increasing. We observe no statistically significant change in the number of 30 days past due occurrences on auto loans. Total auto loan size also increased after the law implementation.

We find increases in the number, total credit, and total unpaid balances on student loans while detecting no statistically significant change in the amount past due on student loans. However, we interpret the student loan results with some caution, as evidence from parallel trends and placebo tests indicate the results may not be robust to omitted factors. There was little effect of the law on first lien mortgages. We find only small reductions in the number of first lien mortgages after individuals covered by the mandated aged out of the coverage (after they turned 26 years old). There is also a small reduction in the number of first mortgages in bankruptcy which may be due to additional liquidity provided by health insurance.

Overall, these findings imply that health insurance coverage allows individuals to expand their consumption, borrowing, and investment. Thus, to evaluate the total effect of a health insurance expansion, it is not sufficient to look only at health-related effects of such an expansion. Health insurance does not only reduce financial distress of insured individuals, as shown by previous studies on this topic, but also it may allow them to better smooth their consumption and possibly to invest into human capital through the use of student loans.

3.2 Conceptual Framework

To examine the effect of the dependent coverage mandate on consumption, we adopt the framework of Kimball (1990) and Carroll (1997) and present a simple two-period consumption/savings model in which consumers are assumed to be impatient and face both income uncertainty and a borrowing constraint. Within the context of the mandate, we do not explicitly model the purchasing of health insurance, since young adults receive health insurance coverage through their parents' plan. We assume that the individual has an additively time-separable utility function and faces exogenous interest rates r. Income in the first period, y_1, is assumed to be given, and income in the second period is stochastic such that $y_2 = \tilde{y} + \epsilon_2$, where $\tilde{y}$ is the mean income and $\epsilon_2 \sim (0, \sigma_{\epsilon_2})$. Individuals choose consumption C_1 and level of investment in a liquid asset b in period 1 and C_2 in period 2 and assume that individuals incur medical expenditure M_t in each period. Therefore, individual i is faced with with the following maximization problem:

$$\max_{c_1,c_2,b} u(c_1) + \beta \mathbb{E}[u(c_2)] \tag{3.1}$$

$$s.t.$$

$$c_1 + b + M_1 = y_1 \tag{3.2}$$

$$c_2 + M_2 = y_2 + (1+r)b \tag{3.3}$$

$$b \leq -L \tag{3.4}$$

where $u'(c_t) > 0, u''(c_t) < 0, u'''(c_t) > 0$ for $t \in \{1, 2\}$ and $R \equiv 1 + r$. The assumption of a positive third derivative produces the necessary incentive for consumers to engage in precautionary savings.[1] Equation (3.4) describes the borrowing constraint, which takes the form of an exogenous credit constraint L.[2] For simplicity, we assume that medical expenditure M_t is an exogenously determined scalar. Substituting the first and second period budget constraints into the maximization problem yields the

[1]This assumption implies that marginal utility is convex.

[2]The credit limit is $-L$ because borrowing in the two-period model is represented by negative values of b, while positive values of b represent lending by the consumer.

Lagrangian:

$$\mathcal{L} = u(y_1 - b - M_1) + \beta\mathbb{E}[u(Rb - y_2 - M_2)] + \lambda(b + L) \tag{3.5}$$

Solving for the optimality condition gives the credit constrained Euler equation,

$$u'(y_1 - b - M_1) = \beta\mathbb{E}[u'(Rb + y_2 - M_2)] + \lambda \tag{3.6}$$

where λ in Equation (3.5), the Lagrange multiplier on the borrowing constraint, is the shadow value of increasing the credit limit by \$1. If the borrowing constraint is non-binding, then $\lambda = 0$ and the Euler equation between the first and second periods is satisfied. If the borrowing constraint is binding, then the consumer may not be able to finance all first period consumption and the marginal utility of consuming today will be greater than the marginal utility of consuming tomorrow. This implies that the consumer saves less and must consume his or her current income.[3]

The discount factor β also determines how much the consumer borrows in the first period. If β is sufficiently low[4], then the marginal value of consuming in the second period will be lower. This leads the agent to consume more in the first period. However, as Carroll (1997) explains, individuals will not drawn down their entire first period resources, as the income uncertainty in the second period generates a *precautionary savings motive*, which leads individuals to save some of their first period resources in the event of a lower income realization. These two incentives are key to

[3]The constrained consumer is said to consume *hand-to-mouth* because she consumers all of her current period income.

[4]The model in Carroll (1997) was calibrated with a discount factor of 96% (or a discount rate of 4 percent). Gourinchas and Parker (2002) estimated discount rates of 4-4.5%. Carroll and Samwick (1997) estimated discount rates between 10-15% while Aydin (2016) assumed a discount factor of 90% (a discount rate of approximately 11%).

generate *buffer-stock savings* behavior: individuals, because they are impatient, wish to consume more in the first period. However, due to the uncertainty of second period income, will 'save for a rainy day.' To balance these two divergent incentives, "buffer-stock savers" will have a target amount of resources they wish to hold. Depending on which incentive is stronger, the consumer will either save or borrow.

Using Equation (3.6), we can derive a number of testable implications from the simple model. First, if access to health insurance reduces medical expenditures in the second period, M_2, then the agent will experience an income effect and consumption increases and saving decreases in the first period. A low β pushes individuals to consume and borrow even more in the first period. However, the liquidity constraint λ dictates the increase in first period consumption. If the borrowing constraint is binding in the first period, a reduction in M_2 will not increase consumption at all. If the decrease in M_2 causes the individual to increase borrowing to increase consumption, which in turns causes the borrowing constraint to bind in the first period, then the increase in first period consumption is dampened.

Young adults that gained health insurance through the implementation of the dependent coverage mandate experienced two income effects. For those individuals that were uninsured and paying medical costs out-of-pocket (OOP), gaining insurance directly reduces the amount of money paid through lower OOP costs. Individuals that previously had own-name ESI coverage prior to the mandate and then switched to parental ESI, also experienced an income effect. We would expect to see a reduction in medical expenditures, though not as significant an improvement as those that were previously uninsured.

If we assume that medical expenditure risk increases the uncertainty of future income for consumers, we can then interpret the provision of health insurance as a reduction in the uncertainty of second period income. This implies that access to health insurance leads to a *risk effect* and first-period consumption will subsequently increase. To illustrate this, suppose a mean-preserving spread of second period income y_2. Given that we previously assumed that u' is convex (i.e. $u''' > 0$), then by Jensen's inequality we can re-write Equation (3.6) as

$$u'(y_1 - b - M_1) < \beta \mathbb{E}[u'(Rb + y_2 - M_2)] + \lambda \tag{3.7}$$

The increased uncertainty of income in the second period causes the marginal utility of consuming in the second period to be higher than in the first period, which causes savings (in the form of b) to increase and consumption to decrease. This increase in savings in the first period is known as *precautionary savings*. If income uncertainty in the second period decreases, then the opposite result emerges, with the marginal utility of first period consumption higher than in the second period, leading to an increase in first period consumption and decreased saving. For the *risk effect*, a low β leads to the same behavior as with the income effect: increased consumption and borrowing in the first period, as the marginal value of consuming in the second period is lower with a lower β. However, as with the reduction in medical expenditure M_2, the magnitude of the increase in first period consumption is dictated by the liquidity constraint. If the borrowing constraint is binding in the first period, or becomes binding as the agent increases first-period consumption, the increase will be restricted by the credit limit and the agent's consumption will be not be optimal.

For those young adults who became covered under their parents' insurance health plans, there were two levels of the risk effect. Those individuals transitioning from being uninsured to insured experienced the greatest risk reduction, reducing the maximum exposure to large medical expenditures. Individuals that moved from own-name ESI to a parent's health insurance also experienced a risk reduction, albeit at a lower level since they were previously covered. For these young adults, this more tempered reduction occurs through either cost savings via reduced deductibles or premiums or through increased quality of coverage (or a combination of both).

In summary, based on the framework developed above, we form the following hypotheses regarding consumption behavior after the passage of the mandate:

1. If individuals are borrowing constrained, increases in first-period consumption due to either the *risk effect* or the *income effect* will be limited by the constraint.

2. If the provision of health insurance reduces future income uncertainty, we expect to see a *risk effect* that results in increased current period consumption as the marginal value of consuming in the second period decreases. However, the level of the increase is ambiguous.

3. If access to health insurance reduces medical expenditures, we expect to see an *income effect* that results in an increase in first-period consumption. The level of increase in consumption is also ambiguous.

Empirically, we test hypotheses 2 and 3 regarding both the sign of the effect and obtain estimates of the magnitudes. Hypothesis 1, while not estimated, provides context for our estimates by helping us understand where in the distribution our effects are.

3.3 Data

The data for this chapter come from the Federal Reserve Bank of New York / Equifax Consumer Credit Panel (CCP). We use the same criteria as in the previous chapter to select a sample of young individuals born in 1982, 1983, 1985, and 1986 from this data set. Those individuals who were born in 1982-1983 form our control group because they were 27 and 28 years old in 2010 when the ACAs dependent coverage mandate was enacted and implemented (26 year olds are excluded as partially treated, because while the mandate does not cover them the ACA extended the tax deductibility of dependent coverage through age 26). Individuals born in 1985 and 1986 are included in our treatment group because they were 24 and 25 years old at the time of the law passage and they were covered by the ACA. We track credit characteristics of these two groups between Q2:2009 and Q4:2013. We also impose the same set of restrictions as in the previous chapter to select this group of individuals.

In this study, we focus on four broad types of consumer credit variables which may be affected by the expansion in health insurance coverage from the ACA. In particular, we examine how young people covered by the mandate changed their behavior with credit cards, auto loans, student loans and mortgages after the law was implemented. These four types of credit are most widely used by borrowers, and thus, we hypothesize that these credit variables may be affected by the expansion in health coverage. Tables 3.1 and 3.2 summarize the credit variables used in this study.

For these types of credit, we look at various margins of adjustment. First, we examine the extensive margin by tracking changes in the total number of loans of a particular type, as well as measures of short and long term delinquencies on these loans, such as the number of 30 days past due occurrences, and the number of loans current or in bankruptcy. Second, we explore whether there were any effects along the intensive margin by looking at such variables as amounts of particular credit type that are non-delinquent and overall loan balances. Finally, we investigate whether lenders reacted to the potential improved liquidity situation of young borrowers by providing them with higher credit limits and increasing loan sizes.

We also focus on certain variables or subtypes of credit more suitable or interesting for our analysis. In addition to credit card variables, we look at the number of credit

Table 3.1: Summary Statistics: Treatment Group

Variable	Obs	Mean	Std. Dev.
Credit applications	1,639,193	0.64	1.126
Number of active credit cards	1,639,193	2.05	1.761
Number of credit cards 30 days past due	1,639,193	0.035	0.225
Amount of bank card current	1,414,056	2382.05	3798.68
Credit card balance	1,639,193	2627.44	8282.75
Retail card credit limit	965,461	2666.69	3037.8
Number of auto bank loans	1,639,193	0.274	0.503
Number of auto bank loans current	1,639,193	0.265	0.496
Number of auto bank loans 30 days past due	1,639,193	0.003	0.056
Amount of auto bank loans current	394,431	14105	10232.26
Total auto bank balance	405,466	14024.76	10354.82
Total auto bank loan size	405,466	19158.49	11326.74
Number of first mortgage loans	1,639,193	0.173	0.394
Number of first mortgage loans current	1,639,193	0.165	0.385
Number of first mortgage loans in bankruptcy	1,639,193	0.0002	0.016
Amount of first mortgage loans current	262,709	147000.6	83438.59
Total first mortgage balance	274,901	147698.8	85025.71
Total first mortgage loan size	274,901	152547.3	87145.83
Number of student loans	1,639,193	1.848	3.237
Total credit on student loans	1,639,193	12186.6	26886.74
Past due amount on student loans	1,639,193	425.15	4180.25
Student loan total unpaid balance	1,639,193	11900.05	26576.02

Note: Authors' calculations using data from the FRBNY Consumer Credit Panel/Equifax. Data is for full sample period, Q2:2009-Q4:2013.

applications (inquiries) within last three months. This variable may show whether individuals are applying for credit. We also consider the number of active credit cards, defined as revolving accounts with an update within the last three months, and a positive balance. Using these measures allows us to see if credit cards are used more often, and by how much. We also we consider subtypes of these kinds of credit cards separately bu examining changes on bank credit cards and department store (retail) cards.

Table 3.2: Summary Statistics: Control Group

Variable	Obs	Mean	Std. Dev.
Credit applications	1,780,161	0.639	1.111
Number of active credit cards	1,780,161	2.262	1.938
Number of credit cards 30 days past due	1,780,161	0.035	0.227
Amount of bank card current	1,557,171	3445.42	5898.42
Credit card balance	1,780,161	3981.27	9765.09
Retail card credit limit	1,073,000	3138.53	3575.76
Number of auto bank loans	1,780,161	0.308	0.536
Number of auto bank loans current	1,780,161	0.299	0.53
Number of auto bank loans 30 days past due	1,780,161	0.003	0.057
Amount of auto bank loans current	472,010	15256.53	11257.07
Total auto bank balance	483,692	15165.62	11254.88
Total auto bank loan size	483,692	21604.55	12877.4
Number of first mortgage loans	1,780,161	0.33	0.523
Number of first mortgage loans current	1,780,161	0.31	0.511
Number of first mortgage loans in bankruptcy	1,780,161	0.0009	0.032
Amount of first mortgage loans current	517,980	170209.9	105989.2
Total first mortgage balance	546,807	171396	107895.1
Total first mortgage loan size	546,807	178290	115303.8
Number of student loans	1,780,161	1.536	3.005
Total credit on student loans	1,780,161	13275.26	29692.39
Past due amount on student loans	1,780,161	490.24	5032.23
Student loan total unpaid balance	1,780,161	12476.73	30294.83

Note: Authors' calculations using data from the FRBNY Consumer Credit Panel/Equifax. Data is for full sample period, Q2:2009-Q4:2013.

For auto loans, we focus exclusively on loans are extended by banks but not specialized finance companies. Even though this distinction is rough, auto bank loans offer an advantage, as they are less volatile than auto finance loans in our sample of young adults. We also consider a set of combined auto loan variables. Most of our results are qualitatively similar for these combined categories of auto loans. However, these combined variables are rather volatile as well and do not always pass parallel trends tests.

Our student loan data consist of all student loan types and lenders including federal and state institutions, private lenders such as banks, credit unions, schools, etc. We also include joint and shared student loans. The mortgage variables we use are based on first lien mortgages. For short, we denote them first mortgages. While the majority of people in our sample will have at most only one first lien mortgage, in principle it is possible for them to have multiple properties and thus multiple first mortgages on these properties.

3.4 Methodoloy and Results

3.4.1 Parallel Trends Tests and Graphical Evidence

To test the validity of the main identifying assumption of the DID methodology, that the treatment and control groups have parallel trends before the treatment period, we conduct a series of tests. The first test is based on the following specification:

$$y_{it} = \gamma_0 + T_t \times Treated_i \Gamma + T_t \Pi + \gamma_2 Treated_i + X_{it}\beta + \zeta_{it} \tag{3.8}$$

where we interact the treatment age group indicator (Treated) with time indicators (T) for each quarter in our data. We omit the first time period dummy (Q2:2009) to avoid multicollinearity. Hence, these coefficients should be interpreted as changes in the outcome variable relative to the omitted period. This specification also includes all time period and treatment indicators without the interaction. In addition, we control for age, birth year, age × birth year fixed effects, state fixed effects, state-specific linear time trends, uninsurance and poverty rates at the county level, state unemployment rates for the populations aged 19-25 and 26-34, and the interaction of the treatment indicator with the overall state unemployment rate. We run this specification separately for each of our outcomes of interest (y) as described in the previous section.

Figures 3.1 to 3.4 summarize the estimated coefficients on the interactions of the calendar time indicators with the treatment group indicator from equation (3.8). Each panel of these figures shows the results for one variable of interest as indicated in the title of the panel. The coefficients in these panels show the difference between the treatment and control groups before the law's passage (Q2:2009Q1:2010), after the treatment group was mandated to receive health insurance by the ACA (Q2:2010Q4:2012), and after the treatment group aged out of the mandate (after turning 26 years old). The solid lines in Figures 3.1 to 3.4 show estimated coefficients, while the dashed lines represent 95 percent confidence intervals.

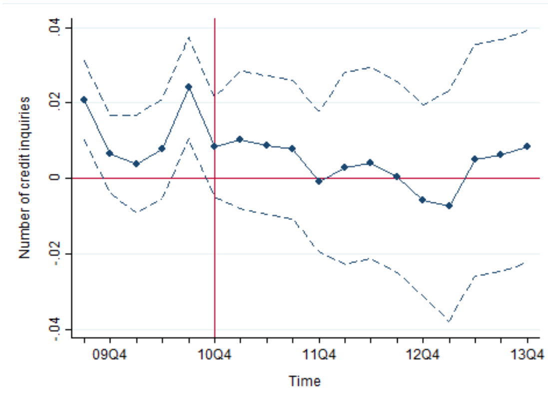
Figure 3.1 plots the coefficients for credit applications (inquiries), number of active credit cards, number of cards 30 days delinquent, amount of bank cards current, credit card balance, and retail credit card limit. The plots for these outcomes of interest show that before the law enactment, differences between the treatment and control groups are economically small and not statistically different from zero. This finding suggests that both treatment and control groups had similar trends in the period before the enactment of the mandate (Q3:2009-Q1:2010).

Figure 3.1 also suggests that the credit card variables began to diverge in the treatment and control group after the enactment and implementation of the mandate. In particular, the number of credit applications increased immediately after the law enactment, but subsequently returned to pre-enactment levels. The number of active credit cards increased after enactment and continued to increase throughout the sample period. While the number of 30-days-past-due occurrences increased after the law passage, the economic magnitude of this increase is very small. The amount of bank card balances current (shown in panel D) increased on average by \$50 during the law implementation and decreased by $100 after the affected population aged out of the health insurance mandate. Finally, panels E and F of Figure 3.1 show that credit card balances and retail credit card limits increased by $150 and $100, respectively, during the period of the ACAs mandate coverage for 24-25 year old individuals. Overall, these preliminary results suggest that persons covered by the law mandate used their credit cards more, applied for credit more and had higher credit card balances and limits compared to the control group of similar young individuals not covered by the health insurance mandate.

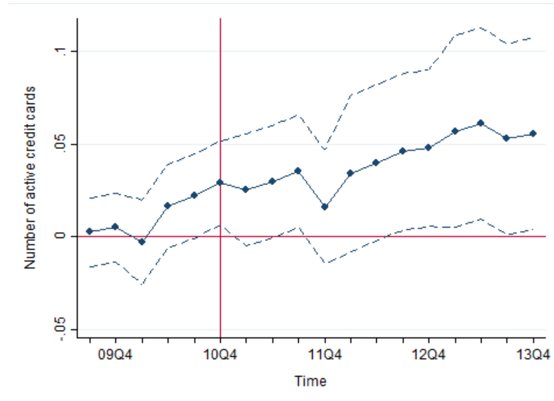
Figure 3.2 summarizes changes in the auto bank variables before and after the implementation of the mandate in the treatment and control groups. In particular, this figure shows the total number of auto bank loans, the number of auto bank loans current, and 30 days past due occurrences for this type of credit. We also plot the amount of auto bank loans current, total balance, and loan size. Similar to Figure 3.1, Figure 3.2 indicates that the treatment and control groups have parallel trends in these credit variables before the treatment (Q2:2010), which supports the parallel trends assumption of the DID methodology. In addition, individual panels in Figure 3.2 reveal that the number of auto bank loans and the number current auto bank loans appear to grow after the implementation of the mandate for the treatment group compared to the control group. Panel C of Figure 3.2 shows that the number of auto bank 30 days past due decreased for the treated group while covered under the mandate (Q4:2010-Q4:2012). On the other hand, the amount of bank auto loans current, total balance, and loan size do not seem to show large changes during the treatment period. Overall, these results suggest that the health insurance coverage motivated individuals to obtain additional auto loans and stay current on these loans.

The differences between the treatment and control groups in student loan variables available in our data are presented in Figure 3.3. The four panels of this figure show

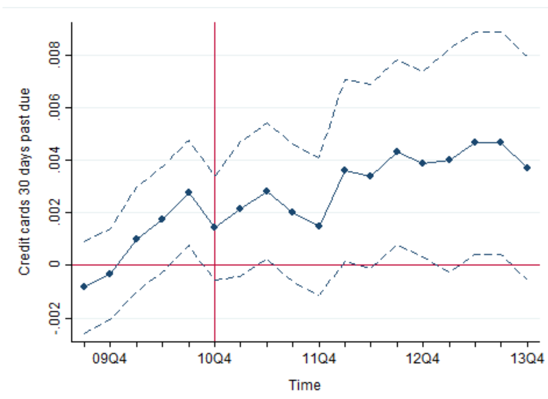
Panel A: Credit Applications



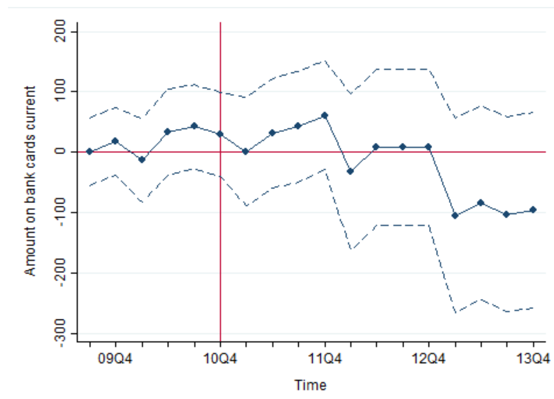
Panel B: Number of active credit cards



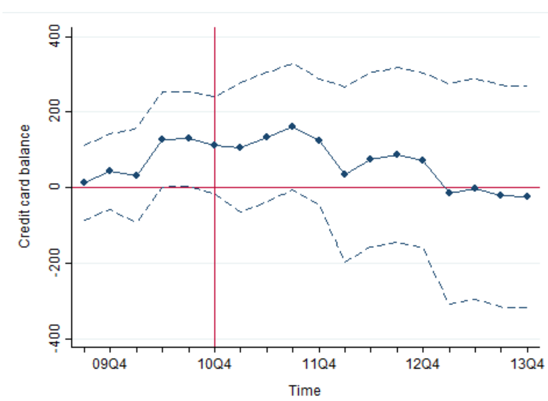
Panel C: Number of credit cards 30 days past due



Panel D: Amount on bank cards current



Panel E: Credit card balance



Panel F: Retail card credit limit

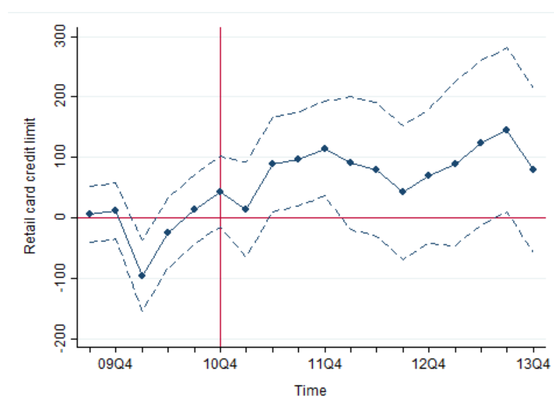
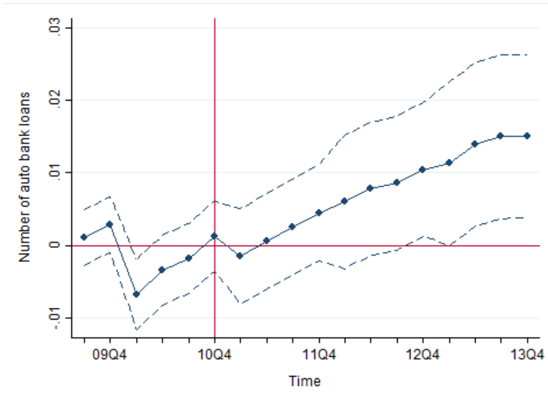
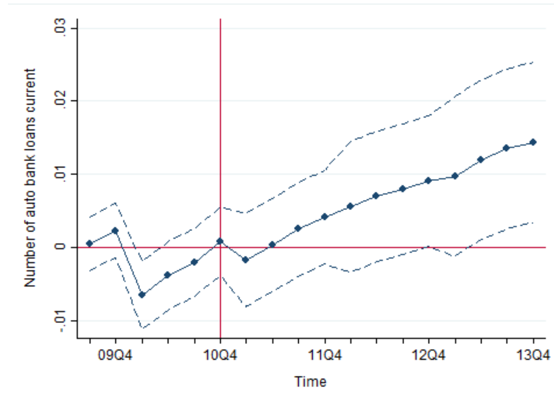


Figure 3.1: Application for Credit and Credit Card Borrowing after the Mandate. Note: Authors calculations using data from FRBNY Consumer Credit Panel / Equifax.

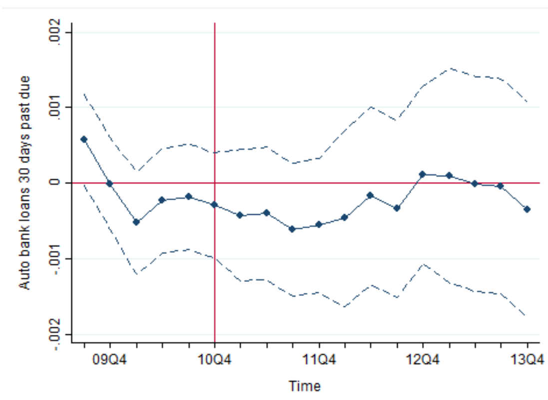
Panel A: Number of auto bank loans



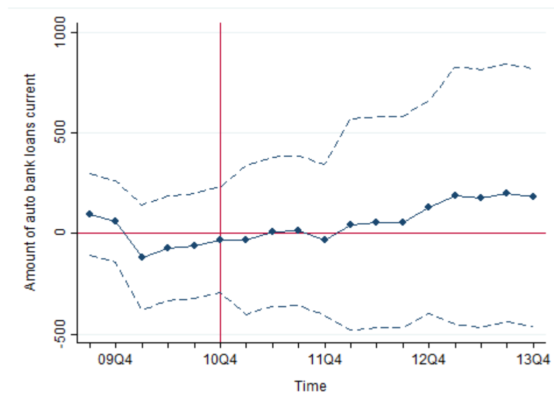
Panel B: Number of auto bank loans current



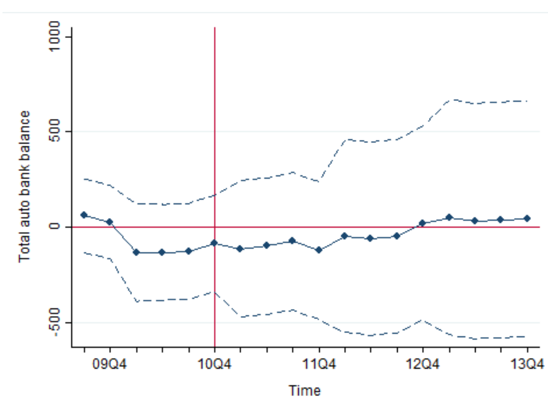
Panel C: Number of auto bank loans 30 days past due



Panel D: Amount of auto bank loans current



Panel E: Total auto bank balance



Panel F: Total auto bank loan size

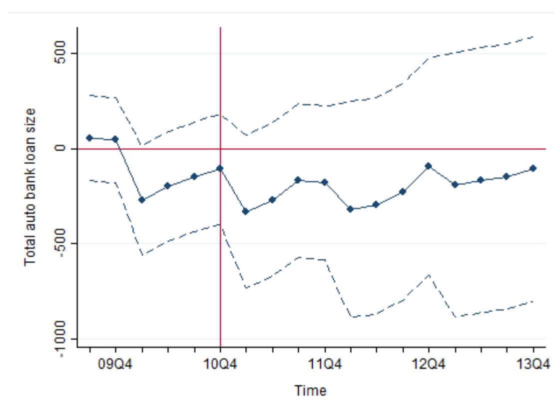
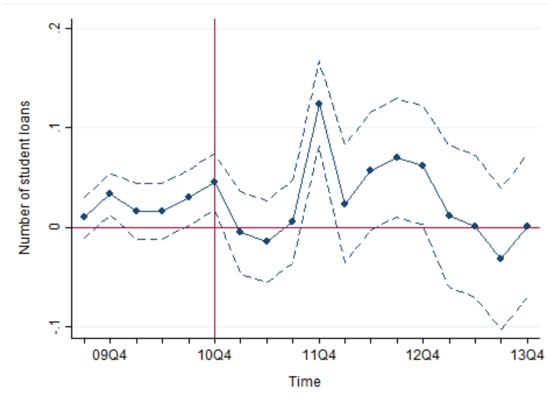


Figure 3.2: Auto Bank Credit After the Mandate. Note: Authors calculations using data from FRBNY Consumer Credit Panel / Equifax.

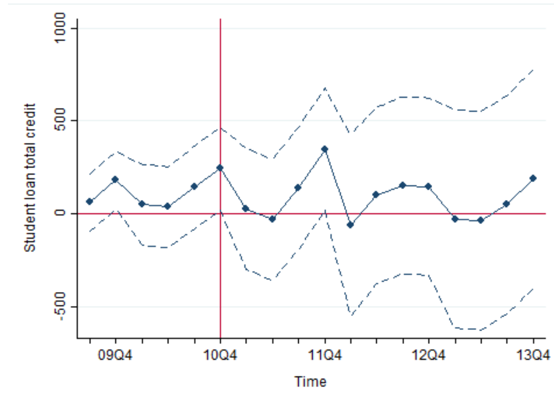
changes in the number of student loans, amount of total credit on student loans, past due amount on these loans, and the total unpaid balance. Figure 3.3 suggests that both treatment and control groups have similar trends in these variables before the law implementation. However, after the law went into effect in Q4:2010, individuals covered by the law increased their student loan borrowing by carrying more student loans (panel A) and higher total credit on student loans (panel B). Unpaid balances on student loans (panel D) also increased in this period, while past due amount showed little change (panel C). Taken together, these trends may suggest that health insurance coverage may encourage additional student loan borrowing which may lead to investments into education and human capital, however we cannot fully reject the possibility of differential trends due to unobserved characteristics or trends.

Changes in first lien mortgages before and after the mandate was implemented are presented in Figure 3.4. As can be seen from this figure, the treatment and control groups have parallel trends in the pre-implementation period for the number of mortgages, the number of mortgages current, and mortgages in bankruptcy. The two groups trend similarly in the amounts of mortgage loan current, total balance, and loan size. Panels A and B of Figure 3.4 show that the number of mortgages and the number of mortgages current began to decline after the mandate was implemented in Q4:2010, but experienced a moderate recovery in the 2012-2013 period. Panel C shows that the number of first mortgages in bankruptcy decreased for the treated group compared to the controls after the mandate was enacted. For the measures along the intensive margin, both the amount of mortgage loans current and total balance seem to trend downward after the law implementation (panels D and E). Finally, Panel F of Figure 3.4 indicates that mortgage loan size declined. These findings imply that treated individuals did not increase, or in some cases decrease, their borrowing with mortgages.

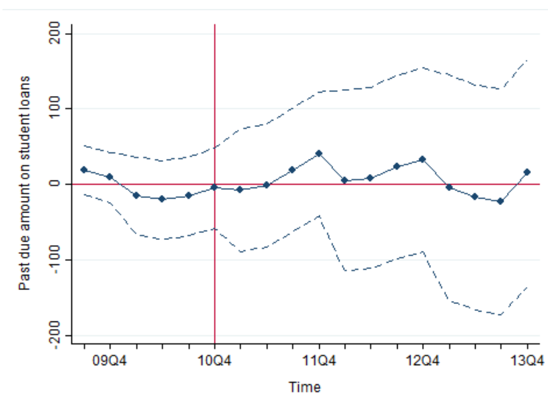
Panel A: Number of student loans



Panel B: Student loan total credit



Panel C: Past due amount on student loans



Panel D: Unpaid balance on student loans

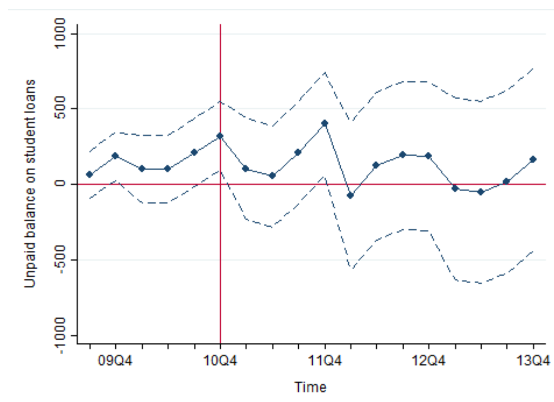
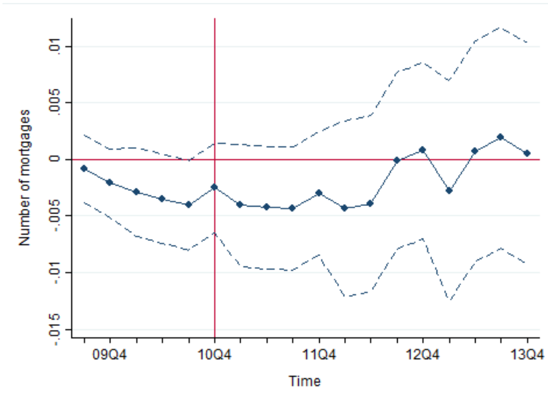


Figure 3.3: Student Loan Borrowing after the Mandate. Note: Authors calculations using data from FRBNY Consumer Credit Panel / Equifax.

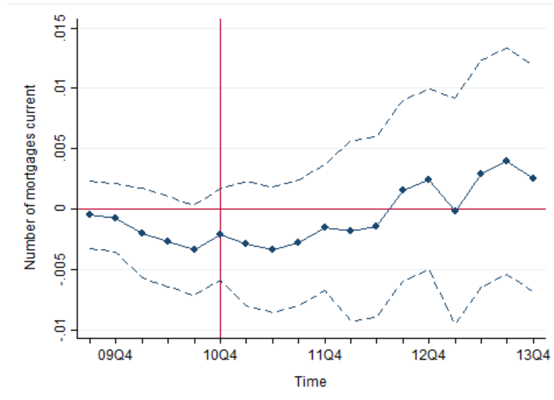
In addition to inspecting graphs of credit variables to determine whether treatment and control groups have similar trends before the treatment period, we formally test the parallel trends assumption using a regression model. We specify the following model and fit on pre-treatment data from Q2:2009-Q1:2010:

$$y_{it} = \delta_0 + \delta_1 Treated_i \times Time + \delta_2 Treated_i + \delta_3 Time + X_{it}\beta + \nu_{it} \tag{3.9}$$

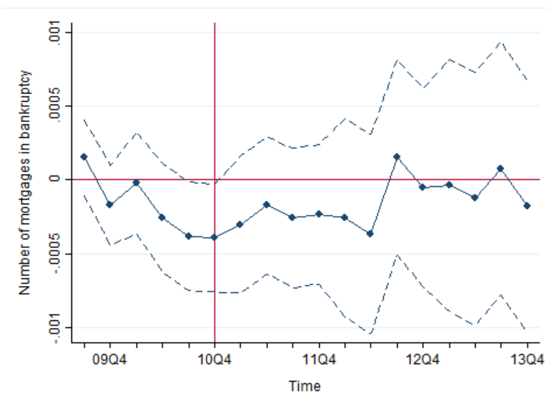
Panel A: Number of first mortgage loans



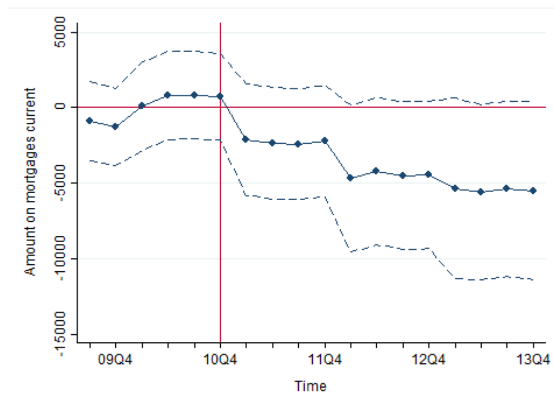
Panel B: Number of first mortgage loans current



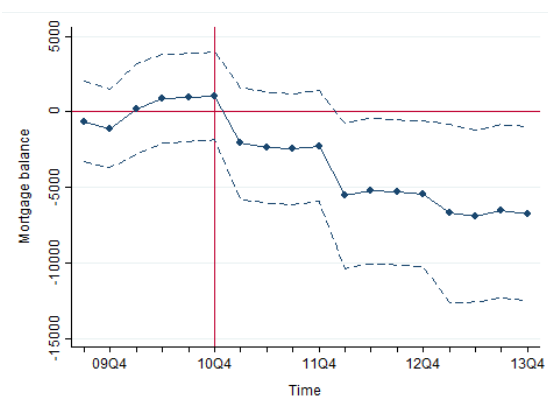
Panel C: Number of first mortgage loans in bankruptcy



Panel D: Amount of first mortgage loans current



Panel E: Total first mortgage balance



Panel F: Total first mortgage loan size

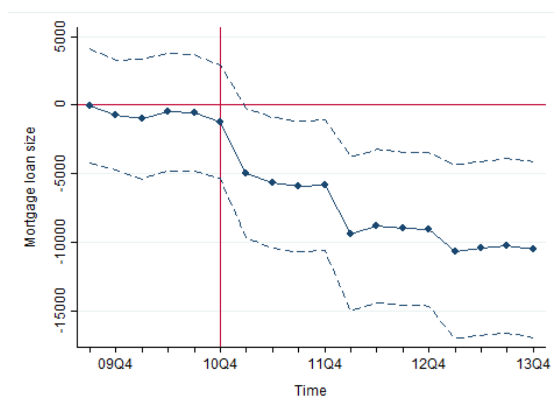


Figure 3.4: First Mortgage Credit after the Mandate. Note: Authors calculations using data from FRBNY Consumer Credit Panel / Equifax.

where $Time$ is a linear time trend and all other variables are defined as in equation (3.8). We use this specification to test whether there is a difference in linear trends in the treatment and control groups in the pre-treatment period (prior to Q2:2010).

Table 3.3 summarizes the results of this test for all credit variables considered in the paper. As can be seen from the second column of Table 3.3, most differences in linear trends between the treatment and control groups are very small economically. Furthermore, except for student loan variables, none of the differences in linear trends in the treatment and control groups are statistically different from zero at the conventional levels. Even for student loan variables, only the difference in the number of student loans is significant at the 5 percent level. Differences in the other student loan variables are only significant at 10 percent level or not significant at any level.

3.4.2 Main Results

We use a standard difference-in-differences model (DID) to estimate the effect of the dependent coverage mandate on credit outcomes of young adults. In this model, the effect of the mandate is equal to the difference between the treatment and control groups before and after the law implementation. To causally identify the effect of the mandate on credit outcomes, we estimate the following regression model:

$$\begin{aligned} y_{it} = &\alpha_0 + \alpha_1 Treated_i \times Enact_t + \alpha_2 Treated_i \times Implement_t + \\ &\alpha_3 Treated_i \times AgeOut_t + \alpha_4 Treated_i + \alpha_5 Enact_t + \alpha_6 Implement_t + \\ &\alpha_7 AgeOut_t + X_{it}\beta + \mu_i + T_t + \epsilon_{it} \end{aligned} \tag{3.10}$$

We specify the effect of the law flexibly and allow it to vary in the enactment phase (Q2:2009Q3:2009), the implementation phase (Q4:2010Q4:2012), and the age-out period (Q1:2013Q4:2013). We also include calendar time fixed effects, age fixed

Table 3.3: Test of Parallel Trends

Variables	Difference in linear trends	Standard error	No of obs.
Credit applications	0.0032	(0.003)	1,235,838
Num of active credit cards	0.0025	(0.005)	1,063,045
Num of credit cards 30 days past due	-0.0002	(0.0004)	1,474,857
Amount of bank card current	8.52	(14.32)	756,056
Credit card balance	21.18	(25.47)	873,138
Retail card credit limit	5.69	(11.87)	553,582
Num of auto bank loans	0.0014	(0.001)	1,474,857
Num of auto bank loans current	0.0012	(0.001)	1,474,857
Num of auto bank loans 30 days past due	-0.000002	(0.0002)	1,474,857
Amount of auto bank loans current	27.79	(51.38)	228,709
Total auto bank balance	12.47	(49.29)	247,910
Total auto bank loan size	20.88	(57.11)	247,910
Num of first mortgage loans	-0.001	(0.001)	1,474,857
Num of first mortgage loans current	-0.0004	(0.001)	1,474,857
Num of first mortgage loans in bankruptcy	-0.0001	(0.0001)	1,474,857
Amount of first mortgage loans current	-748.61	(649.17)	144,059
Total first mortgage balance	-670.28	(665.42)	160,664
Total first mortgage loan size	-507.67	(1,014.24)	160,664
Number of student loans	0.0166**	(0.005)	1,539,127
Total credit on student loans	89.58*	(40.08)	1,539,127
Past due amount on student loans	4.33	(8.60)	1,539,127
Student loan total unpaid balance	92.47*	(40.65)	1,539,127

Note: Hubert-White SEs reported in parentheses.Author's calculations using data from the FRBNY Consumer Credit Panel / Equifax.

effects and the interaction of age and cohort fixed effects to allow each cohort of individuals to have its own life-cycle profile in credit variables. Time fixed effects are included to capture any seasonal and business cycle fluctuations. We control for individual fixed effects to capture any time-invariant individual characteristics which may affect credit variables. All reported standard errors are robust and corrected for heteroskedasticity.

It is important to emphasize that our estimates are intent-to-treat effects (ITT). Because our data do not contain any information on individual-level insurance status for young adults or their parents, we cannot see if this status changed after the law was adopted or whether parents had insurance to which they could add their eligible dependents. While intent-to-treat effects are important, they are not treatment-on-the-treated (TOT) effects, which would show how much health insurance coverage affected newly enrolled individuals. In other words, ITT effects show only the effect of the law mandate, not the effect of expanded health coverage on the treated individuals. Many individuals intended to be treated by the law change, in reality, would not be treated due to having their own insurance before the law implementation, or not having parents with health insurance, among other possibilities. It is possible to calculate a back-of-the-envelope estimate of the TOT effect from ITT effect we provide by multiplying our estimates by 14. This calculation assumes that the law increased the share of the treatment group that received health coverage as dependents by 7 percentage points as in Akosa Antwi et al. (2013).

Tables 3.4 through 3.7 present the coefficients for the interactions of the treatment group indicator with the three time period indicators, i.e. enactment, implementation and age-out. These tables summarize the effects of the health insurance coverage expansion on the 22 credit variables covering credit cards, auto bank loans, student loans, and mortgages. In addition to the coefficients, Tables 3.4 through 3.7 report the average values of the corresponding credit variables in each of the time periods of interest to highlight the economic significance of the estimated effects.

Table 3.4 shows the effects of expanding health insurance coverage on young individuals for credit applications (inquiries), number of credit cards, number of cards 30 days past due, amount on bank cards current, credit card balance, and retail credit card limits. This table suggests that young persons affected by the law applied for credit more often, with the highest and the only statistically significant increase in the enactment phase. On average, credit applications increased by 0.012 in this period, which is a 1.8 percent increase relative to the base of 0.661 inquiries per person per quarter in this period. While credit applications increased mostly in the enactment phase, the number of active credit cards, defined as cards with an update within the last three months and a positive balance, increased in all periods of the law implementation. On average, these cards grew by 0.023 in the enactment phase, 0.027 in the treatment period, and 0.039 in the post-treatment. These changes constitute 1.1, 1.2, and 1.8 percent increases relative to the averages in the respective periods. While there were more applications for credit and higher use of available credit cards by individuals covered by the ACA, the number of credit card delinquencies (30 days past due occurrences) barely changed after the law implementation. Thus, it seems that affected young borrowers continued to use their credit cards responsibly.

The three intensive margin measures of credit card activity presented in columns 4 to 6 of Table 3.4 show that the health insurance coverage mandated by the ACA increased dollar value of borrowing on credit cards. In particular, amounts on bank cards current increased by statistically significant \$50 and \$38, or 1.8 and 1.3 percent, in the enactment and implementation periods, respectively. After the expansion in health insurance, affected individuals also increased their credit card balance by an average of \$93 (3%) and \$58 (1.8%) in the enactment and implementation phases. Finally, as the last column of table 3 shows, lenders increased credit card line limits to the covered individuals in all three phases of the law expansion by \$93, \$142 and \$178. Since credit card credit limits are often considered as a measure of credit supply, we can conclude that lenders increased credit supply to individuals eligible to receive health insurance coverage under the mandate.

Table 3.4: Main Results: Credit Cards

Coefficient	Credit applications	Num of active credit cards	Num of credit cards 30 DPD	Amount of bank card current	Credit card balance	Retail card credit limit
Enactment	0.0121**	0.0232***	0.0014*	49.88***	92.88***	93.30***
(Q2:2010-Q3:2010)	(0.0043)	(0.0046)	(0.0006)	(15.14)	(21.28)	(12.88)
Implementation	0.0053	0.0265***	0.0006	37.77*	58.30*	142.31***
(Q4:2010-Q4:2012)	(0.0050)	(0.0051)	(0.0007)	(17.48)	(27.27)	(14.43)
Age-out	0.0100	0.0391***	0.0009	-39.46	-37.70	177.99***
(Q1:2013-Q4:2013)	(0.0069)	(0.0070)	(0.0010)	(25.69)	(36.08)	(20.49)
Avg in Enactment	0.661	2.08	0.035	2788.5	3071.3	2682.0
Avg in Implementation	0.657	2.16	0.034	2904.4	3288.9	2817.1
Avg in Age-out	0.628	2.22	0.033	3141.5	3671.6	3303.3
R-squared	0.2179	0.7133	0.1137	0.6552	0.7661	0.6895
Observations	5,782,847	5,090,248	7,034,512	3,740,623	4,236,374	2,577,996

Notes: This table shows the effect of the dependent coverage mandate on credit variables of young adults covered by the mandate (24-25 years old in 2010) compared to a control group of young adults not covered by the law (26-27 years old in 2010). Only the coefficients on the interaction of the treatment indicator with enactment, implementation, and age-out periods are reported. Hubert-White robust standard errors are reported in parentheses. Author's calculations using data from the FRBNY Consumer Credit Panel / Equifax.

Taken together, these results for credit card variables and credit applications imply that eligible individuals who were targeted to receive health insurance coverage from the mandate shopped for credit and used their credit cards more than before. As credit card spending is a measure of consumption expenditure, one important implication of this result is that providing individuals with health insurance may allow them to increase consumption. This finding is consistent with the predictions of our precautionary savings model, which implies that reducing uncertainty and riskiness may allow individuals to hold smaller precautionary savings and increase their consumption. While we do not observe household savings directly, Lee (2016a) documents that household that gained health insurance coverage through the mandate decreased these savings. Our results are consistent with that finding.

Table 3.5 summarizes the effects of the dependent coverage mandate on auto bank lending. This table shows that the number of auto bank loans increased significantly in all periods of law implementation. The largest change is in the post-treatment period and it is equal to 0.0126 additional loans on average, which is a 4 percent increase relative to the base of 0.314 loans per person per quarter. The second column of Table 3.5 suggests that most, if not all, of these additional auto loans remained current during the sample period. In particular, the number of current auto bank loans increased by 0.0111 in the post-treatment period, which is a 3.6 percent increase. There was no change in the number of these loans in short term (30 days) delinquency as the third column of Table 3.3 suggests.[5]

The amounts of auto bank loans current, total auto bank balances, and loan size all increased for the covered population. The larger and statistically significant increases

[5]The results for all auto loans (auto bank and auto finance) are presented in Appendix Table B.1. They are very similar to the results for bank auto. However, we decided to focus on bank auto in the main part of the paper because this type of auto lending is less volatile for young adults during our sample period.

occurred in the implementation and age-out periods. Table 3.3 shows that amount of auto credit current increased by $200 in the implementation phase and $248 in the age-out phase. Similarly, auto bank loan size grew by $177 and $293 per quarter in these time periods.[6]

Overall, these findings suggest that health insurance coverage mandate allowed covered individuals to borrow more on auto loans both in terms of the number of loans and dollar amounts on these loans. In addition, these borrowers seem to maintain additional loans and balances current and in good standing. Since automobiles may be considered consumer durables, our results are consistent with Aydin (2016), who finds that increased credit availability can lead to increases in durable spending. Also, since a car may be used to commute to work, and having a car may allow the owner to find or maintain a better job, these additional investments into vehicles by insured individuals (as evidenced by increases in auto loan numbers and balances) may suggest both increased consumption of automotive services and potentially better employment opportunities for these persons.

[6]See Appendix Table B.1 for comparable results for all auto loans.

Table 3.5: Main Results: Auto Loans

Coefficient	Num of auto bank loans	Num of auto bank loans current	Num of auto bank loans 30 DPD	Amt of auto bank loans current	Total auto bank balance	Total auto bank loan size
Enactment	0.0046***	0.0040***	0.0003	85.35	41.99	92.22
(Q2:2010-Q3:2010)	(0.001)	(0.001)	(0.0002)	(57.27)	(57)	(51.68)
Implementation	0.0092***	0.0084***	0.0003	199.05**	143.30*	176.73**
(Q4:2010-Q4:2012)	(0.0012)	(0.0012)	(0.0003)	(65.2)	(63.99)	(58.85)
Age-out	0.0126***	0.0111***	0.0005	247.85*	169.62	292.49**
(Q1:2013-Q4:2013)	(0.0016)	(0.0016)	(0.0003)	(102.28)	(98.04)	(92.6)
Avg in Enactment	0.282	0.273	0.003	13489.8	13432.9	19226.9
Avg in Implementation	0.292	0.283	0.003	14598.2	14495	20415.2
Avg in Age-out	0.314	0.306	0.003	16602.6	16502	22538.7
R-squared	0.6579	0.6421	0.1679	0.6971	0.7006	0.789
Observations	7,034,512	7,034,512	7,034,512	1,147,877	1,230,683	1,230,683

Notes: This table shows the effect of the dependent coverage mandate on credit variables of young adults covered by the mandate (24-25 years old in 2010) compared to a control group of young adults not covered by the law (26-27 years old in 2010). Only the coefficients on the interaction of the treatment indicator with enactment, implementation, and age-out periods are reported. Hubert-White robust standard errors are reported in parentheses. Author's calculations using data from the FRBNY Consumer Credit Panel / Equifax.

Student loans are very important credit type for this population because these loans may help young adults to finance their college or post-college education and investment into human capital. While our data do not allow us to observe directly individuals education status, college or university attendance, or other educational outcomes, we can examine the financial side of education as captured by the student loan variables. The effects of the health insurance mandate on student loan variables are summarized in Table 3.6. In this table, we show the effects of the health insurance coverage expansion on the total number of student loans, total credit on student loans, past due amount and total unpaid balance on these loans.

Table 3.6: Main Results: Student Loans

Coefficient	Number of student loans	Total credit on student loans	Past due amount on student loans	Student loan total unpaid balance
Enactment	0.0126**	71.26*	0.48	89.41**
(Q2:2010-Q3:2010)	(0.0043)	(28.31)	(13.24)	(29.39)
Implementation	0.0407***	247.05***	18.04	278.41***
(Q4:2010-Q4:2012)	(0.0047)	(29.33)	(14.91)	(30.23)
Age-out	0.0059	269.46***	15.16	290.04***
(Q1:2013-Q4:2013)	(0.0068)	(44.55)	(22.16)	(46.22)
Avg in Enactment	1.486	11,510.28	362	11,059.44
Avg in Implementation	1.769	13,347.74	477.5	12,749.94
Avg in Age-out	1.859	14,358.95	606.7	13,680.78
R-squared	0.8376	0.8703	0.5837	0.8878
Observations	7,454,240	7,454,240	7,454,240	7,454,240

Notes: Only the coefficients on the interaction of the treatment indicator with enactment, implementation, and age-out periods are reported. Hubert-White robust standard errors are reported in parentheses. Author's calculations using data from the FRBNY Consumer Credit Panel / Equifax.

As can be seen from the first column of this table, this population, on average, holds 1.5 1.9 student loans. The only credit instrument used more often on average is credit cards (see Tables 3.1 and 3.2 for means). Results in Table 3.6 indicate that individuals who were covered by the health insurance mandate increased their holdings of student loans in the enactment and treatment periods by 0.013 and 0.041 loans, which are 0.9 percent and 2.3 percent additions respectively. In addition, as reported in column 2 of this table, total credit on student loans increased in all periods of the law implementation with largest effects in the implementation and age-out phases equal to \$247 (1.9 %) and \$269 (1.9 %). While the total credit on student loans increased, there is little change in the amount of past due on these loans (column 3). For this variable, all coefficients are statistically insignificant. Finally, the last column of this table shows results for the student loan total unpaid balance. The unpaid balance increased in all periods considered, with the largest effects of \$278 and \$290 in implementation and age-out. These effects constitute 2.2 percent changes relative to means in these periods.

Overall, the increasing number of student loans and amount of credit without a change in the past due amount suggest that covered individuals borrowed more using student loans, but without going into default on these loans. In addition, larger unpaid balances on student loans to young adults with health insurance indicates that these persons are continuing their education or even just beginning it with the help of student loans. These results imply that health insurance may allow not only an expansion in consumption, as evidenced in the earlier results on credit cards and auto loans, but also additional investment into human capital.

Table 3.7 summarizes the effects of the health insurance expansion on first lien mortgage variables. Even though mortgages are not very common among the 24-28-years-old group considered in this study, we report them for completeness, as this type of credit is important for the economy. The first two columns of this table show the results for the number of first mortgages. The first thing to notice is that the average number of mortgages increases from the 0.23 to 0.3 during our sample period as the population we consider gets older. This average is consistent with the homeownership rate among this age group which, according to the US Census, was 22 percent for persons younger than 25 and 34 percent for those aged 25-29 in 2012-2013. Results for the number of mortgages suggest that there is not much change in these loans during the enactment and implementation phases. However, there is a small and statistically significant decline in the age-out period. The second column of Table 3.7 shows the results for the number of first mortgages current. Similar to the total number of mortgages, there is a small, but statistically significant decline in the number of mortgages current in the age-out period only. One possible explanation for these results is that treated individuals may lose health insurance coverage or receive less generous benefits post-treatment (Dahlen, 2015). Therefore, it may be harder for these young adults to get additional mortgages or even keep the existing ones. Another possible explanation is that there is a certain relocation of credit from mortgages into student loans, auto loans and possibly credit cards (which we documented earlier in the study).

The third column of this table shows the effect of health insurance expansion on the number of first mortgages in bankruptcy. While it is very rare for young people to have mortgages and file for bankruptcy, as is shown by the mean values of this variable, there is a statistically significant decline in this type of event during the enactment and implementation periods. This finding is consistent with insured young individuals being able to avoid financial distress after receiving health coverages as is documented in the previous chapter and earlier studies (e.g., Gross and Notowidigdo (2011); Hu et al. (2016); Mazumder and Miller (2016)).

Table 3.7: Main Results: First Mortgage Loans

Coefficient	Num 1st mortgage loans	Num 1st mortgage loans current	Num of 1st mortgage bankruptcy	Amt of 1st mortgage loans current	Total 1st mortgage balance	Total 1st mortgage loan size
Enactment	-0.0003	-0.0006	-0.0003**	-260.72	-50.64	-267.21
(Q2:2010-Q3:2010)	(0.0007)	(0.0007)	(0.0001)	(364.49)	(355.07)	(374.09)
Implementation	0.0013	0.0007	-0.0004**	-419.83	-122.07	-389.36
(Q4:2010-Q4:2012)	(0.0008)	(0.0008)	(0.0001)	(397.25)	(383.05)	(406.04)
Age-out	-0.0032**	-0.0034**	-0.0003	-273.37	-367.19	-800.6
(Q1:2013-Q4:2013)	(0.0012)	(0.0012)	(0.0002)	(616.22)	(576.63)	(559.58)
Avg in Enactment	0.2314	0.2154	0.0005	157,344	159,353	163,941
Avg in Treatment	0.2647	0.2507	0.0007	160,714	161,550	167,936
Avg in Post-treatment	0.3032	0.2921	0.0005	171,345	171,100	178,859
R-squared	0.7685	0.7501	0.3357	0.8949	0.8973	0.876
Observations	7,034,512	7,034,512	7,034,512	968,049	1,045,115	1,045,115

Notes: This table shows the effect of the dependent coverage mandate on credit variables of young adults covered by the mandate (24-25 years old in 2010) compared to a control group of young adults not covered by the law (26-27 years old in 2010). Only the coefficients on the interaction of the treatment indicator with enactment, implementation, and age-out periods are reported. Hubert-White robust standard errors are reported in parentheses. Author's calculations using data from the FRBNY Consumer Credit Panel / Equifax.

The last three columns of Table 3.7 summarize our results for the amounts of mortgages current, mortgage balance, and loan size. While all these variables seem to be trending down during our study period, none of the coefficients is statistically different from zero. Also, point estimates are very small compared to the average values of these amounts and balances. Overall, the results for first mortgages may suggest very little effect of the health insurance expansion on this type of credit. Despite some small declines in the number of mortgages post treatment, no statistically significant effect is present for the amount of mortgage current, mortgage balance or loan size.

3.4.3 Placebo Tests

This section presents results of a placebo test to examine if our earlier results are driven by some common trends for young adults instead of the dependent coverage mandate. To implement this test, we defined a placebo treatment group as individuals aged 29 or 30 when the mandate was implemented in 2010 and compare the outcomes of this group to the outcomes of a comparable control group defined as individuals aged 31 and 32 in 2010. We use the same specification as presented in equation (3.10) to conduct this test. Tables B.2 to B.5 in the Appendix section summarize results of the placebo test on our credit variables of interest. These tables are constructed in the same manner as our main results tables (Tables 3.4 through 3.7). Table B.2 shows the results of the placebo test for the credit card variables and credit applications. There are very few statistically significant coefficients in this table and even they are significant at the 10 percent level only. The economic magnitudes of these coefficients are also much smaller than the coefficients for the main results presented in Table 3.4. Overall, the placebo test for these variables shows that there is no effect of the dependent coverage mandate on credit cards of individuals not affected by the law.

Table B.3 shows the results of the placebo test for the auto bank variables. Similar to table B.2, most coefficients in this table are small and statistically insignificant. Again we find no effect of the law on the group which was not affected by it. Table B.5 summarize placebo test results for mortgage variables. While there are statistically significant coefficients in this table for the number of mortgages and the number of mortgages current, all these coefficients have the opposite sign to the coefficients in our main analysis (Table 3.7). This finding shows that there are no common trends in mortgages among young adults which can explain our main findings.

Finally, Table B.4 shows the placebo test results for student loan outcomes. This table seems to suggest that there is an effect of the law on student loan outcomes in the placebo group. As we note in the main analysis, some of our student loan variables do not pass parallel trends tests and should be treated with a caveat. This placebo test adds further doubts to the reliability of the results for student loan variables. Nevertheless, we decided to report this set of variables for completeness.

3.5 Discussion

We find that the dependent coverage mandate led young adults to receive more credit cards, have higher limits on credit cards, take out more auto loans and student loans, and carry higher balances of credit cards and auto loans. While young adults increased their debt and credit along these margins, their debt past due remained unchanged, with the exception of a slight decrease in mortgages in bankruptcy. While some of the changes in young adults financial behavior were transitory, most persisted even after they turned age 26 and were no longer covered by the mandate.

Our results clearly show that the mandate led young adults to take on more debt, and strongly suggest an increase in consumption (particularly credit card and auto) and investment into education. The exact reason why the mandate has these effects is less clear as our analysis cannot fully distinguish between the likely possibilities. Insurance reduces the chance of incurring large unexpected health expenses, which could allow young adults to reduce their precautionary savings. Similarly, the reduced risk from health expenditures could lead young adults to increase risk-taking elsewhere. Alternatively, because young adults covered by the mandate do not pay the full cost of the insurance expansion (see Section 2.1), the mandate could simply be functioning as a subsidy that increases the purchasing power of young adults, naturally leading to an increase in consumption.

The fact that many of the changes in young adults credit and debt persist for at least two years after the end of their coverage through the mandate may be our most interesting and puzzling finding. Presumably young adults should be taking on less risk, rebuilding their precautionary savings, and/or consuming less to reflect the loss of their subsidized health insurance. Instead, though credit card applications and bank card debt decline for young adults as they age out of treatment, most types of debt persist, while some such as auto loan balances actually increase relative to the control group. One possible explanation is that the transitory insurance led to a permanent increase in the ability to borrow (certainly suggested by the continued increase in credit limits we observe), and that formerly treated young adults continue to take advantage of this. Another explanation is that young adults took advantage of the insurance to invest in education, which permanently raised their earning ability and therefore their spending. Dillender (2014) provides evidence for this explanation, finding that young adults covered by the mandate earn higher wages, and that these higher wages were entirely driven by increased educational attainment.

3.6 Conclusion

In this paper, we examined the effect of providing health insurance coverage on consumption and borrowing decisions of newly insured young individuals. To identify the effect of health insurance on individual credit outcomes, we used the exogenous variation in insurance coverage provided by the 2010 dependent coverage mandate of the Affordable Care Act. Our strategy relies on the quasi-experimental nature of the mandate, which required insurance companies to extend coverage on family insurance plans to eligible dependents until their 26 birthday. Based on this provision, we defined a treatment group as individuals who were ages 24 and 25 in 2010 and compared the outcomes of this group to the outcomes of a control group comprising of individuals who were ages 27 and 28 in 2010. We used a simple difference-in-differences approach with these two groups to estimate the causal effect of the health insurance mandate on credit outcomes of individuals eligible to be insured.

We find that individuals affected by the health insurance mandate changed their credit behavior after becoming eligible for health insurance coverage. These individuals increased their consumption and borrowing on credit cards, auto loans, and student loans. They increased both the number of loans as well as amounts or balances on these loans. We do not detect any deterioration in the short-run performance on these loans after the insurance expansion, as the number of loans current and the amount current remained unchanged. In addition, lenders seemed to extend more credit to individuals with health insurance coverage, as seen in higher credit card limits and larger loan sizes for auto and student loans.

These findings have important policy implications, as they show that the effects of health insurance may go beyond health outcomes and spill over into consumption, savings, and investment decisions of affected individuals. Consistent with economic theory, we find that health insurance can reduce uncertainty about future income and motivate individuals to increase current consumption, borrowing, and even prompt investment into human capital through additional student loan borrowing. These spillover effects from health insurance may be important to take into account in designing future health and health insurance policies and making projections about economic effects of changes in these policies.

REFERENCES CITED

Akosa Antwi, Y., Moriya, A. S., & Simon, K. (2013). Effects of federal poicy to insure young adults: Evidence from the 2010 affordable care act's dependent-coverage mandate. *American Economic Journal: Economic Policy*, *5(4)*, 1-28.

Antwi, Y. A., Moriya, A. S., & Simon, K. I. (2015). Acces to health insurance and the use of inpatient medical care: Evidence from the affordable care act young adult mandate. *Journal of Health Economics*, *39*, 171-187.

Arellano, M., & Bond, S. (1991). Some tests of specification for panel data: Monte carlo evidence and an application to employment equations. *The Review of Economic Studies*, *58*, 277-297.

Aydin, D. (2016). *The marginal propensity to consume out of liquidity: Evidence from random assignment of 54,522 credit lines.*

Ayyagari, P., & He, D. (2016). The role of medical expenditure risk in portfolio allocation decisions. *Health Economics*. (10.1002/hec.3437)

Bailey, J. (2016). *Health insurance and the supply of entrepreneurs: New evidence from the affordable care act* (Working Paper). Temple University.

Bailey, J., & Chorniy, A. (2016). Employer-proivded health insurance and job-mobility: Did the affordable care act reduce job lock? *Contemporary Economic Policy*, *34(1)*, 173-183.

Barbaresco, S., Courtemanche, C. J., & Qi, Y. (2015). Impacts of the affordable care act dependent coverage provision on health-related outcomes of young adults. *Journal of Health Economics*, *40*, 54-68.

Barcellos, S. H., & Jacobson, M. (2015). The effects of medicare on medical expenditure risk and financial strain. *American Economic Journal: Economic Policy*, *7(4)*, 41-70.

Bessen, J., & Meurer, M. (2006). Patent litigation with endogenous disputes. *The American Economic Review*, *96*, 77-81.

Bessen, J., & Meurer, M. J. (2013). The private costs of patent litigation. *Journal of Law, Economics, and Policy*, *9*, 59-95.

Bessen, J., & Meurer, M. J. (2014). The direct costs from NPE disptues. *Cornell Law Review*.

Blundell, R., Griffith, R., & Windmeijer, F. (2002). Individual effects and dynamics in count data models. *Journal of Econometrics*, *108*, 113-131.

Brevoort, K., Grimm, P., & Kambara, M. (2015). *CFPB data point: Credit invisibles.* (Consumer Financial Protection Bureau)

Brown, M., Grigsby, J., van der Klaauw, W., Wen, J., & Basit, Z. (2016). Financial education and the debt behavior of the young. *The Review of Financial Studies*, *29(9)*, 2490-2522.

Busch, S. H., Golberstein, E., & Meara, E. (2014). ACA dependent coverage provision reduced high out-of-pocket health care spending for young adults. *Health Affairs*, *33(8)*, 1361-1366. (10.1377/hlthaff.2014.0155)

Cameron, A., & Trivedi, P. (2013). *Regression analysis of count data, second edition.* New York: Cambridge University Press.

Carroll, C. D. (1997). Buffer-stock saving and the life cycle/permanent income hypothesis. *The Quarterly Journal of Economics*, *112(1)*, 1-55.

Carroll, C. D., Hall, R. E., & Zeldes, S. P. (1992). The buffer-stock theory of saving: Some macroeconomic evidence. *Brookings Papers on Economic Activity*, *2*, 61-156.

Carroll, C. D., & Samwick, A. A. (1997). The nature of precauationary savings. *Journal of Monetary Economics*, *40(1)*, 41-71.

Chamberlain, G. (1992). Comment: Sequential moment restrictions in panel data. *Journal of Business & Economic Studies*, *10*, 20-26.

Chen, J., Vargas-Bustamante, A., & Novak, P. (2016). Reducing young adults' health care spending through the aca expansion ofdependent coverage. *Health Services Research*. (Forthcoming)

Choi, J. (1988). Patent litigation as an information transmission mechanism. *The American Economic Review*, *88*, 1249-1263.

Chua, K.-P., & Sommers, B. D. (2014). Changes in health and medical spending among young adults under health reform. *Journal of the American Medical Association*, *311(23)*, 2427-2439.

Crampes, C., & Langinier, C. (2002). Litigation and settlement in patent infringement cases. *The RAND Journal of Economics*, *33*, 258-274.

Dahlen, H. M. (2015). "aging out" of dependent coverage and the effect on us labor market and health insurance choice. *American Journal of Public Health*, *105(S5)*, S640-S650.

Debbaut, P., Ghent, A., & Kudlyak, M. (2016). The CARD act and young borrowers: The effects and the affected. *Journal of Money, Credit, and Banking*, *48(7)*, 1495-1513.

Depew, B., & Bailey, J. (2015). Did the affordable care act's dependent coverage mandate increase premiums? *Journal of Health Economics*, *41*, 1-14.

Dillender, M. (2014). Do more health insurance options lead to higher wages? evidence from states extending dependent coverage. *Journal of Health Economics*, *36*, 84-97.

Finkelstein, A., Taubman, S., Wright, B., Bernstein, M., Gruber, J., Newhouse, J. P., ... Baicker, K. (2012). The oregon health insurance experiment: Evidence from the first year. *Quarterly Journal of Economics*, *127(3)*, 1057-1106.

Fulford, S. L., & Schuh, S. (2016). *Consumer revolving credit and debt over the life cycle and business cycle* (Working Pape No. 15-17). Federal Reserve Bank of Boston.

Goda, G. S., Farid, M., & Bhattacharya, J. (2016). *The incidence of mandated health insurance: Evidence from the affordable care act dependent care mandate* (Working Paper No. 21846). NBER.

Goldman, D., & Maestas, N. (2013). Medical expenditure risk and household portfolio choice. *Journal of Applied Economics*, *28(4)*, 527-550.

Gourinchas, P.-O., & Parker, J. A. (2002). Consumption over the life cycle. *Econometrica*, *70(1)*, 47-89.

Gross, T., & Notowidigdo, M. J. (2011). Health insurance and the consumer bankruptcy decision: Evidence from the expansions of medicaid. *Journal of Public Economics*, *95*, 767-778.

Hall, B., & Ziedonis, R. (2001). The patent paradox revisited: An empirical study of patenting in the u.s. semiconductor industry, 1979-1995. *The RAND Journal of Economics*, *32*, 101-128.

Hall, B., & Ziedonis, R. (2007). *An empirical analysis of patent litigation in the semiconductor industry.* (Paper presented at ASSA meetings, Chicago, IL 4-7 January 2007)

Harhoff, D., & Reitzig, M. (2004). Determinants of opposition against EPO patent grants - the case of biotechnology and pharmaceuticals. *International Journal of Industrial Organization*, *22*, 443-480.

Hausman, J., Hall, B., & Griliches, Z. (1984). Econometric models for count data with an application to the patents-R&D relationship. *Econometrica*, *52*, 909-938.

Hu, L., Kaestner, R., Mazumder, B., Miller, S., & Wong, A. (2016). *The effect of the patient protection and affordable care act medicaid expansion on financial well-being* (Working Paper No. 22170). NBER.

Kiebzak, S., Rafert, G., & Tucker, C. E. (2016). The effect of patent litigation and patent assertion entities on entrepreneurial activity. *Research Policy*, *45(1)*, 218-231.

Kimball, M. S. (1990). Precautionary saving in the small and in the large. *Econometrica*, *58(1)*, 55-73.

Kortum, S., & Lerner, J. (1999). What is behind the recent surge in patenting? *Research Policy*, *28*, 1-22.

Lanjouw, J., & Schankerman, M. (2001). Characteristics of patent litigation: A window on competition. *The Review of Economic Studies*, *32*, 129-151.

Lanjouw, J., & Schankerman, M. (2004). Protecting intellectual property rights: Are small firms handicapped? *Journal of Law and Economics*, *47*, 45-74.

Lee, D. (2016a). Effects of dependent coverage mandate on household precuationary savings: Evidence from the 2010 affordable care act. *Economics Letters*, *147*, 32-37.

Lee, D. (2016b). *Effects of the dependent coverage mandate on household financial portfolio: Evidence from the 2010 affordable care act* (Working Paper). Peking University. (https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2744437)

Lee, D., & van der Klauw, W. (2010). An introduction to the consumer credit panel. *Federal Reserve Bank of New York Staff Report*, *479*.

Lerner, J. (1995). Patenting in the shadow of competitors. *Journal of Law and Economics*, *38*, 463-495.

Liang, K., & Zeger, S. (1986). Longitudinal data analysis using generalized linear models. *Biometrika*, *73*, 13-22.

Mazumder, B., & Miller, S. (2016). The effects of the massachusetts health reform on household financial distress. *American Economic Journal: Economic Policy*, *8(3)*, 284-313.

Montalvo, J. (1997). GMM estimation of count-panel-data models with fixed effects and predetermined instruments. *Journal of Business and Economic Statistics*, *15*, 82-89.

Pakes, A., & Griliches, Z. (1984). Patents and R&D at the firm level: A first look. In Z. Griliches (Ed.), *R&d, patents, and productivity.* Chicago: University of Chicago Press.

Patent assertion and U.S. innovation (Report). (2013). Council of Economic Advisers.

Salomon, R. M., & Shaver, J. M. (2005). Learning by exporting: New inights from examining firm innovation. *Journal of Economics and Management Strategy*, *14(2)*, 431-460.

Somaya, D., Williamson, I., & Zhang, X. (2007). Combining patent law expertise with R&D for patenting performance. *Organization Science*, *18*, 922-937.

Starr-McCluer, M. (1996). Health insurance and precautionary savings. *The American Economic Review*, *86(1)*, 285-295.

Thorne, D., Warren, E., & Sullivan, T. A. (2009). The increasing vulnerability of older americans: Evidence from the bankruptcy court. *Harvard Law and Policy Review*, *87*, 87-101.

U.S. General Accounting Office. (2003). *Technology transfer: Agencies' rights to federally sponsored biomedical inventions* (Tech. Rep. No. GAO-03-536). U.S. General Accounting Office. (www.ga.gov/assets/240/238890.pdf)

Wooldridge, J. (1997). Multiplicative panel data models without the strict exogeneity assumption. *Econometric Theory*, *13*, 667-678.

APPENDIX A

CHAPTER 1 APPENDIX

Table A.1: Placebo Tests for Measures of Financial Distress

Coefficient	Number of Accounts in Third-Party Collections	Amount in Third-Party Collections	Total Past Due, Revolving Accounts	New Incidence of Bankruptcy Filing	New Incidence of Third-Party Collection
Treatment Group: 1982-1983					
Control Group: 1980-1981					
Enactment	-0.001	15.935	-5.577	-0.0004**	-0.0007
(Q2:2010-Q3:2010)	(0.003)	(11.038)	(5.647)	(0.000)	(0.001)
Implementation	-0.001	32.693**	-1.963	-0.0005***	-0.002
(Q4:2010-Q4:2012)	(0.004)	(16.648)	(7.616)	(0.000)	(0.001)
Age-Out	0.001	14.771	7.598	-0.0002	-0.002
(Q1:2013-Q4:2013)	(0.009)	(32.668)	(14.373)	(0.000)	(0.002)
R^2	0.5405	0.335	0.568	0.206	0.176
Num. of Obs.	7,340,119	3,438,358	5,236,941	7,146,117	7,282,954
Treatment Group: 1983					
Control Group: 1982					
Enactment	0.006	-4.871	-4.596	0.0002	0.001
(Q2:2010-Q3:2010)	(0.004)	(14.382)	(6.557)	(0.000)	(0.002)
Implementation	0.011*	-14.013	2.156	0.00004	-0.001
(Q4:2010-Q4:2012)	(0.006)	(23.587)	(10.250)	(0.000)	(0.002)
Age-Out†	0	0	0	0	0
(Q1:2013-Q4:2013)	(.)	(.)	(.)	(.)	(.)
R^2	0.493	0.308	0.482	0.144	0.162
Num. of Obs.	4,584,283	2,161,023	3,279,981	4,477,509	4,541,841
Treatment Group: 1980-1981					
Control Group: 1978-1979					
Enactment	-0.002	-18.205*	-2.650	-0.00004	-0.0003
(Q2:2010-Q3:2010)	(0.003)	(10.918)	(6.117)	(0.000)	(0.001)
Implementation	0.0005	-6.013	-14.761*	-0.0002	-0.0006
(Q4:2010-Q4:2012)	(0.004)	(16.268)	(8.501)	(0.000)	(0.001)
Age-Out	-0.001	-9.398	-19.600	-0.0001	-0.001
(Q1:2013-Q4:2013)	(0.009)	(34.123)	(17.272)	(0.000)	(0.002)
R^2	0.493	0.220	0.481	0.155	0.163
Num. of Obs.	9,198,616	4,286,672	6,680,531	8,933,354	9,128,992

† Estimates for the Age-out period are omitted in the second panel due to collinearity with control variables.
Note: Standard errors clustered at the individual level. All regressions include individual, time, and statel fixed effects. Author's calculations using data from the FRBNY Consumer Credit Panel / Equifax.

APPENDIX B

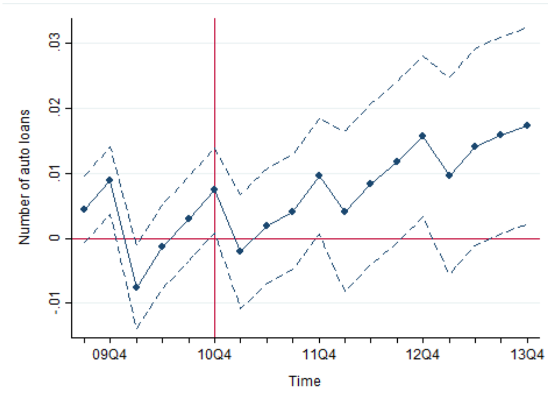
CHAPTER 2 APPENDIX

Table B.1: The Effect of the ACA's Dependent Coverage Mandate on All Auto Loans

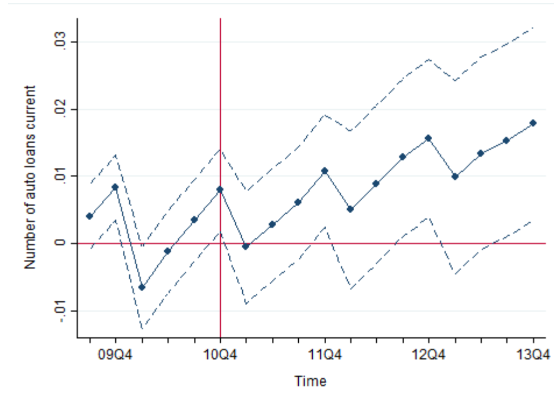
Coefficient	Num of auto loans	Num of auto loans current	Num of auto loans 30 DPD	Amt of auto loans current	Total auto balance	Total auto loan size
Enactment	0.0096***	0.0087***	0.0001	49.96	-40.78	130.21**
(Q2:2010-Q3:2010)	(0.0014)	(0.0013)	(0.0004)	(49.47)	(92.39)	(45.52)
Implementation	0.0175***	0.0167***	0	121.07*	16.47	279.86***
(Q4:2010-Q4:2012)	(0.0015)	(0.0015)	(0.0004)	(55.86)	(83)	(51.32)
Age-out	0.0190***	0.0174***	0.0006	39.59	-156.43	213.53**
(Q1:2013-Q4:2013)	(0.0021)	(0.0021)	(0.0006)	(82.02)	(147.25)	(74.67)
Avg in Enactment	0.282	0.273	0.003	13489.8	13432.9	19226.9
Avg in Implementation	0.292	0.283	0.003	14598.2	14495	20415.2
Avg in Age-out	0.314	0.306	0.003	16602.6	16502	22538.7
R^2	0.6579	0.6421	0.1679	0.6971	0.7006	0.789
Num. of Obs.	7,034,512	7,034,512	7,034,512	1,147,877	1,230,683	1,230,683

Notes: This table shows the effect of the dependent coverage mandate on credit variables of young adults covered by the mandate (24-25 years old in 2010) compared to a control group of young adults not covered by the law (26-27 years old in 2010). Only the coefficients on the interaction of the treatment indicator with enactment, implementation, and age-out periods are reported. The rest of the specification is described in equation (10). Each column is a separate regression on the dependent variable indicated in the first row. ***, **, * - indicate significance at the 1, 5, and 10 % level, respectively. Hubert-White robust standard errors are reported in parentheses.

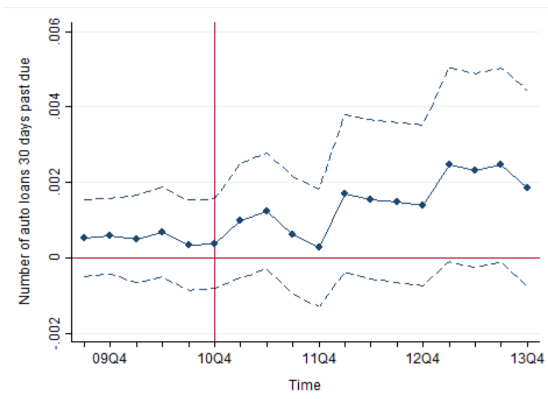
Panel A: Number of auto loans



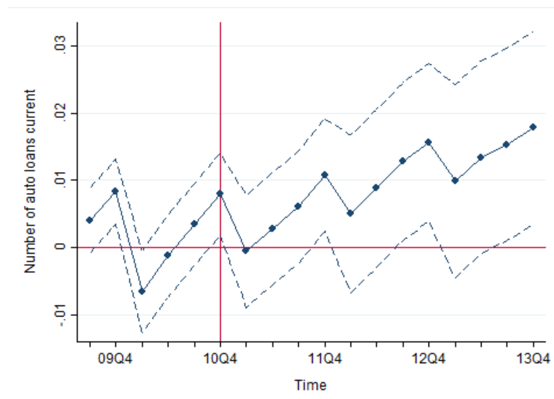
Panel B: Number of auto loans current



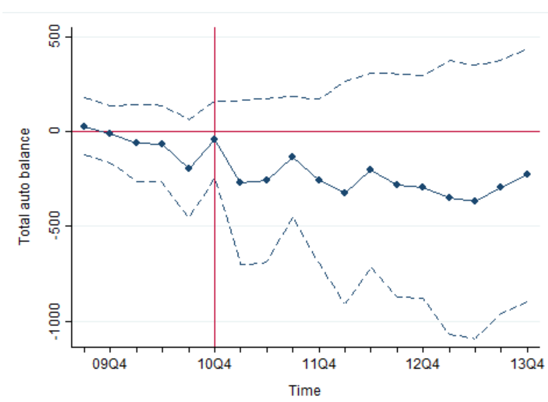
Panel C: Number of auto loans 30 days past due



Panel D: Amount of auto loans current



Panel E: Total auto balance



Panel F: Total auto loan size

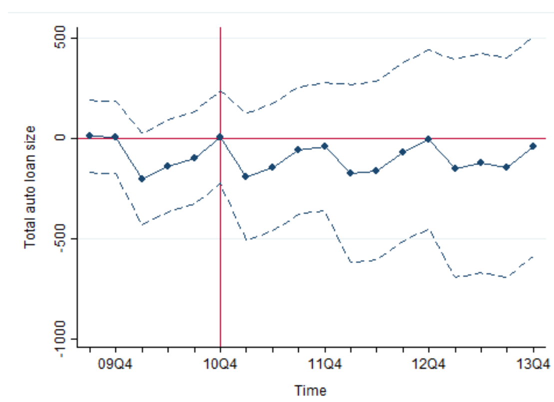


Figure B.1: All Auto Credit after the Mandate. Note: Authors calculations using data from FRBNY Consumer Credit Panel / Equifax.

Table B.2: Placebo Tests for Credit Cards

Coefficient	Credit Applications	Number of Actve Credit Cards	Number of Credit Cards 30 Days Past Due	Amount of Bank Card Current	Credit Card Balance	Retail Card Credit Limit
Treatment Group: 1982-1983						
Control Group: 1980-1981						
Enactment	-0.004	0.006**	0.0008	32.855**	45.429***	24.566***
(Q2:2010-Q3:2010)	(0.004)	(0.003)	(0.000)	(13.743)	(12.918)	(9.481)
Treatment	-0.005	0.001	0.0009*	49.194**	69.384***	31.616**
(Q4:2010-Q4:2012)	(0.004)	(0.003)	(0.001)	(21.425)	(19.823)	(13.357)
Post-Treatment	0.001	0.016**	0.002	73.747*	78.279**	35.447
(Q1:2013-Q4:2013)	(0.009)	(0.006)	(0.001)	(37.991)	(36.076)	(26.056)
R^2	0.222	0.696	0.107	0.679	0.687	0.696
Num. of Obs.	5,870,775	4,462,501	7,117,870	3,897,363	4,398,509	2,721,570

Treatment Group: 1983						
Control Group: 1982						
Enactment	0.014**	0.009**	0.0001	29.452	40.534**	28.378**
(Q2:2010-Q3:2010)	(0.006)	(0.004)	(0.001)	(20.194)	(17.931)	(13.150)
Treatment	0.005	0.010**	0.001	47.998	64.927**	28.175
(Q4:2010-Q4:2012)	(0.007)	(0.005)	(0.001)	(32.634)	(29.192)	(18.864)
Post-Treatment†	0	0	0	0	0	0
(Q1:2013-Q4:2013)	(.)	(.)	(.)	(.)	(.)	(.)
R^2	0.201	0.640	0.107	0.646	0.659	0.690
Num. of Obs.	3,720,749	2,812,152	3,516,749	1,917,139	2,167,065	1,332,124

Treatment Group: 1980-1981						
Control Group: 1978-1979						
Enactment	-0.004	0.002	0.001	30.290**	40.911***	28.159***
(Q2:2010-Q3:2010)	(0.004)	(0.003)	(0.001)	(14.374)	(13.947)	(9.845)
Treatment	-0.010*	0.007**	0.001**	20.407	42.818**	35.800**
(Q4:2010-Q4:2012)	(0.005)	(0.003)	(0.001)	(20.895)	(51.408)	(14.035)
Post-Treatment	-0.006	0.005	0.001	32.666	51.408	48.441*
(Q1:2013-Q4:2013)	(0.008)	(0.007)	(0.001)	(41.732)	(40.133)	(27.220)
R^2	0.210	0.662	0.108	0.702	0.711	0.707
Num. of Obs.	7,440,798	5,711,889	7,061,722	3,902,961	4,396,104	2,777,926

† Estimates for the Age-out period are omitted in the second panel due to collinearity with control variables.
Note: Standard errors clustered at the individual level. All regressions include individual, time, and statel fixed effects. Author's calculations using data from the FRBNY Consumer Credit Panel / Equifax.

Table B.3: Placebo Tests for Auto Bank Loans

Coefficient	Number of Auto Bank Loans	Number of Auto Bank Loans Current	Number of Auto Bank Loans 30 Days PD	Amount of Auto Bank Loans Current	Total Auto Bank Balance	Totall Auto Bank Loan Size
Treatment Group: 1982-1983						
Control Group: 1980-1981						
Enactment	0.001*	0.001	0.0002	45.428	52.866	104.38***
(Q2:2010-Q3:2010)	(0.001)	(0.001)	(0.000)	(40.377)	(37.828)	(38.187)
Treatment	0.001	0.001	0.0003	101.14*	100.10*	167.43***
(Q4:2010-Q4:2012)	(0.001)	(0.001)	(0.000)	(60.822)	(57.090)	(57.152)
Post-Treatment	0.004**	0.003	0.0004	22.248	83.001	110.88
(Q1:2013-Q4:2013)	(0.002)	(0.002)	(0.000)	(143.739)	(136.437)	(135.274)
R^2	0.667	0.653	0.174	0.700	0.706	0.788
Num. of Obs.	7,117,870	7,117,870	7,117,870	1,251,995	1,332,452	1,332,452
Treatment Group: 1983						
Control Group: 1982						
Enactment	-0.001	-0.001	-0.0002	15.226	30.461	19.711
(Q2:2010-Q3:2010)	(0.001)	(0.001)	(0.000)	(57.947)	(54.171)	(53.997)
Treatment	-0.001	-0.0004	-0.0002	-15.698	34.452	24.276
(Q4:2010-Q4:2012)	(0.001)	(0.001)	(0.000)	(85.626)	(80.237)	(79.127)
Post-Treatment†	0	0	0	0	0	0
(Q1:2013-Q4:2013)	(.)	(.)	(.)	(.)	(.)	(.)
R^2	0.662	0.647	0.175	0.698	0.703	0.788
Num. of Obs.	3,516,749	3,516,749	3,516,749	611,621	652,485	1,3332,452
Treatment Group: 1980-1981						
Control Group: 1978-1979						
Enactment	-0.0004	-0.00003	-0.0004	47.207	51.951	11.489
(Q2:2010-Q3:2010)	(0.001)	(0.001)	(0.000)	(41.369)	(39.161)	(40.058)
Treatment	-0.0003	0.001	-0.0003	109.39*	120.19**	71.984
(Q4:2010-Q4:2012)	(0.001)	(0.001)	(0.000)	(63.022)	(59.534)	(60.079)
Post-Treatment	0.001	0.002	-0.001	25.899	17.424	220.18
(Q1:2013-Q4:2013)	(0.002)	(0.002)	(0.000)	(144.984)	(138.769)	(137.450)
R^2	0.673	0.659	0.180	0.697	0.703	0.789
Num. of Obs.	7,061,722	7,061,722	7,061,722	1,259,457	1,335,796	1,335,796

† Estimates for the Age-out period are omitted in the second panel due to collinearity with control variables.
Note: Standard errors clustered at the individual level. All regressions include individual, time, and statel fixed effects. Author's calculations using data from the FRBNY Consumer Credit Panel / Equifax.

Table B.4: Placebo Tests for Student Loans

Coefficient	Number of Student Loans	Total Credit on Student Loans	Past Due Amount on Student Loans	Studentl Loan Unpaid Balance
Treatment Group: 1982-1983				
Control Group: 1980-1981				
Enactment	0.006***	86.949***	34.887***	91.916***
(Q2:2010-Q3:2010)	(0.002)	(12.025)	(10.667)	(11.782)
Treatment	0.018***	217.40***	28.329**	230.71***
(Q4:2010-Q4:2012)	(0.003)	(17.849)	(14.291)	(17.176)
Post-Treatment	-0.012	243.84***	49.850*	241.39***
(Q1:2013-Q4:2013)	(0.007)	(44.451)	(28.800)	(45.019)
R^2	0.843	0.922	0.577	0.918
Num. of Obs.	7,585,483	7,585,483	7,585,483	7,585,483

Treatment Group: 1983				
Control Group: 1982				
Enactment	0.004*	34.361***	-9.990	33.912**
(Q2:2010-Q3:2010)	(0.003)	(17.461)	(15.000)	(17.288)
Treatment	0.010**	102.97***	11.421	107.82***
(Q4:2010-Q4:2012)	(0.001)	(25.426)	(19.761)	(25.411)
Post-Treatment†	0	0	0	0
(Q1:2013-Q4:2013)	(.)	(.)	(.)	(.)
R^2	0.807	0.879	0.506	0.870
Num. of Obs.	4,717,059	4,717,059	4,717,059	4,717,059

Treatment Group: 1980-1981				
Control Group: 1978-1979				
Enactment	0.008***	52.781***	17.474	40.398***
(Q2:2010-Q3:2010)	(0.002)	(12.018)	(11.531)	(11.553)
Treatment	0.016***	127.77***	27.456*	105.65***
(Q4:2010-Q4:2012)	(0.002)	(18.378)	(14.928)	(18.045)
Post-Treatment	-0.002	109.70***	27.408	95.063***
(Q1:2013-Q4:2013)	(0.007)	(42.210)	(28.700)	(41.995)
R^2	0.807	0.912	0.503	0.905
Num. of Obs.	9,500,485	9,500,485	9,500,485	9,500,485

† Estimates for the Age-out period are omitted in the second panel due to collinearity with control variables.

Note: Standard errors clustered at the individual level. All regressions include individual, time, and statel fixed effects. Author's calculations using data from the FRBNY Consumer Credit Panel / Equifax.

Table B.5: Placebo Tests for First Mortgage Liens

Coefficient	Number of First Mortgage Loans	Num of 1st Mortgage Loans Current	Num of 1st Mortgage Loans in Bankruptcy	Amt of 1st Mortgage Loan Current	Total First Mortgage Balance	Totall First Mortgage Loan Size
Treatment Group: 1982-1983						
Control Group: 1980-1981						
Enactment	0.004***	0.004***	-0.0001	-270.63	-217.79	-199.78
(Q2:2010-Q3:2010)	(0.001)	(0.001)	(0.000)	(230.460)	(230.668)	(265.724)
Treatment	0.008***	0.007***	-0.0002	-455.28	-410.40	-398.29
(Q4:2010-Q4:2012)	(0.001)	(0.001)	(0.000)	(317.600)	(319.661)	(348.431)
Post-Treatment	0.008***	0.008***	0.00001	-1139.2*	-1517.9**	-1311.5**
(Q1:2013-Q4:2013)	(0.002)	(0.002)	(0.000)	(686.823)	(663.851)	(668.022)
R^2	0.796	0.781	0.361	0.884	0.888	0.883
Num. of Obs.	7,117,870	7,117,870	7,117,870	1,471,702	1,599,405	1,599,405

Treatment Group: 1983						
Control Group: 1982						
Enactment	0.003***	0.002**	-0.0001	-71.96	31.527	-152.36
(Q2:2010-Q3:2010)	(0.001)	(0.001)	(0.000)	(317.204)	(318.869)	(335.907)
Treatment	0.005***	0.004***	-0.0002	-386.74	111.46	-209.85
(Q4:2010-Q4:2012)	(0.001)	(0.001)	(0.000)	(433.188)	(443.697)	(456.149)
Post-Treatment†	0	0	0	0	0	0
(Q1:2013-Q4:2013)	(.)	(.)	(.)	(.)	(.)	(.)
R^2	0.784	0.768	0.352	0.897	0.900	0.878
Num. of Obs.	3,516,749	3,516,749	3,516,749	640,139	691,633	691,633

Treatment Group: 1980-1981						
Control Group: 1978-1979						
Enactment	0.004***	0.004***	-0.0001	-308.02	-140.77	-97.182
(Q2:2010-Q3:2010)	(0.001)	(0.001)	(0.000)	(261.933)	(254.310)	(298.267)
Treatment	0.008***	0.008***	-0.0002	-422.15	-600.31*	-374.16
(Q4:2010-Q4:2012)	(0.001)	(0.001)	(0.000)	(350.567)	(352.245)	(379.430)
Post-Treatment	0.008***	0.008***	-0.0003	683.88	539.36	619.04
(Q1:2013-Q4:2013)	(0.002)	(0.002)	(0.000)	(779.437)	(751.100)	(766.576)
R^2	0.811	0.798	0.367	0.879	0.883	0.878
Num. of Obs.	7,061,722	7,061,722	7,061,722	1,779,894	1,948,789	1,948,789

† Estimates for the Age-out period are omitted in the second panel due to collinearity with control variables.
Note: Standard errors clustered at the individual level. All regressions include individual, time, and statel fixed effects. Author's calculations using data from the FRBNY Consumer Credit Panel / Equifax.