

**THREE ESSAYS ON THE EVALUATION OF
PUBLIC POLICY PROGRAMS**

by

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This dissertation consists of three chapters, each evaluating a different public policy. The first chapter studies the effect of internet on music sales. Internet usage has increased dramatically over the past few years. Concurrently, the sales from music CDs have witnessed a huge decline. I analyze the effect of downloading music on the current downturn in CD sales by looking at the progressive disappearance of the traditional stores. To identify the causal impact of downloading and control for endogeneity, I instrument state internet penetration rates by information on the adoption of Video Franchise Law (VFL). Results indicates that implementation of VFL increases internet access in states which adopt it, and explains 58.7 percent of total store closings in those states.

The second chapter analyzes whether enactment of the federal Family Medical Leave Act (FMLA) differentially affected states that previously implemented maternity leave laws at the state level than those states which did not. Additionally, we study whether FMLA caused an increase in female employment and labor force participation in those states that expanded its benefits and relaxed the eligibility criteria. Finally, we analyze the Paid Family Leave program in California, comparing how the change in female employment differs from those states which have FMLA alone and those which have complemented the benefits of FMLA. Our results confirm the positive and significant effect of FMLA on female employment and also a significantly positive impact on female employment for some states when they complement the benefits and eligibility criteria of FMLA.

The third chapter analyses labor market impacts of the implementation of all the state

and local governments' EITC supplement. We examine whether the substantial expansions in the EITC program created by these supplements are an effective means of providing work incentives. Exploiting variation in the policy over time both across states and within states between different demographic groups, we find the EITC supplements have raised labor supply among single women, but had no effect on the labor supply of married women. Our results indicate the state and local governments' EITC expansions to be less effective compared to the federal EITC expansions.

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PREFACE

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1.0 INTRODUCTION

My dissertation focuses on the study of the impact of different public policy programs. The broad objective is to empirically investigate how the implementation of policies enacted at the federal and state governments level have affected the retail industry as well as the labor market outcomes in various sectors of the economy. Specifically, I study how the implementation of a franchising law varies the availability of internet services across different states and hence affects the sale of music. I further study the impact of various state and federal maternity leave policies on female labor supply and finally analyze how the state and local governments' supplements of the Earned Income Tax Credit policy has affected the female employment.

The first chapter, “The Effect of the Internet on Music Sales”, examines how the arrival of the digital technology in the form of increased internet usage is posing a threat to the survival of the traditional brick and mortar businesses. In order to address this issue, I specifically consider the case study of the music industry, since the music retail has been adversely affected by an advent of the internet as a major distribution channel of music in digital form. Over the past few years, with an increase in internet usage there has been a concurrent decline in the sale from music CDs. Previous literature (Zentner 2006, Oberholzer-Gee and Strumpf, 2007) have analyzed the effect of file sharing on record sales but there has not been a consensus regarding its detrimental effect on music sales. In this chapter, I use two sources of exogenous variation in the internet access across US states to measure the impact of music downloads on the progressive disappearance of the traditional music CD stores. Given that the sale of CDs is the most important source of revenue for the music industry, and the record stores represent a major share of this revenue, it is crucial to estimate the extent to which a

change in the consumer preference in the form of downloading music is causing a decline in the old format. The goal is to analyze whether an increase in the availability of broadband internet service, which allows for faster access to music downloads and may thus reduce the demand for physical CDs, has affected the number of CD store closings in a given region over a period of time. Establishing a causal effect of an increased internet access on store closings is difficult due to the presence of unobserved heterogeneity in a taste for music as well as the potential problem of reverse causality. To address these concerns, I use information on the passage of Video Franchise Law (VFL) and the differences in the household telephone adoption rate in the 1960 Census as two sources of exogenous variation in the availability of the internet access across the states. Employing an instrumental variable (IV) estimation strategy and using CPS Computer and Internet Use Supplements data for state internet penetration rates and CD store closings data from the research group Almighty Institute of Music Retail, I find that the implementation of VFL significantly increases the availability of internet access in the states which adopt it, and explains about 58.7 percent of the total store closings in those states. As a second set of instruments for state internet penetration rates, I use the 1960 Census data on the household telephone adoption rates to identify a variation across states in online access, and find that internet has diffused faster in states with historically higher rates of telephone adoption which in turn causes a significantly higher number of music store closures in these states.

The second chapter, “The Effect of Parental Leave on Female Employment: Evidence from State Policies” (joint work with Ana Espinola Arredondo) examines the effect of federal and state parental leave policies on female labor market outcomes. We study the impact of three distinct leave policies on female employment, the Family and Medical Leave Act (FMLA) which is a federal policy, the state expansions of the FMLA, and finally the Paid Family Leave program implemented by California. Researches on the effect of such policies are particularly important given the recent increasing trends in employment of women with young children. We employ a difference-in-difference-in differences estimation strategy and data from Integrated Public Use Microdata Series (IPUMS) to conduct our analysis. In contrary to the previous research (Waldfogel, 1999) which fails to find an effect of FMLA on

female employment, we show that the introduction of the FMLA has a significantly positive impact on the employment of women in those states which had no state law providing parental leave benefits compared to the states applying the Temporary Disability Insurance (TDI) before FMLA was enacted. Further, we find that the impact of FMLA expansion on female employment and labor force participation has been significantly higher in the states with most generous expansions (in terms of improving the benefits and relaxing the eligibility criteria of FMLA) compared to the states which did not expand FMLA. Finally, in order to get an intuitive understanding of the effects of further increases in the generosity levels of parental leave policies, we consider the recent enactment of California's Paid Family Leave program. Specifically, we find no impact of the introduction of this law on the female labor market outcomes, primarily due to a low-take up rates which results from a lack of worker awareness of the available benefits provided by the Paid Family Leave program.

The third chapter "The Labor Market Effects of State and Local Expansions of the Earned Income Tax Credit" (joint work with Alexis León) analyzes the labor market impacts of the implementation of all the state and local governments' EITC supplement using March Current Population Survey data from 1984 to 2008. Previous literature (Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001) have established the significant positive impact of the federal EITC on the labor force participation rates of single women with children. The contribution of this chapter to the existing literature is threefold. Firstly, it serves as a further test for the theory of labor force participation and labor supply, and contributes to answering the question of whether EITC payments by state and local governments affect participation in the same way as the federal EITC does. Secondly, it helps to assess the effectiveness of these state programs, that is, whether the EITC programs are indeed an effective means of boosting labor force participation in a state, or are they expensive programs that do not achieve these goals. To the extent that state governments can allocate the funds to alternative means (like child care centers) that may also achieve the same desired impact on female employment, an estimate of the labor market effects of state EITC programs will help inform that choice. Finally, while the federal and some of the state EITC programs were implemented and subsequently expanded at a time of strong female labor force participation

growth, many of these state and local government programs implemented in the last decade coincide with a period of generally flat female participation rates. If these EITC programs are found to have failed to affect female participation, this would suggest that future increases in the federal EITC may well fail to lead any more women to the labor force either. Our results indicate that the EITC expansion increased the labor supply among single women, but has no substantial impact on the labor market outcomes of married women. Further, we show that the EITC expansions were most effective in a time period when the benefit levels were low, indicating the state and local government EITC expansions to be less effective compared to the federal EITC expansions.

2.0 THE EFFECT OF THE INTERNET ON MUSIC SALES

2.1 INTRODUCTION

Over the last two decades, digital technologies have permeated the recording industry where music has been encoded in digital form and stored on CDs. Such digital technologies gained popularity due to their quality and ease of transportation. However, the music industry has been experiencing rapid changes over the last few years. The Recording Industry Association of America (RIAA) reports that there has been a decline in CD sales since 1999, with the highest being a 20.5 percent fall in 2007 compared to the previous year. This huge decrease in the sale of CDs, which is the most popular format, is a cause of alarm for the music industry¹. This downward trend in the revenue from the sale of CDs is evident from the increasing number of music CD stores that went out of business in recent years across the United States².

Internet piracy and illegal MP3 downloads have often been blamed for the ongoing downturn in CD sales (Liebowitz, 2003; Zentner, 2003). Previous studies have analyzed the effect of file sharing on record sales with somewhat conflicting results. Zentner (2003), Peitz and Waelbroeck (2004) and Liebowitz (2005) find a negative effect of file-sharing on music sales. On the other hand, Oberholzer-Gee and Strumpf (2007) and Boorstin (2004) find no evidence of such detrimental effects of internet piracy on music sales. Liebowitz (2003) also analyzes alternative reasons which could affect the record sale such as price of CD and change in taste, but concludes that these reasons cannot explain the observed reduction in sales.

¹Sales of CDs account for more than 85 percent of the dollar value of the total music sold (RIAA).

²According to the research group Almighty Institute of Music Retail, about 2,700 record stores went out of business across the country since 2003.

The effect of the internet on record sales is not just limited to online piracy and peer-to-peer file-sharing, however. According to the International Federation of the Phonographic Industry (IFPI) and Nielsen SoundScan, consumers are downloading more than ever from authorized music-download sites in the recent years. The research firm Ipsos-Insight reports in their quarterly digital music behavior that in late June 2003, one out of six (roughly equivalent to ten million people) of U.S. music downloaders aged 12 and older had paid to download music online.

In the past, bandwidth restrictions have limited the distribution of music in digital form over the internet. However, these restrictions are disappearing due to advances in networking (broadband) technologies. Consumers are now able to download and play high-quality music in digital form directly through the internet at a price considerably less than what they would otherwise pay for a CD. According to sales figures from IFPIA, the sales of digital music has been rising due to legal downloading, for example, in 2005 it nearly tripled over 2004 levels, but were not enough to help the music industry see overall growth. As bandwidth increases and better compression techniques are being available, the internet is becoming a major distribution channel of music in the digital form.

The availability of internet services is not the same across the different states, however. Prior to 2005, all cable companies and other companies interested in offering cable services to consumers, were required to negotiate separate agreements with each city before they could lay cable in the ground or place cable along utility poles. In some states these agreements were valid for up to as long as 15 years. In order to establish these agreements, the company needed to enter into negotiations with as many as 2,500 or more cities per state. In recent years, in order to streamline the video franchising process, some states have passed Video Franchise Laws (VFL), which permit state issued agreements. Under the law, cable providers are not required to obtain any other separate franchise agreements. According to the proponents of this law, these policies should bolster innovation, spur broadband investment, increase competition and result in lower prices and better quality of service³. The adoption

³TIA Advocates Video Franchise Legislation Reform at September 14th Press Briefing, by Alan J Weiss-

of a VFL acts as a source of variation across states in the availability of the internet.

Differences in state housing characteristics and the historical patterns of adoption of household appliances also play a crucial role in determining how internet adoption is diffusing across states. Internet penetration differs across states to a considerable degree, and the adoption of household appliances as well as housing characteristics gives a plausible source of variation, with internet availability being lesser in states with a lower household appliances adoption rate than in ones with a higher rate. Referring to the states with lower household appliances adoption rates as being more rural, we can say that internet availability is lower in the more “rural” states as compared to the urbanized states. These sets of instruments for state internet penetration rates are used by Betsey Stevenson (2006).

The arrival of digital technology has changed the way people relate to music. The rise in portable MP3 player ownership among U.S. downloaders, coupled with the growth in paid downloading suggests that digital music enthusiasts may be shifting their overall music acquisition and listening behaviors from a physical to a digital approach (Kleinschmit, About.Com). It is interesting to analyze the extent to which this new method of music distribution is driving the decline in the old format, by looking at the progressive disappearance of the traditional CD stores. The focus of the music industry so far has been on how legal downloads has failed to fill the revenue gap created by the shortfall in traditional CD sales. What deserves further examination, however, is whether legal downloads are actually causing that shortfall.

Theoretically, the downloaded product can be a complement for music in regular formats (such as a CD) as well. People might download some tracks from the internet and decide to purchase the CD. So the actual effect of downloading on the sale of music is an empirical question. In this paper, I look at the impact of downloads on the number of traditional CD store closings, using Current Population Survey (CPS) Computer and Internet Use

berger. To date, nineteen states have passed Video Franchise Laws. Texas was the first state to adopt a VFL in 2005. This was followed by California, Indiana, Kansas, New Jersey, North Carolina, South Carolina and Virginia in 2006. Connecticut, Florida, Georgia, Illinois, Iowa, Michigan, Missouri, Nevada, Ohio and Wisconsin passed a VFL in 2007. Tennessee was the only state to implement VFL in 2008.

Supplements data for internet availability and CD store closings data from the research group Almighty Institute of Music Retail. In particular, I analyze whether an increase in the availability of broadband internet service, which allows for faster access to music downloads and may thus reduce the demand for physical CDs, has affected the number of CD store closings in a given region over a period of time⁴. However, establishing a causal effect is difficult due to the fact that a positive association between internet penetration and store closings may just reflect the fact that people switch to music downloads whenever CD stores nearby go out of business, thereby raising the demand for faster network connections. Ordinary least squares (OLS) will not, in general, provide consistent estimates of the causal effect of downloading on store closings. In order to control for reverse causality and other potential endogeneity concerns, I use information on the passage of VFLs as a source of exogenous variation in broadband internet access. This overcomes the potential endogeneity problem since it directly affects downloading, as people in these states have greater access to internet connections, but it does not affect the outcome variable of interest. Employing an instrumental variable (IV) estimation strategy by using the VFLs as an instrument for state internet penetration rates, I find that an increase in internet availability is associated with 14.5 more music store closings in a state, which explains 58.7 percent of the observed music store closings in states that adopted VFLs. Examining the CD stores going out of business gives us an estimate of the impact of the internet on music CD sale, as well as an idea about its detrimental effect on employment. Each traditional CD store is a source of employment for many local workers, who might be forced into unemployment due to the subsequent closing down of these stores⁵.

As a second set of instruments, I use the variation across states in the household appliances adoption rate to instrument for state internet penetration rates. As a communication technology, it is not unusual that internet penetration closely mirrors that of the telephone.

⁴Music CD stores represented over 50.8 percent of total music sales in 1998 and this was reduced to 31.1 percent by 2007. This is a significant reduction in the share of sales by the music stores and the effect of increased internet access on an increase in the number of music stores closures might explain some of the observed reduction in sales.

⁵According to a news article in Rolling Stones, more than 5,000 record-company employees have been laid off since 2000.

Using the 1960 Census data for the percentage of people in each state having telephone connection interacted with year effects to instrument for online access (Stevenson 2006), I find that an increase in internet availability by 10 percentage points increase on average, the number of music stores that go out of business by four, which is lesser compared to that predicted by using the VFL to instrument for state internet penetration rates.

The rest of the paper is organized as follows. Section 2.2 discusses the related literature. Section 2.3 develops the estimation framework. Section 2.4 describes the data and reports the main empirical results. Section 2.5 concludes.

2.2 RELATED LITERATURE

A growing literature analyzes the impact of online file-sharing on music CD sales. The leading study to date is Liebowitz (2003), who looks at a 30-year time series of sales in the US record industry using numbers by the RIAA (Recording Industry Association of America) until 2002. He explains the annual trend in national record sales using a wide variety of factors including the macro-economy, demographics, changes in recording format and listening equipment, prices of albums and other entertainment substitutes, and changes in music distribution. He concludes that none of these factors can fully explain the decline in recent sales and hence it is file sharing that has reduced aggregate sales. Peitz and Waelbroeck (2003) provides cross-country evidence in support of the claim of losses due to internet piracy incurred by the music industry. Their results suggest that internet piracy played a significant role in the decline of CD sales in 2001, but was not substantial to account for the subsequent drop in 2002.

Using a panel of weekly album sales and information on the weekly number of downloads by album for the US, Oberholzer and Strumpf (2004) find that music downloading has an effect on sales that is statistically indistinguishable from zero. They use two identification strategies: across albums variation and within-album variation across weeks. To establish causality, they employ track length, network congestion and international school holidays

as instruments. This approach has some problems, however. The first obstacle is that CDs are durable goods and hence, if substitution or displacement occurs, it need not occur necessarily within one week. If an individual decides to download instead of purchase, his decision may very well be reflected in his future purchasing behavior; hence the absence of contemporaneous substitution does not rule out substitution more generally. Also, the variation in a particular album's popularity over time would tend to induce a spurious positive relationship between purchases and downloads.

Hui and Png (2003), use international panel data for 1994–98 to estimate that each additional download reduces sales by 6.6 percentage points. However, the time period they study predates the growth of broadband and widespread file sharing. Zentner (2003) uses international time-series aggregate data, in conjunction with internet connectedness, to document that places with more internet connections have experienced sharper reductions in album sales. The instrument he uses is a measure of internet sophistication across countries. A possible problem with this instrument is that, it is a choice variable, hence jointly endogenous with the interest in downloading music. Zentner mentions in his paper that a better instrument would be to use variation in internet availability at the regional level⁶. In this paper, I use exogenous variation in internet availability at the state level, since internet adoption across states closely followed that of VFLs and the household appliance adoption rate in the early years of 1960s.

Another group of researchers use phone surveys or internet panels to determine if individuals who download also purchase fewer music albums⁷. A general difficulty with these studies is that they do not consider the appropriate causal relationship. People might turn to purchasing faster network connections to download music just because there are fewer albums available for purchase due to local music stores closing down. An additional problem is the accuracy and the population sample of the data. Those who agree to have their inter-

⁶Zentner could not use this instrument, since he did not have information on internet availability at the within country regional level for the different countries in his dataset.

⁷These are mostly industry studies which have reached mixed conclusions about the effect of file sharing. The studies include Pew Internet Project (2000), Forrester (2002), IFPI (2002), Ipsos-Reid (2002), Jupiter Media Metrix (2002), Edison Media Research (2003), Nielsen//NetRatings (2003).

net behavior discussed or monitored are unlikely to be representative of all internet users⁸. In the CPS Computer and Internet Use supplements data, I overcome this sample selection problem by exploiting differences in the internet penetration at the state levels, as opposed to individual downloading behavior.

A third approach is to see how geographic variability in correlates of downloading, such as the availability of high-bandwidth internet access, influences record sales. The first study in this respect was Fine (2000), which was a legal battle against Napster. The plaintiff hired Soundscan, a company that developed an information system to capture point-of-sale data on music sales in more than 18,000 stores throughout the US. In the report, Fine compared sales means for the first quarter of years 1997, 1998, 1999 (when Napster was not available) and 2000 (when Napster was available), of all stores, within one mile of any college or university. He found that from the first quarter of 1999 to the first quarter of 2000, while national sales grew 6.6 percent, sales near all universities dropped 2.6 percent, sales near most wired schools dropped 6.2 percent and sales near schools where Napster was banned after the first quarter of 2000 fell 8.1 percent. However, as pointed out by Fader (2000), sales near universities were falling since 1998, at a time when Napster was not available and in which national sales were growing, casting doubts on the conclusion of Fine's report. In this paper I use plausible sources of exogenous variation in internet availability across states, namely the implementation of the VFL, as well as the household appliances adoption rate from 1960.

2.3 RESEARCH DESIGN AND THE IDENTIFICATION STRATEGY

Internet access became widely available to residential users by the middle of 1990s. For the first few years, a dial-up connection was the primary mechanism to access the internet,

⁸It is likely that individuals incorrectly self-report their downloading in phone data, especially since downloading is considered illegal. Another problem is, these internet surveys depend on individuals who willingly agree to have all of their internet behavior monitored, and such individuals are not likely to be representative of those who engage in illegal behavior.

in which, a standard telephone line is used for connecting to the internet. However, the implementation and availability of the internet has undergone a major change in the last few years. Adoption of high-speed internet at home grew twice as fast in 2005 than in the same time frame in 2004 (Joint Center, 2005). A significant part of the increase is tied to internet newcomers who have bypassed dial-up connections and gone straight to high-speed connections. Figure 1 shows the trend in internet access at home, with broadband connection experiencing a steady increase since June 2000. With computers now almost as common in American homes as cable television service, the internet continues to expand in importance as a form of communication, information, entertainment, and as the most important source for downloading music. Home broadband adoption in rural areas, (31 percent), continues to lag high speed adoption in urban centers and suburbs. Many states realize that the availability of broadband facilities is an important factor for economic growth and social welfare and there are some common actions taken by state governors to adopt strategies to facilitate broadband access. Adoption of statewide VFL is one such measure, which influences the deployment of broadband service since companies build infrastructure to simultaneously provide both video and broadband services (Windhausen, 2008).

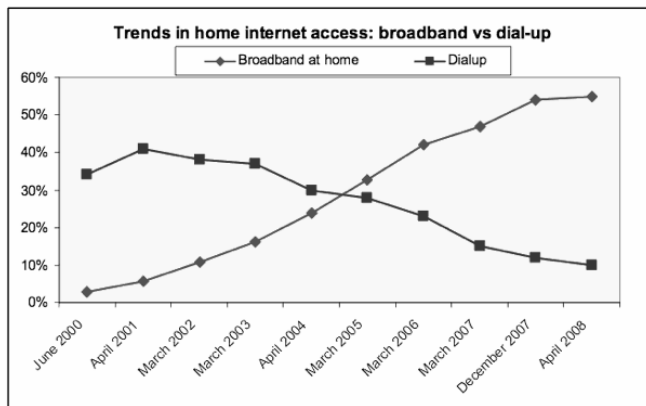


Figure 1: Trends in home internet access 2000 to 2008

Concurrently, a trend in the downturn of music sales has been observed globally over the last few years (Zentner, 2006). Music stores have been shrinking as a source of sales and are

mainly being replaced by online retail⁹. There has been a huge decline in the revenue from the sale of CDs since the year 2000 (see Figure 2). This downward trend in the sale from CD continues in spite of its real price falling by 9 percent over the last ten years (RIAA Year End Report, 2007).

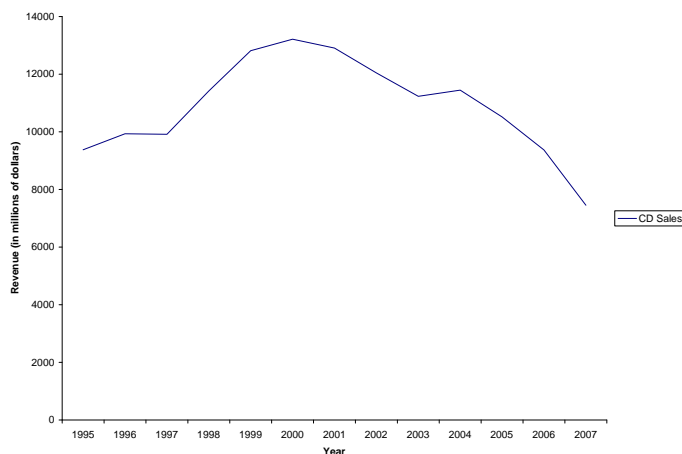


Figure 2: CD sales revenue figures 1995 to 2007

One of the most important factors which explains the fall is a huge increase in music downloading in the recent years. An associated rise in the access to the internet makes downloading faster and easier. Figure 3 provides visual evidence of the positive relationship between the VFL adoption and state internet penetration rates. The fitted line slopes upward, indicating that implementation of VFL by states coincides with higher internet access in these states.

In order to estimate the effect of state internet penetration on the music store closings, the following regression equation is used:

$$C_{s,t} = \alpha + \beta I_{s,t} + X'_{s,t} \gamma + \delta_s + \delta_t + \varepsilon_{s,t} \quad (1)$$

⁹According to the 2007 Consumer Profile Report of RIAA, the share of sales in music specialty stores declined from 50.8 percent in 1998 to 31.1 percent in 2007. At around the same time, digital downloads increased from 6 percent in 2005 to 12 percent in 2007.

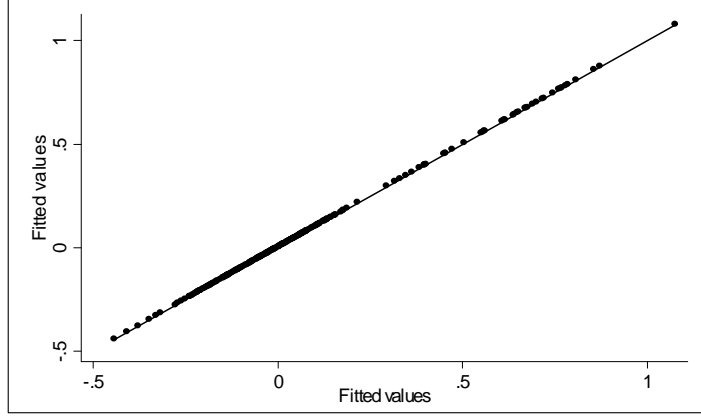


Figure 3: First-Stage:VFL states and the Internet penetration rates in 2007

For each state s at year t , the dependent variable $C_{s,t}$ is the total number of store closings. $X'_{s,t}$ is a vector of covariates including demographic characteristics such as mean education, age and household income of state s in year t . Since high-speed internet adoption has been concentrated among the young, educated and higher income group, the substitution of downloads for physical sale of music CDs and hence for store closings might be higher among the young, educated and higher-income, and thus be correlated with those demographic characteristics. $X'_{s,t}$ also includes the proportion of white and female in state s in year t . δ_s and δ_t represent the state-of-residence main effects and the year main effects respectively; $\varepsilon_{s,t}$ is a disturbance term and the variable $I_{s,t}$ captures the state internet penetration rates, which is the key regressor of interest. The parameter of interest in this regression is β , the sign of which determines the impact of downloading on CD store closings. However, the potential endogeneity concern between internet penetration rates and store closing makes it difficult to isolate the causal impact of downloading on CD store closings. One potential source of exogenous variation in the access to internet comes from the different rates at which it has diffused across states. In order to establish causality, I use the VFLs as a source of exogenous variation in broadband internet access. The causal relationship of interest is captured by the following reduced-form regression equation:

$$C_{s,t} = \alpha + \beta VFL_{s,t} + X'_{s,t}\gamma + \delta_s + \delta_t + \varepsilon_{s,t} \quad (2)$$

where $VFL_{s,t}$ is a dummy variable taking a value of one for each state s that adopted the law in year t .

In an alternative specification of equation (2), and following the approach in Stevenson (2006), I use the rurality of a state as a determinant of state internet penetration rates, as predicted by the adoption of telephone by households in the 1960 Census to compare with the results obtained from using VFLs as an instrument.

$$C_{s,t} = \alpha + \gamma phone_{s,t=1960} + X'_{s,t}\gamma + \delta_s + \delta_t + \varepsilon_{s,t} \quad (3)$$

Here $phone_{s,t=1960}$ is the percentage of people in state s who had a phone connection in the 1960 Census interacted with the year effects, which acts as an instrument for the state internet penetration rates. In addition to the set of controls mentioned above, $X'_{s,t}$ contains an additional set of covariates, the percentage of people in a state living on plots of land between one and ten acres in the 1960 Census interacted with the year effects. Since states with more people on large acres of land have lesser internet access, it is important to control for this state housing characteristic, otherwise the phone measures might overestimate the impact of downloading on store closures.

In another alternate specification, I control for the presence of big-box megastores like Wal-Mart and Best Buy, which are important competitors of CD stores apart from the downloading of music. Failing to control for the presence of these megastores might overestimate the impact of downloading on the CD store closings to the extent there might be correlation between states having above-average internet penetration and states having above-average presence of these big box retailers. In this case I estimate the following regression equation:

$$C_{s,t} = \alpha + \beta VFL_{s,t} + X'_{s,t}\gamma + \eta Megastores_{s,t} + \delta_s + \delta_t + \varepsilon_{s,t} \quad (4)$$

$Megastores_{s,t}$ denoting the number of big-box megastores in state s in year t . The results from the OLS and reduced-form estimation are reported in the next section.

2.3.1 *Exogeneity of the Video Franchise Laws*

In this section I address some of the potential concerns with my identification strategy. My estimation strategy is valid as long as we assume that the VFL adoption influenced store closings exclusively through internet penetration and not through some other channel. However, there could be other sources of bias, for example, a possibility that the states adopting VFLs might be the ones where the local governments are offering tax breaks or changing the zoning laws in order to bring big-box retailers or sports arenas which might serve as live concert venues to their area, which in turn might accelerate the demise of the independent music stores. On the other hand, there might be the possibility that states which enacted the VFLs are also passing small business subsidy programs or any other form of aid which could help music stores survive in the face of fierce competition from high speed internet access. Hence an important question to address here is, whether the adoption of the VFL could be endogenous. Of particular concern here is that some state characteristics like business friendliness or the state unemployment rates could be driving the VFL adoption and hence potentially biasing the results. In order to probe into the possible factors that might have driven the adoption of VFL, I estimate a probit model to calculate a predicted probability of adoption of the law based on the predictors¹⁰. In particular, I use the state business tax climate rankings reported annually by the Tax foundation as an indicator of the business friendliness of the states¹¹. I also considered the per capita state gross domestic products, state unemployment rates and mean household income of a state as other possible factors which might have influenced the VFL adoption.

¹⁰Table 34 in the appendix confirms that similar results hold even when we assume a linear model.

¹¹The raw correlation coefficient indicates a positive association between VFL adoption and business friendliness (though the correlation coefficient is only about 0.08). A particular concern is whether VFL adopting states are becoming less business friendly over time. I found that in contrary, these states are becoming increasingly business friendly over time (this is also true for the years before VFL adoption)

The results from the probit estimation are reported in Table 33 in the appendix¹². The results indicate that as the state business ranking increases (a higher rank means the state is less conducive to businesses), there is no statistically significant change in the likelihood of VFL adoption. A similar effect is estimated when the role of per capita state gross domestic product and mean household income are assessed on the adoption of VFL. However, the state unemployment rates appear to have increased the likelihood of VFL adoption¹³. In particular, an increase in state unemployment rate leads to a higher probability that the state passes a VFL. Since a VFL adoption increases investment in broadband which encourages economic growth, promotes businesses and results in increased job opportunities, it is not surprising that states with higher unemployment rates have taken an initiative in adopting the law. These state characteristics being possible factors driving VFL adoption could potentially lead to biasing the results if they happen to influence the store closures as well (my outcome variable of interest in the main regression). I discuss about the existence and any possible directions of these biases in the main results section.

One of the other concerns might be that the increased availability of the internet access due to the adoption of VFLs are offering some alternate forms of entertainment (other than downloading music) which people in these states might turn to as a substitute for downloading music. One such potential alternative could be to download movies. However, it should be kept in mind that downloading movies takes a much longer time compared to song tracks over the internet, which might suggest it is not a suitable substitute. The evidence from recent literature regarding the possible impact of substitutes such as movie viewership and DVD sales by Leibowitz (2003), suggests that these other forms of entertainment cannot explain the decrease in the sale of CDs, hence we can not conclude that people have switched to other sources of entertainment as a result of the faster internet access.

Finally, we need to recognize the possibility of other unobservable state-year specific shocks that might be correlated with the introduction of the VFLs. In particular, if consumer

¹²Table 35 in the appendix confirms that similar results hold even when we assume a linear model.

¹³I found no evidence of the age composition or the racial mix of the population being factors driving the VFL adoption.

preference is shifting away from music CDs towards MP3s faster in the states which adopted VFL compared to the Non-VFL adopting states, this could be a possible explanation for the increased number of music stores going out of business in the VFL states. Although it is very difficult to fully account for this kind of a taste-shifter, I include covariates for the demographic characteristics of the population of the states which might be able to approximate any changes in underlying consumer tastes. Also, as pointed out by Leibowitz (2003), a change in the taste for music cannot explain the recent downturn in the sale of music CDs¹⁴. Hence, while it is impossible to fully disprove such unobservable state-year shocks being correlated with VFL adoption, I am confident that, given the inclusion of demographic controls that might proxy for such state-year shifts (together with the findings in the literature regarding the nature of such changes in internet tastes and how they could not explain trends in CD sales), such concerns can be largely ruled out in my analysis.

¹⁴In particular, he used the financial success of concerts as a measure of the market valuation of music and found that there had been an increase in the real value of the concern revenues, which rules out the concern of a dissatisfaction with the state of music. Recent figures from RIAA indicate that revenue from concert sales has gone up from 1.6 percent in 2004 to 3 percent in 2008.

2.4 DATA AND MAIN RESULTS

2.4.1 *Data Sources and Descriptive Statistics*

Data on the total number of music store closings for a given state and year are obtained from Almighty Institute of Music Retail for the years 2003 to 2008. Almighty Institute of Music Retail is an industry research group that collects data to keep up a comprehensive retail database, in order to better facilitate communications between record labels and music retail, mainly for marketing purposes. Almighty Institute maintains detailed records of all the music stores in each state in the U.S. that went out of business since 2003. They claim to have assembled a comprehensive directory of every US prerecorded music outlet, based on the type of store¹⁵.

The data on state internet penetration rates are obtained from the Current Population Survey Computer and Internet Use Supplements. The October 1997, December 1998, August 2000, September 2001, October 2003 and October 2007 CPS Computer and Internet Use Supplements ask respondents about their households' computer and internet use. This is in addition to the usual survey questions on employment, demographics, geographic location and educational attainment of each individual surveyed. The information on the adoption of VFLs by different states are obtained from Save Access¹⁶ and Miller & Van Eaton¹⁷. Data on variation in household appliances adoption rate across states is obtained from the Integrated Public Use Microdata Series (IPUMS) from the U.S. Census of Population for 1960, one-percent sample.

Table 1 shows the descriptive statistics for the overall sample, as well as among the states which adopted VFL and the states which did not implement VFL for the period of 1997-

¹⁵The various categories into which each store is divided are: Independent Stores, Chain Stores, Big-box retailers, Online/Mailorder Retailers, One Stops, Mass Merchants or Lifestyle/Nontraditional retailers with music sections.

¹⁶The primary work of Save Access is to track legislative issues and news articles around PEG and Local Video Franchising in order to serve the PEG community (<http://saveaccess.org>).

¹⁷Miller & Van Eaton is a law firm offering specialized services in communications law. It covers a wide range of issues that relate to every communications industry: cable television, broadcasting, telephony, and wireless communications (<http://www.millervaneaton.com/>).

1998, 2000-2001, 2003 and 2007. There are some differences to be noted from these summary statistics. The average number of CD stores that went out of business (the outcome variable) were higher in the states that adopted VFL (12 stores) compared to the ones which did not (4 stores)¹⁸. The state internet penetration rates were higher by 0.5 percentage point in states which passed the VFL compared to the rest of the states. Table 1 also summarizes other covariates used in the analysis. The proportion of married, fraction of female in the total population, and other demographics are very similar across the VFL and Non-VFL states. Also, there is very little difference in the average educational attainment of the residents between these two types of states. The VFL states have a higher median household income on average compared to the states which did not adopt the VFL. These descriptive statistics suggests that the difference in the number of CD store closures between the two groups of states might be explained by the increase in the internet access associated with the implementation of VFL. In a later part of this section, I turn to the regression analysis in order to control for state and year effects in addition to a set of covariates explained above.

The summary statistics for the music store closings for the years 2003 through 2007 are reported in Table 2. Both the total and average number of store closings were highest in 2006, with the state of California having the largest number (116).

Figure 4 shows the total number of store closings in each of the years 2003 through 2008. We can see that the number of music stores that went out of business increased sharply between 2003 and 2004, then declined somewhat in 2005, and there were a substantially higher number of stores closures between 2005 and 2007.

Figure 5 shows the states which adopted VFL separately for each of the years 2005 through 2007, and Figure 6 depicts the total number of music store closings for each state in 2007¹⁹

¹⁸Considering only the year 2007 (when all of the 18 states had enforced VFL), the difference in the number of storeclosings between the states adopting VFL (19 stores) and states which did not (6 stores) are even higher.

¹⁹The first, second and third quartiles of the total store closures are the criteria for this division.

Table 1: Summary Statistics

| | Overall | VFL States | Non-VFL States |
|----------------------|----------------------|------------------------|-----------------------|
| Stores Closed | 7.29 (1.20) | 12.37 (2.57) | 4.27 (0.87) |
| Internet Penetration | 43.76 (1.06) | 43.97 (1.67) | 43.49 (1.35) |
| Married | 55.91 (0.28) | 56.37 (0.22) | 55.71 (0.42) |
| Female | 0.51 (0.0004) | 0.51 (0.001) | 0.51 (0.001) |
| White | 0.83 (0.006) | 0.82 (0.006) | 0.84 (0.01) |
| Black | 0.12 (0.07) | 0.14 (0.006) | 0.104 (0.009) |
| Hispanic | 0.08 (0.005) | 0.09 (0.009) | 0.07 (0.006) |
| Less than HS | 23.21 (1.05) | 23.75 (1.69) | 23.05 (1.33) |
| College degree | 14.17 (0.36) | 14.09 (0.57) | 14.12 (0.46) |
| Income | 49577.03 (513.04) | 51104.82 (719.1525) | 48426.82 (679.022) |
| State unemployment | 4.54 (0.07) | 4.52 (0.09) | 4.56 (0.09) |
| Fraction of States | | 0.38 | 0.62 |
| Observations | 294 | 114 | 186 |

Notes: Standard errors are in parenthesis. The internet penetration data are from CPS Computer Use and Supplements for the years 1997-1998, 2000-2001, 2003 and 2007. The store closings data are from Almighty Research for the years 2003 and 2007. The figures reported for the number of stores closed pertain to the years' 2003 through 2007, since these are the years for which the store closings data are available.

Table 2: Summary Statistics: Store closures

| | 2003 | 2004 | 2005 | 2006 | 2007 |
|----------------------------------|---|-----------------|---|-----------------|-----------------------|
| Average number of Store closings | 3.22 (0.56) | 12.10 (1.98) | 7.41 (1.36) | 16.35 (2.74) | 11.37 (2.19) |
| Median number of Store closings | 2 (MO) | 7 (OR) | 3 (RI) | 12 (AZ) | 5 (WI) |
| Minimum number of Store closings | 0 (AR, MD, NH, ID, ME, MT, NV, ND, RI, VT, WY) | 0 (WY) | 0 (DC, AL, DE, ID, ME, NH, SD, VT, WY) | 0 (DE, WV) | 0 (MT, RI, VT, WV) |
| Maximum number of Store closings | 14 (CA, MI) | 70 (CA) | 40 (CA) | 116 (CA) | 86 (CA) |
| Total number of Store closings | 160 | 593 | 363 | 801 | 557 |
| No. of observations | 49 | 49 | 49 | 49 | 49 |

Notes: Standard errors are in parenthesis. The abbreviations for the states pertain to the State Fips Code classified by the Bureau of Labor Statistics. The store closings data are from Almighty Research for the years 2003 through 2007.

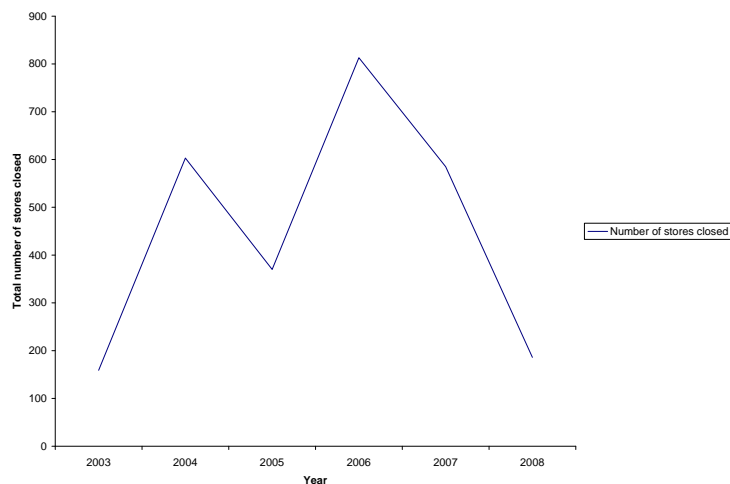


Figure 4: Total number of Store closures from 2003 through 2008

Table 3 reports the descriptive statistics among the rural and urban states, as defined by the household telephone adoption rate in the 1960 Census. The summary statistics show that internet penetration rates are 46 percent in the more urbanized states compared to 39 percent in the rural states. These states are classified as urban or rural based on the percentage of people in each state having a phone connection in the 1960 Census dataset²⁰. There is very little difference in the demographic characteristics and educational attainment between the residents of urban and rural states. The median household income is higher for the urban states, while the state unemployment rate is lower.

As alternative outcome variables, I use the different categories of stores that went out of business in my sample, to see whether internet availability affected one type of stores more than another, or whether there might have been a substitution among the different types of stores. Tables 27 and 28 in the appendix report the descriptive statistics for the types of store closings and those between the VFL and non-VFL states as well as the rural and urban states respectively. Next we turn to the regression analysis and discussion of the estimates.

²⁰Since around 70 percent of the households on average had a phone connection in the 1960 Census, if the percentage of people in a particular state having a telephone is less than 70 percent, it is classified as a rural state.

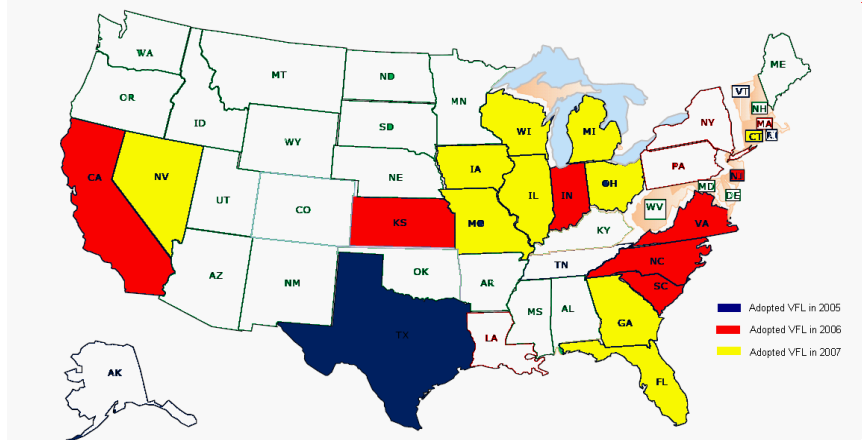


Figure 5: States which adopted VFL in 2005, 2006 and 2007

2.4.2 OLS Estimates

The relationship of interest is captured by equation (1), where variable $I_{s,t}$ is the regressor of interest. I estimate various specifications of equation (1) for store closures and the results are shown in Table 4.

Specifications 1 and 2 reports results which includes the state of residence main effects as well as year main effects, whereas specifications 3 and 4 shows result without considering the state-fixed effects. Specification 1 shows results from models where the standard errors are adjusted to cluster at the state level (reported in parenthesis) and when standard errors are adjusted for unknown forms of heteroskedasticity and serial correlation (reported in brackets). In specification 2, a heteroskedastic error structure with no cross-sectional correlation is assumed. The coefficient on $I_{s,t}$ has the expected sign in all the specifications, although it is statistically significant only when I assume heteroskedastic error structure with no cross-sectional correlation, both with and without considering the state-fixed effects, as reported in columns 2 and 4 of table 4 respectively. The results indicate that if state internet penetration rates increase by ten percentage points on average, ten additional music stores go out of business in a given state for a given year. The OLS results imply that, for the average observed increase in internet penetration rates between years 2003 and 2007 in VFL states,

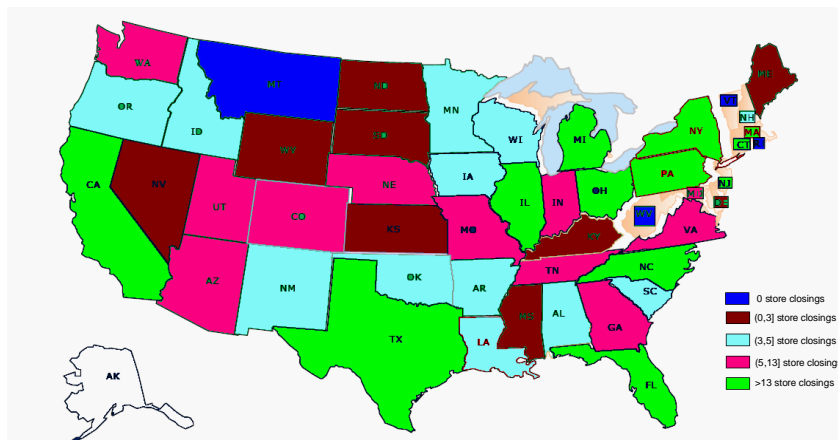


Figure 6: Map of the states with store closings in 2007

we would expect to see an increase in music store closings by ten. The OLS estimation controls for all of the observed covariates discussed in the section on research design and identification strategy.

Using the different categories of store as alternative outcome variables, the results reported in table 29 in the appendix indicate that an increase in internet availability differentially affected store closures based on the type of store. The estimates suggest that there were more independent stores closing down compared to chain stores or the big box retailers. In fact, there seems to be a substitution across the different types of store closures, i.e., an increase in independent store closings and a decrease in big-box and chain store closings

As argued above, the OLS results must be interpreted with some caution because the internet penetration rate is likely to be endogenous, with the most prominent source of bias being a simultaneity between a strong taste for music and acquisition of internet connections. Alternatively, it might be true that people switch to downloading music and thus increasing the demand for faster internet connections whenever local music stores go out of business. Given these potential sources of bias, I turn to an IV approach.

Table 3: Summary Statistics: Rural versus Urban States

| | Overall | Urban States | Rural States |
|----------------------|----------------------|----------------------|---------------------|
| Stores Closed | 7.29 (1.20) | 8.06 (1.706) | 6.14 (1.5) |
| Internet Penetration | 43.76 (1.06) | 46.16 (1.35) | 39.67 (1.71) |
| Married | 55.91 (0.28) | 56.50 (0.25) | 56.36 (0.22) |
| Female | 0.51 (0.0004) | 0.50 (0.001) | 0.51 (0.001) |
| White | 0.83 (0.006) | 0.87 (0.01) | 0.79 (0.01) |
| Black | 0.12 (0.07) | 0.07 (0.004) | 0.16 (0.01) |
| Hispanic | 0.08 (0.005) | 0.07 (0.004) | 0.09 (0.01) |
| Less than HS | 23.21 (1.05) | 21.11 (1.31) | 27.09 (1.83) |
| College degree | 14.17 (0.36) | 15.02 (0.48) | 12.46 (0.54) |
| Income | 49577.03 (513.04) | 52367.39 (621.07) | 44541.2 (692.28) |
| State unemployment | 4.54 (0.07) | 4.30 (0.08) | 4.83 (0.09) |
| Fraction of States | | 0.63 | 0.36 |
| Observations | 294 | 102 | 186 |

Notes: Standard errors are in parenthesis. The states are classified as urban or rural depending on the percentage of people in each state having a telephone connection in the 1960 Census. The internet penetration data are from CPS Computer Use and Supplements for the years 1997-1998, 2000-2001, 2003 and 2007. The store closings data are from Almighty Research for the years 2003 and 2007.

Table 4: OLS Estimates of the Effect of State Internet Penetration Rates on Store closures

| | Store closures (1) | Store closures (2) | Store closures (3) | Store closures (4) |
|-----------------------------------|-----------------------------|-----------------------|-----------------------------|-----------------------|
| Internet penetration rates | 1.267 [1.166] (1.656) | 1.025*** (0.291) | 0.984 [0.687] (0.741) | 1.047*** (0.279) |
| Controls for state fixed effects? | Yes | Yes | No | No |
| Observations | 98 | 98 | 98 | 98 |

Notes: Standard errors in brackets are adjusted for unknown forms of heteroskedasticity and serial correlation. Standard errors in parenthesis are robust. The Internet penetration data are from CPS Computer Use and Supplements and the store closings data are from Almighty Research for the years 2003 and 2007. ***denotes significance at the 1% level. All the regressions are weighted by population and include year-fixed effects. Other controls include population, mean education, age, and the proportion of female, white, black and Hispanic for each state-year. Columns (3) and (4) reports results for regressions without including the state-fixed effects.

2.4.3 *Reduced-form Estimation and Main Results*

In order to estimate the parameter β in equation (1) consistently, we require a variable which is correlated with $I_{s,t}$ but not with the error term $\varepsilon_{s,t}$. As discussed above, I instrument the endogenous regressor $I_{s,t}$ with $VFL_{s,t}$, the dummy variable indicating the adoption of a Video Franchise Law. The first-stage regression model is specified as follows:

$$I_{s,t} = \alpha + \beta VFL_{s,t} + X'_{s,t}\gamma + \delta_s + \delta_t + \varepsilon_{s,t} \quad (5)$$

A positive relationship is depicted in Figure 7, which plots the internet penetration rates (ipr) against the VFL dummy for the year 2007 (when all of the eighteen states had implemented the law).

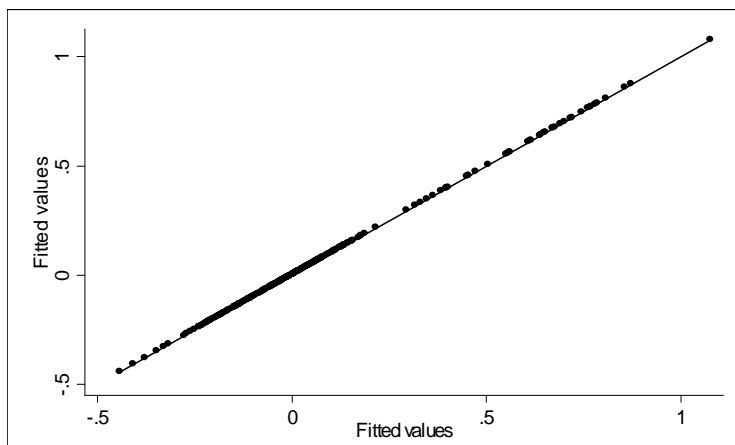


Figure 7: First-Stage:VFL states and the Internet penetration rates in 2007

A positive relationship is depicted in Figure 5, which plots the internet penetration rates (ipr) against the VFL dummy for the year 2007 (when all of the eighteen states had implemented the law). Table 5 reports the estimates from the first-stage regression. In all the specifications (depending on the assumptions on the error structure as explained above),

Table 5: First Stage Estimates of the Effect of VFL Adoption on State Internet Penetration Rates

| | Internet penetration rates (1) | Internet penetration rates (2) | Internet penetration rates (3) | Internet penetration rates (4) |
|--------------------------------------|--------------------------------------|--------------------------------------|--------------------------------------|--------------------------------------|
| VFL | 0.735 [0.650] (0.702) | 1.013*** (0.456) | 1.269 [0.932] (0.969) | 1.740*** (0.581) |
| Controls for state fixed effects? | Yes | Yes | No | No |
| Observations | 294 | 294 | 294 | 294 |

Notes: Standard errors in brackets are adjusted for unknown forms of heteroskadasticity and serial correlation. Standard errors in parenthesis are robust. The Internet penetration data are from CPS Computer Use and Supplements for the years 1997, 1998, 2000, 2001, 2003 and 2007. ***denotes significance at the 1% level. All the regressions are weighted by population and include year-fixed effects. Other controls include population, mean education, age, and the proportion of female, white, black and Hispanic for each state-year. Columns (3) and (4) reports results for regressions without including the state-fixed effects.

we find a sizeable and positive relationship between $I_{s,t}$ and the corresponding instrumental variable $VFL_{s,t}$. For example, the entry in the second column of Table 5 indicates that adoption of VFL by state s in year t is associated with a 10 percentage point increase in the internet penetration rate. The t-statistics for the significance of the estimated coefficient on the instrument is 2.99 when feasible generalized least squares is employed to allow for heteroskadasticity across states, without including the state-fixed effects (column 4 in Table 5). The reason for a weak first-stage might be the lack of sufficient data for the period in which most of the states implemented the VFL²¹.

Using the phone measures as an instrument for state internet penetration rates, as explained in section 2.4.3, we obtain positive and statistically significant relationship between $I_{s,t}$ and the corresponding instrumental variable $phone_{s,t}$. Figure 8 shows the state internet penetration rates in 2003 plotted against the predicted internet penetration using state ownership rates of telephones in 1960.

²¹Data for state internet penetration rates are not available for 2005 and 2006, when about 11 states adopted VFL.

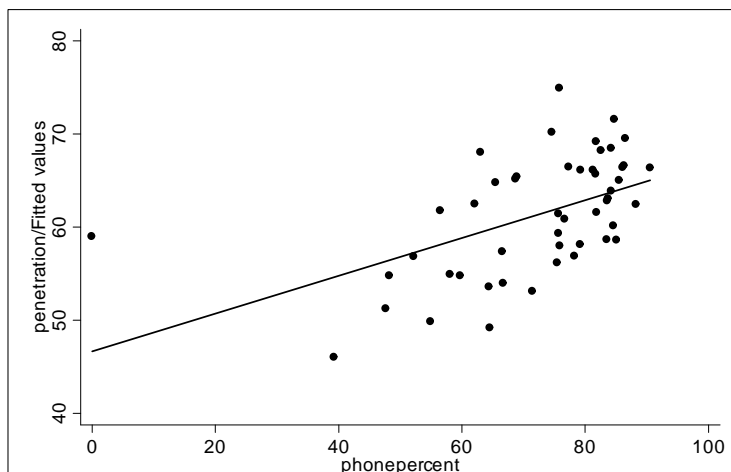


Figure 8: Internet Penetration predicted by 1960 phone ownership rates

The figure clearly demonstrates the huge predictive power of the historical patterns of adoption of telephone for internet penetration across states. It strongly suggests that the internet is diffusing to households in a way that is very similar to the pattern of other household innovations, in particular that of telephone. The relationship depicted in Figure 8 exists for all the years of data. But an useful instrument for state internet penetration rate should have a time-varying component, since the internet diffusion across the states is not uniform over the years. For this purpose, I use the 1960s phone measures interacted with year effects to instrument for internet access across the states. The first stage of this regression is shown in Table 6. In our preferred specification (column 1 in Table 6), where robust standard errors are clustered at the state level, a 10 percentage points increase in ownership of telephone is associated with a 2.5 percentage points increase in the internet availability (for the year 2001). The F-statistics associated with the estimated coefficients on the instruments indicate a strong first-stage.

Figure 9 shows the average number of store closures between the states which adopted VFL and the states which did not. The states which implemented the VFL saw a higher increase in the number of stores that went out of business, owing to the increase in internet access that is associated with the adoption of the law.

Table 6: First Stage Estimates of the Effect of phone measures on State Internet Penetration Rates

| | Internet penetration rates (1) | Internet penetration rates (2) | Internet penetration rates (3) |
|--------------------------------------|--------------------------------------|--------------------------------------|--------------------------------------|
| Phone*Year=1997 | 0.008 (0.069) | 0.008 (0.08) | -0.018 (0.058) |
| Phone*Year=1998 | 0.167*** (0.068) | 0.167*** (0.072) | 0.126*** (0.059) |
| Phone*Year=2000 | 0.246*** (0.073) | 0.246*** (0.075) | 0.211*** (0.056) |
| Phone*Year=2001 | 0.251*** (0.056) | 0.251* (0.071) | 0.209*** (0.056) |
| Phone*Year=2003 | 0.224*** (0.065) | 0.224* (0.072) | 0.172*** (0.057) |
| Phone*Year=2007 | 0.134** (0.068) | 0.134** (0.077) | 0.068 (0.060) |
| Controls for state fixed effects? | Yes | Yes | Yes |
| F-statistic | 14.97 | 11.09 | Chi2= 120.59 |
| Observations | 294 | 294 | 294 |

Notes: Standard errors are in parenthesis. The Internet penetration data are from CPS Computer Use and Supplements for the years 1997, 1998, 2000, 2001, 2003 and 2007. *denotes significance at the 1% level. ** denotes significance at the 5% level. All the regressions are weighted by population and include year fixed effects. Other controls include population, mean education, age, and the proportion of female, white, black, Hispanic and percentage of people living on large acres of land for each state-year.

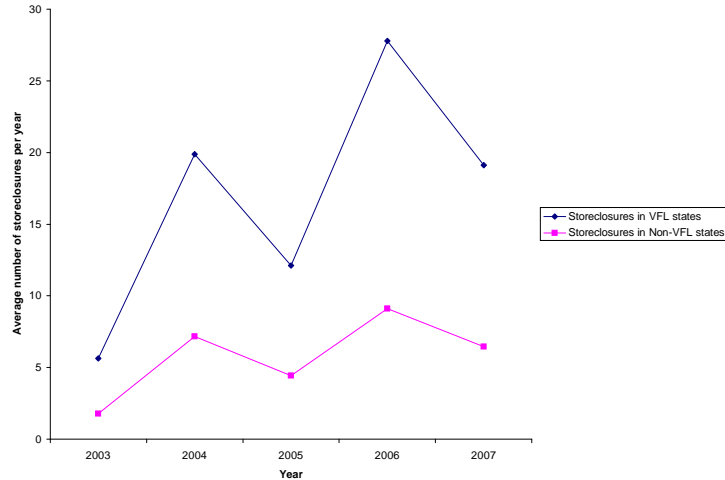


Figure 9: Total number of Store closures from 2003 through 2007

Figure 10 shows visual evidence of the reduced-form relationship between the store closures and the instrument. The fitted line has a positive slope, indicating that the adoption of VFL coincides with a higher number of music stores going out of business.

The reduced-form estimates of equation (2) represents the main results which are shown in Table 7. The findings are consistent with the existence of a positive statistically and economically significant causal impact of VFL adoption on store closures. When we adjust the standard errors for unknown forms of heteroskedasticity and serial correlation (Table 7 column 1), the reduced-form estimate implies that adopting VFL causes on an average, 14 more store closures in a state for a given year. This indicates that an increase in internet availability, through the adoption of VFL by the states, explains 58.7 percent of the total store closings in those states. The model also explains 15.8 percent of all store closures in the US²²

The estimation results for equation (3) are reported in Table 8. The coefficients on $phone_{s,t}$ have the expected sign in all three specifications. However, the findings are generally

²²Note that the specifications do not control for the presence of Megastores. The results including the presence of Megastores as control are reported in table 10.

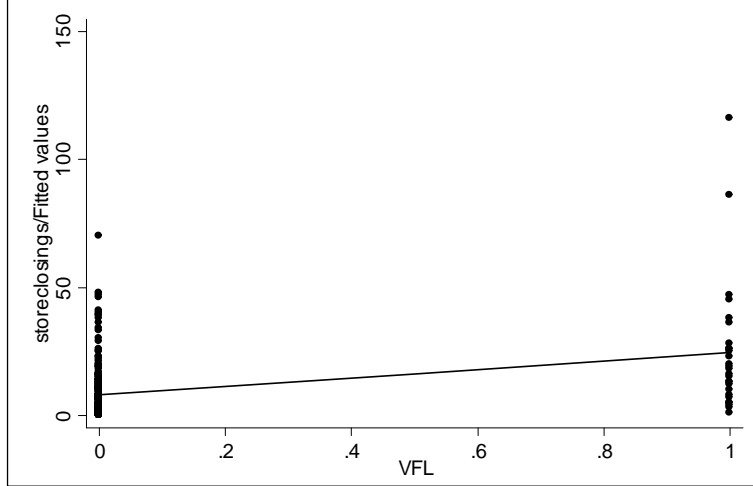


Figure 10: Reduced-form:store closings and VFL adoption by states

consistent with a positive and statistically significant causal impact only when a feasible generalized least squares method is employed to allow for heteroskedasticity²³. These results indicate that a 10 percentage points increase in phone ownership is associated with 4 more store closings in a state for a given year (result for 2006, column 3 in Table 8). Hence the reduced-form approach yields results that are broadly consistent with the OLS results, thus providing supporting evidence for their findings.

The two-stage least-squares (2SLS) estimates of equation (2) represents the IV results which are reported in table 9. Here the state internet penetration rates are instrumented by the VFL dummy. These results indicate an economically significant positive causal relationship between the states adopting the VFL and store closures. However, because of the sparsity of data, the results are not statistically significant at any common levels of confidence. The IV results suggest that state internet penetration rate is mediating the relationship between VFL and store closings that we find in the reduced form estimation. Using the adoption of telephones in the 1960 Census as an instrument for internet penetration, the 2SLS estimates in table 26 in the appendix indicate a similar relationship between phone

²³Homoskedasticity may be a restrictive assumption for panels, where the cross-sectional unit, in this case the states may be of varying size and as a result may exhibit different variation.

Table 7: Reduced-form Estimates of the Effect of VFL Adoption on Store closures

| | Store closures (1) | Store closures (2) | Store closures (3) | Store closures (4) |
|--------------------------------------|---------------------------------|-----------------------|---------------------------------|-----------------------|
| VFL | 14.471*** [6.571] (9.017) | 8.344*** (1.739) | 19.435*** [7.058] (8.550) | 13.347*** (1.739) |
| Controls for state fixed effects? | Yes | Yes | No | No |
| Controls for year fixed effects? | Yes | Yes | Yes | Yes |
| Observations | 245 | 245 | 245 | 245 |

Notes: Standard errors in brackets are adjusted for unknown forms of heteroskedasticity and serial correlation. Standard errors in parenthesis are robust. The store closures data are from Almighty Retailers for the years 2003 through 2007. ***denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level. All the regressions are weighted by population and include year-fixed effects. Other controls include population, mean education, age, and the proportion of female, white, black and Hispanic for each state-year. Column (2) and Column (4) reports results from GLS estimation. Columns (3) and (4) reports results for regressions without including the state-fixed effects.

Table 8: Reduced-form Estimates of the Effect of phone measures on Store closures

| | Store closures (1) | Store closures (2) | Store closures (3) |
|--------------------------------------|-----------------------|-----------------------|-----------------------|
| Phone*Year=2003 | 0.155 (0.276) | 0.155 (0.298) | 0.219 (0.157) |
| Phone*Year=2004 | 0.273 (0.264) | 0.273 (0.302) | 0.291** (0.159) |
| Phone*Year=2005 | 0.297 (0.289) | 0.297 (0.312) | 0.305** (0.160) |
| Phone*Year=2006 | 0.347 (0.275) | 0.347 (0.304) | 0.381*** (0.158) |
| Phone*Year=2007 | 0.363 (0.281) | 0.363 (0.331) | 0.355*** (0.166) |
| Controls for state fixed effects? | Yes | Yes | Yes |
| F-statistic | 2.52 | 1.62 | Chi2= 24.03 |
| Observations | 245 | 245 | 245 |

Notes: Standard errors are in parenthesis. The store closures data are from Almighty Retailers for the years 2003 through 2007. All the regressions are weighted by population and include year-fixed effects. Other controls include population; mean education, age, the proportion of female, white, black, Hispanic and percentage of people living on large acres of land for each state-year. *** denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level.

Table 9: 2SLS Estimates of the Effect of VFL Adoption on Store closures

| | 2SLS (1) | 2SLS (2) |
|-----------------------------------|--------------------------------|--------------------------------|
| VFL | 21.823 [25.574] (36.261) | 13.229 [10.320] (10.764) |
| Controls for state fixed effects? | Yes | No |
| Observations | 245 | 245 |

Notes: Standard errors in brackets are adjusted for unknown forms of heteroskedasticity and serial correlation. Standard errors in parenthesis are robust. The store closures data are from Almighty Retailers for the years 2003 and 2007. All the regressions are weighted by population and include year-fixed effects. Other controls include population, mean education, age, and the proportion of female, white, black and Hispanic for each state-year. Column (2) reports results for regressions without including the state-fixed effects.

connection and store closings, which supports the reduced-form results.

The reduced form results for the alternate outcome variables depending on the type of stores is reported in table 30 in the appendix. The estimates imply that there has been an increase in the number of independent, chain and other category of store closures and a decrease in big-box retail stores, following the adoption of VFL. These results indicate that the increase in internet availability through the adoption of VFLs in the states which implemented it, have not uniformly affected the different types of music store closures. There have been more independent and chain store closures compared to the big-box retailers, suggesting a substitution among the different categories of music stores.

In an alternate specification of the reduced-form regression equation, I controlled for the presence of big-box megastores like Wal-Mart and Sam's Club, which are the main competitors of the music CD stores. The results from the reduced-form estimation are reported in table 10. Adjusting the standard errors for unknown forms of heteroskedasticity and serial correlation (Table 10 column 1), the reduced-form estimate implies that adopting VFL causes on an average, 9 more store closures in a state for a given year.

Now I turn to a discussion about the existence of any possible sources of bias in the main estimation regressions. From the above discussion of the exclusion restrictions we identified state unemployment rates as a factor that might have driven the adoption of VFL. I included this factor in my main regression. Also I included the state business rankings to see whether

Table 10: Reduced-form Estimates of the Effect of VFL Adoption on Store closures controlling for the presence of megastores

| | Store closures (1) | Store closures (2) | Store closures (3) | Store closures (4) |
|--------------------------------------|--------------------------------|-----------------------|--------------------------------|-----------------------|
| VFL | 9.276** (6.358) [8.184] | 6.161*** (1.483) | 13.330** (8.261) [9.626] | 6.291*** (2.165) |
| Megastores | 0.508*** (0.193) [0.168] | 0.377 (0.099) | 0.084 (0.021) [0.028] | 0.109 (0.013) |
| Controls for state fixed effects? | Yes | Yes | No | No |
| Observations | 245 | 245 | 245 | 245 |

Notes: Standard errors in brackets are adjusted for unknown forms of heteroskedasticity and serial correlation. Standard errors in parenthesis are robust. The store closures data are from Almighty Retailers for the years 2003 through 2007. ***denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level. All the regressions are weighted by population. Other controls include population, mean education, age, and the proportion of female, white, black and Hispanic for each state-year. Columns (3) and (4) reports results for regressions without including the state-fixed effects.

this leads to any potential bias. Including this index is important, since it might be that stores in states which are more business friendly face lower costs, thereby being less likely to incur losses and thus more likely to stay in business longer; or perhaps being more business friendly means that the companies will have a faster turnover and that will accelerate the demise of the CD stores. So theoretically, the impact of the business friendliness of a state on store closures (and hence the sign of the possible bias) is not clear a priori. The results reported in table 35 in the appendix indicate that an increase in business ranking (i.e.: states becoming less business friendly) leads to more store closures, which might lead to a potential downward bias of the reduced form estimates. However, since business friendliness is uncorrelated to VFL adoption, this does not affect the observed relationship between the VFL regressor and store closures²⁴. The state unemployment rates have no statistically significant impact on store closures which rules out the possibility of any bias on my reduced form estimates. A positive coefficient on per capita state gdp might indicate a potential upward bias on the reduced form estimates, but again, since per capita state GDP is uncorrelated to the VFL

²⁴I also checked whether including these covariates one at a time changes the result; the results are very similar to the ones reported in table 35.

adoption, this does not affect the observed relationship between the VFL regressor and store closures²⁵ ..

2.4.4 *Falsification*

In order to support the validity of my instrument for state internet penetration rates, in this section I present a falsification exercise pretending that the adoption of VFL by the states occurred in 2003 instead of in 2005, 2006 or 2007. The first-stage regression model is specified as before:

$$I_{s,t} = \alpha + \beta VFL_{s,t} + X'_{s,t}\gamma + \delta_s + \delta_t + \varepsilon_{s,t} \quad (6)$$

with the VFL dummy taking a value of one between the years 2003 and 2004 for the 18 states which adopted the Video Franchise Law. Table 31 in the appendix reports the estimates from the first-stage regression of this falsification exercise. In all of the specifications (depending on the assumptions on the error structure as explained above), I find point estimates which are small and not statistically significant. This exercise confirms that the positive impact of adoption of VFL on internet penetration rates in the states which adopt the law are indeed true effects.

Table 32 in the appendix shows the results for the reduced-form estimates from the falsification exercise. In my preferred specification (when the standard errors are adjusted for unknown forms of heteroskedasticity and serial correlation) as well as other specifications including the state-fixed effects, I find no effect of the adoption of VFL on store closures in those states. However, the estimates without including the state of residence main effects are statistically different from zero, indicating that the VFL-adopting states might be having more store closures even before the adoption of the law. When I repeated the exercise

²⁵It is somewhat counter intuitive that an increase in per capita state gdp (which is an indication of the economic prosperity of a state) is causing more music CD store closings. I further checked whether different types of stores were affected differently and found an increase in per capita state gdp causes an increase in independent store closings but a reduction in big-box retailers. This is in line with the argument provided in the discussion of exclusion restrictions that states might be bringing big-box retailers or sports arenas which in turn might accelerate the demise of the independent music stores.

pretending VFL was adopted two years prior to when it actually was, the results are qualitatively similar and are reported in panel B of table 32. In spite of not being able to rule out a concern about an upward trend in store closures in the VFL adopting states before the adoption, to the extent that the falsification test is satisfied for my main specification, this is not a concern here.

2.5 CONCLUSIONS

Internet usage has increased dramatically over the past few years. At about the same time, the sales from music has witnessed a huge decline. In this paper, I use two sources of exogenous variation in the internet access across US states to measure the impact of music downloads on the progressive disappearance of the traditional music CD stores. Specifically, I use information on the passage of Video Franchise Law and the differences in the household telephone adoption rate in the 1960 Census as the two sources of variation in the availability of the internet access across the states. The sale of CDs is a very important source of revenue for the music industry, accounting for more than 85 percent of total music sold in the US. The share of record stores, which represent a significant portion of the sale from CDs saw a decline of over 8 percent within the last three years (RIAA Consumer Profile, 2007). Hence, it is crucial to estimate the extent to which a change in the consumer preference in the form of downloading music is driving out the old format.

Using CPS Computer and Internet Use Supplements data for state internet penetration rates and CD store closings data from Almighty Institute of Music Retail, I find that the implementation of VFL increases the availability of internet access in the states which adopt it, and explains 58.7 percent of the total store closings in those states. Also, a 10 percentage points increase in the phone ownership is associated with 4 more stores going out of business in a state for a given year. The results suggest that a variability in the access to the internet can explain the difference in the music store closures across states. These empirical results support the idea that technological progress in the household in the form of increased internet access played an important role in the demise of the traditional music CD stores. Although

due to data limitations I am using store closings as my outcome variable of interest, in the event of data availability, looking at the total number of stores in a given region over a period of time would have been a preferable outcome²⁶.

These results have significant implications for public policy. While the adoption of VFLs by states have the impetus for economic growth and social welfare, there is some caution to be exercised by state governors while implementing these policies. In addition to the detrimental effects of illegal downloading on the music sales that is well documented in the literature, the increased availability in faster network connections now urge a rise in the legal downloads as well. This aggravates the situation for an already struggling music industry by driving the traditional music stores out of business and further reducing revenue from the sale of CDs.

²⁶To address the concern of whether there were a large number of music store openings in the states which adopted VFL over the period of time, I did a Lexis-Nexus search of relevant newspaper articles. I found there were very few store openings over this period. Specifically, there were 5 openings in New York, 4 in California, 3 in Massachusetts and 1 in Ohio.

3.0 THE EFFECT OF PARENTAL LEAVE ON FEMALE EMPLOYMENT: EVIDENCE FROM STATE POLICIES

3.1 INTRODUCTION

Over the past decades, parental leave policies have been an important issue of debate given their possible impacts on employment, leave taking and earnings. In particular, since 78 percent of labor force is composed of families where both parents work (Grant *et. al*, 2005), parental leave policy is a relevant topic both at the federal and state level of the government. Research on the effect of such leave policies are particularly important given the recent trends in female employment. For instance, more than half (52 percent) of women with children under the age of one were employed in 1998 compared to only two-fifths ten years earlier (Waldfogel, 1999).

In spite of the substantial increase in the percentage of women with young children in the labor force, the Family and Medical Leave Act (FMLA) is the only law in the United States at the federal level to directly address family leave issues. In particular, FMLA requires employers with 50 or more employees to allow twelve weeks of unpaid, job-protected leave following the birth or adoption of a child. Enacted in 1993, the legislation covers private, state and local government employees who worked at least 1250 hours in the last 12 months. Therefore, year 1993 is identified as the beginning of job-protected maternity leave policies at the federal level. In addition, given that the percentage of employees under maternity leave coverage increased dramatically after the enforcement of FMLA (86 percent in 1995 as compared to 39 percent 1991, Waldfogel (1999a)), it is worthwhile analyzing its impact on female employment.

Hence after, some states¹ have introduced different extensions to the FMLA which relax the employee's eligibility -such as the minimum number of hours worked and the required number of workers in the firm- covering both public and private sector employees. Although FMLA is the only law at the federal level which provides some assistance to new parents, there were laws prevalent at the state level long before the federal act came into effect. In particular, the Temporary Disability Insurance (TDI), which provides wage replacement for non-work connected sickness or injury, includes disabilities related with pregnancies. The TDI is implemented by five states², which started to apply this law from 1942 to 1969. The Paid Family Leave program, implemented by California in 2004, has been the most recent law enacted with regard to parental leave policy. In particular, it provides partially paid leave for a period of 6 weeks.

Hence, the above policies basically differ in either their medical requirements for eligibility or the paid/unpaid leave they provide. On the one hand, TDI provides paid leave, but only if the mother's maternity is associated with medical conditions which support a doctor's decision of considering it a temporary disability³. On the other hand, FMLA provides unpaid leave but it does not require the consideration of maternity as a temporary disability. Finally, California's Paid Family Leave program can be considered as the most generous of these programs since it provides a paid leave (as TDI) and, in addition, it does not require the temporary disability consideration (as FMLA).

This paper analyzes the impact of the existing parental leave policies on female employment. That is, do these different policies have the same impact on female employment? In particular, we examine whether the introduction of FMLA differently affected female employment in those states having some paid parental leave policy before FMLA was enacted -those which previously implemented the TDI- and those states which did not have any

¹Connecticut, Maine, Oregon and Vermont are the states whose FMLA expansion covers both the public as well as private sector employees. There are seven other states plus District of Columbia which also expanded FMLA but does not cover both public and private sector employees.

²California, Hawaii, New Jersey, New York and Rhode Island. Puerto Rico also has TDI law.

³After the Pregnancy Discrimination Act in 1978, every pregnancy is treated as a temporary disability.

kind of paid parental leave policy –those which neither passed TDI nor any other form of state maternity leave policy⁴. Additionally, we examine if change in the female employment was higher in those states which expanded the benefits (and relaxed the eligibility criteria) of FMLA than in those which did not. Finally, we analyze whether California’s Paid Family Leave program has a differential impact on female employment compared to (1) states that did not expand FMLA, and (2) states which expanded FMLA. The answer to these questions provides some interesting policy recommendations about parental leave programs.

Previous studies have analyzed the impact of FMLA on female employment, coverage, leave-taking and earnings. One of the most relevant works in this respect is Waldfogel (1999a). She used March CPS data for the years 1992-1995 to compare the change in female employment, coverage, earnings and leave-taking between states which had no job-protected maternity leave legislations before FMLA (39 states) to the 11 states and DC that had prior job-protected parental leave laws. Essentially, her analysis grouped together a set of 12 states, each of which had a different state maternity leave policy and hence are likely to have different impacts on female employment. Also, some of the states that she considered as having no job-protected maternity leave legislations before FMLA (among the 39 states), had some form of maternity leave policy prevalent before FMLA was passed in 1993. Hence there was variation within her no protection states in the actual amount of protection that women in these states were granted before FMLA was passed. Using a difference-in-difference-in-differences estimation strategy, her results indicate that there has been an increase in coverage and leave-taking following the enforcement of FMLA, whereas there has been no change in employment. Given that eligible women in some of the experimental states actually received treatment, this would have the effect of biasing the estimates towards zero.

The difference between our work and the former paper is twofold. First, we identify those five states that had some form of paid parental leave policy before FMLA was enacted, i.e. those states which previously implemented TDI. This specific classification allows us to

⁴There were eighteen states which had no maternity leave policy before the implementation of FMLA. They are, Alabama, Arkansas, Delaware, Idaho, Indiana, Kentucky, Michigan, Mississippi, Missouri, Nebraska, Nevada, New Mexico, North Carolina, South Carolina, South Dakota, Texas, Utah and Wyoming.

work with a homogeneous group of states which have been affected by the same law, and it captures the uniform differential impact of FMLA on female employment between those states applying TDI and those applying no state parental leave policies⁵. Second, we go one step beyond the analysis of FMLA and additionally compare whether the impact of FMLA on female employment differs between those states which expanded its benefits and relaxed its eligibility criteria and those which did not expand them, which Waldfogel (1999a) did not analyze. This enables us to study whether the impact on female employment and labor force participation is different when certain states make the parental leave policy more generous. In contrast to the previous results, we find a positive and statistically significant impact of FMLA on female employment in the states which had no law prior to FMLA compared to the states which had TDI⁶.

Another important contribution to the literature is Kallaman (1996). She studies the effect of the FMLA on female employment and wages. Her results show a positive (negative) and significant effect of FMLA on employment (wages). Kallaman's work does not make any distinction between the effects of FMLA and those of other policies. On the contrary, our paper focuses on comparing the relative performance of the above federal and state policies on female employment and labor force participation which greatly enriches the analysis. More recently, Hofferth and Curtin (2006) studied how implementation of the FMLA affected mothers' return to the work place and their changes in wages. However, as mentioned before, our paper is mainly focused on the effect of maternity leave policies on female employment across different states.

We use a difference-in-differences (DD) and difference-in-difference-in-difference (DDD) estimation strategy, where the first difference is between the states which received the treatment (states that had TDI) and the states which did not. The second source of difference has to do with the time of the treatment, i.e.: before and after 1993, when FMLA was

⁵Note that California, New Jersey and Rhode Island had some maternity leave legislations before FMLA was enacted, so the differential effect on female employment cannot be attributed to TDI alone for these three states.

⁶Using the same states as the experimental and non-experimental groups considered by Waldfogel, we replicate her results, i.e.:obtain no statistically significant impact of FMLA on female employment.

passed. The third difference is between the treated group and the control group, which did not receive the treatment. Some of the results obtained in this paper are the following. First, the impact of FMLA in those states which did not previously enact TDI is significantly more positive on female employment than the impact of this law in those states which already had TDI. The increase in female employment and labor force participation in those states that most generously expanded the benefits and eligibility criteria of FMLA is significantly higher than in those that did not. Finally, the introduction of California's Paid Family Leave program in 2004 has not yet provoked changes in female employment which can be considered significantly different from those in states applying either the FMLA or the expanded FMLA alone.

As mentioned above, the effects of FMLA on employment have been shown to be positive (but small) by both theoretical and empirical literature; see Klerman and Leibowitz (1994), Ruhm (1997) and Waldfogel (1996). This paper, then, goes in the line of this literature, but provides a much richer analysis and comparison about the effects of FMLA on female employment in states with and without previously enacted TDI, examines the labor market effects of the FMLA expansions implemented by different states, and finally provides a comparison about the impact of the most recent and generous parental leave policy in California with respect to those policies currently applied in the rest of the country.

The paper is organized as follows. In section 3.2 we develop an overview of the parental leave laws in United States. Section 3.3 explains the theoretical effects of mandated leave policies on female employment. Section 3.4 lays out the research design and methodology. Section 3.5 describes the data used in the analysis. In sections 3.6 through 3.9 we present our results to the main questions analyzed above, and provide some intuition. Section 3.10 concludes and suggests further research in the analysis of parental leave policies at the federal and state level.

3.2 OVERVIEW OF PARENTAL LEAVE LAWS

Parental leave policy has undergone many changes during the last decades. Figure 11 illustrates the evolution of maternity leave laws in US. The first state to introduce a paid maternity leave through TDI was Rhode Island in 1942, followed by California (1946), New Jersey (1948), New York (1949) and Hawaii (1969). TDI is offered to all workers in these states by state mandate. The program provides temporary income to workers with non-work related short-term disabilities, including pregnancy and childbirth. It is funded by either employee or employer or both, and ranges in coverage from 6 to 52 weeks⁷

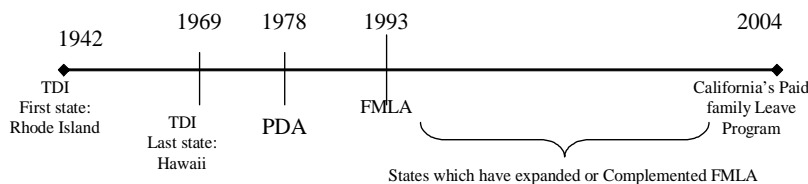


Figure 11: Parental leave laws: 1942-2004

In particular, TDI can be understood as a form of paid maternity leave, since disability insurance policies provide paid leave at state level and benefits for disability related to pregnancy and childbirth to ensure the same treatment as other medical disabilities. The TDI coverage for the first few weeks after a birth is routine, since all mothers are considered medically disabled for a certain period of time after child-birth. A study by the University of Rhode Island (2005) suggests that about 62 percent of TDI claimants were women, and the most explanatory variable to account for such high number of women filing claims was TDI coverage for childbirth (Employment Bulletin, RI Department of Labor and Training).

The Family and Medical Leave Act (enacted in 1993) is the only law in terms of parental leave policy at the federal level. The legislation covers private, state and local government employees. However, the coverage under the Family and Medical Leave Act is not universal.

⁷See table 41 in the appendix for a detailed explanation about weekly benefit amounts and duration of the TDI program.

FMLA guarantees parents unpaid, job-protected leave of up to 12 weeks following the birth or adoption of a child, but only if they meet certain qualifying conditions. First, they must work for a firm with 50 or more employees, which means only about 60 percent of workers in the private-sector are covered (Han and Waldfogel, 2003). Second, they must have worked at least 1,250 hours in the past year for their employers, which reduces the share of workers covered to 46 percent (Cantor et al, 2001).

Over the years a considerable number of states have relaxed as well as complemented the requirements of FMLA. This paper considers four states for whose FMLA expansion not only affected *public* but also *private* sector workers⁸ and have made the FMLA more generous. In particular, the state of Connecticut expanded FMLA in 1997 applying it to employees who worked 1000 hours for at least 12 months prior to a request for a leave, and eligible employees are entitled 16 workweeks of leave (instead of only 12). In 1997, Maine extended the applicability of its family medical leave laws by lowering the requirements for employee eligibility to 15 employees. Oregon (1996) made FMLA more generous by applying it to employers with 25 or more employees, and established that those workers who are employed for 180 days or more are eligible for 12 weeks of leave within a 12 month period. Finally, similar to Oregon, Vermont (1998) reduced the number of minimum employees to 10, who should have worked for one year on an average of 30 hours a week to be eligible for 12 weeks of leave during any 12 month period. In addition, all of these four states allowed the possibility of substituting accrued paid leave for maternity leave purposes (see table 42 of appendix for details about the extensions of FMLA).

Finally, one of the most recent parental leave policy to be enacted is California's Paid Family Leave (PFL) program, which came into effect in July 2004. California is the first state in the country to create a comprehensive paid family leave program. It provides workers with paid leave during a period of six weeks, covering approximately 55 percent of their pre-taxed weekly wage, up to a maximum of \$840, while on leave. This insurance program is fully funded by the employees. The new mothers in California are now eligible for Paid Family

⁸There are eight other states which expanded the FMLA benefits, but they did not cover both private and private sector employees. In addition, the relaxations in eligibility and expansions in benefits were minimal.

Leave benefits in addition to the TDI benefits. While FMLA offers 12 weeks of unpaid leave for those working for employers with 50 or more employees, Paid Family Leave program guarantees that 6 of those weeks would be paid.

3.3 THEORY

The theory on mandated benefits suggest that they preserve the employers' ability to tailor arrangements to their workers and to offer more than minimum packages. In terms of their allocational effects on employment, mandated benefits represent a tax at a rate equal to the difference between the employers cost of providing the benefit and the employee's valuation of it (Summers, 1989). In other words, it is assumed that workers and firms will voluntarily agree to the provision of family leave if the expected benefits exceed the associated costs. Parental leave benefits could also increase occupational segregation, by increasing the relative cost of employing women in certain types of jobs, and lead to higher unemployment for the groups who are most likely to use it. (Ruhm and Teague, 1997). However, there are a considerable number of economic arguments that have been made favoring leave entitlements usually focus on retaining firm-specific human capital or decreasing unemployment by reducing the need for women to change jobs, if they wish to spend time at home with young children. (Trzcinski, 1991).

According to the theory (Ruhm, 1997), a mandated leave raises labor costs and so shifts the labor demand curve to the left (from D_1 to D_2 in Figure 12). Since employees receive benefit from the leave entitlement, the supply curve simultaneously shifts to the right (from S_1 to S_2). Hence the equilibrium wages fall while employment may either increase or decrease, depending on the relative size of the shifts. Figure 12 illustrates the case where the expenses to employers exceed the benefits to the employees, and employment decreases (from E_1 to E_2). However, there could be additional dynamic effects. For instance, labor productivity would rise if parental leave increases firm-specific human capital by allowing individuals to return to their old jobs. This causes the demand curve to shift to the right (from D_2 to D_3 in Figure 2), increasing employment and decreasing the wage reduction or actually leading

to a rise in earnings. The proponents of leave mandates frequently use this reasoning to anticipate increases in both wages and employment for the groups who are most likely to use parental leave.

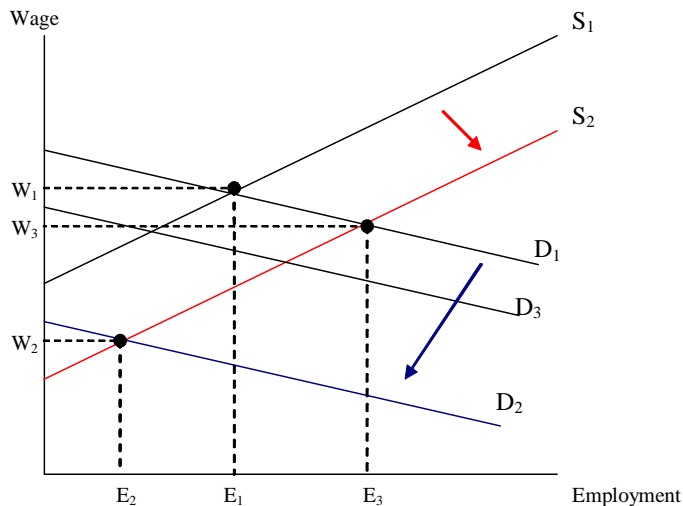


Figure 12: Theoretical predictions of mandated leave policies

The theory does not provide an unambiguous prediction regarding the effects of parental leave programs on employment. On one hand, it predicts that the cost associated with the leave might be borne by women as a group, in the form of lower employment (as employers might shift away from hiring women), or in the form of lower wages (when employers pass on the cost of benefits to women). On the other hand, it also predicts that these effects might be offset by an increase in the labor force attachment of individual women.

3.4 RESEARCH DESIGN

3.4.1 Identification Strategy

The research design is based on the study developed by Waldfogel (1999a). In the first part of our analysis, the eighteen states which did not have parental leave policy prior to FMLA –those states neither applying TDI nor any other form of state parental leave policy–

constitute the *experimental* states. The five states which did have TDI prior to FMLA are the *non-experimental* states. For the second part of the study, the four states which expanded or complemented FMLA are considered as the experimental states for each particular case, with the non-experimental states being the states applying only FMLA. Finally, the state of California (experimental state) is compared with those states which only have FMLA or have expanded it (non-experimental states). We could use DD analysis to compare the change in outcomes for the experimental states to the change in outcome for the non-experimental states. However, this approach may produce biased estimate if there were other differences between these two groups of states that affected both the passage of state laws and the change in their outcome variables over time (Bertrand and Mullainathan, 2004). To overcome this problem, we use a DDD methodology.

We study the impact of state parental leave policies on the employment of women. Since we use a DDD estimation strategy, this requires identifying one or more treatment groups (the groups who are affected by the passage of a law) and one or more control groups (those who are not affected by the law but are otherwise affected by the same underlying factors, so that their behavior can be used to approximate how the treatment group would have acted in the absence of the policy). The different parental leave policies that we are analyzing are expected to primarily benefit women of childbearing ages, therefore, we use women aged 19 to 45 with children older than one year old, or women in the same age group who are childless as our treatment group. In addition, we consider an alternative treatment group in our analysis, which is composed of women aged 19 to 45 with children under the age of one. These groups of women are included in the treatment group because they are eligible and more likely to obtain the benefits of parental leave policies. Also, we focus on these women because they are the potential beneficiaries of the law and might bear some of the costs of the law (in terms of employers passing along the costs of these benefits to those potentially benefitting from it). In order to isolate the impact of the parental leave policies from other factors (like economic shocks or other policy changes) that affect labor market outcomes, we use control groups. Ideally, a control group should be similar in characteristics to the treatment group, and hence likely to respond in the same way to a given policy change, except

for the fact that it does not receive the treatment. For this purpose, we use women aged 46 to 60 as the control group, since they may plausibly substitute for women under 45 in employment but are more likely to have reached the end of their reproductive lives and hence less likely to take advantage of parental leave laws. However, comparison between younger females and older females might not be sufficient to identify the effect of parental leave policies. These older women have their own labor market trends which differ from that of the younger women, and there could be some age-specific factors that might make their preferences towards work different from that of younger females. For this purpose (and as is customary in the literature)⁹, we use men between the age of 19 and 45 as an alternative control group. Men of the same age as women in the treatment group, while also potential substitutes in employment, are less likely to have been affected by changes in tastes for work as older women. However, men have their own work preferences and might face different labor market prospects compared to women (mainly due to gender discrimination in the workplace). Since each of the control groups we use are plausible but imperfect comparisons for the treatment group, we use both types of workers alternatively as control groups in order to better identify the effects of parental leave policies on female employment¹⁰.

Our estimates of the effects of the *federal* parental leave policy on female employment is the difference between the change in female employment after the introduction of the FMLA in those states which previously applied TDI and those which did not. Another estimate of this effect is obtained from the difference in the change of the female employment between those states which expanded FMLA and those which did not. Finally, the estimate of the effect of the *state* parental leave policies on female employment measures the difference between the change in the female employment in California –after the enactment of the Paid Family Leave program– and that in the states which expanded FMLA and those which did not.

⁹See Baker and Milligan (2008), Rodgers and Zveglic (2003)

¹⁰Also, since our analysis closely follows that of Waldfogel (1999a) who used men as alternate control group, in order to replicate as well as compare our results to this study, we include this group.

Table 11: Summary Statistics

| Variable | Treatment | Alternative Treatment | Control | Alternative Control |
|--------------------------------|--------------|-----------------------|--------------|---------------------|
| Age | 32.28 (0.00) | 28.16 (0.01) | 51.96 (0.01) | 31.80 (0.01) |
| College Graduate | 0.44 (0.00) | 0.42 (0.00) | 0.35 (0.001) | 0.46 (0.00) |
| High School Drop Out | 0.17 (0.00) | 0.18 (0.00) | 0.26 (0.00) | 0.17 (0.00) |
| Employed | 0.74 (0.00) | 0.58 (0.00) | 0.66 (0.00) | 0.91 (0.00) |
| Fraction in States with no Law | 0.56 (0.00) | 0.57 (0.00) | 0.55 (0.00) | 0.56 (0.00) |
| Observations | 1,210,503 | 164,265 | 618,343 | 1,269,131 |

Notes: Treatment refers to the women aged 19 to 45 with children older than one year old or childless women; Alternative Treatment refers to women aged 19 to 45 with children under the age of one. Control refers to women aged 46 to 60 and Alternative Control refers to men aged 19 to 45. Data are from IPUMS-CPS for the years 1979-2007.

3.5 DATA

We use IPUMS-CPS data from 1979-2007, which is an integrated set of data from the March Current Population Survey (CPS). In the second part of our study, which analyzes the effects of the FMLA expansions, we also use data from CPS where time period depends on the particular year in which the state expanded FMLA. Finally, the analysis of California uses data from the 2001-2007 CPS.

Table 11 presents the summary statistics for the period 1979-2007 (mean and standard error) of the characteristics of the treatment and control groups. Column 1 shows the characteristics of women in their childbearing age with their youngest child above the age of one or women without children. Column 2 contains women between 19-45 whose youngest child is less than one year. Column 3 shows women aged between 46-60 and column 4 shows the summary statistics of men aged between 19-45.

There are some differences to be noted from the summary statistics. Women with children

above the age of one or without children (column 1) have, on average, the same age as men (32 years), whereas women in alternative treatment group are on average younger than men (the alternative control group). The percentage of employed in the treatment group is lower (74 versus 91 percent) than the alternative control group, and the percentage employed for the alternative treatment group is even lesser. Both treatment and alternative treatment group women on average are more educated than women aged between 46-60 (control group). Notice that, the percentage of women and men in the eighteen states which had no law prior to FMLA is fairly constant, so we can be certain that any of the results are not driven by compositional changes. The summary statistics indicate that any differences in the labor force participation among the groups are not driven by differences in the demographic characteristics of those groups. The only difference between the treatment (and alternative treatment) and control (and alternative control) groups are that the former have been affected by changes in parental leave laws whereas the latter have not been affected by such changes.

Table 12 presents the summary statistics for the case of California (2001-2007). Column 1 shows the treatment group, column 2 the alternative treatment group, while columns 3 and 4 show the control and the alternative control group, respectively.

Some differences can also be pointed out from the above table. Women with children above the age of one or without children (column 1) have, on average, the same age as men (33 years), whereas women in alternative treatment group are younger (30 years); treatment group works less than the alternative control group (73 versus 89 percent), whereas alternative treatment group are less likely to be employed compared to both control groups. Both the treatment and alternative treatment group women are similarly educated compared to women and men in the control groups.

Table 12: Summary Statistics: California

| Variable | Treatment | Alternative treatment | Control | Alt. Control |
|--------------------------------|--------------|-----------------------|--------------|--------------|
| Age | 32.95 (0.05) | 30.45 (0.11) | 51.49 (0.04) | 32.51 (0.05) |
| College Graduate | 0.56 (0.00) | 0.53 (0.01) | 0.57 (0.00) | 0.51 (0.00) |
| High School Drop Out | 0.18 (0.00) | 0.22 (0.01) | 0.19 (0.00) | 0.20 (0.00) |
| Employed | 0.73 (0.00) | 0.55 (0.01) | 0.71 (0.00) | 0.89 (0.00) |
| Fraction in States with no Law | 0.61 (0.00) | 0.65 (0.00) | 0.61 (0.00) | 0.61 (0.00) |
| Observations | 21,733 | 2,880 | 112,45 | 23,236 |

Notes: Treatment refers to the women aged 19 to 45 with children older than one year old or childless women; Alternative Treatment refers to women aged 19 to 45 with children under the age of one. Control refers to women aged 46 to 60 and Alternative Control refers to men aged 19 to 45. Data are from IPUMS-CPS for the years 2001-2007.

3.6 EMPIRICAL RESULTS FOR FEMALE EMPLOYMENT

The likely direction of the effects of maternity leave policies on female employment is not clear a priori. On the one hand, if women are more inclined to take leave and return to their previous jobs, then we expect to find a positive effect on employment. On the other hand, if employers pass on the costs of mandated benefits to those potentially benefitting from it (women of childbearing age), then we might observe a decrease in employment. Further, if these two effects offset each other, we might find no impact on employment at all. Therefore, the direction of the effect of maternity leave policies on female employment is an empirical question. This section, given the above research design, estimates the following equation

$$\begin{aligned}
 Y_{ilt} = & \alpha_0 + \alpha_1 t + \alpha_2 NS_l + \alpha_3 TG_i + \alpha_4 NS_l \times TG_i + \beta_0 POST_t + \beta_1 NS_l \times POST_t \\
 & + \beta_2 TG_i \times POST_t + \beta_3 NS_l \times TG_i \times POST_t
 \end{aligned} \tag{7}$$

where i indexes the individual, l the state and t the year. The outcome variable of interest, Y_{ilt} , is a dummy equal to one if a woman worked for at least one week during the previous year, and zero otherwise. NS_l is a dummy equal to one if the l^{th} state has no state parental

leave policy before 1993, and zero if the state had TDI. TG_i is a dummy taking a value one if individual i belongs to the treatment group and zero otherwise. Finally, $POST_t$ is a year dummy taking value one after FMLA was enacted (i.e. after 1993) and zero otherwise. Since FMLA was enacted in 1993, we conduct our analysis pre and post 1993, in order to capture the impact of the law on female employment. We define employment as working for a positive number of weeks during the year. In alternate specifications, we use labor force participation rate as the outcome variable, where it is defined as participation in the labor force during the preceding week.

3.7 ANALYSIS OF FMLA IN STATES WITH AND WITHOUT TDI

Table 13 reports the differential impact of FMLA on female employment between states that had no law prior to FMLA and states which had TDI before FMLA. Column (3) shows the difference between the change in female employment for experimental states –those with no previous laws– and non-experimental states –those with TDI, before and after 1993. Column (4) represents the change of female employment between the treatment and control group, i.e. DDD. Finally, DDD adjusted 1 controls for marital status –i.e. dummy variable married takes a value of one if the individual is married and zero otherwise–, DDD adjusted 2 includes a time trend since the data contains more pre than post years, and DDD adjusted 3 controls for both marital status and time trend. Notice that the Treatment group represents women aged 19-45 with their youngest child above age one or childless women, while control group are women aged 46-60 years and alternative control group are men aged 19-45 years.

In order to analyze the possibility of a different impact of FMLA depending on the woman’s educational attainment, we differentiate our sample by education levels. In particular, the dummy variable $Educ_i$ is one when individual i has less than 12 years of schooling, and zero otherwise. We divide our sample into two groups, one represents high school dropouts and the second group contains the individuals who have a high school degree, a

Table 13: The estimates of Female Employment between states with no law and states with TDI before FMLA

| | NoLaw(1) | TDI(2) | DD(3) | DDD(4) | DDD. adj1 | DDD. adj2 | DDD. adj3 | |
|-----------------------|----------|--------|-------|--------|---------------------|---------------------|---------------------|---------------------|
| Treat | Pre'93 | 0.790 | 0.740 | | | | | |
| | Pos'93 | 0.720 | 0.660 | 0.016 | | | | |
| Control | Pre'93 | 0.630 | 0.630 | | | | | |
| Obs.675,816 | Pos'93 | 0.740 | 0.720 | 0.020 | -0.004 (0.004) | -0.001 (0.004) | -0.004 (0.004) | -0.004 (0.004) |
| standard errors | | | | | | | | |
| Alt. Control | Pre'93 | 0.920 | 0.900 | | | | | |
| Obs.963,726 | Pos'93 | 0.920 | 0.890 | 0.012 | 0.006*** (0.003) | 0.007*** (0.003) | 0.006*** (0.003) | 0.007*** (0.003) |
| standard errors | | | | | | | | |
| marital status? | | | | | no | yes | no | yes |
| linear time trend? | | | | | no | no | yes | yes |

Notes: The Pre '93 period consists of the years 1978 - 1992. Robust Standard errors are reported. *** denotes statistically significant at one-percent level.

college degree or are college dropouts. In this case, equation 7 can be rewritten as,

$$\begin{aligned}
 Y_{ilt} = & \alpha_0 + \alpha_1 t_l + \alpha_2 NS_l + \alpha_3 TG_i + \alpha_4 NS_l \times TG_i + \beta_0 POST_t + \beta_1 NS_l \times POST_t + \beta_2 TG_i \times POST_t \\
 & + \beta_3 NS_l \times TG_i \times POST_t + \alpha_5 Educ + \alpha_6 NS_l \times Educ_i + \alpha_7 TG_i \times Educ_i + \alpha_8 NS_l \times TG_i \times Educ_i \\
 & + \beta_4 POST_t \times Educ_i + \beta_5 NS_l \times POST_t \times Educ_i + \beta_6 TG_i \times POST_t \times Educ_i \\
 & + \beta_7 NS_l \times TG_i \times POST_t \times Educ_i
 \end{aligned} \tag{8}$$

The results obtained from both equations (with and without the dummy variable Educ) are shown in tables 13 and 14. In addition, the same analysis is conducted for labor force participation, where Y_{ilt} is a dummy equal to one if a woman participated in the labor force during the preceding week and zero otherwise (tables 15 and 16 contain the results obtained for labor force participation)

According to the PDA in 1978, the TDI should treat every pregnancy as a temporary disability. Hence the coverage of the TDI extended to cover pregnancy as mandatory after the passage of the PDA. In order to analyze the possibility of a differential impact of FMLA on female employment and labor force participation rates, we restrict the sample for the pre-FMLA period from 1978 through 1992.

Table 14: The estimates of Female Employment between states with no law and states with TDI before FMLA, by education groups

| | | NoLaw(1) | TDI(2) | DD(3) | DDD(4) | DDD. adj1 | DDD. adj2 | DDD. adj3 |
|-----------------------|-----------------------|----------|--------|-------|---------------------|---------------------|---------------------|---------------------|
| Treat | Pre'93 | 0.910 | 0.860 | | | | | |
| | Pos'93 | 0.880 | 0.810 | 0.017 | | | | |
| Control | Pre'93 | 0.770 | 0.780 | | | | | |
| | Obs.675,816 Pos'93 | 0.840 | 0.810 | 0.031 | -0.013 (3.76) | -0.015 (4.92) | -0.013 (3.96) | -0.015 (5.12) |
| F-statistic | | | | | | | | |
| Alt. Control | Pre'93 | 0.810 | 0.800 | | | | | |
| | Obs.963,726 Pos'93 | 0.790 | 0.780 | 0.001 | 0.016*** (12.11) | 0.017*** (12.66) | 0.016*** (12.08) | 0.007*** (12.65) |
| F-statistic | | | | | | | | |
| marital status? | | | | | no | yes | no | yes |
| linear time trend? | | | | | no | no | yes | yes |

Notes: The Pre '93 period consists of the years 1978- 1992. Robust Standard errors are reported. *** denotes statistically significant at one-percent level.

3.7.1 Analysis of the results

Table 13 suggests that when the treatment group is compared to the control group as observed in column 4, the introduction of FMLA has an effect on female employment which is indistinguishable from zero. However, when the treatment group is compared to the alternative control group, i.e. men, the DDD estimates are positive and significant. This suggests female employment increased in states which had no law compared to the TDI states after the implementation of FMLA, when women in their childbearing ages are compared to men. When we further analyze by the educational attainment, table 14 shows significant DDD estimates for high-school dropout women, when the employment of female are compared to men.

We further check whether the high educated women (having more than a high school degree) are differentially affected by the implementation of FMLA. The results indicate (tables not reported) that there is a no impact of FMLA on the employment of higher educated women in the states which had no law compared to the TDI states, when women in childbearing ages are compared to men. Also, the estimates are statistically indistinguishable from zero when these women are compared to the control group, i.e. older women.

One reason for the insignificant results for the comparison between treatment group and control group that is older women could be explained by the fact that the paper considers a long period of time and hence on average the effects are obscured although there might be a different impact for some of the years compared to others. However, repeating the analysis by separately taking each of the years after 1993 did not change the results.

A possible explanation for the results can be obtained from the literature analyzing the theoretical predictions of mandated leave policies (Ruhm, 1997). In particular, this literature suggests that, after the introduction of maternity leave policies, labor demand shifts downwards, decreasing employment level. However, as women's awareness of the policy increases, we should expect a downward shift in the labor supply curve, raising employment level as a consequence. Nonetheless, in the long run, women are more likely to return to their original jobs (reducing the employer adjustment costs) and work more hours to be eligible for the benefits. The former effects may increase the employer's labor demand—due to the higher productivity levels of female employees—shifting it upwards, what is referred by the literature as the dynamic effect of mandated leave policies. In this context, our results reflect that the dynamic effect of FMLA is slightly positive and significant for women in their childbearing ages (when compared to men).

Regarding our results with respect to labor force participation, table 15 shows that the DDD estimates are positive and significant when we compare treatment group to the alternative control group. Hence, the change in female labor force participation after the FMLA was enacted (between experimental and non-experimental states) was significantly higher for those women potentially benefiting from the law (in the treatment group) than for men (in the alternative group). Therefore, the change in female labor force participation after the implementation of the FMLA was higher for women in states which had no maternity leave laws prior to FMLA compared to women in states which have TDI

Table 16 analyzes the possibility of a different impact of FMLA, depending on the woman's educational attainment. Indeed, we also observe a positive and significant DDD estimates for high-school dropout women, when the employment of female are compared to

Table 15: The estimates of FLFP rates between states with no law and states with TDI before FMLA

| | | NoLaw(1) | TDI(2) | DD(3) | DDD(4) | DDD.adj1 | DDD.adj2 | DDD.adj3 |
|---|--------|----------|--------|-------|---------------------|---------------------|---------------------|---------------------|
| Treat | Pre'93 | 0.745 | 0.706 | | | | | |
| | Pos'93 | 0.686 | 0.623 | 0.024 | | | | |
| Control Obs.675,816 standard errors | Pre'93 | 0.581 | 0.595 | | | | | |
| | Pos'93 | 0.712 | 0.699 | 0.026 | -0.002 (0.005) | 0.003 (0.005) | -0.003 (0.005) | -0.004 (0.005) |
| Alt. Control Obs.963,726 standard errors | Pre'93 | 0.919 | 0.907 | | | | | |
| | Pos'93 | 0.9081 | 0.886 | 0.010 | 0.014*** (0.003) | 0.014*** (0.003) | 0.014*** (0.003) | 0.014*** (0.003) |
| marital status? | | | | | no | yes | no | yes |
| linear time trend? | | | | | no | no | yes | yes |

Notes: The Pre '93 period consists of the years 1978 through 1992. Robust Standard errors are reported. DDDadj 1 reports estimates controlling for marital status, DDDadj2 reports estimates controlling for a linear time trend and DDDadj3 reports estimates controlling for both. *** denotes statistically significant at one-percent level.

men. There is a positive effect on the female labor force participation when we analyze by the high educated group of women. Hence, the aforementioned shift in female labor supply can be mainly explained by a change in willingness to work for low-educated women, and also changed incentives for higher educated women.

Table 36 in the appendix analyzes the results from a falsification exercise pretending that FMLA was adopted in 1981 instead of the actual year 1993 of its implementation. Hence pretending that treatment occurred several years before it actually did, I find that none of the estimates are statistically significant. As reported in table 36, the DDD estimates are positive but insignificant when we compare the treatment group both to the control group (older women) and to the alternative control group (men). Therefore, the change in female labor force participation after the implementation of the FMLA (pretending it was adopted in 1981) was not statistically different for women in states which had no maternity leave laws prior to FMLA compared to women in states which have TDI.

Table 16: The estimates of FLFP rates between states with no law and states with TDI before FMLA, by education groups

| | | NoLaw(1) | TDI(2) | DD(3) | DDD(4) | DDD adj1 | DDD adj2 | DDD adj3 |
|-----------------------|-----------------------|----------|--------|-------|---------------------|---------------------|---------------------|---------------------|
| Treat | Pre'93 | 0.860 | 0.808 | | | | | |
| | Pos'93 | 0.836 | 0.763 | 0.021 | | | | |
| Control | Pre'93 | 0.732 | 0.753 | | | | | |
| | Obs.675,816 Pos'93 | 0.821 | 0.807 | 0.035 | -0.014 (4.22) | -0.017 (5.64) | -0.015 (4.63) | -0.042 (4.22) |
| F-statistic | | | | | | | | |
| Alt. Control | Pre'93 | 0.774 | 0.769 | | | | | |
| | Obs.963,726 Pos'93 | 0.746 | 0.740 | 0.001 | 0.020*** (17.92) | 0.020*** (19.09) | 0.019*** (18.09) | 0.021*** (17.92) |
| F-statistic | | | | | | | | |
| marital status? | | | | | no | yes | no | yes |
| linear time trend? | | | | | no | no | yes | yes |

Notes: The Pre '93 period consists of the years 1978 through 1992. Robust Standard errors are reported. DDDadj 1 reports estimates controlling for marital status, DDDadj2 reports estimates controlling for a linear time trend and DDDadj3 reports estimates controlling for both. *** denotes statistically significant at one-percent level.

3.8 ANALYSIS OF EXTENSIONS OF FMLA

Using equation (7) we study the effects of the expansions of FMLA that the aforementioned four states introduces in their jurisdiction. The results obtained for Connecticut and Vermont are presented in tables 37 and 38 in the appendix respectively. Specifically, in this section we report the estimates of the alternative treatment group, which is composed of women aged 19 to 45 with children under the age of one, comparing with both the control group and the alternative control group. The results using the treatment group are very similar that of the alternative treatment group, with the only difference being that none of the results are statistically indistinguishable from zero when we use the treatment group.

The DDD estimates are positive and statistically significant for Oregon, (table 17). In particular, the labor force participation rates are higher by 18.84 percentage points in Oregon compared to states which did not expand FMLA, when we compare women with young children to older women in the control group. The labor force participation rates for women with young children in Oregon are higher by 11.04 percentage points when compared with men in the same age group. This result indicates that making the FMLA more generous in

Table 17: The estimates of FLFP rates between Oregon and states with no law before FMLA

| | | Oregon | Only FMLA | DD | DDD | DDD adj. 1 |
|--|--------|--------|--------------|--------|---------------------|---------------------|
| Alt. Treat | Pre 96 | 0.417 | 0.592 | | | |
| | Pos 96 | 0.527 | 0.570 | 0.131 | | |
| Control Obs. 28,463 standard errors | Pre 96 | 0.747 | 0.668 | | | |
| | Pos 96 | 0.720 | 0.699 | -0.056 | 0.189*** (0.075) | 0.188*** (0.075) |
| Alt. Control Obs. 58,817 standard errors | Pre 96 | 0.909 | 0.913 | | | |
| | Pos 96 | 0.929 | 0.913 | 0.021 | 0.111* (0.069) | 0.105* (0.069) |
| marital status? | | | | | no | yes |
| linear time trend? | | | | | no | no |

Notes: Robust Standard errors are reported. *** denote statistically significant at the one-percent level. * denotes statistically significant at ten-percent level. DDDadj 1 reports estimates controlling for marital status. Pre 96 years are '93-'95. Pos 96 years are '96-'98.

terms of eligibility and leave options are encouraging more women in the childbearing age with young infants to participate in the labor force, compared to similar women in states which offer no such generosity. The results are very similar if we control for the marital status of the individual and include a linear time trend. However, when we further divide the sample into two groups based on their educational attainment, the difference in the employment growth of women with infants between Oregon and states that had no law before FMLA is not significant

The women in the childbearing age with young infants are also 17.15 percentage points more likely to be employed in Oregon compared to similar women in states which do not complement the FMLA (table 18) and 10.01 percentage points more likely to be employed when compared to men. Similar to the result for labor force participation, exploring the possibility of a differential impact depending on the educational attainment, we do not find anything statistically indistinguishable from zero. Note that Oregon is the state offering the most generous benefits and eligibility criteria in its expansion of FMLA. In particular, this state reduced the minimum number of firm employees from 50 to 25, and the number of days worked for the same firm to 180. Intuitively, the positive effect of such generous expansion may come from two sources. First, women may increase their willingness to work,

Table 18: The estimates of Female Employment between Oregon and states with no law before FMLA

| | | Oregon | Only FMLA | DD | DDD | DDD adj. 1 |
|--|--------|--------|-----------|--------|---------------------|---------------------|
| Alt. Treat | Pre 96 | 0.554 | 0.661 | | | |
| | Pos 96 | 0.686 | 0.661 | 0.131 | | |
| Control Obs. 28,463 standard errors | Pre 96 | 0.785 | 0.706 | | | |
| | Pos 96 | 0.768 | 0.729 | -0.040 | 0.172*** (0.069) | 0.171*** (0.069) |
| Alt. Control Obs. 58,817 standard errors | Pre 96 | 0.916 | 0.931 | | | |
| | Pos 96 | 0.949 | 0.931 | 0.031 | 0.100* (0.063) | 0.096* (0.063) |
| marital status? | | | | | no | yes |
| linear time trend? | | | | | no | no |

Notes: Robust Standard errors are reported. *** denotes statistically significant at the one-percent level. * denotes statistically significant at the ten-percent level. DDDadj 1 reports estimates controlling for marital status. Pre_96 years are '93-'95. Post_96 years are '96-'98.

which shifts the female labor supply downwards. Second, a generous FMLA expansion raises the opportunity cost from losing the job, which increases worker's effort and productivity, ultimately leading to a greater employer's demand for female workers.

The DDD estimates are positive and statistically significant at the 5-percent level for female labor force participation rates, for Maine (table 19). The female labor force participation rates are higher by 14.57 percentage points in Maine compared to states which did not expand FMLA, when we compare women with young children to older women in the control group. The labor force participation rates for women with young children in Maine are higher by 13.38 percentage points when compared with men in the same age group. The results are very similar if we control for the marital status of the individual and include a linear time trend. However, the estimates are not statistically significant for female employment, and when we divide the sample into groups based on their educational attainment¹¹

One of the reasons of not observing any significant impact on female employment and female labor force participation in the other two studied states might be that the expansions are not effective enough to invoke any change in the employment behavior of women in these states. With a more relaxed eligibility criteria and greater complementarity to the FMLA,

¹¹The results for female employment and two different educational groups are not reported.

Table 19: The estimates of Female Labor Force Participation rates between Maine and states with no law before FMLA

| | | Maine | Only FMLA | DD | DDD | DDD adj.1 |
|-----------------------------|--------|-------|-----------|--------|---------|--------------|
| Alt. Treat | Pre 97 | 0.619 | 0.595 | | | |
| | Pos 97 | 0.748 | 0.588 | 0.136 | | |
| Control Obs. 27,886 | Pre 97 | 0.716 | 0.685 | | | |
| | Pos 97 | 0.724 | 0.703 | -0.009 | 0.146** | 0.142** |
| Standard errors | | | | | (0.079) | (0.079) |
| Alt. Control Obs. 56,213 | Pre 97 | 0.897 | 0.912 | | | |
| | Pos 97 | 0.901 | 0.913 | 0.003 | 0.134** | 0.137** |
| Standard errors | | | | | (0.074) | (0.074) |
| marital status? | | | | | no | yes |
| linear time trend? | | | | | no | no |

Notes: Robust Standard errors are reported. ** denotes statistically significant at the five-percent level. DDDadj 1 reports estimates controlling for marital status. Pre 97 years are '94-'96. Pos 97 years are '97-'99.

the states might be able to initiate an increase in female labor force participation rate as well as in female employment. Also, the small size of the state of Vermont which leads to very small sample sizes of all groups in this state, it is not surprising that none of the results are significant.

3.9 ANALYSIS OF CALIFORNIA'S PAID FAMILY LEAVE PROGRAM

In this subsection, we use equation (1) to examine how California's Paid Family Leave program has affected the change in female employment and labor force participation rates. We compare California's Paid Family Leave program with those states applying the expanded and non-expanded FMLA. Table 20 analyzes how the female labor force participation changed in California before and after the enactment of the Paid Family Leave program in 2004. In particular, Column 3 (DD estimates) in table 20 compares the change in the female labor force participation with respect to that in states which only had FMLA (non-expanded), and the four states which have expanded FMLA programs. Finally, Column 4 (DDD estimates) compares the change in female labor force participation between the

Table 20: The estimates of FLFP rates between California and states with no law before FMLA

| | | California | Only FMLA | DD | DDD | DDD adj.1 |
|--|--------|------------|-----------|--------|-------------------|-------------------|
| Treat | Pre 04 | 0.761 | 0.778 | | | |
| | Pos 04 | 0.738 | 0.756 | -0.002 | | |
| Control Obs. 143,023 standard errors | Pre 04 | 0.718 | 0.718 | | | |
| | Pos 04 | 0.721 | 0.719 | 0.002 | -0.007 (0.013) | -0.006 (0.013) |
| Alt. Control Obs.193,978 standard errors | Pre 04 | 0.899 | 0.908 | | | |
| | Pos 04 | 0.889 | 0.899 | -0.001 | -0.005 (0.008) | -0.005 (0.083) |
| marital status? | | | | | no | yes |
| linear time trend? | | | | | no | no |

Notes: Robust Standard errors are reported. DDDadj 1 reports estimates controlling for marital status.
Pre 04 years are '01-'03. Pos 04 years are '04-'07.

treatment and control groups.

Table 21 analyzes change in female labor force participation between California and the states which expanded FMLA, before and after the enactment of the Paid Family Leave program in 2004. The results for the change in female employment are given in tables 39 and 40 in the appendix.

From these tables we can see that the DDD estimates are both economically and statistically insignificant. Hence the introduction of the Paid Family Leave program did not initiate an increase in female labor force participation, when we compare them with the labor force participation of women in states which had no law before FMLA as well as states which expanded FMLA. This is true for female employment as well. The results are very similar if we consider women with young infants (alternative treatment group) as our treatment group. Also, when we further carry out the analysis dividing the women in the sample into two different educational groups, the results are still statistically insignificant¹². In the context of changes in supply and demand curves as commented above, these results would suggest that the decrease in labor demand offsets the increase in female labor supply in the three-

¹²The results for the different educational groups are not reported.

Table 21: The estimates of FLFP rates between California and states with which expanded FMLA

| | | California | Exp. FMLA | DD | DDD | DDD adj.1 |
|--|--------|------------|-----------|--------|-------------------|-------------------|
| Treat | Pre 04 | 0.816 | 0.795 | | | |
| | Pos 04 | 0.790 | 0.769 | -0.001 | | |
| Control Obs. 54,838 standard errors | Pre 04 | 0.682 | 0.773 | | | |
| | Pos 04 | 0.685 | 0.776 | -0.003 | -0.003 (0.016) | -0.003 (0.016) |
| Alt. Control Obs. 74,235 standard errors | Pre 04 | 0.899 | 0.899 | | | |
| | Pos 04 | 0.889 | 0.885 | 0.004 | -0.008 (0.115) | -0.008 (0.115) |
| marital status? | | | | | no | yes |
| linear time trend? | | | | | no | no |

Notes: Robust Standard errors are reported. DDDadj 1 reports estimates controlling for marital status.
Pre 04 years are '01-'03. Pos 04 years are '04-'07.

year period following the implementation of the law, and future years of data are needed to observe whether the dynamic effect on labor demand finally raises female employment. Whenever a new policy is implemented at the state level, people are not immediately aware of that and hence a certain period of time is required before we can observe the full effects of such a policy. It should also be noted that California is a state where women having children can utilize the benefits of TDI and FMLA. Hence it might not be possible for Paid Family Leave program to initiate any further changes to the employment behavior of women, especially when such a parental leave policy is fully funded by the employees themselves. A relevant policy recommendation would be to implement such a policy where there could be equal contributions on part of the employer in addition to that of an employee.

Another possible explanation for the insignificant results of the Paid Family Leave program in California could be the low take-up rates. According to the statistics reported by the Employment Development Department of California, the percentage claim of PFL benefits was only 1.01% in 2005 and 1.07% in 2006. A study by Rothschild (2007) suggests the possible reasons for the low take-up rates¹³. The most important of them is that a

¹³Jesse Rothschild and Rothschild-Landry Holding Inc.(November 2007)

significant portion of California's workforce is undocumented, due to the high percentage of immigrant population in California. Also, workers use available sick leave, vacation leave instead of parental leave and hence no claims are filed. The surveys conducted support the lack of worker awareness of the available benefits provided by the Paid Family Leave program, which further adds to the ineffectiveness of such parental leave policies.

3.10 CONCLUSIONS

This paper examines the effect of federal and state parental leave policies on female employment and labor force participation, analyzing the interaction of these laws when more than one coexists in the same labor market. First, this study shows that the introduction of the Family and Medical Leave Act (FMLA) has a significantly positive impact on the employment of women in those states which had no state law providing parental leave benefits than in those applying the Temporary Disability Insurance (TDI). However, the estimated effects are quite small. Since the FMLA is granting unpaid leave, one implication of this result is that the FMLA is not really affecting the employment decisions of women in childbearing ages since most of them cannot afford to take leave which provides no payment. In general, the results suggest that the benefits associated with the FMLA are not enough to alter the labor force participation decisions of women who are the potential beneficiaries of the law by a substantial amount.

Second, we find that the impact of FMLA expansion on female employment and labor force participation in those states with the most generous expansions (in terms of improving the benefits and relaxing the eligibility criteria of FMLA) is significantly higher than in those states which did not expand FMLA. In particular, the higher opportunity cost from losing the job and its consequences on productivity may explain these interesting results.

Finally, in order to get an intuitive understanding of the effects of further increases in the generosity levels of parental leave policies, we consider the recent enactment of California's Paid Family Leave program. Specifically, we obtain that there is no impact of the introduction of this law, which may be due to the short time period of its application. Additionally, another explanation for the limitation of this policy (neither positive nor negative effect) on female employment and labor force participation is based on the previous adjustments in California's female labor market prior to the passage of this leave program, which reduces the possibility of further changes in the labor market behavior of both female workers and their employers. Also, the low take-up rates of the policy plays a crucial role in explaining its lack of effectiveness.

More extensive analysis should be conducted specially in the case of California, in order to test the hypothesis. In addition, further research can be carried out to examine the impact of additional increases in the generosity levels of federal and state parental leave policies on female employment and labor force participation, as well as the interaction of these policies in the same labor market, as is suggested in this paper.

4.0 THE LABOR MARKET EFFECTS OF STATE AND LOCAL EXPANSIONS OF THE EARNED INCOME TAX CREDIT

4.1 INTRODUCTION

EITC is one of the major federal programs providing assistance to the working poor. It encourages low-income workers with children to enter and remain in the labor market by supplementing the earnings of those working for low wages, hence "making work pay". It is a subsidy on labor earnings which raises the income of recipients without providing a disincentive to work. The EITC operates as a tax credit that serves to offset the payroll taxes and supplement the wages of low-income workers. The EITC's popularity relative to means-tested cash transfers like the Aid to Families with Dependent Children (AFDC) and TANF programs is due, at least in part, to the perception that the EITC rewards work. Unique among income-transfer programs for the poor, the EITC conditioned its benefits on earnings. Benefits actually increased with family earnings through a portion of the income distribution, before eventually phasing out at higher incomes. This was just the opposite of the traditional welfare programs like AFDC and Food Stamps, which provided maximum benefits to families without earnings and then reduced benefits at a very high rate as family earnings increased. As a result, EITC was viewed not as a replacement for traditional welfare, but as a supplement and as a potential source of cash assistance for the working poor, a group often overlooked by most poverty programs. The growing popularity of the federal EITC has led several states and some local governments to implement their own programs in recent years. The individuals who qualify for the federal EITC are entitled to receive the state supplement, which is typically a fixed percentage of the federal credit. The labor market effects of the federal EITC are well known; Eissa and Leibman (1996) found

a positive, significant effect and Meyer and Rosenbaum found that as much as 40 percent of the increase in labor force participation of single women with children between 1980 and 2000 can be attributed to the EITC expansions. The recent literature has also studied some of the state programs, most of which have focussed on one of the state supplements to the federal EITC (Hoffman, 2007; Cancian and Levinson, 2006), with somewhat contradictory results.

In this paper, we analyze the labor market impacts of the implementation of all the state and local governments' EITC supplement¹ using Current Population Survey data from 1984 to 2006. This is important for three reasons: it serves as a further test for the theory of labor force participation and labor supply, and contributes to answering the question of whether EITC payments by state governments affect participation in the same way as the federal EITC does. Given that at least one study found no significant effects of state EITCs (Cancian and Levinson, 2006), this question seems to be unresolved at this point. Moreover, since Cancian and Levinson looked at only one particular state EITC supplement (Wisconsin), it is worth checking the effect on labor force participation of all the states together, thus creating additional variation. A second reason is to help assess the effectiveness of these state programs, that is, whether the EITC programs are indeed an effective means of boosting labor force participation in a state, or are they expensive programs that do not achieve these goals. To the extent that state governments can allocate the funds to alternative means (like child care centers) that may also achieve the same desired impact, an estimate of the labor market effects of state EITC programs will help inform that choice. Hence, the magnitudes of these effects are key determinants of the gains or losses from the changes in income redistribution. Finally, while the federal and some of the state EITC programs were implemented and subsequently expanded at a time of strong female labor force participation growth, many of these state programs implemented in the last decade coincide with a period of generally flat female participation rates. If these EITC programs are found to have failed to affect female participation, this would suggest that future increases in the federal EITC may well fail to lead any more women to the labor force either. No other study has analyzed

¹Till date, there are twenty-three states and the District of Columbia, and four local governments which have implemented the EITC supplement.

the labor market impact of all of the state EITC programs that have been implemented till date. Our focus here will be on married women because they are the demographic group of workers whose behavior is most susceptible to policy changes.

The remainder of the paper is organized as follows. Section 4.2 explains the eligibility criteria and the general structure of the EITC and outlines its theoretical impact on participation and hours worked. Section 4.3 discusses the previous literature. Section 4.4 describes the data and the identification strategy and presents the empirical results. Section 4.5 concludes.

4.2 STRUCTURE OF FEDERAL AND STATE EITC

The EITC was established in 1975 for low-income working families with children. It is the largest cash transfer program for lower income families at the federal level. An unusual feature of the credit is its explicit goal to use the tax system to encourage and support those who choose to work. It is a tax credit for working families and individuals who earn less than a specified yearly wage. The EITC provides a wage supplement that is equal to a percentage of the earnings of the low-income individuals. The credit offsets the taxpayer's income tax liability or provides a refund in a situation where the family does not owe any taxes. Since its inception, the EITC has undergone three substantial expansions, the 1986 Tax Reform Act, the 1990 Omnibus Budget Reconciliation Act (OBRA) and the 1993 OBRA. It represents the largest single source of funding for low-income working families (\$41.5 billion in 2005, National Center for Children in Poverty) and has been found to produce discernible increases in employment and reductions in welfare receipt among single parents, as well as large decreases in poverty (Neumark and Wascher, 2000).

The EITC is refundable, meaning that the Treasury Department pays it out regardless of whether the taxpayer has any federal income tax liability. A taxpayer's eligibility for the earned income tax credit depends on his earned income (or in some cases adjusted gross income), and the number of qualifying children who meet certain age, relationship and

residency tests². First, the taxpayer must have positive earned income, defined as wage and salary income, business self-employment income, and farm self-employment income. Also, the taxpayer must have adjusted gross income and earned income below a specified amount (in 2006, the maximum allowable income for a taxpayer with two or more children was \$36,348; Tax Policy Center). Second, a taxpayer must have a qualifying child, who must be under age 19 (or 24 if a full-time student) or permanently disabled and residing with the taxpayer for more than half the year. Prior to 1993, the EITC was only available to families with children, and even now, the maximum credit available to families with children is much larger than that available to childless taxpayers. In addition, households with two or more children are able to claim a higher EITC than households with only one child, starting in 1991. The amount of the credit to which a taxpayer is entitled depends on the taxpayer's earned income, adjusted gross income, and the number of EITC-eligible children in the household.

There are three regions in the EITC credit schedule. The initial phase-in region transfers an amount which is equal to the subsidy rate times their earnings. In tax year 2006, the subsidy rate of the EITC was 7.66 percent for taxpayers with no child, 34 percent for taxpayers with one child and 40 percent for those with two or more children. In the flat region, the family receives the maximum credit (\$412, \$2747 and \$4536, respectively in tax year 2006), while in the phase-out region, the credit is phased out at the phase-out rate 7.66 percent for taxpayers with no child, 15.98 percent for those with one child and 21.06 percent for taxpayers with two or more children (2006). Figure 13 shows the EITC (2006) parameters for single parent families with one child, and two or more children.

Figure 14 shows how the introduction of the EITC shifts the budget constraint of an otherwise untaxed individual from ADE to ABCDE. With the EITC, an individual faces a new budget constraint, where each hour chosen produces at least as much after-tax earnings as it did before the earned income tax credit was introduced. Since the credit is not available to a taxpayer who does not work, that is, one who has zero earnings, his well-being has not changed. Hence any taxpayer who preferred to work before will still prefer working and some

²Since 1993, a small amount of the credit is available to taxpayers without children.

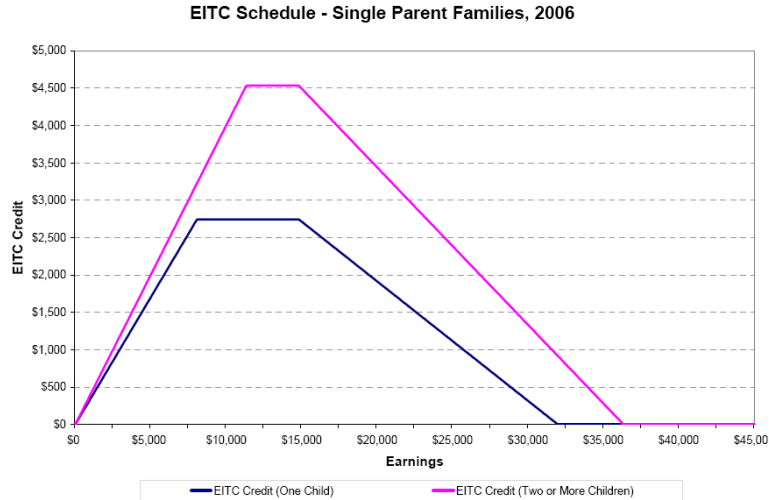


Figure 13: Federal EITC schedule, Single Parent Family 2006

may find that the additional after-tax income from the EITC makes it worth entering the labor force. Therefore, the impact of the EITC on the labor force participation (extensive margin) of taxpayers is unambiguously positive. The impact on the hours worked (intensive margin) of a taxpayer already working, however, depends on which region of the EITC he was in before the credit was introduced. There is a theoretically ambiguous effect on labor supply of a worker in the phase-in range. This is because, the credit increases the workers's effective wage so that the substitution effect encourages additional hours while the income effect causes hours to decrease. So the relative magnitudes of the substitution and income effect determines whether hours worked increases or decreases. For a worker in the flat region, there is an unambiguous decrease in labor supply, since there is only an income effect, which reduces hours worked. In the phase-out region, the EITC again reduces labor supply, since there is both a negative substitution effect from the credit being phased out and a negative income effect from the additional income received from the credit. Beyond the credit region, the taxpayers may have an incentive to reduce their hours of work in order to receive the credit.

The labor market incentives of the EITC are mixed. While the program has an unambiguously positive theoretical effect on participation, but conditional on participation the

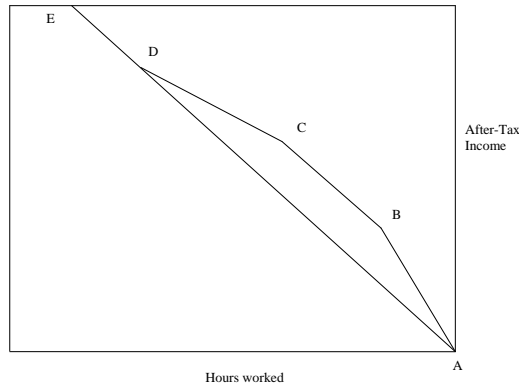


Figure 14: Federal EITC Budget Constraint

program has largely ambiguous effects on the labor supply decisions. There are also reasons to believe that all of these effects will be affected by complexities and lags in the tax code. The tax credits for income earned in one year are not received until taxpayers' file their EITC forms the following year. Also, workers' limited understanding of the EITC may reduce their responsiveness. For these reasons, the magnitude of the actual effect of the EITC on hours worked is an empirical question. Table 22 shows the complete federal EITC rate schedule since 1975.

Over the years, twenty four states and a few cities and counties have adopted their own EITC programs: Colorado, Delaware, Illinois, Indiana, Iowa, Kansas, Louisiana, Maine, Maryland, Massachusetts, Michigan, Minnesota, Nebraska, New Jersey, New Mexico, New York, North Carolina, Oklahoma, Oregon, Rhode Island, Vermont, Virginia, Washington, Wisconsin and the District of Columbia. These are set as a supplement to the federal EITC by offering benefits equal to some designated proportion of the federal payments³, typical percentages ranging from 5 percent to 43 percent. Most of these states provide EITC supplements which are refundable for taxpayers with zero liability⁴. Wisconsin is the only state that differentially affects families with three or more children, by providing a more

³Minnesota's credit for families with children is not structured as a percentage of the federal credit. Depending on income level, the credit for families with children may range from 25 percent to 45 percent of the federal credit. Also, Indiana has an EITC that is not based on the federal credit, but applies to families with children, where earned income exceeded 80% of total income and total income is below \$12,000.

⁴Delaware, Iowa, Maine and Virginia provide non-refundable EITC supplements.

Table 22: Federal EITC rate schedule 1975-2007

| Earned Income Tax Credit Parameters, 1975–2007 [Dollar amounts unadjusted for inflation] | | | | | | | |
|---|--------------|-------------------------|--------------------------|----------------|--------------------------|--------------------------|------------------------------|
| Tax Year | | Credit | Flat Region | Maximum Credit | Credit | Phase-Out Region | |
| | | Phase-In Rate (percent) | Begins at Earnings Level | | Phase-Out Rate (percent) | Begins at Earnings Level | Earnings Eligibility Ceiling |
| 1975–78 | | 10 | \$4,000 | \$400 | 10 | \$4,000 | \$8,000 |
| 1979–84 | | 10 | 5,000 | 500 | 12.5 | 6,000 | 10,000 |
| 1985–86 | | 14 | 5,000 | 550 | 12.22 | 6,500 | 11,000 |
| 1987 | | 14 | 6,080 | 851 | 10 | 6,920 | 15,432 |
| 1988 | | 14 | 6,240 | 874 | 10 | 9,840 | 18,576 |
| 1989 | | 14 | 6,500 | 910 | 10 | 10,240 | 19,340 |
| 1990 | | 14 | 6,810 | 953 | 10 | 10,730 | 20,264 |
| 1991 | One child | 16.7 | 7,140 | 1,192 | 11.93 | 11,250 | 21,250 |
| | Two children | 17.3 | 7,140 | 1,235 | 12.36 | 11,250 | 21,250 |
| 1992 | One child | 17.6 | 7,520 | 1,324 | 12.57 | 11,840 | 22,370 |
| | Two children | 18.4 | 7,520 | 1,384 | 13.14 | 11,840 | 22,370 |
| 1993 | One child | 18.5 | 7,750 | 1,434 | 13.21 | 12,200 | 23,050 |
| | Two children | 19.5 | 7,750 | 1,511 | 13.93 | 12,200 | 23,050 |
| 1994 | No children | 7.65 | 4,000 | 306 | 7.65 | 5,000 | 9,000 |
| | One child | 26.3 | 7,750 | 2,038 | 15.98 | 11,000 | 23,755 |
| | Two children | 30 | 8,425 | 2,528 | 17.68 | 11,000 | 25,296 |
| 1995 | No children | 7.65 | 4,100 | 314 | 7.65 | 5,130 | 9,230 |
| | One child | 34 | 6,160 | 2,094 | 15.98 | 11,290 | 24,396 |
| | Two children | 36 | 8,640 | 3,110 | 20.22 | 11,290 | 26,673 |
| 1996 | No children | 7.65 | 4,220 | 323 | 7.65 | 5,280 | 9,500 |
| | One child | 34 | 6,330 | 2,152 | 15.98 | 11,610 | 25,078 |
| | Two children | 40 | 8,890 | 3,556 | 21.06 | 11,610 | 28,495 |
| 1997 | No children | 7.65 | 4,340 | 332 | 7.65 | 5,430 | 9,770 |
| | One child | 34 | 6,500 | 2,210 | 15.98 | 11,930 | 25,750 |
| | Two children | 40 | 9,140 | 3,656 | 21.06 | 11,930 | 29,290 |
| 1998 | No children | 7.65 | 4,460 | 341 | 7.65 | 5,570 | 10,030 |
| | One child | 34 | 6,680 | 2,271 | 15.98 | 12,260 | 26,473 |
| | Two children | 40 | 9,390 | 3,756 | 21.06 | 12,260 | 30,095 |
| 1999 | No children | 7.65 | 4,530 | 347 | 7.65 | 5,670 | 10,200 |
| | One child | 34 | 6,800 | 2,312 | 15.98 | 12,460 | 26,928 |
| | Two children | 40 | 9,540 | 3,816 | 21.06 | 12,460 | 30,580 |
| 2000 | No children | 7.65 | 4,610 | 353 | 7.65 | 5,770 | 10,380 |
| | One child | 34 | 6,920 | 2,353 | 15.98 | 12,690 | 27,413 |
| | Two children | 40 | 9,720 | 3,888 | 21.06 | 12,690 | 31,152 |
| 2001 | No children | 7.65 | 4,760 | 364 | 7.66 | 5,950 | 10,710 |
| | One child | 34 | 7,140 | 2,428 | 15.98 | 13,090 | 28,281 |
| | Two children | 40 | 10,020 | 4,008 | 21.06 | 13,090 | 32,121 |
| 2002 | No children | 7.65 | 4,910 | 376 | 7.65 | 6,150 | 11,060 |
| | One child | 34 | 7,370 | 2,506 | 15.98 | 13,520 | 29,201 |
| | Two children | 40 | 10,350 | 4,140 | 21.06 | 13,520 | 33,178 |
| 2003 | No children | 7.66 | 4,990 | 382 | 7.66 | 6,240 | 11,230 |
| | One child | 34 | 7,490 | 2,547 | 15.98 | 13,730 | 29,666 |
| | Two children | 40 | 10,510 | 4,204 | 21.06 | 13,730 | 33,692 |
| 2004 | No children | 7.66 | 5,100 | 390 | 7.66 | 6,390 | 11,490 |
| | One child | 34 | 7,660 | 2,604 | 15.98 | 14,040 | 30,338 |
| | Two children | 40 | 10,750 | 4,300 | 21.06 | 14,040 | 34,458 |
| 2005 | No children | 7.66 | 5,220 | 399 | 7.66 | 6,530 | 11,750 |
| | One child | 34 | 7,830 | 2,662 | 15.98 | 14,370 | 31,030 |
| | Two children | 40 | 11,000 | 4,400 | 21.06 | 14,370 | 35,263 |
| 2006 | No children | 7.66 | 5,380 | 412 | 7.66 | 6,740 | 12,120 |
| | One child | 34 | 8,080 | 2,747 | 15.98 | 14,810 | 32,001 |
| | Two children | 40 | 11,340 | 4,536 | 21.06 | 14,810 | 36,348 |
| 2007 | No children | 7.66 | 5,590 | 428 | 7.66 | 7,000 | 12,590 |
| | One child | 34 | 8,390 | 2,853 | 15.98 | 15,390 | 33,241 |
| | Two children | 40 | 11,790 | 4,716 | 21.06 | 15,390 | 37,783 |

Source: Tax Policy Center; Joint Committee on Taxation, Ways and Means Committee 2004 Green Book

Note: For married couples filing joint returns, the Phase-Out Region beginning and ending levels are increased above listed values by \$1,000 in tax years 2002-04, by \$2,000 in tax years 2005-07, and by \$3,000 in tax year 2008.

Table 23: State EITC supplements (1984-2006, percentages of the federal credit)

| State children | CO | DC | DE | IL | IN | IA | KS | ME | MD 1+ | MA | MN 0 | MN 1+ | MI | NE | NJ | NY 1+ | OK | OR | RI | VT | VA | WI 1 | WI 2 | WI 3+ | |
|-------------------|-----|----|----|----|----|-----|----|------|----------|----|---------|----------|----|----|------|----------|----|----|-------|----|----|---------|---------|----------|------|
| 1984 | | | | | | | | | | | | | | | | | | | | | | | | | |
| 1985 | | | | | | | | | | | | | | | | | | | | | | | | | |
| 1986 | | | | | | | | | | | | | | | | | | | 22.21 | | | | | | |
| 1987 | | | | | | | | | | | | | | | | | | | 23.46 | | | | | | |
| 1988 | | | | | | | | | | | | | | | | | | | 22.96 | 23 | | | | | |
| 1989 | | | | | | | | | | | | | | | | | | | 22.96 | 25 | | | 5 | 25 | 75 |
| 1990 | | | | | | 5 | | | | | | | | | | | | | 22.96 | 28 | | | 5 | 25 | 75 |
| 1991 | | | | | | 6.5 | | | | | 10 | 10 | | | | | | | 27.5 | 28 | | | 5 | 25 | 75 |
| 1992 | | | | | | 6.5 | | | | | 10 | 10 | | | | | | | 27.5 | 28 | | | 5 | 25 | 75 |
| 1993 | | | | | | 6.5 | | | | | 10 | 10 | | | | | | | 27.5 | 28 | | | 5 | 25 | 75 |
| 1994 | | | | | | 6.5 | | | | | 15 | 15 | | | | 7.5 | | | 27.5 | 25 | | | 4.4 | 20.8 | 62.5 |
| 1995 | | | | | | 6.5 | | | | | 15 | 15 | | | | 10 | | | 27.5 | 25 | | | 4 | 16 | 50 |
| 1996 | | | | | | 6.5 | | | | | 15 | 15 | | | | 20 | | | 27.5 | 25 | | | 4 | 14 | 43 |
| 1997 | | | | | | 6.5 | | | | 10 | 15 | 15 | | | | 20 | | 5 | 27.5 | 25 | | | 4 | 14 | 43 |
| 1998 | | | | | | 6.5 | 10 | | 10 | 10 | 25 | 25 | | | | 20 | | 5 | 27 | 25 | | | 4 | 14 | 43 |
| 1999 | 8.5 | | | | | 6.5 | 10 | | 10 | 10 | 25 | 33 | | | | 20 | | 5 | 26.5 | 25 | | | 4 | 14 | 43 |
| 2000 | 10 | 10 | | 5 | | 6.5 | 10 | 5 | 15 | 10 | 25 | 33 | | | 10 | 22.5 | | 5 | 26 | 32 | | | 4 | 14 | 43 |
| 2001 | 10 | 25 | | 5 | | 6.5 | 10 | 5 | 16 | 15 | 25 | 33 | | | 15 | 25 | | 5 | 25.5 | 32 | | | 4 | 14 | 43 |
| 2002 | 10 | 25 | | 5 | | 6.5 | 15 | 5 | 16 | 15 | 25 | 33 | | | 17.5 | 27.5 | 5 | 5 | 25 | 32 | | | 4 | 14 | 43 |
| 2003 | 10 | 25 | | 5 | 6 | 6.5 | 15 | 4.92 | 20 | 15 | 25 | 33 | | | 20 | 30 | 5 | 5 | 25 | 32 | | | 4 | 14 | 43 |
| 2004 | 10 | 25 | | 5 | 6 | 6.5 | 15 | 4.92 | 20 | 15 | 25 | 33 | | | 20 | 30 | 5 | 5 | 25 | 32 | 20 | | 4 | 14 | 43 |
| 2005 | 10 | 25 | 20 | 5 | 6 | 6.5 | 15 | 4.92 | 20 | 15 | 25 | 33 | | | 20 | 30 | 5 | 5 | 25 | 32 | 20 | | 4 | 14 | 43 |
| 2006 | 10 | 35 | 20 | 5 | 6 | 6.5 | 15 | 5 | 20 | 15 | 25 | 33 | 10 | 8 | 20 | 30 | 5 | 5 | 25 | 32 | 20 | | 4 | 14 | 43 |
| Refundable? | Y | Y | N | Y | Y | N | Y | N | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | N | Y | Y | Y | Y |

Notes: children is left blank if the state supplement applies irrespective of the taxpayer's number of children. Maryland also had a non-refundable EITC of 50 percent for families with children from 1987-2006.

generous state EITC supplement for these families. State EITCs are set as a supplement to those who already qualify for the federal EITC, and the level is assigned to equal a given percentage of the federal payment. So the effective EITC rate π in a given state is such that, $\pi = (\text{federal EITC rate}) * (1 + \text{state EITC supplement})$. For example, a single parent with one child who earned \$7500 in 2007 would be in the phase-in range of income, and is eligible for a federal EITC payment of \$2550. If he lives in Rhode Island, which provides a 25 percent state EITC supplement, his effective phase-in rate would be $(34 * 1.25)$ and hence would receive an additional \$637.50 from the state government. Therefore, a 25 percent EITC supplement is equivalent to a 25 percent expansion of the federal EITC for residents of that particular state.

The details on state supplements are provided in table 23. While most of the states introduced EITC supplements in the mid to late 1990s, a considerable number of them implemented their state EITCs in the last few years.

The first state to enact an EITC supplement was Rhode Island in 1986. Initially, it offered a non-refundable EITC until 2002, and henceafter a small portion is made refundable.

There are four other states, Delaware, Iowa, Maine, and Virginia which offer credits that are nonrefundable. Such a credit is available only to the extent that it offsets a family's income tax. A nonrefundable EITC can provide substantial tax relief to families with state income tax liability, but it provides no benefits to working families that have income too low to owe any income taxes. Over the years, many states have increased their credits several times since first passing them and some states which had non-refundable state EITC supplements have made them refundable.

State EITCs are financed in whole or in part from funds available in a state's general fund that is, the same funding sources that are typically used for other types of tax cuts. When an EITC is used to offset the effects of a regressive tax increase such as a sales tax increase, a part of the proceeds of the revenue increase may be set aside for the EITC. Current federal regulations also offer the opportunity to finance a portion of the cost of a refundable credit from a state's share of the federal Temporary Assistance to Needy Families (TANF) block grant, but most states have very limited availability of such funds because the value of the TANF block grant has eroded over time and also states face costly new work requirements under the federal budget law⁵. There are four local governments who have also implemented their EITC programs in recent years. The first was Montgomery county in Maryland, which enacted the local EITC in 1999, set at 20 percent of the federal payment. The second local government and first city to pass its own EITC was Denver in 2002, a program which was discontinued in 2004 due to insufficient TANF funds. This was followed by New York City in 2004, which enacted a refundable EITC with the tax credit set at 5 percent of the federal credit. Finally, San Francisco launched a city-level, refundable EITC in 2005, which was initially set at 10 percent and increased to 16 percent in 2006.

4.3 PREVIOUS RESEARCH

Most of the previous studies of the labor supply effects of the EITC have focussed on the three major discrete policy changes in the federal EITC as the main source of variation. Eissa

⁵Colorado's EITC was funded by a state surplus under the Taxpayer Bill of Rights (TABOR). It was suspended in 2002 when surplus funds ran out.

and Liebman (1996) use a difference-in-differences approach to analyze the 1986 expansion of the EITC and found that the labor force participation rates of single women with children increased significantly compared to childless single women, whereas they found no change in the relative hours worked by single women with children who were already in the labor force. Meyer and Rosenbaum (2001) also determines the effect of EITC on the labor force participation of single women, by considering the impact of other tax and welfare changes like AFDC, Medicaid, Food Stamps and their implicit tax rates and earnings disregards, as well as AFDC waivers instituting time limits or work requirements, with their major source of variation being differences in incentives faced by single women with and without children. They found that the EITC played a dominant role in increasing the labor force participation rates of single mothers. Grogger (2003) looks at the effect of the federal EITC expansions and finds it to be the single most important policy measure for explaining the decrease in welfare and the rise in work and earnings among female-headed families.

Eissa and Hoynes (2004) look at the effect of federal EITC expansions from 1984-1996 on the labor supply of married couples, to find that increasing the EITC credit had a small positive impact on the labor supply of the husband, but a larger negative effect on the wives' labor supply, due to the marginal rate in the phase-out range. Hence, their finding suggests that EITC expansions reduced total family labor supply, and that the EITC is effectively subsidizing married mothers to stay at home. This is consistent with the earlier findings by Dickert et al (1995) and Ellwood (2000), who, using structural and quasi-experimental methods respectively, conclude that EITC does reduce the labor force participation of married women with children.

Neumark and Wascher (2001) exploit state level variation in EITC to see its effect on poor families. In order to get around the problem of endogeneity, they control for the state unemployment rate and welfare generosity. Analyzing a period from 1985-1994, when seven states implemented EITC supplements, they conclude that an increase in a state's EITC supplement helps families rise above poverty-level earnings by inducing labor market entry in families that initially did not have an adult in the workforce. Leigh (2005) also

uses variation in the state EITC supplements to examine the effect of the EITC on pretax wages. Using data from the Current Population Survey Merged Outgoing Rotation Group for the period 1984-2002, he uses variation across states in the presence and generosity of state EITC add-ons to create cross-sectional variation in the average tax rate faced by women with children. He finds a very strong negative effect of the credit on wages especially for low-skilled workers, implying very little benefit of the credit for its nominal recipients. However, he finds no effect of the credit on the wages of college graduates, and although workers with children receive a more generous tax credit than childless workers, the hourly wages of both groups are similarly affected by an increase in the generosity of the EITC. Leigh also uses an instrumental variable strategy by constructing a simulated instrument for the EITC parameters in an employee's labor market, to find that wages respond to variation in the fraction of eligible employees and the average EITC rate, but do not respond to changes in the marginal EITC rate.

Our work goes along the lines of Leigh, but using additional variation with more states offering their own EITC programs in the recent years⁶, and focussing on the labor force participation rates and week worked as outcomes, which he did not analyze specifically. Also, we use a richer set of regressors in different specifications to measure the presence and magnitude of the EITC offered by both the state and the local governments. This includes using the maximum credit received in some specifications, the phase-in rate in others, or whether the state is offering a sizeable enough credit in some other specifications. We focus here on married women in order to investigate whether added EITC payments from the state and local governments contribute to increasing the female labor force participation, above and beyond the positive impact that the federal EITC expansions have been shown to have on the national female participation trends. We do the analysis separately for married women with one child, married women with 2 children, and married women with 3 or more children so that we can control for the compositional effects that could arise because the fertility rates have not been constant over time.

⁶Eight more states enacted the state EITC supplement since Leigh's analysis in 2002.

More recently, Cancian and Levinson (2006) have examined the effect of the EITC on labor supply comparing outcomes in Wisconsin, which supplements the federal EITC for families with three children differently, to outcomes in the states which do not supplement the federal EITC. They find no effect of the EITC on labor force participation, which is somewhat surprising given the findings of previous research. Hoffman (2007) looks at the effects of a non-refundable state EITC supplement considering the example of Delaware and finds atypical distributional impacts of this policy. Specifically, he finds that the lower income half of the EITC recipient population is ineligible for the Delaware non-refundable EITC. Married couples and both single-parent and two-parent families with less than two children often lose eligibility and substantial portions of the benefits. Thus treating EITC as a tax relief, as in a non-refundable EITC, rather than income support operating through the tax system, eliminates the advantages of an EITC program. This finding encourages us to use additional variation by grouping together states with and without non-refundable EITC programs and see how female labor force participation rates vary between them.

4.4 DATA AND MAIN RESULTS

4.4.1 *Data Sources and Summary Statistics*

The data we use come from the Current Population Survey Merged Outgoing Rotation Group, and the March CPS for the period 1984-2006. The CPS is the monthly household survey conducted by the Bureau of Labor Statistics to measure labor force participation and employment of about 60,000 households per month. In the Outgoing Rotation Group surveys, every household that enters the CPS is interviewed each month for 4 months, then ignored for 8 months, then interviewed again for 4 more months. The usual weekly earning questions are asked only to households in their 4th and 8th interview. New households enter each month, so one fourth of the households are in an outgoing rotation each month. The March CPS is an annual demographic file of between 50,000 and 60,000 households per year, and includes detailed labor market and income information.

Our primary sample includes women of prime working age (between 18 and 55 years). In order to determine the EITC eligibility, we consider any member of the household who is under the age of 19 years (or under 24 years if a full-time student) to be a dependent child for tax purposes⁷. There are potential limitations with the MORG data. From January 1994 until October 1999, the basic monthly CPS (from which the MORG sample is drawn) did not ask respondents for their number of children. For this period, therefore, The MORG records for January 1994 to October 1999 were merged with the March CPS records for the same years, resulting in a successful merge rate of around 75 percent. We exclude women who are self-employed, ill or disabled, or in school full time during the previous year. We also exclude women with positive earned income but zero hours of work. The resulting size of the primary sample, after pooling all 22 years is 2,117,003 observations.

Table 24 presents summary statistics for the primary sample separately by family size and marital status. The demographic variables used in the analysis include age, race, marital status, education, number of children, labor force participation rates and the state unemployment rate. Summary statistics show that married women are older and have about the same education levels as the single women, and this is true for women both with and without children. Of interest is the pattern of labor force participation. The participation rate is lower for married women compared to single women, but declines more sharply with the first child in case of married women.

Table 25 presents the labor force participation rates of women separately by marital status and the number of children, between states with and without state EITC supplements, as well as for all of the states together. As mentioned above, the labor force participation rates are lower for married women with children, compared to single women both with and without children. However, the labor force participation rates for married women (both with and without children) are higher in the states which supplement the federal EITC, compared to those states which do not offer any such state program. This is further evidenced in Figure

⁷Due to data limitations, we omit some criteria for determination of eligible children- we assume that the residency requirement is met.

Table 24: Labor Force participation rates by demographic groups

| | Single | | | Married | | |
|-------------------------------------|----------------|----------------|----------------|----------------|----------------|----------------|
| | No child | 1 child | 2+ children | No child | 1 child | 2+ children |
| Labor Force (all states) | 0.89 (0.30) | 0.86 (0.34) | 0.86 (0.34) | 0.88 (0.31) | 0.83 (0.37) | 0.82 (0.38) |
| Labor Force (EITC states) | 0.89 (0.30) | 0.86 (0.34) | 0.86 (0.33) | 0.89 (0.30) | 0.84 (0.36) | 0.83 (0.37) |
| Labor Force (non-EITC states) | 0.89 (0.31) | 0.86 (0.34) | 0.86 (0.34) | 0.87 (0.32) | 0.83 (0.37) | 0.81 (0.38) |
| Observations (all states) | 211048 | 131704 | 555046 | 420248 | 253610 | 544831 |

Notes: Standard deviations are in parentheses.

The data are from survey years 1984-2006 of the Merged Ongoing Rotation Groups and March CPS. The sample contains women between the ages of 18 and 55 years. We exclude women who are self-employed. LFP: Labor force participation, which equals 1 if employed or unemployed, and zero if the individual is not in the labor force. Education: 1 refers to less than high school, 2 refer to high school degree, 3 refer to some college, 4 refer to college degree and 5 refer to advanced degree.

3, which shows the labor force participation rates of single and married women between 1986-2006, in a four-year period interval.

Table 25: Summary Statistics

| | Single | | | Married | | |
|--------------|------------------|------------------|------------------|------------------|-----------------|-----------------|
| | No child | 1 child | 2+ children | No child | 1 child | 2+ children |
| LFP | 0.89 (0.31) | 0.86 (0.34) | 0.86 (0.34) | 0.88 (0.31) | 0.83 (0.37) | 0.82 (0.38) |
| Age | 32.56 (11.73) | 31.54 (10.26) | 32.07 (10.67) | 41.04 (10.49) | 36.64 (8.86) | 36.62 (7.67) |
| Education | 2.72 (1.00) | 2.53 (0.95) | 2.75 (1.06) | 2.80 (1.08) | 2.77 (1.08) | 2.79 (1.08) |
| Children | 0 (0) | 1 (0) | 2.86 (0.34) | 0 (0) | 1 (0) | 2.86 (0.34) |
| Black | 0.16 (0.37) | 0.21 (0.40) | 0.15 (0.36) | 0.06 (0.24) | 0.07 (0.26) | 0.07 (0.26) |
| Hispanic | 0.06 (0.25) | 0.09 (0.28) | 0.08 (0.28) | 0.05 (0.22) | 0.07 (0.26) | 0.09 (0.29) |
| Other | 0.04 (0.20) | 0.04 (0.19) | 0.05 (0.21) | 0.04 (0.20) | 0.05 (0.21) | 0.05 (0.22) |
| Observations | 211048 | 131704 | 555046 | 420248 | 253610 | 544831 |

Notes: Standard deviations are in parentheses.

The data are from survey years 1984-2006 of the Merged Ongoing Rotation Groups and March CPS. The sample contains women between the ages of 18 and 55 years. We exclude women who are self-employed. LFP: Labor force participation, which equals 1 if employed or unemployed, and zero if the individual is not in the labor force. Education: 1 refers to less than high school, 2 refer to high school degree, 3 refer to some college, 4 refer to college degree and 5 refer to advanced degree.

4.5 CONCLUSIONS

A key goal of the EITC is to redistribute income to working poor families. In practice, the EITC is an important income source for many vulnerable families, including many single-mother families making the transition from welfare to work under recent welfare reforms. To many analysts, the EITC is preferable to other programs aimed at low-income families because it is tied to work. For families with a single worker earning low wages, the more hours worked, the greater their EITC. Thus, given its basic structure, the EITC unambiguously targets resources to low-income working families.

A less certain advantage of the EITC is its ability to increase labor supply. We analyze the labor market impacts of the implementation of all the state and local governments' EITC supplement using Current Population Survey data from 1984 to 2006 to examine the labor market consequences of the EITC. We find that the labor force participation rates for married women with children are higher in states which supplement the federal EITC program, compared to single women both with and without children in the states which do not provide any such supplement.

5.0 APPENDIX

Table 26: 2SLS Estimates of the Effect of Phone measures on Store closures

| | 2SLS (1) | 2SLS (2) |
|-----------------------------------|--------------------------------|-----------------------------|
| Phone | 15.073 [26.357] (37.467) | 3.105 [2.269] (2.140) |
| Controls for state fixed effects? | Yes | No |
| Observations | 245 | 245 |

Notes: Standard errors in brackets are adjusted for unknown forms of heteroskedasticity and serial correlation. Standard errors in parenthesis are robust. The store closures data are from Almighty Retailers for the years 2003 and 2007. All the regressions are weighted by population and include year-fixed effects. Other controls include population, mean education, age, and the proportion of female, white, black and Hispanic for each state-year. Column (2) reports results for regressions without including the state-fixed effects.

Table 27: Store closures based on the type of store

| | 2003 | 2004 | 2005 | 2006 | 2007 |
|--|-----------------|----------------|----------------|-----------------|----------------|
| Average number of Indie Store closings | 1.755 (0.31) | 6.89 (1.18) | 3.26 (0.72) | 5.69 (0.91) | 6.81 (1.53) |
| Average number of Others Store closings | 1.53 (0.29) | 5 (0.85) | 4.06 (0.70) | 10.45 (1.86) | 4.51 (0.77) |
| Average number of Chain Store closings | 2.14 (0.33) | 4.23 (0.64) | 4.25 (0.65) | 10.19 (1.75) | 3.89 (0.55) |
| Average number of Big Box Store closings | 1 (0.72) | 1.84 (0.32) | 2.07 (0.42) | 1 (0.32) | 2.35 (0.51) |
| Average number of Other Store closings | 0.22 (0.08) | 1 (0.19) | 0.43 (0.12) | 0.78 (0.23) | 0.78 (0.21) |
| Total number of Store closings | 160 | 593 | 363 | 801 | 557 |

Notes: Standard errors are in parenthesis. The abbreviations for the states pertain to the State Fips Code classified by the Bureau of Labor Statistics. The store closings data are from Almighty Research for the years 2003 through 2007.

Table 28: Summary Statistics: Store closures based on the type of store between states

| | Overall | VFL States | Non-VFL States | Urban States | Rural States |
|------------------------|----------------|-----------------|----------------|----------------|----------------|
| Indie Store Closures | 4.89 (0.47) | 8.08 (0.97) | 2.94 (0.37) | 5.31 (0.68) | 4.33 (0.54) |
| Others Store Closures | 5.11 (0.49) | 8.62 (1.06) | 2.97 (0.35) | 5.59 (0.69) | 4.52 (0.63) |
| Chain Store Closures | 3.98 (0.42) | 6.6 (0.91) | 2.38 (0.31) | 4.39 (0.61) | 3.46 (0.51) |
| Big box Store Closures | 0.49 (0.07) | 0.94 (0.18) | 0.23 (0.05) | 0.47 (0.08) | 0.55 (0.17) |
| Other Store Closures | 0.64 (0.08) | 0.82 (0.006) | 0.84 (0.01) | 0.73 (0.12) | 0.51 (0.09) |
| Number of observations | 245 | 95 | 155 | 155 | 85 |

Notes: Standard errors are in parenthesis. The states are classified as urban or rural depending on the percentage of people in each state having a telephone connection in the 1960 Census. The internet penetration data are from CPS Computer Use and Supplements for the years 1997-1998, 2000-2001, 2003 and 2007. The store closings data are from Almighty Research for the years 2003 and 2007.

Table 29: OLS Estimates of Effect of State Internet Penetration Rates on Store closures based on the type of store

| | Indie (1) | Chain Stores (2) | Big box (3) | Other (4) | Indie (5) | Chain Stores (6) | Big box (7) | Other (8) |
|-----------------------------------|-------------------------------|------------------------------|------------------------------|----------------------------|-------------------|---------------------|--------------------|-------------------|
| Internet penetration rates | 1.389*** [0.89] (1.258) | -0.104 [0.256] (0.364) | -0.261 [0.214] (0.304) | 0.245 [0.175] (0.25) | 1.041* (0.209) | -0.139* (0.052) | -0.228* (0.037) | 0.238* (0.028) |
| Controls for state fixed effects? | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 98 | 98 | 98 | 98 | 98 | 98 | 98 | 98 |

Notes: Standard errors in brackets are adjusted for unknown forms of heteroskedasticity and serial correlation. Standard errors in parenthesis are robust. The store closures data are from Almighty Retailers for the years 2003 and 2007. All the regressions are weighted by population and include year-fixed effects. ***denotes significance at the 1% level. *denotes significance at the 10% level. In columns (5) through (8) a heteroskedastic error structure with no cross-sectional correlation is assumed. Other controls include population, mean education, age, and the proportion of female, white, black and Hispanic for each state-year.

Table 30: Reduced-form Estimates of the Effect of VFL Adoption on Store closures based on the type of store

| | Indie (1) | Chain Stores (2) | Big box (3) | Other (4) | Indie (5) | Chain Stores (6) | Big box (7) | Other (8) |
|-----------------------------------|------------------------------|---------------------------|------------------------------|-----------------------------|------------------|------------------------|------------------|------------------|
| VFL | 4.516** [2.26] (2.522) | 9.044 [5.74] (6.61) | -0.106 [0.447] (0.426) | 0.941 [0.769] (0.977) | 2.797* (0.89) | 4.241* (1.21) | -0.192 (0.24) | 0.372* (0.18) |
| Controls for state fixed effects? | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observation | 98 | 98 | 98 | 98 | 98 | 98 | 98 | 98 |

Notes: Standard errors in brackets are adjusted for unknown forms of heteroskedasticity and serial correlation. Standard errors in parenthesis are robust. The store closures data are from Almighty Retailers for the years 2003 through 2007. All the regressions are weighted by population and include year-fixed effects. Columns (1) through (4) reports results for regressions where standard errors are adjusted to cluster at the state level. In columns (5) through (8) a heteroskedastic error structure with no cross-sectional correlation is assumed. Other controls include population, mean education, age, and the proportion of female, white, black and Hispanic for each state-year.

Table 31: First Stage Estimates of the Effect of VFL Adoption on State Internet Penetration Rates: Falsification test

| | Internet penetration rates (1) | Internet penetration rates (2) | Internet penetration rates (3) | Internet penetration rates (5) |
|-----------------------------------|--------------------------------------|--------------------------------------|--------------------------------------|--------------------------------------|
| VFL | -0.398 (0.928) [0.634] | -0.337 (1.447) | -0.316 (0.721) [0.854] | -0.306 (1.893) |
| Controls for state fixed effects? | Yes | Yes | No | No |
| Observations | 276 | 276 | 276 | 276 |

Notes: The Internet penetration data are from CPS Computer Use and Supplements for the years 1997, 1998, 2000, 2001, 2003 and 2007. All the regressions are weighted by population and include year fixed effects. Other controls include population, mean education, age, and the proportion of female, white, black and Hispanic for each state-year. Column (1) shows results when the standard errors are clustered at the state level, in brackets and when standard errors are adjusted for unknown forms of heteroskedasticity and serial correlation, in parenthesis. Column (2) reports results from GLS estimation. Columns (3) and (4) reports results for regressions without including the state-fixed effects.

Table 32: Reduced Form Estimates of the Effect of VFL Adoption on State Internet Penetration Rates: Falsification test

| | | Store closures (1) | Store closures (2) | Store closures (3) | Store closures (4) |
|-----------------------------------|-----|-------------------------------|-----------------------|--------------------------------|-----------------------|
| Panel A | VFL | 13.469 (11.842) [9.977] | 7.245 (6.268) | 5.623*** (1.995) [2.938] | 8.384*** (1.266) |
| Panel B | VFL | 6.572 (4.712) [6.436] | 2.202 (1.620) | 8.628*** (3.029) [3.728] | 8.876*** (1.436) |
| Controls for state fixed effects? | | Yes | Yes | No | No |
| Observations | | 218 | 218 | 218 | 218 |

Notes: Standard errors are in parenthesis. The store closures data are from Almighty Retailers for the years 2003 through 2007. Panel A denotes results pretending VFL was adopted in 2003. Panel B reports results pretending VFL was adopted two years prior to the actual period when it was adopted. ***denotes significance at the 1% level. All the regressions are weighted by population and include year fixed effects. Other controls include population, mean education, age, and the proportion of female, white, black and Hispanic for each state-year. Column (1) shows results when the standard errors are clustered at the state level, in brackets and when standard errors are adjusted for unknown forms of heteroskedasticity and serial correlation, in parenthesis. Column (2) reports results from GLS estimation. Columns (3) and (4) reports results for regressions without including the state-fixed effects.

Table 33: Estimated Probit Models for the Adoption of Video Franchise Laws

| | Pr(VFL) (1) | Pr(VFL) (2) | Pr(VFL) (3) | Pr(VFL) (4) | Pr(VFL) (5) |
|--------------------------|--------------------|-------------------|---------------------|------------------|---------------------|
| Business ranking | 0.0006 (0.0009) | | | | 0.001 (0.001) |
| Per Capita State GDP | | -0.001 (0.002) | | | -0.0007 (0.002) |
| State unemployment rates | | | 0.046*** (0.015) | | 0.043*** (0.014) |
| Household income | | | | 0.001 (0.005) | 0.002 (0.004) |
| Observations | 245 | 245 | 245 | 245 | 245 |

Notes: Robust standard errors are in parenthesis. Per capita state GDP and household income are scaled by thousand. The state business ranking data are from Tax Foundation's State Business Tax Climate Index; State GDP, Household income and unemployment rates data are from Bureau of Economic Analysis. ***denotes significance at the 1% level. Other controls include population, mean education, age, and the proportion of female, white, black and Hispanic for each state-year.

Table 34: Regression of VFL adoption on state characteristics

| | VFL (1) | VFL (2) | VFL (3) | VFL (4) | VFL (5) |
|-----------------------------|------------------|-------------------|---------------------|------------------|---------------------|
| Business ranking | 0.002 (0.002) | | | | 0.001 (0.002) |
| Per Capita State GDP | | -0.010 (0.014) | | | -0.012 (0.013) |
| State unemployment rates | | | 0.062*** (0.031) | | 0.063*** (0.030) |
| Household income | | | | 0.011 (0.014) | 0.012 (0.015) |
| Observations | 245 | 245 | 245 | 245 | 245 |

Notes: Robust standard errors are in parenthesis. Per capita state GDP and household income are scaled by thousand. The state business ranking data are from Tax Foundation's State Business Tax Climate Index; State GDP, Household income and unemployment rates data are from Bureau of Economic Analysis. ***denotes significance at the 1% level. Other controls include population, mean education, age, and the proportion of female, white, black and Hispanic for each state-year. All regressions include the state-fixed effects and year-fixed effects.

Table 35: Reduced-form regression including additional covariates

| | Store closures (1) | Store closures (2) |
|-----------------------------------|-----------------------|-----------------------|
| VFL | 8.344*** (1.739) | 7.490*** (1.735) |
| Business ranking | | 0.102*** (0.041) |
| Per Capita State GDP | | 0.512** (0.293) |
| State unemployment | | 0.384 (0.915) |
| Controls for state fixed effects? | Yes | Yes |
| Observations | 245 | 245 |

Notes: Results are from FGLS estimation. The store closures data are from Almighty Retails for the years 2003 through 2007. ***denotes significance at the 1% level. Per capita state GDP and household income are scaled by thousand. All the regressions are weighted by population. Other controls include population, mean education, age, and the proportion of female, white, black and Hispanic for each state-year. Column (1) shows results without including any additional controls. Column (2) shows results including state GDP, business ranking and megastores. All regressions include year-fixed and state-fixed effects.

Table 36: Falsification: The estimates of Female Employment between states with no law and states with TDI before FMLA assuming FMLA adopted in 1981

| | | NoLaw(1) | TDI(2) | DD(3) | DDD(4) | DDD.adj1 | DDD.adj2 | DDD.adj3 |
|-----------------------------|--------|----------|--------|--------|------------------|------------------|------------------|------------------|
| Treat | Pre'81 | 0.686 | 0.680 | | | | | |
| | Pos'81 | 0.684 | 0.674 | 0.005 | | | | |
| Control Obs.675,816 | Pre'81 | 0.539 | 0.543 | | | | | |
| | Pos'81 | 0.552 | 0.568 | -0.012 | 0.015 (0.012) | 0.017 (0.011) | 0.016 (0.012) | 0.017 (0.012) |
| standard errors | | | | | | | | |
| | | | | | | | | |
| Alt. Control Obs.963,726 | Pre'81 | 0.924 | 0.912 | | | | | |
| | Pos'81 | 0.919 | 0.908 | -0.001 | 0.005 (0.007) | 0.006 (0.007) | 0.005 (0.007) | 0.006 (0.007) |
| standard errors | | | | | | | | |
| | | | | | | | | |
| marital status? | | | | | no | yes | no | yes |
| linear time trend? | | | | | no | no | yes | yes |

Notes: The Pre '81 period consists of the years 1978 - 1980. Pos' 81 period consists of 1981-1984. Robust Standard errors are reported.

Table 37: The estimates of Female Labor Force Participation rates between Connecticut and states with no law before FMLA

| | | Connecticut | Only FMLA | DD | DDD | DDD adj.1 |
|-----------------------------|--------|-------------|--------------|--------|------------------|------------------|
| Alt. Treat | Pre 97 | 0.582 | 0.595 | | | |
| | Pos 97 | 0.684 | 0.588 | 0.108 | | |
| Control Obs. 27,884 | Pre 97 | 0.700 | 0.685 | | | |
| | Pos 97 | 0.758 | 0.703 | 0.039 | 0.069 (0.079) | 0.068 (0.079) |
| standard errors | | | | | | |
| | | | | | | |
| Alt. Control Obs. 56,190 | Pre 97 | 0.885 | 0.912 | | | |
| | Pos 97 | 0.883 | 0.913 | -0.002 | 0.111 (0.074) | 0.112 (0.073) |
| standard errors | | | | | | |
| | | | | | | |
| marital status? | | | | | no | yes |
| linear time trend? | | | | | no | no |

Notes: Robust Standard errors are reported. DDDadj 1 reports estimates controlling for marital status. Pre 97 years are '94-'96. Pos 97 years are '97-'99.

Table 38: The estimates of FLFP rates between Vermont and states with no law before FMLA

| | | Vermont | Only FMLA | DD | DDD | DDD adj.1 |
|---|--------|---------|-----------|-------|------------------|------------------|
| Alt. Treat | Pre 98 | 0.575 | 0.599 | | | |
| | Pos 98 | 0.666 | 0.587 | 0.104 | | |
| Control Obs. 27,672 standard errors | Pre 98 | 0.750 | 0.692 | | | |
| | Pos 98 | 0.771 | 0.709 | 0.005 | 0.099 (0.081) | 0.099 (0.076) |
| Alt. Control Obs. 54,698 standard errors | Pre 98 | 0.899 | 0.912 | | | |
| | Pos 98 | 0.915 | 0.915 | 0.013 | 0.091 (0.076) | 0.088 (0.076) |
| marital status? | | | | | no | yes |
| linear time trend? | | | | | no | no |
| Notes: Robust Standard errors are reported. DDDadj 1 reports estimates controlling for marital status. Pre 98 years are '95-'97. Pos 98 years are '98-'00. | | | | | | |

Table 39: The estimates of Female Employment between California and states with no law before FMLA

| | | California | Only FMLA | DD | DDD | DDD adj.1 |
|---|--------|------------|-----------|--------|-------------------|-------------------|
| Treat | Pre04 | 0.787 | 0.807 | | | |
| | Pos 04 | 0.764 | 0.783 | 0.001 | | |
| Control Obs. 143,023 standard errors | Pre 04 | 0.745 | 0.745 | | | |
| | Pos 04 | 0.744 | 0.739 | 0.005 | -0.005 (0.124) | -0.004 (0.012) |
| Alt. Control Obs. 193,978 standard errors | Pre 04 | 0.921 | 0.921 | | | |
| | Pos 04 | 0.903 | 0.905 | -0.002 | 0.001 (0.008) | -0.001 (0.008) |
| marital status? | | | | | no | yes |
| linear time trend? | | | | | no | no |
| Notes: Robust Standard errors are reported. DDDadj 1 reports estimates controlling for marital status. Pre 04 years are '01-'03. Pos 04 years are '04-'07. | | | | | | |

Table 40: The estimates of Female Employment between California and states which expanded FMLA

| | | California | Exp. FMLA | DD | DDD | DDD adj.1 |
|--|--------|------------|-----------|--------|-------------------|-------------------|
| Treat | Pre 04 | 0.843 | 0.831 | | | |
| | Pos 04 | 0.819 | 0.814 | -0.006 | | |
| Control Obs. 54,838 standard errors | Pre 04 | 0.704 | 0.801 | | | |
| | Pos 04 | 0.702 | 0.795 | 0.004 | -0.013 (0.016) | -0.012 (0.016) |
| Alt. Control Obs. 74,235 standard errors | Pre 04 | 0.904 | 0.927 | | | |
| | Pos 04 | 0.886 | 0.910 | -0.002 | -0.005 (0.012) | -0.005 (0.012) |
| marital status? | | | | | no | yes |
| linear time trend? | | | | | no | no |

Notes: Robust Standard errors are reported. DDDadj 1 reports estimates controlling for marital status.
Pre 04 years are '01-'03. Pos 04 years are '04-'07.

Table 41: Weekly Benefit amount and Duration of Benefits in TDI states

| State | Status | Weekly Benefit Amount | Duration |
|-------|-------------------|--|--|
| CA | All workers | \$50-\$336 based on schedule of high quarter wage | 6 – 52 weeks, \$300 - \$14,472, computed as lesser of 52 x weekly benefit amount or the total of base period wages. Duration separate from UI |
| HI | Employed workers | \$14-\$357. For an average weekly wage of less than \$26, weekly benefit amount is the average weekly wage up to a maximum of \$14. If average weekly wage is \$26 or more, weekly benefit amount is 58% of average weekly wage with a maximum of 66-2/3 percent of average weekly wage. | Uniform 26 weeks in benefit year |
| | Unemployed worker | Same as UI | Balance of weeks claimant would have been eligible for benefits in his UI benefit year but not more than 26 weeks. |
| NJ | Employed workers | \$68-\$470 (based on schedule of average weekly wage). Average weekly wage determined by dividing wage from 1 employer during base weeks in weeks. If less than average using all employment during last 8 weeks, use earnings from all employers. | Computed as lesser of 26 x weekly benefit amount or 1/3 base period wages. Limit applies to benefits in any 12 consecutive month periods. Duration separate from UI and from benefits as an unemployed disable worker. |
| | Unemployed worker | \$61-\$475 (based on schedule of average weekly wage). Average weekly wage determined by dividing wage from 1 employer in all base weeks by numbers of base weeks. If not 20 base weeks with any 1 employer, average base weeks with all employers. | 15-26 weeks, \$1,095-\$9,464 computed as ¾ weeks, but not more than 26 x weekly benefit amount. Duration under UI and disability during employment limited to 150% of duration for either program separately. |
| NY | All workers | \$20 - \$170 on basis of one-half average weekly wage in last 8 weeks, or portion thereof, in covered employment prior to commencement of disability. If average is less than \$20, weekly benefit is average wage. | Uniform potential 26 weeks in any 52 consecutive weeks or for any single period of disability, \$520 (or less if weekly benefit amount is less than \$20 - \$4,420). Duration separate from UI. |
| RI | All workers | \$53 - \$588 (4.62% of high quarter wages up to 85% of state's average weekly wage in preceding calendar year, plus the greater of \$10 or 7% of the individual's benefit rate for each dependent up to 5 dependents). | 12 - 30 weeks \$636 - \$17,640 computed as 36% of total base period wages plus dependent allowance if any. |

Table 42: Classification of the States

| States | TDI | FMLA only | Expa. FMLA | N° weeks | N° Hours | N° Workers | Others |
|----------------------|-----|-----------|------------|----------|----------|------------|--------------------|
| Alabama | | ✓ | | | | | |
| Alaska | | | | | | | |
| Arizona | | ✓ | | | | | |
| Arkansas | | | ▪ | | | | |
| California | X | | | | | | |
| Colorado | | | | | | | |
| Connecticut | | | ▪ | 16 | 1000 | 75 or more | accrued sick leave |
| Delaware | | ✓ | | | | | |
| Florida | | | | | | | |
| Georgia | | | | | | | |
| Hawaii | X | | ▪ | | | | |
| Idaho | | ✓ | | | | | |
| Illinois | | | | | | | |
| Indiana | | ✓ | | | | | |
| Iowa | | | | | | | |
| Kansas | | | | | | | |
| Kentucky | | ✓ | | | | | |
| Louisiana | | | | | | | |
| Maine | | | ▪ | 12 | 1250 | 15 or more | accrued sick leave |
| Maryland | | | | | | | |
| Massachusetts | | | | | | | |
| Michigan | | ✓ | | | | | |
| Minnesota | | | ▪ | | | 21 or more | |
| Mississippi | | ✓ | | | | | |
| Missouri | | ✓ | | | | | |
| Montana | | | | | | | |
| Nebraska | | ✓ | | | | | |
| Nevada | | ✓ | | | | | |
| New Hampshire | | | | | | | |
| New Jersey | X | | ▪ | | | | |
| New Mexico | | ✓ | | | | | |
| New York | X | | | | | | |
| North Carolina | | ✓ | | | | | |
| North Dakota | | | | | | | |
| Ohio | | | | | | | |
| Oklahoma | | | | | | | |
| Oregon | | | ▪ | 12 | 1250 | 25 or more | accrued sick leave |
| Pennsylvania | | | | | | | |
| Rhode Island | X | | ▪ | | | | |
| South Carolina | | ✓ | | | | | |
| South Dakota | | ✓ | | | | | |
| Tennessee | | | | | | | |
| Texas | | ✓ | | | | | |
| Utah | | ✓ | | | | | |
| Vermont | | | ▪ | 12 | | 10 or more | accrued sick leave |
| Virginia | | | | | | | |
| Washington | | | ▪ | | | | |
| West Virginia | | | | | | | |
| Wisconsin | | | ▪ | | | | |
| Wyoming | | ✓ | | | | | |
| District of Columbia | | | ▪ | | | | |

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