THREE ESSAYS ON LABOR AND HEALTH ECONOMICS

By

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ABSTRACT

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In chapter 1, I investigate the effectiveness of tax credits on health insurance premiums. There was a renewed interest in using tax credits to increase health insurance coverage after the push to repeal the Affordable Care Act (ACA). The Health Insurance Tax Credit (HITC) was implemented between 1991–1993 to reduce the burden of health insurance premiums primarily for low-income families. Although it was active for three years, this policy has been studied in only one previous study. In this chapter, I examine the effectiveness of the HITC by using the Survey of Income Program Participation (SIPP), and I provide the first estimates of its effects on healthcare utilization and selfreported health status. My results align with previous studies and suggest the HITC increased the health insurance take-up by 5.8 percentage points. The implementation of the HITC also significantly improved the self-reported health status of respondents.

In the second chapter, I analyze the effects of dependent coverage mandates on working fathers job mobility and compensation. Due to the low rates of health insurance coverage among young adults, some state governments began mandating health insurance companies to allow adult children to stay on their parents' health insurance plans. First implemented in 1995, these mandates aimed to increase health coverage among young adults. In 2010, the federal government enacted a more comprehensive version of the dependent coverage mandate as part of the Affordable Care Act. These state- and federal-level efforts successfully increased insurance rates for young adults, but they might have also come with unintended consequences for parents. Parents who placed a high value on health insurance for their young adult children might be reluctant to leave jobs with employer-provided health insurance, and employers might offset the mandated-incurred health care costs by reducing other types of employee benefits or earnings. To assess the extent of such consequences, I study the effects of both the state and federal dependent health insurance mandates on fathers. By analyzing the 2004 and 2008 SIPP panels, which are linked with Detailed Earnings Records and Business Registrar data from the United States Census, I examine the mandates' effects on fathers' voluntary job separation rates (job-lock and job-push) and changes in their compensation. After the implementation of the mandates, I observe a significant decrease in the likelihood of voluntary job separation among eligible working fathers aged 45–64 with employer-provided health insurance.

In the last chapter, we investigate the impact of lifetime earnings on retirement wealth. Venti and Wise (1999, 2001) directly examine this question by utilizing data that was superior to that available to previous researchers and conclude that "the bulk of the dispersion must be attributed to differences in the amount that households choose to save." In this paper, we examine the extent that a remaining problem in their data affected their results: Their measure of lifetime earnings, despite being based on administrative data, was subject to topcoding in each year. Using the 2001 SIPP that was not subject to the same problem, we find that the effect of the topcoding was substantial. At least 35 percent of individuals were misclassified in each of the top four deciles. When replicating a key result of Venti and Wise (2001), our findings suggest that the correlation between lifetime earnings and savings was about 50% greater than what was found when using censored deciles. This increased explanatory power came largely at the expense of the other variables in the regression model.

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Chapter 1. Effectiveness of Tax Credits for Health Insurance Premium: Evidence from the Health Insurance Tax Credit

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1.1 Introduction

Health insurance access and affordability continues to dominate the political landscape in the United States. Since the beginning of the Trump administration, a significant component of the debate is to repeal and replace the Affordable Care Act (ACA), a government-led health policy enacted in 2010. For instance, the GOP tax bill that was recently passed includes getting rid of the individual mandate under the ACA. As a result, there are substantial fears that this change, alongside others, could increase premiums as healthy individuals will exit the market. A common method to reduce the burden of premiums, and thus encourage take-up, is using tax credits.

In this paper, I revisit the effectiveness of tax credits that were intended to promote coverage take-up and health-care utilization by examining the implementation and repeal of the Health Insurance Tax Credit (HITC). The overall circumstances of tax credits offered under the HITC differ from current laws and proposals but still provide a natural experimental setting to explore the effectiveness of tax credits on health insurance coverage and other health-related outcomes.¹ To date, only Cebi and

¹With the HITC, some people were given tax credits for which they were not eligible before. In contrast, the most currently proposed bills [e.g., American Health Care Act (AHCA), Better Care Reconciliation Act (BCBA), Obamacare Repeal Reconciliation Act (ORRA) and Health Care Freedom Act (HCFA)] often suggest reducing those tax credits.

Woodbury (2014, hereafter CW) has explored the effectiveness of the HITC, but they solely examined the aspect of change in health insurance.

This paper begins by replicating CW based on a different data set, the Survey of Income and Program Participation (SIPP). I find a 7.1 percentage points (pp) increase in private health insurance coverage for low-educated, working single mothers due to the HITC combined with the Earned Income Tax Credit (EITC). This result is somewhat larger than the 4.7 pp increase found by CW using the Current Population Survey (CPS). I extend the analysis to the policy change in 1994-1995 (when the EITC was expanded and the HITC was repealed) to disentangle the effect of the EITC from that of the HITC.² This analysis suggests that the effect of the HITC alone is 5.8 pp while CW concluded 3.6 pp by using a different DDD approach.³ Given the consistency of my results with the previous paper, this paper assures the effectiveness of the HITC on coverage. I also estimate the effect of the HITC on health-care utilization and health status, finding a statistically significant improvement in health status.⁴

1.2 Methods

1.2.1 Empirical Strategy

The HITC was a supplement to the EITC from 1991 through 1993 and was introduced as part of the Omnibus Budget Reconciliation Act of 1990. It was a refundable tax credit available to EITC-eligible taxpayers who purchased private health insurance and

²The increase in the maximum amount of credits through the EITC (by \$239 in 1991, \$132 in 1992 and \$110 in 1993) has the potential to elevate the demand for health insurance. Therefore, it is problematic to identify the effect of the HITC *per se* on coverage without considering the policy change in 1994-1995.

 $^{^{3}}$ While my analysis is based on 1989-1995, CW instead enlarges the sample by adding earlier years going back to 1985 to isolate the effect of the HITC.

⁴It is reasonable to hypothesize that if low-income populations experienced a significant increase in insurance enrollment, then the effects of health-care utilization and health status in the active-HITC period might have subsequently increased.

had at least one child covered. The HITC was structured to vary by earned income.⁵

To examine how the adoption of the HITC with a parallel increase in the EITC affected single mothers, I first follow a difference-in-differences (DD) strategy used in CW by assessing changes in the 1991-1993 policy that simultaneously implemented the HITC and expanded the EITC, compared to 1989-1990. This strategy estimates the effect of the HITC combined with the EITC by comparing the average change in the outcomes for the treatment group with that of the control group.⁶ As in CW, my treatment group is working single mothers not exceeding a high school education and the control group is working single women (without children) not exceeding a high school education.

My primary outcome of interest, *private health insurance coverage*, is based on the following SIPP questions: (1) 'Was the respondent covered by a private health insurance plan specifically under her own name?' (2) 'Was this individual-type plan covering only the respondent?' (3) 'Was this family-type plan covering all of the children in the respondent's family?'

Single women without children (the control group) are defined as *covered* if they answered 'yes' to the first two questions. Single mothers with children (the treatment group) are defined as *covered* if they answered 'yes' to the first and third questions.⁷

⁵For example, if annual income was between \$1 and \$7,140, the credit was 6 percent of income. If one's income was between \$7,140 and \$11,250, the credit stayed constant at \$428. The credit gradually decreased to zero at the income-level of \$21,250. The average amount received by HITC-qualified individuals was about 23 percent of the overall average cost for health insurance premiums (GAO, 1994).

⁶The DD assumes that the trend in outcomes for both groups would have been the same without the 1991-1993 policy. Therefore, any deviation from this trend is attributed to the 1991-1993 policy.

⁷One advantage of the SIPP is that it has information about the type of health insurance: individual or family. Despite this, due to limitations of the data (e.g., in 1989, subsequent questions of health insurance only asked whether it covers 'all' or 'only the respondent'), I consider the treatment group to be *covered* only if her family coverage accommodates '*every* child in her family.' Therefore, the findings below may be conservative estimates of the effects of the HITC because mothers whose family plans only covered one of their children might not be considered as *covered*.

In the baseline specification, I use the same linear probability model as CW (OLS):

$$y_{ist} = \beta_0 + \beta_1 * Treat_{ist} + \beta_2 * 1(t = 91, 92, 93)_t +$$

$$\beta_3 * Treat * 1(t = 91, 92, 93)_{ist} + \beta_4 * Z_{ist} + \epsilon_{it}$$
(1)

where i, t and s index individual, time (year) and state, respectively. The outcome variables of interest, y_{ist} , are binary variables indicating whether the individual [1] 'was covered by private health insurance under the respondent's name,' [2] 'had visited a physician at least once in the previous year,' and [3] 'had a health status that was good, very good, or excellent.' *Treat*_{ist} represents an indicator for a single mother. $1(t = 91, 92, 93)_t$ is a dummy for the years 1991 to 1993. *Treat* $* 1(t = 91, 92, 93)_{ist}$ is equal to unity only if she was in the treatment group and the tax year is 1991, 1992 or 1993. The estimate of β_3 denotes the effect of the 1991-1993 policy, the combined effect of the HITC and the EITC. Z_{ist} includes same controls as used in CW.⁸

Although estimating (1) provides useful information about the HITC combined with the EITC, β_3 , I wish to estimate the effect of the HITC in isolation of the EITC. While CW also extends their analysis to isolate the HITC effect, their Difference-in-Difference-in-Differences (DDD) does not work with my data.⁹ Instead, I conduct a different DDD by further including the information from 1994 to 1995 when the HITC was repealed and the EITC was expanded again.¹⁰ Including the additional information

⁸This includes race, age, categories of monthly total earned income that are converted to 1989 dollars (<\$500, \$500-\$1000, \$1000-\$1500, \$1500-\$2000, >\$2000), work status (Full-time/Full-month, Part-time/Full-month, Full-time/Part-month) and number of children in the household. I also control for state-fixed effects, state unemployment rates (URT_{st}) and the interaction of (URT_{st}) with the treatment group indicator.

⁹In the SIPP, the main questionnaire was changed before and after 1989 (e.g., the SIPP explicitly asked, "whether a respondent has *private* health insurance" in 1989 onwards, whereas it asked more generally, "whether a respondent has health insurance" before 1989).

¹⁰Congress passed the OBRA of 1993 with an enactment of the largest EITC expansion in history (e.g., a mother with one child could receive a maximum credit of \$1434 in 1993, while the maximum credit increased to \$2038 in 1994).

from 1994 to 1995 enables me to net out the possible influence of the EITC. Specifically, I estimate the model below:

$$y_{ist} = \delta_0 + \delta_1 * Treat_{ist} + \delta_2 * 1(t = 91, 92, 93)_t + \delta_3 * 1(t = 94, 95)_t + \delta_4 * Treat * 1(t = 91, 92, 93)_{ist} + \delta_5 * Treat * 1(t = 94, 95)_{ist} + \delta_6 * Z_{ist} + \epsilon_{it}$$

$$(2)$$

The only change in (2) is that $1(t = 94, 95)_t$ and $Treat * 1(t = 94, 95)_{ist}$ are now included.¹¹ While δ_4 estimates the change in coverage among single mothers from 1991 to 1993, relative to 1989-1990, δ_5 estimates the change in coverage among single mothers from 1994 to 1995, relative to 1989-1990. With an additional assumption that the 1994-1995 EITC expansion had the same impact as that of 1991-1993, the change attributed solely to the HITC can be estimated as $\delta_4 - \frac{\delta_5}{2}$.¹²

1.2.2 Data

I use data from the SIPP. Households are interviewed once every four months and asked questions about the previous four months. Within each SIPP panel, the sample is randomly divided into four rotation groups. One rotation group is interviewed each month and after all rotation groups complete their first interview, the first wave of the panel concludes. This continues typically for eight waves of each panel over the

¹¹The sample now has 1989-1995 time frame.

¹²To see why this quantity is an estimate of the HITC only, denote the 1991-1993 EITC effect on coverage as a_1 , the HITC effect on coverage as a_2 and the 1994-1995 EITC effect on coverage a_3 . Then, I can rewrite $\delta_4 = a_1 + a_2$ and $\delta_5 = (a_1 + a_2) + (a_3 - a_2)$. The first term on the right-hand side (RHS) of δ_5 is the effect of the 1991-1993 policy and the second term on the RHS represents the subsequent change in 1994-1995. With an additional assumption that $a_1 = a_3$, I can rewrite $\delta_5 = 2a_1$. Therefore, $a_2 = \delta_4 - \frac{\delta_5}{2}$ measures the 'HITC effect on coverage.' All of this analysis is based on the assumption that the effect of removing the HITC, $-a_2$, is the negative of the effect of adding the HITC, a_2 .

course of three years.¹³ For each SIPP panel, I select a wave that represents each year from 1989 to 1995. I use single women drawn from the third wave of the 1989 to 1993 panels (September to December for each year). Responses from SIPP Panel-1993 wave 6 and wave 9 represent periods from September to December in 1994 and September to December in 1995, respectively.¹⁴ Table 1.1 provides the descriptive statistics of the sample from 1989 to 1995.¹⁵

1.3 Results

1.3.1 Coverage Rates

Columns 2-3 in Table 1.2, representing results from (1), indicate that private health insurance coverage was greater by about 7.1 pp than it otherwise would have been for single mothers from 1991 to 1993.¹⁶ Even with a different data set, my results are very similar to that of CW that estimated an increase in coverage of about 4.7 pp due to the 1991-1993 policy.¹⁷

In Table 1.3, I verify the robustness of the results. First, there were statewide reforms [i.e., state Aid to Families with Dependent Children reforms (AFDC) and state-specific EITCs] that might have confounded the results. Since state AFDC re-

¹³Multistage-stratified sampling is the other important aspect of the SIPP. To take this into account, I report both the weighted (WLS) and unweighted (OLS) estimates of the linear regression analyses below in Table 1.2 and 1.4. I only report the weighted estimates for the remaining tables.

 $^{^{14}}$ I use 6th and 9th interviews of individuals from SIPP-1993 Panel to examine the HITC repeal along with the EITC expansion in 1994-1995 as there was no new sample collected during these years.

¹⁵Table 1.9 in the appendix shows the changes in outcome variables of interests during this period.

 $^{^{16}}$ If I take the number of the HITC eligible families headed by working single mothers with low education from the 1991 CPS (i.e., 2,485,000) and the estimated coverage increase of about 7.1 pp, there would be an increase in enrollment by about 176,435 people who would have otherwise not enrolled in health insurance.

¹⁷In addition to this, Table 1.6 in the non-published appendix shows the event history analysis where I disaggregated the HITC combined with the EITC effect by years to explore whether this had a similar magnitude in all three years. Further, leaving out the interaction term of 1989 and the treatment dummy, there was no significant pre-treatment effect in 1990, possibly supporting the validity of the common trend assumption.

Variables	Single Women	Single Mothers
Age (in years)	30.9	30.3
	(7.93)	(7.90)
% w/<12 years of education	.168	.230
, ,	(.374)	(.420)
% w/=12 years of education	.831	.769
	(.374)	(.420)
Number of Kids	-	1.86
		(1.04)
% White	.833	.755
	(.372)	(.430)
% Black	.136	.220
	(.343)	(.414)
% Others	.029	.024
	(.170)	(.155)
% Full-time, Full-Month	.801	.729
	(.398)	(.444)
% Part-time, Full-Month	.168	.231
	(.374)	(.422)
% Full-time, Part-Month	.019	.022
	(.137)	(.149)
% Part-time, Part-Month	.010	.015
	(.102)	(.123)
Total Income (Monthly \$	1,254	$1,\!123$
At the time of the interview)	(881)	(713)
% Unemployment Rate in one's State	6.24	6.22
	(1.40)	(1.35)
Private Health Insurance under own name-covered	.634	.128
	(.481)	(.334)
Annual Office Visit (Extensive Marign; Any)	.704	.671
	(.456)	(.469)
Self-Reported health good/very good/excellent	.917	.916
	(.275)	(.276)
Observations	2,279	4,216

Table 1.1: Summary Statistics, 1989-1995

Covered by Private Health Insurance	(1)	(2)	(3)
	CW (2014)	WLS	OLS
Treat, β_1	128***	472***	468***
	(.024)	(.072)	(.069)
$1(t=91,92,93), \beta_2$	142***	079**	091***
	(.001)	(.032)	(.029)
Treat*1(t=91,92,93), β_3	.047***	.071*	.081**
	(.012)	(.038)	(.032)
State FE	Y	Y	Υ
Observations	$21,\!152$	4,722	4,722
R-squared	.336	.391	.382

Table 1.2: Estimates from Equation (1), 1989-1993

Table 1.3: Robustness Check, 1989-1993

Covered by Private Health Insurance	(1)	(2)	(3)
Treat	409***	487***	454***
	(.057)	(.081)	(.068)
1(t=91,92,93)	103***	078**	083**
	(.033)	(.036)	(.036)
Treat*1(t=91,92,93)	.080*	.059	.072*
	(.044)	(.042)	(.040)
Excluding states that adopted welfare waivers	Y		
Excluding states that had state-level EITCs		Υ	
Including an indicator for high school graduates			Υ
State FE	Y	Υ	Y
Observations	$3,\!573$	4,328	4,722
R-squared	.389	.387	.340

forms restricted the duration of welfare reliance, it resulted in eligibles getting into the labor force, often accompanied by health insurance. Therefore, I exclude states that instituted an AFDC reform and re-estimate (1). The result in column 1 of Table 1.3 was similar to Table 1.2, suggesting that the coverage increase was not mainly due to AFDC reforms. Because state-EITC benefits could also provide another source of variation for the impact of tax credits on coverage (Baughman, 2005), I exclude states that had their own specific-EITC benefits. The result in column 2 remains unchanged, thereby showing that state-specific EITCs are not a significant concern. With reference to Eissa and Hoynes (2006) that respondents might adjust their income to be eligible for the HITC, column 3 addresses this concern by re-estimating (1) without income being included.¹⁸

¹⁸While I exclude income controls, I do consider education-level in column 3. Table 1.7 and Table 1.8 in the non-published appendix analyze two additional robustness checks. They assess whether the increase in private coverage is shifting either from the uninsured population or from the Medicaid enrollees. Both Tables 1.7 and 1.8 offer evidence that the combined effect of the HITC and the EITC from 1991 to 1993 was a result of the uninsured population obtaining private coverage.

Private Health Insurance	(1)	(2)	(3)	(4)	(5)
	CW (2014)	WLS	OLS	WLS	OLS
HITC alone (1985-1993 sample)	.036***				
_	(.011)				
$a_2 = \delta_4 - \frac{\delta_5}{2}$.058**	.061***	.062**	.067***
		(.027)	(.022)	(.027)	(.022)
Treat, δ_1		473***	475***	441***	445***
		(.048)	(.047)	(.045)	(.045)
$1(t=91,92,93), \delta_2$		079***	093***	090***	104***
		(.029)	(.025)	(.032)	(.028)
$1(t=94,95), \delta_3$		037	047*	050*	059**
		(.025)	(.023)	(.028)	(.026)
Treat*1(t=91,92,93), δ_4		.070*	.081***	.078**	.092***
		(.034)	(.028)	(.036)	(.028)
Treat*1(t=94,95), δ_5		.024	.038	.031	.050*
		(.030)	(.026)	(.033)	(.029)
Income	Y	Y	Y		
High school graduates	1	1	1	Y	Y
				-	-
State FE	Υ	Y	Y	Y	Υ
Observations	26,796	$6,\!495$	$6,\!495$	$6,\!495$	$6,\!495$
R-squared	.328	.395	.385	.351	.345

Table 1.4: Estimates from the Equation (2), 1989-1995

In an effort to isolate the HITC, Table 1.4 suggests that the HITC effect $(a_2 = \delta_4 - \frac{\delta_5}{2})$ is about 5.8 pp based on estimates from (2). Given the aforementioned DDD assumption, I conclude that about eighty percent (5.8/7.1) of the 7.1 pp increase in coverage of single mothers was associated with the HITC only, just as CW attributed three-quarters (3.6/4.7) of the 4.7 pp increase to it.¹⁹

¹⁹Here, I am treating the EITC-expansion effects, a_1 (1991-1993) and a_3 (1994-1995), as identical. However, as the EITC expansion in 1994 was larger than in 1991 (e.g., in 1994 the maximum EITC increased by 42.1% while in 1991 it increased by 25.1%), it might be more reasonable to relax the assumption as $a_3 \ge a_1 \ge 0$. If so, the bounds on the HITC effect, a_2 , would be $(\delta_4 - \frac{\delta_5}{2}, \delta_4)$, which is (5.8, 7.0) pp.

1.3.2 Health-Care Utilization and Health Status

Using (1) with utilization as an outcome variable, columns 1-2 in Table 1.5 show the effect of the HITC along with the EITC on the probability of an office visit.²⁰ Columns 5-6 make use of the question regarding self-reported health to construct a binary measure for the outcome variable that represents a 'good, very good, or excellent health status.²¹ To disentangle the HITC effect only, I re-estimate (2) and the corresponding results can be found in columns 3-4 and 7-8.

 $^{^{20}}$ I define *utilization* as a physician's office visit at least once per year that represents the extensive margin effects on health-care demand. The primary reason for this is that the frequency of utilization could be confounded with the individual's health status, leading to biased estimates of the HITC along with the EITC effect on health-care utilization (Currie and Gruber, 1996). One way to mitigate this concern is explicitly focusing on preventive care (e.g., an office visit for a routine check-up that is recommended once a year). In other words, an absence of an office-visit per year suggests an inaccessibility to the health-care system, regardless of health status.

²¹Refer to notes under Table 1.5 for additional details.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Office	Visit			Health: g	ood/very g	ood	
	(Exten	sive Mar	rgin; %)		/excellent	t		
	Eq.(1)		Eq. (2))	Eq. (1)		Eq. (2)	
Т.	.056	.054	039	033	.061	.059	.033	.034
	(.086)	(.086)	(.076)	(.076)	(.052)	(.051)	(.049)	(.049)
1(t=91,93)	.039	.038	.053	.051	059***	063***	053***	057***
	(.043)	(.043)	(.041)	(.041)	(.020)	(.020)	(.019)	(.019)
1(t=94,95)			.007	.002			045***	049***
			(.040)	(.038)			(.027)	(.028)
T.*1(t=91,93)	.026	.026	.0003	.002	.065***	.068***	.056***	.060***
	(.049)	(.050)	(.044)	(.045)	(.021)	(.022)	(.020)	(.021)
T.*1(t=94,95)			$.077^{*}$.081*			.021	.025
			(.041)	(.040)			(.034)	(.035)
$a_2 = \delta_4 - \frac{\delta_5}{2}$			038	038			.046***	.047***
_			(.041)	(.042)			(.016)	(.017)
Income	Y		Y		Y		Y	
High school.		Υ		Υ		Υ		Υ
State FE	Υ	Υ	Υ	Υ	Y	Υ	Υ	Υ
Obs.	$3,\!668$	$3,\!668$	$4,\!563$	4,563	$3,\!668$	$3,\!668$	$4,\!563$	$4,\!563$
R-squared	.044	.038	.038	.034	.042	.047	.042	.046

Table 1.5: Health-Care Utilization & Self-Reported Health Status

Overall, Table 1.5 displays that gaining private coverage did not translate into a significant increase in office visits, but it might have improved single mothers' health status by about 4.6 pp.²² This is consistent with recent findings on whether the coverage expansion under the ACA has affected health status.²³ As the likelihood of having 'at *least one annual office visit*' is not the only dimension to measure utilization, if there were more thorough measures, it would have been helpful to see the reason for improved health status during 1991-1993.²⁴

 $^{^{22}}$ Since the economy suffered a recession in the early 1990s, it might be plausible that the general population experienced worsening health status as its consequence (Ruhm, 2000). Interestingly, Table 1.5 results indicate that the HITC might have mitigated this degrading trend.

²³After the ACA's coverage expansion, there was an improvement in self-reported health status (Sommers et al., 2015; 2017).

 $^{^{24}}$ One possible mechanism might be, as a result of the coverage, people in the treatment group were more likely to have a primary doctor.

1.4 Conclusions

For decades, disagreement over health-care policy has been a major concern, and if anything, that concern has grown with the enactment and potential repeal of the ACA. Given this, I revisit the effects of the HITC, the tax credit policy passed in the early 1990s, to provide a better evidentiary base for the usefulness of tax credits.

My estimates substantiate the findings of CW that the implementation of the HITC with the EITC increased the coverage by 7.1 pp. Also, I find that coverage was increased by 5.8 pp solely due to the HITC. These estimates are based on a data set distinct from CW, and as for isolating the HITC, a distinct strategy is constructed based on the HITC repeal and the EITC expansion. Although it was a significant increase, a magnitude of 5.8 pp shows that the HITC was not large enough to encourage everyone to be insured. Even with the HITC and the EITC in place simultaneously, the uninsured rate was still around 16.4 % in 1991-1993 (Cohen et al., 2009), implying that achieving universal health-care by only tax-credit schemes will be challenging.²⁵

Moving beyond health insurance, my results indicate that the HITC improved self-reported health. This suggests that for low-income families, the inability to acquire coverage might be the primary barrier for improved health status. Considering all findings of the HITC, *making tax credits available for low-income families*, a relatively inexpensive mechanism to implement, can be effective to provide incentives for coverage take-up and to achieve better health (albeit, if appropriate regulations were enforced). With that said, the ultimate repeal of the HITC validates the fact that it

²⁵A possible explanation for this small magnitude is that the modest amount of tax credits, relative to high premiums, was insufficient to incentivize eligible individuals to enroll in those benefits (GAO, 1994). The other possible explanations would be insufficient outreach and lack of publicity regarding the HITC. This problem is also documented with the ACA exchanges, as about half a million fewer people signed up for insurance in 2017. This number would have been higher with an equal level of publicity under the Obama Administration.

could have been more effective if the appropriate regulations had been in place.²⁶ As the U.S. is at the cusp of policy reforms, this paper has provided timely insights into the various aspects of tax credits.

²⁶Even when the market was mostly unregulated, the HITC still resulted in an increase of the coverage and improved health status in 1991-1993. However, the HITC did not specify minimum benefits that must be included in insurance plans (as in a recent replacement proposal—AHCH includes a clause about removing 10 Essential Health Benefit mandates), enabling insurance companies to sell valueless plans to tax-credit eligible candidates (Sanger-Katz, 2017). This ultimately resulted in its repeal despite the relative success.

APPENDIX

Event history analyses for the 1991-1993 policy regime

This table below offers additional evidence for Table 1.2 by showing that there was no significantly different pre-trend in 1990. This also shows that the estimated effect of the HITC along with the EITC had the similar magnitude in all three years, 1991, 1992 and 1993.

Private Health Insurance coverage	(1)	(2)
	WLS	OLS
Treat	477***	473***
	(.091)	(.089)
Treat* $I(t=89)$	-	-
Treat* $I(t=90)$.032	.025
	(.050)	(.046)
Treat* $I(t=91)$.095	.109*
	(.065)	(.062)
Treat* $I(t=92)$.112**	.124**
	(.047)	(.045)
Treat* $I(t=93)$.068	.076*
	(.051)	(.044)
State FE	Υ	Υ
Observations	4,722	4,722
R-squared	.390	.338

Table A.1: Event History Analyses, 1989-1993

Additional robustness-check for uninsured coverage rates

This section answers the question that may arise whether or not an increase in private coverage in 1991-1993 was from the uninsured population. To do this, I re-estimate equation (1) in the main text by replacing the outcome variable with an indicator for having neither private nor public coverage. The results below display that the effect on un-insured rates (-7.3 pp) was about the same size (with a negative sign) as the effect on private coverage in Table 1.2 (7.1 pp). This supports the idea that the increase in private health insurance coverage from 1991 to 1993 mostly came from the uninsured population.

Uninsured	(1)	(2)
	WLS	OLS
Treat	105**	124**
	(.049)	(.050)
I(t=91,92,93)	.039	.041
	(.034)	(.031)
$T_{reat} * I(t - 01 02 03)$	- 073**	- 060*
1(0-51,52,55)	(0.01)	(030)
	(.001)	(.030)
State FE	Y	Υ
Observations	4,722	4,722
R-squared	.142	.142

Table A.2: Change in Uninsured Coverage Rates, 1989-1993

Additional Robustness Check for Change in Coverage among Medicaid Enrollees

Due to the concern that private health insurance coverage might have arisen from Medicaid enrollees switching their coverage, I use an additional individual fixed effects (FE) estimation strategy with the following equation:

$$y_{ist} = \beta_0 + \beta_1 * Treat_{ist} * 1(t = 91, 92)_t + \beta_2 * Z_{ist} + \epsilon_{it}$$
(3)

By taking advantage of the panel nature in the SIPP, I track the changes in coverage among Medicaid enrollees at the beginning of their interviews. Through the SIPP-1990 Panel which covers 1990 to 1992, I try to get a conclusive result whether the Medicaid enrollees changed to private health insurance in 1991 and 1992. In the above equation, the estimate of β_1 is my primary interest. Corresponding results in Table 1.8 do not have any statistical significance. This suggests that the possibility of being overstated due to the change in coverage among Medicaid enrollees would not be an issue.

Private Health Insurance Coverage	(1)	(2)
$Treat^*(t=91,92)$	071	042
	(.063)	(.047)
(t=91)	.051	.022
	(.059)	(.043)
(t=92)	.056	.028
	(.064)	(.052)
Observations	742	742
R-squared	.083	.088
Number of Individuals	106	106

Table A.3: Private Coverage among Medicaid Enrollees, 1990-1992

Outcome	1989	1990	1991	1992	1993	1994	1995	Total
Single mothers								
Insurance that	.148	.139	.133	.122	.128	.115	.114	
covers children	(.355)	(.346)	(.340)	(.327)	(.334)	(.320)	(.318)	
Office Visit (Any)	.640	.670	.655	-	.670	.711	-	
	(.480)	(.469)	(.475)	-	(.470)	(.453)	-	
Health:good/very-good	.922	.919	.919	-	.926	.894	-	
excellent	(.267)	(.273)	(.273)	-	(.260)	(.307)	-	
Obs.	439	889	458	672	601	579	578	4216
Single Women								
Insurance	.700	.669	.608	.570	.628	.642	.633	
	(.459)	(.470)	(.489)	(.495)	(.484)	(.480)	(.482)	
Office Visit (Any)	.695	.726	.677	-	.719	.686	-	
	(.461)	(.446)	(.468)	-	(.449)	(.464)	-	
Health:good/very-good	.950	.927	.891	-	.917	.901	-	
excellent	(.218)	(.259)	(.311)	-	(.275)	(.297)	-	
Obs.	220	457	276	382	328	316	330	2279

 Table A.4: Coverage Rates, Office Visit and Health-Status Report

Chapter 2. The Effects of the Dependent Health Insurance Coverage Mandates on Fathers' Job Mobility and Compensation

2.1 Introduction

Historically, young adults aged 19–25 experienced lower health insurance coverage rates than other groups. The main reasons for this might be that (1) young adults are generally healthy so they may perceive less need for health insurance, and (2) they often work in entry-level jobs that are less likely to provide health insurance (Barkowski and McLaughlin, 2018). The alternative to employer-provided health insurance (EPHI) is enrollment in a non-group plan, which can be too expensive for young adults given their generally lower income compared to that of other working-age groups. Because of these factors, young adults may forego purchasing health insurance. Seeking to increase health insurance for this young adult population, both state and federal policymakers mandated that health insurance companies expand the age that children could remain covered under their parents' health insurance.

Although many studies find positive effects of these mandates on young adults' health insurance coverage rates (Levine et al., 2011; Dillender, 2014; Cantor et al., 2012; Akosa Antwi et al., 2013), the literature lacks studies detailing the implications for the parents of those young adults. Because the mandates increased the value of jobs with EPHI for parents who had eligible children, parents' job mobility could be constrained. Understanding such potential effects is important because middle-aged workers (aged 45–64) are in the prime earning years of their careers.

Specifically, the mandates might limit the job choices for parents because they could provide a more comprehensive and relatively cheaper insurance plan as a valuable safeguard for their adult children's health and financial security while promoting their children's career progression.²⁷ Thus, the mandates might increase the parents' cost of leaving an employer with EPHI compared to the time period prior to the implementation of the mandates. I, therefore, expect that workers could be less likely to leave their jobs when their children were eligible for dependent coverage mandates. Conversely, for parents without EPHI, these mandates made their then-current state of employment less attractive; as such, I expect that these people could be more inclined to pursue jobs with EPHI. Consequently, this research addresses the extent to which the state and federal dependent health insurance mandates caused fathers to experience job-lock and job-push, that is, respectively, remaining in their jobs for fear of losing EPHI and seeking out jobs with EPHI that they would otherwise not have chosen.

In addition to highlighting the unintended consequences that accompanied the mandates, this paper further contributes to the literature in two important ways. First, this paper exploits the state and federal mandates together, as suggested by Barkowski and McLaughlin (2018). Examining state and federal mandates in tandem is important because they had a shared primary objective of increasing coverage among young adults and had similarities in the eligibility criteria. Second, this research uses a comprehensive dataset (the combination of survey and administrative data) to examine whether

²⁷Given that health insurance enrollment decisions in the United States were often made at the immediatefamily level as opposed to the individual level (Cutler and Gruber, 1996), covering dependents through parental coverage is a cost effective decision. Brandeisky (2015) shows that, in 2015, an individual premium cost an average of \$486 a month for young adults. By adding two or more dependents to the parents' plan, however, a health insurance premium cost an average of \$1,377 a month, thus lowering the cost per individual.

the implementation of the mandates caused a decrease in annual earnings or other types of compensation. This decrease could occur because the mandates increased the relative costs for employers to hire those parents with eligible adult children.²⁸

I observe that working fathers with eligible children experienced a 42 percent decrease in the likelihood of voluntary separation from employers providing EPHI. My results also provide weak evidence suggesting that fathers who decided not to separate from such employers in the current wave could experience a modest reduction in earnings.²⁹ Taken together, all of these findings about the potential effects on parents would allow for a holistic understanding of the mandates' effects.

The following section explains the institutional details of the dependent coverage mandates. Section 3 presents the literature review and Section 4 describes the methods. Section 5 discusses my results, and Section 6 concludes this paper.

2.2 Institutional Details

Before the dependent coverage mandates required insurance providers to extend the age limit for dependents, most public health plans (e.g., Medicaid and CHIP) and private health plans (e.g., self-insured EPHI, EPHI through an insurance company, or plans through the non-group market) removed young adults from their parents' polices.³⁰ This most commonly occurred when they turned 19 unless they were enrolled in a college or university as a full-time student. If a dependent was a full-time student, then he or she was typically covered through the age of 22. This left many young adults

 $^{^{28}}$ In 2012, the average employer contribution for employees' family plans was about 73 percent or about \$ 11,429 (The Kaiser Family Foundation, 2017). As the dependent children were covered under the family plan, the financial burden of the insurance premium and health care cost could be transferred not only to the parents, but also to their employers (Chen, 2018).

²⁹As explained further in the data section, each wave is a four-month period.

³⁰The dependent, in this case, referred to biological or legally fostered children.

uninsured if they were not currently attending college. Moreover, in some states, the tax code defined coverage of dependents (19 years of age or older) as a taxable benefit, deterring parents' employers from extending coverage to their adult children. These factors contributed to 31 percent of young adults being uninsured in 2009, which equals approximately 9.2 million people between the ages of 19 and 25 (Busch et al., 2014).

To increase health insurance coverage for this young adult population, state policymakers expanded access to dependent coverage. In the absence of state funds to expand public programs, many states required firms to offer dependent coverage as part of their plans for increasing the age threshold, generally up to 23–25 years of age (Goda et al., 2016). By 2010, 30 state-level dependent coverage expansions were in effect (see Table 2.9).³¹ Because of the state-level mandates, the dependent coverage rate increased by approximately 11.9 percent (Burgdorf, 2014; Monheit et al., 2011).

Following the states' lead, the federal government enacted the dependent coverage expansion through the 2010 Affordable Care Act (ACA), which required insurers to expand coverage to children up to the age of 25 on their parents' plans. Whereas some state mandates limited eligibility based on factors other than age, the federal law was straightforward: any insurance plan that already offered dependent coverage must offer the same level of benefits at the same price to dependents 25 years of age or younger.³² Furman and Fiedler (2015) find that due to the federal mandate, the

³¹While almost all states with state-level mandates expanded their eligibility to 23–25 years of age, some states extended the provision to age 29 (i.e., New York, New Jersey and Pennsylvania) and other states extended an indefinite age of eligibility (i.e., Iowa and Texas). Some states also required student status, single marital status or financial dependency to be qualified as a dependent. Beyond the differences in eligibility among dependents, the parents with EPHI from self-insured firms were exempt from the state mandates under the Employee Retirement Income Security Act of 1974. Lastly, most states did not regulate the employee-paid premiums that could be levied for coverage of older dependents, potentially allowing firms to raise prices above what employees could afford.

³²The federal mandate did not depend on co-residence with parents, student status, marital status or financial dependency. It applied to all insurance plans including self-insured EPHI, fully-insured EPHI and plans from the non-group market.

uninsured rate among young adults dropped by more than 40 percent from 2009 to 2014, which translates to 4.5 million additional young adults with coverage.³³ Cantor et al. (2012) note that the federal mandate was a "rare public policy success in the effort to cover the uninsured [young adults]."³⁴

2.3 Literature Review

A large body of empirical literature exists regarding job-lock. The majority of empirical and anecdotal evidence suggests that mobility constraints in the labor market stem from the fear of losing health care coverage.³⁵. For instance, Rashad and Sarpong (2008) find that individuals with EPHI were 60 percent less likely to voluntarily leave their jobs compared to those receiving insurance elsewhere. Most job-lock studies rely on the idea that a worker's demographic characteristics—such as proximity to retirement or health status—might lead him or her to value insurance more highly than others, making that worker more vulnerable to job-lock (Kapur and Rogowski, 2007; Blau and Gilleskie, 2001; Bradley et al., 2005). Compared to the abundant literature about job-lock, fewer researchers study job-push, and one example of such studies suggests that EPHI encouraged some workers to leave jobs that are otherwise desirable (Anderson, 1997).

There is a considerable amount of literature about the state and federal mandates that focus on decreasing uninsured rates and job-lock for young adults (Levine et

³³These gains alleviated more than two-thirds of the gap in uninsured rates between young adults and other non-elderly adults (Furman and Fiedler, 2015).

³⁴There is one caveat that is worth noting: this coverage extension often did not work well for young adults living out-of-state because their parents' plan might only provide expensive, out-of-network coverage (Goldman, 2013; Reinicke, 2018).

³⁵Some findings in the literature, however, suggest that there is little evidence of job-lock phenomenon (Gilleskie and Lutz, 2002; Kapur, 1998)

al., 2011; Monheit et al., 2011; Cantor et al., 2012; Antwi et al., 2013; Sommers et al., 2013; Colman and Dave 2017; Kofoed and Fraiser, 2019). Most papers in the literature, however, study state- and federal-level mandates independently. For example, Antwi et al. (2013) and Cantor et al. (2012) only discuss the federal mandate; they justify omitting the effects of the state mandates by arguing these effects were negligible. Yet, including the effects of the state mandates is necessary given the impacts of both the state and federal mandates on various outcomes such as young adults' insurance rates, marriage rates and educational attainment (Barkowski and McLaughlin, 2018; Gamino, 2018; Barkowski et al., 2018).

Despite the plethora of papers regarding the effects of the dependent coverage mandates on young adults, few researchers consider other populations. There is only one paper that studies the effects of the dependent coverage mandates on parents' retirement decisions (Biehl et al., 2018), but this paper relies solely on the federal mandate for identifying variation by using the Health and Retirement Study (HRS) data. It is also limited by only considering retirement decisions without analyzing other types of voluntary job separation.

Even though little prior work explicitly investigates the link between the dependent coverage mandates and parents, there is some evidence that mandates related to child health insurance affected parents' voluntary job separation. For instance, Chatterji et al. (2016) find that the prohibition of the pre-existing condition exclusions for children increased the likelihood of leaving an employer voluntarily by 37 percent among fathers of disabled children relative to fathers of healthy children. Barkowski (2017) also finds that Medicaid eligibility for household members (especially for eligible children) increased the probability of a voluntary job separation by 34 percent among working fathers with EPHI. Hamersma and Kim (2009) find that Medicaid decreased job-push, suggesting that unemployed fathers or working fathers without EPHI felt less need to move to jobs that offer insurance. As a caveat, Barkowski (2017) and Hamersma and Kim (2009) focus on low-income workers, and Chatterji et al. (2016) investigate job mobility of parents with disabled children. Since both studied groups might be systematically different from the general group of middle-aged fathers, a more comprehensive approach for this group merits discussion.

In addition to job mobility, several papers examine whether health-benefit related mandates influenced eligible workers' annual earnings or other types of compensation as these mandates raised the relative cost for firms to insure their workers. This exploration is critical since the efficiency of these mandates largely depended on the extent to which their costs were shifted to group-specific wages (Gruber, 1994).³⁶ In spite of several regulations (e.g., non-discrimination laws) that prevented these shifts, Gruber (1994) finds a substantial shift in the costs of the group-specific mandate to the wages of the targeted group. This study inspired subsequent articles seeking to determine the effects of health insurance mandates on earnings.³⁷ Alternatively, coworkers might share the cost of providing additional insurance. A study by Goda et al. (2016) suggests that the wage reduction of employees (including those who did not benefit from the mandates) could range from \$30 to \$1,500 per worker depending on

³⁶In examining the efficiency of group-specific mandates, a central consideration is whether the cost of the mandate was shifted to the wages of the group that benefited. Without the ability to adjust wages accordingly, there might be substantial deadweight loss from these mandates even if the benefit was valued by the group (Gruber, 1994).

³⁷Monheit and Rizzo (2007) review the relevant literature regarding the costs of various mandates for employees and employers.
the number of coworkers sharing the costs.³⁸

2.4 Methods

2.4.1 Data

In my analysis, I include married fathers between the ages of 45 and 64 with a youngest child between the ages of 19 and 29, but I exclude fathers with a youngest child aged 26—the cutoff age for the federal mandate. This is because the adult children's exact dates of birth are unknown and it is unclear whether they were treated by the mandates.³⁹ Moreover, this sample excludes responses from states that had no age limit and states that extended the provision to age 29. Among family units, I only focus on fathers because they have more persistent attachment to their jobs than mothers. That being said, the wage-labor supply elasticity of fathers is often much smaller than that of mothers (Blundell and MaCurdy, 1999).⁴⁰ Finally, one of my sample selection criteria is based on the youngest child because fathers have an incentive to secure health insurance for their children until their youngest child acquires his or her own access to health insurance.⁴¹

To assess the effects of the dependent health insurance coverage mandates, I

 $^{^{38}}$ Despite the wage reduction caused by the dependent coverage mandates, Goda et al. (2016) do not find any evidence that suggests workers reduced their labor supply in response to lowered wages.

 $^{^{39}}$ I can only observe the year when the youngest child was born. I, however, do not omit fathers with children who were at the cutoff age for the state mandates (generally less than or equal to 25) because they received coverage through the federal mandate in later years. Additionally, I do not extend the child's age past 30 because adult children over 30 are systematically different from adult children in their early 20s in terms of life stage.

⁴⁰In Appendix 4, I do the same analysis for mothers but do not find any significant changes in their labor market outcomes caused by the mandates. This may be attributable to mothers' expectation that they would not remain in their jobs for a long time. If true, this would suggest that they placed less value on the benefits provided by their companies and were less likely to be influenced by the mandates related to their employer-provided benefits.

⁴¹I use the youngest child to construct my samples, including the sample for job-push analysis. For the job-push analysis, however, any child could affect fathers' job mobility decisions. So I also run the job-push analysis based on the oldest child. This does not change my results.

leverage detailed information on individuals using the 2004 and 2008 Survey of Income and Program Participation (SIPP) panels, which are linked to the Detailed Earnings Records (DER) and Business Registrar (BR) data. The SIPP is a nationally representative household survey. The time period covered in my data is January 2004 to December 2012—when most state-level dependent coverage provisions and the federal mandate were implemented. The entire sample is divided into four subsamples called rotation groups. One rotation group is interviewed every 4 months, which hereafter is called a wave. Most SIPP questions ask the respondent to report information regarding the four months prior to the interview (United States Census Bureau, 2001).

I use the respondents' health insurance, demographic characteristics and employment records from the core questions of every SIPP wave.⁴² The SIPP provides a detailed set of information about current employment for up to two jobs in a given wave. I only include the job that is considered the 'primary job'—the job in which the individual worked the most hours. The data provide the main reasons that fathers left employers within this wave, if applicable, which allows me to separate voluntary versus involuntary job separation. In my analysis, I focus on voluntary job separation—transitioning between jobs, becoming unemployed, leaving the labor force or transitioning from working for an employer to self-employment.⁴³

Although the SIPP provides detailed, self-reported demographic characteristics, the linked dataset between the SIPP and administrative records on earnings—DER and BR—provides highly accurate measures of earnings and total monetary compen-

 $^{^{42}\}mathrm{None}$ of the variables that I use are imputed.

⁴³Involuntary job separation includes layoffs, childcare problems, family/personal obligations, illness/injury, school/training, employer bankruptcy/change in ownership, termination of a temporary job, and unsatisfactory work conditions.

sation. To construct this combined dataset, I first link the respondents' information from the SIPP to the DER, which includes their W-2 information such as wages and employer contributions to retirement benefits.⁴⁴ This SIPP-DER linked data can be extended with BR data based on the EIN information available in the DER. The BR data includes information like type of firms (i.e., single-unit or multi-unit) and the parent company for all companies in the United States.⁴⁵ The SIPP-DER-BR data allows me to identify whether some respondents with two or more W-2s worked for the same parent company and to calculate the total compensation from their primary jobs. In my analysis, about 49.8 percent of the sample had two or more W-2s on file. Of these respondents, about 20 percent had W-2s from the same parent companies. Linking SIPP data with the administrative data, therefore, enables a comprehensive understanding of the compensation adjustments caused by the mandates.⁴⁶ Earnings and other monetary compensation are inflation-adjusted using the yearly CPI-U indices and by defining 2012 as the base year.

Although most SIPP questions involve asking the respondent to report information for each of the four months prior to the interview month, I only include the responses from the interview month in order to mitigate seam bias.⁴⁷ This means that the analysis is conducted at the father-wave level rather than the father-month level.

To code the eligibility criteria for the mandates, I compile the data regarding ⁴⁴The SIPP-DER linkage is only available until the end of 2012 through the United States Census, so the responses for 2013 from the 2008 SIPP panel cannot be included in the analysis. While combining these

datasets, I also omit respondents who did not have Social Security Numbers.

⁴⁵Firms themselves sometimes change or have multiple EINs for tax purposes or for multiple locations.

⁴⁶Bridges et al. (2003) find substantial measurement error in SIPP wage data. They conclude that the mean SIPP wages were understated by 7.5 percent relative to the DER wages. Gottschalk and Huynh (2005) also suggest that respondents with SIPP information but without DER records had lower earnings than respondents with observed earnings in both data sets, possibly reflecting informal work arrangements.

⁴⁷The seam bias is the tendency for respondents to report higher rates of events between survey waves than within survey waves (Blank and Ruggles, 1996).

state laws (e.g., age limit and timing of implementation) from Depew (2015), Cantor et al. (2012) and the National Conference of State Legislatures (2010). I demonstrate the change in eligibility for fathers from three example states in the first three rows in Table 2.1. These states introduced state-level mandates before the federal mandate. The last row in Table 2.1 presents the change in eligibility for fathers in states without state-level mandates. I code the fathers in these states as eligible after September 2010 when the ACA was implemented.

States	Pre-S	tate Law	State Law Period From 2010			2010	
	Elig.	Inelig.	Beginning Year	Elig.	Inelig.	Elig.	Inelig.
IN		19–29	2008	19–23	24-29	19-25	27–29
Co		19–29	2006	19–24	25 - 29	19 - 25	27 - 29
\mathbf{CT}		19–29	2009	19 - 25	27 - 29	19 - 25	27–29
MI		19–29			19–29	19–25	27 - 29

Table 2.1: Examples of Childrens' Age Eligibility by State

In Table 2.2, I include the sample means for the outcome variables and covariates for job-lock and job-push in two panels. For both panels, the columns titled *Always Ineligible* contain the descriptive statistics for fathers who were not affected by the state and federal mandates. This is the intersection of the fathers whose youngest child was *ineligible* across all time periods in Table 2.1. *Ever Eligible* is the group of fathers who were affected by the mandates at some point in my analysis. This is the union of fathers, shown in Table 2.1, whose youngest child was *eligible* during any time period from 2004 to 2012. Although *Always Ineligible* fathers within my sample were generally older and were more likely to not have completed highschool, almost all other characteristics are comparable to *Ever Eligible*.

For the job-lock analysis in columns 1 and 2 of Table 2.2, I include fathers who, in the previous wave, were employed (but not self-employed) and had EPHI under their own name. The sample includes approximately 11,500 working fathers, 71 percent of whom had an *Ever Eligible* youngest child.⁴⁸ The rate of voluntary job separation within a 4-month wave, on average, is 1.7 percent. These rates are similar to those reported in several papers based on SIPP data (Barkowski, 2017; Chatterji et al., 2016). However, some other studies find the rates that differ from my rate of 1.7 percent. For example, Bansak and Raphael (2008), shows that roughly 18 percent of workers with EPHI separated from their employers within a year. This difference can be explained by the fact that they consider all separations, not just voluntary ones.⁴⁹

In columns 3 and 4 of Table 2.2, I include the fathers in the job-push sample using the same selection criteria (e.g., their and their children's ages) as those in the job-lock sample. The only important difference is that the job-push sample includes fathers who, in the previous wave, were unemployed or did not have EPHI from their employers.⁵⁰ Selecting the sample in this way limits the chance that job-push and job-lock would be conflated since individuals without EPHI would not be affected by job-lock.

The last two rows in Table 2.2 provide the average total compensation and

 $^{^{48}\}mathrm{Due}$ to the United States Census Disclosure rules, the total number of observations are rounded to the nearest 500.

⁴⁹Another difference is that I look for the job separation that happens within a wave, while Bansak and Raphael use a between-wave measure for job-mobility. Further explanation for the within and between wave measures in the SIPP can be found in Appendix A of Chatterji et al. (2016).

 $^{^{50}}$ Compared to those people in the job-lock sample in columns 1 and 2, the fathers in this job-push sample were more likely to not hold a high school diploma, less likely to work in public sectors and less likely to belong to a union. Thus, these fathers in this sample were not randomly selected.

	Job-Lock	Sample	Job-Push	Sample
Variables	Always Inel.	Ever Elig.	Always Inel.	Ever Elig.
Eligible	-	.416	-	.476
C .		(.494)		(.500)
Age	56.2	53.2	56.6	52.9
	(4.85)	(5.17)	(5.02)	(5.31)
Highschool dropouts	.048	.036	.109	.036
	(.206)	(.188)	(.307)	(.188)
Highschool graduates	.274	.262	.231	.310
	(.444)	(.442)	(.420)	(.456)
Some college or higher	.677	.702	.615	.571
	(.464)	(.459)	(.472)	(.493)
Non-hispanic white	.806	.821	.769	.762
	(.382)	(.386)	(.429)	(.441)
African American	.065	.071	.092	.119
	(.259)	(.257)	(.274)	(.292)
Hispanic	.048	.059	.062	.095
	(.225)	(.235)	(.298)	(.337)
Asian and others	.065	.054	.077	.048
	(.222)	(.224)	(.242)	(.196)
Public Sector worker	.226	.214	.031	.071
	(.424)	(.413)	(.139)	(.233)
Union worker	.258	.226	.046	.071
	(.439)	(.416)	(.195)	(.237)
Dependent Variables				
Voluntary Job-	.016	.018	.015	.014
Separation rates	(.120)	(.127)	(.131)	(.131)
N. of Observation $[1,000]$	3.10	8.40	0.65	2.10
Ln(Annual Earnings	10.9	11.0	10.1	10.3
- in the DER)	(.725)	(.785)	(1.19)	(.997)
Ln(Tot. Monetary Comp.	11.0	11.0	10.2	10.3
- in the DER)	(.744)	(.793)	(1.21)	(1.02)
N. of Observation $[1,000]$	3.00	8.20	0.55	1.90

Table 2.2: Descriptive Statistics of Fathers

annual earnings from linked SIPP-DER data.

2.4.2 Identification Strategy

This study examines the effects of both the federal- and state-level dependent coverage mandates on fathers. The primary comparison is between two groups of fathers within each state before and after the implementation of the mandates: those who had a youngest child whose age is beneath mandate thresholds and those who had a youngest child whose age is above mandate thresholds.

The model is specified as

$$y_{ijt} = \beta_0 + \beta_1 * Elig_{ijt} + \beta_2 * X_{it} + \beta_3 * time_t + \beta_4 * state_j + \epsilon_{ijt}$$

$$\tag{4}$$

where (4) is a difference-in-differences (DID) framework for individual i in state j and at time (wave) t.⁵¹

To investigate job-lock where I only include fathers with EPHI in the previous wave, the outcome variable— y_{ijt} —is set to one if a voluntary job separation happened in the current wave. Because this is a binary outcome, I use a probit analog of equation (4).⁵² Elig_{ijt} is the main independent variable and indicates whether fathers have eligible children. It is determined by three things—state of residence, year of interview and youngest child's age—and for a given year, fathers are coded as eligible if they were living in a state with a mandate in effect and had a youngest child whose age was at or beneath the mandated age. For instance, in the case of a father whose youngest child

⁵¹Because my sample consists of fathers who were at risk of leaving a job and the separation was observed at most once for each father, my specification is equivalent to a discrete time hazard model. Thus, it is not possible to include individual fixed effects because there would not be enough variation remaining (Klerman and Haider, 2004).

⁵²This also applies to job-push analysis below.

was 24 years old living in Colorado in 2006, I would code him as eligible $(Elig_{ijt}=1)$ because Colorado enacted a dependent coverage mandate at that time. For another father in Colorado in the same year but with a youngest child who was 25 years old, I would code him as ineligible $(Elig_{ijt}=0)$ because the child's age exceeded the limit of Colorado's mandate.⁵³ If fathers experienced job-lock, I would expect the coefficient of $Elig_{ijt}$, β_1 , to be negative.

 X_{it} contains other covariates including father's age and dummy variables. These dummy variables are indicators of high-school dropouts, high-school graduates, Black/Hispanic/Asian respondents, public sector workers and union workers. I include full sets of state and year indicators, denoted with $time_t$ and $state_j$, to focus on withinstate variation.⁵⁴ In addition, I incorporate indicators for all the children's ages from 20–29 (the indicator for 19-year-olds serves as the baseline group) to account for the time-invariant behavioral difference of fathers with young adult dependents of various ages.⁵⁵

As one of the specification checks, I also make use of the variation within state and year that arose due to state and federal policy changes that broadened eligibility to more age groups. I use this variation to examine whether fathers with children whose age was near but above the maximum limit were affected at the time when they should not have been. To investigate this, I define a placebo group as part of the treated group who had not been affected by the state mandates but were eventually affected

⁵³Other requirements—most importantly, student status—are inappropriate to use for eligibility imputation because they are jointly determined outcomes. For example, a state mandate might incentivize individuals to pursue or terminate student status, so using it to determine eligibility would introduce bias (Depew, 2015).

⁵⁴As a specification check, I also examine whether including a linear state time trend would affect my results.

 $^{^{55}\}mathrm{As}$ mentioned, I do not include fathers with a 26-year-old child in the sample and so leave out their age indicators in the regression.

by the federal one. To examine this, I consider the regression below.

$$y_{ijt} = \beta_0 + \beta_1 * Placebo_{ijt} + \beta_2 * Elig_{ijt} + \beta_3 * X_{it} + \beta_4 * time_t + \beta_5 * state_j + \epsilon_{ijt}$$
(5)

This equation compares the actual effect of the policies across specific age groups versus the placebo effect, thereby making use of in-state age variation. Equation (5) is developed based on the same criteria as equation (4): the child's age, year and state of residence. *Placebo_{ijt}*, is an indicator for the fathers whose youngest child was slightly older than the age eligibility of the state mandate but still younger than that of the federal mandate. ⁵⁶ *Elig_{ijt}* is defined in the same way as *Elig_{ijt}* in equation (4) and captures the actual policy change of state and federal mandates. For example, in Indiana, fathers with a youngest child aged 19–23 had been eligible under the state mandate since 2008. In this state, therefore, *Elig_{ijt}* is equal to one for those fathers with children between the ages of 24–25 from 2008 to 2010.⁵⁷ From 2010 to 2012, the duration where my analysis ends, *Placebo_{ijt}* is equal to zero across all states.

The empirical strategy I rely on to detect job-push is conceptually similar to the one I use for job-lock, demonstrated in equation (4). The main difference, however, is that it includes those fathers who did not have EPHI in the previous wave. Therefore, for the job-push analysis, y_{ijt} is equal to one if fathers voluntarily left their jobs without EPHI or indicated a change in employment status from unemployed to employed in the current wave. A positive estimate of β_1 would provide evidence of job-push, meaning

 $^{^{56}}$ My interest is not in examining the placebo effect among those fathers whose youngest child was aged 27–29 because they were not targeted by both the state and federal mandates.

⁵⁷This is because fathers were not affected by state mandates; however, they would eventually be affected by the federal mandates.

that fathers who did not have EPHI from their employers would be more likely to seek new jobs with EPHI to cover their child.

I also examine the mandates' impact on working fathers' annual earnings or total monetary compensation by using the fathers who staved in their jobs with EPHI during the current wave—a subset of the aforementioned job-lock sample. Since employers could easily identify the group of working fathers with eligible children, they might respond to the extra cost of providing dependent coverage by reducing other types of compensation for this group. To examine whether the cost of the mandate was transferred to working fathers with eligible children, in equation (4), I replace y_{iit} with the natural log of the annual earnings and total monetary compensation. I analyze total monetary compensation because employers might decrease other compensation (e.g., employer contribution toward deferred compensation) instead of directly adjusting eligible fathers' earnings to avoid violating non-discrimination laws (Anand, 2017). I define this total monetary compensation as the sum of annual earnings and deferred compensation. Because my outcome variable is not binary in this analysis, I run a linear regression instead of probit analog of equation (4). If there was any compensation reduction for those working fathers whose youngest child was eligible for the mandates, I would expect a negative estimate of β_1 . An important note in this analysis is that a small number of responses are automatically omitted when zero compensation was reported.⁵⁸

⁵⁸This omission of responses occurs because I use the natural log of compensation for the dependent variable. Thus, I omit the fathers who self-reported that they were employed with EPHI in the SIPP—a primary sample selection criteria for job-lock analysis—but demonstrated zero earnings in the DER. Earnings might be absent from the DER for some working fathers because their employers failed to report the employees' wages to the Social Security Administration. These workers without DER data were more likely to work in private households, construction, agriculture and informal occupations (e.g., street and door-to-door sales work, dancing or bartending) (Roemer, 2002).

2.5 Results

2.5.1 Job-Lock

Table 2.3 shows the evidence for an increase in job-lock among working fathers due to the dependent coverage mandates. After the mandates took effect, the average probability of leaving an employer for any voluntary reason was 0.8 percentage points lower for working fathers with eligible children than for other fathers. This 0.8 percentage point decrease is a 42 percent decrease in voluntary job separation given that the average separation rate of *ever eligible* working fathers before the implementation was approximately 1.9 percent.⁵⁹ Column 1 does not include any control variables, and column 2, the preferred estimate, incorporates all covariates. The last column provides results with control variables and the state time trends. ⁶⁰

The magnitude of my results in Table 2.3 is comparable to the effects of similar child-targeted mandates on the mobility decisions of working fathers. Barkowski (2017) finds that Medicaid eligibility for one household member resulted in a 34 percent increase in the likelihood of a voluntary job separation among working member(s) in the household. Chatterji et al. (2016) also demonstrate that the ACA prohibition on pre-existing condition exclusions increased the probability of job separation by 35 percent for married fathers with disabled children compared to fathers with healthy children.⁶¹

⁵⁹Even when I run a logit regression, I observe similar effect sizes (see Appendix Table 2.10).

⁶⁰In Tables 2.4, 2.5, 2.6, 2.8 and 2.11, I only report the results with covariates that do not have state time trends as in column 2 of Table 2.3.

⁶¹Chatterji et al. (2016) and Barkowski (2017) focus on whether parents' reliance on employment for health insurance decreased; I examine whether it increased. The primary idea, however, remains the same: health insurance mandates for children could affect fathers' labor market decisions.

Voluntary Job Separation	[1]	[2]	[3]
Eligible	007†	008*	009*
Covariates	(.004)	(.003) Y	(.003) Y
State Time Trends			Y
N. of Observations [1,000]	11.5	11.5	11.5
Dependent variable means Ever eligible, before Mandate	.019	.019	.019

Table 2.3: Job-Lock

In Table 2.4, I examine the robustness of the results. To compare these estimates with the main results, column 1 is taken directly from column 2 of Table 2.3.⁶² In column 2, I expand the control group by including working fathers whose children were aged 27–33 to see if the result would vary depending on the age range of the children. In column 3, I also examine whether expanding the time period with the 2001 SIPP panel would alter the results. Here, I omit five states (i.e., Wyoming, Vermont, Maine, South Dakota and North Dakota) because they were sampled together in the 2001 SIPP. As I am unable to verify the exact implementation dates for the mandates in Georgia, Nevada, South Carolina and Wyoming with more than one source (Goda et al., 2016, I exclude fathers from these states in column 4. In column 5, I exclude the state mandates that had student status requirements (i.e., Florida, Idaho, Louisiana, Massachusetts, North Dakota, Rhode Island and South Dakota).⁶³ In column 6, I compare the real and the placebo effects, and the resulting estimate of placebo is

 $^{^{62}\}mathrm{I}$ also repeat the results of Table 2.3 (column 2) in Tables 2.5 and 2.6 for ease of comparison.

 $^{^{63}}$ As mentioned in *Institutional Details*, full-time students aged 19–22 years old before the mandates were implemented were often considered to be eligible under their parents' plans. In my main analysis, however, I assume all working fathers with children aged 19–22 as eligible **only after** the mandates were implemented. This may raise a concern whether it is valid to consider the states that required student status as mandated states because this only applied to students. I therefore examine whether excluding those states would alter the results significantly.

-0.003 with 0.009 standard error. This suggests that there was no effect of the state mandates among those fathers with children who did not meet the state-mandated age criteria. Still, this column shows that the actual policy change (which resulted in a 0.8 percentage point decrease with 0.004 standard error) is contributing to the main result as in column 1. In the last column, I exclude 2008 and 2009 responses because fathers might be affected by the Great Recession, and their labor force decisions during this time could be different compared to those from other periods. For instance, I expect that voluntary job separation would be significantly lower in 2008–2009 because the overall availability of alternative jobs would be lower. Except for column 7, which has a -0.011 estimated coefficient, almost all results in this table have a similar magnitude of around a 0.8 percentage point decrease with statistical significance in column 1.⁶⁴

In Table 2.5, I examine the heterogeneity of the results. Working fathers with lower education might be less responsive to the mandates because of a lack of understanding of the dependent coverage mandates. On the other hand, the working fathers might be more likely to have children who needed the dependent health coverage because their children would generally be less educated and less likely to secure jobs with EPHI. Column 2 shows the effects on working fathers who did not receive a Bachelor's degree, and column 3 shows the effects on working fathers who completed a four-year degree or above. The estimated coefficient on $Elig_{ijt}$ is -0.007 with a standard error 0.003 in column 2. This suggests that fathers with less education had less job mobility as a result of the mandates, supporting the latter hypothesis. Columns 4 and 5 show that fathers whose wives did not have EPHI would be more likely to experience

⁶⁴Though the result in column 5 is statistically insignificant, it has a p-value of around 0.13.

Voluntary Job Sep.	[1] Preferred Table 3 [2]	[2]	[3]	[4]	[5]	[6]	[7]
Eligible	008* (.003)	007* (.003)	007* (.003)	008* (.004)	006 (.004)	008* (.004) 003	011** (.004)
						(.009)	
Child Aged 19-33		Y					
Including 2001 SIPP			Υ				
w/o States-Unclear Dates				Υ			
w/o States-Student-Status					Y		
Equation (5)						Υ	
$\rm w/o~2008$ and 2009							Y
N. of Observations [1,000]	11.5	15.5	14.5	11.0	10.0	11.5	9.6

Table 2.4: Robustness Checks

job-lock because they were the only source of health insurance for the household. The estimated coefficient in column 4 is -0.009 with a standard error of 0.004, reflecting less job mobility for fathers with wives lacking EPHI after the mandates.

Table 2.3	5: Het	cerogeneity	Tests
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Vol. Job Sep.	[1] Preferred Table 2.3 [2]	[2] Lower Educ.	[3] Higher Educ.	[4] No Spouse HI	[5] Spouse HI
Elig.	008* (.003)	007* (.003)	006 (.009)	009* (.004)	004 (.006)
Obs. [1,000]	11.5	7.80	3.70	9.20	2.30

Table 2.6 illustrates the results of four falsification tests. In column 2, I examine whether involuntary job separation (e.g., layoff) increased due to the dependent cov-

erage mandates. I use the same sample as that in column 1, but I change my outcome variable from voluntary to involuntary job separation. As involuntary job separation could not be related to the new eligibility for the mandates, no effect would be expected. In columns 3 to 5, I investigate whether there were any contemporaneous changes that affected parents differently based on the age of their children, which could bias my results. In these columns, I consider fathers who were covered by EPHI but whose coverage was not affected by the mandates (due to the ineligible ages of their children) and examine if their behaviors changed when the mandates were implemented. Therefore, my sample in columns 3-5 comprises working fathers in three groups: those whose youngest child was between the ages of 8-18, 30-40 and 27-36, respectively. In column 3, for example, I consider the placebo (state or federal) mandates' eligibility by subtracting 11 from the original age eligibility criteria based on fathers whose youngest child was aged 8–18. If the mandate expanded coverage to dependent children up to the age of 23, I would consider this state's placebo age limit to be 12. By doing this, I can examine whether those working fathers with ineligible children under 19 seemed to be affected by the mandates. I repeat this process for working fathers with children aged 30 to 40 in column 4. For these working fathers, I add 11 to the original age eligibility. That being said, if the mandate increased the age limit to 23, I would consider this state's placebo age limit to be 34).

One drawback of the two falsification tests shown in columns 3 and 4 is that they do not include any fathers who are in my main sample. To rectify this, I use the sample containing working fathers whose youngest child aged 27–36 in column 5 of Table 2.6. With this analysis, I can examine whether there were any time-effects. This refers to circumstances that had changed over time and affected parents differently based on the ages of their children. In this column, I include the same *Always Ineligible* fathers whose youngest child was aged 27–29 that I use in my main analysis. However, I alter the *Ever Eligible* group—whose children were aged from 19–25—by adding 11 to the original age limit, resulting in an overall sample of fathers with 27- to 36-year-old children. None of the falsification tests have any significant effects, and the point estimates are appreciably different from my main findings in column 1 of Table 2.6.

	[1] Pref.	[2] Invol. Sep.	[3] Elig. Age-11	[4] Vol. Job Sep. Elig. Age+11	[5] Elig. Age+11 for 19-25
Eligible	008* (.003)	002 (.003)	.003 (.004)	003 (.007)	.006 (.007)
Observations [1,000]	11.5	11.5	16.5	6.50	8.00

 Table 2.6:
 Falsification Tests

2.5.2 Job-Push

Fathers may not only experience job-lock due to these mandates, but also the twin phenomenon of job-push. In Table 2.7, I examine whether the fathers without EPHI were incentivized to leave their jobs due to the increase in opportunity costs of staying in their employment status. Unlike Hamersma and Kim (2009) and Barkowski (2017) who study the Medicaid expansions, my 1 percentage point estimate of increased voluntary job separation of the eligible fathers is not significant at a conventional level. As such, I do not find evidence of job-push for these fathers.

One explanation is the time horizon in which parents were affected by the policies. While Medicaid influenced parents for a long period of time, the dependent coverage mandates only affected parents for a shorter period when their children were in their early 20s. Parents, therefore, might be less motivated to change their employment status (in this case, finding a new job with EPHI) for such a short-term benefit. Additionally, parents who greatly valued insurance for their kids probably would have moved to such jobs already. Because I only use the sample that consists of fathers who did not have EPHI through their jobs, this might mean that they had a lower demonstrated need and value of health insurance.

Voluntary Job Separation	[1]	[2]	[3]
Eligible	.012	.012	.022
Covariates	(.022)	(.020) Y	(.026) Y
State Time Trends			Y
N. of Observations [1,000]	2.80	2.80	2.80

Table 2.7: Job-Push

2.5.3 Reduction in Compensation

	[1]	[2]	[3]
	ln(Earnings)	ln(Tot. Comp.)	ln(Earnings)
	SIPP-	DER-BR	Public SIPP
Eligible	079	091†	002
	(.054)	(.054)	(.041)
N. of Observations [1,000]	11.0	11.0	11.0

 Table 2.8: Annual Earnings and Total Monetary Compensation 1

To examine whether employers adjusted employee compensation in response to rising health insurance costs, I analyze the change in total monetary compensation and annual earnings. Table 2.8 presents the effects of this mandate on earnings and other compensation for working fathers who did not leave their jobs with EPHI, based on both the administrative data and the public data. Column 1 presents no evidence for a reduction in annual earnings while column 2 shows there is weak evidence of decline in total compensation by about 9 percent.⁶⁵ This means that employers might try

 $^{^{65}}$ The results, however, should be interpreted with caution. As mentioned, non-discrimination laws might have prevented employers from differentially compensating employees. In addition, all workers might have

to adjust other monetary compensation instead of directly decreasing the earnings for those fathers with EPHI. Through this examination, I observe that fathers with EPHI not only experienced job-lock, but also could experience compensation reduction. Said another way, because the average total compensation of treatment group was about \$70,000 before the implementation of the mandates, a weakly supported 9 percent decrease in total compensation could result in a \$6,300 decrease among fathers with eligible children after the mandates.⁶⁶

To compare the results in columns 1-2 with the result based on the public data, I include column 3 of Table 2.8.⁶⁷ Unlike the results from the administrative data, I do not find any significant effect on compensation by using the public SIPP data alone.⁶⁸

2.6 Conclusions

While both the state and federal dependent coverage provisions successfully increased health insurance rates among young adults, previous research did not make it clear whether the mandates had any effects on their parents. To address this gap, I analyze the effects of both the state and federal mandates on fathers' dependence on employment. My investigation is unique in the literature because I bring together three important analytic features: (1) a focus on fathers (whom are not targeted by the mandates) whose responsiveness is the key determinant for the effectiveness of the mandates; (2) a usage of both the state and federal mandates to achieve more credible

borne the cost of the mandate since many non-parents were potential future users of the policy and it would have been difficult for firms to implement wage offsets when workers became parents.

⁶⁶To examine whether including fathers with zero compensation changes my results, I also add one to both dependent variables for those who had zero compensation and examine the same analysis again in Table 2.11. This result shows the consistent decrease in compensation, as shown in Table 2.8.

⁶⁷Although the earnings reported in the public SIPP are monthly, I aggregate them into annual earnings for each father and use this as an outcome measure for column 3.

⁶⁸Due to data unavailability, I do not examine the effect of the mandates on total monetary compensation from the public SIPP data.

variation; and (3) a usage of administrative data on the earnings measure that have not been used in the relevant literature.

I find that the mandates decreased voluntary job separation by about 0.8 percentage points among eligible working fathers (aged 45–64, with EPHI) than would otherwise have been. This decrease in voluntary job separation represents that, on average, 0.8 percentage points of more fathers would stay in their jobs for each wave than prior to the implementation of the mandates. As such, after one year—comprising three waves—2.4 percentage points more fathers would remain in their current jobs providing EPHI.⁶⁹ If I assume that each father covers one child, my model suggests that the mandates could increase the young adult dependents who have health insurance coverage by at least 2.4 percentage points. This approximation of increased coverage among young adults aligns with another paper that suggests an increase of coverage among adult dependents about 5.3 percentage points during 2010–2011 (Cantor et. al., 2012). My estimate, the 2.4 percentage points increase in coverage, is relatively smaller compared to the 5.3 percentage points and is plausible given that not all fathere considered a job change when they started to cover their newly eligible young adult dependents. While covering a more broadly defined population, my observation that the child-targeted mandates could affect parents' mobility is consistent with previous findings (Bansak and Raphael, 2008; Chatterji et al., 2016; Hamersma and Kim, 2009). Additionally, my estimates are robust to a variety of specification checks, although some effects have a change in magnitude and lose statistical significance. I discover no evidence of job-push, and I find weak evidence of compensation reduction

⁶⁹This conversion between wave and year is necessary for my comparison because I report flow changes for job mobility, while other papers report change in dependent coverage rates in stocks.

among eligible fathers. My paper provides new insights into the effectiveness of the dependent coverage mandates and emphasizes how health insurance access can have far-reaching consequences for both targeted individuals and their household members.

Since many other insurance-related changes were implemented in 2014 (e.g., premium subsidies for private coverage, the Medicaid expansion and individual mandate), it is important to understand the ways in which the dependent coverage mandates might be affected. For example, people with incomes below 133 percent of the federal poverty level (FPL) could qualify for expanded adult Medicaid while those who have incomes from 133–400 percent of FPL could qualify for premium subsidies for private insurance. Therefore, these people might have chosen an expanded public coverage or newly subsidized form of coverage after 2014. My analysis of the dependent coverage mandates from 2004 to 2012 implies that these two policies could have decreased the EPHI-related constraints that both young adults and their parents faced. Additionally, the repeal of the individual mandate—effective beginning in 2019—could also contribute to further fluctuation in the rates of dependent coverage and the job mobility of fathers. Due to enforcement of the individual mandate in 2014, young adults' health insurance take-up could increase, while simultaneously decreasing job mobility among fathers. The opposite can happen after 2019. With these subsequent changes, future research might examine how the fathers' incentives to change jobs could be further modified.

Additionally, as there is a clear trade-off—an increase in coverage of young adults and decrease in job mobility among fathers—induced by these mandates, a more thorough welfare analysis is necessary to examine how the mandates affected the economy's overall well-being and to determine if this policy should be continued.

APPENDIX

	Full Year Implemented	Maximum age
Federal Mandate	2010	25
State Mandates		
Colorado	2006	24
Connecticut	2009	25
Delaware	2008	23
Florida*	2008	24
Idaho [*]	2008	24
Illinois	2010	25
Indiana	2008	23
Kentucky	2008	25
Louisiana*	2009	23
Maine	2007	24
Maryland	2008	24
$Massachusetts^*$	2007	25
Minnesota	2008	24
Missouri	2008	24
Montana	2008	24
New Hampshire	2007	25
New Mexico	2003	24
North Dakota*	1995	25
Rhode Island*	2007	24
South Dakota*	2005	23
Utah	1995	25
Virginia	2007	24
Washington	2009	24
West Virginia	2007	24
Wisconsin	2007	26

Table B.1: Implementation of the Dependent Coverage Laws

Voluntary Job Separation	[1]	[2]	[3]
Eligible	007†	008*	010*
	(.004)	(.004)	(.003)
Covariates		Υ	Υ
State Time Trends			Y
N. of Observations [1,000]	11.5	11.5	11.5

Table B.2: Alternative Regression Results (Logit)

Table B.3: Annual Earnings and Total Monetary Compensation 2

	$[1] \\ ln(Earnings+1)$	[2] ln(Tot. Comp.+1)
Eligible	225† (.113)	239* (.114)
N. of Observations [1,000]	11.5	11.5

Table B.4: Working Mothers' Job Mobility

Voluntary Job Separation	[1]	[2]	[3]
Eligible	.003 $(.005)$.004 $(.005)$.002 $(.005)$
Covariates	· · ·	Ŷ	Ŷ
State Time Trends			Υ
N. of Observations [1,000]	8.80	8.80	8.80

Chapter 3. Reconsidering Venti and Wise: Choice or Chance?

3.1 Introduction

Why do many households accumulate substantial wealth by retirement, while many other households accumulate very little? These are fundamental questions in economics, addressed in classic papers from Friedman (1953) to Dynan, Skinner, and Zeldes (2004), to name just a few. Moreover, this question is of fundamental importance to numerous policy questions related to optimal taxation, retirement, and the social safety net.

In companion articles, Venti and Wise (1999, 2001) directly examine the question by utilizing data that were superior to that available to previous researchers. In particular, they made use of the Health and Retirement Survey (HRS), which contained high-quality data on a wide variety of assets, demographics, health, and previous inheritances, as well as near-lifetime earnings from Social Security Administration (SSA) data. Based on these sets of data, Venti and Wise (2001) write, "Thus we conclude that the bulk of the dispersion must be attributed to differences in the amount that households choose to save." This central finding continues to be cited by researchers.⁷⁰

In this paper, we replicate one of the central analyses presented in Venti and Wise (2001), but instead use the 2001 Survey of Income and Program Participation (SIPP). One significant benefit of the SIPP over the HRS is that the SIPP provides lifetime earnings information using Internal Revenue Service (IRS) data, rather than

⁷⁰For example, relatively recent papers that have cited Venti and Wise (2001) results include Campbell (2015), Gustman and Steinmeir (2015), Cronqvist and Siegel (2017), De Nardi and Fella (2017), and Lusardi et al. (2017).

SSA data used by Venti and Wise (2001).⁷¹ The drawback to SSA earnings measure is that earnings are only recorded up to the taxable maximum. Importantly, for the time period analyzed in Venti and Wise (2001), this taxable maximum amount was low enough that over 30 percent of the HRS sample was at the maximum taxable limit during their prime earning years, potentially having led to a substantial measurement error. Such measurement error concerns are potentially problematic because measurement error in one regressor generally biases the results for all regressors in a multiple regression analysis. Cognizant of this data limitation, Venti and Wise (2001) make use of lifetime income deciles in their analysis, but it is not possible to assess the extent to which such a procedure alleviated the data problem.

With our SIPP data, we address this issue directly. We show that there remains substantial measurement error even when individuals are classified into deciles: over 30 percent of individuals are misclassified in each of the top five deciles of lifetime earnings, and over half of individuals are misclassified in the 9th decile. We then examine directly how this misclassification affects their main analysis. Our findings suggest that the correlation between lifetime earnings and savings was about 50% greater than what is found when using censored deciles, and almost double the variation of the data can be explained by lifetime earnings alone.

The rest of the paper is structured as follows. Section 2 briefly summarizes the insights from a lifecycle model of savings about wealth dispersion. Section 3 discusses Venti and Wise's (2001) analysis we replicate. Section 4 provides details of the SIPP data, and Section 5 presents our replication exercise. We conclude in Section 6.

 $^{^{71}}$ While IRS data is now available for the HRS, it was not available at the time of Venti and Wise (2001) publication. Other papers using the HRS data at the time, such as Haider and Solon (2006), similarly used the SSA data for lifetime earnings.

3.2 Background

The workhorse model for understanding the savings decision is the lifecycle model, in which households seek to maximize the expected discounted lifetime utility subject to an asset constraint.⁷² Such a model delivers the standard prediction that individuals will save to smooth consumption over time. Thus, this most basic model predicts a relationship between lifetime earnings and savings at retirement, so that consumption can be smoothed between the working years and the retirement years.

Even in the basic lifecycle model, households with identical lifetime earnings may enter retirement with different levels of savings. Specifically, the optimal level of consumption could increase or decrease over time, depending on whether the subjective discount factor is less than or greater than the interest rate. Thus, households with different subjective discount factors should choose to save differently out of lifetime earnings. In addition, the standard model has been modified along numerous other dimensions that could also lead to individuals to choose to save differentially, including differential tastes for retirement, differential bequest motives, differential levels of risk tolerance (leading to differential choices in savings vehicles), and differential expectations about health expenditures, to name just a few. Thus, while the most basic model predicts a direct relationship between lifetime earnings and savings at retirement so that consumption can be smoothed, there are numerous reasons why individuals may choose to accumulate different levels of wealth at retirement given the same level of lifetime income.

 $^{^{72}}$ See the discussion in Dynan, Skinner and Zeldes (2004) for a particularly well-targeted discussion of the lifecycle model and the accompanying literature relevant for our purposes. See Alan et al. (2015) and Bozio et al. (2017) for a recent contribution that focuses on whether the rich save more. See Benartzi and Thaler (2007) for a discussion of behavioral factors associated with savings.

The link between lifetime income and wealth at retirement would be further diminished by numerous factors that might broadly be considered as chance. The optimal savings rate is determined by one's expected lifetime resources. Shocks to these resources, perhaps income shocks related to unexpected spells of unemployment, wealth shocks related to unexpected inheritances or divorce, or expenditure shocks related to unexpected medical bills, could all affect retirement wealth.

3.3 Venti and Wise's Analysis

Venti and Wise (2001) make use of high-quality HRS data to examine the issue of choice versus chance in a straightforward, direct, and descriptive way. One of their key analyses is a series of regressions in which they successively regress wealth at retirement on lifetime earnings and various sets of regressors that are classified as being related to chance variables, investment choice variables, and taste variables. Specifically, the authors estimate the following household-level regression,

$$Wealth_i = \beta_0 + \beta_1' LifeEarn_i + \beta_2' Chance_i + \beta_3' Invest_i + \beta_4' Taste_i + \epsilon_i$$
(6)

In their specifications, $LifeEarn_i$ is measured by decile of lifetime earnings in SSA earnings, which are topcoded at the taxable maximum in each year (we describe the rest of the regression variables below). The authors explicitly note that they use deciles of lifetime earnings because of the measurement error induced by using topcoded annual earnings to calculate the lifetime earnings; in other words, the hope is that censored earnings data will be sufficient to classify individuals into the correct decile of lifetime earnings. The focus of the regression analysis is not the coefficients per se, but rather the amount of the variation in wealth that can be explained by these variables. To measure the amount of the variation that can be explained, Venti and Wise (2001) focus on the percent reduction of the root mean square error (RMSE), which is the square root of the variance of the residuals from the regression equation. For comparison, our analysis will follow suit.

The potential effect of measurement error for linear regression is well-studied and summarized succinctly in numerous reviews (e.g., Griliches, 1986; Bound, Brown and Mathiowetz, 2001). It is useful to begin with the standard results for the case when just one regressor in a multiple regression suffers from classical measurement error, which is the case when the measurement error is mean-zero and uncorrelated with the other elements of the model. Namely, the coefficient on the error-ridden regressor will be downward-biased to an extent that is related to the relative variance of the measurement error, and the coefficients on the other regressors (assumed to be measured without error) will be biased with the direction determined by the sign of their partial correlation with the error-ridden regressor. Specifically, the other regressor sthat have a positive (negative) partial-correlation with the error-ridden regressor will tend to be upward (downward) biased. See Bound, Brown, and Mathiowetz (2001) for a precise statement of this result, as well as a discussion for moving beyond this special case.

These standard results are not directly applicable (6) because lifetime earnings is not entered linearly, but rather is entered as a collection of decile indicator variables. With that said, the typically provided intuition goes through for numerous extensions: "using x_j as a proxy for x_j^* will partially, but only partially, control for the confounding effects of x_j^* on the estimates of the effect of other variables on outcomes" (Bound, et al. 2001, p. 3713). In other words, if the censored lifetime earnings deciles inadequately control for actual lifetime earnings, then the role for lifetime earnings will tend to be understated and the remaining correlation will spill over to the other regressors in the regression equation. For example, if education (a "taste variable") is positively correlated with lifetime earnings (after controlling for the other variables in the model), education should be expected to upward biased. In such circumstances, the role for lifetime earnings would be expected to be understated and the role for education would be overstated.

3.4 Data

We use wave 3 of the 2001 SIPP. The SIPP is a nationally representative household survey that collects detailed information on income, wealth, demographics, employment, and health. The 2001 SIPP can also be linked to the Detailed Earnings Records (DER) of the IRS. The DER is drawn from the Social Security Administration's Master Earnings File and includes all earnings reported on W-2 tax forms. Unlike the HRS-SSA dataset, the linked records (SIPP-DER) are not top coded at the maximum taxable SSA earnings (Bollinger et al., 2015).

While the SIPP is representative of the US population, our purpose here is to replicate an analysis using the HRS. Thus, we only include SIPP households that satisfy the primary sample selection criterion of the HRS: Households where the respondent (or spouse for married individuals) is in the age range of 51 to 61, which we refer to as "HRS-eligible households." Following the sample construction of Venti and Wise (2001), we retain households that can be matched to the administrative data, including those who have zero W-2 earnings for some years.⁷³ Additionally, the household must have completed a wealth topical module when the survey was conducted.

In Table 3.1, we provide basic descriptives from our analysis sample, as well as descriptives for all the other HRS eligible households in the SIPP (i.e., those households not matched to DER earnings histories). As can be observed, the households in our sample are positively selected in terms of education, income, and wealth. The amount of selection is greater in these data than what is reported for the HRS sample that can be matched to SSA earnings histories.⁷⁴

3.5 Results

Our central research question is the extent to which measurement error in lifetime earnings due to the use of topcoded Social Security earnings data affected Venti and Wise's (2001) conclusions. To estimate the amount of measurement error they faced, we first create a measure of lifetime earnings for our sample that imposes the amount of censorship faced in the Venti and Wise (2001) sample. To do so, we impose the same amount of censorship in our data that is reported for the HRS sample used in Haider and Solon (2006): Venti and Wise's (2001) do not report censorship, but Haider and Solon (2006) use the HRS and show censorship. We then classify individuals into deciles based on 20 years of censored earnings, to mimic the Venti and Wise (2001)

 $^{^{73}}$ One difference between our sample and that of Venti and Wise (2001) is that they additionally exclude households that reported working for any level of government for five years. The SIPP does not collect such information, so we are unable to impose this restriction.

⁷⁴In Appendix Table 3.5, we provide a direct comparison of the relative selection in the two data sets, making use of individual results reported in Venti and Wise (2001).

	Matched HRS-Eligible	Unmatched HRS-Elig.
	Households	Households
	(N=2,500)	(N=1,700)
Head Characteristics		
Age	54.4	55.5
Education	14.0	13.5
Male	60.0	55.8
Black	10.0	11.7
Hispanic	1.2	1.2
Married	56.0	64.7
Widowed/Divorced/Separated	34.0	32.4
Poor Health	12.0	17.6
Spouse Characteristics (if married)		
Number of HH with spouses	1,400	1,000
Age	53.6	53.9
Education	13.8	13.5
Male	28.0	35.0
Black	5.7	7.0
Hispanic	0.7	1.0
Poor Health	10.7	15.0
Household Characteristics		
Children	0.24	0.25
2001 Household Income (public SIPP)	61,630	$50,\!310$
2001 Wealth	498,100	298,700
Lifetime household earnings-censored	820,700	
Lifetime household earnings-uncensored	981,600	

Table 3.1: Basic Descriptives for SIPP Household Sample

variable, and based on 20 years of uncensored earnings.

	Censored Decile									
Uncensored Decile	1	2	3	4	5	6	7	8	9	10
1 (Lowest)	99	0	0	0	0	0	0	0	0	0
2	0	95	5	0	0	0	0	0	0	0
3	0	3	92	6	0	0	0	0	0	0
4	0	1	3	89	$\overline{7}$	0	0	0	0	0
5	0	1	0	4	82	14	0	0	0	0
6	0	0	0	1	8	71	19	0	0	0
7	0	0	0	0	2	8	65	24	0	0
8	0	0	0	0	1	2	10	45	42	0
9	0	0	0	0	0	2	3	16	38	40
10 (Highest)	0	0	0	0	0	2	3	14	21	60

Table 3.2: Percent of Each Uncensored Classified into Each Censored

Table 3.2 shows the extent to which the censored earnings causes there to be measurement in lifetime earnings deciles. First, unsurprisingly, there is no measurement error in the lowest decile: All individuals who were classified into the first decile indeed belonged in the first decile. Perhaps surprisingly, measurement error creeps into the measure even by the 2nd decile: 95 percent of individuals who should have been classified into the 2nd decile were classified as such based on the censored earnings data, with 4 percent incorrectly classified into the 3rd decile. The correct classification rate then becomes successively worse: 80 to 90 percent were correctly classified in the 4th through 5th deciles, 71 percent in the 6th decile, 65 percent in the 7th decile, 45 percent in the 8th decile, 38 percent in the 9th decile, and then back to 60 percent for the 10th decile. Thus, for two of deciles with the highest income, less than half of the households are classified correctly.

	Venti and Wise using the HRS	Our SIPP Analysis
Lifetime	20 years of topcoded-	20 years of W-2 earnings
	SSA earnings earnings deciles	
Wealth variables	Bequeathable wealth	Bequeathable wealth
	: 15 categories,	: 14 categories,
	imputations based on	imputations based on
	bracket info	longitudinal info
	Defined benefit pensions	Defined benefit pensions
	: included	: not available
Taste variables	Education: coding not required	Education: 5 categories
	Race/Ethnicity: info not provided	Race/Ethnicity: 4 categories
Chance variables	Marital status: 4 categories	Marital status: 3 categories
	Children: indicator for any; number of children	Children: indicator for any; number of children
	Health: poor self-reported health for head (and spouse)	Health: poor self-reported health for head (and spouse)
	Age: continuous for head	Age: continuous for head
	Inheritances: indicator for any; amount <1980 , 1980-1988, and >1988	Inheritances: not available
Choice variables	Percent held in	Percent held in
	10 wealth categories	8 wealth categories

Table 3.3: Comparing the Venti and Wise to Our SIPP Varibles

To assess the extent to which this measurement error in lifetime earnings affects their key analysis, we repeat the regression in equation (6) while attempting to follow the Venti and Wise (2001) specification as closely as possible. However, the SIPP does not ask the same questions as the HRS, so we are not able to replicate Venti and Wise's (2001) analysis exactly. To highlight the similarities and differences in our specifications, we compare them directly in Table 3.3. The main difference between Venti and Wise (2001) and our analysis is that we do not include information on defined benefit pension wealth or on inheritances.

Table 3.4
Table 3.4

		Censored	Uncensored	4th
	VW (2001)	Deciles	Deciles	Poly.
Lifetime earn	5.1	11.9	17.2	20.4
Lifetime earn + Chance	9.1	13.3	18.4	24.1
Lifetime earn + Invest	13.0	22.6	26.3	26.9
Lifetime earn $+$ Chance $+$ Invest	15.3	24.5	29.2	32.6
$\label{eq:lifetime} {\rm Lifetime\ earn\ +\ Chance\ +\ Invest\ +\ Taste}$	16.0	27.7	31.3	34.8

We present our key results in Table 3.4, in which we assess how well the regressors of equation (6) predict retirement wealth based on the reduction of the RMSE. Column (1) repeats the results reported in Venti and Wise (2001). Column (2) replicates their analysis with the 2001 SIPP, censoring lifetime earnings as it is in Venti and Wise (2001). While the reductions in RMSE are markedly higher across all sets of regressors, the pattern of adding regressors is largely the same: The inclusion of chance variables, investment variables, and taste variables all increase the reduction in RMSE. With that said, it is interesting that the reductions in RMSE are noticeably larger in the 2001 SIPP for all regressor sets. Perhaps this difference is due to our exclusion of
defined benefit pension wealth, but given that defined benefit pension wealth is usually a function of earnings and years of service, we doubt this is the case.⁷⁵

The purpose of our analysis is to assess the extent to which measurement error in lifetime earnings affected the analysis, which can be assessed by comparing column (2) to columns (3) and (4). Our preferred estimates are in column (4). The role of lifetime income in predicting retirement wealth goes up substantially. When comparing columns (2) and (4), the raw difference in the role of lifetime earnings almost doubles, from 11.8 percent reduction in RMSE with censored deciles to 20.5 with lifetime earnings included as a polynomial. Thus, our results suggest that the potential role for lifetime earnings is dramatically understated in Venti and Wise (2001).

Second, as discussed in Section 2, measurement error in one variable will generally bias the results for all other variables. This can be observed in the smaller increase in the total reduction in RMSE for the full model when comparing columns (2) and (4)—the reduction in RMSE when all variables are included increased only from 27.9 to 34.6. This is consistent with the view that, by only "partially controlling" for the role of lifetime earnings due to the censored nature of the data, the remaining variables in the regression are also biased. Thus, the role of lifetime earnings did not just increase, but the importance of other variables declined. In column (2), the amount of reduction in RMSE that is observed by just including lifetime earnings is about 42 percent of the overall reduction (comparing the 11.8 percent reduction to the 27.9 overall reduction). In column (4), the same role of lifetime earnings is 60 percent of the overall reduction (comparing the 20.5 reduction to the 34.2 overall reduction).

⁷⁵In other words, given that defined benefit pension wealth is often strongly related to the same factors that determine lifetime earnings, we would have expected that the inclusion of defined benefit pension wealth would have delivered even larger reductions in RMSE.

3.6 Conclusion

In this paper, we replicate an influential analysis by Venti and Wise (1999, 2001) that seeks to understand the determinants of retirement wealth. Despite the better data available in the HRS along numerous dimensions, their analysis still faced an important data problem: the measure of lifetime earnings was subject to fairly serious topcoding concerns. We use W-2 data in the 2001 SIPP to assess the extent to which the topcoding in the HRS affected the results. Our findings indicate that association of lifetime earnings, measured both by its absolute importance and by its relative importance in reducing the RMSE, with retirement wealth is much greater than suggested by their analysis. While much variation remains in the accumulation of retirement wealth, our results suggest that the simple and direct role that lifetime earnings plays should not be overlooked.

While the findings in this study are instructive, further work remains. First, we provide results for the 2001 SIPP panel. Similar analyses should be undertaken with the 1996, 2004, and 2008 panels. Second, we closely follow Venti and Wise's analysis strategy because of the potential role of measurement error. With that said, another influential paper on savings behavior, Dynan, Skinner and Zeldes (2004), uses panel data on wealth. Their paper could also be replicated with SIPP panels, thus providing a comprehensive examination of savings behavior with one data set.

APPENDIX

Our main research question is to determine the extent to which measurement error in lifetime earnings deciles influenced the conclusions in Venti and Wise (1999, 2001). Thus, we attempt to impose the same degree of measurement error in the SIPP data. To do so, we rely on the censorship rate reported in Haider and Solon (2006), who use the same HRS Social Security earnings histories, because Venti and Wise (1999, 2001) do not report the censorship rate in their sample.

In Table 3.6, we report the taxable limit for the 20 years before the 1992 HRS and the censorship rate reported in Table 3.1 in Haider and Solon (2006). In Haider and Solon (2006), the sample is composed of only male respondents who were 59-61. For our primary analysis, we take the 59-61 men in the 2001 SIPP and find the level of earnings that would provide the same rate of censorship. We report this earning level in column (3) of Table 3.6. We then use this earnings value as the topcode for all individuals in our 2001 SIPP analysis sample (men and women, regardless of age).

To assess the sensitivity of our results to this procedure, we also compute the top-code dearnings value that we would find if we used all men ages 51-61. We report this alternative topcode value in column (4) of Table 3.6. As can be observed, the top-code values are quite similar. We also replicate Table 3.2 in the text based on these alternative top-code values, showing the results in Table 3.7. Again, we find the degree of measurement is quite similar. Finally, we repeated the key analysis in Table 3.4 based on censored earnings deciles (column 2), delivering estimates of 11.8, 12.5, 22.6, 24.8, and 27.8. None of the estimates vary by more than 0.3 as compared to the values reported in Table 3.4.

Our key dependent variable is bequeathable wealth, which we obtain from the

SIPP 2001 wave 3 topical module. Table 3.8 provides detailed information about the wealth measures we use. We divide the variables into nine asset categories that broadly correspond to those categories used by Venti and Wise (2001). The main difference between our wealth variable and the Venti and Wise (2001) wealth variable is that we do not include information on defined benefit pensions.

	SIPP-	SIPP-	HRS-	HRS-	
	matched	unmatched	matched	unmatched	
HH in survey year	56,610	52,430	$53,\!434$	54,253	
Percent female	50.0	53.3	54.0	53.0	
Age	55.5	55.6	55.4	55.6	
Percent non-white	13.4	16.7	13	15	
Education	13.6	13.2	12	12	
Percent married	69.2	70.0	76	76	

Table C.1: Comparing Administrative Data Availability

	SSA Maximum	Censorship Rate Equivalent Top-		Equivalent Top-	
	Taxable Earnings	Reported in	Code for 2001	Code for 2001	
Years Before	Relevant to	Haider and SIPP Using 59-61		SIPP Using 51-61	
Survey	$1992 \ HRS$	Solon (2006)	Men	Men	
1	53,400	9.1	73,190	79,900	
2	$51,\!300$	10.4	$68,\!930$	$72,\!830$	
3	48,000	12.2	67,270	68,760	
4	45,000	13.6	$56,\!350$	61,340	
5	43,800	12.7	58,100	61,760	
6	42,000	13.4	57,440	60,100	
7	39,600	14.3	57,410	$56,\!380$	
8	37,800	15.1	$52,\!190$	$53,\!270$	
9	35,700	14.7	50,260	$52,\!280$	
10	32,400	17.2	$46,\!830$	47,480	
11	29,700	18.8	44,430	44,780	
12	$25,\!900$	22.5	40,920	40,920	
13	22,900	26.7	$37,\!880$	$36,\!590$	
14	17,700	40.0	28,000	28,140	
15	16,500	36.3	29,380	29,040	
16	$15,\!300$	37.1	27,030	27,300	
17	14,100	37.1	26,710	$25,\!550$	
18	13,200	39.5	24,380	$23,\!350$	
19	10,800	49.6	19,640	17,900	
20	9,000	55.3	16,960	$15,\!900$	

Table C.2: Determining Censorship in SIPP

Table C.3: Men 51-61 Topcode Values

				Cen	sore	l Dec	cile			
Uncensored Decile	1	2	3	4	5	6	7	8	9	10
1 (Lowest)	100	0	0	0	0	0	0	0	0	0
2	0	94	6	0	0	0	0	0	0	0
3	0	4	91	6	0	0	0	0	0	0
4	0	1	3	88	8	0	1	0	0	0
5	0	1	0	4	82	13	0	0	0	0
6	0	0	0	1	$\overline{7}$	72	19	0	0	0
7	0	0	0	0	2	8	65	24	0	0
8	0	0	0	0	1	2	10	46	41	0
9	0	0	0	0	0	2	3	17	39	39
10 (Highest)	0	0	0	0	0	2	3	12	20	61

Table C.4: 2001 SIPP Wealth Variables

Category	Elements
Business	Value of first business (tvbva1)
	Total debt of first business (tvbde1)
	Value of second business (tvbva2)
	Total debt of second business (tvbde2)
Checking/Savings	Amount in interest earning account (tiaita) Amount in non-interest checking account (talicha)
Donda /II S. Socuritica	Amount of bonds (securities (timis)
Donus/ 0.5. Securities	Face value of U.S. savings bonds (talsby)
	race value of 0.5. savings bolids (taisby)
Stocks	Value of stocks/funds (esmiv)
	Amount of debt on the stocks/mutual funds (esmimay)
Real estate – house	Current value of residence (tpropval)
	Principal owed on mortgage (tmip)
	Market value of mobile home (tmhval)
	Debt of mobile home (tmhpr)
Real estate – other	Market value of rental property (trtmv)
	Principal owed on rental property (trtpri)
	Equity in other real estate (tothreva)
Valation	
venicies	A mount and for first second and third valides
	(talamt_ta2amt_ta3amt)
	(taranit, tazanit, tazanit)
Retirement	Market value of IRA (talrb)
	Market value of Keogh (talkb)
	Market value of 401k (taltb)
	Current face value of life insurance (talliv)
Debt	Amount owed for store bills/credit cards (ealidab)
	Amount of loans owed through credit union or bank (ealidal)
	Amount of other debt (ealidao)

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