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THREE EMPIRICAL STUDIES OF
HUMAN CAPITAL, LABOR SUPPLY, AND HEALTH CARE

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Merve Cebi

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By

Merve Cebi

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ABSTRACT

THREE EMPIRICAL STUDIES OF HUMAN CAPITAL, LABOR SUPPLY, AND HEALTH CARE

By

Merve Cebi

Locus of Control and Human Capital Investment Revisited

Locus of control (LOC) is a psychological concept that measures the extent to which an individual believes she has control over her life (internal control) as opposed to believing that luck controls her life (external control). Findings from the early empirical literature suggested that internal LOC is related to higher educational attainment and earnings. However, a key concern in the early literature is that LOC could merely be a proxy for unobserved ability, which could itself increase education and earnings. To distinguish between the effects of LOC and the effects of ability, Coleman and DeLeire (2003) present a model of human capital investment that incorporates LOC. I test the predictions of the Coleman-DeLeire model using data from the National Longitudinal Survey of Youth. My findings fail to support Coleman and DeLeire's predictions and suggest that LOC is not a significant determinant of educational outcomes once cognitive ability is controlled for; however, LOC does lead to higher earnings later in life.

Employer-Provided Health Insurance and Labor Supply of Married Women

This work presents new evidence on the effect of husbands' health insurance on wives' labor supply. Previous cross-sectional studies have estimated a significant negative effect of spousal coverage on wives' labor supply. However, these estimates

potentially suffer from bias because wives' labor supply and the health insurance status of their husbands are interdependent and chosen simultaneously. This paper attempts to obtain consistent estimates by using several panel data methods. In particular, the likely correlation between unobserved characteristics of husbands and wives affecting labor supply—such as preferences for work—can be captured using panel data on intact marriages, and potential joint job choice decisions can be controlled using fixed-effects instrumental variables methods. The findings, using data from the Current Population Survey and the National Longitudinal Survey of Youth, suggest that the negative effect of spousal coverage on labor supply found in cross-sections results mainly from spousal sorting and selection. There is only a small estimable effect of spousal coverage on wives' labor supply.

Health Insurance Tax Credits and Health Insurance Coverage of Low-Income Single Mothers

The Omnibus Budget Reconciliation Act of 1990 introduced a refundable tax credit for low-income families who purchased health insurance coverage for their children. This health insurance tax credit (HITC) existed during tax years 1991, 1992, and 1993, and was then rescinded. We use Current Population Survey data and a difference-in-differences approach to estimate the HITC's effect on private health insurance coverage of low-income single mothers. The findings suggest that during 1991-1993, the health insurance coverage of single mothers was about 6 percentage points higher than it would have been in the absence of the HITC.

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CHAPTER 1

LOCUS OF CONTROL AND HUMAN CAPITAL INVESTMENT REVISITED

1.1. Introduction

The determinants of educational attainment have been the subject of intensive research. A consensus has emerged that certain variables affect education, including socioeconomic variables, family background measures, and personal attributes such as cognitive and noncognitive skills. In an attempt to identify the impact of noncognitive skills, a strand of literature has focused attention on the social-psychological concept of "locus of control," which measures the extent to which an individual believes she has control over her life (internal control) as opposed to believing that luck controls her life (external control).

The early empirical literature was limited to including locus of control in wage or educational attainment regressions along with measures of cognitive skill. (See, for example, Andrisani 1977, 1981). Findings from this literature suggested that internal locus of control is related to higher educational attainment and higher earnings. However, a key concern in the early literature is that internal locus of control could merely be a proxy for unobserved ability, which could itself increase education and earnings.

To distinguish between the effects of locus of control and the effects of ability, the subsequent literature has begun to explore the mechanism by which locus of control affects educational outcomes. In particular, Coleman and DeLeire (2003) present a model of human capital investment that explicitly incorporates locus of control. This model distinguishes among four groups of teenagers:

- Internal Graduates — teenagers who graduate from high school and believe that graduating will lead to higher wages and higher-skill occupations
- External Graduates — teenagers who graduate from high school but do not believe that graduating will lead to higher wages or higher-skill occupations
- Internal Dropouts — teenagers who drop out and believe that dropping out will lead to lower wages and worse occupational outcomes
- External Dropouts — teenagers who drop out and do not believe that dropping out will lead to lower wages or worse occupational outcomes

Coleman and DeLeire's model implies that, among high school graduates, internal teenagers will say they expect higher earnings in the future than external teenagers — that is, Internal Graduates will have higher earnings expectations than External Graduates. However, among dropouts, the model implies the opposite — that is, Internal Dropouts will have lower earnings expectations than External Dropouts (Coleman and DeLeire 2003, equation 5). The intuition behind this asymmetry, which is depicted in the top panel of Figure 1.1, is that internal teenagers perceive a relationship between their current actions and future outcomes, whereas external teenagers do not.

Coleman and DeLeire contrast their model with an alternative model in which locus of control is simply a proxy for ability. This alternative model does not produce the asymmetric effect of locus of control on expected outcomes (conditional on educational attainment). Rather, if locus of control is simply an aspect of ability, internal teenagers will expect better outcomes than external teenagers regardless of whether they graduate from high school, as shown in the bottom panel of Figure 1.1. Thus, Coleman and

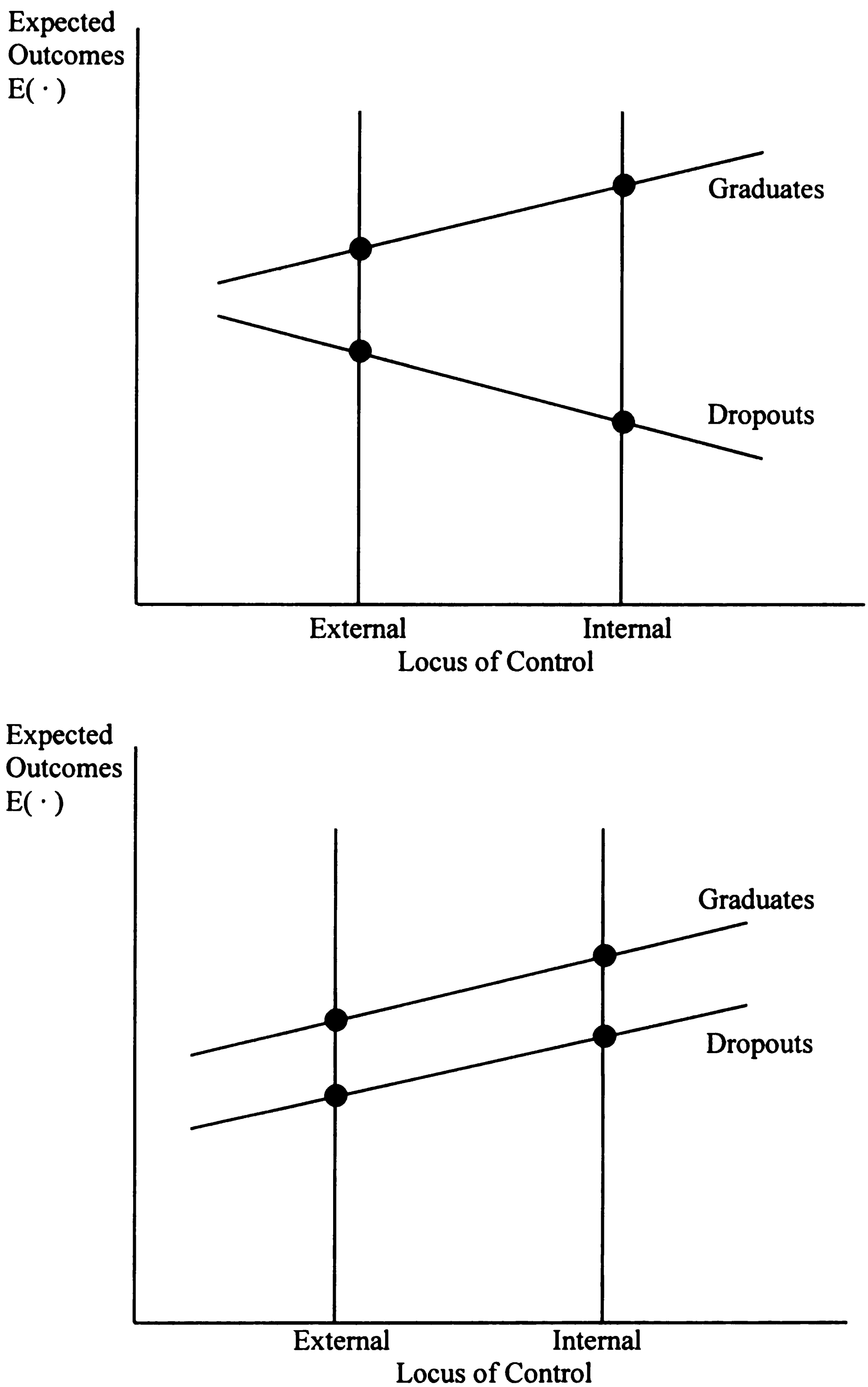

Figure 1.1. Relationships between Expected Outcomes and Locus of Control for High School Graduates and Dropouts in the Coleman-DeLeire Model (top) and when Locus of Control is a Proxy for Ability (bottom)

DeLeire's model and the alternative ability-based model offer distinct and empirically testable implications.

Using data from the National Education Longitudinal Study (NELS), Coleman and DeLeire find evidence that supports their model. Consistent with the predicted pattern of expectations, Internal Dropouts expect to receive lower wages and to be in lower-skilled occupations than do External Dropouts.

This study reexamines the effect of locus of control on educational attainment and tests the predictions of Coleman and DeLeire's model using data from the National Longitudinal Survey of Youth (NLSY). First, I investigate whether locus of control is an important predictor of educational attainment for a teenage sample of 10th and 11th graders in 1979. Second, given information on these teenagers' educational attainment three years later, I examine the effect of locus of control on their occupational expectations. Third, the NLSY provides an opportunity to study the subsequent labor market outcomes of the teenage sample. Because the respondents are between the ages of 37 and 45 as of the 2002 survey, it is possible to examine the impact of teenagers' locus of control on their adult earnings.

1.2. Data

The NLSY is a sample of 12,686 young men and women between the ages of 14 and 22 at the time of the first interview in 1979. Since their first interview, they have been reinterviewed annually until 1994, and biennially from 1996 to the present.

The NLSY consists of three subsamples: a representative sample of the noninstitutionalized civilian youths; an oversample of blacks, Hispanics, and economically disadvantaged whites; and a sample of respondents who were enlisted in the military. In this study, I use the nationally representative sample of 6,111 respondents in order to derive estimates using a random sample. Observations are included if (1) respondents had valid measures of education for the years 1979-1982; (2) information on respondents' locus of control scale was available; (3) respondents were in the 10th or 11th grade in 1979.[1] Applying these restrictions resulted in a final sample of 1,737 individuals.

The Rotter Internal-External Locus of Control Scale, collected in the 1979 survey, is a four-item questionnaire designed to measure the extent to which individuals believe they have control over their lives (internal control) as opposed to believing that luck controls their lives (external control). Respondents were asked to select one of each of four paired statements,[2] and then decide if the selected statement was much closer or slightly closer to their opinion of themselves. A four-point scale was generated for each of the paired items, and the resulting scores are individually standardized. The average of the standardized scores is used to create the locus of control scale. Higher scores indicate greater internal control, whereas lower scores indicate greater external control.

In 1980, the NLSY data were supplemented by a series of achievement tests known as the Armed Forces Vocational Aptitude Battery (ASVAB). The scores for selected parts of the ASVAB are then used to construct a composite Armed Forces

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[1] Ninth graders are not included in the sample because although most students would have graduated from high school, they would not be old enough to attend college by the time of the 1982 survey. The results for high school graduation are robust to the inclusion of 9th graders. For sake of brevity, these results are not reported but are available from the author upon request.

[2] 1. (a) What happens to me is my own doing; or (b) Sometimes I feel that I do not have enough control over the direction my life is taking.
2. (a) When I make plans, I am almost certain that I can make them work; or (b) It is not always wise to plan too far ahead, because many things turn out to be a matter of good or bad fortune anyhow.
3. (a) In my case, getting what I want has little or nothing to do with luck; or (b) Many times, we might just as well decide what to do by flipping a coin.
4. (a) Many times, I feel that I have little influence over the things that happen to me; or (b) It is impossible for me to believe that chance or luck plays an important role in my life.

Qualifications Test (AFQT) score for each respondent. The NLSY provides the raw and standard scores for each subset of the ASVAB, as well as two percentile scores: an AFQT80 and an AFQT89.[3]

The percentile scores are the most widely used measures of ability by researchers. However, Blackburn (2004) discusses that the AFQT percentile ranking is not a correct measure of ability since ability follows a normal distribution while a percentile follows a uniform distribution. He advises the use of raw or standard scores as a more appropriate measure of the AFQT performance. In this study, the AFQT measure is constructed as the sum of standard scores for the verbal, math knowledge, and arithmetic reasoning subtests of the ASVAB.

The implications of Coleman and DeLeire's model concern the labor market expectations of teenagers conditional on educational attainment. It thus is essential to have information on expectations collected after the decision about educational attainment has been made. In the 1979 and 1982 surveys, NLSY respondents were asked about their "Occupational Expectations at Age 35 (Census 3-Digit)." Since the sample used in this analysis consists of 10th and 11th graders in 1979, occupational aspiration of teenagers is extracted from the 1982 survey along with information on their graduation and college enrollment status.[4]

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[3] The two AFQT measures differ in the methods used to calculate the scores. The AFQT80 measure is constructed as the sum of the following subtests of the ASVAB: word knowledge, paragraph comprehension, arithmetic reasoning, and numerical operations. Beginning in 1989, a new formula has been used to calculate a revised percentile score called the AFQT89. The three subtests used in the 1989 scoring version of the AFQT score are verbal, math knowledge, and arithmetic reasoning. Rest of the ASVAB includes the following subtests: mechanical comprehension, general science, electronics information, auto and shop information, and coding speed. Attachment 106 to the NLSY documentation describes the ASVAB subtests in detail.

[4] I use the revised version of the Highest Grade Completed variable to identify high school graduates and college attendees.

Table 1.1 presents descriptive statistics for teenagers by their educational level in 1982. Out of 1,737 observations, 1,370 graduated from high school but only 545 had attended college as of 1982. Teenagers who graduated from high school come from higher income families and have a significantly higher locus of control score than teenagers who dropped out of high school, 0.043 versus -0.172. Similarly, the locus of control score of teenagers who attended college (0.141) is significantly higher than of teenagers who did not (-0.068). Both mothers and fathers of teenagers who attended college obtained more education on average and were more likely to have worked as a professional or manager than those of teenagers who did not attend college.

1.3. Estimation Method and Results

1.3.1. Locus of Control and Educational Attainment

In Table 1.2, the left-hand panel shows the estimated marginal effects of locus of control on high school graduation from probit models. The right-hand panel reports the estimated marginal effects on college attendance. The basic specification is presented in Column 1. It includes dummy variables indicating race, ethnicity, gender, age, residence in an SMSA, and residence in an urban area as controls. According to these estimates, locus of control is an important predictor of educational attainment for teenagers. A one-standard-deviation increase in locus of control is estimated to increase the probability of high school graduation by 5.4 percent, and the probability of college attendance by 7.4 percent.

Column 2 adds indicators of parental education as controls to the basic model. The estimated marginal effect of locus of control remains both economically and statistically significant. In particular, a one-standard-deviation increase in locus of control

Table 1.1. Summary Statistics for Key Variables by Education Level

	Entire Sample	High School Grads	High School Dropouts	Attended College	Did not Attend College
High School Graduate	0.789	1	0	1	0.692
	(0.408)	(0)	(0)	(0)	(0.462)
Attended College	0.314	0.398	0	1	0
	(0.464)	(0.490)	(0)	(0)	(0)
Locus of Control	-0.003	0.043	-0.172	0.141	-0.068
	(0.574)	(0.562)	(0.587)	(0.543)	(0.576)
AFQT	195.857	203.610	166.009	221.361	184.018
	(35.277)	(32.164)	(30.535)	(26.551)	(32.453)
Family Income	20,718	22,615	13,528	26,999	17,879
	(14,028)	(14,116)	(11,069)	(15,838)	(12,110)
Father's Education					
Less than High School	0.401	0.336	0.643	0.167	0.508
	(0.490)	(0.472)	(0.480)	(0.373)	(0.500)
High School	0.331	0.353	0.248	0.308	0.341
	(0.471)	(0.478)	(0.432)	(0.462)	(0.474)
Some College	0.104	0.115	0.063	0.156	0.081
	(0.306)	(0.320)	(0.243)	(0.363)	(0.272)
College and beyond	0.164	0.196	0.046	0.369	0.070
	(0.370)	(0.397)	(0.210)	(0.483)	(0.256)
Mother's Education					
Less than High School	0.376	0.310	0.621	0.145	0.482
	(0.485)	(0.463)	(0.486)	(0.352)	(0.500)
High School	0.445	0.481	0.311	0.495	0.422
	(0.497)	(0.500)	(0.463)	(0.500)	(0.494)
Some College	0.089	0.103	0.038	0.154	0.060
	(0.285)	(0.304)	(0.192)	(0.361)	(0.237)
College and beyond	0.090	0.106	0.030	0.206	0.037
	(0.286)	(0.308)	(0.171)	(0.404)	(0.189)
Father's Occupation[a]	0.219	0.249	0.106	0.406	0.133
	(0.414)	(0.433)	(0.309)	(0.491)	(0.340)
Mother's Occupation[a]	0.083	0.094	0.041	0.163	0.046
	(0.276)	(0.292)	(0.198)	(0.370)	(0.210)
Number of Observations	1,737	1,370	367	545	1,192

Notes: a. A dummy variable indicating adult male (or female) in household worked as a professional or manager when the respondent was 14 years old. Standard deviations are reported in parentheses.

Table 1.2. Marginal Effects of Locus of Control on Educational Attainment from Probit Models

	High School Graduation					College Attendance				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Locus of Control	0.094	0.080	0.071	0.067	0.026	0.130	0.099	0.107	0.103	0.040
	(0.017)	(0.016)	(0.018)	(0.019)	(0.018)	(0.020)	(0.020)	(0.023)	(0.023)	(0.024)
Black	-0.095	-0.036	-0.007	0.006	0.062	-0.038	0.126	0.144	0.165	0.388
	(0.033)	(0.030)	(0.031)	(0.031)	(0.023)	(0.033)	(0.042)	(0.049)	(0.051)	(0.059)
Hispanic	0.036	0.078	0.101	0.111	0.105	-0.007	0.164	0.219	0.228	0.239
	(0.035)	(0.027)	(0.024)	(0.023)	(0.019)	(0.045)	(0.058)	(0.065)	(0.067)	(0.071)
Female	0.008	0.019	0.039	0.041	0.033	0.007	0.035	0.031	0.032	0.060
	(0.020)	(0.019)	(0.021)	(0.021)	(0.020)	(0.022)	(0.022)	(0.025)	(0.025)	(0.026)
Urban	-0.050	-0.054	-0.062	-0.062	-0.055	0.046	0.029	0.023	0.027	0.030
	(0.026)	(0.025)	(0.027)	(0.028)	(0.026)	(0.031)	(0.033)	(0.037)	(0.038)	(0.038)
Parental Education	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Family Structure	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Home Life	No	No	No	Yes	Yes	No	No	No	Yes	Yes
AFQT	No	No	No	No	Yes	No	No	No	No	Yes
Observations	1,737	1,737	1,394	1,394	1,350	1,737	1,737	1,394	1,394	1,350
Pseudo R-square	0.20	0.24	0.26	0.27	0.32	0.11	0.24	0.25	0.25	0.34

Notes: All specifications include dummy variables indicating age and residence in an SMSA. Model (2) also controls for father's and mother's education. Family Structure includes family income, and occupation of both father and mother. Home Life includes dummy variables indicating teenagers or a family member regularly received magazines, newspapers, and held a library card when they were at the age of 14. Standard errors are reported in parentheses.

is estimated to increase teenagers' probability of graduating from high school by 4.6 percent, and their probability of attending college by 5.7 percent. The addition of family income and parents' occupation status in Column 3 produces very similar results to those obtained from previous specifications.

Column 4 adds dummy variables indicating whether teenagers received magazines, newspapers, and held a library card at age 14. The estimated marginal effects are essentially similar and still statistically significant. A one-standard-deviation increase in locus of control is associated with a 3.8 percent increase in teenagers' likelihood of graduating from high school.

Column 5 adds teenagers' AFQT score as a control for cognitive ability. With this addition, the estimated marginal effect of locus of control drops and becomes much less significant. The estimated marginal effect of locus of control on high school graduation is 0.026, with a t-statistic of 1.44 (p-value = 0.15). This implies that a one-standard-deviation increase in locus of control increases the probability of high school graduation by 1.5 percent. The marginal effect of locus of control on college attendance is 0.04 which is significant only at the 10-percent level. This implies that a one-standard-deviation increase in locus of control increases the probability of college attendance by 2.3 percent. The results in Column 5 suggest that locus of control is capturing the marginal effect of the AFQT score on educational attainment in Columns 1-4. The locus-of-control estimates in Columns 1-4 suffer from omitted variable bias. The simple correlation between locus of control and AFQT is 0.28.

1.3.2. Locus of Control and Occupational Expectations

I follow Coleman and DeLeire and estimate the following by OLS,

(1.1) $occexp35 = X\beta + \delta_1\, internal \times grad + \delta_2\, average \times grad + \delta_3\, external \times grad$

$+ \delta_4\, internal \times dropout + \delta_5\, average \times dropout + \delta_6\, external \times dropout + \mathrm{u},$

(1.2) $occexp35 = X\beta + \delta_1\, internal \times college + \delta_2\, average \times college + \delta_3\, external \times college$

$+ \delta_4\, internal \times ncollege + \delta_5\, average \times ncollege + \delta_6\, external \times ncollege + \mathrm{u},$

where the dependent variable is a dummy variable indicating that the teenager expects to work in a high-skilled occupation at age 35. Controls include race, ethnicity, gender, age, residence in an SMSA and in an urban area, and AFQT.

The locus-of-control score of teenagers measured in 1979 is used to construct three dummy variables. The variable *internal* equals 1 if the teenager is above the 75th percentile of the locus of control range. The 25th to 75th percentiles are defined as *average*, and values below the 25th percentile are defined as *external*. The variables *grad* and *dropout* are indicators of whether the teenager had graduated from high school or dropped out of high school as of 1982. Similarly, *college* and *ncollege* are dummy variables indicating whether the teenager did or did not attend college.

Table 1.3 reports the predicted expectations of being in a high-skilled occupation at age 35 that result from estimating Equation 1.1 in the top panel and Equation 1.2 in the bottom panel. Each panel shows predicted occupational expectations for six groups of teenagers: high school graduates (or college attendees) with internal, average, or external locus of control and dropouts (or non-college attendees) with internal, average, or external locus of control. For each group, three predictions are shown: those from (1) a specification that includes no control variables; (2) a specification that controls for race, ethnicity, gender, age, and residence in an SMSA and in an urban area; and (3) a specification that controls for AFQT in addition to the controls in Specification 2.

The results in Table 1.3 suggest that Internal Dropouts have basically the same occupational expectations as External Dropouts. Similarly, internal non-college attendees have roughly the same expectations as external non-college attendees. This implies that the pattern predicted in Coleman and DeLeire's model is not apparent in these data.

Another way to distinguish between the two models is to test whether the gap between Internal and External Graduates' expectations differs from that between Internal and External Dropouts. To see this, recall first that in both the Coleman-DeLeire model and the alternative ability-based model, we should observe only a small (if any) difference in expectations between External Graduates and External Dropouts (compare the top and bottom panels of Figure 1.1). Recall next that in the Coleman-DeLeire model, Internal Graduates have higher expectations than External Graduates, and Internal Dropouts have lower expectations than External Dropouts (as shown in the top panel of Figure 1.1), so there is a large gap between the expectations of Internal Graduates and Internal Dropouts. In the alternative model, in contrast, Internal Graduates have higher expectations than External Graduates, and Internal Dropouts will have higher expectations than External Dropouts, so there is only a small expectations gap between Internal Graduates and Internal Dropouts (bottom panel of Figure 1.1). Together, these predictions suggest that if we estimate the difference-in-differences between External Graduates and External Dropouts, and Internal Graduates and Internal Dropouts, an estimate statistically different from zero would support the Coleman-DeLeire model.

Table 1.3. Predicted Occupational Expectations at Age 35

	High School Graduates and Dropouts								
	(1) No controls			**(2) Controls excluding AFQT**			**(3) Controls including AFQT**		
	HS Grads	HS Dropouts	Difference	HS Grads	HS Dropouts	Difference	HS Grads	HS Dropouts	Difference
	(0.03)	(0.06)	[0.06]	(0.09)	(0.09)	[0.07]	(0.17)	(0.17)	[0.07]
Average Locus	0.44	0.26	0.18	0.43	0.28	0.16	0.42	0.38	0.04
	(0.02)	(0.04)	[0.04]	(0.09)	(0.09)	[0.04]	(0.17)	(0.17)	[0.04]
External Locus	0.39	0.21	0.18	0.39	0.24	0.15	0.41	0.36	0.04
	(0.03)	(0.04)	[0.05]	(0.09)	(0.09)	[0.05]	(0.17)	(0.17)	[0.05]
Difference between	0.11	0.11		0.11	0.08		0.02	0.00	
Internal and External	[0.04]	[0.07]		[0.04]	[0.07]		[0.04]	[0.07]	
Difference-in-Differences			0.00			0.03			0.02
			[0.08]			[0.08]			[0.08]
	College Attendees and Non-Attendees								
	(1) No controls			**(2) Controls excluding AFQT**			**(3) Controls including AFQT**		
	College	No College	Difference	College	No College	Difference	College	No College	Difference
Internal Locus	0.65	0.35	0.30	0.65	0.35	0.30	0.56	0.34	0.22
	(0.03)	(0.03)	[0.05]	(0.08)	(0.08)	[0.05]	(0.14)	(0.14)	[0.05]
Average Locus	0.64	0.30	0.34	0.63	0.30	0.33	0.57	0.34	0.23
	(0.03)	(0.02)	[0.03]	(0.08)	(0.08)	[0.04]	(0.14)	(0.14)	[0.04]
External Locus	0.60	0.26	0.34	0.60	0.27	0.32	0.55	0.33	0.22
	(0.05)	(0.03)	[0.06]	(0.08)	(0.08)	[0.06]	(0.14)	(0.14)	[0.06]
Difference between	0.05	0.09		0.05	0.07		0.01	0.01	
Internal and External	[0.06]	[0.04]		[0.06]	[0.04]		[0.06]	[0.04]	
Difference-in- Differences			-0.04			-0.02			0.00
			[0.07]			[0.07]			[0.07]

Table 1.3 presents the findings on differences in occupational expectations for high school graduates and dropouts. The third column in each panel shows the difference between high school graduates and dropouts for each group (internal, average, and external teenagers). The fourth row reports the difference between internal and external teenagers for both high school graduates and dropouts. Finally, the difference-in-differences estimates are presented in the last row. Parallel findings for college attendees and non-attendees are given in the bottom panel of Table 1.3.

In all specifications, the difference-in-differences estimate is close to zero and statistically insignificant. Once again, the predictions of Coleman and DeLeire's model are not borne out in these data.

1.3.3. Locus of Control and Wages

To examine the relationship between teenagers' locus of control and their wages later in life, I estimate a human capital earnings function similar to that estimated by Andrisani (1977). The dependent variable is log hourly wages measured at the time of the 2000 interview. The estimation results are presented in Table 1.4. Control variables in Column 1 include years of education, race, ethnicity, gender, marital status, residence in an SMSA and in an urban area, a quadratic in age, and a set of occupational dummies. Column 2 adds locus of control, Column 3 adds the AFQT score, and Column 4 includes both.

Based on the estimates in Table 1.4, the return to a year of education without controlling for any measures of ability is 7 percent. As shown in Column 2, adding locus of control does not affect this estimate. However, with the addition of the AFQT score in Column 3, the estimated return falls to 5 percent, which reflects the familiar ability bias

Table 1.4. Effects of Locus of Control on Adult Wages

	(1)	(2)	(3)	(4)
Locus	—	0.057	—	0.036
		(0.015)		(0.015)
AFQT	—	—	0.003	0.003
			(0.001)	(0.001)
Education	0.074	0.072	0.050	0.049
	(0.004)	(0.004)	(0.005)	(0.005)
Black	-0.172	-0.168	-0.057	-0.060
	(0.025)	(0.025)	(0.028)	(0.028)
Hispanic	-0.041	-0.038	0.022	0.022
	(0.032)	(0.032)	(0.032)	(0.032)
Female	-0.312	-0.309	-0.304	-0.302
	(0.017)	(0.017)	(0.017)	(0.017)
Married	0.081	0.080	0.065	0.065
	(0.020)	(0.020)	(0.020)	(0.020)
Urban	0.045	0.045	0.042	0.042
	(0.020)	(0.020)	(0.020)	(0.020)
Observations	4,278	4,278	4,137	4,137
R-squared	0.29	0.29	0.31	0.31

Notes: The dependent variable is the log(hourly wage) in 2000. In addition to the variables shown, all specifications include a quadratic in age, a set of occupational dummy variables, and a dummy for residence in an SMSA. Heteroskedasticity-robust standard errors are in parentheses.

in the estimated returns to schooling. Adding locus of control in Column 4 leaves the estimates unchanged relative to those in Column 3. The coefficient on locus of control is 0.036 and is statistically significant at the 5-percent level. A one-standard-deviation increase in locus of control increases hourly wages by 2.1 percent, while a-one-standard-deviation increase in the AFQT score leads to an 11.5 percent increase in hourly wages. These results suggest that locus of control is in fact capturing a distinct aspect of ability not related to cognitive ability as measured by the AFQT. Combined with the findings in Section 1.3.2, the results suggest that, although locus of control is not a significant determinant of educational outcomes, it is rewarded in the labor market through higher wages.

1.4. Discussion and Conclusion

Using data from the NLSY, the analysis in this paper yields three main findings. First, there is no evidence that locus of control predicts high school graduation and little evidence that it predicts college attendance once the AFQT score is included in models of educational attainment (Section 1.3.1). Second, Internal Dropouts have basically the same occupational expectations as External Dropouts, as do internal non-college attendees and external non-college attendees (Section 1.3.2). Third, locus of control measures a distinct skill not captured by the AFQT, and this skill brings a reward in the labor market (Section 1.3.3).

The finding that locus of control does not predict educational attainment conflicts with Coleman and DeLeire's results. Using data from the NELS, they find that locus of control strongly affects educational attainment, presumably by influencing teenagers' assessments of the returns to education. One possible reason for the difference between their findings and mine could be that the cognitive ability tests available in the NELS differ from those in the NLSY. The NELS contains standardized scores in math, reading, science, and history given when students were in the 8th grade. To make the cognitive ability tests in the NLSY as close as possible to those in the NELS, I do two things. First, I use the sum of standard scores in the verbal, math knowledge, and arithmetic reasoning subtests of the ASVAB, and I omit the rather specific subtests such as "electronics information" or "coding speed." Second, I include dummy variables that identify one of eight age groups. My sample of 10th and 11th graders in 1979 who took the ASVAB tests in 1980 consists of students who were between the ages of 15 and 22. By including age dummies, I attempt to control for effects of age at the time the test is taken. Also, I reestimate all the models using different subtests of the ASVAB to allow each to potentially reflect a different skill. These changes, however, do not affect the findings. Hence, there is a real puzzle — the NELS and NLSY give quite different results.

The finding that locus of control is unrelated to teenagers' occupational expectations in the NLSY also conflicts with Coleman and DeLeire's findings from the NELS. A complete test of the predictions of Coleman and DeLeire's model requires information on teenagers' income expectations in addition to their occupational expectations. However, a question about income expectations is not available in the NLSY, so I am unable to test whether locus of control affects income expectations, and my empirical test is incomplete. It follows that my findings in section 1.3.2 hardly provide a convincing rejection of the Coleman-DeLeire model. This is particularly true given the fact that Coleman and DeLeire find stronger results for income expectations than for occupational expectations.[5]

The finding that locus of control is associated with higher subsequent earnings is consistent with the results from previous research by Andrisani (1977). The estimates based on the NLSY data suggest that, although the return to locus of control is smaller than the return to the AFQT, it is still substantial.

While the data used in this study do not fit the Coleman-DeLeire model that explicitly incorporates locus of control into the human capital investment model, this does not mean that we should abandon the Coleman-DeLeire model and return to the simplistic view of the early empirical literature. Rather, future research should more fully

[5] To see if *actual* wages differ between Internal Dropouts and External Dropouts, I have included an interaction term between locus of control and educational attainment in the wage model. The estimates (not reported here but available upon request) suggest that Internal Dropouts earn, on average, about $2 (in year 2000 dollars) more per hour than External Dropouts; however the estimated difference is insignificant (*p*-value = 0.16) at conventional significance levels using robust inference.

examine the mechanisms by which locus of control affects educational attainment and economic outcomes. Whether attitudes developed during childhood years — as captured by a concept like locus of control — have long-term impacts on economic outcomes is both important and intrinsically interesting. Locus of control is potentially important in analyzing the investments parents, schools, and the public sector make in children.

CHAPTER 2

EMPLOYER-PROVIDED HEALTH INSURANCE AND LABOR SUPPLY OF MARRIED WOMEN

2.1. Introduction

Employment-based health insurance coverage is the most common form of health insurance in the United States: In 2006, 62.2 percent of nonelderly individuals and 70.9 percent of nonelderly workers were covered by employer-provided health insurance (Fronstin 2007). That health insurance is tied to employment in the U.S. health care system has received much attention from both policymakers and researchers. Accordingly, a substantial body of research has been devoted to examining the relationship between health insurance and various labor market decisions, including labor force participation, hours worked, job mobility, and retirement (see the reviews by Currie and Madrian 1999, Gruber 2000, and Gruber and Madrian 2004).

One important group whose labor force outcomes are likely to be affected by the availability of health insurance coverage is married women. Because employers who provide health insurance often provide it to both employees and their families, many married women receive health insurance coverage through their husbands (Madrian 2006). This availability of alternative health insurance may reduce wives' demand for insurance in their own name, and therefore affect their labor market decisions.

In the past decade, a number of studies have examined the relationship between husbands' health insurance and wives' labor supply (Buchmueller and Valletta 1999; Olson 1998, 2000; and Wellington and Cobb-Clark 2000). Using cross-section data and estimating reduced form labor supply equations for wives, they estimate that husbands' health insurance has significant negative effects on their wives' labor supply. However, these cross-sectional estimates potentially suffer from bias due to the simultaneity of wives' labor supply and the health insurance status of their husbands.

There are two main reasons for husbands' health insurance to be endogenous to the labor supply decision of married women. First, unobserved personal characteristics of husbands and wives that affect labor supply—such as preferences for work—might be correlated due to the marriage selection process (Lundberg 1988). For example, if women with strong preferences for leisure, child rearing, or home production tend to marry men who work long hours and hence provide health insurance to the family, then cross-sectional regressions of wives' labor supply on spousal coverage would overstate the negative effect of spousal coverage.

A second possible source of endogeneity is that husbands and wives may make joint job choice decisions, which depend on the health insurance options available to the family (Black 2000, and Scott, Berger, and Black 1989). For example, because coverage rates typically increase with firm size,[6] husbands may sort into larger firms or into industries—such as the manufacturing or the public sector—that are more likely to provide health insurance coverage to their workers.[7] Again, this sorting behavior would lead the cross-sectional estimates to overstate the negative effect of spousal coverage.

This paper attempts to avoid some of the limitations of earlier empirical work, which likely estimates a spurious negative relationship between spousal coverage and

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[6] According to the Employee Benefit Research Institute (EBRI) tabulations of data from the March 2007 Supplement to the Current Population Survey (CPS), 27.0 percent of workers in firms with fewer than 10 employees were covered by their own employer's health plan in 2006, compared with 65.0 percent of workers in firms with 1000 or more employees (Fronstin 2007).

[7] According to the EBRI estimates of the CPS, in 2006, 67.1 percent of workers in the manufacturing sector, and 74.2 percent of workers in the public sector were covered by their own employer's health plan (Fronstin 2007).

wives' labor supply, by using several panel data methods. Using data from the National Longitudinal Survey of Youth (NLSY) and the March Annual Demographic Supplements to the Current Population Survey (CPS), I compare results obtained from three econometric approaches: (1) cross-sectional estimation of linear probability and probit models of labor supply, which (perhaps incorrectly) assumes exogeneity of spousal coverage; (2) cross-sectional instrumental variables estimation to handle the potential endogeneity of spousal coverage; and (3) panel data methods to account for the marriage selection process and the joint job choice decisions. The findings suggest the negative effect of spousal coverage on labor supply found in previous cross-sectional studies results mainly from spousal sorting and selection. Once unobserved heterogeneity is controlled, a relatively smaller estimated effect of spousal coverage on wives' labor supply remains.

The paper is organized as follows. Section 2.2 summarizes the empirical literature on the effects of spousal coverage on labor supply. Section 2.3 describes the econometric methodology and discusses how the possible endogeneity of spousal coverage can be handled. Section 2.4 describes the data, Section 2.5 presents the main results, and Section 2.6 concludes.

2.2. Previous Research

Previous research on the effects of husbands' health insurance coverage on wives' labor supply has relied on the assumption that a husband's coverage is exogenous to the labor supply decisions of his wife (Buchmueller and Valletta 1999; Olson 1998, 2000; and Wellington and Cobb-Clark 2000). Using cross-sectional data, these studies estimate that husbands' health insurance has significant negative effects on wives' labor supply. In particular, Buchmueller and Valletta (1999) use data from the April 1993 CPS and estimate that a husband's health insurance reduces his wife's probability of working by 12 percent and her hours of work by 36 percent.

Buchmueller and Valletta's key independent variable is whether or not the husband has health insurance coverage from an employer (spouse's insurance). Olson (1998) points out that a wife's labor supply decision is affected not by her husband having his own health insurance (that is, whether or not it covers the wife, as used by Buchmueller and Valletta 1999), but rather by whether she receives coverage through her husband's health insurance plan (spousal coverage). Still, using data from the March 1993 CPS, Olson finds effects similar to those estimated by that Buchmueller and Valletta: Spousal coverage reduces the probability a wife will work by 11 percent and her hours of work by 20 percent.

Like Olson (1998), Wellington and Cobb-Clark (2000) estimate the labor supply effects of spousal coverage. They use the March 1993 CPS data and estimate a 23 percent reduction in wives' labor force participation due to coverage through husbands' health insurance.[8] Conditional on working, spousal coverage is estimated to reduce hours of work by 17 percent for white wives and 8 percent for black wives.

Buchmueller and Valletta (1999), Olson (1998, 2000); and Wellington and Cobb-Clark (2000) all note that the assumption that spousal coverage is exogenous to the wives' labor supply is questionable. Buchmueller and Valletta use a multinomial logit model in an attempt to account for the unobserved heterogeneity among married couples.

[8] The larger effects estimated by Wellington and Cobb-Clark (2000) may be explained by their inclusion of both spouse's insurance and the key variable, spousal coverage in labor supply models. Because estimated coefficients of spouse's insurance are positive and significant, the net effects of spousal coverage estimated by Wellington and Cobb-Clark (that is, husbands have insurance that covers their wives) are about the same magnitude as those estimated by Olson (1998).

Olson shows that the estimated effects are sensitive to econometric specification and the underlying exogeneity assumption. Wellington and Cobb-Clark try to sign the bias that would result from ignoring the potential endogeneity of spousal coverage. The present study takes a different approach from earlier work in estimating the effect of spousal coverage on wives' labor supply, particularly along the methodological lines described in the next section.

2.3. Empirical Methodology

An empirical model that relates the labor supply of married women to their health insurance coverage by their husbands' employer-provided plan can be specified:

$$LS_i = \delta SPCOV_i + WIFE_i \beta_1 + HUSBAND_i \beta_2 + FAMILY_i \beta_3 + v_i \tag{2.1}$$

where *LS* is the labor supply of a married woman in family *i*. The key variable, *SPCOV*, equals one if the wife in family *i* is covered by her husband's employer-provided health insurance, and zero otherwise. *WIFE* is a vector of wives' personal characteristics (education, experience, experience squared, and race); *HUSBAND* denotes husbands' personal characteristics (education, experience, and experience squared); and *FAMILY* denotes family characteristics (presence of children under age 6, number of children under age 18, region of residence, and family non-wage income). Finally, v represents unobservable factors that affect the labor supply of wife in family *i*.

Estimating equation (2.1) by ordinary least squares (or probit) may produce inconsistent estimates if unobservable characteristics among married couples that affect wives' labor supply (v) are systematically related to the availability of husbands' health insurance coverage (*SPCOV*). One possible solution to this endogeneity problem is to find valid instrumental variables for *SPCOV*. To be valid, instruments must be correlated with husbands' employer-provided health insurance but unrelated to unobservables affecting wives' labor supply. Some previous studies have suggested husbands' job characteristics (having a part-time job, working in a small firm, and being self-employed) as instruments for *SPCOV* (Abraham and Royalty 2005). These job characteristics negatively affect the availability of husbands' employer-provided health insurance, but they are likely correlated with wives' preferences for work or ability due to the marriage selection process and the jointness of job choice decisions.

Panel data offers additional possibilities to avoid the potential problem of endogeneity. As long as unobservables that affect wives' labor supply remain constant over time, panel data may solve the endogeneity problem in equation (2.1). For example, an empirical model with unobserved effects can be written:

$$LS_{it} = \delta\, SPCOV_{it} + WIFE_{it}\beta_1 + HUSBAND_{it}\beta_2 + FAMILY_{it}\beta_3 + c_i + u_{it} \qquad (2.2)$$

where i indexes families, and t indexes time; c_i represents time-invariant unobserved effects on wives' labor supply; and u_{it} represents time-varying unobserved effects on wives' labor supply.

If we can assume that time-invariant unobserved effect, c_i, are uncorrelated with each explanatory variable in equation (2.2) across all time periods, then equation (2.2) becomes a random effects (RE) model. Possible time-invariant influences on labor supply include differences in ability or in preferences for work. It is likely that both factors are correlated with the availability of spousal coverage because of the marriage selection process. In this case, the RE estimator is inconsistent, and first-differencing (FD) or fixed-effects (FE) methods are required for consistent estimation.[9]

FD and FE methods produce consistent estimates of the effect of spousal coverage on wives' labor supply by allowing for arbitrary correlation between unobserved individual effects and the explanatory variables in equation (2.2). The consistency of FD and FE estimators depends on three assumptions: (1) time-varying unobserved effects that are uncorrelated with the explanatory variables across all time periods; (2) sufficient variation in spousal coverage over time; and (3) strict exogeneity of explanatory variables.

The strict exogeneity assumption rules out feedback effects from wives' labor supply to husbands' health insurance coverage in subsequent years. For example, if husbands switch to jobs which would provide health insurance to the family because their wives decide to leave the labor force or work short hours (and hence are less likely to receive health insurance coverage from their own employer), the strict exogeneity assumption is violated, and the FD and FE estimators are inconsistent. In this case, one possible approach to consistent estimation involves using instrumental variables methods applied after a FD or FE transformation, provided valid instruments are available.

2.4. Data

To examine the effect of spousal coverage on labor supply decisions of wives, I use data from the National Longitudinal Survey of Youth (NLSY). The NLSY is a sample of 12,686 young men and women aged 14 to 22 at the time of the first interview

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[9] The choice between FD and FE depends on the assumptions about time-varying unobserved effects. The FE estimator is more efficient if they are serially correlated, while the FD estimator is more efficient when they follow a random walk.

in 1979. Since their first interview, they were reinterviewed annually until 1994, and then biennially from 1996 to the present. Each survey collected information on demographic characteristics, employment, income, and family structure. In this study, I use eight interviews of the NLSY: 1989, 1990, 1992, 1993, 1994, 1996, 1998, and 2000. These years are selected because the questionnaire included detailed questions on the availability and sources of health insurance coverage only in these survey years. Specifically, respondents were asked whether they were covered by a health plan. If the respondent answered "yes," the interviewer asked who paid for the plan. Responses included current employer, previous employer, spouse's employer, purchased directly, and Medicaid or welfare source. If the respondent was married, the same set of questions on health insurance coverage was asked about the wife or husband. These questions allow me to identify the wives who had health insurance coverage through their husbands' employer-provided health insurance plan.

I am able to follow a sample of 20,396 married women from eight interviews of the NLSY over twelve years. An observation is included if (1) the respondent had an intact marriage from 1989 through 2000; (2) both the respondent and her husband were between the ages of 25 and 64; (3) information on the respondent's source of health insurance was available; and (4) the respondent was not covered by public health insurance. The resulting sample, after pooling all 8 years, includes 12,822 married women from the NLSY.

A second source of data I use is the March 2000 Annual Demographic Supplement to the Current Population Survey (CPS), mainly for comparison purposes. The March CPS provides information on demographic characteristics, employment, income, family structure, and health insurance that are comparable to those collected in the NLSY. Using information on both health insurance coverage and sources of that coverage, I can differentiate between wives with and without coverage through their husbands' employer-provided health insurance plan. The criteria used to select the CPS sample are similar to the NLSY: Married couples aged 25 to 64 who did not receive public health insurance. The final sample consists of 19,515 married women from the CPS.

I examine three alternative measures of labor supply: (1) working, a binary variable indicating labor force participation defined as positive hours per week; (2) full-time, a binary variable indicating hours worked per week is greater than or equal to 35; and (3) hours, usual hours worked per week.

Table 2.1 displays the three measures of married women's labor supply of by spousal health insurance coverage for both the CPS (left-hand panel) and the NLSY (right-hand panel). The top panel of Table 2.1 shows the labor supply of the entire sample of married women (includes both workers and non-workers), and the bottom panel shows the labor supply of working married women (a subsample of the entire sample of married women because it includes only women whose weekly hours of work are greater than zero).

The top left-hand panel in Table 2.1 suggests potential negative effects of spousal coverage on married women's labor supply in the CPS. Wives who were covered by their husbands' employer-provided health insurance (72.8 percent) were less likely to work than wives who were not (83.8 percent). When they worked (bottom left-hand panel of Table 2.1), they were much less likely to work full-time: 65.0 percent of wives with spousal coverage worked full-time, compared with 83.4 percent of wives without spousal coverage.

The negative correlation between married women's labor supply and spousal coverage is also apparent in the NLSY. The fraction working (top right-hand panel of Table 2.1) was much lower for wives with spousal coverage (73.8 percent) than for wives without spousal coverage (90.1 percent). Among working wives (bottom right-hand panel of Table 2.1), those who had spousal coverage were 24.5 percentage points less likely to work full-time (57.8 percent versus 82.7 percent).

Table 2.2 displays mean characteristics of the entire sample of married women by spousal coverage in the CPS (left-hand panel) and the NLSY (right-hand panel). Overall, the characteristics of married women in the two data sets are quite similar; however, there are some important differences between wives who have spousal coverage and wives who do not. Wives with spousal coverage are more likely to have children under age 6 and more children under age 18 than wives without spousal coverage. Also, they tend to be married to more educated husbands. For example, in the CPS, 64 percent of wives with spousal coverage are married to husbands with some college or more, compared with 54 percent of wives without coverage. (The fractions are 59 percent versus 46 percent in the NLSY).

Table 2.3 displays mean characteristics of the sample of working married women by spousal coverage in both the CPS and NLSY. The presentation is similar and findings are analogous to those in Table 2.2.

Table 2.1. Labor Supply of Married Women by Spousal Health Insurance Coverage, 2000 CPS and NLSY

	CPS		NLSY	
	Wife Covered	Wife Not covered	Wife Covered	Wife Not covered
All married women				
Working (%) (Weekly hours>0)	72.8	83.8	73.8	90.1
Full-time (%) (Weekly hours>=35)	47.3	69.9	42.7	74.5
Hours (weeks)	25.0	32.5	25.7	36.1
Number of observations	10,019	9,496	1,076	1,113
Working married women				
Full-time (%) (Weekly hours>=35)	65.0	83.4	57.8	82.7
Hours (weeks)	34.4	38.8	34.9	40.1
Number of observations	7,337	7,907	807	1,010

Notes: The data are from the March 2000 Annual Demographic Supplement to the Current Population Survey (CPS) and the 2000 interview of the 1979 National Longitudinal Survey of Youth (NLSY). Means are tabulated using CPS March supplement and NLSY weights.

Table 2.2. Summary Statistics for Married Women by Spousal Health Insurance Coverage, 2000 CPS and NLSY

	CPS			NLSY		
Variable	All	Wife Covered	Wife Not Covered	All	Wife Covered	Wife Not Covered
Spousal coverage	0.52	1	0	0.52	1	0
Labor supply measures						
Working	0.78	0.73	0.84	0.82	0.74	0.90
Full-time	0.58	0.47	0.70	0.58	0.43	0.74
Hours	28.61	25.04	32.55	30.67	25.74	36.10
Education						
Less than high school	0.09	0.07	0.11	0.05	0.05	0.05
High school	0.33	0.34	0.32	0.40	0.38	0.42
Some college	0.28	0.30	0.27	0.24	0.23	0.25
College	0.21	0.21	0.20	0.18	0.20	0.15
More than college	0.09	0.08	0.10	0.13	0.13	0.14
Experience	22.57	22.21	22.96	19.74	19.74	19.75
Race						
White	0.88	0.89	0.86	0.87	0.90	0.84
Black	0.07	0.06	0.08	0.07	0.06	0.09
Other	0.05	0.05	0.06	0.06	0.05	0.06
Presence of children under age 6	0.24	0.27	0.20	0.27	0.30	0.23
Number of children under age 18	1.09	1.24	0.92	1.83	2.02	1.61
Family nonwage income	4,089	4,468	3,671	5,277	6,080	4,415
Husband's education						
Less than high school	0.11	0.07	0.15	0.08	0.06	0.10
High school	0.30	0.29	0.32	0.39	0.35	0.44
Some college	0.26	0.27	0.25	0.21	0.21	0.22
College	0.21	0.22	0.19	0.17	0.21	0.12
More than college	0.12	0.14	0.10	0.15	0.17	0.12
Husband's experience	24.38	23.78	25.04	21.64	21.08	22.27
Number of observations	19,515	10,019	9,496	2,189	1,076	1,113

Notes: See Table 2.1.

Table 2.3. Summary Statistics for Working Married Women by Spousal Health Insurance Coverage, 2000 CPS and NLSY

	CPS			NLSY		
Variable	All	Wife Covered	Wife Not Covered	All	Wife Covered	Wife Not Covered
Spousal coverage	0.49	1	0	0.47	1	0
Labor supply measures						
Working	1	1	1	1	1	1
Full-time	0.74	0.65	0.83	0.71	0.58	0.83
Hours	36.68	34.41	38.85	37.60	34.89	40.06
Education						
Less than high school	0.07	0.05	0.08	0.04	0.04	0.04
High school	0.32	0.33	0.31	0.41	0.40	0.41
Some college	0.29	0.31	0.28	0.25	0.24	0.25
College	0.22	0.22	0.21	0.17	0.18	0.15
More than college	0.10	0.09	0.11	0.14	0.14	0.14
Experience	22.03	21.72	22.32	19.71	19.73	19.68
Race						
White	0.87	0.89	0.86	0.86	0.89	0.84
Black	0.08	0.07	0.08	0.08	0.06	0.10
Other	0.05	0.04	0.05	0.06	0.05	0.06
Presence of children under age 6	0.22	0.24	0.19	0.23	0.25	0.21
Number of children under age 18	1.04	1.19	0.89	1.71	1.90	1.55
Family nonwage income	3,990	4,428	3,572	4,799	5,545	4,146
Husband's education						
Less than high school	0.09	0.06	0.12	0.08	0.05	0.10
High school	0.31	0.30	0.32	0.41	0.37	0.44
Some college	0.27	0.29	0.26	0.22	0.22	0.23
College	0.21	0.22	0.20	0.16	0.21	0.12
More than college	0.12	0.14	0.10	0.13	0.15	0.11
Husband's experience	24.05	23.54	24.54	21.76	21.17	22.31
Number of observations	15,244	7,337	7,907	1,817	807	1,010

Notes: See Table 2.1.

2.5. Empirical Findings

This section presents estimates of the effect of husbands' health insurance coverage on wives' labor supply from three econometric approaches. These include (1) cross-sectional estimates from linear probability and probit models; (2) cross-sectional instrumental variable estimates; and (3) panel estimates.

2.5.1 Cross-Sectional LPM and Probit Estimates

Table 2.4 displays the estimated marginal effects of spousal coverage on labor force participation of married women from linear probability and probit models [equation (2.1)]. The left-hand panel in Table 2.4 shows the estimated marginal effects for married women in the CPS, and the right-hand panel shows the estimated marginal effects for married women in the NLSY. Controls include wives' personal characteristics (education, experience, experience squared, and race); husbands' personal characteristics (education, experience, and experience squared); and family characteristics (presence of children under age 6, number of children under age 18, region of residence, and family non-wage income).

The estimate of main interest is the marginal effect of spousal coverage on labor force participation. This is essentially similar in size and statistical significance in both models and both data sets. The estimated marginal effect, about -0.11, suggests that spousal health insurance coverage reduces wives' probability of participation by 11 percentage points. This represents a reduction of 12 percent compared with wives who do not have spousal coverage.

These participation estimates are very similar to those obtained by Buchmueller and Valletta (1999) and by Olson (1998), who estimated that spousal coverage reduced married women's labor supply by 12 percent and 11 percent respectively. However, they are smaller than the 23 percent reduction (19.5 percentage points) estimated by Wellington and Cobb-Clark (2000).

Table 2.5 displays the estimated marginal effects of spousal coverage on full-time employment of working married women from linear probability and probit models. The estimates are conditional on positive labor force participation. The controls used in the estimation are the same as those in Table 2.4. In both the CPS and NLSY, the estimated marginal effect of spousal coverage is about -0.16, which suggests that the probability of working full-time is 16 percentage points (22 percent) lower for working wives with spousal coverage than for working wives without spousal coverage.

2.5.2 Cross-Sectional IV Estimates

Table 2.6 shows the coefficients on spousal coverage in three labor supply models estimated by OLS and 2SLS using data from the CPS. Because coefficients of control variables are of limited interests, they are not reported. The controls included are the same as before (see the table notes). Again, three alternative measures of labor supply are used as dependent variables: (1) a binary variable indicating labor force participation (working); (2) a binary variable indicating hours worked per week is greater than or equal to 35 (full-time); and (3) usual hours worked per week (hours).

The instruments for spousal coverage are a dummy variable indicating whether the husband is self-employed and a dummy variable indicating whether the husband works part-time. This choice of instruments is based on the assumption that a wife's coverage by her husband's employer-provided health insurance is negatively correlated with the husband's having a part-time job or being self-employed, but unrelated with

Table 2.4. Linear Probability and Probit Estimates of Married Women's Labor Force Participation, 2000 CPS and NLSY

Dependent Variable: *working*	CPS		NLSY	
Variable	**LPM**	**Probit**	**LPM**	**Probit**
Spousal coverage	-0.103	-0.109	-0.106	-0.115
	(0.006)	(0.006)	(0.018)	(0.017)
High school	0.117	0.087	0.148	0.112
	(0.013)	(0.010)	(0.045)	(0.030)
Some college	0.168	0.132	0.160	0.112
	(0.014)	(0.010)	(0.048)	(0.027)
College	0.195	0.149	0.182	0.121
	(0.015)	(0.009)	(0.054)	(0.022)
More than college	0.249	0.172	0.262	0.138
	(0.017)	(0.007)	(0.061)	(0.017)
Experience	0.013	0.011	-0.009	-0.012
	(0.002)	(0.002)	(0.025)	(0.023)
Experience squared	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.001)	(0.001)
Black	0.053	0.056	0.046	0.048
	(0.011)	(0.012)	(0.022)	(0.021)
Other race	-0.036	-0.041	0.046	0.045
	(0.014)	(0.015)	(0.023)	(0.020)
Midwest	0.034	0.039	0.039	0.038
	(0.008)	(0.008)	(0.028)	(0.023)
South	-0.016	-0.016	-0.005	-0.011
	(0.008)	(0.009)	(0.026)	(0.024)
West	-0.018	-0.016	-0.008	-0.011
	(0.009)	(0.009)	(0.030)	(0.028)
Presence of children under age 6	-0.106	-0.119	-0.117	-0.120
	(0.009)	(0.010)	(0.023)	(0.024)
Number of children under age 18	-0.036	-0.035	-0.035	-0.033
	(0.003)	(0.003)	(0.008)	(0.007)
Family nonwage income	0.005	0.005	-0.002	-0.001
	(0.001)	(0.001)	(0.000)	(0.000)

Table 2.4. (cont'd)

Dependent Variable: *working*	CPS		NLSY	
Variable	**LPM**	**Probit**	**LPM**	**Probit**
Husband's education				
High school	0.022	0.021	0.011	0.010
	(0.012)	(0.011)	(0.030)	(0.030)
Some college	0.023	0.022	0.019	0.022
	(0.012)	(0.012)	(0.033)	(0.032)
College	-0.046	-0.054	0.009	0.004
	(0.014)	(0.015)	(0.040)	(0.038)
More than college	-0.092	-0.117	-0.046	-0.049
	(0.016)	(0.019)	(0.045)	(0.050)
Husband's experience	0.001	0.001	0.000	0.000
	(0.002)	(0.002)	(0.007)	(0.007)
Husband's experienced squared	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Number of observations	19,515	19,515	1,763	1,763
R-squared	0.103	0.099	0.141	0.161

Notes: The data are from the March 2000 Annual Demographic Supplement to the Current Population Survey (CPS) and the 2000 interview of the 1979 National Longitudinal Survey of Youth (NLSY). Figures are estimated changes in the probability of labor force participation from linear probability and probit models. Robust standard errors are in parentheses. The R-squared for the probits is the pseudo-R-squared.

Table 2.5. Linear Probability and Probit Estimates of Working Married Women's Full-Time Work, 2000 CPS and NLSY

Dependent Variable: *full-time*	CPS		NLSY	
Variable	**LPM**	**Probit**	**LPM**	**Probit**
Spousal coverage	-0.153	-0.156	-0.169	-0.174
	(0.007)	(0.007)	(0.023)	(0.024)
High school	-0.044	-0.043	0.083	0.085
	(0.015)	(0.017)	(0.055)	(0.055)
Some college	-0.031	-0.029	0.053	0.059
	(0.016)	(0.018)	(0.059)	(0.058)
College	-0.010	-0.006	0.052	0.061
	(0.018)	(0.020)	(0.068)	(0.063)
More than college	0.059	0.066	0.113	0.111
	(0.020)	(0.020)	(0.076)	(0.059)
Experience	-0.001	-0.002	-0.035	-0.036
	(0.002)	(0.002)	(0.028)	(0.032)
Experience squared	0.000	0.000	0.001	0.001
	(0.000)	(0.000)	(0.001)	(0.001)
Black	0.128	0.133	0.097	0.104
	(0.012)	(0.012)	(0.027)	(0.028)
Other race	0.107	0.104	0.115	0.116
	(0.015)	(0.014)	(0.028)	(0.025)
Midwest	0.025	0.024	-0.005	-0.007
	(0.010)	(0.010)	(0.037)	(0.036)
South	0.068	0.068	0.067	0.069
	(0.010)	(0.010)	(0.032)	(0.032)
West	0.023	0.022	-0.080	-0.095
	(0.011)	(0.010)	(0.039)	(0.042)
Presence of children under age 6	-0.039	-0.047	-0.036	-0.037
	(0.011)	(0.011)	(0.028)	(0.029)
Number of children under age 18	-0.057	-0.057	-0.033	-0.035
	(0.004)	(0.004)	(0.010)	(0.010)
Family nonwage income	-0.002	-0.002	-0.002	-0.002
	(0.001)	(0.001)	(0.000)	(0.000)

Table 2.5. (cont'd)

Dependent Variable: full-time	CPS		NLSY	
Variable	**LPM**	**Probit**	**LPM**	**Probit**
Husband's education				
High school	-0.006	-0.006	-0.010	-0.003
	(0.013)	(0.015)	(0.038)	(0.044)
Some college	-0.025	-0.025	0.035	0.041
	(0.014)	(0.016)	(0.042)	(0.047)
College	-0.068	-0.070	-0.045	-0.037
	(0.016)	(0.019)	(0.051)	(0.058)
More than college	-0.107	-0.117	-0.020	-0.008
	(0.018)	(0.023)	(0.055)	(0.060)
Husband's experience	0.003	0.003	0.000	-0.002
	(0.002)	(0.002)	(0.009)	(0.011)
Husband's experienced squared	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Number of observations	15,244	15,244	1,473	1,473
R-squared	0.083	0.076	0.142	0.131

Notes: The data are from the March 2000 Annual Demographic Supplement to the Current Population Survey (CPS) and the 2000 interview of the 1979 National Longitudinal Survey of Youth (NLSY). Figures are estimated changes in the probability of full-time work from linear probability and probit models. Robust standard errors are in parentheses. The R-squared for the probits is the pseudo-R-squared.

unobservable factors affecting the wife's labor supply decisions. The latter of these assumptions is tenuous because the husband's having a part-time job or being self-employed is likely correlated with the wife's preferences for work if there is spousal sorting and selection. However, the purpose is to see whether an IV approach that has been used in the past yields plausible findings.

The top panel of Table 2.6 displays the estimated effects of spousal coverage on the participation of all married women. The OLS estimate repeats the main finding from Table 2.4. The 2SLS estimate is essentially similar (-0.091). As expected, the standard error of the 2SLS estimate is larger than the OLS standard error (0.024 versus 0.006), but the estimate is still significant at conventional levels (*p*-value= 0.000).

In the model of working married women's full-time work (middle panel of Table 2.6), the OLS estimate is -0.153, which suggests that spousal coverage reduces wives' probability of full-time work by 15.3 percentage points (a 21.9 percent reduction). But the estimate is quite different when we use 2SLS: The estimated spousal effect is now 0.236 (*p*-value= 0.000). Similarly, in the hours equation for working married women (bottom panel of Table 2.6), the OLS estimate (-3.772) suggests that wives with spousal coverage work almost 4 hours less per week than wives without spousal coverage (*p*-value= 0.000), but 2SLS estimate is 3.413 with a *p*-value of 0.000.

Table 2.7 shows the estimated effect of spousal coverage on labor supply for women in the NLSY. The presentation is similar to that of Table 2.6. Because information on whether the husband is self-employed is not available in the NLSY, husbands' part-time work status is the only available instrument for spousal coverage. This, however, does not affect the main finding: Once again, the OLS and 2SLS estimates differ in significant ways.

That OLS and 2SLS estimations produce such different results suggests the importance of testing for the endogeneity of spousal coverage in the labor supply equations. The regression-based Hausman test that compares OLS and 2SLS estimates (see Wooldridge 2002, Section 6.2.1) is well-suited for this. Heteroskedasticity-robust

Hausman test statistics are given in both Tables 2.6 and 2.7 (see 'Hausman test'). The test statistic suggests strong evidence of endogeneity of spousal coverage in the three labor supply models for both samples in both data sets (p-values = 0.000). This suggests in turn that 2SLS is needed for consistent estimation provided the validity of the instruments, which is what we consider next.

Because the 2SLS estimation in the CPS uses two instruments for spousal coverage, there is one overidentifying restriction. The overidentifying restriction can thus be tested in the model estimated using the CPS data, but not when using the NLSY. In order to determine the validity of the instruments, I use the Sargan statistic. This is calculated as N×R-squared from a regression of the IV residuals on the full set of instruments (Wooldridge 2002, Section 6.2.2; Baum, Schaffer, and Stillman 2003). The null hypothesis is that the variables used as instruments for spousal coverage (husband's self-employment and part-time work) are uncorrelated with unobservables affecting wives' labor supply.

Table 2.6 displays the results. In the model of married women's participation, the Sargan test rejects the hypothesis of instrument validity (p-value = 0.039). For the sample of working married women, the instruments pass the overidentification test in the full-time work equation, which suggests that the instruments are valid (p-value = 0.728), but in the hours equation, we reject the validity assumption of the instruments at the 10-percent significance level (p-value = 0.089).

That Sargan test produces inconsistent results is not actually surprising because it evaluates the entire set of overidentifying restrictions, but requires that at least some of the instruments be valid. However, as discussed before, using variables for husband's

Table 2.6. OLS and 2SLS Estimates of the Effect of Spousal Coverage on Labor Supply: 2000 CPS

All married women **Dependent variable:** ***working***	**OLS**	**2SLS**
Spousal coverage	-0.103	-0.091
	(0.006)	(0.024)
First stage F-statistic		675.38
[p-value]		[0.000]
Hausman test		-0.103
[p-value]		[0.000]
Sargan statistic		4.269
[p-value]		[0.039]
Working married women **Dependent variable:** ***full-time***	**OLS**	**2SLS**
Spousal coverage	-0.153	0.236
	(0.007)	(0.033)
First stage F-statistic		575.42
[p-value]		[0.000]
Hausman test		-0.177
[p-value]		[0.007]
Sargan statistic		0.121
[p-value]		[0.728]
Working married women **Dependent variable:** ***hours***	**OLS**	**2SLS**
Spousal coverage	-3.722	3.413
	(0.186)	(0.934)
First stage F-statistic		575.42
[p-value]		[0.000]
Hausman test		-4.160
[p-value]		[0.000]
Sargan statistic		2.889
[p-value]		[0.089]

Notes: The data are from the March 2000 CPS. Robust standard errors are in parentheses. In brackets are p-values. All models include wives' characteristics; husbands' characteristics; and family characteristics as controls. The instruments for spousal coverage include a dummy variable indicating whether the husband is self-employed and a dummy variable indicating whether the husband is working part-time. The first stage F-statistic is a test statistic for joint significance of the instruments in the first stage regression of spousal coverage on the exogenous variables and instruments. The Hausman test is a regression-based Hausman test. The Sargan statistic is a test of overidentifying restrictions.

Table 2.7. OLS and 2SLS Estimates of the Effect of Spousal Coverage on Labor Supply: 2000 NLSY

All married women **Dependent variable: *working***	**OLS**	**2SLS**
Spousal coverage	-0.106	0.083
	(0.018)	(0.288)
First stage t-statistic		-2.820
[p-value]		[0.005]
Hausman test		-0.103
[p-value]		[0.000]
Working married women **Dependent variable: *full-time***	**OLS**	**2SLS**
Spousal coverage	-0.169	0.094
	(0.023)	(0.331)
First stage t-statistic		-2.980
[p-value]		[0.003]
Hausman test		-0.171
[p-value]		[0.000]
Working married women **Dependent variable: *hours***	**OLS**	**2SLS**
Spousal coverage	-3.082	11.740
	(0.711)	(11.503)
First stage t-statistic		-2.980
[p-value]		[0.003]
Hausman test		-2.765
[p-value]		[0.000]

Notes: The data are from the 2000 interview of the 1979 National Longitudinal Survey of Youth (NLSY). Robust standard errors are in parentheses. In brackets are p-values. All models include wives' personal characteristics (education, experience, experience squared, and race); husbands' personal characteristics (education, experience, and experience squared); and family characteristics (presence of children under age 6, number of children under age 18, region of residence, and family non-wage income) as controls. The instrument for spousal coverage is a dummy variable indicating whether the husband is working part-time. The first stage t-statistic is a test statistic for the significance of the instrument in the first stage regression of spousal coverage on the exogenous variables and the instrument. The Hausman test is a regression-based Hausman test. Sample sizes are 1,763 (all married women) and 1,472 (working married women).

work status as instruments for spousal coverage raises concerns because the husband's having a part-time job or being self-employed is likely correlated with the wife's preferences for work due to spousal sorting and selection.

Given that cross-sectional IV estimation does not provide a solution to the endogeneity problem of spousal coverage, I now turn to panel estimates.

2.5.3 Panel Estimates

Table 2.8 shows estimates of the spousal coverage effect using the NLSY. Unlike the estimates in sections 2.5.1 and 2.5.2, these estimates take advantage of the panel nature of the NLSY and use the 1989, 1990, 1992, 1993, 1994, 1996, 1998, and 2000 interviews of the NLSY. Table 2.8 displays estimates of four different models: (1) pooled ordinary least squares (POLS); (2) random effects (RE); (3) fixed effects (FE); and (4) first differencing (FD). The POLS and RE models include a full set of year dummies, wives' personal characteristics (education, experience, experience squared, and race); husbands' personal characteristics (education, experience, and experience squared); and family characteristics (presence of children under age 6, number of children under age 18, region of residence, and family non-wage income) as controls. The FE and FD models include the same controls, except race, which is not time varying.

The POLS estimates of the effect of spousal coverage are given in column 1 of Table 2.8. The reported standard errors are robust to serial correlation and heteroskedasticity. The estimated effect of spousal coverage is negative and statistically significant in the three labor supply models for both samples of married women. The results suggest that married women with spousal coverage are 9.4 percentage points (14 percent) less likely to be working, and those who work are 18.4 percentage points (28 percent) less likely to work full time. In the hours equation for working married women, the estimated effect of spousal coverage is -3.790 (p-value = 0.000), which suggests that married women with spousal coverage work almost 4 hours less per week (a 13 percent reduction) than married women without spousal coverage.

Column 2 in Table 2.8 displays the random effects (RE) estimates. The estimated spousal coverage effects are still negative and statistically significant but they are smaller in absolute value than the POLS estimates. Thus, it seems that controlling for random unobserved effects (assuming they are uncorrelated with explanatory variables) diminishes the negative effect of spousal coverage on wives' labor supply.

The fixed effects (FE) model in column 3 produces estimated coefficients for spousal coverage that are even smaller in absolute value. The FE estimates are about half the size of the RE estimates. For example, spousal coverage is estimated to reduce weekly hours worked by 1.6 hours for working married women, compared with 2.5 hours in the RE model.

Because the key assumption underlying the consistency of RE is whether unobserved effects and the explanatory variables are correlated, I test this assumption using a Hausman test that compares random and fixed effects estimates (see Wooldridge 2002, Section 10.7.3). The test strongly rejects (p-value = 0.000) the hypothesis that unobserved effects are uncorrelated with spousal coverage in the three labor supply models for both samples of married women. This suggests that the random effects estimators are inconsistent but the fixed effect estimator is consistent conditional on strict exogeneity of the explanatory variables.

Table 2.8. Panel Estimates of the Effect of Spousal Coverage on Labor Supply: 1989-2000 NLSY

All married women **Dependent variable:** ***working***	**(1) POLS**	**(2) RE**	**(3) FE**	**(4) FD**
Spousal coverage	-0.094	-0.051	-0.026	-0.005
	(0.009)	(0.007)	(0.009)	(0.009)
Hausman test		125.16		
[p-value]		[0.000]		
Strict exogeneity test			-0.020	
[p-value]			[0.120]	
Number of observations	12,888	12,888	12,888	8,543
Working married women **Dependent variable:** ***full-time***	**(1) POLS**	**(2) RE**	**(3) FE**	**(4) FD**
Spousal coverage	-0.184	-0.132	-0.088	-0.052
	(0.012)	(0.009)	(0.013)	(0.012)
Hausman test		99.79		
[p-value]		[0.000]		
Strict exogeneity test			-0.036	
[p-value]			[0.280]	
Number of observations	10,584	10,584	10,584	7,072
Working married women **Dependent variable:** ***hours***	**(1) POLS**	**(2) RE**	**(3) FE**	**(4) FD**
Spousal coverage	-3.790	-2.504	-1.596	-0.989
	(0.318)	(0.238)	(0.338)	(0.380)
Hausman test		82.81		
[p-value]		[0.000]		
Strict exogeneity test			-0.883	
[p-value]			[0.196]	
Number of observations	10,584	10,584	10,584	7,072

Notes: The data are from the 1989-2000 interviews of the 1979 National Longitudinal Survey of Youth (NLSY). Robust standard errors are in parentheses. In brackets are p-values. POLS and RE models include a full set of year dummies, wives' personal characteristics (education, experience, experience squared, and race); husbands' personal characteristics (education, experience, and experience squared); and family characteristics (presence of children under age 6, number of children under age 18, region of residence, and family non-wage income) as controls. FE and FD models include the same controls, except race, which is not time varying. Hausman test is based on the comparison of estimates obtained from the RE and FE models. Strict exogeneity test is the test for feedback effects from dependent variable to future values of explanatory variables.

Finally, estimates obtained using first differencing (FD) are given in column 4 of Table 2.8. The FD estimates are even smaller in absolute value than the FE estimates but generally the same order of magnitude. To choose between FE and FD, I test whether the differenced errors are serially uncorrelated. The results indicate that there is substantial negative serial correlation in the differenced errors ($\hat{\rho} \approx -0.30$ with p-value = 0.000), suggesting fixed effects is more efficient than first differencing (Wooldridge 2002, Section 10.7.1).

As mentioned above, consistency of FE estimates requires that spousal coverage be strictly exogenous with respect to time-varying unobserved effects—u_{it} in equation (2.2), after accounting for the individual effect—c_i in equation (2.2). To test for feedback effects from wives' labor supply to future values of spousal coverage, I generate the lead of the spousal coverage variable and use it as a regressor in the FE model (Wooldridge 2002, Section 10.7.1). The estimated leads of spousal coverage are insignificant in the three labor supply models for both samples of married women. This suggests that there is no evidence against strict exogeneity of spousal coverage after netting out the individual effect using the FE.

2.6. Conclusion

The empirical analysis in this paper yields three main findings. First, there is strong evidence that spousal coverage is endogenous to the labor supply of married women (section 2.5.2). This results from the simultaneity of wives' labor supply decisions and the health insurance status of their husbands. Second, cross-sectional instrumental variables estimation does not provide a viable solution to the endogeneity problem (section 2.5.2). The close link between health insurance and labor supply makes it difficult to identify suitable instruments that are correlated with wives' coverage by their husbands' health insurance, but unrelated with wives' labor supply decisions. This is confirmed by the results of the test for overidentifying restrictions (see 'Sargan test' presented in Table 2.6), that reject the hypothesis of instrument validity. Third, once unobserved heterogeneity is controlled, spousal coverage has a smaller negative effect on wives' labor supply (section 2.5.3).

Specifically, it appears that controlling for sorting and selection of married couples diminishes the negative effect of spousal coverage found in cross-sections. The FE estimates in Table 2.8 suggest that spousal coverage reduces wives' probability of participation by 7.7 percent. Conditional on working, spousal coverage is estimated to reduce the probability wives' work full-time by 15 percent, and their hours of work by 6.5 percent. In contrast, the cross-sectional estimates, which are similar to those estimated by Buchmueller and Valletta (1999), Olson (1998, 2000), and Wellington and Cobb-Clark (2000), suggest that spousal coverage reduces wives' probability of participation by 14 percent; working wives' probability of full-time work by 28 percent; and their hours of work by 13 percent.

The results have potentially important implications for the debate over health care reform in the United States. A major goal of most proposed reforms is to expand access to health care and health insurance coverage. Doing this generally entails uncoupling health insurance from employment, as would happen under universal single-payer health care, or at least weakening the link, as occurs under a plan like that adopted by Massachusetts in 2007. The question is whether the divorce of health insurance from employment would reduce labor supply of workers who currently work (or have adjusted their work hours) so as to acquire health insurance. The results of the present analysis suggest that universal coverage would reduce the labor supply of married women, but not as significantly as estimated by previous work.

CHAPTER 3

HEALTH INSURANCE TAX CREDITS AND HEALTH INSURANCE COVERAGE OF LOW-INCOME SINGLE MOTHERS

3.1. Introduction

Between 2000 and 2006, the percentage of the U.S. nonelderly population without health insurance coverage gradually rose from 15.6 percent to 17.9 percent (Fronstin 2007). Dissatisfaction with this level and growth of uninsurance has spurred interest in reforming the existing system of financing health care, which is dominated among the nonpoor and nonelderly by employer-provided health insurance. One approach, described by Pauly (1999) and Cogan, Hubbard, and Kessler (2005) among others, is to adopt a refundable health insurance tax credit (HITC) under the federal personal income tax. Such a policy would grant a tax credit up to a prespecified maximum — for example, $1,000 for an individual or $2,000 for a family — on a tax return where the filer purchased a private health insurance policy (either provided by an employer or purchased in the market).

An HITC would reduce the price of employer-provided health insurance and in addition would extend the tax-favored treatment of health insurance to individuals who do not have access to employer-provided health insurance. Accordingly, it would be expected to increase the percentage of individuals and families covered by private health insurance. But the extent to which the HITC would reduce the number of uninsured individuals has been controversial. Pauly and Herring (2001, 2002), Pauly, Song, and Herring (2001), and Wozniak and Emmons (2000) simulated a variety of HITC policies and found that a "reasonably generous" credit could reduce the number of uninsured individuals by roughly 50 percent. However, simulations by Gruber (2000a, b) and Gruber and Levitt (2000) suggested that the HITC might reduce the number of uninsured by only about 10 percent. Emmons, Madly, and Woodbury (2005) replicated Gruber's simulation model and found (as is often true) that relatively minor changes in assumptions could result in substantial changes in simulated impacts of the HITC. Their conclusions echoed those of Pauly, Song, and Herring (2001): simulations of the impact of health insurance tax credits are highly uncertain because little empirical evidence exists to guide modelers in choosing appropriate behavioral assumptions.[10]

Two strands of empirical literature do consider the effects of tax subsidies and tax credits for health insurance. The first is a rather large literature — reviewed by Cutler and Zeckhauser (2000) — examining how sensitive employees are to out-of-pocket premiums when they select employer-provided health insurance. This literature suggests employees are highly sensitive to premiums when they choose plans; for example, Cutler and Reber (1998) estimate an elasticity of plan take-up with respect to the employee premium of about -0.2.

A second (much smaller) empirical literature has examined the responsiveness of employee take-up of health insurance to changes in employee premiums. Chernew, Frick, and McLaughlin (1997) and Blumberg, Nichols, and Banthin (2001) both examined matched employer-employee data, and both estimated the elasticity of insurance take-up with respect to premiums to be less than -0.1. Gruber and Washington (2005) examined a change in the tax treatment of federal employees' health insurance premiums in which

[10] Pauly, Song, and Herring (2001) also note that health insurance tax credits could lead to broader changes in health insurance markets, including greater price competition among insurers, that are not accounted for in simulation models.

the employee's share, previously paid with after-tax earnings, became payable with pre-tax earnings. Their main finding is that federal employees' take-up of health insurance was minimally responsive to the change in subsidy.

Two points about the existing empirical evidence are worth noting. First, the existing studies offer evidence on the effect of a subsidy to health insurance (at the marginal tax rate), or on the effect of different premiums on health plan selection, rather than evidence on the effect of a tax credit that reimburses an individual dollar-for-dollar for health insurance premium payments. Second, the worker populations studied are quite heterogeneous; that is, the studies focus on workers whose earnings range widely, rather than on low-wage workers. As a result, it is difficult to draw strong conclusions about the effects of an HITC on health insurance coverage from existing research.

In this paper, we attempt to obtain direct evidence on the impact of tax credits on health insurance coverage of low-earnings workers by examining the impact of a supplemental tax credit that Congress added to the Earned Income Tax Credit (EITC) during 1991, 1992, and 1993. This policy provided a refundable tax credit of up to $428 in 1991 ($451 in 1992, and $465 in 1993) to EITC-eligible households that bought health insurance for a qualifying child. We treat this supplemental credit as a natural experiment and estimate its impact on the health insurance coverage of single mothers using a standard difference-in-differences approach applied to Current Population Survey data. We first describe the tax credit, the approach to estimation, and the data we use. We then present the main findings, followed by several sensitivity tests and a discussion of possible alternative explanations of the findings.

3.2. The Health Insurance Tax Credit, 1991-1993

When Congress passed the Omnibus Budget Reconciliation Act (OBRA) of 1990, it added a supplemental credit for health insurance purchases to the basic Earned Income Tax Credit (EITC) program (U.S. Government Accountability Office 1991, 1993). This HITC was a refundable tax credit for low-income workers with one or more children who bought health insurance — either employer-provided or private nongroup — covering the child or children. The credit offset only the cost of health insurance and did not cover co-payments, deductibles, or out-of-pocket health expenses. To encourage participation, the credit was refundable, so taxpayers with no federal income tax liability could still receive a payment from the Internal Revenue Service. The HITC was repealed effective December 31, 1993, so it was available only during tax years 1991, 1992, and 1993.[11]

The HITC had the same eligibility criteria as the EITC: To receive a credit, a household needed to have earnings and a qualifying child. To qualify, a child needed to meet three requirements: (1) be a child, stepchild, grandchild, or foster or adopted child of the taxpayer; (2) have the same place of residence as the taxpayer for more than half the tax year; and (3) be under age 19 (or 24 if a full-time student) or be permanently disabled. Unlike the basic EITC, the HITC remained the same regardless of the number of qualifying children in the family.

The HITC schedule closely followed the EITC's. (Appendix 1, Table A1.1 gives details of the HITC schedules and of the EITC schedules in the years before, during, and after the HITC existed. Figure A1.1 in Appendix 1 is a graphical representation of the HITC and EITC schedules in 1991). In 1991, a taxpayer with earnings and a qualifying

[11] See U.S. Government Accountability Office (1991, 1993, and 1994) for discussions of why Congress eliminated the credit.

child could receive a credit up to $428 if he or she bought private health insurance that covered the child. For households with earned incomes of $1 to $7,140, the credit was 6 percent of earned income. For households with earnings between $7,140 and $11,250, the credit was $428 (6 percent of $7,140). For households with earnings between $11,250 and $21,250, the credit phased out at a rate of 4.28 percent per marginal dollar earned and fell to $0 for earnings at or above $21,250 (see Appendix 1, Figure A1.1). In 1991, the maximum HITC of $428 was 36 percent of the maximum EITC of $1,192. Like the EITC schedule, the HITC schedule was indexed to inflation.

In 1991, the HITC's first year, the average credit was $233, or 23 percent of the reported average annual health insurance premium of $1,029. Also in 1991, 2.3 million taxpayers received health insurance credits of $496 million (U.S. Government Accountability Office 1991). A U.S. Government Accountability Office (GAO) study (1994) estimated that the take-up rate for the HITC in 1991 (its first year) was in the range of 19 to 26 percent. In contrast, the take-up rate for the regular EITC was 80 to 86 percent. The GAO attributed the relatively low HITC take-up rate to two factors. First, interviews with taxpayers at IRS service sites in six cities suggested that fewer than 30 percent of EITC-eligible taxpayers knew the credit existed. Second, the GAO suggested the credit was too modest to induce low-income workers to buy health insurance.

The GAO's findings appear to have played a role in persuading Congress to eliminate the HITC in 1993 (effective 1994). However, as the evidence below suggests, the implicit conclusion that the HITC was ineffective may have been premature. Given the subsequent attention that has been paid to tax credits for health insurance, it is curious that no analysis of the HITC's impact on health insurance coverage appears to exist.

3.3. Approach to Estimation

We treat the HITC as a natural experiment and adopt a difference-in-differences approach to estimating its effects on the health insurance coverage of low-education working single mothers. The approach follows a large literature on the labor supply effects of the EITC including Eissa and Leibman (1996), Eissa and Hoynes (2004), Hotz, Mullin and Scholz (2005) and Meyer and Rosenbaum (2000).

The population potentially affected by the HITC was low-income working families with children. If the HITC had any effect on private health insurance coverage, then the coverage of low-income working families with children would have been greater than otherwise between 1991 and 1993. For three reasons, we focus on the HITC's possible effect on private health insurance of working single mothers with less than a high school education. First, working single mothers were roughly 44 percent of all EITC-eligible households in 1990, making them the largest group of taxpayers eligible for the EITC and hence for the HITC (Liebman 2000). Second, for households headed by a single woman, we can plausibly ignore decisions made jointly with other family members (Eissa and Liebman 1996). Third, by focusing on high school dropouts, we can estimate the effect of the HITC on a group that is likely to be eligible (because it is likely to have low earnings) without conditioning explicitly on income or earnings. Conditioning on income or earnings is ruled out because the EITC creates incentives for earners to change their hours of work so as to qualify for the credit (Eissa and Hoynes 2005). Accordingly, our main treatment group is low-education working single mothers.

A convincing difference-in-differences approach requires a comparison or "control" group that is as similar as possible to the treatment group without being eligible for the HITC. In particular, the treatment and control groups should (1) face common underlying trends in economic conditions and policies (other than the HITC) that would be expected to affect health insurance coverage and (2) be essentially comparable in the way they could be expected to respond to changes in economic incentives (Angrist and Krueger 1999, Blundell and MaCurdy 1999, and Meyer 1995). Following Eissa and Liebman's (1996) line of reasoning, we use *working single women without children and with less than a high school education* as the control group. Because they do not have children, these women are ineligible for the HITC, but they should face essentially similar labor markets, tax policy (apart from the HITC), and other economic conditions as low-education working single mothers (the treatment group).

Two concerns invariably arise with the difference-in-differences approach. First, if the treatment and control groups do differ in their characteristics, each may be affected differently by contemporaneous shocks (other than the HITC). In this case, the difference-in-differences approach may still be valid if we can control convincingly for observables that capture characteristics of individuals that are likely correlated with health insurance coverage. Second, the difference-in-differences estimator may be contaminated if the compositions of the treatment and control groups change over time. In the present case, substantial changes in tax and welfare programs affected single mothers and increased their work incentives between 1984 and 1996 (Meyer and Rosenbaum 2000, 2001). If single mothers who entered the labor force in later years were more likely to work part-time and hence less likely to have employer-provided health insurance, then changes in the characteristics of single mothers would have blunted any rise in private health insurance that may have occurred as a result of the HITC. We can again mitigate this problem by controlling for observable characteristics using regression, but it will also be important to examine the samples carefully to see whether and how much they did in fact change over time.

The model we estimate can be written:

$$\Pr(ins_i = 1 | \bullet) = F[\beta_0 + \beta_1\, treatment_i + \beta_2\, HITC_t + \beta_3\, treatment_i \times HITC_t + X_i \beta] \qquad (3.1)$$

where i indexes individuals and t indexes years;

ins is a binary indicator of private health insurance coverage;

treatment equals one for a working single woman with a dependent child and less than a high school education, and zero otherwise;

HITC equals one for 1991, 1992, and 1993 (years during which the HITC was in effect), and zero for 1988, 1989, and 1990 (years before the HITC was in effect); and

treatment×*HITC* captures the change in coverage rates for working single mothers, relative to working single women without children, after the HITC took effect.

In the basic specification we estimate, the vector of controls X includes age, indicators of race (white, black, and other), number of children under age 6, number of children aged 6-18, and number of children aged 19-24 *and* a full-time student, indicators of work status (full-time/full-year, full-time/part-year, part-time/full-year, and part-time/part-year),[12] earned income,[13] and unearned income.[14] For some of the specification

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[12] Full-year work implies at least 50 weeks of work in the previous year. Full-time work implies usual weekly hours of 35 or more in the previous year.
[13] Earned income includes income from wages, salaries, and self-employment.
[14] Unearned income includes income from unemployment compensation, worker's compensation, social security or railroad retirement, supplemental security, public assistance or welfare, veteran payments, survivors benefits, disability, retirement funds, interest, dividends, rent, educational assistance, child support, alimony, contributions, financial assistance from friends, and other nonearnings.

tests reported in Table 3.8, we include additional controls. We let F denote the standard normal cumulative density and estimate equation (3.1) as a probit.

3.4. Data

We estimate equation (3.1) using data from the March 1989-1994 Annual Demographic Supplements to the Current Population Survey (CPS), which provide information for tax years 1988 through 1993. Respondents to the March 1989, 1990, and 1991 CPS constitute a before-HITC sample (tax years 1988, 1989, and 1990).

Respondents to the March 1992, 1993, and 1994 CPS constitute a during-HITC sample (tax years 1991, 1992, and 1993). The relevant unit of observation is the tax-filing unit, which in the CPS implies allocating primary families and subfamilies to separate tax-filing units.

The sample includes women aged 19 to 44 who worked (had annual hours greater than zero), were single (widowed, divorced, or never married), and had less than a high school education. We exclude women who reported negative earnings, those in school full-time, those who were separated from their spouse, and those who reported being ill or disabled. The resulting sample, after pooling all six years, includes 3,661 observations.

We allocate working single women with at least one dependent child to the treatment group, and working single women without a child to the control group. We consider any child in the tax-filing unit who was under age 19 (or under age 24 if a full-time student) to be a dependent child for tax purposes. Consistent with the literature on the EITC, we do not try to impose the support or residency test for the HITC eligibility.[15]

[15] This is mainly due to limitations of the CPS. However, using data from the SIPP and IRS, Scholz (1994) shows that the support test does not greatly change estimates of EITC eligibility.

We examine the six-year period — 1988 through 1993 — for two reasons. First, when Congress passed the OBRA of 1993, which repealed the HITC, it enacted the largest expansion of the EITC in the credit's history (see Appendix 1, Table A1.1; Baughman and Dickert-Conlin 2003). In 1993, a mother of one child with earnings up to $7,750 could receive a credit of 18.5 percent of earned income, resulting in a maximum credit of $1,434. In 1994, the credit rate rose to 26.3 percent of earned income, resulting in a maximum credit of $2,038. Also beginning in 1994, eligibility for the credit was expanded to include families with no children. For these families, the credit was 7.65 percent of earnings up to $4,000, resulting in a maximum 1994 credit of $306. As a result, the EITC expansion of 1994 through 1996 makes it impossible to separate the effect of eliminating the HITC from that of expanding the EITC.

The second reason for choosing the period between 1988 and 1993 is that the CPS remained unchanged throughout this period. In March 1988, the Bureau of Labor Statistics modified the CPS health insurance questions to capture more accurately the insurance coverage of dependents.[16] The next important revisions to the CPS health insurance questions occurred in March 1995, when BLS introduced a more detailed set of health insurance questions. In particular, previous surveys asked about employer coverage as a subset of private coverage, but beginning in March 1995, the survey asked separate questions about employer-provided and other types of private health insurance. This change led to an increase in the number of persons reporting employer-provided coverage.

[16] For more detailed information, see Appendix T of Unicon's CPS Utilities (2005), which describes the changes in health insurance questions on March file.

During the years we examine, the CPS health insurance questions read as follows:

75A. Other than government sponsored policies, health insurance can be obtained privately or through a current or former employer or union. Was anyone in this household covered by health insurance of this type at any time during 19xx [last year]?

75B. Who was that?

75C. Was …'s health insurance coverage from a plan in …'s own name?

75F. What other persons were covered by this health insurance policy? Possible answers are Spouse, Children in household, Children not in the household, Other, and No one.

These questions allow us to define three alternative measures of health insurance:

1. **private insurance coverage**, defined broadly to include coverage by a privately purchased or employer-provided health insurance plan, whether or not in the respondent's own name [that is, positive responses to questions 75A and 75B]
2. private insurance **in the respondent's own name** [a subset of the first definition because it implies a positive response to question 75C]
3. private insurance in the respondent's own name that **covers children in household** [a subset of the second definition because it implies a "children in household" response to question 75F]

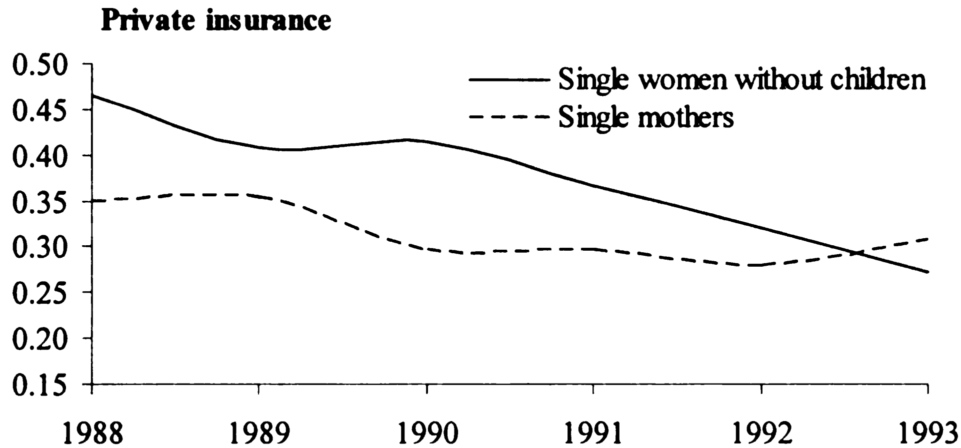
Table 3.1 and Figure 3.1 show average private health insurance coverage rates for both working single mothers and working single women without children from 1988 through 1993. We report the three measures of private health insurance coverage defined above. Both Table 3.1 and Figure 3.1 show decreases in the private health insurance coverage rates of single mothers between 1988 and 1993. For example, the rate of insurance coverage in own name fell by 5.4 percentage points (from 32.3 to 27 percent); however, only about one-fifth of this decrease occurred after 1990. The coverage rate for single women without children also fell between 1988 and 1990, but fell sharply even after 1990 (from 37.8 to 20.9 percent). A likely explanation for the drop after 1990 is the recession of 1991, which would have reduced both employment and access to employer-provided health insurance of single women.

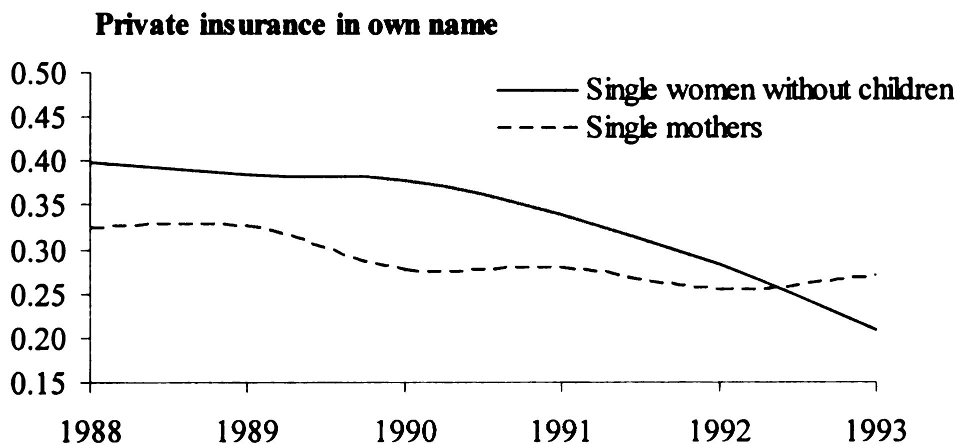
Table 3.1. Health Insurance Coverage Rates for Low-Education Working Single Mothers and Low-Education Working Single Women without Children

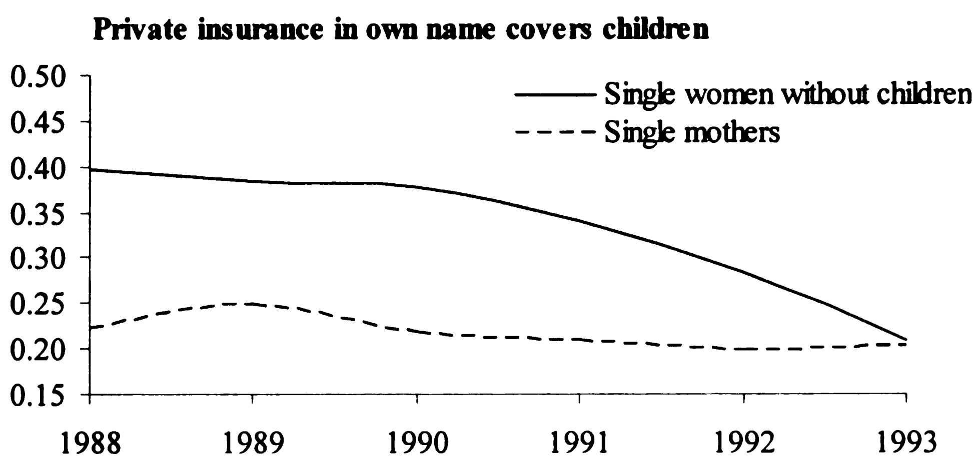
	1988	1989	1990	1991	1992	1993
Single mothers						
Private insurance	0.350	0.353	0.297	0.297	0.279	0.308
Private insurance in own name	0.323	0.325	0.276	0.278	0.255	0.270
Private insurance in own name that covers children	0.221	0.249	0.218	0.209	0.197	0.202
Single women without children						
Private insurance	0.466	0.409	0.414	0.367	0.320	0.272
Private insurance in own name	0.398	0.385	0.378	0.341	0.283	0.209

Notes: The data are from the March 1989-1994 Annual Demographic Supplements to the Current Population Survey (CPS). The sample contains working single women with less than a high school education. We define "working" as having positive hours and positive earnings during the year. We exclude women who are in school full-time, those who are separated from their spouse, and those who report being ill or disabled. Means are tabulated using CPS March supplement weights. Sample sizes are 2,228 (single mothers) and 1,433 (single women without children).

Figure 3.1. Health insurance coverage rates for low-education working single mothers and low-education working single women without children







Notes: See Table 3.1.

Table 3.2 displays mean characteristics of working single mothers and working single women without children pooled for 1988 through 1993. The two groups differ in some important ways. Relative to single women without children, single mothers are more likely to be black (33.6 versus 14.7 percent) and less likely to work full-time, full-year (37.8 versus 49.1 percent). Also, single mothers have average earnings that are lower than single women without children ($7,257 versus $8,686).

Table 3.2. Summary Statistics: Low-Education Working Single Mothers and Low-Education Working Single Women without Children

Variable	**Single mothers**	**Single women without children**
Age (years)	30.5	29.7
White (%)	63.8	81.0
Black (%)	33.6	14.7
Other race (%)	2.6	4.4
Has children under age 6 (%)	48.6	0
Has children aged 6-18 (%)	69.9	0
Has children aged 19-24 and a full-time student (%)	2.0	0
Full-time, full-year (%)	37.8	49.1
Part-time, full-year (%)	8.9	9.2
Full-time, part-year (%)	31.7	29.0
Part-time, part-year (%)	21.6	12.8
Earned income ($)	7,257	8,686
Unearned income ($)	1,833	464
Number of observations	2,228	1,433

Notes: See Table 3.1. Dollar amounts are converted to 1993 dollars using the Consumer Price Index, All Urban Consumers (CPI-U).

Table 3.3 displays summary statistics for both single mothers and single women without children in the before-HITC and during-HITC years. Overall, the characteristics of both single mothers and single women without children appear quite stable over the years in the sample. The only important change is in labor force attachment, reflecting the recession in 1991 and 1992. For single mothers, the fraction working full-time, full-year declined from 39.0 percent to 36.5 percent between the before-HITC and during-HITC periods. For single women without children, the trend in the fraction working full-time, full-year is similar to that for single mothers, falling from 50.4 in the before-HITC period to 47.6 percent in the during-HITC period.

Table 3.3. Summary Statistics: Low-Education Working Single Mothers and Low-Education Working Single Women without Children, Before and During HITC

	Single mothers		**Single women without children**	
Variable	Before HITC (1988-90)	During HITC (1991-93)	Before HITC (1988-90)	During HITC (1991-93)
Age (years)	30.3	30.7	29.6	29.8
White (%)	64.6	63.0	81.8	80.1
Black (%)	32.9	34.4	15.5	13.7
Other race (%)	2.5	2.7	2.6	6.2
Has children under age 6 (%)	48.6	48.7	0	0
Has children aged 6-18 (%)	70.5	69.3	0	0
Has children aged 19-24 and a full-time student (%)	1.5	2.4	0	0
Full-time, full-year (%)	39.0	36.5	50.4	47.6
Part-time, full-year (%)	7.2	10.7	6.8	11.6
Full-time, part-year (%)	31.6	31.7	31.7	26.0
Part-time, part-year (%)	22.2	21.1	11.1	14.7
Earned income ($)	6,770	7,764	8,003	9,419
Unearned income ($)	1,599	2,077	472	455
Number of observations	1,153	1,075	741	692

Notes: See Table 3.1. Dollar amounts are converted to 1993 dollars using the Consumer Price Index, All Urban Consumers (CPI-U).

3.5. Empirical Findings

In the following analysis, the outcome of interest is coverage by private health insurance defined as whether a working single woman has private insurance in her own name that covers her child or children. We focus on this outcome because the HITC could be used only to purchase a health insurance policy — either in the market or through an employer or union — covering a qualifying child.

3.5.1. Main Findings — Single Women with Less than a High School Education

Table 3.4 displays the average private health insurance coverage rates for single mothers and single women without children in the years before and during the HITC. The first row shows that health insurance coverage for single mothers fell by 2.4 percentage points between 1988-90 and 1991-93. The second row shows that, over the same time period, coverage fell for single women without children by 9 percentage points. The implication is that, after netting out the declining trend in insurance coverage, the private health insurance coverage of single mothers was higher by 6.5 percentage points than it would have been without the HITC. The robust standard error of this point estimate is 0.031.

Table 3.5 reports estimates of the key coefficients in equation (3.1).17 The estimates in column 1 come from a specification that includes no control variables. Column 2's estimates control for age, race (white, black, and other), number of children under age 6, number of children aged 6-18, and number of children aged 19-24 and a full-time student. Column 3's estimates control in addition for work status (full-time/full-

[17] The procedure we use to compute the probit difference-in-differences is presented in Appendix 2.

year, full-time/part-year, part-time/full-year, and part-time/part-year), earned income, and unearned income.

In column 1 of Table 3.5, the coefficient on treatment (working single mothers) is -0.145 and statistically significant (p-value = 0.000). With the addition of demographic characteristics in column 2, it falls to -0.080 (p-value = 0.006). In column 3, when we control for work status and income along with demographic characteristics, it changes slightly from -0.080 to -0.084 (p-value = 0.000). That the coefficient on treatment falls as controls are added to the model suggests that observable characteristics other than the presence of children are important in explaining the difference between single mothers and single women without children in private health insurance coverage.

In column 1, the coefficient on HITC is negative (-0.090) and statistically significant (p-value = 0.000). This is consistent with the declining trend in average health insurance coverage for both single mothers and single women without children. Including additional controls (columns 2 and 3) leaves this estimate essentially unchanged.

The estimate of main interest is the coefficient on the interaction term. This is essentially similar in size and statistical significance across the three specifications. The estimate in column 3 is 0.063 (p-value = 0.019), which suggests that private health insurance coverage of working single mothers with less than a high school education was higher by 6.3 percentage points than it would have been without the HITC. The finding is consistent with the law of demand — a drop in the price of health insurance should induce consumers to demand more of it.

Table 3.4. Private Health Insurance Coverage Rates of Low-Education Working Single Mothers and Low-Education Working Single Women without Children

	Before HITC (1988-1990)	During HITC (1991-1993)	Difference
Single mothers	0.244	0.220	-0.024
	(0.013)	(0.013)	(0.018)
	[1,153]	[1,075]	
Single women without children	0.389	0.299	-0.090
	(0.018)	(0.017)	(0.025)
	[741]	[692]	
Difference	-0.145	-0.080	—
	(0.022)	(0.022)	
Difference-in-differences	—	—	0.065
			(0.031)

Notes: See Table 3.1. Figures are average private health insurance coverage rates. Robust standard errors are in parentheses. Sample sizes are in brackets.

Table 3.5. Difference-in-Differences Estimates of the Effect of the HITC on Private Health Insurance Coverage of Low-Education Working Single Mothers from Probit Estimates of Equation (3.1)

Dependent variable: *Covered by private health insurance*	(1)	(2)	(3)
Treatment (working single mothers)	-0.145	-0.080	-0.084
	(0.020)	(0.029)	(0.026)
HITC	-0.090	-0.087	-0.104
	(0.023)	(0.022)	(0.021)
Treatment $\times$ *HITC*	0.065	0.060	0.063
	(0.030)	(0.028)	(0.028)
Number of observations	3,661	3,661	3,661
Pseudo R-squared	0.020	0.061	0.256

Notes: See Table 3.1. Figures are estimated changes in the probability of private health insurance coverage from probit models. Standard errors are in parentheses. The specification in column 1 includes no control variables. The specification in column 2 includes age, indicators of race (white, black, and other), number of children under age 6, number of children aged 6-18, and number of children aged 19-24 and a full-time student. The specification in column 3 adds indicators of work status (full-time/full-year, full-time/part-year, part-time/full-year, and part-time/part-year), earned income, and unearned income to the controls in specification 2. The marginal effects of the complete set of regressors are reported in Appendix 1, Table A1.3.

3.5.2. Findings for Single Women with More Education

We would expect the estimated effect of the HITC on low-education single mothers to be greater than its effect on single mothers with more education for a simple reason: Low-education single mothers are more likely to be in low-wage jobs and hence eligible for the EITC and HITC. For single mothers with more education, we would expect a smaller estimated HITC effect because fewer are eligible.

We view this as an interesting hypothesis to test because it may suggest whether the HITC estimates in Table 3.5 are convincing. No reason exists to suppose that single mothers with more than high school (and hence higher earnings on average) should have experienced improved health insurance coverage between 1988-90 and 1991-93. Accordingly, finding that single mothers with high school, more than high school, and college experienced health insurance coverage increases similar to those of single mothers with less than high school would cast considerable doubt on the findings in Table 3.5.

Table 3.6 displays tests of the hypothesis that single mothers with higher levels of education — hence higher earnings — were less likely to experience an HITC-induced improvement in health insurance coverage than single mothers with low education by estimating equation (1) using samples of women with successively more educational attainment. The specifications are the same as those in column 3 of Table 3.5; that is, they include demographic characteristics, indicators of work status, earned income, and unearned income as controls. We refer to this specification as the "basic specification."

The first row of Table 3.6 repeats the main finding from comparing single mothers and single women with less than a high school education (column 3 of Table

3.5). The second row of Table 3.6 shows the results of comparing single mothers and single women with a high school education. The estimate on the interaction term is 0.043 with a *p*-value of 0.002, which suggests that private health insurance coverage rate of single mothers with a high school education was 4.3 percentage points higher than it would have been without the HITC. This accords with the prior expectation that the HITC effect on single mothers should be less as educational attainment increases.

The third and fourth rows of Table 3.6 show findings from comparisons for single mothers with some college education and with college or more. As expected, these comparisons produce still smaller estimated effects of the HITC on health insurance coverage. For single mothers with some college education, the estimated HITC effect on health insurance coverage is 2.5 percentage points, but the estimate is not statistically significant (row 3 of Table 3.6). Similarly, the estimated effect of the HITC on health insurance coverage of single women with college or more is –0.6 percentage points which is essentially zero. That we observe negligible responses to the HITC among single mothers more than high school — women who are unlikely to be eligible for the HITC — tends to increase our confidence that we are isolating an HITC effect for low-education single women in Table 3.5.

3.5.3. Findings Disaggregated by Year

The estimates in Tables 3.5 and 3.6 are restrictive because they lump together the three before-HITC years and the three during-HITC years. We would like to know whether it is reasonable to restrict the estimated effect of the HITC to be the same in the three during-HITC years (1991, 1992, and 1993). Accordingly, we estimate a model that includes a set of year dummies (for 1988, 1989, 1991, 1992, and 1993), a treatment group

Table 3.6. Difference-in-Differences Estimates from Alternative Treatment and Comparison Groups

Dependent variable: *Covered by private health insurance*	*Treatment*	*HITC*	*Treatment ×HITC*
1. Treatment group: Single mothers with less than high school [N=2,228] Control group: Single women with less than high school [N=1,433]	-0.084 (0.026)	-0.104 (0.021)	0.063 (0.028)
2. Treatment group: Single mothers with high school [N = 6,794] Control group: Single women with high school [N=6,608]	-0.166 (0.016)	-0.107 (0.011)	0.043 (0.016)
3. Treatment group: Single mothers with some college [N=3,630] Control group: Single women with some college [N=5,060]	-0.173 (0.020)	-0.098 (0.012)	0.025 (0.021)
4. Treatment group: Single mothers with college [N=1,179] Control group: Single women with college [N=5,736]	-0.184 (0.034)	-0.048 (0.009)	-0.006 (0.024)

Notes: Estimates in row 1 are identical to those in column 3 of Table 3.5. Estimates in rows 2, 3, and 4 come from applying the same model to different samples of women, as indicated.

dummy, and interactions of the treatment and year dummies. We estimate this model for our main group of interest — working single women with less than a high school education, and report estimates from a specification that is analogous to that in the basic specification. The estimates are year-specific difference-in-differences estimates of the HITC effect on private health insurance coverage rates.

Table 3.7. Difference-in-Differences Estimates of the Change in Private Health Insurance Coverage Relative to 1990, Low-Education Working Single Mothers

Dependent variable: *Covered by private health insurance*	
Treatment (working single mothers)	-0.104 (0.032)
1988×treatment	0.025 (0.042)
1989×treatment	0.036 (0.044)
1990×treatment	—
1991×treatment	0.064 (0.046)
1992×treatment	0.064 (0.046)
1993×treatment	0.110 (0.046)
Number of observations	3,661
Pseudo R-squared	0.264

Notes: See Table 3.1. Figures are estimated changes in the probability of private health insurance coverage from a probit model. Standard errors are in parentheses. The specification includes age, race, number of children, work status, earned income, unearned income, and a set of year indicators. The marginal effects of the complete set of regressors are reported in Appendix 1, Table A1.4.

The estimates, reported in Table 3.7, suggest that the 1991, 1992, and 1993 coverage rates of single mothers with less than high school were higher by 6.4, 7.2, and 11 percentage points than they would have been in the absence of the HITC (although only the estimate for 1993 has a p-value less than 0.05). The pattern of these estimates — increasing over time — has at least two possible interpretations. First, it could be that the impact of the HITC increased as the existence of the program became known and its implications better understood. This would be consistent with the pattern of participation in several social programs (Madrian and Shea 2001; Remler and Glied 2003; Currie

2006). Alternatively, it could be that we have omitted one or more variables that affected the health insurance coverage of low-education mothers (but not other low-education single women). We investigate the latter possibility next.

3.6. Sensitivity Tests

This section describes findings from several sensitivity tests that attempt to control for changes in the early 1990s other than the HITC and other possible influences on private health insurance coverage. These include (1) expansion of the Medicaid program, (2) state-level economic conditions and state fixed effects, and (3) welfare reform and the introduction of state-level EITCs.

3.6.1. Medicaid Crowd-Out

During the years we are examining, eligibility for Medicaid expanded substantially to include low-income pregnant women and children with no ties to the AFDC Program (see Appendix 1, Table A1.2). Because Medicaid and private health insurance are potential substitutes — they offer similar health coverage, and Medicaid is much less costly — Medicaid expansion may have drawn some low-income single mothers out of private health insurance and into Medicaid. Cutler and Gruber (1996) first referred to such substitution as "crowding out," and to the extent it exists, the estimates in Tables 3.5, 3.6, and 3.7 may give downward-biased estimates of the HITC's impact on private health insurance coverage. Alternatively, the availability of the HITC could conceivably have drawn some low-income single mothers out of Medicaid and into private health insurance, in which case the estimates in Table 3.5 would overstate the net impact of the HITC on health insurance coverage.

To address these concerns, we estimate variants of equation (3.1) that use two alternative dependent variables: medicaid, a dummy indicator of whether a woman had Medicaid coverage, and insured, a dummy indicator of whether a woman had coverage from either Medicaid or private health insurance. In the first case, the question addressed is whether Medicaid coverage of low-education single mothers changed (relative to low-education single women without children) during the HITC years. In the second case, the question addressed is whether overall health insurance coverage of low-education single mothers changed (relative to low-education single women without children) during the HITC years.

Row 2 of Table 3.8 displays findings from the model in which medicaid is the dependent variable. The estimates offer only marginal evidence that relative Medicaid coverage of low-education single mothers was higher during the HITC period than in the prior years. (The point estimate is 1.4 percentage points, but the standard error is large.) We interpret this finding as evidence that, if the Medicaid expansions did crowd out private health insurance during the HITC years, crowd-out was slight.

Row 3 of Table 3.8 displays findings from the model in which insured (by either private health insurance or Medicaid) is the dependent variable. The estimates suggest that during the HITC period, relative net health insurance coverage of low-education single mothers was higher by 8.4 percentage points (p-value = 0.007) than in the preceding years. Estimates from the basic specification in row 1 (which repeats the main findings from Table 3.5, column 3) suggest that the HITC was the main factor in this net increase in health insurance coverage, and that Medicaid expansion played a relatively minor role. Specifically, rows 1, 2, and 3 suggest that 6.3 percentage points of the 8.4

percentage points were due to an increase in private coverage, and 1.4 percentage points were due to an increase in Medicaid coverage (although the latter is not statistically significant). A relatively small residual (0.7 percentage points) cannot be explained by either the HITC or changes in Medicaid.

3.6.2. State-Level Economic Conditions and State Fixed Effects

It is also possible that changes in private health insurance coverage of low-education single mothers were related to state-level economic conditions or to state fixed effects. To account for these possibilities, rows 4, 5, and 6 of Table 3.8 report estimates from models that successively add to the basic specification the year-specific contemporaneous state unemployment rate (to control for cyclical labor market influences on health insurance coverage), a set of state dummy variables (to control for time-invariant state-level effects on health coverage), and both the year-specific state unemployment rate and state fixed effects.

The findings reported in rows 4,5, and 6 of Table 3.8 suggest that including these state-level controls in the model produces estimates of the HITC effect that are essentially similar to the basic specification — an increase in single mothers' private health insurance coverage of roughly 6.5 percentage points (with p-values less than 0.02).

3.6.3. Welfare Reform and State EITCs

Although the findings in Table 3.5 are consistent with the HITC increasing private health insurance coverage of low-education single mothers, an alternative explanation for this increase is the welfare reforms adopted by some states after 1990. California, Michigan, New Jersey, Oregon, and Utah all implemented a welfare waiver in 1993 (DeLeire, Levine, and Levy 2006). These waivers changed the nature of the AFDC by imposing time limits on AFDC participation and introducing work requirements for AFDC participants (Meyer and Rosenbaum 2000). If these changes moved single mothers into the labor market and onto private (employer-provided) health insurance, the estimates in Tables 3.5, 3.6, and 3.7 could overstate the HITC's impact on private health insurance coverage.

To address the above possibility, we restrict the sample to low-education single women in states that did not have welfare waivers. Row 7 of Table 3.8 shows that restricting the sample in this way leaves the estimated HITC impact on single mothers essentially unchanged — an increase in the private health insurance coverage of single mothers of 6.3 percentage points (p-value = 0.035).

In addition to implementing welfare reforms, several states made changes in their EITCs during the period we examine. By 1993, six states had their own EITCs: Minnesota, Vermont, and Wisconsin had refundable tax EITCs, while Iowa, Maryland, and Rhode Island had non-refundable EITCs (Baughman 2005). When we restrict our sample to single women in states that did not have state-level EITCs, we again see no major change in the estimated effect of the HITC. The estimates reported in row 8 of Table 3.8 show a relative increase in the coverage rate of single mothers of 6.5 percentage points (p-value = 0.017), suggesting that changes in state EITCs were not responsible for the HITC effect we observed earlier.

Row 9 of Table 3.8 shows the findings when we restrict the sample to women in states with neither welfare reforms nor state-level EITCs. Again, the main findings are essentially unchanged.

Table 3.8. Sensitivity Tests for HITC Effects on Low-Education Working Single Mothers

Dependent variable: *Covered by private health insurance* (except noted)	*Treatment*	*HITC*	*Treatment* × *HITC*	Number of observations	Pseudo R-squared
1. Basic specification	-0.084	-0.104	0.063	3,661	0.256
	(0.026)	(0.021)	(0.028)		
2. Basic specification, but dependent variable is whether covered by *Medicaid*	0.129	0.059	0.014	3,661	0.132
	(0.022)	(0.020)	(0.025)		
3. Basic specification, but dependent variable is *Insured* (either private insurance or Medicaid)	-0.018	-0.074	0.084	3,661	0.067
	(0.027)	(0.027)	(0.030)		
4. Add state dummies to the basic specification	-0.090	-0.106	0.068	3,661	0.291
	(0.021)	(0.020)	(0.028)		
5. Add state unemployment rate to the basic specification	-0.085	-0.079	0.063	3,661	0.259
	(0.021)	(0.021)	(0.029)		
6. Add state dummies and state unemployment rate to the basic specification	-0.089	-0.136	0.067	3,661	0.292
	(0.021)	(0.023)	(0.028)		
7. Sample restricted to single women in states without welfare waivers in 1993	-0.089	-0.120	0.063	2,845	0.268
	(0.030)	(0.022)	(0.033)		
8. Sample restricted to single women in states without state EITCs	-0.093	-0.106	0.065	3,484	0.258
	(0.027)	(0.020)	(0.023)		
9. Sample restricted to single women in states without welfare waivers in 1993 and without state EITCs	-0.102	-0.125	0.067	2,668	0.270
	(0.028)	(0.024)	(0.031)		

Notes: See Table 3.5. Estimates in row 1 (basic specification) are the same as Table 3.5, column 3. The basic specification (row 1) includes age, race, number of children, work status, earned income, and unearned income as controls. Standard errors are in parentheses.

3.7. Conclusion

The ongoing debate over how best to increase health insurance coverage in the United States has led to a range of proposals for reform. It seems unlikely that an HITC would receive serious consideration unless it could be expected to increase the health insurance coverage of low-wage workers.

The Health Insurance Tax Credit of 1991 through 1993 offers a natural experiment that we have used to examine the effectiveness of tax credits for health insurance. The main findings reported in Table 3.5 suggest that the HITC increased the private health insurance coverage of low-education single mothers by 6.3 percentage points on average during 1991, 1992, and 1993. This estimate implies an elasticity of health insurance take-up with respect to tax credits of roughly 1.1. The calculation is straightforward: The HITC we are examining covered 23 percent of the reported average annual health insurance premium in 1991 (U.S. Government Accountability Office 1991). The HITC-induced 6.3 percentage points increase in health insurance coverage of single mothers during 1991, 1992, and 1993 implies a 25.8 percent increase in private coverage (because 24.4 percent of single mothers had private health insurance coverage during the before-HITC years). Accordingly, a 23 percent reduction in the price of health insurance led to a 25.8 percent increase in health insurance coverage, which implies an elasticity of 1.1.

It is not entirely surprising that the findings offered here differ from previous empirical findings. Gruber and Washington's (2005) study found convincing evidence that extending the tax subsidy on the employee's share of health insurance premiums to federal employees had little or no effect. The policy change Gruber and Washington

(2005) examine differs in two ways from the HITC of 1991-1993. First, it was a tax subsidy, as opposed to a refundable tax credit, and hence was less valuable in dollar terms. Second, it applied to a group of relatively high-wage workers, most of whom were already covered by health insurance, as opposed to a low-income group of workers most of whom were (presumably) not previously covered. Accordingly, Gruber and Washington's findings do not necessarily conflict with those presented here, although they do appear to at first blush.

The findings presented here appear favorable to the idea of a health insurance tax credit, in that they suggest enough eligible low-wage workers with children would take up a credit to significantly increase the percentage of such workers who are covered by health insurance. Still, questions about the feasibility of a health insurance tax credit remain. In particular, during the existence of the HITC, reports emerged that some insurers offered policies of little real value that happened to cost the same amount as the credit. Indeed, a Congressional investigation found that some insurers sold policies for children covering only "cancer, heart attacks, strokes, and other diseases that few children have" (Solomon 2007). Senator Lloyd Bentsen, the HITC's original sponsor, considered these abuses serious enough that he led efforts to repeal the HITC. Such abuses raise serious concerns about tax credits for health insurance. It is an open question whether such concerns could be mitigated by the existence of a well-developed private health insurance market existed, or failing that, could be addressed with appropriate regulation of health insurance.

Although the results presented here are consistent with the HITC increasing health insurance coverage substantially, the analysis is hampered by small sample sizes.

It seems possible that further research using different data sources — such as the Survey of Income and Program Participation and the National Longitudinal Survey, both of which cover the years in question — could give more convincing evidence because both would allow us to observe the same individuals before and after the adoption of the HITC.

APPENDICES

APPENDIX 1

Table A1.1. Earned Income Tax Credit Parameters

Basic Tax Credit parameters, 1987-1996, nominal dollars

Tax year	Phase-in rate (%)	Phase-in range ($)	Maximum credit ($)	Phase-out rate (%)	Phase-out range ($)
1987	14.00	0-6,080	851	10.00	6,920-15,432
1988	14.00	0-6,240	874	10.00	9,840-18,576
1989	14.00	0-6,500	910	10.00	10,240-19,340
1990	14.00	0-6,810	953	10.00	10,730-20,264
1991					
One child	16.70	0-7,140	1,192	11.93	11,250-21,250
Two children	17.30	0-7,140	1,235	12.36	11,250-21,250
1992					
One child	17.60	0-7,520	1,324	12.57	11,840-22,370
Two children	18.40	0-7,520	1,384	13.14	11,840-22,370
1993					
One child	18.50	0-7,750	1,434	13.21	12,200-23,050
Two children	19.50	0-7,750	1,511	13.93	12,200-23,050
1994					
No child	7.65	0-4,000	306	7.65	5,000-9,000
One child	26.30	0-7,750	2,038	15.98	11,000-23,755
Two children	30.00	0-8,245	2,528	17.98	11,000-25,296
1995					
No child	7.65	0-4,100	314	7.65	5,130-9,500
One child	34.00	0-6,160	2,094	15.98	11,290-24,396
Two children	36.00	0-8,640	3,110	20.22	11,290-26,673
1996					
No child	7.65	0-4,220	323	7.65	5,280-9,500
One child	34.00	0-6,330	2,152	15.98	11,610-25,078
Two children	40.00	0-8,890	3,556	21.06	11,610-28,495

Health Insurance Tax Credit parameters, 1991-1993, nominal dollars

Tax year	Phase-in rate (%)	Phase-in range ($)	Maximum credit ($)	Phase-out rate (%)	Phase-out range ($)
1991	6.00	0-7,140	428	4.28	11,250-21,250
1992	6.00	0-7,520	451	4.28	11,840-22,370
1993	6.00	0-7,750	465	4.28	12,200-23,050

Source: U.S. House of Representatives (1998); Government Accountability Office (1991, 1994).

Table A1.2. Major Legislative Changes Affecting Low-Income Women, 1987-94

Tax year	*Earned Income Tax Credit*
1988	The beginning and end of the phase-out range increased by about $3,000.
1991	The credit rates rose by 2 percentage points. Additional credits established for families with two or more children. The new increment to the maximum credit for a second child was $43. Supplemental credits added for child health insurance premiums and children under age 1.
1992	The credit rates rose by 1 percentage point. The increment to the maximum credit for a second child rose to $60.
1993	The credit rates rose by 1 percentage point. The increment to the maximum credit for a second child rose to $77.
1994	The credit rates rose by about 10 percentage points. The increment to the maximum credit for a second child rose to $490. Supplemental credits for health insurance and children under age 1 repealed. Small credits established for taxpayers without children and between the ages of 25 and 65. The IRS began to notify eligible taxpayers of the advance payment option, which had been available since 1979.

Tax year	*Medicaid*
April 1987	States permitted to extend Medicaid coverage to children under age 2 in families below 100 percent of the poverty line
July 1988	States permitted to extend Medicaid coverage to children under age 5 in families below 100 percent of the poverty line.
October 1988	States permitted to extend Medicaid coverage to children under age 8 in families below 100 percent of the poverty line, and to children under age 1 in families below 185 percent of the poverty line.
July 1989	States required to extend Medicaid coverage to children under age 1 in families below 75 percent of the poverty line.
April 1990	States required to extend Medicaid coverage to children under age 6 in families below 133 percent of the poverty line.
July 1991	States required to extend Medicaid coverage to children under age 19 in families below 100 percent of the poverty line.

Source: U.S. House of Representatives (1998); Meyer and Rosenbaum (2000); General Accountability Office (1993).

Table A1.3. Full Results for Table 3.5 Estimates: Results from Probit Models

Dependent variable: *Covered by private health insurance*	**(1)**	**(2)**	**(3)**
Treatment	-0.145	-0.080	-0.084
	(0.020)	(0.029)	(0.026)
HITC	-0.090	-0.087	-0.104
	(0.023)	(0.022)	(0.021)
Treatment $\times$ *HITC*	0.065	0.060	0.063
	(0.030)	(0.028)	(0.028)
Age	—	0.011	0.005
		(0.001)	(0.001)
White	—	-0.012	-0.009
		(0.035)	(0.031)
Black	—	-0.057	-0.033
		(0.036)	(0.033)
Number of children under age 6	—	-0.046	-0.004
		(0.015)	(0.014)
Number of children aged 6-18	—	-0.030	-0.019
		(0.010)	(0.009)
Number of children aged 19-24 and a full-time student	—	-0.034	-0.029
		(0.053)	(0.047)
Full-time, full-year	—	—	0.259
			(0.034)
Part-time, full-year	—	—	0.077
			(0.035)
Full-time, part-year	—	—	0.138
			(0.027)
Part-time, part-year	—	—	—
Earned income	—	—	0.018
			(0.002)
Unearned income	—	—	0.005
			(0.003)
Number of observations	3,661	3,661	3,661
Pseudo R-squared	0.020	0.061	0.256

Notes: Figures are estimated changes in the probability of private health insurance coverage from probit models. Standard errors are in parentheses.

Table A1.4. Full Results for Table 3.7 Estimates: Results From Probit Models

Dependent variable: *Covered by private health insurance*	
Treatment (working single mothers)	-0.104
	(0.032)
1988	0.036
	(0.036)
1989	0.012
	(0.039)
1990	—
1991	-0.037
	(0.034)
1992	-0.094
	(0.034)
1993	-0.144
	(0.034)
1988×treatment	0.025
	(0.042)
1989×treatment	0.036
	(0.044)
1990×treatment	—
1991×treatment	0.064
	(0.046)
1992×treatment	0.072
	(0.049)
1993×treatment	0.110
	(0.046)
Age	0.005
	(0.001)
White	-0.008
	(0.030)
Black	-0.031
	(0.034)
Number of children under age 6	-0.004
	(0.013)
Number of children aged 6-18	-0.019
	(0.009)
Number of children aged 19-24 and a full-time student	-0.027
	(0.055)

Table A1.4. (cont'd)

Dependent variable: *Covered by private health insurance*	
Full-time, full-year	0.257
	(0.034)
Part-time, full-year	0.077
	(0.039)
Full-time, part-year	0.138
	(0.028)
Part-time, part-year	—
Earned income	0.018
	(0.002)
Unearned income	0.006
	(0.003)
Number of observations	3,661
Pseudo R-squared	0.264

Notes: Figures are estimated changes in the probability of private health insurance coverage from a probit model. Standard errors are in parentheses.

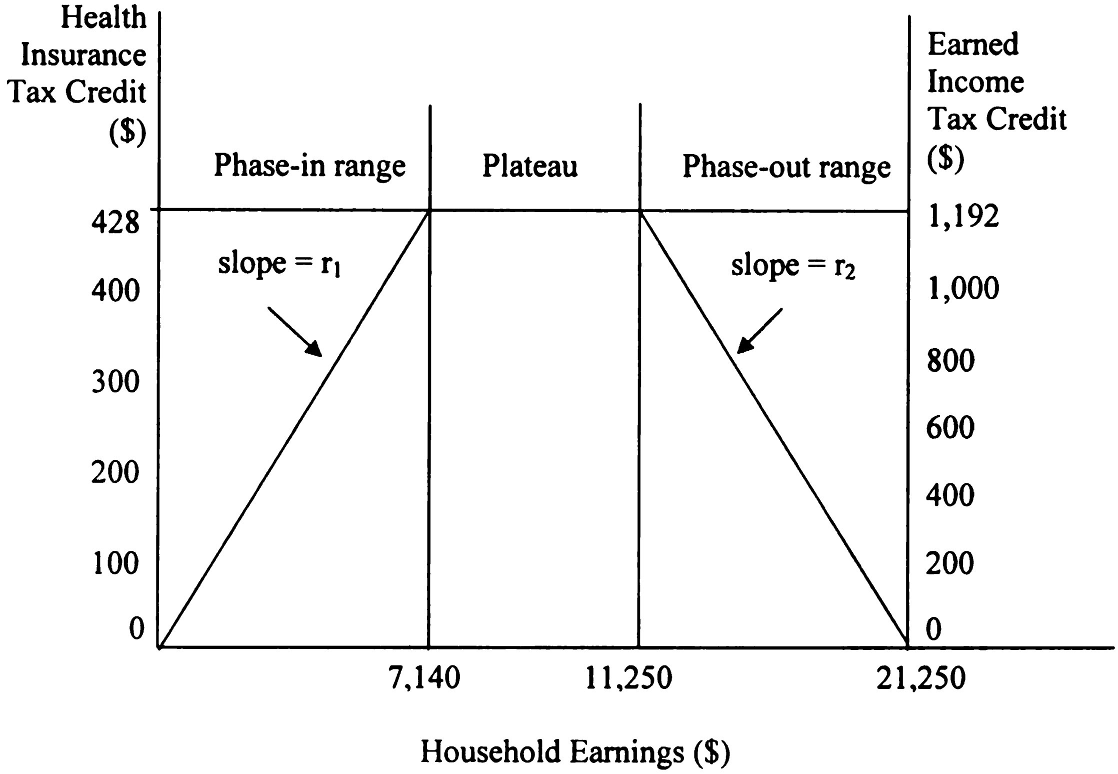


Figure A1.1. Earned Income Tax Credit and Health Insurance Tax Credit Schedules, 1991

APPENDIX 2

Calculating Difference-in-Differences Estimates in Linear Probability and Probit Models

In a linear probability model, calculating the difference-in-differences (DD) estimate is trivial. Let y be the outcome variable of interest, which takes on two values: zero and one. In the simplest case, we have two time periods (pre and post) and two groups (control and treatment). The linear probability model can be written as

$$\Pr(y=1\mid\bullet)=\beta_0+\beta_1 treatment+\beta_2 post+\beta_3 treatment\times post+X\beta \tag{A2.1}$$

where X denotes a vector of explanatory variables. The linear probability DD estimate is simply the OLS estimator of β_3, the coefficient on the interaction between *treatment* and *post*.

In a probit model, the coefficient on the interaction term no longer has the simple interpretation in equation (A2.1). To illustrate, consider the following probit model

$$\Pr(y=1\mid\bullet)=\Phi(\beta_0+\beta_1 treatment+\beta_2 post+\beta_3 treatment\times post+X\beta) \tag{A2.2}$$

where Φ is the standard normal cumulative distribution function. The marginal effect of the interaction term is

$$\frac{\Delta\Pr(y=1\mid\bullet)}{\Delta treatment\times post}=\begin{bmatrix}\Phi(\beta_0+\beta_1 treatment+\beta_2 post+\beta_3+X\beta)-\\ \Phi(\beta_0+\beta_1 treatment+\beta_2 post+X\beta)\end{bmatrix} \tag{A2.3}$$

As discussed by Ai and Norton (2003), Norton, Wang, and Ai (2004), and DeLeire (2004), many authors incorrectly interpret equation (A2.3) as the DD estimate. However, the DD estimate from the probit is

$$\frac{\Pr(y=1\mid\bullet)}{\Delta treatment \Delta post} = \frac{\Delta\left[\Phi(\beta_0+\beta_1+\beta_2 post+\beta_3 post+X\beta)-\Phi(\beta_0+\beta_2 post+X\beta)\right]}{\Delta post}$$

$$= \begin{Bmatrix} \left[\Phi(\beta_0+\beta_1+\beta_2+\beta_3+X\beta)-\Phi(\beta_0+\beta_1+X\beta)\right]- \\ \left[\Phi(\beta_0+\beta_2+X\beta)-\Phi(\beta_0+X\beta)\right] \end{Bmatrix} \qquad \text{(A2.4)}$$

Equations (A2.3) and (A2.4) clearly show that the marginal effect of a change in the interaction term is not equal to the marginal effect of a change in both interacted variables.

Following DeLeire (2004), we calculate the DD estimates from probits by taking the discrete double difference of the standard normal cumulative distribution function. In particular, we first estimate the probit model (equation 3.1)

$$\Pr(ins_i=1\mid\bullet)=\Phi(\beta_0+\beta_1 treatment_i+\beta_2 HITC_t+\beta_3 treatment_i\times HITC_t+X_i\beta)$$

Then, we predict four counter factual probabilities for each observation in the sample, plugging in the observed values for each explanatory variable. The predicted probabilities are:

1. Predicted probability of private health insurance coverage for the treatment group in the post-HITC period: $\Phi(\hat{\beta}_0+\hat{\beta}_1+\hat{\beta}_2+\hat{\beta}_3+X_i\hat{\beta})$
2. Predicted probability of private health insurance coverage for the treatment group in the pre-HITC period: $\Phi(\hat{\beta}_0+\hat{\beta}_1+X_i\hat{\beta})$

3. Predicted probability of private health insurance coverage for the control group in the post-HITC period: $\Phi\left(\hat{\beta}_0 + \hat{\beta}_2 + X_i\hat{\beta}\right)$

4. Predicted probability of private health insurance coverage for the control group in the pre-HITC period: $\Phi\left(\hat{\beta}_0 + X_i\hat{\beta}\right)$

Using the predicted probabilities, we calculate the DD for each observation as

$$= \begin{Bmatrix} \left[\Phi\left(\hat{\beta}_0 + \hat{\beta}_1 + \hat{\beta}_2 + \hat{\beta}_3 + X_i\hat{\beta}\right) - \Phi\left(\hat{\beta}_0 + \hat{\beta}_1 + X_i\hat{\beta}\right)\right] - \\ \left[\Phi\left(\hat{\beta}_0 + \hat{\beta}_2 + X_i\hat{\beta}\right) - \Phi\left(\hat{\beta}_0 + X_i\hat{\beta}\right)\right] \end{Bmatrix}$$

The probit DD estimate is the average of DD across all observations:

$$= n^{-1}\sum_{i=1}^{n} \begin{Bmatrix} \left[\Phi\left(\hat{\beta}_0 + \hat{\beta}_1 + \hat{\beta}_2 + \hat{\beta}_3 + X_i\hat{\beta}\right) - \Phi\left(\hat{\beta}_0 + \hat{\beta}_1 + X_i\hat{\beta}\right)\right] - \\ \left[\Phi\left(\hat{\beta}_0 + \hat{\beta}_2 + X_i\hat{\beta}\right) - \Phi\left(\hat{\beta}_0 + X_i\hat{\beta}\right)\right] \end{Bmatrix}$$

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