The Effect of Health Insurance Regulation on Wages: A Study in Maternity Benefits

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Economists typically assume that workers pay for employer-provided health insurance through lower wages. This assumption, however, has little empirical support. To test this proposition directly, I analyze whether there was a wage cost of the 1995-1998 insurance industry reforms known as "drive-through delivery" laws. These laws, enacted by many states in 1995-1997 and by the federal government in 1998, increased the minimum length of post-delivery hospital stay of mother and child that must be covered by private health insurance policies. Using data from the Current Population Survey, I apply a difference-in-difference-in-difference (DDD) model to examine the effect of the laws on the wages of married women in several age cohorts relative to all other workers ages 20-50. The data indicate that the "drive-through delivery" laws of the mid-1990s decreased the wages of women of child-bearing age by an amount approximately equal to the benefit of the new insurance policies. This suggests that the incidence of the required benefits falls almost entirely on the beneficiaries, as predicted by an equilibrium view of the markets for labor and insurance.

Health care policy is a particularly timely issue as the recent federal legislation to reform America's health care system may be the most significant social legislation enacted in the past 50 years. Not surprisingly, much of the reform package centers around reforming various aspects of health insurance—its affordability, accessibility, and coverage. The recent legislation is only one example of government regulation of health insurance markets; health insurance and the markets where the product is sold are regulated at both the state and federal level. Many states have adopted minimum standards of coverage that employer-provided health insurance policies must meet. For example, states have mandated coverage for diabetic supplies, mammography exams, and drug and alcohol rehabilitation.[1] Only two types of benefits are mandated by all 50 states: the length of stay for mothers and newborns after delivery and breast reconstruction after mastectomy or lumpectomy[2]. These regulations have consequences on the markets for insurance and labor, some of which have been studied by economists like Jonathon Gruber.[3]

The effect of regulation on markets depends on what type of policy is passed. A policy that increases benefits will necessitate a rise in costs, but the incidence will vary with the type of funding. Regulation in the form of mandates is attractive if the goal is efficiency. If the beneficiaries value the mandated benefits, the deadweight loss caused by mandates will be less in

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[1] Bunce, Victoria Craig and JP Wieske. "Health Insurance Mandates in the States 2008." Council for Affordable Health Insurance, VA 2008. <www.cahi.org>.

[2] National Women's Law Center. "Mandated Insurance Laws: Important Health Protections for Women and Their Families." Wash, DC. <www.nwlc.org>.

[3] Gruber, Jonathan. "The Incidence of Mandated Maternity Benefits," *The American Economic Review*, June 1994, 84(3), pp. 622-41.

absolute value than the deadweight loss caused by an equivalent tax.[4] Additionally, mandating benefits provided by private health insurers may be more politically feasible than regulation by taxation because it has no direct impact on government budgets.

The "drive-through delivery" laws of the mid-1990s are one example of mandated regulation of health insurance markets. These laws, enacted by many states from 1995-1997 and by the federal government in 1998 increased the minimum length of stay post-delivery for mother and child. During the 1990s, increasing prevalence of managed-care policies and other efforts to control costs lead to a rapid decline in the length of post-partum stays. Public health officials became concerned that such rapid discharge of new mothers put the mother and newborn at risk. The media began using the term "drive-through delivery" to refer to the brief length of time mothers and newborns were staying in the hospital in the mid 1990s. As Evans et al. point out, the "For legislatures, mandating a minimum postpartum length of stay seemed to be a reasonable and direct solution at that time."[5] In response to these concerns, the drive-through delivery laws required insurers to cover, in most cases, at least 48 hours of hospital coverage after delivery. The variation in the timing and location of these laws creates a useful natural experiment. This paper uses data from the Current Population Survey to examine the effects of drive-through delivery laws on the wages of the targeted demographic group to whom the benefits accrued: women of child-bearing age.

The paper will proceed as follows: In Section I, I will present a background of drive-through delivery laws in the United States. Next, I will present my econometric model and

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[4] Summers, Lawrence H. "Some Simple Economics of Mandated Benefits." *The American Economic Review*, May 1989, 79(2), pp. 177-83.

[5] Evans, William N., Craig Garthwaite, and Heng Wei. "The impact of early discharge laws on the health of newborns." *Journal of Health Economics*, 2008, 27, pp. 843-70.

discuss its key assumptions in Section II. Sections III and IV will discuss the data set and the results of the model. Finally, Section V will consider the implications of these results on efficient health care policy reform and the equilibrium theory of wages and labor.

I) History and Background of Drive-Through Delivery Laws

Insurance markets in the mid 1990s were marked by rapidly increasing use of managed care plans. These plans, designed to reduce waste by discouraging unnecessary medical care, gave concern to some physicians and consumers that appropriate levels of medical care would be sacrificed to save resources. One consequence of managed care plans was the decreasing length of post-partum stay. According to Thilo et al., (1998) the length of stay after an average vaginal delivery decreased from 3.9 days in 1970 to 2.1 days in 1992.[6] Some physicians believed that shorter stays after delivery contributed to poor health outcomes for newborns. The so-called "drive-through delivery laws" of the mid 1990s were an attempt to address the problems associated with short post-partum stay. These laws, passed by 32 states between 1995 and 1997, and by the federal government in 1998, prohibited insurance companies from limiting their coverage of post-delivery hospital stays. Most of the laws, drawing on suggestions from the American Academy of Pediatrics and the American College of Obstetrics and Gynecology, mandated that insurance companies cover no less than 48 hours after vaginal delivery or 96 hours after delivery by C-section. The following table shows the year of adoption of drive-through delivery laws by states. States listed in 1998 either passed their own drive-through delivery law effective that year or were not subject to any drive-through delivery law until the federal law took effect.

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[6] Thilo, E. H., S.F. Townsend, and G. Merenstein. "The history of policy and practice related to the perinatal hospital stay." *Clinics in Perinatalogy*, June 1998, 25(2), pp. 257-70.

Table 1: Year of Institution of Drive-Through Delivery Laws
1995: Maryland, Massachusetts, New Jersey, North Carolina
1996: Alabama, Alaska, Connecticut, Florida, Georgia, Illinois, Indiana, Iowa, Kansas, Kentucky, Maine, Minnesota, Missouri, New Mexico, New York, Ohio, Oklahoma, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Virginia, Washington
1997: California, New Hampshire
1998: Arizona, Arkansas, Colorado, Delaware, Hawaii, Idaho, Louisiana, Michigan, Mississippi, Montana, Nebraska, Nevada, North Dakota, Oregon, Texas, Utah, Vermont, West Virginia, Wisconsin, Wyoming

Opponents of the drive-through delivery laws believed that the laws would increase the cost of insurance plans without substantially altering health outcomes. Medical and economic studies have found differing results about the effectiveness of increasing post-partum stay on health outcomes. A review of the literature in 1995 by Braveman et al. found that the effects of shorter lengths of stay were inconclusive in previous studies.[7] Before one considers outcomes, however, one must consider whether the legislation had any real impact on post-delivery length of stay. Liu, Dow, and Norton studied the consequences of drive-through delivery laws on lengths of stay and found that on average, the laws increased length of stay by 33.8% (.48 days) and decreased early discharges, those in which mother and child stayed for less than 30 hours, by 16 percentage points. [8]

If Liu, Dow, and Norton were correct, the drive-through delivery laws had real consequences on the average cost of delivering a child. Specifically, insurance policies covering

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[7] Braveman, P. et al. "Early discharge of newborns and mothers: a critical review of the literature." *Pediatrics*, 96(4), pp. 716-26.

[8] Liu, Zhimei, Wiliam H. Dow, and Edward C. Norton. "Effect of drive-through delivery laws on postpartum length of stay and hospital charges." *Journal of Health Economics*, 2004, 23, pp. 129-55.

childbirths were now more generous because they were required to cover a longer length of stay.

Assuming the insurance market approximates perfect competition (a reasonable assumption

given the precise nature of actuarial work), the increased benefits will come at a cost. This paper

studies how the cost of the laws was distributed among its beneficiaries. Because the individuals

most likely to benefit from increased maternity benefits are women of child-bearing age, they are

used as the treatment group. According to an equilibrium view of the markets for insurance and

labor, those in this age range who have higher predicted instances of childbirths (and therefore

stand to gain more from the regulation) will pay for a greater portion of the benefits. As

Summers points out[9], this assumes the beneficiaries value the benefits. Workers will not accept

lower wages for benefits they do not value. Assuming the market for labor also approximates

perfect competition, businesses are not able to pay higher insurance premiums without passing

on the cost to employees; thus, the beneficiaries of more generous insurance policies will pay for

the benefits in the form of lower wages.

II) Econometric Model

To study the effects of the drive-through delivery laws on the wages of women of child-

bearing age, I use a difference-in-difference-in-difference (DDD) model. The DDD model

examines the difference in wages between those affected by the law and those not affected while

controlling for time trends, location by state, and the interaction between the two. The classic

difference-in-difference (DD) model compares two levels of difference: individuals in the

control and treatment groups with pre-treatment and post-treatment locations or times. For

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[9] Summers, Lawrence H. "Some Simple Economics of Mandated Benefits." *The American Economic Review*, May 1989, 79(2), pp. 177-83.

example, a well-cited article by Card and Krueger[10] examined the effect of the 1992 change in minimum-wage laws on employment. Their difference-in-difference model compares pre-1992 and post-1992 fast-food restaurants in New Jersey to pre-1992 and post-1992 fast-food restaurants in Pennsylvania. By subtracting the difference in treatment and control restaurants from the time trend (the change between pre-1992 and post-1992), a reliable estimate for the effect of the laws on employment is found. The key assumption of this model is that the trend in employment in the treatment state in the hypothetical absence of treatment is approximated by the trend in employment in the control state. Applying this to the Card and Krueger paper, the key assumption is that the trends in pre-1992 and post-1992 employment in Pennsylvania approximate what the trends in pre-1992 and post-1992 employment in New Jersey would have been in the absence of new employment laws.

To examine the effects of the drive-through delivery laws, a simple difference-in-difference model would examine the difference in wages over time for the likely beneficiaries (women of child-bearing age) in states with and without reform laws. In the Card and Krueger paper, there were only two states in the analysis, one treatment and one control, because there was only one discrete legislated change. In the case of drive-through delivery laws, however, although there is significant variation in the timing of the states' adoption of the laws, all states were eventually treated by the passage of the federal legislation which took effect in 1998. Establishing the time trends that wages would have taken in the absence of the intervention necessitates a third level of difference for comparison. In this case, we use all men ages 20-50 and women who are not in the child-bearing age cohort. Thus, the model must include as a basis

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[10] Card, David and Alan B. Krueger. "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania." *The American Economic Review*, Sep. 1994, 84(4), pp. 772-93.

of comparison not only secular changes in states but also time trends as well as the interaction between the two.[11]

In addition to the three levels of comparison, many observable factors have a large correlative relationship to income. Thus it was necessary to control for race, gender, marital status, and education level, as well as functions of age and age squared. Since wages tend to be distributed exponentially, the natural log is taken to linearize the variable. The following equation describes the DDD model:

$$(1) \quad \ln(\text{wage})_{ijt} = \beta_0 + \beta_1 X_{ijt} + \beta_2 \text{treat}_i + \tau_t + \delta_j + (\tau_t \times \delta_j) + (\tau_t \times \text{treat}_i) + (\text{treat}_i \times \delta_j)$$

$$+ \alpha(\text{reform}_{jt} \times \text{treat}_i) + \varepsilon_{ijt}$$

In this equation, the subscript i denotes individuals, the subscript j, states, and the subscript t, years. The outcome, "$\ln(\text{wage})_{ijt}$," is the natural log of nominal wages. Using real wages was not necessary because the output will be a percent change. β_0 is the constant estimated by the OLS regression. "X_{ijt}" stands for an array of observable characteristics including race, gender, marital status, education level, and age. The variables "τ_t" and "δ_j" stand for fixed year and state effects, respectively and these are captured by a complete set of dummy variables for the state and year effects respectively. The treatment group, women of child-bearing age, is given by the dummy variable "treat_i," which equals 1 if the observation belongs to a woman of child-bearing age, and 0 otherwise. After the first level of difference is controlled for by the fixed effects, the second level is given by a series of interactions. Therefore, the model includes a set of unique state-specific time effects, state-specific interactions with the treatment

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[11] My use of the DDD model is strongly informed by Jonathon Gruber's "Incidence of Mandated Maternity Benefits."

group, and year specific interactions with the treatment group. Finally, the coefficient α captures the third level of difference, the interaction between the state effects, year effects, and the treatment group, that identifies the treatment effect. In this case, the variable is produced by two dummy variables. The first variable is "reform$_{jt}$" which equals 1 in a state after it reforms and zero otherwise. For example, reform equals 1 in Maryland starting in 1995, since Maryland passed its drive-through delivery legislation in that year. Given the Federal law change, reform equals 1 in all states starting in 1998. This variable collapses both the state and year information in the treatment effect variable. The second variable is simply the "treat$_i$" dummy variable which identifies whether the person is in the group of people likely to be impacted by the law. The variable α provides the estimate for the effect of the drive-through delivery laws on the wages of women of child-bearing age. The results of estimates for α for several age cohorts will be reported in Section IV.

Since the DDD model controls for time trends, differences among states, and the interaction of these terms, the assumptions required by the model are not onerous. For a classic difference-in-difference model, we assume that the trends between the treatment and control groups would be similar in the absence of intervention. The DDD model requires a similar assumption for the pre-treatment and post-treatment trends both in the treated states and during the treated times. Assuming the 50 states pre-treatment and post-treatment are, on average, adequate control groups for themselves, the DDD model will fail only if there are exogenous shocks to the sampled population that correspond with the timing of the drive-through delivery laws among states, time periods, and treatment groups. In my analysis, all 50 states are used and the number of states with drive-through delivery laws varies each year from 0 in 1994 to 50 in 1998. Thus, the type of exogenous shock that could compromise the model is highly unlikely.

The following graph shows the trend of wages in treatment and control groups from 1990 through 2000. In this graph, the treatment group is defined as unmarried females, ages 23-27

Table 2: Income from Wages, 1990-2000

Wages, 1990-2000
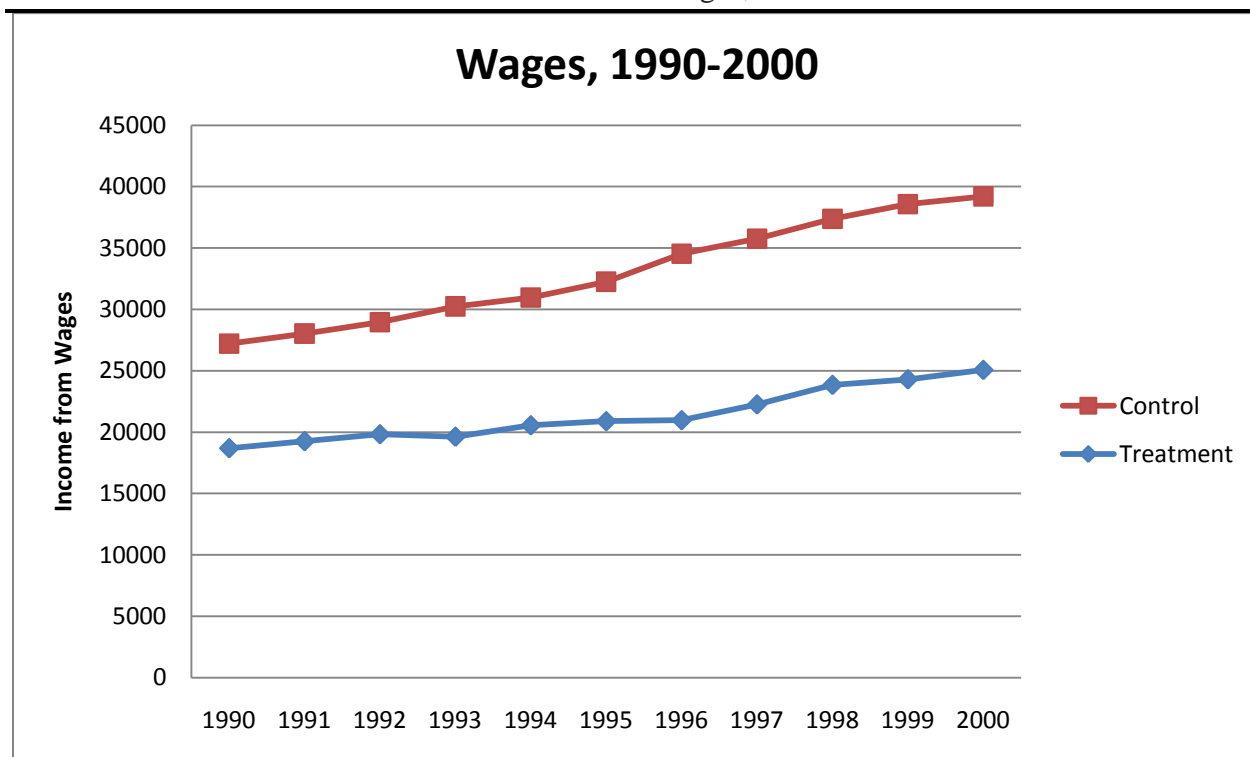
The pre-treatment trends show that the control group, all other workers, is a fairly good approximation of the trend in wages of the treatment group, women of child-bearing age. The year 1993 presents a slight challenge to the assumptions of the model, as there is a discrepancy in the wage trend without the influence of a drive-through delivery law. Fortunately for the DDD model, this year appears to be an anomaly. As the drive-through delivery laws begin to take effect in 1995-1998, substantial differences in the wages of the treatment and control group are evident. The year 1996, in which the majority of the laws were passed, shows large differences in the wage compensation for the control and treatment groups.

III) Data

The data used in this analysis are from the March Annual Demographic File and Income Supplement, more commonly known as the March Current Population Survey (March CPS), from the University of Minnesota's Minnesota Population Center. Data were collected for years 1990-2000. The years 1994-2000 were used in the regression analysis. The Current Population Survey (CPS) is a monthly survey of non-institutionalized populations conducted jointly by the US Census Bureau and Bureau of Labor Statistics. It was instituted after the Great Depression in the 1940s and was designed to measure unemployment. The survey is household based, gathering data from approximately 60,000 households and 160,000 people and asks a standard set of demographic and work-related questions. Annually, in March, a supplemental form (the March CPS) is sent with additional questions including detailed information about health insurance and sources of income in the previous year. The data is collected as microdata, meaning all observations are from individuals and households.

The control group is workers ages 20-50. Those who did not own private insurance, who worked less than 30 hours week, or who worked less than 40 weeks a year were excluded. Fertility rates were measured for the sample age group by examining what percentage of females in the given age range had a child under the age of one, i.e. had given birth to a child within the last year. The following table gives the sample means for the four age cohorts used in the regression analyses. It should be noted that for the control group to function accurately in the DDD model, it is not necessary that the means of the samples be similar, but merely the trends over time (See Table 2).

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Table 3: Sample Means for Wage-Earners

	Regression 1		Regression 2		Regression 3		Regression 4	
Treatment Age Range	20-40		20-30		23-27		24-26	
Variable	Treat	Control	Treat	Control	Treat	Control	Treat	Control
Income	25881.44	36300.88	22405.74	35877.51	22479.59	35418.73	22507.8	35249
	(18727)	(31071)	(14873)	(30592)	(13341)	(30318)	(13041)	(30196)
Age	30.2	37.08	25.4	36.9	25	36.6	25	36.4
	(5.8)	(7.63)	(2.98)	(7.42)	(1.39)	(7.6)	(.8)	(7.7)
Percent Nonwhite	0.2	0.12	0.18	0.13	0.18	0.13	0.17	0.13
	(.4)	(.33)	(.39)	(.34)	(.38)	(.34)	(.38)	(.34)
N:	27,177	187,466	14,274	200,369	7,251	207,392	4,499	210,144

Numbers in parentheses are standard deviations.

The treatment group in the following results consists of unmarried females of several age cohorts. The reason unmarried females are used as the treatment group is that adequate information about spouses is not available in the CPS. Although marital status is documented, and was used in my model, the age, income, and insurance status of the spouse is unavailable. Additionally, it is difficult to distinguish between married females who are covered by their own health insurance and those covered by their spouse. Because analyzing the effect of drive-

through delivery laws on married females was not feasible, unmarried females are used in the treatment group and married females remain in the control group.

To calculate the cost of the laws, I use an estimate of the cost of delivery from the Healthcare Cost Utilization Project (HCUP), sponsored by Agency for Healthcare Research and Quality (a division of the Department of Health and Human Services). The HCUP report finds an average cost of delivery in 2003 of $8,300[12]. According to Liu, Dow, and Norton, the drive-through delivery laws effectively increased hospital stay by 33.8%[13]. Multiplying this proportion by the average cost of delivery and by the fertility rate of a target age cohort will yield the cost estimate for the average delivery under the new legislation. For example, for women ages 23-27, with a fertility rate of .174, the cost of the increased insurance benefits would be $488.14. This method is imprecise because increased costs will not necessarily be equivalent to a proportional increase in length of stay, particularly if there are decreasing marginal costs after admission. Because the time change is small (.48 days), however, this estimate should be close to the real increase in costs due to the legislation.

IV) Results

The results presented in the following table are compiled from four regressions run using the previously enumerated DDD model (Equation 1)[14]. The regressions use four age cohorts of unmarried females as the treatment groups, labeled "$treat_i$" in the model.

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[12] U.S. Department of Health and Human Services. "Hospitalizations Related to Childbirth, 2003." <http://www.hcup-us.ahrq.gov/reports/statbriefs/sb11.jsp>.

[13] Liu, Zhimei, Wiliam H. Dow, and Edward C. Norton. "Effect of drive-through delivery laws on postpartum length of stay and hospital charges." *Journal of Health Economics*, 2004, 23, pp. 129-55.

[14] All regressions run using STATA 10.

Table 4: Effect of Drive-Through Delivery Laws on Wages of Unmarried Females

Sample Age	Fertility Rate	Effect on Wages	R^2	Observations
20-40	0.105	0.005	0.2975	214,643
		(0.015)		(16,738)
20-30	0.168	-0.011	0.2959	214,643
		(0.02)		(8,823)
23-27	0.174	-0.057	0.2948	214,643
		(0.028)		(4,438)
24-26	0.178	-0.091	0.2944	214,643
		(0.035)		(2,782)

Note: the numbers in parentheses under "Effect on Wages" are standard errors. The numbers under "Observations" are the sizes of the entire sampling group; the sizes of the treatment groups specifically are in parentheses.

Overall, it appears that there is a large transference of costs to the beneficiaries of the mandated increase in benefits. The distribution among the age range, however, is not even. The data suggest that as fertility increases, the incidence of the costs of the legislation also increases. The demographic "women of child-bearing age," including all working women capable of natural child birth, may pay the costs of new benefits on whole. But the greatest costs are imposed on unmarried women ages 23-27 and particularly 24-26.

According to the model, the age cohort 20-30 experienced a 1.1% decrease in wages. Average wages for this group over this time are $21,204.34. Dividing their expected increase in costs ($464.34) by the average wage, we would expect a wage drop of 2.2%. The experimental

result of 1.1% is somewhat lower than expected. It should be noted that the estimate of the decreased wages for this group was not statistically significant at the 95% confidence level.

The first age cohort that sees a statistically significant impact on wages is unmarried females ages 23-27. Average wages for this group over this time are $21,391.64. Dividing the expected increase in costs (488.14) by the average wage, we would expect a wage drop of 2.3%. According the DDD model, wages fell 5.7%. The estimated costs for the age cohort of 24-26 year-olds is very similar; yet this group's wages fell by 9.1%.

The data show that the age ranges including older women paid for less than the expected amount of the benefits. The age cohorts including women 23-27 years old and 24-26 years old, on the other hand, paid for more than the expected amount of the benefits. Although there is variation by age cohort, the results suggest that on the whole, the costs of the drive-through delivery laws were paid for by the decreased wages of the beneficiaries.

V) Discussion and Conclusion

An equilibrium model would predict that the age demographic groups would pay for the costs of the bill in proportion to their fertility rate. My results suggest that while this is true on the whole, there are some discrepancies, especially a rather large jump in the wage decrease for the age group 24-26 without a corresponding jump in fertility. This jump cannot be attributed to women dropping out of the workplace as only full-time workers were used in the data set. Furthermore, unless the likelihood of women dropping out of the workplace was increased solely by the drive-through delivery laws (which is counter-intuitive), the chance of women leaving cannot be responsible for the reduction in wages. The women aged 23-27 also experienced a

wage decrease greater than expected. There are three potential reasons these age cohorts may pay a greater share of the benefits than they receive.

First, the women may value the benefits of the insurance policy at a rate greater than the value of the benefits. If, for example, the extra day of hospital coverage is worth more to the women than it costs the insurance company to provide, the women would rationally accept lower wages to pay for these highly valued benefits.

Second, the response by employers to the additional insurance benefits may be less than perfectly rational. For example, the employer may overestimate the chances of women in a given age cohort having a child. This is especially plausible given the large jumps in fertility within a narrow age range. An overestimate of the fertility rates of employees would lead to a greater than predicted reduction in wages. Specifically, the employers may confuse the likelihood of the average woman having a child with the likelihood of one of their employees having a child. Women who have a full-time job with insurance benefits are far less likely than average women to have a child. If the employer mistook the higher rate of fertility in the average population for the rate of employee fertility, wages may be decreased in an irrational manner.

Third, insurance companies could charge more in premiums than the value of the coverage. The extra charge could be due to increased administrative costs or protection from legislation regarding the new laws. These charges are likely to be small, however, and if insurance markets approximate perfect competition, the cost of the policy is likely to increase by an amount very near the cost of the increased benefits.

As there is a large jump in wage discrepancy simply between women ages 23-27 and ages 24-26, it is unlikely that any of the previous explanations can fully account for why the

effect of the laws affects these demographics differently. Further research will have to be conducted to see why 23 and 27-year-old unmarried women did not feel the effects of the legislation as strongly as their 24, 25, and 26-year-old peers. Additionally, if data can be obtained with pertinent spousal information, researchers will be able to examine accurately the effect of laws on married couples in which the woman is of child-bearing age. On the whole, however, the data show that the costs of the mandated benefits were paid for by the beneficiaries in the form of lower wages; this is consistent with the prediction of an equilibrium view of the markets for insurance and labor.

Relative to the history of private health care markets, health insurance is a new phenomenon. It was not until the late 20th century, for example, that many governments sought higher levels of health insurance coverage among their citizens as a policy goal. Governments at the federal, state, and local levels are still experimenting with regulations designed to correct deficiencies in the markets, encourage behavior by subsidization, or discourage behavior by taxation. It is important for policy-makers to remember, as the effects of the drive-through delivery laws show, that policy changes, even those ostensibly levied on insurance companies or businesses, can have real effects on employees. Legislators may very well decide that lowered wages are acceptable given the benefits of regulation, but the costs of those benefits should not be ignored in the policy-making process.

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