The Impact of Protective Measures for Female Workers

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Policies designed to protect female workers have controversial effects on labor market outcomes, both in theory and in practice. The analysis uses repeated cross-sections of household survey data for Taiwan to estimate the impact of working-hours restrictions and maternity benefits. Differential coverage across industrial sectors and demographic groups provides a unique opportunity to identify the impact of both policies in a single natural experiment framework. While working-hours restrictions have a negative impact on women's actual hours worked and employment, maternity benefits increase these labor inputs, implying that women value the opportunity to return to jobs they might otherwise have to leave.

I. Introduction

Governments worldwide have intervened in their labor markets by implementing special regulations to protect women, particularly working-

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hours restrictions and mandated maternity benefits. These protective measures were designed to achieve various social objectives, such as providing women with more time for family responsibilities and reducing women's exposure to the danger of sexual assault at night. Both types of legislation are widespread across countries, and they are included among the conventions of the International Labour Organization (ILO). However, the measures can have the unintended effect of raising the cost to firms of hiring women. Restrictions on women's night work and overtime work can limit the ability of firms to run extra shifts, and mandated maternity benefits, when financed by firms, act as a tax on the employment of women. In response to these mandates, firms may lower women's wages or substitute away from female labor. Women will also adjust their labor supply, depending on the degree to which the mandates constrain their working-hours options and the degree to which they value the benefits. Because the combination of supply and demand changes leads to ambiguous predictions of labor market outcomes, the impact of protective measures largely becomes an empirical issue.

This study quantifies the impact of protective legislation for women using a natural experiment framework applied to Taiwan's 1984 Labor Standards Law. As in many other countries, Taiwan's labor standards cover workers in industry but not in services. We exploit the differential coverage across sectors to calculate the labor market effects of the new labor standards, which contain provisions that restrict women's working hours and require employer-provided maternity benefits. Three years after enacting the law, the government created a viable enforcement structure that set the new Labor Law apart from previous regulations. This lag in enforcement allows us to identify the effects of both the law itself and the enforcement mechanism on women's labor market outcomes. The analysis uses repeated cross-sections of household survey data from 1982 to 1989 to estimate a set of equations for wages, hours worked, and employment.

Results indicate that the working-hours provisions have a negative impact on women's actual hours worked and employment, while the maternity benefits lead to an increase in these labor inputs. These effects do not occur until after 1987, when the government began its credible enforcement efforts. The increase in female employment implies that women value maternity benefits by more than the firm's expected cost, probably due to the employment guarantee. Job-protected maternity leaves provide women with the opportunity to return to jobs they might otherwise have to leave, thus boosting their firm-specific human capital and overall labor market experience. Neither protective measure has a significant wage impact, regardless of enforcement. The negative but insignificant wage response for maternity leave may reflect countervailing wage effects arising from gains in women's productivity.

The findings add depth to the labor law literature, where evidence on the impact of protective measures is inconclusive for industrialized countries and scant for developing countries. For example, Landes (1980) finds that early maximum-hours legislation for female workers in the United States negatively affected women's employment, while Goldin (1988) suggests that shorter workdays actually encouraged women with household responsibilities to enter the labor market. Research on more recent changes in working-hours restrictions in a number of industrialized countries has similarly found falling, rising, or even unchanging employment effects (Hunt 1999). Studies on maternity and parental leaves have generally found positive employment effects—although not always significant ones—in industrialized economies. Evidence on the wage effect has been mixed, reflecting variations in such factors as voluntary provision by firms, financing by national insurance, maternity leave duration, and the wage compensation rate. The Taiwan case provides a valuable opportunity to contribute to both of these debates on working-hours legislation and maternity benefits. In practice, the estimates should help legislators in other countries make more informed decisions as they revise their labor codes to include or repeal standards that protect women.

II. The Labor Standards Law

In 1984, the government enacted the Labor Standards Law, the first comprehensive labor law since Taiwan's industrialization.[1] At the national level, Taiwan already had more than 100 labor regulations in effect, but enforcement was often inadequate or nonexistent (U.S. Department of Labor, Bureau of Labor Statistics 1972). The main piece of existing legislation, the Factory Act, was limited to factories using mechanical power and employing more than 30 workers. The new Labor Standards Law aimed to increase compliance by unifying the various regulations and expanding coverage to all enterprises, regardless of size, in agriculture, industry, and some services.[2] As a result, the Labor Law maintains many features of the old laws but broadens their scope. In 1984, the Factory Act would have covered only 30% of all paid nonfarm employees, as compared with 67% covered by the new Labor Law. Among women, the Factory Act would have covered 37% of employees, as compared with 63% covered by the Labor Law. Even within manufacturing, the Factory

[1] The government promulgated the Labor Standards Law in August 1984 and added the Enforcement Rules for the Labor Standards Law in February 1985. These two laws are referred to collectively as the "Labor Law." Council of Labor Affairs (1992) has the text of the Labor Law and its main predecessor, the Factory Act.

[2] The Labor Law covers transportation, warehousing, telecommunications, and mass communication, but it excludes all other services. Civil service regulations cover government employees except where the Labor Law is more beneficial to them.

Act's firm-size constraint would have limited coverage to 62% of employees, whereas the Labor Law provided complete coverage.[3]

Two protective measures in the Labor Law apply to the working hours of women: a night-work prohibition and an overtime limit. The Labor Law prohibits women from working between 10 P.M. and 6 A.M., with exceptions for certain occupations or irregularities. These hour conditions are less restrictive than the Factory Act and the ILO standard (ILO Convention no. 89). The new law further limits women's overtime to 2 hours per day or 24 hours per month. The ceiling on men's overtime is higher by 1 hour per day and 22 hours per month. In contrast, the Factory Act imposed the same limits on overtime for both men and women. A feature of the Labor Law that directly affects the cost of employing young women is maternity leave. The law requires firms to grant female workers maternity leave before and after childbirth for a combined period of 8 weeks. Women receive full pay during their leave if their tenure exceeds 6 months and half pay otherwise. Also, employers may not dismiss a worker during her maternity leave. Maternity provisions in the Labor Law are more generous than those in the Factory Act, and they meet most of the ILO's standards (ILO Convention no. 103). Taiwan's protective measures are on par with those found in the other Asian tigers and Japan, reflecting the close similarity across the region in such legislation (Nataraj, Rodgers, and Zveglich 1998). Only Singapore does not place any working-hours restrictions on women.

The creation of a new law means little without adequate enforcement. Although charged with enforcing the Factory Act and other labor laws, local police departments often enforced these measures poorly or ignored them altogether. Initially, the Labor Law suffered from similar poor compliance among firms (Winn 1987). Three years after the promulgation of the Labor Law, the government took action to remedy the enforcement shortcoming by establishing the cabinet-level Council of Labor Affairs (CLA). Since the Labor Law was more of a legal reform than an innovation, creating this viable enforcement structure may have had a more significant impact on the labor market than the enactment of the law itself. Evidence cited in Chiu (1993, p. 259) shows that, through the end of 1989, the CLA had inspected almost 3,000 establishments, accounting for 3% of all establishments and almost 40% of employees covered by the Labor Law. Of those businesses it inspected, the CLA fined close to half and sent about 6% to court for violating one or more of the new labor standards. The inspections revealed that firms were more likely to comply with the requirements to adopt protective measures for women than they were with the other provisions in the Labor Law.

[3] These calculations assume that all manufacturing firms with more than 30 employees used mechanical power.

III. Theory

The impact of protective measures for women on their labor market outcomes can be predicted using a competitive labor market framework. This discussion focuses on partial equilibrium effects in industries covered by the legislation. Consider, first, the imposition of night-work prohibitions, which constrain the time of day when workers can be employed, and overtime limits, which constrain the total number of hours that workers may work within a day. Following Landes (1980), there are two groups of workers, females and males, and the group-specific labor supply is the number of people employed times the average number of hours per worker. The number of people employed is an increasing function of earnings (i.e., hours times the wage), while the preferred amount of working hours is an increasing function of the wage only. To focus only on the quantity effects, the Landes model assumes that women and men are perfect substitutes in production. As such, the wage is equal across workers. In equilibrium, total labor demand, which is a decreasing function of the wage, equals the sum of the group-specific labor supplies.

A working-hours restriction imposed on female workers will entail labor market effects if it is binding, that is, if women are working at night before a night-work prohibition or if women become hours-constrained by a cap on their total working hours.[4] The immediate impact of the regulation is that women's working hours become inelastic with respect to the wage for hours subject to the restriction (night or overtime), leading to an unambiguous decrease in the average working hours of women. Since the demand for total labor is not directly affected by the policy, this reduction in women's working hours will cause pressure for the wage to increase, leading firms to substitute men for women in production. As long as men's labor supply is not perfectly elastic, the wage for both men and women will rise, increasing the level of employment and average working hours of men. The effect on women's employment is ambiguous since the decline in hours and increase in wages have an ambiguous effect on earnings. Landes (1980) argues that, under plausible assumptions for the United States in the early 1900s, the model predicts a decline in women's employment following the introduction of working-hours legislation.

Introducing imperfect substitution between women and men into the model does not substantively change the predictions regarding the quan-

[4] Taiwan's particularly tight labor market during the 1980s, with unemployment rates less than 3% throughout the period for both men and women, makes it likely that the legislation would be binding and unlikely that firms would voluntarily agree to hours constraints on their workers.

tity of labor, although the demand effects are somewhat muted.[5] Women's wages will rise relative to men's as women's labor becomes relatively more scarce. Adding a richer labor supply structure to the basic model introduces possible second-order effects. In particular, Goldin (1988) argues that women may actually value shorter work days, given traditional gender differences in household responsibilities. Under these conditions, working-hours restrictions would induce more women to join the labor force over time and lead to an unambiguous rise in female employment. In the case of imperfect substitution, the initial rise in women's relative wages would be dampened by the inflow of new workers.

Consider next the imposition of mandated maternity benefits in a separate model of the labor market for young female workers. When employers bear the cost of providing the benefits, the mandate acts as a tax on the employment of young women. Employers lower their wage offers to eligible women by the amount of the expected cost of complying with the mandate, thus reducing their relative demand for young female workers. The expected cost is composed of wage payments to the beneficiary, administrative costs, and payments to temporary replacement workers, each weighted by the probability that female workers take maternity leave. If public funding covers beneficiary payments, then the decline in firms' relative demand for female workers will be smaller. On the supply side, those workers who value the benefit will accept a lower wage for a given quantity of labor supplied (Summers 1989). The value that women place on maternity benefits depends on the compensation rate and the leave duration.

The model predicts an unambiguous fall in young women's average wages after maternity leave is imposed. However, the first-order effect on women's employment depends on the differential values that firms and beneficiaries assign to the maternity benefits. When workers value the benefits by less than the cost to firms, female employment declines. When workers value the benefits by more than the cost to firms, female employment increases. A rise in employment could reflect more women entering the labor market before childbirth in order to gain eligibility for leave benefits, women returning to work sooner than they would have otherwise, or an accounting phenomenon from classifying women on leave as employed (Klerman and Leibowitz 1997; Ruhm 1998). When workers and employers value the benefits equally—in which case the equilibrium wage declines by the full cost of the benefits—the net effect on female employment is zero.

The first-order effect on working hours involves similar ambiguities.

[5] In the extreme case where there is no substitution between the two types of workers, the predictions for women still hold, but there will be no change in labor market outcomes for men.

Women who already work may increase their hours prior to the leave since the compensation package during the leave is often based on total previous earnings. Similarly, women who were working part time before childbirth may decide to increase their hours in order to qualify for leave benefits. A positive effect on working hours could also result from firms' decisions to shift away from hiring more workers toward employing existing workers for longer hours given the greater fixed costs associated with hiring female workers. Maternity leave benefits could cause total hours to fall if women choose to work fewer hours in exchange for more time with their children.

The employment guarantees that generally accompany maternity benefits legislation introduce the possibility of second-order effects. Waldfogel (1998) argues that job-protected maternity leaves help women to maintain favorable job matches and to avoid search costs from seeking alternative employment. Hence, women may assign additional value to the maternity benefits, increasing the likelihood that net female employment will rise.[6] A job-protected maternity leave can boost the productivity of female workers by reinforcing their labor force attachment and by increasing their firm-specific experience and training. Wage offers from employers may then rise over time, possibly by enough to overcome the initial cost of maternity benefits. As Ruhm (1998) notes, this firm-specific human capital argument is most plausible in the presence of a market failure, such as under conditions of adverse selection with asymmetric information for employees likely to take leave. Otherwise, the positive wage effect implies that workers and firms may have voluntarily negotiated a mutually better arrangement. Job-protected leaves could, however, encourage women to take a longer break from work than they would have otherwise taken. To the extent that long leaves cause women's job market skills to deteriorate, wage offers could fall.

IV. Methodology

The empirical exercise estimates the magnitude of changes in female labor market outcomes—wages, hours worked, and employment—due to the legal reform and creation of the enforcement agency. The notation that follows uses superscripts W, H, and E to indicate variables and coefficients specific to these labor market outcomes. The analysis begins by dividing workers into four demographic groups by age (younger, older) and gender (male, female). Since the differential coverage across industrial sectors is used to isolate the Labor Law effects from other contemporaneous changes, the analysis also divides workers into two sectors (un-

[6] Women have an additional incentive to assign value to maternity benefits when the benefits serve as a type of insurance to cover insufficient household savings for child rearing.

covered and covered). Next, the analysis estimates a wage function using the sample of nonfarm paid employees for each demographic group. Letting W_{ijt} denote wages for demographic group i in sector j at time t, the wage function is

$$W_{ijt} = \beta_i^W X_{ijt}^W + \sigma_{ij}^W S_{ij} + \tau_{it}^W T_{it} + \delta_{ijt}^W (T_{it} \times S_{ij}) + \varepsilon_{ijt}^W, \quad (1)$$

where X_{ijt}^W is a vector of observable productivity characteristics, S_{ij} is a dummy variable equal to one if the individual is employed in the covered sector, T_{it} is a dummy variable equal to one for years after the Labor Law was passed, and ε_{ijt}^W is an error term.[7]

For the coefficients, σ_{ij}^W is a sector fixed effect, τ_{it}^W is a period-specific effect, and δ_{ijt}^W is the period-sector interaction. The returns to observable productivity characteristics, β_i^W, are allowed to vary across demographic groups but are assumed invariant over time and between sectors. The sector fixed-effect controls for time-invariant differences between covered and uncovered industries. For example, the effect of an age bias in an industry's hiring practices will be captured by the sector fixed effect. The period-specific effect controls for shocks common to both sectors at a given point in time. For example, the impact of an economy-wide recession will be captured by the period-specific effect. The interaction term δ_{ijt}^W shows the effect of all aspects of the labor law on a given demographic group.[8] This term is referred to as the "difference-in-difference" (DD) estimator.

Separate regressions are estimated for each demographic group to allow for differences in rates of return to observable characteristics and to avoid the implicit assumption of identically distributed error terms—the unobservable characteristics—across demographic groups. The DD estimator for each demographic group shows the overall impact of the introduction and enforcement of the new Labor Law. To identify the effects of the protective measures for female workers, the DD estimators are differenced across demographic groups.[9] Working-hours restrictions will affect all females, while maternity benefits will only affect younger females. Any difference between older females and older males in the overall effect of the Labor Law will be due to the workings-hour restrictions. Using sub-

[7] Estimations also include a time dummy for the years after the enforcement mechanism was put into place and the associated sector interaction. These additional variables are omitted from the discussion without loss of generality.

[8] Demographic, sector, and time subscripts will be omitted in the discussion that follows unless necessary for the sake of clarity.

[9] This sequential differencing procedure is comparable to Gruber (1994), except that we use industry differences rather than location differences in coverage to identify policy effects. Also, we estimate separate regressions for each demographic group while Gruber pools the demographic groups together. While there is an efficiency cost to not pooling, the assumptions behind our specification are less restrictive.

scripts to denote the DD estimators for younger females (YF), older females (OF), younger males (YM), and older males (OM), the magnitude of the impact of working-hours restrictions on female workers' wages is denoted as $\delta^W_{\mathrm{OF}} - \delta^W_{\mathrm{OM}}$.

Any difference between younger females and younger males in the overall effect of the Labor Law will be due to both the working-hours restrictions and the maternity benefits. Therefore, we denote the magnitude of the impact of maternity benefits as the differential effect of the Labor Law on younger females and younger males ($\delta^W_{\mathrm{YF}} - \delta^W_{\mathrm{YM}}$) minus the effect of the working-hours restrictions ($\delta^W_{\mathrm{OF}} - \delta^W_{\mathrm{OM}}$). Comparing the DD estimators for younger females and older females is not sufficient to identify the effect of maternity benefits due to some age-specific, gender-blind provisions in the law, such as retirement benefits. To properly identify the effects of protective measures, we must assume that the changes in labor market outcomes for younger and older males differ only by these age-specific provisions. As the Labor Law does not include provisions aimed solely at younger men or older men, this rather unrestrictive assumption seems plausible.

In the estimation of the human capital wage function, the dependent variable is log monthly earnings. Since the observable productivity characteristics, X^W, include log monthly hours worked, the estimated Labor Law effects represent the effect of the policy change on hourly wages.[10] The remaining independent variables include a dummy variable for part-time work; a set of binary variables for education and major; potential experience and its square; enterprise specific tenure and its square; percent female within an occupation; a binary variable for supervisor status; industry, location, and urban dummies; and a binary variable for marital status. For women, a binary variable for married women with preschool age children is also included.

The second equation is an hours-worked function, which is estimated using the sample of nonfarm paid employees for each demographic group. Letting H denote working hours, the hours function is

$$H = \beta^H X^H + \sigma^H S + \tau^H T + \delta^H (T \times S) + \varepsilon^H. \tag{2}$$

The dependent variable is log monthly hours. The observable productivity characteristics for the hours-worked function include those from the wage

[10] Log monthly hours worked are calculated as the reported weekly hours multiplied by (52/12). We include log monthly hours among the independent variables in the wage estimations rather than calculate an implicit wage to avoid introducing measurement errors (Hamermesh 2000). For example, dividing reported earnings by reported hours understates the marginal payment for an additional hour of work by those working overtime. The fact that earnings are reported on a monthly basis while hours are reported on a weekly basis may also introduce measurement errors, although the direction of the bias is uncertain.

function (excluding the time variables), plus a variable for other household income.[11] Once the estimation is performed for the four demographic groups, the Labor Law effects on hours worked are calculated using the δ^H terms in the same manner as the approach using the δ^W terms.

The estimation of the Labor Law effects on employment is complicated by the fact that the dependent variable is a categorical variable. The probability of sector participation for each demographic group is estimated using a multinomial logit procedure and the entire sample of workers and nonworkers. Let E denote a categorical variable indicating industrial sector of employment, which takes on the values 0 if "nonparticipant," 1 if "uncovered," and 2 if "covered."[12] The multinomial logit estimates a set of coefficients such that the probability an individual will be in a given sector is

$$\Pr(E = j) = \frac{\exp(\beta_j^E X^E + \tau_j^E T)}{1 + \exp(\beta_1^E X^E + \tau_1^E T) + \exp(\beta_2^E X^E + \tau_2^E T)}, \quad (j = 1, 2). \quad (3)$$

The independent variables in X^E include the education, potential experience, location, urban, married, and preschool-age children variables found in X^W. In addition, a dummy variable for business owner within the household is included for all demographic groups, a dummy variable for school as the major activity is included for the younger groups, and a dummy variable for housework as the major activity is included for the female groups.[13] To calculate the difference in the probability of employment in sector j between two periods, we difference the predicted probabilities evaluated at the mean of X^E with the time dummy changing from zero to one. Differencing across sectors then gives the DD estimators; that is,

$$\begin{aligned}\delta^E = {} & [\Pr(\mathrm{E} = 2, \mathrm{T} = 1) - \Pr(\mathrm{E} = 2, \mathrm{T} = 0)] \\ & - [\Pr(\mathrm{E} = 1, \mathrm{T} = 1) - \Pr(\mathrm{E} = 1, \mathrm{T} = 0)]. \end{aligned} \quad (4)$$

[11] In the hours-worked function, the independent variables from the wage function serve as proxies for wage offers. Other labor supply studies that take a similar approach include Eissa and Leibman (1996), Buchmueller and Valletta (1999), and Cullen and Gruber (2000). One can also include a predicted wage in the hours function; our substantive results do not change using this approach.

[12] A richer specification of the employment decision would involve a sequential choice structure: first a participation choice, then an industry choice. Data limitations in terms of appropriate identifying variables prevent the pursuit of this strategy.

[13] The survey asks individuals "What was your main activity in the previous week?" The range of responses include "attending school and working," "attending school but not working," "housework and working," and "housework but not working." Hence, some students and housewives are participating in the labor force.

Having derived the DD estimators, the Labor Law effects are calculated in the same manner as the Labor Law effects on wages.

V. Data

To test the Labor Law's impact on female wages, hours worked, and employment, this study uses household survey data from the Manpower Utilization Survey, produced by the Directorate-General of Budget, Accounting, and Statistics, Executive Yuan, Taiwan. The surveys have collected earnings and employment data for the month of May in every year since 1978.[14] Because the government enacted the Labor Law in August of 1984 and established the CLA in 1987, we use 1982–84 data for the period before the law was enacted ("pre-law") and 1985–86 data for the period after the law was enacted but before enforcement began ("post-law"). We use 1987–89 data for the period after the CLA was established ("post-CLA"). Only civilian, nonfarm, private-sector paid employees are included in the wage and working-hours estimations.

Labor Law coverage is categorized according to one-digit Standard Industrial Classification codes. The uncovered industries are commerce, business services, and social and personal services. All other industries fall within the covered sector. The categories "younger" and "older" are defined in a way that isolates the effects of maternity benefits. In particular, maternity benefits are an anticipated cost to the firm. The probability that a firm will have to provide maternity benefits for a worker depends on the fertility rate of her demographic group.[15] In 1985, fertility rates were 129 live births per thousand women for women aged 20–24 years, 158 for women aged 25–29 years, and 56 for women aged 30–34 years. The comparable figure for women aged 35–39 years was only 12 (Directorate-General of Budget, Accounting, and Statistics [DGBAS] 1993). Other years in the 1980s show a similar pattern in fertility rates. Hence, the age of 35 years is used as the dividing line between "younger" and "older" workers. The data are pooled across years and sectors within demographic groups, but the model is estimated separately across groups.

To ensure that the final sample includes only workers with a higher degree of labor market attachment, the sample is limited to individuals of working age (20–64 years old). However, using this same age range in

[14] Monthly earnings are in constant 1991 prices deflated using the Consumer Price Index. The monthly earnings of individuals whose reported earnings exceeded the top code are multiplied by 1.2. Since more males hit the top code, the conversion avoids understating male wages.

[15] This reasoning lies behind an alternative modeling approach in Gruber (1994), whereby age-specific fertility rates are included (among other data on insurance coverage) in the construction of predicted individual-specific costs. This cost measure in turn replaces the group dummy, and in a linear specification it yields a coefficient that signals the extent to which costs are shifted to wages.

every year (i.e., a time series of cross-sections of all the working-age observations) introduces two potential sources of sampling bias. First, some observations that would be included in the full sample in some periods would be dropped in others. For example, individuals who are 20 years old in the post-law period would not have met the minimum age requirement in the previous period. Second, some observations that are in one demographic group in one period would be in another demographic group in others. For example, individuals who are 30 years old in 1983 would be in the younger group in the pre-law period but in the older group in the post-CLA period. If the data included the same individuals in every year, the analysis could be limited to those individuals who are classified in the same demographic group in each period. Since we do not have a true panel, we must follow pseudocohorts in the estimations. In the subsample of the working-age population, the younger group includes only individuals who would have been aged 20–27 years in 1982 and aged 27–34 years in 1989 (i.e., individuals in the high-fertility range in every year), and the older group contains only individuals who would have been aged 35–57 years in 1982 and aged 42–64 years in 1989 (i.e., individuals in the low-fertility range in every year).[16]

Descriptive statistics for the final sample of paid employees are found in table 1. These sample means show that the younger groups are more educated than the older groups, and the covered sector draws a disproportionate share of less-educated workers relative to the uncovered sector. Gender differences in educational attainment are also pronounced, particularly with the relatively large share of older women who have attained just primary school or below and the relatively large share of younger women with high school and vocational degrees. With just one exception, workers in the covered sector have more experience and establishment-specific tenure than workers in the uncovered sector. Large shares of workers are employed in the North, where Taipei is located, and in an urban location. Relative to men, women exhibit a greater tendency to work part time and a lower tendency to hold supervisory positions.

VI. Identification

The identification of the Labor Law effects rests on the differential coverage by industry. However, the estimator may pick up the effect of exogenous trends that differ by sector. For example, the period is one of explosive growth in Taiwan's manufacturing, which may spur covered-

[16] Robustness tests are performed using various pooled samples ranging in size from pseudocohorts to the full sample. In every case, the direction of the policy effect is unchanged. Using the full sample does cause a large downward bias in the magnitude of the maternity benefit impact on hours worked, which mostly comes from including the very young and the very old in every year.

Table 1
Variable Means and Sample Sizes by Demographic Group and Sector

	Younger Females		Older Females		Younger Males		Older Males	
	Uncovered	Covered	Uncovered	Covered	Uncovered	Covered	Uncovered	Covered
Education (%):								
Primary and below	9.93	30.17	56.82	85.39	11.43	25.41	36.90	67.36
Middle school	14.36	25.02	11.38	7.26	22.28	30.98	14.05	12.48
High school/vocational	51.38	36.98	19.06	5.83	39.44	31.16	26.48	13.15
Junior college and above	24.34	7.84	12.75	1.52	26.85	12.45	22.57	7.01
Potential experience (years)	6.54	9.38	29.27	32.38	9.10	11.07	30.33	31.60
Tenure (years)	2.80	3.04	6.26	4.78	2.87	3.52	8.27	8.70
Location (%):								
North	59.21	48.73	54.07	44.59	60.42	48.94	56.36	47.60
Central	15.79	23.39	16.38	27.78	15.54	23.08	16.36	21.67
South	23.03	27.14	25.68	26.52	22.34	26.76	25.33	28.89
East	1.97	.73	3.87	1.12	1.69	1.22	1.96	1.84
Urban	71.39	35.07	59.32	26.51	70.31	40.94	66.17	38.27
Other (%):								
Part-time workers	3.04	5.78	11.48	11.84	2.08	3.39	6.27	6.87
Supervisor	2.87	2.40	8.07	2.84	8.70	7.05	24.89	11.39
Percent female	63.43	59.06	58.92	48.56	37.92	24.98	35.32	21.29
Married	24.76	44.05	70.27	85.79	42.79	50.68	85.13	88.17
Preschool children	14.93	27.13	7.12	6.07				
Sample size:								
Pre-law (1982–84)	3,236	6,592	740	2,584	3,255	10,739	1,920	7,235
Post-law (1985–86)	2,526	5,200	662	2,244	2,250	7,627	1,347	5,168
Post-CLA (1987–89)	4,938	8,239	1,437	4,305	4,011	11,852	2,462	8,916

NOTE.—Authors' calculations.

sector employment growth relative to the uncovered sector. To the degree that sectoral differences in the effects of omitted factors follow a monotonic trend, adding sector-specific time trends to the model should remove the potential bias in the parameter estimates. The trend coefficients are allowed to differ by sector but not by demographic group. In a constrained estimation strategy, each labor market outcome is first regressed on a trend, a sector dummy, and a trend-sector interaction, using the pooled sample of all demographic groups. Then each labor market outcome is regressed for each demographic group separately on the full set of independent variables, including the trends, subject to the constraint that the trend coefficients are the values from the first-stage estimation.

A related source of bias is the influence of contemporaneous sector-specific shocks. In particular, the liberalization of foreign currency controls in 1987 probably had a larger impact on the covered sector, which is dominated by traded goods, than on the uncovered sector. However, this policy should not bias the model's estimates since the shock is not expected to have a differential gender effect. Similarly, the government lifted martial law in July of 1987 and legalized strikes in 1988. The resulting increase in workers' bargaining power, which is likely to have affected manufacturing more than services, suggests that we should view enforcement institutions more broadly to include both government agencies and worker organizations. A contemporaneous shock that did have differential effects across sectors and gender is the establishment of new fines for employers who neglect to abide by the "equal pay for equal work" clause. In principle, the equal pay clause will limit firms' ability to lower women's wages relative to those of men, resulting in smaller estimated negative changes in wages. In practice, legislation that requires equal pay for equal work within an occupation and enterprise will have little impact on women's relative wages if women are segregated by occupation and industry, as is the case in Taiwan (Blau and Kahn 1996).

Using industry as the main identifying variable requires the government's choice of covered industries to be exogenously determined. If policy placement is correlated with unobservable sector-specific factors, the estimated coefficients on the policy variables will be subject to endogeneity bias. For example, if industries are covered by the law because they tend to have women work long hours or are less likely to provide maternity benefits, then using the variation across industries in a single period puts a downward bias on the estimated policy effects. The estimation strategy corrects for endogenous policy placement through the sector fixed effect term (Rosenzweig and Wolpin 1986). Even so, detailed evidence from Taiwan's government archives, newspapers, and periodicals and interview data in Chiu (1993) strongly suggest that the choice of covered industries is exogenous. Furthermore, pressure from the United States to adopt core labor standards—which did not include protective

measures for women—in sectors producing export goods helped influence Taiwan's choice to cover manufacturing while leaving services exempt (Zimmerman 1992).

Another potential source of bias arises from the nonrandom allocation of workers across industries. For example, evidence on the fertility and employment decisions of married women suggests that women's labor force attachment grew during the period: women waited longer before marrying, had fewer children, and sought alternate child-care arrangements (DGBAS 1994). More productive female workers may have selected into industries that were mandated to provide maternity leave, in which case the estimated wage effect may be biased upward. Including a broad range of productivity characteristics in the model helps to ensure that the estimates will reflect the impact of the labor law and not inherent differences between the treatment and control groups. The model further controls for time-invariant factors that differ by sector and demographic group by including a sector dummy in the separate regressions for each demographic group. Selection bias is more problematic if the unobservable factors associated with sector choice are changing over time. Borjas (1999) suggests using longitudinal data to track individual workers over time to eliminate this problem. In the absence of panel data, the study's use of pseudocohorts will mitigate the bias by tracking samples of workers drawn from the same population over time.

Finally, workers moving between industrial sectors in response to the law change may also bias the results. For example, if young women switch from uncovered to covered sector employment in response to the provision of maternity benefits, the estimated employment change will be overstated. In essence, these individuals are counted twice: once when they leave the uncovered sector and again when they enter the covered sector. Although the share of workers switching industrial sectors is only about 1% for any demographic group in any period, we address this problem by also estimating the model excluding any workers who have changed sectors of employment in the past year.[17]

VII. Estimation Results

A. Difference-in-Means Calculations

Table 2 reports the effect of the protective measures on wages, hours worked, and employment, without controlling for detailed worker characteristics. For each demographic group and time period, the table reports the mean log hourly wage, mean log monthly hours, and probability of employment in a given sector. The wage and hour means are calculated

[17] The survey includes information on previous industry, occupation, and type of employment for all workers with less than 24 months of enterprise-specific tenure.

Table 2
Average Labor-Market Outcomes and Difference Estimates of Labor Law Effects

	Log Hourly Wage*		Log Monthly Hours†		Employment (%)‡	
	Covered	Uncovered	Covered	Uncovered	Covered	Uncovered
Younger females:						
Pre-law (1982–84)	3.82	3.70	5.35	5.32	12.02	23.49
	(.01)	(.01)	(.00)	(.00)	(.24)	(.31)
Post-law (1985–86)	4.03	3.82	5.34	5.30	11.07	21.77
	(.01)	(.01)	(.01)	(.00)	(.29)	(.38)
Post-CLA (1987–89)	4.03	3.84	5.34	5.32	11.04	22.29
	(.02)	(.01)	(.01)	(.01)	(.24)	(.32)
Older females:						
Pre-law (1982–84)	3.79	3.59	5.27	5.28	2.52	9.55
	(.02)	(.01)	(.01)	(.00)	(.10)	(.19)
Post-law (1985–86)	3.93	3.69	5.26	5.29	2.80	10.34
	(.02)	(.01)	(.01)	(.00)	(.13)	(.24)
Post-CLA (1987–89)	3.93	3.71	5.30	5.27	3.15	10.32
	(.03)	(.02)	(.01)	(.01)	(.11)	(.20)
Younger males:						
Pre-law (1982–84)	4.08	4.08	5.37	5.34	12.67	40.23
	(.01)	(.01)	(.00)	(.00)	(.26)	(.39)
Post-law (1985–86)	4.25	4.24	5.3	5.34	13.25	43.49
	(.01)	(.01)	(.01)	(.00)	(.31)	(.46)
Post-CLA (1987–89)	4.32	4.29	5.37	5.36	13.39	42.14
	(.02)	(.01)	(.01)	(.00)	(.26)	(.37)
Older males:						
Pre-law (1982–84)	4.32	4.27	5.32	5.30	5.70	23.57
	(.01)	(.01)	(.01)	(.00)	(.14)	(.26)
Post-law (1985–86)	4.43	4.35	5.32	5.31	5.46	21.76
	(.01)	(.01)	(.01)	(.00)	(.17)	(.31)
Post-CLA (1987–89)	4.46	4.40	5.31	5.32	5.87	20.71
	(.02)	(.01)	(.01)	(.00)	(.15)	(.25)
	Estimates of Labor Law Effects					
Working-hour restrictions:						
Law effects	−.20		.61		2.06	
	(.35)		(.17)		(.60)	
CLA effects	−.24		−5.79		1.10	
	(.34)		(.16)		(.06)	
Maternity benefits:						
Law effects	−8.22		−1.40		−5.51	
	(.42)		(.20)		(.12)	
CLA effects	−4.56		3.75		.93	
	(.42)		(.20)		(.12)	

NOTE.—Authors' calculations. Standard errors are in parentheses.
* Wage rate is calculated as monthly earnings divided by monthly hours worked. Labor Law effects are in log points times 100.
† Labor Law effects are in log points times 100.
‡ The figures represent the probability of paid employment in a given sector. Labor Law effects are in percentages.

from a regression of the dependent variable on the set of sector, demographic, and temporal dummies (and their interactions), using the pooled sample of civilian, nonfarm, private-sector paid employees. The employment probabilities are calculated from a multinomial logit estimation of sector choice on the set of demographic and temporal dummies (and their interactions), using the pooled sample of all individuals.

These sample means show modest wage discrepancies between the covered and uncovered sectors, while men consistently have higher wages,

on average, than women. Working-hours discrepancies between sectors and demographic groups are modest, although younger groups do exhibit a greater tendency, on average, to exceed the statutory work week.[18] Differentials in sample means between the two sectors are more pronounced for employment. All demographic groups are more likely to choose jobs in the uncovered sector, reflecting Taiwan's structural transformation out of manufacturing-based jobs into services during the 1980s. The last set of numbers report the effects of the working-hours restrictions and maternity benefits. The calculations to derive these figures follow the differencing procedure across demographic groups described above for δ^W, δ^H, and δ^E. Results indicate that working-hours restrictions entail a sizable cutback in actual hours worked after enforcement, while the employment effect is positive and the wage effect inconsequential. Maternity benefits are associated with a substantial wage penalty, both after the Labor Law was passed and after enforcement began. Women's total labor input fell after maternity benefits were enacted but rose with enforcement.

B. Point Estimates

Simply examining the difference in means does not control for compositional changes within the sectors over time. Table 3 presents the estimated impact of the protective measures on wages, hours worked, and employment.[19] Estimates for the basic model indicate that the working-hours restrictions have a negative impact on women's actual hours worked and employment. Women's working hours decline by about 6.1%, and women's employment falls by almost 1 percentage point. These effects do not occur until after 1987, when the government began its credible enforcement efforts. Total labor input (employment times working hours) declines by about 7% in the post-CLA period. Before enforcement, the legislation had virtually no bite. Working-hours restrictions have no significant impact on women's relative wages, regardless of enforcement. The results are robust to excluding workers who have switched sectors of employment from the estimation, and they are robust to adding a time trend to the model. Our results are consistent with the theoretical prediction of a decrease in women's hours worked, and they support the Landes (1980) prediction of a concurrent decline in women's employment. The finding that the restrictions do not entail significant wage effects suggests a high degree of substitutability between men and women.

[18] A worker working the statutory work week of 48 hours would have about 5.34 log monthly hours. Straight-time hours, overtime hours, and nighttime hours are not reported separately.

[19] Coefficient estimates on the observable characteristics in the wage, hours-worked, and employment equations are, with few exceptions, significant at a 95% level of confidence, and the statistically significant coefficients have the expected signs.

Table 3
Estimated Labor Law Impact on Women's Wages, Hours Worked, and Employment

	Log Hourly Wage (Log Points × 100)	Log Monthly Hours (Log Points × 100)	Employment (%)
Basic model:			
Working-hours restrictions:			
Law effects	1.66	.61	−.10
	(3.16)	(2.04)	(.37)
CLA effects	−1.34	−6.10**	−.90*
	(3.10)	(2.00)	(.40)
Maternity benefits:			
Law effects	−4.58	−.65	−1.13
	(3.65)	(2.26)	(1.01)
CLA effects	−3.63	4.50*	2.54*
	(3.60)	(2.23)	(1.10)
Model without sector changers:			
Working-hour restrictions:			
Law effects	1.20	.69	−.17
	(3.17)	(2.04)	(.37)
CLA effects	−.66	−6.22**	−.86*
	(3.12)	(2.01)	(.40)
Maternity benefits:			
Law effects	−3.62	−.48	−.94
	(3.67)	(2.28)	(1.01)
CLA effects	−4.06	4.26+	2.48*
	(3.63)	(2.25)	(1.09)
Model with time trends:			
Working-hour restrictions:			
Law effects	2.04	.62	−.00
	(3.13)	(2.04)	(.37)
CLA effects	−.95	−6.09**	−.79*
	(3.07)	(2.00)	(.40)
Maternity benefits:			
Law effects	−5.29	−.70	−.95
	(3.61)	(2.26)	(1.01)
CLA effects	−4.08	4.49*	2.76*
	(3.57)	(2.23)	(1.10)

NOTE.—Authors' calculations. CLA = Council of Labor Affairs. Standard errors are in parentheses.
+ Statistically significant at the .10 level (two-tailed test).
* Statistically significant at the .05 level (two-tailed test).
** Statistically signficant at the .01 level (two-tailed test).

The impact of maternity leave provisions differs substantially. After enforcement began, women's working hours increased by 4.5% and their employment rose by 2.5 percentage points. The increase in total labor input after enforcement is about 7%, with the bulk of the change being due to increased hours worked. This increase almost exactly offsets the decline in young women's labor supply from the working-hours restrictions. Average wages of younger women, on the other hand, show no significant change due to maternity benefits, either before or after the enforcement structure was put into place. These results are robust to the exclusion of workers who change their sector of employment, and they

are robust to adding a time trend to the model. The employment and hours results are consistent with the theoretical prediction that the high value of maternity benefits to female workers actually leads them to increase their participation rate, hours worked, or both. Women in Taiwan apparently value not only the financial benefits but also the opportunity to return to their previous employers after childbirth. The lack of a significantly negative wage response may reflect the theoretical prediction that, over time, gains in women's firm-specific human capital and their productivity can counteract the restrictive effects of the mandated benefits.[20]

C. Plausibility

Taiwan's working-hours restrictions for women differ in structure from those of countries examined elsewhere. However, our substantive conclusions are consistent with Hunt (1999), which examines Germany's reduction in the standard workweek for both men and women. Hunt finds a pass-through effect from standard hours to actual hours that is close to one-for-one. Given that average standard hours in Germany fell from 39.5 in 1985 to 37.4 in 1994, Hunt's pass-through estimates correspond to a reduction of about 5% in actual hours. Taiwan's estimated reduction in actual hours of 6.1% is somewhat larger. Starting from a base-period average of 45 hours worked per week for older women in the covered sector, a 6.1% decline in hours due to the legal restrictions corresponds to a decline in an average workweek of about 2.5 hours. Hunt's estimated employment effects are generally imprecise but indicative of declines in the neighborhood of 2%–4%. Taiwan's estimated reduction in employment is considerably smaller.

Regarding maternity benefits, our finding of a 2.5-percentage-point increase in employment falls in the range of estimates found in three benchmark studies: Kane (1998), Ruhm (1998), and Waldfogel (1999). The employment effects are all positive, with the magnitude increasing as the benefits become more generous. Waldfogel's study of fairly short, unpaid leaves in the United States yields a mildly positive employment effect, while Ruhm's results for 9-month compensated leaves in Europe indicate that employment-population ratios rose by up to 4%. The 2.5-percentage-point increase in employment for Taiwan may seem large considering that

[20] The wage results are also robust to using a constructed hourly wage as the dependent variable. The point estimates change somewhat, but none of the coefficients for working-hours restrictions or maternity benefits are statistically significant. The hours-worked results are also robust to the use of an instrumental variables approach, in which the experience variables are used as the instruments to identify the wage effects on hours, following examples in Killingsworth and Heckman (1986). Point estimates do differ somewhat, but our conclusions remain the same.

the maternity benefits are not as generous as those in Europe. However, with relatively lower female labor force participation rates in Taiwan, there was more scope for the new legislation to induce women to enter the workforce. At the time of the law change, Taiwan's female labor force participation rate stood at 45%, as compared with 85% for Sweden, 60% for Switzerland, 59% for the United Kingdom and Norway, and 66% for the United States (Blau and Kahn 1996).[21] To add further perspective, the 2.5% result holds for the treatment group, which includes only paid, nonfarm, private sector, young females employed in the covered sector. The overall labor force participation rate would change by less than 2.5 percentage points.

Among existing maternity leave studies that estimate hours-worked equations, the strongest effects are found in Gruber (1994), which reports that hours worked rose by 5% in the United States following the mandated provision of maternity benefits in health insurance plans. For Taiwan, the increase in hours worked after enforcement is smaller but still substantial. A likely explanation is that the compensation package in Taiwan is based on total previous earnings, so employees can increase the amount of the benefit they receive by adjusting their weekly hours worked prior to taking leave. The estimated .05 log point change in hours worked represents an increase in actual hours worked of about 2.1 hours per week; that is, young women in the covered sector would have increased their weekly hours from about 46.2 hours before enforcement to 48.3 hours after enforcement. If we consider the combined effect of the working-hours restrictions and maternity-leave provisions after enforcement, the change in weekly hours in the covered sector would have been a decline of .02 log points, which amounts to 45.5 hours per week. Finally, the wage effects of −4.6% after enactment and −3.6% after enforcement, although imprecisely estimated, are consistent with other studies of mandated benefits in which pecuniary costs are imposed on firms via the mandated provision of paid health benefits (Gruber 1994; Kane 1998; Ruhm 1998).

VIII. Conclusion

This analysis has used a single natural experiment framework to identify the labor market impact of two types of protective measures for female workers: working-hours restrictions and mandated maternity benefits. Evidence based on Taiwan data indicates that working-hours restrictions lead to a decline in women's actual hours worked and employment but to no significant change in their wages. Results for maternity benefits indicate a sizable increase in women's working hours and employment, also without a significant reduction in wages. Working-hours restrictions

[21] Just a few European countries, including Italy and Austria, had female labor force participation rates that were in the 44%–45% range.

and maternity benefits had no bite after the legislation was enacted. It took the subsequent creation of a cabinet-level enforcement structure, the Council of Labor Affairs, to see significant labor market impacts. The result provides a stark reminder that, particularly in developing countries, institutional reforms may be necessary to provide the incentive for agents to respond to legal changes that otherwise lack credibility.

The analysis, like others in the natural experiment literature, may draw skepticism as to whether it has truly captured the impact of the legal change. We have presented a number of problems common to natural experiments and then exploited the features of the data and the policy change to address the problems. In particular, the magnitudes of the estimates are robust to the addition of a time trend to the model, suggesting that, for the period under consideration, exogenous trends in the outcome variables did not differ across the covered and uncovered sectors. The estimates are potentially subject to endogeneity bias from the nonrandom allocation of workers across industries. However, controlling for demographic characteristics and following pseudocohorts helps to mitigate this bias through the comparison of similar workers over time. Also, model estimations with and without workers who have switched sectors of employment yield similar results, suggesting that movements between sectors are not problematic to the analysis.

In addition to their direct labor market consequences, protective measures for women can facilitate the realization of broader social objectives. For example, both working-hours restrictions and maternity benefits can improve the quality of child care by encouraging women in the labor force to spend more time at home. Infants, in particular, can gain valuable health benefits during the crucial early months with their mothers from frequent and more extended breastfeeding, close monitoring, and the limited need for outside child care. Ruhm (2000) supports these arguments with evidence that increasing the duration of parental leave lowers the mortality rate in early childhood. Our study suggests that, for women, maternity benefits can be a less costly policy instrument than working-hours restrictions in achieving such social goals. Protective measures that constrain women more than men in their working-hours options can hinder women's progress toward equity in the labor market, while maternity benefits support women's efforts to remain and advance in the labor market.

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