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# **DOES LABOR LEGISLATION BENEFIT WORKERS? WELL-BEING AFTER AN HOURS REDUCTION**

Daniel S. Hamermesh, Daiji Kawaguchi and Jungmin Lee\*

## **ABSTRACT**

Are workers in modern economies working “too hard”—would they be better off in an equilibrium with fewer work hours? We examine changes in life satisfaction of Japanese and Koreans over a period when hours of work were cut exogenously because employers suddenly faced an overtime penalty effective at lower standard hours. Using repeated cross sections we show that life satisfaction increased relatively among those workers most likely to have been affected by the legislation, with the same finding using Korean longitudinal data. In a household model estimated over the Korean cross-section data we find some evidence that a reduction in the husband’s workhours increased his wife’s well-being. These results are consistent with the claim that legislated reductions in work hours can increase workers’ utility.

\*Professor in economics, Royal Holloway University of London, Sue Killam professor emeritus, University of Texas at Austin, network director, IZA, and research associate, NBER; professor, University of Tokyo, and research associate, IZA; professor, Seoul National University, and research associate, IZA. We thank participants at a seminar at the University of Texas at Austin and especially Steve Trejo for helpful comments.

## 1. Background

Labor legislation existed in ancient economies, is ubiquitous today, and is usually justified as benefiting a particular group of workers. Much of the discussion surrounding these laws is about their overall effect on labor markets. What has hardly ever been examined is whether those workers at whom the legislation is targeted do in fact view themselves as having benefited from it. In this study we consider this question in light of a model of the impact of two particular pieces of legislation that increased employers' disincentives to employ overtime hours.

In some cases, the discussions surrounding a piece of labor legislation suggest that its purpose is to benefit society by restricting the behavior of some agents who may be directly affected (e.g., “work-spreading” under the overtime and child-labor provisions of the U.S. Fair Labor Standards Act of 1938). In other cases, the legislation is rationalized as benefiting a group of workers who are believed to have been subjected to some adverse working condition (e.g., the U.S. Occupational Safety and Health Act of 1970, and the U.S. Keating-Owen Act of 1916 limiting child labor). The question in these latter instances is whether the “protected” group is better off because of the protection.

Until the mid-1980s the work year in Japan was one of the longest among wealthier nations. Indeed, *karoshi*—death from overwork—was viewed as causing the demise of as many as 10,000 Japanese workers per year (Nishiyama and Johnson, 1997). Partly in response to concerns about excessive work, between the mid-1980s and mid-1990s Japan reduced the standard workweek from 48 to 40 hours, hoping to reduce employers' incentives to demand long workweeks. In Korea, also partly due to worries about death from overwork (*kwarosa*), the standard workweek was reduced from 44 to 40 hours between 1999 and 2009.<sup>1</sup>

The question here is whether those workers at whom reductions in work hours were targeted actually did benefit from the legislation aimed at aiding them. There is no question that these legislative

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<sup>1</sup>See [http://english.yonhapnews.co.kr/n\\_feature/2013/04/24/61/4901000000AEN20130424009700315F.HTML](http://english.yonhapnews.co.kr/n_feature/2013/04/24/61/4901000000AEN20130424009700315F.HTML) .

changes directly affected hours: In Lee *et al* (2012) we showed that the Japanese overtime legislation and the similar Korean legislation more than a decade later did reduce workhours economy-wide. And Kawaguchi *et al* (2013) demonstrated that in both countries the reduction was especially great among workers in those demographic groups that were targeted—workers in groups that had especially long workhours before the legislation’s enactment. In Japan that study showed that a one standard-deviation increase in the propensity to have been affected by the legislation reduced weekly work time by 0.4 hours, and in Korea by 2.2 hours.

An obvious cost of an exogenous reduction of workhours is the reduction of output. Indeed, Hayashi and Prescott (2002) claim that the reduction of workhours was part of the reason why Japan experienced a decade-long stagnation in the 1990s.<sup>2</sup> In a perfectly competitive labor market with no market failure, the workings of competition will equate the marginal rate of substitution of consumption for leisure to the marginal product of labor. This equality leaves no room for labor regulations to improve the welfare of workers. Indeed, the demand-side incentives that successfully lead employers to cut work time will reduce the well-being of the affected workers.<sup>3</sup>

It is possible, however, that informational asymmetries and externalities are prevalent in the labor market—in particular, that the need to cooperate in the workplace creates a “rat race” (Akerlof, 1976) that generates sub-optimal excess effort. For examples, a law firm’s inability to learn its associates’ commitment to the firm may cause a rat race among its associates (Landers *et al*, 1996); a workaholic boss may impose inefficiently long workhours on his co-workers who are better off staying with him than going elsewhere (Hamermesh and Slemrod, 2008).<sup>4</sup> As another possibility, high unemployment and/or monopsony

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<sup>2</sup>They do not, however, mention any effect on welfare or well-being, because they adopted a reduced-form model in which the reduction of work hours in equilibrium is induced by a change in preferences.

<sup>3</sup>A layperson’s view of this worsening was cogently expressed by Matias (2002, p. 7): “While the Eight-Hour-Day Restoration and Workplace Flexibility Act was intended to ‘protect California’s working people’ as it has for over 80 years, in fact it eliminates a great deal of workplace flexibility by prohibiting the employer and employee from tailoring their working relationship to suit each other’s needs. The law no longer permits employees to take comp time in lieu of pay for extra hours worked.”

<sup>4</sup>Arrow and Dasgupta (2009) present an interesting theoretical discussion of the rat-race idea and how one might infer its existence from various kinds of data.

(Manning, 2003) might give employers the power to exploit workers, leading hours to depart from what they would be in the utility-maximizing wage-hours package. Thus, in presence of market failure in the labor market the reduction of workhours initiated by stricter regulation does not necessarily reduce production and thus the consumption of workers.

Given the relevance of these possible market failures to the labor market, we ask if the regulation of workhours at least improves indices of the welfare of targeted workers, namely their life satisfaction. It thus also speaks to the general question of whether, at least along the dimension of hours of work, a modern labor market produces outcomes that may be sub-optimal for some workers even at times of full employment.

Pezzini (2005) is perhaps the closest relative of this study, although she could not link legislative changes to those particular individuals most likely to be affected by them. Ludwig *et al* (2012) did do this, demonstrating that the happiness and physical and mental health of individuals who were randomly selected into an experiment in housing mobility rose relative to those not selected. Neither of these studies focused on the impact of particular legislation on overall happiness of individuals likely to have been affected. Estevao and Sá (2008) did examine the effects of the imposition of a 35-hour workweek in France, but they focused only on the narrow and very partial question of satisfaction with work hours and could not identify those workers most likely to have been affected by the law. No previous study has examined whether the purported direct beneficiaries, either those likely to have been affected or those explicitly affected by some piece of labor legislation, actually felt themselves generally better off as a result of the legislation and could thus be viewed as having benefited from its enactment.<sup>5</sup>

More generally, this study focuses on whether individuals who are targeted by a particular public policy actually feel that they have benefited from it. While the answer would seem obvious, examination of the impact of a piece of legislation on the well-being of those who are expected to be its beneficiaries is missing from the evaluation of policy. We hope to begin filling that gap.

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<sup>5</sup>One effort (Gruber and Mullainathan, 2005) analyzed how increased taxes on cigarettes affected smokers' happiness.



## 2. Institutional, Empirical and Theoretical Motivation

As Kawaguchi *et al* (2013) discuss, in Japan in the 1980s and in Korea in the early 2000s, legislation was enacted to provide incentives to reduce long hours. In Japan, the labor standards act defines the legal standard for workhours. With consent of the workers' representatives, employers are allowed to ask for overtime, but it must be compensated by at least a 25 percent wage premium. Historically, legal standard workhours had been 8 hours per day and 48 hours per week, but Japanese legislators passed a revised labor standards act in 1987 to reduce weekly standard hours to 40 hours. The revised law became applicable from April 1988 with a moratorium period given to small- and medium-sized establishments and those in industries with long hours.<sup>6</sup> By 1994, except for a few industries with long hours, and for some smaller establishments, the law was fully effective. Even these exemptions ended early in 1997.

In Korea, in May 2000, the Korean Tripartite Commission, consisting of the representatives of employees, employers, and the government, established the Special Committee on Reduction of Working Hours to investigate the possible introduction of a shorter workweek. The Committee arrived at a consensus on the general principles of gradual reduction of working hours for the purpose of creating new jobs and improving the living standard of workers. In August 2003, the bill to revise the Labor Standards Act passed the Congress. The new law lowering weekly standard hours from 44 to 40 began to be applied in mid-2004, initially covering workplaces of 1000+ employees, and by mid-2008 applying to all but the smallest establishments (with overtime hours penalized at 50 percent): establishments in all industries with 300-999 employees in July 2005, those with 100-299 employees in July 2006, those with 50-99 employees in July 2007, and those with 20-49 employees in July 2008.

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<sup>6</sup>Additional restrictions on work hours have been imposed on female workers, such as prohibitions on overtime exceeding two hours a day, six hours a week and 150 hours a year. The 1985 amendments to the Labor Standards Law exempted a variety of occupations and industries from such overtime restrictions on women (Kato and Kodama, 2014). This policy change, however, should not affect our empirical results, because it occurred before the initial cut in standard hours in 1988. Moreover, we calculate the propensity score for having been affected by the policy change, a key variable in the following empirical analysis, based on the time use survey in 1986.

Let us consider the impact of a change in legislated overtime provisions in standard hours when the change does not affect the straight hourly wage rate. We use the example of Japan, with standard hours dropping from 48 to 40, but we could easily present the example, *mutatis mutandis*, of Korea's cut in standard hours from 44 to 40. Figure 1a depicts this change in the standard textbook case, with workers "optimally" choosing their hours after standard hours drop from 48 to 40. In this case, workers who maintain their jobs are unequivocally at least as well off as before the legislation, even if the employer chooses their hours of work for them along this new constraint.<sup>7</sup>

There is substantial evidence (Trejo, 1991) that hourly wage rates drop when the overtime penalty is increased. Indeed, in the long run with a completely inelastic labor supply to the market the increase in labor costs must produce this effect. With labor supply less than completely inelastic, the decline in hourly wages will be reduced as some people leave the labor force (and thus in the long run as employment decreases). Redrawing the description to reflect this fact yields Figure 1b, which we have drawn for the special case where total earnings are fixed. The graph shows that if the regular hourly wage rate is decreased sufficiently, the legislation can make workers worse off. If we had drawn the Figure with an even larger decline in the hourly wage, or a larger cut in hours in response to the cut in standard hours, an improvement in well-being would have been still less likely.

While the model in Figure 1a shows that the legislation might improve workers' welfare, non-competitive models can also yield situations in which a cut in standard hours raises employees' well-being. Reducing exploitation by imposing a limit on hours worked can raise workers' welfare in a monopsony model. As another example of a non-competitive model, in a rat-race model in which workers put in sub-optimal excess effort to distinguish themselves from slightly inferior workers (Akerlof, 1976), imposing a

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<sup>7</sup>Going back to Ehrenberg (1971) and continuing in Hart (2004), the evidence is pretty clear that such legislation would induce an increase in employment, although it would probably reduce total work-hours by raising average labor costs (see Hamermesh, 1993, Chapter 4).

limit on hours will increase at least some workers' utility.<sup>8</sup> Less closely related models, such as Layard's (2006) view of the effects of jealousy on labor supply, or the Levine *et al* (2013) model of peer pressure, could also generate what appears to be a rat-race equilibrium in hours of work.

This discussion should demonstrate that the net effect of hours legislation on the well-being of those workers who are targeted depends on a variety of factors and is an empirical issue. The essential points to note are: 1) Nothing requires the consumer's equilibrium either before or after the cut in standard hours to be one that she would have chosen freely. Indeed, even with free choice, the worker could be made better off if the original equilibrium resulted from speed-ups arising from informational problems; 2) The effect on workers' welfare cannot be inferred by looking at the change in hours—or at the change in their feelings about their hours alone. The entire earnings-hours package (and no doubt other, non-pecuniary aspects of the job too) will be affected by the legislation, suggesting at the very least that one should examine overall job satisfaction; 3) Even that approach is too narrow: There is no reason to assume that responses to imposed changes in work time are separable from decisions about allocating non-market time (suggested by the results of Kawaguchi *et al*, 2013, Burda and Hamermesh, 2010, and Aguiar *et al*, 2013). If nothing else, a change in earnings, even with no change in market hours, will affect the allocation of non-market time due to the differential complementarity of purchased goods and various uses of time. That being the case, examining how the legislated change in standard hours affects overall life satisfaction makes sense and is the best method of evaluating the welfare impact of the legislation, since any cut in work hours engendered by the cut in the standard workweek will produce effects on all outcomes arising from changes in the agent's constrained utility maximization.

### **3. Data and Descriptive Statistics**

The ideal data set for analyzing the impacts of these targeted reductions in the standard workweek would be longitudinal; would contain very large random samples of workers and non-workers; would be

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<sup>8</sup>We recognize the difficulties of linking changes in objective outcomes to changes in subjective well-being (see, e.g., Stevenson and Wolfers, 2009). The difference here is that the objective outcome is a well-identified and singular legislative change, arguably not part of any inchoate social trend.



drawn from time periods just before it became evident that the legislation would be enacted and just after its full effects would be expected to be felt; would include all the relevant demographic information necessary to impute the propensity to be affected by the legislation; and would include measures of the respondents' feelings about various characteristics of their jobs, and the job overall, as well as information on their general life satisfaction. Unsurprisingly, no data sets in either Japan or Korea satisfy all these criteria.<sup>9</sup>

Given the absence of a perfect set of data sets, the best available data for Japan are the National Surveys of Lifestyle Preferences (*Kokumin Seikatsu Senkoudo Chosa*), a series of cross-section household surveys querying one adult per randomly selected household. The subjects include workers and non-workers regardless of the type of labor contract, so that part-time workers or those with fixed-term contracts are included. We use the 1984 and 1987 waves, which were fielded before the overtime legislation was seriously discussed, and the surveys obtained in 1996 and 1999, which were fielded about when or after it was fully effective. Because the sample sizes in each survey are fairly small, we combine the 1984 and 1987 data sets to constitute the Pre-legislation period and combine the 1996 and 1999 data sets to constitute the Post-legislation period. In the National Surveys respondents answered, "Are you satisfied with your life, or not satisfied?" along the scale 5: satisfied – 1: dissatisfied, which we view as a measure of life satisfaction.

For Korea we use data from the General Social Surveys (*Sahwe Chosa*), 2003 and 2009, the former to represent the Pre-legislation period and latter to represent the Post-legislation period. The survey includes those who do not or do work regardless of contract type, and it includes part-time workers. One advantage of the Korean GSS is its large sample size, with the number of respondents varying from about 40,000 to 80,000. The surveys, targeted at all adults in each surveyed household, contain a question regarding life satisfaction that is asked and coded identically to that in the Japanese surveys. Like the Japanese data sets,

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<sup>9</sup>Our approach to circumventing the lack of an ideal data set (and the one we used in Kawaguchi *et al.*, 2013) is similar to that proposed by Botosaru and Gutierrez (2014).

these have all the demographic information required to compute the scores  $\pi$  indicating sample respondents' propensities to have had their hours altered by employers' responses to the legislated changes.

That we can infer that the potential impacts of these legislative actions achieved their goals of shortening workweeks of those working long hours can be seen from regressions estimating the probability of working 43-48 hours (in Japan) or 41-44 hours (in Korea). Using the example of Japan:

$$(1) \quad \pi_i = \text{Prob}(48 \geq H_i > 43) = F(\gamma^* X_i),$$

where  $i$  is an individual,  $\gamma^*$  a vector of parameter estimates from the samples Pre-legislation, and  $X$  a vector of demographic variables. In the regressions we calculate  $\pi$  based on the propensity to work at least 43 and no more than 48 hours because the information on usual hours in the Japanese surveys is presented in brackets in the Japanese Time Use Survey of 1986. The  $X$  variables differ between the countries, but we use all of those that were collected in both the Pre- and Post-legislation surveys.

We calculate the propensity scores in (1) by estimating logit models describing the propensity to work in time-use surveys that were conducted in each country around the Pre- period rather than from the surveys that we use to infer impacts on life satisfaction. Thus for Japan we estimate the coefficients  $\gamma^*$  in (1) using the Japanese Time Use Survey (*Shakai Seikatsu Kihon Chosa*) for 1986, while for Korea we use the Korean Time Use Survey (*Senghwal Sigam Chosa*) for 1999. In both cases we related actual weekly hours worked (not daily diary information) to the demographics of the sample respondents.

The results of estimating these logits, shown in Table 1, demonstrate that the legislation could have had differential effects on work hours that were correlated with workers' demographic characteristics. Male, middle-aged and more educated workers were more likely to have been affected by the legislation in both countries; and we showed that it is these groups in particular for which workweeks showed the greatest declines between 1986 and 1996 in Japan, and between 1999 and 2009 in Korea.<sup>10</sup> Using the estimates of

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<sup>10</sup>See Kawaguchi *et al* (2013) and Lee *et al* (2012). Averaging over all adults in both Japan and Korea, the amount of market work decreased by 18 minutes per average day from the Pre- to the Post period. Moreover, the impact of  $\pi$  on the change in work time on the average day was negative and statistically significant. A two standard-deviation change in  $\pi$  in these data implied a 0.49 hour weekly decline in market work in Japan and a 3.66 hour weekly decline in Korea.

(1), we take samples from the National Surveys of Lifestyle Preferences and the Korean GSS before and after the legislation was effective to impute  $\pi_{ict}$ , the propensity to be affected by the legislation for person  $i$  in year  $t$  with demographic characteristics belonging to a cell,  $c$ , defined by gender  $\times$  age  $\times$  education.

Table 2 presents descriptive statistics for the Japanese and Korean samples. These statistics and all the subsequent parameter estimates are based on data weighted by the sampling weights in the particular surveys. We restrict all the analyses for Japan to people ages 15-64, those for Korea to non-students ages 16-64.<sup>11</sup> In both countries the potentially affected respondents in these age ranges are more likely on average to be older, to have higher educational attainment, and to be married. On average male respondents unsurprisingly have higher propensities to have been affected by the changes in the demand for overtime work that were induced by the legislated cuts in the standard workweek. Most important, for both men and women the ranges of these propensities are very large, as are their standard deviations. This dispersion seems sufficiently large to allow us to test whether the impacts of the legislation in each country can be inferred for those people most likely to have been affected.

In Japan for both sexes, average life satisfaction decreased over the period when the legislation was being implemented, while it did not change much in Korea. As is usual in the life satisfaction literature, more Japanese state that they are at least somewhat satisfied than otherwise. For both sexes in Japan, the dispersion in the responses is quite large. Examining the entire distribution of responses among Japanese men in both periods, 7.2 percent of the respondents state that they are satisfied (5); 39.3 percent that they are rather satisfied (4); 29.4 percent neither satisfied nor dissatisfied (3); 17.1 percent rather dissatisfied (2); and 7.0 percent dissatisfied (1).

Remarkably, in the Korean data the average person rates him/herself at slightly less than the middle of the five-point distribution. Again, for both sexes the dispersion is large, although for both sexes it is less than in the Japanese data. Over the whole period among men, for example, 4.3 percent of the respondents

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<sup>11</sup>The Japanese age measures are recorded in intervals, so that we cannot delete 15-year-olds to make the samples entirely comparable to the Korean samples.

state that they are satisfied (5); 18.2 percent that they are rather satisfied (4); 50.0 percent neither satisfied nor dissatisfied (3); 21.7 percent rather dissatisfied (2); and 5.8 percent dissatisfied (1).

There are no consistent gender differences in the average extent of life satisfaction in these data. In Japan women in this age range seem to be more satisfied than men, in Korea the opposite is true. This inconsistency mirrors findings across data sets on satisfaction in four developed Western countries (Hamermesh and Abrevaya, 2013).

#### 4. Basic Estimates Using Social Surveys

The general model to be estimated is:

$$(2) \quad S_{ict} = \beta_1\pi_{ic} + \beta_2POST_{ict} + \beta_3\pi_{ic}POST_{ict} + \varepsilon_{ict} ,$$

where  $S$  is life satisfaction,  $\pi$  is the propensity that person  $i$  in group  $c$  observed at time  $t$  (Pre- or Post-) would be affected by the legislation,  $POST$  is the time period after the legislation has become effective,  $i$  is an individual,  $\varepsilon$  is an error term, and the  $\beta$  are the parameters that we will estimate. Equation (2) is a standard double-difference approach, with the methodological twist that the treatment-control difference is not bivariate but rather varies along the unit interval of the propensity score (scaled throughout as a percentage). We estimate (2) separately for men and women, since the literature demonstrates that the determinants of measures of life satisfaction differ by gender (e.g., Frijters *et al*, 2004; Blanchflower and Oswald, 2004; Clark *et al*, 2008). While we only present least-squares estimates of (2), in every case we also estimated the appropriate ordered probits, obtaining results that were qualitatively identical to those presented in the tables. In both countries the equations *ipso facto* include all potential workers, so that any effects that we find on life satisfaction reflect responses both to the movements shown in Figures 1a and 1b and to any loss of employment. Since we include all respondents in the relevant age group, independent of their employment status, we are implicitly accounting for any changes in employment that the legislation may have engendered.

##### A. Life Satisfaction in Japan

Before discussing the estimates of (2) for Japan we examine the simple means of changes in life satisfaction by quartiles of the distributions of the imputed  $\pi$ . These and the pre-period means of life

satisfaction by quartile are shown in Figures 2a and 2b separately for men and women. While the increases (actually, the smaller decreases) in satisfaction with  $\pi$  are not always monotonic, in general we observe the largest increases (actually, the smallest decreases) among those people with the highest likelihood of having been affected by the legislation.

In Table 3 we present the parameter estimates of (2), describing the responses on the five-point life-satisfaction measure to the propensity score. The estimates of  $\beta_3$  are both positive, and the impact on female life satisfaction is highly significant statistically. Taking these estimates and the standard deviations of  $\pi$  and life satisfaction, the effect of a two standard-deviation increase in  $\pi$  on life satisfaction among men (women) is 0.07 (0.16) standard deviations. Extrapolating far above the highest cell propensity—going from a man (woman) whose hours without the legislation would surely have been below 43 or more than 48 to someone whose hours would certainly have been within that range—would increase life satisfaction by 0.63 (1.74) standard deviations<sup>12</sup>.

### *B. Life Satisfaction in Korea*

Figures 3a and 3b list the Pre- period means of average life satisfaction by quartile of the distributions of  $\pi$  and show the mean changes in average life satisfaction by quartile separately for Korean men and women. For women the increase is monotonic across the quartiles, while for men the main difference is between the upper three quartiles and the lowest quartile of the distribution.

Table 4 presents the results for Korea analogous to those in Table 3. The estimates of (2) are qualitatively identical to those for Japan. For men but not for women in Korea we observe significantly differentially higher life satisfaction in 2009 than in 2003 among people whose characteristics would in 1999 have made them more likely to have been working between 40 and 44 hours in 2009 absent the legislation. A two standard-deviation increase in the propensity to have been affected by the legislation increased male (female) life satisfaction by 0.05 (0.02) standard errors. Going far beyond the sample range,

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<sup>12</sup>The propensity score is calculated for the probability of working between 43 and 48 hours, not between 40 and 48 hours, because the interval available in the 1986 Japan Time Use Survey is between 43 and 48 hours.

moving from a person whose hours would certainly not have been in the affected range to one whose hours certainly would have been, the legislated impact on life satisfaction would have been 0.25 (0.24) standard deviations among men (women). These effects of the legislation are in line quantitatively, albeit somewhat smaller, than those we showed for Japan.

### *C. Correlated Trends or Legislated Effects on Life Satisfaction?*

To circumvent difficulties in distinguishing the effects of the legislation from those of trends in relative satisfaction that were specific to the groups likely to have been affected by the legislation, we create placebo tests for changes in relative satisfaction after the legislation (in Japan) and before the legislation (in Korea).<sup>13</sup> We examine whether the correlated relative changes that we observed in the estimates in the previous sub-sections were parts of trends that began before (Korea) or continued after (Japan) the period when the legislation was becoming effective.

For Japan the 2002 and 2005 waves of the National Surveys asked the same questions that we used in the estimates in Table 3. Combining these in order to obtain larger samples, and imputing the same propensity scores  $\pi_{ic}$  to the sample respondents in those years, we re-estimate (2) comparing responses in 1996 and 1999 to those in 2002 and 2005. Thus we are essentially asking whether any correlated trends that might have been generating the results in Sub-section A persisted in the half-decade after the legislation became fully effective in Japan.

The first two columns of Table 5 present the results of estimating (2) over this “Post-Post” period. As in Table 3 the estimates of (2) for men suggest that there also was no correlated trend in relative life satisfaction during this Post-Post period. Among women, however, the results are different here: While relative life satisfaction increased significantly among those Japanese women most likely to have been affected during the time the legislation was implemented, there was no similar relative trend after this time.

Given the timing of the legislation, we cannot perform the same comparisons for the period after the legislation was fully effective in Korea as we did for Japan. Moreover, because the Korean GSS do not

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<sup>13</sup>Regrettably there are no comparable data before the Pre-period in Japan.

contain responses on life satisfaction for the period before the legislation was implemented, we cannot make a “Pre-Pre” to Pre- comparison of changes in relative life satisfaction across demographic groups using the GSS. The World Values Survey for Korea for 1990 and 2001 did, however, contain the same demographic information as in the GSS, and it also included responses about life satisfaction, which were coded on a 10 to 1 scale. We identify 1990 as the Pre-Pre period and 2001 as the Pre- period and re-estimate (2) using this comparison. We impute propensity scores for both years in the WVS using the same equations used to impute the scores in the GSS that underlay the results in Table 4.

The results of this re-estimation are presented in the third and fourth columns of Table 5 and are comparable to the estimates in Table 4. As with the Japanese results, among women here there is no trend (before the legislation in this case) in relative life satisfaction that is correlated with the propensity to have been affected by the legislation, although the point estimate of  $\beta_3$  is negative here, while it was positive (in Table 4) after the legislation was implemented. Among Korean men, however, there is a correlated significant **negative** trend before the legislation began to be implemented: Those men who were more likely to have been affected by the law saw their relative satisfaction decline before the implementation. This result is the reverse of what we observed for Korean men in the estimates of (2) in Table 4. This placebo test strengthens the conclusion that the results there for Korean men arise from the impacts of the law.

Perhaps other factors were varying during the experimental period in such a way as to raise the life satisfaction of those workers who were most likely to have been affected by the law. One possibility is that tax rates became less progressive (and thus relatively lower for the highly-educated, middle-aged workers who were most likely to have worked long hours). While in Japan the tax structure did become slightly less progressive over the “treatment” period, there was no change in progressivity in Korea during its “treatment” period. Another possibility is that the returns to schooling and experience, both variables that raise the imputed  $\pi$ , rose during this period. In Japan, as the estimates in Kambayashi *et al* (2008) show, exactly the opposite happened; and in Korea an examination of wage ratios between various quantiles shows

no significant increase in relative wages between the 90<sup>th</sup> and the median or 10<sup>th</sup> percentiles of earnings during the treatment period.

Still another possibility is that there were differential (by the propensity scores) changes in unionization over the treatment period in each country. No direct evidence can be provided on this point, but there was no overall change in unionization in Korea during the treatment period. Unionization did decline in Japan from 29 to 24 percent of the work force between 1985 and 1995; but since the most heavily unionized workers were those who had worked longer hours, if anything this change would bias the results against finding a positive impact of the legislation on life satisfaction.<sup>14</sup>

Another possibility is that the results are generated because each country was at a different stage of the business cycle in the Pre- and Post- periods. The national unemployment rate in Japan in 1986 was 2.8 percent, in 1996 3.4 percent. It is difficult to believe this small increase could have generated any differential impacts on changes in life satisfaction, especially since, if anything, a rise in unemployment is likely to have a negative impact particularly on the industrial workers whose propensity to have been affected by the legislation was greatest. The opposite is true in Korea—the national unemployment rate fell over the treatment period from 6.6 percent to 3.6 percent. This change would bias results in favor of finding an effect of the legislation, since it implies an increase in the demand for hours of those most likely to have been affected by the law. Given the magnitude of the decrease in unemployment, and given the results below for Korea, we do not believe the bias could have been very large.

One group that includes some middle-aged and highly-educated workers is the self-employed. Their life satisfaction should not have been affected by the legislation. To examine this hypothesis we re-estimated (2) on sub-samples restricted to individuals in Korea who were self-employed. (The Japanese data sets for 1984 and 1987, the pre-legislation years, lack information on self-employment status.) In

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<sup>14</sup>A possible explanation for the different results for men and women in Japan is the enactment in 1986 of an equal opportunity law (see Abe, 2011). While this legislation was simultaneous with the change in the Japanese overtime law, it seems unlikely as an explanation for our results, because: 1) It particularly affected younger women (new labor-force entrants), not the prime-aged women with the higher  $\pi$ ; and 2) The law was strengthened in 1997, yet the placebo tests show that the relationship between  $\pi$  and the change in life satisfaction did not exist after 1996.



unpublished results we find that the estimates of  $\beta_3$  were surprisingly negative for men and women, although with t-statistics far below one. This additional placebo test suggests the validity of our main results

We cannot exclude the possibility that the significant positive effects that we found in Sub-sections A and B among women in Japan and among men in Korea were the results of temporary changes that occurred during the time the legislation was becoming effective in each country. We can, however, conclude that these impacts, and the insignificant but positive impacts among Japanese men and Korean women, were not parts of continuing trends in relative satisfaction that were occurring in each country. At the very least we have found no evidence that those workers most likely to have been affected by the legislation were worse off as a result, and the preponderance of evidence suggests that they were better off.

#### **5. Estimates Using the Korean Labor and Income Panel Study (KLIPS)**

The KLIPS is Korea's longitudinal household survey. Its start preceded the legislation we are examining, and here we use the 1999 and the 2009 waves, the former because it is the last that clearly precedes serious discussion of the legislation, the latter because the implementation of the legislation had by then been completed (and it is the same year that we used as the Post- period in the previous section). These data provide the chance to compare changes in the behavior of specific people who would or would not have qualified for coverage in the base year, clearly justifying using this information. They do not obviate the difficulties of distinguishing changes in relative satisfaction due to other causes from those due to the legislation; but they do remove any problems that result from the need to impute propensity scores to the sample respondents. The trade-off for this gain is that the requirement that the respondent worked for pay in both the Pre- and Post- periods means that the sample is selected on those people with the closest attachment to the labor force. For that reason, because it necessarily misses those who are no longer employed because of the legislation, we might expect to find larger effects of the legislation here than in Section 4.B.

The KLIPS contains responses on life satisfaction for all years, on a five-point scale: Very satisfied; satisfied; middle; not satisfied; very unsatisfied.<sup>15</sup> It also elicited responses about satisfaction with work hours and satisfaction with earnings (both on the same five-point scale). While we treat the dependent variable as continuous and present only least-squares estimates, we again also estimated every equation by ordered probit, with results that are qualitatively the same as those presented.

For all workers information on reported hours of work and overtime hours was collected for each year. We used these responses to form two variables: Indicators for any overtime and for total hours between 41 and 44 per week (to match the specifics of the legislation) in the base year, viewing each of these as indicators of treatment status under the legislation. The KLIPS also obtained all the usual demographic information, which we use to form indicators of marital status and educational attainment and a quadratic in age. We restrict the sample to individuals who worked in the base year and were age 22-49 in 1999 (32-59 in 2009), thus ensuring that nearly all those included would have completed schooling by the base year and would not have been of retirement age in the final year.

Descriptive statistics for the KLIPS samples are presented in Table 6. Life, hours and earnings satisfaction are shown for 1999 and 2009. The size of the sub-sample is far below half of the entire KLIPS sample. One reason for this is that overtime data are only collected for those who actually worked, which reduces the KLIPS sample down to around 3500. The remaining reductions are due to our restricting the age range in the sub-samples that we use and to the disqualification of the few workers who did not respond on the satisfaction questions in both years. The average individual who worked in the base year (all members of the sub-sample) saw increased life satisfaction and satisfaction with these crucial aspects of work over this period.<sup>16</sup> That ninety percent of the sub-sample are men results from its restriction to those

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<sup>15</sup>We thank John Haisken-De New and Markus Hahn for instruction in the use of Panel Whiz (Hahn and Haisken-DeNew, 2013).

<sup>16</sup>Those who worked overtime in 1999 were 0.4 percent more likely to be working in 2009 than others, an effect that was only marginally significant statistically and, more important, trivial in size (since 82 percent of those working in 1999 were working in 2009).

who worked in both the Pre- and Post- periods. The average age is slightly below the mid-point of the eligible age range, consistent with labor-force participation declining in the late fifties in Korea. Most important for our purposes, forty percent of members of this sub-sample worked overtime in the Pre-period, with over two-thirds of those working more than four hours of overtime per week. Finally, we should note that in this sample, as in the Korean GSS, hours worked decreased substantially over this period.

To examine the relationship between life satisfaction after the full impact of the legislation had been felt and one's overtime status or hours of work in the base period, we estimate models relating current (2009) satisfaction to satisfaction in the base period (1999), a measure of base-period overtime work or of weekly hours between 41 and 44 inclusive, and the demographic variables discussed above.<sup>17</sup> Notice that controlling for baseline satisfaction allows for individual heterogeneity in the determination of satisfaction.

The upper panel in Table 7 includes the parameter estimates for base-period life satisfaction and the indicators for overtime work in 1999 or 1999 hours in the 41-44 range. The results in Column (1) show that, all else equal, those who worked overtime in 1999 are significantly happier after the legislation has become fully effective, 0.13 standard deviations more than otherwise identical workers who put in no overtime in 1999.<sup>18</sup> Moreover, in unpublished results, we find that if we use life satisfaction in 2010, which on average is two years after the legislation became fully effective, as the dependent variable, the estimate of the effect of the indicator for overtime in the base year 1999 is smaller and not significant statistically. The same is true in other unpublished results using life satisfaction in 2007, during which entire year the legislation was still being phased in.<sup>19</sup> Both results suggest the value of linking the timing of the post-period exactly to the timing of the legislation.

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<sup>17</sup>Given the relatively small sample sizes, we pool men and women and include an indicator for gender. None of the results changes qualitatively if we exclude the small fraction of sample respondents who are women.

<sup>18</sup>The results change little if we estimate only (2)—i.e., delete all the controls: The coefficient estimate on lagged life satisfaction rises to 0.156, that on the overtime indicator to 0.131. The analogous re-estimates for the second model are 0.150 and 0.191.

<sup>19</sup>There is substantial persistence in overtime: The one-year correlations between indicators of the presence of overtime in this sub-sample average +0.40.

The second specification uses base-period hours in the 41-44 hour range. The estimates, shown in Column (2) of the upper panel of Table 7, are similar to those from the first specification. The impact of hours in that range on current life satisfaction holding base-period satisfaction constant is 0.20 standard deviations. If we expand the second specification to include an indicator for hours above 44, thus indicating workers not facing any substitution effect on the demand for their hours, the impact of this second indicator is essentially zero, while the estimated impact of the indicator for 41-44 hours changes little.

The absence of GSS data for 1999 forced us to use 2003 as the Pre- period in the previous section. Here we have both the 1999 data that we have used as the Pre-period and also data for 2003. We can thus examine how the results on these longitudinal data would differ if we had been forced to use 2003 as the Pre- period here by re-estimating the models in Table 7 using overtime status or base-period work hours and life satisfaction in 2003 as the base. That re-estimation yielded unpublished results in the same direction as those shown in the upper panel of Table 7, but with smaller impacts and less statistical significance. This comparison within the panel data set implies that the cross-section estimates in the previous section may be understating the impacts of the legislation on life satisfaction. The main point here, however, is that the results shown in the upper panel of Table 7, obtained by an entirely different method from those presented in Table 4, corroborate the latter completely, demonstrating again that the legislation appears to have improved the well-being of the workers at whom it was targeted.

The bottom panel of Table 7 lists the estimates of equations describing current satisfaction with hours (earnings) holding base-period satisfaction with hours (earnings) constant. The estimates in Columns (1) and (2), and those in Columns (3) and (4), of the bottom panel are based on the specifications underlying Columns (1) and (2) of the upper panel respectively. The results on satisfaction with work hours tend to confirm those of Estevão and Sá (2008), albeit on longitudinal data and on individuals, showing that workers affected by the legislation were increasingly happy with their hours. Those who had performed overtime work were more satisfied with their earnings, with an impact in standard deviation units of 0.11; workers in the 41-44 hour range in the base period showed a larger increase in satisfaction with their

earnings, 0.35 standard deviations. Given that their hours were cut and their hourly earnings probably increased, this is not surprising.

With these longitudinal results we have required that members of the sub-sample be employed both in the Pre- period (1999) and the Post- period (2009), allowing us to examine changes in hours and earnings satisfaction as well as in life satisfaction. This restriction meant that, unlike in the cross-section results in Section 4, we were unable to examine the impacts of changes in employment on life satisfaction. To do that we expand the sample to include people who worked in 1999, were between 32 and 59 in 2009, but did not work in 2009. Re-estimating the equations in this larger sub-sample ( $N = 1775$ ), the parameter estimate in the first column of Table 7 drops to 0.066 (s.e. = 0.029), while that in the second column drops to 0.106 (s.e. = 0.032). Accounting for those who were employed in 1999 but not in 2009, and thus for any declines in employment that may have been caused by the legislation, does reduce its impact on well-being; but that impact remains positive and statistically significant.

## **6. Joint Estimation of a Household Model**

Throughout the analysis we have treated individuals' satisfaction as determined solely by their demographic characteristics and the possible direct impact of the legislation on them. In a married couple, however, changes in one spouse's hours of work induced by the legislation may: 1) Alter the other spouse's hours of work (Stancanelli and van Soest, 2012; Goux *et al*, 2014) and also change his/her allocation of non-market time. These changes may in turn lead to a change in the spouse's life satisfaction; and 2) Even with no change in the spouse's time allocation, his/her satisfaction may be altered by the induced direct effect on the time allocation and/or life satisfaction of the spouse who was directly affected by the legislation. These two mechanisms suggest examining the interactions of spouses' satisfaction in response to the labor-market shocks that each faced during the period when legislation that reduced the demand for weekly hours was enacted.

While the Japanese data set included only one respondent per household, the Korean GSS includes all adults.<sup>20</sup> Taking married men ages 19-64 and women ages 16-64, we use the Korean GSS to form couples, which are the unit of observation in this Section.<sup>21</sup> For each couple in the Pre- and Post- periods (2003 and 2009) we include all the controls used in Section IV and assign the imputed propensity scores  $\pi^h$  and  $\pi^w$  as the husband's and wife's propensities to have been affected by the legislation.

Before examining the impacts of the spouses' likelihoods of being affected by the legislated incentives to employers to reduce weekly hours, we first examine the relationship between the spouses' life satisfaction in the two cross sections. The contingency table relating the husband's and wife's life satisfaction is presented in Table 8 for the 27,150 couples in the sample. In over half the couples the partners' expressions of life satisfaction are identical, and in over ninety percent the spouses' subjective well-being is no more than one point apart.<sup>22</sup> (The chi-square test of randomness in this contingency table is  $\chi^2(16) > 13,000$ ,  $p < 0.001$ .) These results mirror those in research that has focused on the joint life satisfaction of spouses (Güven *et al*, 2012) and suggest that using this sample of couples to study people's reactions to their spouse's labor-market shocks rests on a firm basis.

To be a bit formal in the discussion of family effects, let us first assume for convenience of notation that we can equate life satisfaction with utility  $U$ . Then we can write the spouses' utility as:

$$(3a) \quad U^h = \alpha_1 U^w(\pi^h, \pi^w) + \alpha_2 \pi^h ,$$

and:

$$(3b) \quad U^w = \beta_1 U^h(\pi^h, \pi^w) + \beta_2 \pi^w ,$$

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<sup>20</sup>This exercise is also possible using the KLIPS, but the small sample size makes analyzing couples' behavior in this area of little value.

<sup>21</sup>There are a number of multiple-adult households in these data. To be sure that we match husband and wife, we use only one couple per household, match by searching on the adults' ages, requiring that the husband be no more than 15 years older than his wife, that the wife be no more than 10 years older than her husband, and using the older(st) couple if there are two or more matched couples.

<sup>22</sup>We were told that each spouse answers sensitive questions like this outside the presence of the other spouse. The similar responses thus do not arise from one partner answering the question for the other.

which allows us to test various hypotheses about the jointness of their life satisfaction and the effects of each spouse's likelihood of being affected by the legislation on theirs and their spouse's satisfaction.<sup>23</sup>

Differentiating (3a) and (3b), these four effects, two own- and two cross-effects, can be written as:

$$(4a) \quad \frac{\partial U^h}{\partial \pi^h} = \alpha_1 \frac{\partial U^w}{\partial \pi^h} + \alpha_2 ;$$

$$(4b) \quad \frac{\partial U^h}{\partial \pi^w} = \alpha_1 \frac{\partial U^w}{\partial \pi^w} ;$$

$$(4c) \quad \frac{\partial U^w}{\partial \pi^h} = \beta_1 \frac{\partial U^h}{\partial \pi^h} ;$$

$$(4d) \quad \frac{\partial U^w}{\partial \pi^w} = \beta_1 \frac{\partial U^h}{\partial \pi^w} + \beta_2 .$$

Based on this joint model we estimate the system:

$$(5a) \quad S^h = \alpha_1 S^w + \alpha_2 \pi^h \cdot POST + a_1 \pi^h + a_2 POST + u_h ;$$

$$(5b) \quad S^w = \beta_1 S^h + \beta_2 \pi^w \cdot POST + b_1 \pi^w + b_2 POST + u_w ,$$

using GMM, where  $S^i$  is the married person's life satisfaction, and the parameters  $a$  and  $b$  are essential for the model but do not enter the calculation of the impact of the change in the propensity scores on the life satisfaction of the partners. Equations (5) generate estimates of the partial derivatives (4) and allow us to infer whether there are cross-effects of one spouse's satisfaction on the other's. More important for our purpose, they indicate the magnitude of the direct and indirect effects on each spouse's life satisfaction of the likelihood of having been affected by the hours legislation.

Table 9 contains the estimates of the parameters in (5). Looking first at the cross-effects on spouse's life satisfaction, the estimates of  $\alpha_1$  and  $\beta_1$ , their life satisfaction is mutually dependent (both parameters are statistically significantly nonzero). Moreover, the husband's life satisfaction has a greater impact on his wife's life satisfaction than the obverse. Considering the estimates of the structural derivatives, the cross-effect of a wife's likelihood of being affected by the legislation on the husband's satisfaction is not

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<sup>23</sup> One of the earliest efforts to specify and estimate a joint household utility function was Browning *et al* (1994).

significantly different from zero. The estimates do however, demonstrate that the differential increase in men's likelihood of having been affected by the legislation causes an increase in their wives' relative life satisfaction.

By what mechanism might the husband's propensity to have had his hours reduced by the legislation generate the indirect effect on his wife's happiness that is consistent with the results in Table 9? Using time-diary data we showed (Kawaguchi *et al*, 2013) that there was very little evidence that the wife's time allocation changed in response to a change in  $\pi^h$ . There was some indication that Korean men most likely to have been affected by the legislation increased their time spent in household production, but the effect was not large. Those being the case, the indirect effect on the wife's satisfaction may instead work through her greater satisfaction from having her husband enjoying more leisure, either because, as suggested by the results in Hamermesh (2002), the pleasure that she derives from their joint and synchronized consumption of leisure exceeds that of her husband, or because purely altruistically she values his extra leisure.

## **7. Conclusions and Implications**

Using legislated changes in the length of the standard workweek in Japan and Korea as exogenous shocks, we have examined how workers' life satisfaction changed in response to the shorter workweeks that employers imposed because the high price of an hour of overtime became effective with fewer hours worked. Our estimates on pooled cross-section data generally suggest that this legislation, whose purpose was to reduce what had been viewed as long hours in both countries (and which did stand out in international comparisons), probably made targeted workers better off. Indeed, among women in Japan and men in Korea we find that those workers who were more likely to have been affected by the legislation were significantly better off relative to otherwise similar workers. These results are confirmed using longitudinal data from Korea. Moreover, estimates of a household model suggest that Korean married men's reduced hours of market work increased the well-being of their wives too.

The difficulty with these inferences is that the same factors that may have affected the likelihood of the legislation having reduced particular workers' hours are also correlated with differential increases in



satisfaction among those same workers. Examinations of temporally adjacent data suggest, however, that there were no longer-term trends in these relative effects in either country. Nor were there any general trends in both countries—in tax progressivity or returns to schooling and experience—that can explain the findings; and we do not observe the same results for self-employed workers, who could not have been affected by the legislation. Either the legislation had positive impacts on particular workers’ well-being, or something else occurred in each country that differentially benefited particular groups of workers and did so only during the period when the laws were being implemented.

This examination implies, at least in these two countries at these particular times, that the labor market may have previously been in an equilibrium that left workers worse off than if they had worked fewer hours. Some mechanism created a situation in which workers would have preferred a wage-hours package that included fewer hours. Altering the price that employers must pay for an extra hour of labor created the incentives for them to offer that package, incentives that might not have been generated without this regulation of the unregulated labor market.

While we noted that there are several reasons why the legislation may have improved outcomes for those directly affected, one of those is that workers are in a “rat race,” perhaps stemming from desires to demonstrate their quality to employers, perhaps from other bases for interdependence. While the rat-race is hardly new, we do not believe it has been tested directly before, and certainly not in the context of protective legislation. Thus one interpretation of our results is its novel demonstration that the rat-race model may have some economy-wide validity.

We should stress that we have only presented two examples. Our results do not even imply that these regulations improved overall economic welfare in these two economies—the gains that we have demonstrated were reaped by certain workers might have been offset by losses imposed on others in the population. All we have shown is that those workers who were more likely to have been affected by the legislation—those at whom it was aimed—saw their life satisfaction increase relative to that of others and were thus presumptively made better off.

Our results say nothing about whether similar regulations in other labor markets would have the same positive impacts on workers. If hours of work had been lower in these two economies then, as they are now and as they were then in most Western economies, the same effects might well not have been observed. All we have shown is that there are cases in developed economies in which workers' well-being may be improved by interventions in the labor market that provide incentives to reduce hours of work.

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**Table 1. Logit Estimates of the Propensity to Be Affected by the Cut in Standard Hours, Based on Time Diary Surveys**

	JAPAN			KOREA	
	MEN	WOMEN		MEN	WOMEN
High school	0.335 (0.014)	-0.067 (0.017)	Education (years)	0.635 (0.102)	0.178 (0.098)
Junior college	0.363 (0.024)	0.173 (0.021)	Education (years) <sup>2</sup>	-0.005 (0.003)	0.008 (0.002)
University	0.464 (0.017)	0.163 (0.030)	Student	-0.751* (0.139)	-0.792 (0.179)
Age	0.193 (0.002)	0.133 (0.003)	Age	0.374 (0.024)	0.165 (0.034)
Age <sup>2</sup> /100	-0.222 (0.003)	-0.194 (0.003)	Age <sup>2</sup> /100	-0.356 (0.019)	-0.177 (0.028)
			Age * years of schooling	-0.006 (0.001)	-0.005 (0.001)
			Metro area	-0.310 (0.046)	-0.193 (0.064)
Mean of dependent variable	0.184	0.118	Mean of dependent variable	0.099	0.038
Mean of predicted values	0.184	0.118	Mean of predicted values	0.095	0.039
N	224,665	248,250	N	36,457	41,959

Note: Standard errors are in parentheses.

**Table 2. Descriptive Statistics, National Surveys Japan, 1984, 1987 (Pre), 1996, 1999 (Post), Ages 15-64; GSS Korea, 2003 (Pre), 2009 (Post), Ages 16-64 (Mean, Standard Deviation, Range)\***

	Japan		Korea	
	Men	Women	Men	Women
Life Satisfaction Pre	3.36 (1.03) [1, 5]	3.50 (0.98) [1, 5]	2.93 (0.88) [1, 5]	2.85 (0.88) [1, 5]
Life Satisfaction Post	3.13 (1.04) [1, 5]	3.23 (1.02) [1, 5]	2.91 (0.91) [1, 5]	2.88 (0.89) [1, 5]
$\pi$	22.05 (5.79) [5.01, 30.91]	13.88 (4.69) [4.56, 20.60]	14.96 (9.40) [0.21, 41.86]	5.63 (4.10) [0.70, 26.33]
Age	43.83 (12.19)	42.05 (12.17)	42.19 (10.52)	40.28 (11.34)
Education	12.13 (2.77) [8, 16]	11.86 (2.30) [8, 16]	12.70 (3.10) [0, 18]	11.73 (3.54) [0, 18]
Married	0.80	0.78	0.79	0.65
N =	5273	5860	34712	23758

\*The age measure in Japan here is a continuous variable imputed at means of five-year intervals. The education measure in Japan here is imputed as 8, 12, 14 or 16 for each of the four categories given.

**Table 3. Determinants of Life Satisfaction, Japan, 1984, 1987, 1996 and 1999, Ages 15-64\***

Ind. Var.:	Men (N=5273)	Women (N=5860)
$\pi$	-0.0049 (0.0095)	-0.0271 (0.0060)
POST (D96-99)	-0.3708 (0.1482)	-0.5193 (0.1036)
$\pi$ ·POST (D96-99)	0.0063 (0.0069)	0.0174 (0.0068)
Adj. R <sup>2</sup>	0.013	0.025

\*The standard errors in parentheses are clustered on the propensity-score cells.



**Table 4. Determinants of Life Satisfaction, Korea, 2003 and 2009, Ages 16-64\***

Ind. Var.:	Men (N=34712)	Women (N=23758)
$\pi$	0.0166 (0.0008)	0.0442 (0.0029)
POST (D2009)	-0.0915 (0.0212)	0.0060 (0.0224)
$\pi \cdot$ POST (D2009)	0.0025 (0.0012)	0.0024 (0.0031)
Adj. R <sup>2</sup>	0.034	0.044

\*The standard errors in parentheses and are clustered on the propensity-score cells.

**Table 5. Placebo Tests for Trends in Life Satisfaction, Japan, 1996, 1999, and 2002, 2005, Ages 15-64; Korea, 1990 and 2001, Ages 16-64\***

	Japan		Korea	
	Men	Women	Men	Women
N=	5233	5587	1687	1775
Ind. Var.:				
$\pi$	0.0014 (0.0071)	-0.0097 (0.0059)	0.0975 (0.0159)	0.0253 (0.0272)
(D2002,2005 Japan) (D2001 Korea)	-0.2627 (0.1672)	-0.1626 (0.0736)	-0.0311 (0.2733)	-0.1913 (0.2550)
( $\pi$ ·D2002, 2005 Japan) ( $\pi$ ·D2001 Korea)	0.0069 (0.0067)	0.0023 (0.0051)	-0.0689 (0.0201)	-0.0401 (0.0355)
Adj. R <sup>2</sup>	0.004	0.006	0.053	0.007

\*The standard errors in parentheses are clustered on the propensity-score cells.

**Table 6. Descriptive Statistics, KLIPS, Workers in Base Year, Ages 32-59 in 2009 N=1023 (Mean, Standard Deviation)\***

Life satisfaction 1999	3.16 (0.80)	Satisfaction with hours 1999	3.10 (0.86)
Life satisfaction 2009	3.52 (0.59)	Satisfaction with hours 2009	3.35 (0.77)
Age 2009	45.02 (7.05)	Satisfaction with earnings 1999	2.61 (0.83)
Male	0.900	Satisfaction with earnings 2009	2.97 (0.79)
Overtime hours 1999	3.502 (6.14)		
Hours 1999	50.13 (11.79)		
Hours 2009	44.55 (10.07)		

\*Standard errors are in parentheses.

**Table 7. Impact of 1999 Overtime Hours on Satisfaction with Life and Job Aspects in 2009, KLIPS  
N=1023\***

	(1)	(2)
<b>Life Satisfaction</b>		
Satisfaction 1999	0.113 (0.022)	0.112 (0.022)
Any overtime 1999	0.079 (0.035)	-----
Hours1999 41-44	-----	0.122 (0.038)
Adj. R <sup>2</sup>	0.137	0.141

	(1)	(2)	(3)	(4)
	<b>Hours Satisfaction</b>		<b>Earnings Satisfaction</b>	
Satisfaction 1999	0.116 (0.027)	0.091 (0.028)	0.084 (0.030)	0.078 (0.029)
Any overtime 1999	0.198 (0.048)	-----	0.083 (0.050)	-----
Hours 1999 41-44	-----	0.224 (0.052)	-----	0.261 (0.053)
Adj. R <sup>2</sup>	0.092	0.093	0.061	0.081

\*Standard errors are in parentheses. The regressions also include a vector of indicator for educational attainment, a quadratic in age, and indicators of gender and marital status.

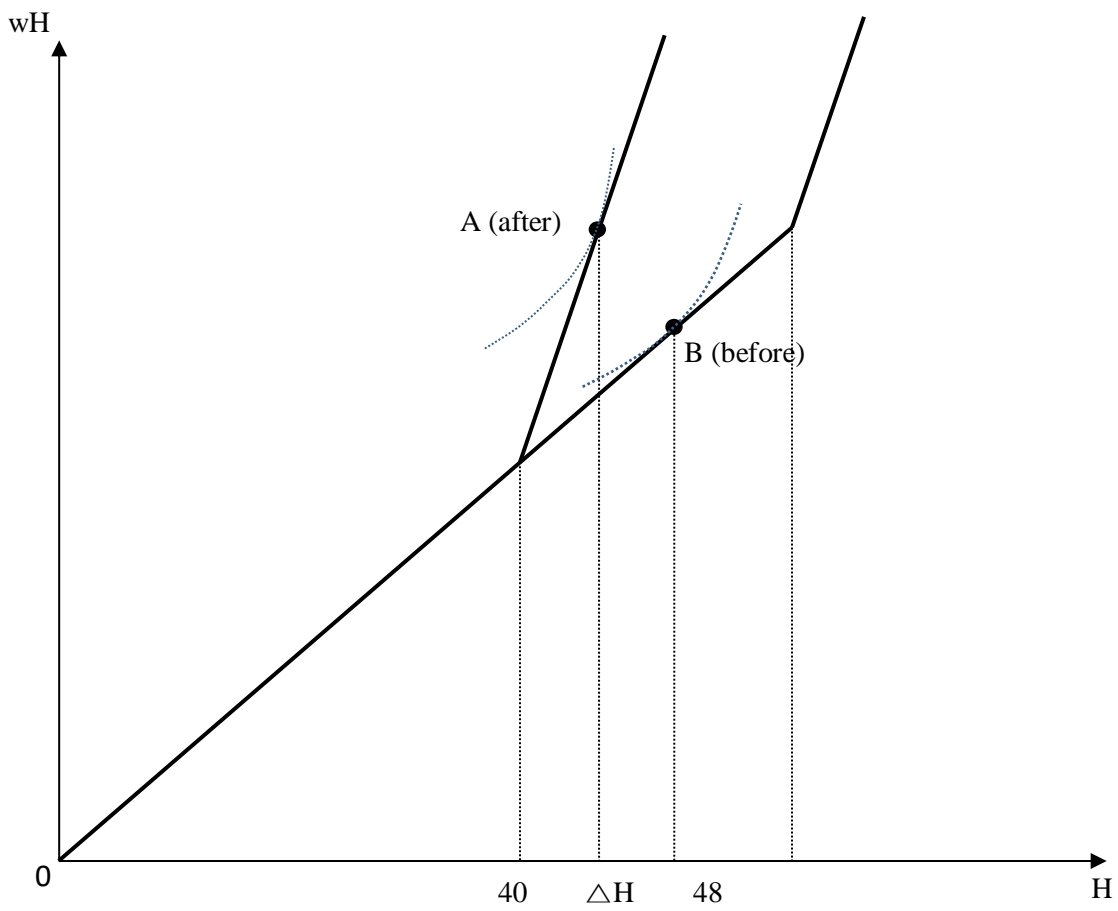
**Table 8. Distribution of Couples' Life Satisfaction, Korea, 2003 and 2009, N = 27,150 (Percentage of Couples in Each Husband-Wife Cell)**

<b>Husband</b>	<b>Wife</b>				
	Very satisfied	Satisfied	Middle	Not satisfied	Very unsatisfied
Very satisfied	2.5	1.8	1.2	0.2	0.1
Satisfied	2.0	10.3	7.5	1.4	0.2
Middle	1.3	8.8	32.7	6.0	1.0
Not satisfied	0.2	1.8	7.6	8.1	1.1
Very unsatisfied	0.0	0.3	1.3	1.4	1.2

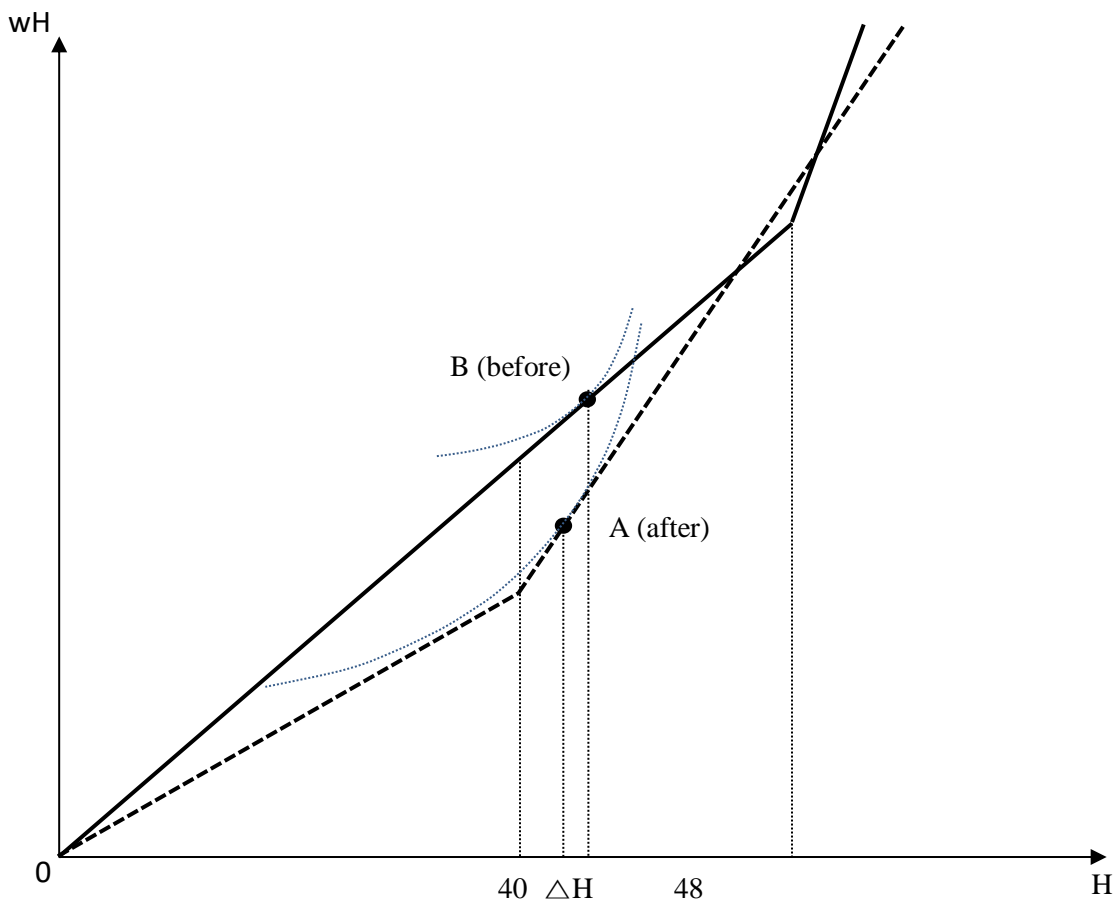
**Table 9. GMM Estimates of Models of Spouses' Life Satisfaction, Korea 2003 and 2009, N=27,150\***

<b>Parameter</b>		<b>Derivative</b>	
$\alpha_1$	0.6108 (0.0829)	$\partial S^h / \partial \pi^h$	0.0040 (0.0014)
$\alpha_2$	0.0019 (0.0008)	$\partial S^h / \partial \pi^w$	-0.0030 (0.0024)
$\beta_1$	0.8505 (0.0498)	$\partial S^w / \partial \pi^h$	0.0034 (0.0012)
$\beta_2$	-0.0024 (0.0020)	$\partial S^w / \partial \pi^w$	-0.0049 (0.0040)

\*Standard errors are in parentheses.



**Figure 1a. Worker Well-Being with a Decrease in Standard Hours and No Change in Straight-time Wage rates**

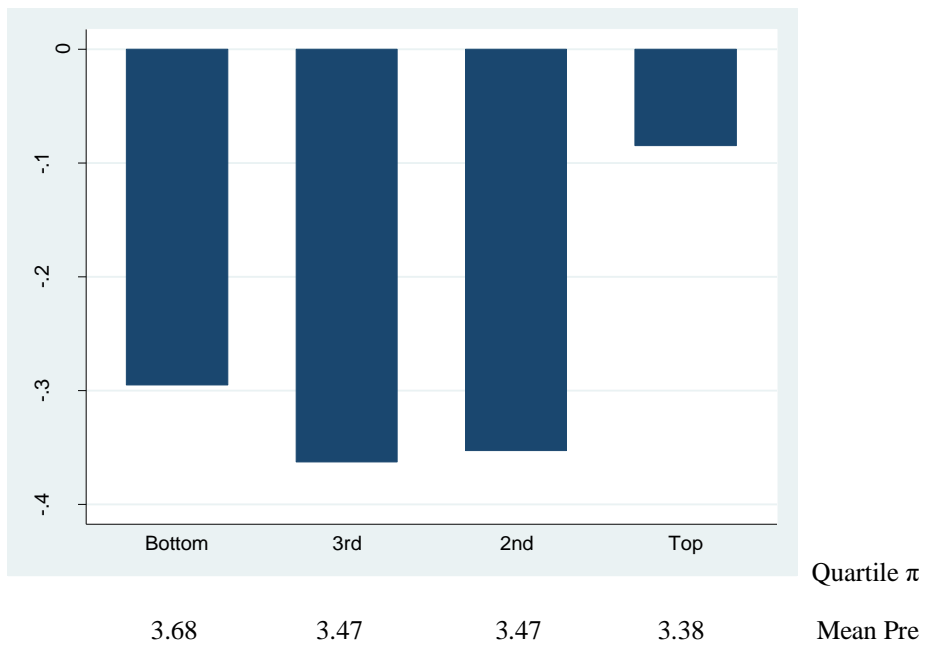


**Figure 1b. Worker Well-Being with a Decrease in Standard Hours and a Drop in Straight-time Wage Rates**

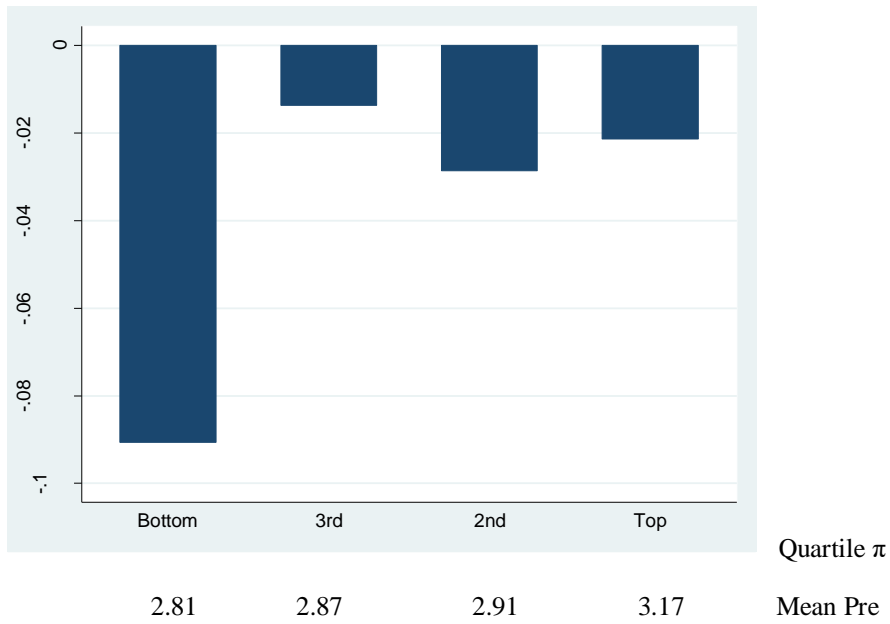




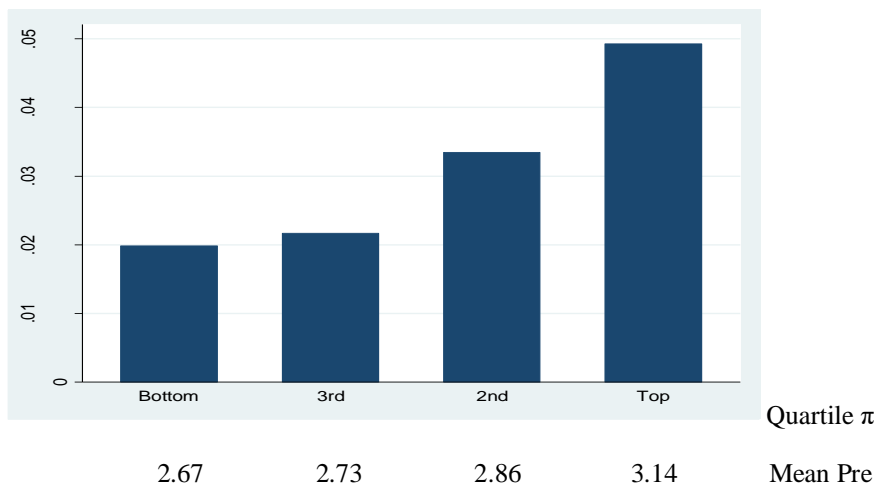
**Figure 2a. Life Satisfaction Post-Pre. Japan, By  $\pi$  Quartile, Males**



**Figure 2b. Life Satisfaction Post-Pre, Japan, By  $\pi$  Quartile, Females**



**Figure 3a. Life Satisfaction Post-Pre, Korea, By  $\pi$  Quartile, Males**



**Figure 3b. Life Satisfaction Post-Pre, Korea, By  $\pi$  Quartile, Females**