

The Health Insurance–Wage Tradeoff: Evidence from Market Reform in New York

Scott Adams*
University of Wisconsin-Milwaukee

Abstract: Pure community rating, which was enacted to improve access to health insurance in New York's small group market in 1993, prevents carriers from charging different premiums based on the ages of a firm's workers. If small firms were adjusting compensation packages prior to reform to offset higher health care costs of older workers, then community rating could lead to greater relative wages for older workers post reform and not necessarily induce adverse selection that results in changes in who is insured. I present evidence showing that relative wages of older workers increased in comparison with other states and large firms within New York following reform. Effects among men were much stronger than the effects for women, which is consistent with more variation in their costs by age.

*Address: Department of Economics, University of Wisconsin-Milwaukee, Milwaukee, WI 53201; Phone: (414) 229-4212; sjadams@uwm.edu

1. Introduction

Health care costs have risen substantially since 2000 after a short period in the mid and late 1990s when the rise in costs stalled.¹ Debate over how best to reform health care delivery has likewise picked up steam. Of particular concern is the costs faced by small businesses in providing health care to workers. This paper uses the case of health insurance market reform in New York to test whether legislated changes in the small group market affect how firms compensate workers. In the process, I provide insights into whether wages are altered to offset the costs of health insurance on worker-specific bases. If they are, some market reforms may lead to changes in wages but not necessarily induce adverse selection that results in changes in who is insured.

New York enacted premium reform in their small group market in 1993. After 1993, insurance companies were no longer able to vary premiums on the basis of individual risk factors such as age. They could only use geographic location to set premiums. Pure community rating differs remarkably from the prior policy regime of underwriting, where specific employee characteristics were used to set the premium for the group. Other research (Buchmueller and DiNardo, 2002) found no detectable relative impact on insurance coverage in New York following reform. This might suggest that firms were able to alter compensation packages to adjust to the reform, thus avoiding the feared adverse selection that a reform like pure community rating could create. This would be true if firms had been offsetting the differing costs of health insurance with wage differences prior to

¹ For a thorough review of trends in health care costs and employer premiums, see the Kaiser Family Foundation's Health Care Marketplace Project at: <http://www.kff.org/about/marketplace.cfm>.

reform. Following passage of pure community rating in New York, I observe whether small firms increased the wages of their older workers because the relative cost of insuring them fell. As a test of whether such changes were the result of the reform and not some other concurrent change in New York, I compare changes in small firms to changes over the same period in large firms. I also use small firms in other states as a comparison group. Both large firms in New York and firms in other states were not subject to pure community rating. The results indicate that the relative wages of older workers increased following reform in comparison with both control groups. Effects among men were stronger than the effects for women, which is consistent with more variation in costs by age for men.

The paper is a contribution to three important lines of research. First, it helps in the understanding of how reforming the health care system through altering the costs faced by employers could have direct influence on how firms compensate workers. Second, given other estimates have shown that insurance coverage at small firms in New York did not change after reform, it provides evidence consistent with a tradeoff of wages and fringe benefits on a worker specific basis. This is a finding that has been difficult to show in the empirical literature due to worker and firm heterogeneity. Finally, it provides information about the use of non-wage benefits in age-compensation profiles. The next section discusses some of the background, both conceptual and empirical, for each of these lines of research. In subsequent sections, I describe the data, outline the empirical approach, and present the results.

2. Background and Existing Evidence

2.1. Pure Community Rating in New York

Pure community rating was enacted in New York as part of a series of reforms targeting the small group health insurance market, most notably to combat the impact of insurer underwriting. New York was not unique with regard to many of its measures, including provisions to increase health insurance portability by limiting the extent to which workers could be denied coverage based on pre-existing conditions and mandates guaranteeing issue and renewal of health insurance with no weight placed on risk.²

New York was the most aggressive in its reforms on at least one count, however—the enacting of pure community rating in the small group and individual market. Community rating, a reform adopted in a number of states, limited the way in which insurance providers could vary the rates of their health plans. Most states that enacted community rating, however, still allowed rates to vary based upon certain characteristics. For example, after reforms were passed in Connecticut, rates could vary based on age and industry, among other things. In New York, however, reforms prevented rates to differ based on virtually every risk factor, including age (the only state to enact such a provision). The New York reform also used a broad definition of the small group market, which defined small firms as 50 or fewer employees.³

² The General Accounting Office (1996) produced a report that details the small group health insurance reforms that were passed in New York and other states during the time of the sample period used in this study.

³ Firms with one or two workers are excluded, but these workers are likely affected by the portion of the law that applies to the individual market. Thus, a husband and wife that run a

Studies of the effects of community rating have largely revolved around the issue of adverse selection and the potential upward spiraling of the number of uninsured. This stemmed from the widespread concern that once risk factors like age were removed from health plan pricing, the premiums for those with low health risks would rise to reflect group averages. Ultimately, the healthiest people in a geographic area would choose to go without coverage due to the high premiums. The remaining enrollees in health insurance plans would be those with the highest medical expenses, inducing even higher premiums and more dropouts. In the end, health insurance would be incredibly expensive and the rates of uninsured workers in the population would skyrocket. This phenomenon has been dubbed the “adverse selection death spiral.” Buchmueller and DiNardo (2002), however, found little evidence consistent with such a death spiral. They found no remarkable changes in the relative rates of uninsurance among workers at small firms in New York.⁴ These results were consistent with the recent findings by Simon (2005) with regard to the New York reform. In results not reported in this paper, I also find no evidence of a shift toward insuring older workers (relative to younger workers) following reform. Moreover, I found no significant changes in the age distribution of older workers at small New York firms.⁵ Simon (2005) did find evidence that a broader look at reforms enacted across states reveals

corner store can now buy health insurance in the individual market without facing a premium that varies by their age.

⁴ Buchmueller and DiNardo did, however, see some dramatic changes in the structure of health insurance markets in New York. The penetration of HMOs in the small group market following reform was significant and greater than the general increasing trends in HMO penetration over the same period.

⁵ These estimates employ the difference-in-difference methodology outlined in Section IV and are available upon request.

significant reductions in coverage rates for low expenditure workers, which is consistent with adverse selection.

If a reform does not change insurance coverage or employment, as appears to be the case with the New York reform, there may be changes in how firms compensate older workers. Even if there are some changes in coverage following a particular state's reform, as Simon's (2005) evidence suggests is the case for many state reforms, the effects might be somewhat mitigated by the ability of firms to alter wages to offset health insurance costs. In a medical underwriting regime, each worker affects the employer's overall costs of providing insurance to its workers. Firms cannot negotiate employee-specific premiums with the carrier, however. Otherwise, they would sacrifice the implicit subsidy offered by the government to provide coverage in the form of favorable tax treatment. Thus, firms must offer the same policy to all of their workers. Given that higher-risk workers are receiving greater compensation in terms of their health care benefits, firms could equalize the difference by varying wages depending on worker characteristics such as age. Alternatively, the firms could purely community rate within their organizations and not vary wages. It is the former behavior that seems more likely, however, and the one that is tested in this paper. After pure community rating is enacted, insurance costs revert to the group average. Older workers are now just as expensive to insure from the firm's perspective as younger workers.

Following the New York reform, there were indeed changes in the costs of insuring workers that varied by age, although the magnitudes are hard to pin down in the small group market. Using data from the Health Insurance Association of America, Hall (2000) reports

that in the small group market, those experiencing rate declines had an average age that was over 40. Those with the largest decreases (in excess of 40%) had an average age over 50 (Hall, 2000). Perhaps more informative are the rates changes in the individual market. The National Center for Policy Analysis reported that Mutual of Omaha's rates for a 55-year old male declined by 32% after the reform. While this change cannot be directly assumed to be the rate changes in the small group market faced by firms with a 55 year old male on the payroll, it is clear evidence that the health insurances costs for that person declined. Absent significant relative changes in insurance coverage and employment following these changes in costs, firms likely increased the wages of older workers in response to the reform, thus shifting compensation from health insurance to wages. The rest of the paper examines whether there is evidence consistent with this behavior.

2.2. *The Incidence of Health Insurance Coverage*

In general, wage offsets for health insurance provision are difficult to identify empirically. I can use the evidence of this paper, which uses an arguably exogenous change in health care costs in New York following premium reform, to help overcome these difficulties and add to the literature. Previous problems in identifying wage offsets begin with the fact that those with health insurance tend to earn more.⁶ There are many differences between firms that offer coverage and firms that do not, many of which are not

⁶ Liebowitz (1983) and Monheit et al. (1985) are widely-cited and found a positive correlation between health insurance and wages. Likely, countless others have found similar results that were never published. I also estimated some simple correlations from recent Current Population Surveys and found a significant positive relationship between health insurance provision and wages.

measured in labor market data. Although early research in this area attempted to control for various characteristics of the job and the quality of the job match, definitive evidence did not emerge because additional omitted factors remain. Factors related to high job satisfaction and low probability of turnover are likely positively correlated with the provision of benefits and higher wages. Moreover, as Gruber (1998) points out, the progressivity of the U.S. tax schedule, coupled with the deductibility of benefits, leads to highly productive people choosing to receive a higher proportion of their total compensation in the form of non-wage benefits.

Several studies have attempted to deal directly with the bias generated from omitted variables. Most of these fail in one way or another to capture unobserved wage or employer heterogeneities, however.⁷ A possible exception is the work by Olson (2002). He found evidence of a compensating differential in a sample of married women, using characteristics about the husband's job as instrumental variables for the health insurance coverage of wives. Although one would expect characteristics of a spouse's job to be endogenous, his model is overidentified because of the use of multiple instruments for coverage. This allows him to test for the exogeneity of his instruments (Newey 1985), which do in fact pass the overidentification test. This provides perhaps the best evidence to date of the existence of wage offsets where health insurance is provided.⁸

⁷ See Gruber (1998) for a detailed review.

⁸ There is recent related evidence from one large national firm in which employees have control over the composition of their compensation package and are observed to reduce both their wages and benefit levels in the face of rising health care costs (Goldman et al. 2005). It is not possible to identify which workers in the CPS are at firms offering such flexible compensation packages. Given that my sample includes no firms with more than

Although Olson's evidence is compelling, attempts to look at the health insurance-wage tradeoff in situations where there are exogenous changes in the cost of providing coverage are still desirable. One such study was conducted by Gruber (1994), who argued that the legislation of mandatory maternity benefits allowed for a quasi-experimental analysis of the health insurance-wage tradeoff. Coverage of the medical costs associated with childbirth in employer-provided plans prior to 1974 was rare. States began rectifying this and eventually a federal law in 1978 mandated comprehensive health insurance coverage for childbirth. Certainly, health insurance provision became more expensive at firms with a large number of women of childbearing age. Thus, an efficient response by firms would be to pass on the costs of health insurance coverage to the workers who benefited from the mandate. In particular, women of childbearing age, as well as young married men, should have experienced a reduction in their wages because they would be the most costly for firms to insure after the law. Indeed, the wages of these groups fell relative to groups not likely to be affected by the law (older men and single men). Gruber therefore found evidence consistent with the health insurance cost-wage tradeoff. This paper is related to Gruber's in that a similar exogenous change in the cost of insuring workers can be used to identify whether employers offset changes in health insurance costs with wage changes.

500 employees, however, it is a safe assumption that most workers in the sample are not offered this flexibility.

2.3. *Health Insurance Costs and the Relative Wages of Older Workers*

Beyond Gruber's evidence, there have been few attempts to assess the health insurance cost-wage tradeoff for particular groups of workers. The rise in health insurance costs throughout the late 1980s and early 1990s, and in the past several years, make such studies of current interest. One group of individuals for whom a rise in health insurance costs might be particularly important is older workers. They use health care services regularly and carriers would likely consider them expensive to insure.

The relative compensation of older workers is a topic of general interest among economists as well. Older workers tend to earn higher wages than younger workers, even after controlling for a wide variety of demographic and job characteristics. One simple explanation is that age is a proxy for workforce experience and accumulated skills, and older workers are paid more because they are more productive. There is evidence that age-earnings profiles rise even faster than age-productivity profiles, however (e.g., Medoff and Abraham (1980) and Kotlikoff and Gokhale (1992)). A leading explanation for this phenomenon is that firms deliberately withhold wages from young workers and delay compensation to later in their work life as a means of inducing optimal effort and reducing turnover. This results in efficient but implicit long-term contracts between firms and workers. The theoretical underpinnings of this explanation can be found in the models of Lazear (1979, 1981).⁹

⁹ Alternatively, steep age-earnings profiles could be due to worker preferences for delaying earnings to later in life. Lowenstein (1991), for example, finds that individuals prefer upward sloping wage profiles to flat wage profiles, even after accounting for the present value of the two income streams. One explanation for this is that workers know they are

It is likely that firms recognize that older workers receive added compensation because of their higher utilization of health care services. Little is known about whether firms adjust age-earnings profiles to account for this greater compensation. If they do adjust the relative wages of older workers, age-earnings profiles would be flatter than would be the case if employers were not the primary providers of health insurance coverage. To test whether firms do cut the relative wages of older workers, researchers could compare the age-earnings profiles of workers at firms providing coverage to those at firms not providing coverage. I conducted this simple test using Current Population Survey (CPS) data and found that age-earnings profiles are actually steeper for workers in firms providing health insurance. This is opposite of what would be expected if firms indeed lower the wages of older workers to offset health insurance costs. As discussed above, however, there are too many sources of heterogeneity that confound the interpretation of these estimated differences to make them informative. More sophisticated approaches are needed.

A unique approach to this question was employed by Sheiner (1999). She found that wage profiles in cities with higher care costs were flatter, suggesting that firms cut the wages of older workers to offset coverage costs. One must interpret her results with caution. Cities with high health care costs differ from other cities in many important ways, some of which may be related to age-earnings profiles. Sheiner essentially treated these differences in costs across cities as exogenous. That is, she assumed there are no empirically relevant reasons why health care costs are greater in Philadelphia than they are

poor savers and prefer to be paid in a manner that basically forces saving on them. Neumark (1995) provides evidence consistent with this explanation.

in Dallas. Although Sheiner includes controls for some basic city characteristics, others remain. For example, higher health care costs may signal a more vibrant economy, which may also signal higher wages for all workers, especially younger workers.

Again, what is desirable are exogenous sources of variation in health care costs across geographic areas or groups of otherwise similar workers stemming from public policies. This was precisely the approach used by Gruber (1994) when he looked at how wages changed in response to the mandating of comprehensive childbirth coverage. In this paper, I argue that enacting pure community rating is another such policy change.

3. Data

To assess the impacts of health insurance reforms in New York, I use the Annual Demographic Supplements of the CPS released from 1989 – 1999. The Annual Demographic Supplements are part of the March survey and refer to the prior calendar year (so the data actually cover 1988 – 1998). Detailed information on earnings, demographic characteristics, and the firm in which one worked is included in the survey. The data set is large enough such that an ample number of observations from the state of New York, as well as from other states, are available to conduct tests of statistical power. Finally, the CPS contains information on firm size. Given that the market reform in New York was targeted to small firms and their workers, this information is critical to identifying workers affected by the legislation. For these reasons, the CPS is uniquely suited to conduct the analyses in this paper.

One major drawback of the CPS is the inability to identify whether firms offer health insurance plans. Only whether one is covered through his firm is known. I limit the sample to full-time workers with health insurance coverage in their own name that is provided to them by their employer. This excludes some workers whose firm offers coverage that is declined. More importantly, however, it does not include individuals working for an employer that does not offer coverage. There are two other noteworthy sample restrictions. First, I restrict attention to workers aged 18 to 64 years. I also restrict attention to workers at firms with fewer than 25 employees and workers at firms with 100-499 workers. The former I denote as small firms, and the latter are larger firms. Small firms are subject to the health care reform in New York. Larger firms are not.¹⁰

4. Empirical Approach

As a starting point, I observe changes in the relative wages of older workers and younger workers in small firms in New York before and after community rating was enacted. There are a number of other changes that could have been improving the relative earnings of older workers at this time, both in New York and elsewhere, so I exploit the limited coverage of the reform, which allows for a few comparison groups to aid in identification. The first comparison group includes workers at large firms in New York.

¹⁰ As noted above, the New York reforms applied to workers in firms with 50 or fewer employees. Unfortunately, CPS classifications do not allow me to identify separately workers in firms with 25-50 workers (only workers in firms with 25-99 workers). Thus, workers at firms with 25-99 workers are excluded from all analyses. I also excluded workers from very large firms (500 or more workers), as their situation is arguably less comparable.

Large firms were not subject to reforms in premium setting during the sample period.

Another relevant comparison group is workers in small firms in other states. Using these comparison groups, I assess the impact of a move to pure community rating in New York through a series of difference-in-difference estimates exploiting both differences over time and differences between groups that are affected and groups that are not affected by the legislation.

I use a regression framework, adding a series of individual and job controls to capture differences between workers in treatment and comparison groups. I begin by estimating the following regression for a sample of New York workers with health insurance coverage from their employer.

$$(1) \quad \ln w_i = X_i\beta + \gamma_1 O_i + \gamma_2 S_i + \gamma_3 A + \gamma_4 O_i \cdot S_i + \gamma_4 O_i \cdot A_i + \gamma_5 S_i \cdot A_i + \gamma_6 O_i \cdot S_i \cdot A_i + \varepsilon_i.$$

X is a vector of covariates that includes individual controls, job controls, and time dummy variables (one for each year).¹¹ O is a dummy variable indicating older workers (defined alternatively as age 50+ or 60+). The reference group is young workers under age 30. S is a dummy variable indicating that a worker is employed by a small firm. A indicates time periods after reform. Since reform occurred in 1993, I use information in the CPS from 1989-1993 for the period before reform. Since the CPS data refer back to the prior calendar year, this is appropriate. The 1995-1999 CPS data compose the post-reform

¹¹ The individual factors include age, race, sex, education and MSA status. The job controls include occupation, industry, and whether one is enrolled in a pension plan.

period. The 1994 CPS data are excluded because part of 1993 was in the pre-reform regime and part was in the post-reform regime.

The estimate of the coefficient on the three-way interaction (γ_6) captures the impact of the reform on the affected workers. A positive estimate is suggestive of a health-insurance wage tradeoff that is age-specific. Specifically, it captures the difference between the change in the old-young wage gap in small firms in New York after reform and the change in the old-young wage gap in larger firms.¹²

Large firms are just one potential comparison group. Changes in the old-young wage gap in small firms in New York can also be compared to changes in the old-young wage gap at small firms in other states. If there is concern that workers at small and large firms are substantially different, this test offers an alternative approach. Using a sample of old and young workers at small firms in all states, the regression equation for this alternative approach is

$$(2) \quad \ln w_i = X_i\beta + \gamma_1 O_i + \gamma_2 N_i + \gamma_3 A + \gamma_4 O_i \cdot N_i + \gamma_4 O_i \cdot A_i + \gamma_5 N_i \cdot A_i + \gamma_6 O_i \cdot N_i \cdot A_i + \varepsilon_i.$$

The new variable is N , which is an indicator that the worker lives in New York. Still, the estimate of γ_6 gives the effect of reform. It compares the change in the old-young wage

¹² Because the CPS is not a true panel of individuals but rather a series of cross-sections that are pooled, workers before and after reform in New York are not the same. My results must be interpreted with this in mind, but I also note that concern should be abated by the fact that I observed no significant changes in employment or insurance coverage due to reform. Nevertheless, I do add covariates to the regressions to capture as much of the differences in these workers as possible.

gap in small firms in New York after reform with the old-young wage gap in small firms in other states that did not enact pure community rating during this period.

The results presented in this paper use all other states as a control group. During the sample period, however, many states had legislation on the books that limited the use of age and/or health conditions in the setting of premiums.¹³ All of the specifications in the paper were reestimated removing these states from the control group. There was no appreciable change in the results. If anything, some of the effects appeared stronger. Since the tests using the broader control group offer more statistical power, the paper reports only these results.

Both equation (1) and (2) utilize a dummy variable indicating an older worker. “Older” is defined as being 50 and over (or 60 and over in some specifications). I also expand the data set to the entire age distribution and replace the dummy variable specification with a linear term in age. This perhaps comes closer to identifying the impact of market reform on the age-earnings profile. If firms flatten the profile in the face of high health insurance costs for older workers, they should make profiles steeper following reform. Equation (1), for example, can be augmented to capture this effect

$$(3) \quad \ln w_i = X_i\beta + \gamma_1 \text{Age}_i + \gamma_2 S_i + \gamma_3 A + \gamma_4 \text{Age}_i \cdot S_i \\ + \gamma_4 \text{Age}_i \cdot A_i + \gamma_5 S_i \cdot A_i + \gamma_6 \text{Age}_i \cdot S_i \cdot A_i + \varepsilon_i.$$

¹³ Maine and Vermont limited but did not prohibit the use of age information in premium setting. Connecticut, Florida, Kentucky, Maine, Maryland, Massachusetts, New Hampshire, New York, Oregon, Vermont, and Washington prohibited the use of health status. California, Colorado, North Carolina, South Carolina, and Virginia limited the use of health status in premium setting (Curtis et al., 1999).

The estimate of γ_6 captures the change in the age-wage profile following reform. A positive estimate suggests that firms do alter compensation profiles in an age-specific fashion.

Reforms in New York allow for one more test in which the sample is limited to just older workers. In this case, the effect of the legislation is identified through the estimate of γ_6 in the regression

$$(4) \quad \ln w_i = X_i\beta + \gamma_1 N_i + \gamma_2 S_i + \gamma_3 A + \gamma_4 N_i \cdot S_i + \gamma_4 N_i \cdot A_i + \gamma_5 S_i \cdot A_i + \gamma_6 N_i \cdot S_i \cdot A_i + \varepsilon_i.$$

Here, I estimate the difference between the change in the small firm-large firm wage gap for older workers in New York and the change in the small firm-large firm wage gap for older workers in other states. This analysis cannot speak to effects on age-earnings profiles but can provide more information on the effect of the reform on the earnings of older workers.

The advantage of using the multiple tests described in this section is that similar estimates across them will provide more reliable information about the effects of the policy. It will also generate more confidence in the conclusions of the paper, which speak to whether firms indeed offset higher health insurance costs with lower wages in an age-specific fashion.

5. Results

5.1. *Wages of older workers relative to young workers (within New York comparisons)*

To see the logic of the difference-in-difference approach used throughout this paper, I present some simple calculations in Table 1. I first report the real log wage differential in the wages of old and young workers at small firms in New York before and after reform in the first row. The differential grew from 0.28 before reform to 0.39 after reform. This difference of 0.11 falls just short of statistical significance at conventional levels but clearly indicates a substantial change in the old-young wage gap. This is obviously greater than the change in the log wage differential that is reported in the second row for a comparison group of large firms in New York, which is actually negative (-0.03). The difference in the difference is 0.14 (reported in the last row), which is not statistically significant but again is substantial. This simple difference-in-difference calculation is roughly equivalent to the coefficient γ_6 in equation (1) without the control vector X .¹⁴

Of course, the results in Table 1 do not account for other differences between workers at small and large firms, as well as differences in worker characteristics over time. For this reason, the regression analysis summarized in equation (1) is employed, adding individual and job controls. Table 2 presents the regression results. In the top panel, data from all CPS years (except 1994) are included in the analysis. Column (1) estimates come from specifications that do not include demographic or job controls and are similar to the calculations from Table 1. The one difference is that I now use nominal log wages with

¹⁴ It is also the case that the dependent variable in equations (1) – (4) is the nominal wage, as year dummy variables are included in all specifications.

year dummy variables as controls. In columns (2) and (3), however, individual and job controls are added. Each cell represents a separate regression. The first row simply estimates how the wage differential for older workers at small firms in New York changes after reform. Since the sample for these estimations only includes small firms, S in equation (1) is effectively set to one, with the policy effect then inferred from the interaction of O and A . The estimate in column (2), which includes individual controls, is a sizable 0.111 and is statistically significant at the .10 level. The estimate increases to 0.128 when job controls are added and is now statistically significant at the .05 level. The third row reports the true difference-in-difference estimate (γ_6 from equation (1)). The impact of the reform is large and positive but falls just short of statistical significance when individual and job controls are included.

When I rerun the tests defining older workers as those 60 and over in columns (4) – (6) in the top panel, the estimates become stronger and far more suggestive that reforms increased the relative wages of older workers. In particular, the difference-in-difference estimate in the third row implies a 0.385 greater increase in the log wage differential for older workers at small firms compared with those at large firms following reform. This difference is significant. Given the cost insuring workers over 60 should fall by relatively more following reform, this result is consistent with firms offsetting health insurance costs with wages.

The estimates in the top panel may mask greater impacts. As noted earlier, the results thus far use information from all years except 1993 (the 1994 CPS), which is the year of reform. That is, 1988-1992 composed the time period before reform and 1994-1998

composed the post-reform period. Although studying the impacts immediately following the policy intervention uses all available information, it may be misleading if firms are slow to adjust wages. Moreover, the reverse is possible. Either insurance carriers or firms may anticipate the pending legislation and adjust their behavior in the period before the reform is passed. For this reason, I reestimate the regressions that generated the results in the top panel of Table 2 excluding the two years before community rating was enacted, the year of its enactment, and the two years following its enactment. This leaves the three year period from 1988-1990 as the “before period” and 1996-1998 as the “after period.” All of the estimates become stronger. The specifications interacting the dummy for old with the dummy for the period after reform in a sample consisting entirely of workers at small firms in New York suggests statistically significant effects in all specifications, even those where old is defined as age 50 or greater. The difference-in-difference results reported in the final row indicate positive effects of reform regardless of whether old is defined as age 50 and older or 60 and older, with the effects statistically significant at the .10 level for the former. For the latter, the effects are statistically significant at the .01 level.

Therefore, the results in Table 2 suggest that the reform did increase the relative wages of older workers at small firms. This indicates that firms likely alter the wages of their workers to offset the cost of providing them with health insurance coverage. To further verify whether there is a relationship between health insurance costs and the slope of the age-earnings profile, I next estimate equation (3), which tests directly the impact of the reform in New York on the steepness of the age-earnings profiles. The results are reported in Table 3, with presentation paralleling that in Table 2. In column (1), when only

individual controls are used, there is a clear positive effect of reform on the age-earnings profile that is statistically significant (at least at the .10 level) in the difference-in-difference specifications. When job controls are added, however, the effects remain substantial and positive but fall short of statistical significance in the key difference-in-difference specifications. Still, the positive effects suggest that there was some steepening of the age-earnings profile following reform and are consistent with the findings from Table 2.

5.2. *Wages of older workers relative to young workers (cross - state comparisons)*

Although the results in the previous subsection generally find an increase in wages for older workers, the effect is identified in specifications relying on large firms as a comparison group. One concern is that there are differences in workers at small and large firms that confound the comparison. I verify the results by using small firms in other states as an alternative comparison group. I estimate specifications that are now based on equation (2) and report the results in Table 4.

The results are similar to the findings in the earlier tables. Specifically, the difference-in-difference results are less pronounced in the top panel when older workers are measured as being 50 and older. When I exclude the two years before and after reform, however, the estimates in columns (2) and (3) are large and significant. In columns (5) and (6), where older workers are defined as those aged 60 years or more, all of the estimates are significant at the .10 level at a minimum, suggesting that pure community rating increased the relative wages of older workers at small firms compared with changes over the same period in other states.

Table 5 reports results that parallel those that appear in Table 3 and are similar. The impact of reform on the age-earnings profile is positive in both columns (1) and (2). The difference-in-difference estimates, however, again fall short of statistical significance.

5.3. *Additional Cross - State Comparisons*

The estimates presented in the prior subsections are the most informative that are possible given the nature of the data and the provisions of the health insurance reform in New York. An additional comparison can be made if one limits the sample to older workers and estimates how the difference in wages between workers at small and large firms changed after reform in New York compared with other states. This employs the regression analysis that is described in equation (4).

The results are reported in Table 6. Like in the previous tables, I first limit attention to workers aged 50 and older in columns (1) – (3). In the previous subsections, the estimates were weaker when I used this broader definition of older workers. It is the same case here. The top row focuses just on New York, looking at how the wages at small firms changed in New York compared with larger firms. This amounts to setting the dummy variable for New York residents (N) in equation (4) to one. Thus, the effect of reform is identified by the coefficient on the interaction of S and A. In columns (1) – (3), the estimates are positive but are not statistically significant. In the second row, when the sample is limited to the after reform period and the effect is identified from the interaction between N and S in equation (4), there is no detectable effect of reform on wages. The same is true for the difference-in-difference estimates in the third row. In the previous

subsections, the effects were stronger when I excluded the two years before and after reform. This is not the case for the results in Table 6, as the results in the bottom panel for columns (1) and (2) are largely negative.

The results for workers over the age of 60 appear in columns (4) – (6). The top panel reports the results for all years except 1993 (the 1994 CPS), the year the legislation was passed. Each estimate suggests that reform positively and significantly (with only one exception that falls just short of significance at the .10 level) affected the wages of older workers. As before, the effects are stronger in the bottom panel.

5.4. *Comparisons by sex*

As another check on these results, I bifurcate the sample by sex. One might suspect health care costs would vary more significantly by age for men than women. The assumption that older workers are more expensive to insure made throughout the paper perhaps ignores the fact that many younger women are quite expensive to insure as well. In fact, Sheiner (1999) shows that costs indeed vary more by age for men than women. This suggests that the wage effects observed throughout the paper should be more significant for men.

Table 7 presents many of the key estimates in the paper. I limit the results to those obtained when the before reform period is defined as 1988-1990 and the after reform period is 1996-1998 (the bottom panel in the previous tables). Results are similar but not as pronounced if the full sample is used, much like the rest of the paper. In the top panel, with older workers defined as 50 and older, the effects on wages are stronger in the male sample

for two out of three of the estimates. These estimates follow the difference-in-difference estimates reported in Tables 2, 4, and 6, respectively. When older workers are defined as age 60 or older, the differences between men and women are significant in all three estimates. This is denoted by an asterisk, which indicates statistical significance of the difference at the .05 level. The results provide more confidence that the results presented earlier indeed reflect real effects of changes in health care costs, as older males are significantly more impacted by community rating than older females.

6. Conclusion

The results of this paper suggest reforms in the small group market that alter the costs of providing coverage to particular groups of workers are likely to be offset by changes in wages for workers, especially in cases where the effects on health insurance coverage are small. This paper examined the case of one such reform—pure community rating in New York.

In addition to understanding the effects of pure community rating, this paper provides some insight into how firms compensate workers. Empirically estimating if and how wages are altered to offset the costs of providing health insurance benefits is difficult given the inability to control completely for differences across workers and firms. The arguably more interesting question of whether these tradeoffs occur at the firm level or whether the firms alter the compensation of specific groups of workers has barely been addressed. Attempts to estimate the relationship between workers with higher health care costs and their relative wages must struggle to control for the underlying heterogeneity

across workers that are correlated with their higher health care costs, in addition to heterogeneity across firms. Promising research in this area will need to continue to exploit exogenous variation in the health care costs of different groups of workers. This paper applied the case of community rating in New York to this question. Most comparisons revealed that the relative wages for older workers at small firms increased following reform, thus suggesting that there is evidence of a health insurance-wage tradeoff that is made by firms on the basis of age. For the oldest workers in the sample, those between the ages of 60 and 65, the effects were statistically significant in almost all of the specifications

For decades, economists have been analyzing the steepness of age-earnings profiles and attempting to explain why they differ from age-productivity profiles. The evidence presented in this paper suggests that age-compensation profiles are actually steeper than age-earnings profiles. The standard age-earnings profiles do not account for the backloading of compensation that comes in the form of health insurance benefits.

References

- Buchmueller, T., and J. DiNardo. 2002. "Did Community Rating Induce an Adverse Selection Death Spiral? Evidence from New York, Pennsylvania, and Connecticut." *American Economic Review* 92(1): 280-94.
- Curtis, R., S. Lewis, K. Haugh, and R. Forland. 1999. "Health Insurance Reform in the Small-Group Market." *Health Affairs* 18(3): 151-160.
- Goldman, D., N. Sood, and A. Liebowitz. 2005. "Wage and Benefit Changes in Response to Rising Insurance Costs." NBER working paper No. 11063.
- Gruber, J. 1998. "Health Insurance and the Labor Market," NBER Working Paper No. 762.
- _____. 1994. "The Incidence of Mandated Maternity Benefits." *American Economic Review* 84(3): 622-41
- Hall, Mark A. 2000. "An Evaluation of New York's Reform Law." *Journal of Health Politics, Policy, and Law* 25(1): 71-99.
- Kotlikoff, L., and J. Gokhale. 1992. "Estimating a Firm's Age-Productivity Profile Using the Present Value of Workers' Earnings." *Quarterly Journal of Economics* 107(4): 1215-42.
- Lazear, E. 1979. "Why Is There Mandatory Retirement?" *Journal of Political Economy* 7(6): 1261-84.
- _____. 1981. "Agency, Earnings Profiles, Productivity, and Hours Restrictions" *American Economic Review* 71(4): 606-20.
- Liebowitz, A. 1983. "Fringe Benefits in Employee Compensation," in J. E. Triplett (ed.), *The Measurement of Labor Cost*. (Chicago: The University of Chicago Press).
- Loewenstein, G., and N. Sicherman. 1991. "Do Workers Prefer Increasing Wage Profiles?" *Journal of Labor Economics* 9(1): 67-84.
- Medoff, J. and K. Abraham. 1980. "Experience, Performance, and Earnings." *Quarterly Journal of Economics* 95(4): 703-36.
- Monheit, A., M. Hagan, M. Berk, and P. Farley. 1985. "The Employed Uninsured and the Role of Public Policy." *Inquiry* 22(4): 348-64.
- Olson, C. 2002. "Do Workers Accept Lower Wages in Exchange for Health Benefits?" *Journal of Labor Economics* 20(2): S91-114.

- Neumark, D. 1995. "Are Rising Earnings Profiles a Forced-Saving Mechanism?" *Economic Journal* 105(428): 95-106.
- Newey, W. 1985. "Generalized Methods of Moments Specification Testing." *Journal of Econometrics* 29(3): 229-56.
- Sheiner, L. 1999. "Health Care Costs, Wages, and Aging." Federal Reserve Board of Governors, working paper.
- Simon, K. 2005. "Adverse Selection in Health Insurance Markets? Evidence from Small Group Health Insurance Reforms." *Journal of Public Economics* 89(10): 1865-1877.
- U.S. General Accounting Office. 1996. *State Mandated Benefits*. (GAO/HEHS-96-125R), US GAO.

Table 1: Relative wages of older workers in New York before and after reform

	Before Reform (1)	After Reform (2)	Column Difference (2) – (1)
<i>Small Firms:</i>			
Older workers – younger workers	.28 (.05)	.39 (.06)	.11 (.07)
<i>Large Firms:</i>			
Older workers – younger workers	.31 (.03)	.28 (.05)	-.03 (.06)
<i>Row Difference</i>	-.03 (.06)	.11 (.08)	.14 (.09)

Standard errors are in parentheses. Older workers are those age 50 and older. Younger workers are age 30 and younger.

Table 2: Regression-adjusted estimates of the effects of pure community rating, workers in New York

	Older workers defined as over 50				Older workers defined as over 60			
	(1)	(2)	(3)	N	(4)	(5)	(6)	N
<i>All years:</i>								
Older worker x after reform (small firms only)	0.096 (0.052)	0.111 (0.057)	0.128 (0.047)	1,849	0.214 (0.075)	0.255 (0.080)	0.246 (0.075)	1,132
Older worker x small firm (after reform only)	0.099 (0.075)	0.005 (0.069)	-0.016 (0.066)	1,601	0.344 (0.131)	0.239 (0.122)	0.212 (0.117)	920
Older worker x small firm x after reform	0.132 (0.063)	0.146 (0.087)	0.116 (0.083)	3,788	0.465 (0.154)	0.439 (0.150)	0.385 (0.144)	2,301
<i>1988-1990 and 1996-1998 as before and after period, respectively:</i>								
Older worker x after reform (small firms only)	0.169 (0.046)	0.216 (0.045)	0.227 (0.040)	1,114	0.340 (0.072)	0.413 (0.075)	0.372 (0.090)	689
Older worker x small firm (after reform only)	0.142 (0.100)	0.036 (0.093)	0.010 (0.093)	913	0.509 (0.173)	0.347 (0.159)	0.311 (0.162)	515
Older worker x small firm x after reform	0.221 (0.075)	0.253 (0.113)	0.194 (0.109)	2,233	0.732 (0.153)	0.696 (0.195)	0.580 (0.192)	1,385
Individual controls	No	No	Yes		No	No	Yes	
Job controls	No	Yes	Yes		No	Yes	Yes	

Note: Coefficient estimates and standard errors are given for the interaction term that identifies the effect of community rating in each specification. Each entry represents results from a separate regression.

Table 3: Regression-adjusted estimates of the effects of pure community rating on the age-earnings profile, workers in New York

	(1)	(2)	N
<i>All years:</i>			
Age x after reform (small firms only)	0.0025 (0.0022)	0.0028 (0.0019)	3,866
Age x small firm (after reform only)	0.0007 (0.0021)	-0.0005 (0.0020)	3,573
Age x small firm x after reform	0.0046 (0.0027)	0.0030 (0.0026)	8,055
<i>1988-1991 and 1996-1998 as before and after period, respectively:</i>			
Age x after reform (small firms only)	0.0053 (0.0025)	0.0050 (0.0022)	2,317
Age x small firm (after reform only)	0.0024 (0.0028)	0.0002 (0.0028)	2,034
Age x small firm x after reform	0.0083 (0.0035)	0.0054 (0.0034)	4,677
Individual controls	Yes	Yes	
Job controls	No	Yes	

Note: Coefficient estimates and standard errors are given for the interaction term that identifies the effect of community rating in each specification. Each entry represents results from a separate regression.

Table 4: Regression-adjusted estimates of the effects of pure community rating, workers at small firms

	Older workers defined as over 50				Older workers defined as over 60			
	(1)	(2)	(3)	N	(4)	(5)	(6)	
<i>All years:</i>								
Older worker x New York (After reform only)	0.023 (0.059)	0.036 (0.044)	0.018 (0.036)	12,527	0.119 (0.113)	0.153 (0.065)	0.122 (0.049)	7,401
Older worker x New York x after reform	0.009 (0.076)	0.040 (0.059)	0.028 (0.049)	26,920	0.103 (0.138)	0.154 (0.087)	0.129 (0.076)	17,172
<i>1988-1990 and 1996-1998 as before and after period, respectively:</i>								
Older worker x New York (After reform only)	0.077 (0.077)	0.103 (0.020)	0.068 (0.028)	7,463	0.159 (0.149)	0.181 (0.084)	0.121 (0.066)	4,355
Older worker x New York x after reform	0.058 (0.097)	0.121 (0.045)	0.093 (0.035)	16,337	0.194 (0.179)	0.259 (0.084)	0.183 (0.077)	10,506
Individual controls	No	No	Yes		No	No	Yes	
Job controls	No	Yes	Yes		No	Yes	Yes	

Note: Coefficient estimates and standard errors are given for the interaction term that identifies the effect of community rating in each specification. Each entry represents results from a separate regression. Standard errors are robust to non-independence within state-year cells (New York vs. elsewhere).

Table 5: Regression-adjusted estimates of the effects of pure community rating on the age-earnings profile, workers at small firms

	(1)	(2)	N
<i>All years:</i>			
Age x New York (After reform only)	0.0013 (0.0018)	0.0001 (0.0015)	28,174
Age x New York x after reform	0.0009 (0.0022)	0.0004 (0.0019)	58,396
<i>1988-1990 and 1996-1998 as before and after period, respectively:</i>			
Age x New York (After reform only)	0.0039 (0.0014)	0.0027 (0.0016)	16,686
Age x New York x after reform	0.0036 (0.0024)	0.0024 (0.0021)	34,939
Individual controls	Yes	Yes	
Job controls	No	Yes	

Note: Coefficient estimates and standard errors are given for the interaction term that identifies the effect of community rating in each specification. Each entry represents results from a separate regression. Standard errors are robust to non-independence within state-year cells (New York vs. elsewhere).

Table 6: Regression-adjusted estimates of the effects of pure community rating, older workers

	Older workers defined as over 50				Older workers defined as over 60			
	(1)	(2)	(3)	N	(4)	(5)	(6)	N
<i>All years:</i>								
Small firm x after reform (New York only)	0.043 (0.071)	0.058 (0.045)	0.043 (0.052)	1,920	0.374 (0.151)	0.394 (0.135)	0.298 (0.130)	433
Small firm x New York (After reform only)	0.037 (0.057)	0.029 (0.043)	-0.006 (0.047)	12,633	0.212 (0.127)	0.235 (0.094)	0.136 (0.085)	2,484
Small firm x New York x after reform	0.001 (0.074)	0.008 (0.049)	-0.023 (0.050)	25,508	0.252 (0.159)	0.283 (0.122)	0.181 (0.111)	5,447
<i>1988-1990 and 1996-1998 as before and after period, respectively:</i>								
Small firm x after reform (New York only)	0.017 (0.090)	0.071 (0.060)	0.046 (0.075)	1,123	0.519 (0.196)	0.564 (0.163)	0.344 (0.214)	275
Small firm x New York (After reform only)	-0.009 (0.073)	-0.001 (0.043)	-0.014 (0.054)	7,697	0.275 (0.165)	0.267 (0.120)	0.176 (0.104)	1505
Small firm x New York x after reform	-0.057 (0.094)	-0.026 (0.053)	-0.035 (0.055)	15,396	0.391 (0.205)	0.392 (0.145)	0.237 (0.134)	3,375
Individual controls	No	No	Yes		No	No	Yes	
Job controls	No	Yes	Yes		No	Yes	Yes	

Note: Coefficient estimates and standard errors are given for the interaction term that identifies the effect of community rating in each specification. Each entry represents results from a separate regression. Standard errors are robust to non-independence within state-year cells (New York vs. elsewhere).

Table 7: The effects of community rating, by sex

	Males	Females
<i>Older workers defined as over 50:</i>		
Older worker x small firm x after reform (sample restricted to NY)	0.224 (0.120)	0.186 (0.091)
Older worker x New York x after reform (sample restricted to small firms)	0.076 (0.049)	0.139 (0.084)
Small firm x New York x after reform (sample restricted to older workers)	0.106* (0.073)	-0.186 (0.091)
<i>Older workers defined as over 60:</i>		
Older worker x small firm x after reform (sample restricted to NY)	0.764* (0.135)	0.224 (0.189)
Older worker x New York x after reform (sample restricted to small firms)	0.361* (0.115)	-0.1151 (0.178)
Small firm x New York x after reform (sample restricted to older workers)	0.473* (0.148)	-0.254 (0.205)

Note: The sample excludes the middle years of the data—that is, 1991 through 1995. Coefficient estimates and standard errors are given for the interaction term that identifies the effect of community rating in each specification. Each entry represents results from a separate regression. Standard errors are robust to non-independence within state-year cells (New York vs. elsewhere). An asterisk (*) indicates that the coefficient for the effect in the male sample is statistically different than the coefficient in the female sample at the .05 level.