The Individual-Specific Incidence of Employer-provided Health Insurance: Evidence from the Affordable Care Act

Conor Lennon*

October 28, 2015

Abstract

The Affordable Care Act's employer mandate affects firms who previously did not provide health coverage much more than firms who did. The difference creates a natural experiment which can be used to examine how labor market outcomes are determined by variation in health care costs at the individual level. Estimates, using the Medical Expenditure Panel Survey (MEPS), suggest that workers with higher health care expenses are less likely to secure employment at firms most affected by the Act and earn lower wages when they do. The reduction in wages amounts to between \$0.30 and \$0.40 in annual earnings for every dollar difference in annual medical expenses.

Keywords: Affordable Care Act, Labor Market Outcomes, Employer Mandate, Mandated Benefits, Benefit Incidence, Health Care Expenses, Employer-provided Health Insurance, Risk pooling, Compensating Differentials

JEL classification: *J31, J32, J38, I12, I13, I18, H51*

1 Introduction

During World War II, employers offered improved health coverage to circumvent wage freezes imposed by the National War Labor Board. Given its origins, it is no surprise that scholars have found that workers still pay for their health coverage in the form of lower wages. For example, Gruber (1993) finds mandates on maternity coverage result in lower wages for those who benefit. Sheiner (1999) and Jensen and Morrisey (2001) exploit regional variation in health care costs to show that elderly workers face lower wages if they live in areas where care is relatively more expensive. More recently, Lahey (2012) found infertility coverage mandates lead to lower wages for females aged $28-42$ while Bailey (2014) found that prostate cancer screening mandates were associated with reductions in wages for older males.

What remains unclear in the literature is at which level this cost-shifting occurs: is it only at the group level or can firms respond to variation in employee health care usage at the individual level? This paper tackles that question using the Affordable Care Act's employer mandate as a natural

*Ph.D. Candidate, Department of Economics, University of Pittsburgh, email: *cjl55@pitt.edu*

experiment. The paper examines how labor market outcomes are affected for high and low health care cost workers at firms who are required to provide coverage because of the Act. Empirical estimates, using data from the Medical Expenditure Panel Survey (MEPS), highlight that workers with higher health care expenses are less likely to secure employment at firms heavily affected by the ACA and earn lower wages when they do. The empirical estimates employ a difference-in-difference estimation framework and take the form;

$$\begin{aligned} LaborMarketOutcome_{it} = \beta_0 + \beta_1 HealthExpenses_{it} + \beta_2 PostACA_{it} \\ + \beta_3 HealthExpense \times PostACA_{it} + \Pi X_{it} + \epsilon_{it} \end{aligned}$$

where $LaborMarketOutcome_{it}$ stands for labor market outcomes of interest for person i at time t. The dependent variable can be any labor market outcome which responds to changes in labor demand. The right hand side of the estimating equation considers the main effect of a continuous measure of health expenses $(HealthExpenses_{it})$, the main effect of the Affordable Care Act $(PostACA_{it})$ (a binary variable taking on the value of 1 after the Act is announced) and the interaction term between the two giving a measure of the effect of the Act on the labor market outcomes of individuals as a function of their health expenditure per year. Some specifications add a third difference between firms who do and do not provide coverage. The MEPS data suggest that pass-through of health care expenses is around 30 to 40 cents of every dollar depending on specification. Given medical expenses are a tax deduction for firms the size of the pass-through is considerable. Consistent with the Act's mandate applying only to full-time employees the data also show that firms appear to avoid the Act's requirements by reducing hours worked for employees with higher health care expenses.[1] As the regression equations flexibly control for demographic characteristics before and after the ACA is announced the estimates suggest firms can and do condition wages on health care expenses at the *individual* rather than just at the *group* level.

The level at which cost-shifting occurs matters because the institution of employer-based insurance creates a cost wedge between workers who are equally productive but who incur different annual health care expenses. The cost wedge between workers is unavoidable as the group insurance market treats each firm as a single risk pool. As a result, firms pay the *actual* cost of their employees' health care usage. Alternatively firms can choose to self-insure, taking the financial risk of large expenses on themselves. In either case, they can only reduce the cost of providing a given level of coverage by cherry-picking employees who will use health coverage less intensively.

Individual-specific cost-shifting would undermine the supposed risk-pooling benefits of employer-based coverage. Groups of employees are seen as ideal risk pools because insurers would "screen" out risky applicants if coverage were purchased individually. Adverse selection would then cause the market to fail. However, this paper shows that firms are incentivized to act as the insurer would and lower the wages of higher-cost employees or exclude them from employment altogether.[2]

[1] See Appendix A.

[2] Employer-provided coverage may have worked well when health care costs were lower. If health care prices are

At first glance, the tailoring of wage and benefit packages at the individual level seems efficient. If worker A has health care costs c_A and worker B has costs $c_B > c_A$ but both workers are equally productive then the difference in their wages should be $c_B - c_A$, all else equal. Also, worker B could have few qualms with such an outcome if they value coverage at its cost. The potential for inefficiency when bundling health insurance with employment only becomes apparent when we consider that for larger values of c_B, it may be impossible for worker B to be profitably employed at any firm that offers coverage.[3] For workers with higher health care costs, the availability of jobs that *do not* provide health coverage may be crucial to securing gainful employment. For them, a broad mandate on employer-based coverage could be especially harmful. In many ways, the ACA, by design, affects higher cost workers the same way the Americans with Disabilities Act (ADA) affected the employment prospects of workers with disabilities. It should surprise few that this paper finds the ACA's effects on higher cost workers are similar to the effects the ADA had on disabled persons (Acemoglu and Angrist, 2001). Indeed, the only surprise is that researchers have struggled to determine how labor market outcomes vary with individual health care usage already. As an example, Gruber's work on the incidence of mandated maternity benefits does not have the data on fertility events needed to test if those who have multiple or complicated births face larger wage reductions as a result of their maternity benefit coverage. Similarly, the data used by Bailey, Lahey, Sheiner, and Jensen and Morrisey do not have the information on actual health care usage that would help determine if two otherwise-identical workers are treated differently by employers because of their health expenses. On the other hand, authors who do leverage individual health care expenses have not been able to causally relate labor market outcomes to individual-specific variation in benefit expenses. Healthier workers might be expected to be systematically more productive, either innately or via reduced absenteeism, meaning the effects of health care expenses are hard to isolate empirically.

The identification problem is best illustrated by Levy and Feldman (2001) who search for individual-specific cost-shifting but ultimately conclude "[w]e attribute our failure to find useful results to the absence of exogenous variation in health insurance status; those who gain or lose health insurance are almost certainly experiencing other productivity-related changes that render our fixed-effects identification strategy invalid." Levy and Feldman also note that "exogenous variation in insurance coverage will be necessary in order to test these hypotheses." The Affordable Care Act provides the necessary exogenous variation.[4]

---

low and determining expenses is in any way costly then it may have made sense to ignore an individual's expected health care expenses when making employment decisions.

[3]Research by the Agency for Healthcare Research and Quality found that "health care expenses in the United States rose from $1,106 per person in 1980 ($255 billion overall) to $6,280 per person in 2004 ($1.9 trillion overall)." Available at http://archive.ahrq.gov/research/findings/factsheets/costs/expriach/index.html (accessed March 1, 2015).

[4]The ACA acts on workers at firms with no coverage as a group. Examining how this variation changes the coefficient on individual health expenses at firms that do not offer coverage after the ACA is announced allows inference of a causal relationship between health expenses and wages and other labor market outcomes. The perfect experiment to test for such a causal relationship would be to exogenously vary which jobs an individual applies for between firms who do and do not offer health insurance. Of course this is not feasible but if it were variation in wage offers could be causally related to individual health expenses without resorting to any higher level of variation such as mandates, firm sizes, or spatial and temporal variation.

The Act mandates that firms with more than 50 full-time employees provide comprehensive and affordable coverage for all workers who work more than 29 hours in a usual week. The Act also mandates coverage that is typically more generous in terms of benefits and eligibility than before.[5] The Act's provisions ensure that all firms will face an increase in benefit expenses but the impact will be greatest where coverage was not previously offered. The paper focuses on how labor market outcomes changed in the period after the announcement of the law (i.e., 2010 through late 2013) but before its full implementation in 2014. Using the period between the Act's announcement and implementation is essential to clean identification as the Act gave firms time to prepare but gave workers little incentives to change their behavior in the same period. Indeed, while individuals and the media struggled to wrap their heads around the new health care law, the health insurance industry reacted swiftly. By mid-2011 there is ample evidence that insurers had developed comprehensive reports advising firms of the Act's regulatory changes and how to prepare for them.[6] Underlining the importance of a swift response to the law, firms were to be experience rated for 2014 based on their employee pool in the prior year.[7]

The cost of not complying with the coverage mandate is significant. From 2014, firms with more than 50 workers were to face a penalty for not providing coverage of $2,000 per full-time employee excluding the first 30 employees. Given the available empirical evidence shows firms can and do pass on the cost of coverage to employees (at least as a group), the penalty represents a significant "stick." It would make little sense to pay the penalty when firms could offer coverage and reduce real wages to cover the cost.[8] Because paying the penalty would only make sense as some kind of protest, a firm who did not provide coverage before the new health care law can be expected to have the strongest economic incentives to institute exclusionary hiring practices or reduce wages, or both. The focus of this paper is examining if reactions to the law were focused on individuals who would be the most costly to cover.

Aiding identification, the Act gave workers no incentives to alter their behavior until January 2014.[9] At that point, the individual health care exchanges would open and whether a firm offers coverage or not would be less relevant to an individual's job search. Of course, workers could decide to change jobs in anticipation of the law's impact. However, behavior which would render the findings in this paper invalid would involve healthy workers joining firms that were not providing coverage *because* of the law. Such a move, if driven by the expected consequences of the Affordable Care Act, would be risky as there is no guarantee any particular firm would offer insurance when

[5] For more details, see Appendix C.

[6] A typical example is the Hudson Institute report for franchise owners in September 2011: http://www.franchise.org/uploadedFiles/HeathCare/The%20Effects%20of%20ACA%20on%20Franchising-%20Final.pdf

[7] http://www.washingtonpost.com/national/health-science/white-house-delays-health-insurance-mandate-for-medium-sized-employers-until-2016/2014/02/10/ade6b344-9279-11e3-84e1-27626c5ef5fb_story.html

[8] If workers value the coverage they are offered, then reductions in even nominal wages are possible.

[9] Except for workers under 26, who were allowed to remain on their parents insurance if their own employer did not offer coverage. The number of working individuals under 26 in the MEPS data is just over 300 each year. Empirical estimates generated using a "under-26" sub-sample show no effects of the ACA on higher cost young people. However, that could be because of the parental coverage mandate or just because so few young people have large medical expenses.

2014 eventually arrived. To make a fully informed decision an individual worker would need to know, before joining a no-coverage firm, the number of *full-time* workers employed, existing and future health care options, and be well-informed about options that would be provided on the new individual health care exchanges in case coverage was not provided by their employer.[10] Even if capable of such omniscience, workers would be free to wait until 2014 to change labor supply decisions. Because the law gave workers no reason to change their behavior the paper proceeds as if the law affects only firms in the period before its full implementation.

There is a risk that this paper, focused on determining if employers respond to variation in health coverage costs at the individual level, will be conflated with an analysis of the Affordable Care Act itself. The Affordable Care Act consists of many regulatory changes and this paper uses just one of its changes to identify an effect that has previously been difficult to observe. The ACA may compensate affected workers in other ways that leave them no worse off overall. At the same time, the paper's findings raise serious questions about the Act's potential efficacy and the wisdom of building the Act around the existing pillar of employer-provided coverage. If firms affected by the Affordable Care Act's mandate treat high and low cost workers differently, it is sensible to think that firms who already provided insurance coverage were already behaving this way. The ACA's employer mandate, by forcing more firms to provide coverage, may lead to worse labor market outcomes for those who would use health coverage the most. Ultimately, the evidence in this paper and the existing literature suggests that the bundling of employment and health insurance creates unnecessary and discriminatory distortions in the labor market.

It is worth noting that the results presented in this paper should be viewed as a lower bound. The estimates are biased towards zero if firms were not convinced the law would ever come into effect or if some firms were unaware of their responsibilities. However, focusing on firm behavior in the pre-implementation period is crucial to cleanly identifying how the employer mandate affects individual workers. Once the law is fully implemented identifying the effect of the employer mandate separately from the rest of the Act's provisions will be difficult. The most likely source of confounding variation will be the already-mentioned distortions introduced by the heavily-subsidized coverage available on the Act's individual health care exchanges. These exchanges will render clean identification impossible. While employers can be expected to continue to try to avoid the costs of the Act after 2014, labor market survey data will be affected by the coverage available on the exchanges. The exchanges provide affordable individual health coverage plans which might affect incentives to participate in the labor market, alter decisions on retirement and self-employment, or remove "job lock" effects. It could also reduce the intensity of unemployed workers' job search. The effects of the new coverage may be much stronger than those observed by Baicker et al. (2014) in the wake of Medicaid expansion in Oregon.

Underlining the importance of studying the pre-implementation period, a naive approach to this question using data from 2012 to 2016 or later may find no "effect" of the employer man-

[10]This is extremely unlikely in the period analyzed in this paper as the website (healthcare.gov) detailing coverage options for individuals was barely functioning as late as December 2013.

date. From that, the researcher may be tempted to state the incidence of health insurance is not individual-specific. Such a claim would be erroneous because the adjustments occurred before the stated implementation date of the Act.[11] Garrett and Kaestner (2015), Mathur et al. (2015), and Even and MacPherson (2015) also focus on the pre-implementation period to examine how the ACA has affected part-time employment rates. The analysis in this paper builds upon their work by examining if the incidence of the changes in the labor market are focused on higher-cost workers.

The paper proceeds with a review of the literature on employer-provided insurance and its effects on the labor market. The existing literature generally exploits state-level mandated benefits as a source of identifying variation. The review highlights that evidence of *individual-specific* incidence has been elusive to date and illustrates the importance of the identifying variation provided by the Affordable Care Act's employer mandate. Section 3 provides a theoretical framework to motivate the empirical analysis in Section 6. The section presents an equilibrium job search model which highlights the expected effects of a mandated benefit which is costlier to provide to some workers versus others. Comparative statics provide testable hypotheses. The empirical section examines the predictions of the model in difference-in-differences and triple-difference frameworks. The data used for this empirical analysis is described in Section 4. Section 5 details the Act's implementation timeline and reiterates the importance of focusing on the pre-implementation period for clean identification. Section 7 concludes.

2 Existing Literature

Summers (1989) provides a succinct analysis of the economics of mandated benefits, highlighting the ways in which they are similar to payroll taxes, where they differ, and why that makes them politically popular. Despite being only six pages long, including references, the paper called for and sparked a wave of research into the empirical regularities of mandated benefits. This paper adds to the existing literature by providing clean identification of the individual-specific incidence of a particular type of mandated benefit: employer-based health insurance.

The paper is very closely related to the work of Jonathan Gruber and regular co-authors on the incidence of mandated benefits, such as Gruber and Krueger (1991), Gruber (1993, 1994), Baicker and Chandra (2006), Baicker and Levy (2008), and others. Baicker and Levy examine the potential for employer mandates to impact a specific group, low-wage earners, finding that many positions where wages are close to the floor provided by the federal minimum wage may not be economically viable if employers were forced to provide health coverage to these workers, too. While not focused on a specific mandate, Baicker and Chandra find rising health care premiums reduce em-

[11] Indeed, many well-known studies of the impact of labor market policies are open to exactly this type of criticism. The most famous is likely Card and Krueger (1994) who study the implementation of a higher minimum wage by surveying selected employers in New Jersey and Pennsylvania the month before a wage increase comes into effect in New Jersey. They then re-survey these employers again 7 to 8 months after the higher minimum wage is in place. However, the wage increase the authors study was announced two years *before* its implementation date.

ployment levels but the effect they find is not group or individual-specific. The authors leverage exogenous variation provided by medical malpractice laws across states. Their method circumvents the endogeneity concerns with state-based mandates.

Gruber (1993) focused on the incidence of mandated maternity benefits. The paper examines changes in labor market outcomes for affected individuals in states that passed maternity benefit mandates and finds that higher coverage costs are shifted to affected workers. Gruber uses non-affected individuals (those not "at risk" for a covered childbirth event, such as single men and females who are past child-bearing age) in the same state as a comparison group for the first difference. The period before and after mandated changes and the difference between experimental and non-experimental states allow for a triple-difference estimation of the impact of the mandate on the treated group: married females of child-bearing age. Single females and married males were also affected by the law but data limitations lead to them being excluded from the analysis. Gruber finds that wages fall for the affected group by approximately the cost of the new benefit. To construct the cost of the mandated benefit Gruber uses complementary proprietary data on the probability of coverage, the type of coverage, and the price of covered events.

Gruber's paper is the type of empirical work Summers suggested would be valuable. Summers was concerned that mandated benefits could introduce exclusionary hiring practices if wages were not free to adjust for the cost of the benefit which employers were forced to provide. Employers could simply refuse to hire workers for whom the mandated benefit would be costly. If a mandated benefit resulted in such behavior Summers saw value in public provision of the benefit: "publicly provided benefits do not drive a wedge between the marginal costs of hiring different workers and so do not give rise to a distortion of this kind." Gruber's findings suggest that wages are actually free to adjust and workers don't pay more than the actual cost of the benefit. In a world where workers value the benefit they receive at its cost these findings minimize concerns about inefficiencies or labor market discrimination. In addition, the firm is no worse off as they should be indifferent between providing the same total compensation to a worker via reduced wages and increased benefits versus higher wages and lower benefits.

However, Gruber's identification strategy is open to criticism. The mandated benefit Gruber studies changes worker incentives in a way that could also explain many of his paper's findings. In particular, the provision of mandated maternity benefits could alter fertility decisions at the margin with follow-on consequences for labor market outcomes. Schmidt (2007) finds that maternity coverage mandates do increase fertility for females under 35. In addition, Gruber mentions that there is a rise in cesarean rates co-incident with the mandates, suggesting that the *type* of person having a baby after the law may also be different, violating the core assumptions of Gruber's identification strategy. Moreover, if the law resulted in marginally more births after the law, Gruber's results may be due to new mothers choosing to 1) reduce their supply of labor or 2) to forgo available employment advancement opportunities until fertility plans have been completed. As non-parents are substitutes for the jobs that the treated group are choosing not to pursue, such a mechanism would widen between-group estimates in both directions simultaneously. The *treated* group in the experimental states "choose" to earn less than those in non-experimental states (i.e., they choose to start or have a larger family with consequences for hours worked, promotions, and wage increases), leaving employment opportunities and promotions open for non-parents. Simultaneously, the non-treated in the non-experimental states face labor market competition from those who would be the treated group in experimental states. The systematic correlation between the mandates, fertility decisions, and wages plausibly violates Gruber's only identifying assumption.

Due to the confounding effects on employee behavior, it is unclear if the estimates Gruber provides are reliable. It may not be the case wages are affected for the treated group, but that the provided benefit allows those at the margin to substitute towards increased fertility rather than labor. Even if the estimates are reliable, it is not clear what the mechanism that determines outcomes actually is: is it firm or worker behavior? This paper is not subject to the same identification concerns. The Affordable Care Act's employer mandate gave employers a multi-year pre-implementation period to adjust the composition of their workforce to minimize the impact of the law. During the same period, the law's effects on individuals (particularly those over 26) are essentially zero.[12] Any potential effects on workers' labor supply or health care decisions can be ignored as workers are free to wait until after the Act's full implementation to adjust their behavior and would be taking a risk to do so in advance. The asymmetrical early impact of the employer mandate allows the findings presented in Section 6 to have a causal interpretation.

A series of authors also find that when an identifiable group is affected by a mandate, labor market outcomes for that group are affected negatively. Complementing Gruber's work, Lahey (2012) examines infertility mandates and finds that older females suffer reductions in employment but not wages. Sheiner (1999) uses regional variation in health care costs to try to causally relate wages and benefit expenses for older Americans. Sheiner argues that older individuals in high cost areas should have relatively lower wages when compared with older workers in lower-cost regions, all else being equal. Her results echo Gruber in showing that employers are able to shift the cost of health insurance onto *groups* who are more expensive to insure but can say nothing about the effects of individual-specific variation within a group.

Thurston (1997) examines the unique experience of Hawaii. Hawaii mandated employer provision of health insurance to full-time workers in 1974. Part time workers were not covered. Thurston estimates that in industries that had mainly full-time employees a 10 percentage point increase in employees who would be covered lead to a 1 percentage point increase in part-time jobs which were not covered by the law. Thurston's findings are confirmed by Buchmueller et al. (2011). However, neither paper explains if, *within* an affected group, individuals with varying costs of coverage are affected differently.

Kolstad and Kowalski (2012) examine the broad effects of Massachusetts 2006 health care reform. The Massachusetts reform was viewed by many as a precursor to the Affordable Care Act and their design and implementation are quite similar. The authors find that wages at firms who

[12] From 2011 onward, the Act mandated that young adults up to the age of 26 were allowed to remain on parent's coverage as a dependent.

were forced to provide coverage fall by approximately the average cost of coverage compared to firms who already provided coverage. It would be possible to repeat the analysis in this paper using Massachusetts data from the time before and after their reform date except the public use Medical Expenditure Panel Survey (MEPS) data does not identify which state a respondent lives in. Data on state of residence is available in the restricted use files which can be accessed at the AHRQ/Census Research Data Centers. However, the MEPS is not on the same scale as the CPS or ACS and examining a single state would limit the number of usable observations to no more than a few hundred in a given year. Placing even greater demands on this data, identification would have to be based on what happens to workers at firms who did not already provide health insurance. In the MEPS data for the country as a whole, this is less than 30% of the working adult population meaning identification would rely on only a few-dozen Massachusetts-based observations per year.

Essentially, the MEPS data contains individual health care usage and expenses but cannot be effectively used with any paper that relies on variation in a single state as it would slice the data too thinly. Moreover, the MEPS in its current form only stretches back to 1996, long after state mandates used for identification in the work of Gruber and others. However, data-sets that report wages, insurance coverage, *and* state of residence, such as the CPS, do not collect data on how much health care services an individual consumes. These data limitations are a major reason why authors have struggled to identify the individual-specific effects of mandated benefits or employer based health insurance.

In an empirical set-up almost identical to Gruber's maternity benefit paper, Bailey (2014) finds that prostate screening mandates are passed on to men over 50, the group most likely to benefit from the improved coverage. Bailey (2013) finds similar results for diabetes mandates. However, neither paper uses individual variation in costs or usage of health care services to examine if the cost is passed on at the group or individual level.

The effects of individual-specific variation are important because experience-rating ensures that different workers cost firms different amounts to cover. For example, many of the positions that Baicker and Levy suggest would be lost in the advent of an employer mandate may be viable if enough employees with very low health care expenses could be found for these positions. The positions only appear non-viable because it is supposed that the workers in those positions would have average health care expenses.

Attempting to address the issue of individual-specific cost-shifting Levy and Feldman (2001) estimate wage change regressions that condition on health insurance coverage, changes in employee premium contributions, health status, and an interaction between health insurance changes and health status. Using data from 1996 Medical Expenditure Panel Survey, they do not find evidence of individual-specific cost-shifting. However, the identification strategy used, examining wages and benefits only for job switchers, introduces severe endogeneity problems. Pauly and Herring (1999), using the 1987 National Medical Expenditure Survey, claim to address the question of whether there is individual-specific cost-shifting. They consider measures of predicted medical expenses and age, interacted with health insurance coverage. They find that wages for older workers rise more slowly for those who have health insurance coverage than for those who do not, suggesting that workers "pay" for their benefits. However, their terminology is loose. Their finding is still a group offset, not an individual-specific offset.

The model and associated empirical results in this paper largely confirm the predictions of Mitchell (1990) who surveyed the literature on compensating differentials in the workplace to predict the effect of mandated benefits. Mitchell expected a mandated health benefit package to cause wages to fall and for firms to treat workers with higher expenses differently to those with lower expenses.

Garrett and Kaestner (2015) and Mathur et al. (2015) are two early examinations of the labor market consequences of the ACA. Similarly to this paper, they both focus on the pre-implementation period but only examine how the law has affected hours worked. As the employer mandate only applies to those who work more than 30 hours per week the authors suspect employers may move towards more part-time employees. Using CPS data they find no or very minor negative effects. However, both take an unusual approach to identification making no adjustments for whether firms do or do not provide cover. As an example, in the Mathur et al. paper the authors focus on how the odds ratio between those working 25-29 hours and 31-35 hours changes after the ACA is announced. They find limited effects that are not statistically significant suggesting the ACA did not cause reduced hours. The authors make no attempt to stratify the sample into firms who do and do not offer coverage nor do they consider the cost of coverage at the individual level. The distinction matters because it is not reasonable to claim that a firm who voluntarily provided coverage to a worker who worked 31-35 hours is affected by the law in the same way a firm who did not provide coverage is affected. The mandate only binds at firms who did not offer coverage to workers who worked 31-35 hours each week before the Act was announced. Lumping firms who do and do not offer coverage into the same bucket biases results towards zero. In addition, the cost of providing coverage is quite small for many employees but expensive for others. A young, single, healthy employee working 32 hours per week might not add much to the firm's costs of coverage.

Even and MacPherson (2015) also focus on the pre-implementation period to examine how the ACA has affected involuntary part-time employment. Their approach is similar to this paper as it allows the Act to have greater bite where coverage was not previously offered. They focus on the proportions of workers offered health coverage within an industry and then construct aThe ACA acts on workers at firms with no coverage as a group. Examining how this variation changes the co-efficient on individual health expenses at firms that do not offer coverage after the ACA is announced allows inference of a causal relationship between health expenses and wages and other labor market outcomes. The perfect experiment to test for such a causal relationship would be to exogenously vary which jobs an individual applies for between firms who do and do not offer health insurance. Of course this is not feasible but if it were variation in wage offers could be causally related to individual health expenses without resorting to any higher level of variation such as mandates, firm sizes, or spatial and temporal variation. counter-factual estimate of what full- and part-time employment would be (absent the ACA) based on prevailing economic conditions. In contrast to Garrett and Kaestner and Mathur et al. they find a strong effect of the Act on part-time employment and suggest that around one million workers may be under-employed as a result of the Act.

An additional important avenue for cost-shifting, the employee's contribution to employer-provided benefits, is examined by Levy (1998). Levy finds that worker contributions play an important role in employee sorting and provide employer flexibility to tailor benefit packages to match their workforces' preferences. The role of employee contributions cannot be examined in this paper as identification often relies on the pre-implementation period at firms where no insurance was in place. In addition, firms would be free to take a wait and see approach with decisions on employee contributions. Data on employee contributions *after* the mandate comes into effect will not be available until late 2017.

Overall, prior studies of the incidence of mandated benefits have been significantly clouded by data availability and suitability, instances of simultaneity bias, and the inseparable interaction between firm and worker reactions to policy changes. The provisions of the Affordable Care Act, in conjunction with the rich data provided by the Medical Expenditure Panel Survey, solves the identification issues and provides a clearer analysis of the impact of mandated benefits, such as mandated health coverage, on labor market outcomes.

3 Model

This section presents a job search model that builds upon the work of Mortensen (1990) and, particularly, Bowlus and Eckstein (2002). Bowlus and Eckstein develop their model to examine racial discrimination. They focus heavily on the equilibrium predictions and structurally estimate their model's parameters to identify the role Beckerian-style discrimination plays in the black-white wage gap (Becker, 1957). The job search model in this paper is similar in spirit but presents employers who face a cost of providing coverage to high expense workers that is above the value those workers place on that coverage. The gap between a worker's valuation and the employer's actual cost emerges under the assumption that the firm pays the full cost of coverage, whereas an individual can insulate themselves from the full cost by purchasing insurance coverage privately.

The model considers a labor market with just two types of employers and two types of workers. In equilibrium, workers maximize utility by choosing to work at any job that meets their type-specific reservation wage. Workers can search on and off-the-job, and switch if they receive a utility-increasing offer. Employers, only some of whom provide insurance coverage, also maximize utility which is represented by the sum of profits per worker. Formally, suppose there are a total of M workers, a proportion $(1 - \theta)$ of whom are type A and θ are type B. Type A workers are considered "healthy". They have productivity P_A. Type B workers are defined as the unhealthy workers with productivity P_B. Healthier workers are assumed to be more productive so that $P_A \geq P_B$.[13] Employers maximize utility that depends on profits and preferences over the types of workers. In particular, a fraction γ_d of employers provide health coverage providing them with a disincentive to hire type B workers. These employers are referred to as type d. Those who do not provide coverage are referred to as type n. Excusing the abuse of notation, type d employers face a cost $w_B + d$ when they hire a type B worker where w_B represents the value of wages and health coverage to the Type B worker.[14] Type A workers have no health care expenses, by assumption. The number of firms is normalized to 1 while θ and γ_d are determined exogenously.

Arrival rates are drawn from a Poisson distribution. For a type A worker, offers arrive at a rate λ_1 if employed and λ_0 if unemployed. Unemployed workers are assumed to search more intensively than employed workers so that $\lambda_0 > \lambda_1$. Arrival rates for each type of worker differ by a scaling term k where $0 \leq k \leq 1$. The arrival rate of offers to unemployed (employed) type B workers from type n employers is $\lambda_0(\lambda_1)$ and $k\lambda_0(k\lambda_1)$ from type d employers. If $k = 0$ then employers who offer coverage never hire type B workers. If $k = 1$ offer arrival rates for both workers are the same at both types of employers. If $d = 0$ or if there are no employers who offer health insurance $(\gamma_d = 0)$ then $k = 1$ by assumption and a standard model of job search with heterogeneous productivity obtains. Employers do not condition offers on current employment status. For employed workers, their jobs are destroyed at a rate δ_i for $i = A, B$ where $\delta_A \leq \delta_B$. Additionally, job destruction rates are not permitted to vary by worker *and* firm type. Adding firm-specific destruction rates would unnecessarily complicate the model by requiring reservation wage rules that differ for each type of firm.

Employers

Employers consider workers' reservation wages and wage offer distributions as given. Therefore, wage offers are conditioned on worker type but not a worker's current wage and employers can only post one wage offer for each *type* of worker. They set wages to maximize utility. For type n employers utility is the sum of their profit times the number of each type of worker;

$$U_n(w_A, w_B) = (P_A - w_A)l_n^A(w_A) + (P_B - w_B)l_n^B(w_B) \tag{1}$$

where w_i are the wages to each type of worker and $l_n^i(w_i)$ is the stock of of type i workers at wage w_i in the steady state. For type d employers;

$$U_d(w_A, w_B) = (P_A - w_A)l_d^A(w_A) + (P_B - d - w_B)l_d^B(w_B) \tag{2}$$

Note that d is sufficiently small so that the firm receives positive utility from type B workers.

[13]The reader can think of this increased productivity as due to less absenteeism, greater physical ability, stamina, etc.

[14]The firm views the cost of the worker as higher than the worker views their total compensation package. In experience rated or self-insured firms, the firm pays the full cost of a worker's coverage plus a salary. For the worker, if they did not have cover, their expenses would potentially not be as high or they may be able to obtain community rated, low cost, subsidized, or even free coverage.

Workers

As in standard job search models, workers choose state-contingent reservation wages to maximize their utility. The reservation wage of an *employed* type A worker is simply their current wage w and they accept any better offer from any firm. The reservation wage while unemployed is solved by equating the value of unemployment and the value of being employed at the reservation wage. The value of unemployment is

$$\begin{aligned}(1+\beta dt)V_U^A &= bdt + \lambda_0(1-\gamma_d)dtE_w^n max(V_E^A(w), V_U^A) \\ &\quad +\lambda_0\gamma_d dtE_w^d max(V_E^A(w), V_U^A) + (1-\lambda_0 dt)V_U^A\end{aligned}$$

where β is the rate of time preference, and b is the value of the time given up when working. The instantaneous value of unemployment is the sum of the value of leisure, the probability of getting a job from a no-coverage firm, the probability of getting an offer from a firm that does provide coverage, plus the probability of remaining unemployed.

The value of being employed is a function of the current wage and all possible transitions. That is, the value of being employed at wage w for a type A worker, is

$$\begin{aligned}(1+\beta dt)V_E^A(w) &= wdt + \lambda_1(1-\gamma_d)dtE_{w'}^n max(V_E^A(w'), V_E^A(w)) \\ &\quad +\lambda_1\gamma_d dtE_{w'}^d max(V_E^A(w'), V_E^A(w)) + \delta_A dtV_U^A \\ &\quad +(1-(\lambda_1+\delta_A)dtV_E^A(w)\end{aligned}$$

which is the sum of the current wage, the probabilities and expected values of job offers from each type of firm, the probability and value of becoming unemployed, plus the probability and value of remaining employed at wage w. The value functions of type B workers are constructed similarly;

$$\begin{aligned}(1+\beta dt)V_U^B &= bdt + \lambda_0(1-\gamma_d)dtE_w^n max(V_E^B(w), V_U^B) \\ &\quad +k\lambda_0\gamma_d dtE_w^d max(V_E^B(w'), V_U^B) \\ &\quad +(1-(\lambda_0(1-\gamma_d)+k\lambda_0\gamma_d)dt)V_U^B\end{aligned}$$

and

$$\begin{aligned}(1+\beta dt)V_E^B(w) &= wdt + \lambda_1(1-\gamma_d)dtE_{w'}^n max(V_E^B(w'), V_E^B(w)) \\ &\quad +k\lambda_1\gamma_d dtE_{w'}^d max(V_E^B(w'), V_E^B(w)) + \delta_B dtV_U^B \\ &\quad +(1-(\lambda_1(1-\gamma_d)+k\lambda_1\gamma_d+\delta_B)dt)V_E^B(w)\end{aligned}$$

The expressions differ in offer arrival and job destruction rates and, consequently, wage offers.

Let $F_n^i(w)$ and $F_d^i(w)$ be the distribution of wage offers for the two types of employers for type i workers ($i = A, B$). The reservation wage for a worker of type i is the value of r_i that equates the value of employment and unemployment. Setting $V_E^i(r_i) = V_U^i$ and solving for r_A and r_B gives;

$$r_A = b + \int_{r_A}^{\infty} \frac{(\lambda_0 - \lambda_1)\left((1-\gamma_d)\left(1 - F_n^A(w)\right) + \gamma_d\left(1 - F_d^A(w)\right)\right)}{\beta + \delta_A + \lambda_1\left((1-\gamma_d)\left(1 - F_n^A(w)\right) + \gamma_d\left(1 - F_d^A(w)\right)\right)} dw \quad (3)$$

and

$$r_B = b + \int_{r_B}^{\infty} \frac{(\lambda_0 - \lambda_1)\left((1-\gamma_d)\left(1 - F_n^B(w)\right) + k\gamma_d\left(1 - F_d^B(w)\right)\right)}{\beta + \delta_B + \lambda_1\left((1-\gamma_d)\left(1 - F_n^B(w)\right) + k\gamma_d\left(1 - F_d^B(w)\right)\right)} dw \quad (4)$$

The reservation wage is composed of the value of time given to labor b, plus an expectation of a random variable which is is increasing with the value of not working (expressed in the numerator in the integrated term) and decreasing with the value of being employed (the denominator in the same term). The numerator is composed of expected wages (the longer term in parentheses) scaled by the difference in arrival rates of offers for unemployed and employed workers. As λ_0 rises, unemployment becomes relatively more attractive, and less attractive if λ_1 rises. The denominator scales the value of unemployment by the rate of time preference (with higher β representing *less* patience), the chance of job destruction (accepting an offer now seems less valuable if the chance of destruction is very high, all else equal) and the likelihood of job offers (and their associated wages) once already employed.

Equilibrium

Standard job search model equilibrium conditions apply:

1. Reservation wages are set to maximize utility.
2. Flows of workers in and out of employment are equal.
3. The utility of the employers is maximized and equal within each type of firm, given the behavior of other agents.

Importantly, because employers' utility is additive the steady-state flows and wage offer distributions for each type of worker can be solved independently.

For type A workers:

Type A workers are treated the same at each type of firm so $F_n^A(w_A) = F_d^A(w_A) = F^A(w_A)$. The equilibrium wage offer distribution is;

$$F^A(w_A) = \frac{1 + \kappa_{1A}}{\kappa_{1A}} \left[1 - \left(\frac{P_A - w_A}{P_A - r_A}\right)^{1/2}\right] \quad r_A \leq w_A \leq wh_A \quad (5)$$

Where κ is a measure of the ratio of offers to job destruction, $\kappa_{1i} = \lambda_1/\delta_i$ and wh_A is such that $F^A(wh_A) = 1$. Note that because the wage distribution of each worker can be solved independently, type A workers are no different to the job searchers in Mortensen (1990). For the reservation wage;

$$\begin{aligned} r_A &= b + (\kappa_{0A} - \kappa_{1A}) \int_{r_A}^{wh_A} \left[\frac{1 - F^A(w_A)}{1 + \kappa_{1A}(1 - F^A(w_A))} \right] dw_A \\ &= b + (\kappa_{0A} - \kappa_{1A}) \int_{r_A}^{wh_A} \left[\frac{1 - \frac{1+\kappa_{1A}}{\kappa_{1A}} \left[1 - \left(\frac{P_A - w_A}{P_A - r_A} \right)^{1/2} \right]}{1 + \kappa_{1A}(1 - \frac{1+\kappa_{1A}}{\kappa_{1A}} \left[1 - \left(\frac{P_A - w_A}{P_A - r_A} \right)^{1/2} \right])} \right] dw_A \end{aligned}$$

Because $F^A(wh_A) = 1$ then;

$$wh_A = P_A - \left(\frac{1}{1 + \kappa_{1A}} \right)^2 (P_A - r_A) \tag{6}$$

and the reservation wage for type A workers is;

$$r_A = \frac{(1 + \kappa_{1A})^2 b + (\kappa_{0A} - \kappa_{1A})\kappa_{1A} P_A}{(1 + \kappa_{1A})^2 + (\kappa_{0A} - \kappa_{1A})\kappa_{1A}} \tag{7}$$

Using the derived expressions for offers and reservation wages, the earnings distribution $G^A(w_A)$ can then be recovered:

$$G^A(w_A) = \frac{1}{\kappa_{1A}} \left[\left(\frac{P_A - w_A}{P_A - r_A} \right)^{1/2} - 1 \right] \quad r_A \leq w_A \leq wh_A \tag{8}$$

For type B workers:

The wage distribution for type B workers is a mixture of two distinct distributions in which type d employers offer lower wages and type n employers offer higher wages. In particular;

$$l_d^B(w_B) = \frac{k\kappa_{0B}(1 + \kappa_{1B}^k)\theta M}{(1 + \kappa_{0B}^k)\left(1 + k\kappa_{1B}\gamma_d(1 - F_d^B(w_B)) + \kappa_{1B}(1 - \gamma_d)\right)^2} \quad r_B \leq w_B \leq wh_d \tag{9}$$

$$l_n^B(w_B) = \frac{\kappa_{0B}(1 + \kappa_{1B}^k)\theta M}{(1 + \kappa_{0B}^k)\left(1 + \kappa_{1B}(1 - \gamma_d)(1 - F_n^B(w_B))\right)^2} \quad wh_d \leq w_B \leq wh_B$$

where $l_i^B(w_B)$ represents the stock of B type workers and $0 \leq k \leq 1$ and the wage offer distribution is

$$F^B(w_B) = \begin{cases} \frac{1+\kappa_{1B}^k}{k\kappa_{1B}} - \left(\frac{1+\kappa_{1B}^k}{k\kappa_{1B}} \right) \left(\frac{P_B - d - w_B}{P_B - d - r_B} \right)^{1/2} & r_B \leq w_B \leq wh_d \\ \frac{1+\kappa_{1B}(1-\gamma_d)}{\kappa_{1B}(1-\gamma_d)} - \left(\frac{1+\kappa_{1B}(1-\gamma_d)}{\kappa_{1B}(1-\gamma_d)} \right) \left(\frac{P_B - w_B}{P_B - wh_d} \right)^{1/2} & wh_d \leq w_B \leq wh_B \end{cases} \tag{11}$$

so that the earnings distribution for type B workers is

$$G^B(w_B) = \begin{cases} \frac{\kappa_{0B}}{\kappa_{1B}\kappa_{0B}^k}\left[\left(\frac{P_B-d-w_B}{P_B-d-r_B}\right)^{1/2} - 1\right] & r_B \leq w_B \leq wh_d \\ \frac{\kappa_{0B}}{\kappa_{1B}\kappa_{0B}^k}\left[\frac{1+\kappa_{1B}^k}{1+\kappa_{1B}(1-\gamma_d)}\left(\frac{P_B-wh_d}{P_B-w_B}\right)^{1/2} - 1\right] & wh_d \leq w_B \leq wh_B \end{cases} \tag{12}$$

where wh_B is the highest wage offered to type B workers; wh_d is the highest wage offered to type B workers at the employers who experience a cost d due to hiring them; $\kappa_{iB}^k = \kappa_{iB}(1-\gamma_d) + k\kappa_{iB}\gamma_d$ for $i = 0, 1$; and $F^B(w_B)$ is the market wage offer distribution, the fraction of all employers paying w_B or less to type B workers. Note that $F^B(w_B) = (1-\gamma_d)F_n^B(w_B) + \gamma_d F_d^B(w_B)$. The derivation of these results is presented in the Appendix.

Properties of Equilibrium

It is relatively easy to show that $G^A(w_A) \leq G^B(w_B)$ and $r_B \leq r_A$ (see Bowlus and Eckstein for details) so that type B workers receive and are willing to accept lower wages, as we might expect just from their lower productivity. It is precisely because wage distributions and reservation wages can be expected to be lower for less healthy individuals that identifying the individual-specific incidence of employer-provided health insurance has troubled the literature to date. Observing that wages are lower for individual high cost workers at firms that provide health insurance does not explain if the effect is due to productivity differences or individual-specific cost-shifting of insurance expenses. The Affordable Care Act provides identification by affecting γ_d *exogenously*. The effects of the Act can then be predicted by examining comparative statics for type B workers with respect to γ_d within the model.

1. Expected Earnings

The ratio of earnings between the two types of workers is negatively related to d and γ_d. Consider the mean earnings of type A workers given by Mortensen;

$$E^A(w_A) = \int_{r_A}^{wh_A} w_A dG^A(w_A) = \frac{1}{1+\kappa_{1A}}(P_A\kappa_{1A} + r_A) \tag{13}$$

Notice the expected wage is not a function of d. For type B workers, their mean earnings are found by considering;

$$
\begin{aligned}
E^B(w_B) &= \frac{k\gamma_d}{k\gamma_d+1-\gamma_d}\int_{r_B}^{wh_d} w_B dG^B(w_B) + \frac{1-\gamma_d}{k\gamma_d+1-\gamma_d}\int_{wh_d}^{wh_B} w_B dG^B(w_B) \\
&= (1-\gamma_d)\frac{1+\kappa_{1B}^k}{1-\gamma_d+k\gamma_d}\left[\frac{\kappa_{1B}(1-\gamma_d)P_B}{(1+\kappa_{1B}(1-\gamma_d))^2}+\frac{r_B}{(1+\kappa_{1B}^k)^2}\right] \\
&\quad +\gamma_d\frac{k}{(1-\gamma_d+k\gamma_d)(1+\kappa_{1B}^k)}\left[\frac{k\kappa_{1B}\gamma_d\,(P_B-d)}{1+\kappa_{1B}(1-\gamma_d)}+r_B\right] \\
&\quad +\gamma_d(1-\gamma_d)\frac{k\kappa_{1B}(1+\kappa_{1B}^k)(2+2\kappa_{1B}(1-\gamma_d)+k\kappa_{1B}\gamma_d)}{(1-\gamma_d+k\gamma_d)(1+\kappa_{1B}^k)^2(1+\kappa_{1B}(1-\gamma_d))^2} \\
&\quad \times(P_B-d)
\end{aligned}
$$

While this is a complicated expression, $\partial r_B/\partial d < 0$, and therefore $\partial E^B(w_B)/\partial d < 0$. It is also straightforward to show that $\partial E^B(w_B)/\partial\gamma_d < 0$. To see this, note that if $\gamma_d = 1$ then

$$E^B_{\gamma_d=1}(w_B) = \frac{k\kappa_{1B}\,(P_B-d)+r_B}{1+k\kappa_{1B}} \tag{14}$$

and that if $\gamma_d = 0$ then

$$E^B_{\gamma_d=0}(w_B) = \frac{1}{1+\kappa_{1B}}(P_B\kappa_{1B}+r_B) \tag{15}$$

Given that $E^B_{\gamma_d=0}(w_B) > E^B_{\gamma_d=1}(w_B)$ and since $E^B(w_B)$ falls between $E^B_{\gamma_d=0}(w_B)$ and $E^B_{\gamma_d=1}(w_B)$ and approaches $E^B_{\gamma_d=1}(w_B)$ as γ_d increases it must be the case that $\partial E^B(w_B)/\partial\gamma_d < 0$. The prediction of the model provides an empirically testable hypothesis:

> **Hypothesis 1:** as the proportion of employers who provide coverage grows, the wages of type B workers can be expected to fall, *all else equal*.

While the proportion of employers who provide health coverage (γ_d) and the specifics of that coverage (affecting d) certainly changes over time, the pre-implementation period of the Affordable Care Act is as close as a researcher can hope to get to variation in γ_d where all else is equal.

2. Labor Stocks and Segmentation

For a single firm who moves from type n to type d (due to the ACA's legislative changes), in equilibrium they will move from employing $l_n^B(w_B^n)$ to $l_d^B(w_B^d)$ of type B workers where $w_B^n \neq w_B^d$. Due to utility equalization among firm types, the model cannot provide an unambiguous prediction on the labor stock change at a *particular* firm *within* a type. Becoming a type d firm decreases the attractiveness of type B workers but type B workers accept lower wages at type d firms. That is, there are competing income and substitution effects and it is not clear from the model exactly what will happen at a given firm.[15]

[15]The ambiguity is described in detail in Appendix B.

However, if a firm was selected randomly and forced to become type d then predictions can be made on the *expected* labor stock at such a firm. Remember that;

$$l_d^B(w_B^d) = \frac{k\kappa_{0B}(1+\kappa_{1B}^k)\theta M}{(1+\kappa_{0B}^k)\left(1+k\kappa_{1B}\gamma_d(1-F_d^B(w_B^d))+\kappa_{1B}(1-\gamma_d)\right)^2} \quad r_B \leq w_B \leq wh_d \tag{16}$$

$$l_n^B(w_B^n) = \frac{\kappa_{0B}(1+\kappa_{1B}^k)\theta M}{(1+\kappa_{0B}^k)\left(1+\kappa_{1B}(1-\gamma_d)(1-F_n^B(w_B^n))\right)^2} \quad wh_d \leq w_B \leq wh_B$$

To simplify the analysis assume a firm keeps its relative position in the wage distribution when moving from type n to type d so that $1-F_d^B(w_B) = 1-F_n^B(w_B)) = p$. Then the labor stocks are simply

$$l_d^B(w_B) = \frac{k\kappa_{0B}(1+\kappa_{1B}^k)\theta M}{(1+\kappa_{0B}^k)\left(1+k\kappa_{1B}\gamma_d(p)+\kappa_{1B}(1-\gamma_d)\right)^2} \tag{18}$$

$$l_n^B(w_B) = \frac{\kappa_{0B}(1+\kappa_{1B}^k)\theta M}{(1+\kappa_{0B}^k)\left(1+\kappa_{1B}(1-\gamma_d)(p)\right)^2}$$

Which means $l_n^B(w_B^n) > l_d^B(w_B^d)$ if

$$1+k\kappa_{1B}\gamma_d(p)+\kappa_{1B}(1-\gamma_d) > k^{1/2}\left(1+\kappa_{1B}(1-\gamma_d)(p)\right) \tag{20}$$

Which is true as k and p are between zero and one. While there is no guarantee a firm would maintain its relative position across the distribution of wage offers, when a large number of firms moves from type n to type d the effect must hold in aggregate.[16] The model's predictions regarding employment levels provide a second testable hypothesis:

> **Hypothesis 2a:** Type n firms who become type d will employ fewer type B workers.

Additionally, the solution to the model shows that the distribution of wages for type B is composed of two disjoint distributions indicating that employers will pay strictly lower wages to type B workers after becoming type d employers giving another testable hypothesis:

> **Hypothesis 2b:** Type n firms who become type d will then employ type B workers at a reduced wage.

3. Unemployment Rate and Duration

The cost of providing coverage for type B workers introduces unemployment rate and duration effects. To see this note that, in equilibrium, all job offers are accepted and since offers are

[16] Note that a firm becoming type d results in equilibrium effects on $l_n^B(w_B)$ and $l_d^B(w_B)$ through an increase in γ_d. The effects are described in full in Appendix B.

drawn from a Poisson distribution, expected unemployment durations are

$$\frac{1}{\lambda_0} \tag{21}$$

for type A workers and

$$\frac{1}{\lambda_0(1-\gamma_d(1-k))} \tag{22}$$

for type B workers. So long as $k \neq 1$, type B workers face longer unemployment spells as the *duration* of unemployment is positively associated with γ_d. Additionally, if $k \neq 0, 1$ and $\delta_A \leq \delta_B$ it can be shown that the *rate* of unemployment is higher for type B workers.

$$ue_B = \frac{\lambda_0(1-\gamma_d) + k\lambda_0\gamma_d}{\delta_B + \lambda_0(1-\lambda_d) + k\lambda_0\gamma_d} \geq \frac{\lambda_0}{\delta_A + \lambda_0} = ue_A \tag{23}$$

where ue_i is unemployment rate of type i. Note that the rate of unemployment is increasing in γ_d for type B workers. These comparative statics provide another testable hypothesis:

> **Hypothesis 3:** After a mandate on coverage is implemented the rate and duration of unemployment for type B workers increases.

Relative separation rates can be higher or lower for Type B workers depending on specific values of the model's parameters. Separation rates are presented in the Appendix for completeness.

Gathering Results

While abstracting from many features of the labor market, the model presented demonstrates that type B workers can be expected to have "worse" labor market outcomes even before any mandate on coverage is implemented. After an increase in γ_d type B workers can expect that in aggregate;

1. Their earnings will fall
2. They will face higher unemployment rates
3. They will search for employment longer

These predictions are "equilibrium" results encapsulated in Hypotheses 1 and 3. With the Affordable Care Act acting as natural experiment affecting γ_d exogenously, these hypotheses can be tested empirically using earnings, unemployment rates, and unemployment duration as dependent variables in a difference-in-differences framework. Firms who are forced to provide coverage (i.e., moving from type n to type d) will employ fewer type B workers than before (Hypothesis 2*a*). Additionally, the model indicates (Hypothesis 2*b*) that employers will pay strictly lower wages to type B workers if they become type d employers. Hypotheses 2*a* and 2*b* are tested empirically by exploiting the variation in health insurance provision at the firm level. By comparing the labor market outcomes of higher-cost workers at firms that do and do not provide coverage to lower-cost workers at the same types of firms, before and after the law, the effect of the Act's employer mandate at the individual level can be observed.

4 Data

The empirical analysis in Section 6 uses data from the Medical Expenditure Panel Survey (MEPS). The Agency for Healthcare Research and Quality describes the MEPS as "a set of large-scale surveys of families and individuals, their medical providers, and employers across the United States. MEPS is the most complete source of data on the cost and use of health care and health insurance coverage."[17] A new cohort joins the survey each calendar year and respondents participate in five detailed interviews across a two-year period which collect data on health care usage, out of pocket costs, insurance coverage, along with demographic and employment information at each interview date. The data is ideal for examining the effect of the new laws on the labor market outcomes of individuals with higher coverage expenses. The MEPS began in 1996 and each year a sub-sample of households participating in the previous year's National Health Interview Survey (NHIS) are selected to participate. The NHIS sampling frame provides a nationally representative sample of the U.S. civilian non-institutionalized population, and reflects an over-sample of minorities. Additional policy relevant subgroups (such as low income households) are over-sampled by the MEPS. Most importantly, the MEPS provides data on the actual health care expenses of individuals, allowing for a researcher to examine if individual labor market outcomes vary with health care consumption. Unfortunately, the MEPS began *after* many state-level mandated benefit programs had been put in place so the data cannot be used to re-visit the impact of variation in insurance mandates at the state level. Even if the data covered the period before these mandates were in place the MEPS public-use files do not provide state of residence. As the MEPS samples only a few dozen people per year from smaller states, even the restricted use data would pose problems if used to study state-level changes.

[17] http://meps.ahrq.gov/mepsweb/

Table 1: Summary Statistics for the MEPS Data (by Year)

	Employer Does Not Offer Coverage								
	2006	2007	2008	2009	2010	2011	2012	2013	**Total**
Sex	%	%	%	%	%	%	%	%	%
Female	54.8	52.4	49.3	51	49.6	50	47.6	51.6	**50.6**
Male	45.2	47.6	50.7	49	50.4	50	52.4	48.4	**49.4**
Total	100	100	100	100	100	100	100	100	100
Race	%	%	%	%	%	%	%	%	%
White	82	78.3	77.2	77	75.3	76.2	73.8	74.2	**76.6**
Black	11.9	14.1	15	15.1	14.6	15.6	15.9	16.4	**14.9**
Other	6.1	7.7	7.9	8	10.1	8.2	10.3	9.5	**8.5**
Total	100	100	100	100	100	100	100	100	100
Education	%	%	%	%	%	%	%	%	%
High School	66.3	64	67.5	66.3	63.1	64.9	61.1	61.7	**64.3**
College	29.3	30.1	27.6	28.3	32.9	30.4	35.5	34.8	**31.3**
More than College	4.5	5.9	4.8	5.4	4	4.7	3.4	3.5	**4.4**
Total	100	100	100	100	100	100	100	100	100
Age (in years)	39.9	39.4	39.5	39.5	39.3	39.0	39.6	39.3	**39.5**
Wage ($Annual)	24,696	25,441	23,368	23,198	23,294	22,975	22,777	20,677	**23,176**
Health Expenses ($Annual)	1,808	2,023	1,737	2,217	2,164	1,316	1,353	1,625	**1,741**
Offered Employer-based Coverage	26.2%	24.9%	26.3%	26.1%	26.1%	27.8%	30.8%	30.3%	**27%**

	Employer Offers Coverage								
	2006	2007	2008	2009	2010	2011	2012	2013	**Total**
Sex	%	%	%	%	%	%	%	%	%
Female	46.8	46.6	49.5	48.5	47.8	48.4	46.6	48.8	**47.9**
Male	53.2	53.4	50.5	51.5	52.2	51.6	53.4	51.2	**52.1**
Total	100	100	100	100	100	100	100	100	100
Race	%	%	%	%	%	%	%	%	%
White	75	73.3	66	68.6	67.3	69.2	65.3	66	**68.7**
Black	16.9	16.7	21.6	20.3	20.3	19.7	21.1	20.6	**19.7**
Other	8.1	9.9	12.4	11	12.4	11.2	13.6	13.4	**11.5**
Total	100	100	100	100	100	100	100	100	100
Education	%	%	%	%	%	%	%	%	%
High School	40.7	39.2	39	36.7	36.9	35.9	31.5	30.5	**36.3**
College	45.7	47	46.3	48.9	48	49.6	54.3	55.2	**49.4**
More than College	13.6	13.9	14.7	14.5	15.1	14.5	14.2	14.3	**14.4**
Total	100	100	100	100	100	100	100	100	100
Age (in years)	41.4	41.3	41.1	41.4	41.1	41.2	40.9	40.9	**41.2**
Wage ($Annual)	52,652	51,841	50,386	49,884	50,454	50,417	51,556	50,883	**50,991**
Health Expenses ($Annual)	3,387	3,524	2,968	3,072	3,108	2,944	2,703	2,706	**3,039**
Offered Employer-based Coverage	73.8%	75.1%	73.7%	73.9%	73.9%	72.2%	69.2%	69.7%	**73%**

These summary statistics represent the age 27-55 sub-sample. All dollar amounts were adjusted to 2013 dollars using the CPI (www.bls.gov).

The data used in this paper focuses on interview three of five for Panels 11 through 18 of the MEPS covering from the end of 2006 to the end of 2013. The third interview is the first set of year-end observations for Panel 18, and is the most recent data available that is suitable to test the hypotheses presented in Section 3. As data on health care expenditures are reported as an annual figure, the analysis cannot meaningfully exploit the quasi-panel nature of the data-set. Instead, the data are treated as a repeated cross-section using only the third interview with each panel as an independent repeated cross-section. Further, the empirical analysis focuses on working-age adults (ages 27-55) who report that they work at firms with more than 50 employees.Those under age 26 are excluded as they are affected by the Affordable Care Act in the pre-implementation period via the Act's extension of parental coverage. Those over age 55 are excluded as labor force participation typically falls after this age, potentially confounding the paper's findings. The findings in the paper do not change significantly when re-estimated restricting the sample to those aged 27-59 or 27-64. Summary statistics for the restricted sample, at firms who do and do not provide cover, are presented in Table 1.

Notice in Table 1 that workers at firms who provide coverage tend to be slightly older, have higher wages, are better-educated, are more likely to be white, and have higher annual health expenses. Notice that employer-provided coverage has fallen from covering almost 74% of the sample to under 70% of the respondents over the period. The type of worker at firms with coverage appears to be trending towards younger, male workers in the period from 2011 to 2013 compared to the 2008-2010 period. The period from 2011-2013 also shows lower overall health expenses relative to 2008-2010. A large body of research has explored why males tend to use less health care services than females, finding that mens' usage is lower as they tend to be less diligent about making and keeping doctor appointments, filling prescriptions, have low fertility-related expenses, and live shorter lives (see Mustard et al., 1998 for more on this topic).

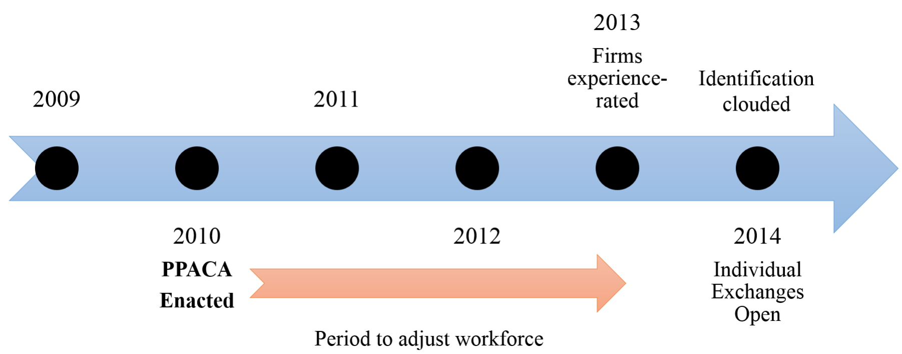
5 ACA Implementation and Identification Strategy

5.1 Implementation

The 2008-2013 waves of the MEPS are ideal for studying the individual-specific effects of employer-provided insurance. When the new health care law was announced in March of 2010, firms were told they would be subject to an employer mandate as of January 1, 2014. The employer mandate required firms with more than 50 full-time employees to have affordable coverage options in place for employees on that date. The original time-line for the implementation of the employer mandate is illustrated in Figure 1. The Act's implementation time-line was altered in February of 2014 when the IRS was instructed not to enforce the mandate until January 2015. As the data in this analysis only covers up to the end of 2013, two months before the decision to delay the implementation, firms should have been behaving as if the mandate would come into effect in January 2014.

Importantly, the cost of coverage to firms in 2014 would be based on the demographic characteristics of the firm's employees in 2013. As part of the underwriting process for employer-based plans, insurance companies collect detailed data on a firm's workforce. The cost of coverage for the firm would be higher if the firm has employees with high expected medical expenses, such as older workers or females who could be expected to have a pregnancy. A firm wishing to minimize its cost of compliance with the ACA would therefore need to begin making adjustments to their workforce *before* 2013.

Figure 1: The Implementation Time-line of the Patient Protection and Affordable Care Act (ACA)



The timeline for the law's implementation illustrates why identification will be clouded by other components of the Act. A researcher seeking to examine the effect of the employer mandate on labor market outcomes using data collected *after* 2014 would have to account for the changes in worker behavior caused by the availability of affordable individual coverage on the Act's exchanges. Failing to account for the effects on individuals would lead to empirical results representing the joint effect of the employer mandate and the individual mandate. Moreover, comparing outcomes from 2014 on-wards to the period immediately before would require an assumption that firms did not prepare for or anticipate the mandate in any way.

Focusing on the pre-implementation period avoids the identification problems that the individual mandate will cause. It also stacks the deck against finding any significant effects in the data as some employers may not be well informed about the law, they may not be sufficiently forward-looking, or they may be considering paying the mandate's financial penalties rather than providing coverage. Employers who fail to react to the Act's announcement are essentially not "treated" by the law yet they are considered treated in the analysis presented in Section 6 biasing results towards zero. Because the Act increased the cost of coverage (see details in Appendix C) at firms who provided coverage even before the law, the control group is also mildly treated. At these firms, the more generous coverage required by the law adds to incentives to avoid hiring high cost workers, again biasing results towards zero. As not all firms can be expected to react to the law by providing coverage and given that firms who already provided coverage faced increased costs, too, the results presented in Section 6 can only be viewed as a *lower bound* on the actual effects of the mandate on individual workers at affected firms.

One concern about comparing the period before 2010 to after is how the 2008-2009 recession impacts the analysis. Difference-in-difference and triple-difference strategies tend to ease these kinds of concerns as the focus is on differences between the labor market outcomes of individuals who work at firms who do and do not provide before and after the law. If the recession affected all firms essentially equally then there are no concerns. However, Siemer (2014) finds reduced employment growth in small relative to large firms. Siemer's findings are relevant because firms that don't offer coverage tend to be smaller. Siemer's estimates suggest small firms have between 4.8 and 10.5% slower employment growth during the recession period. This would bias the paper's estimates toward significance if the reduced growth happened to be biased against healthier workers. If so, it is possible the findings in Section 6 would simply be a product of the effects of the recession. While there is no immediately clear reason a recession should induce smaller firms to reduce their hiring of healthier (perhaps, more productive) workers, this potential source of bias will be addressed as a robustness check using MEPS data from 2006-2010.

5.2 Identification and Estimation

The ACA impacts employees at firms with no existing health care coverage. Examining how this variation changes the co-efficient on individual health expenses at firms that do not offer coverage after the ACA is announced allows inference of a causal relationship between health expenses and labor market outcomes. Ideally, an experiment to test for such a relationship would exogenously vary which jobs an individual applies for across firms who do and do not offer health insurance and then track how wage offers changed in response to health care expenses. In the real world, such an experiment is not feasible. If it were, variation in wage offers could be causally related to individual health expenses without resorting to any higher level of variation such as mandates, firm size, or any form of spatial or temporal variation.

Instead, identification relies on the ACA's employer mandate having a larger impact at firms who do not provide coverage versus those who already do. In the language of the model in Section 3, this means that there are firms who used to be type n but who are now type d. Using this identification strategy provides a causal interpretation of regression estimates which reveal how wages and other employment outcomes change for workers after the ACA as a function of health care expenses. For Hypotheses 1, 2a, and 3 estimation relies on a difference-in-difference approach. Hypothesis 1 examines the "macro" level labor market effects of the Act. The approach examines labor market outcomes for workers with low- and high cost health care before and after the law but does not account for existing insurance coverage options. The estimating equation takes the form;

$$\begin{aligned} LaborMarketOutcome_{it} = \beta_0 + \beta_1 HealthExpenses_{it} + \beta_2 PostACA_{it} \\ + \beta_3 HealthExpense \times PostACA_{it} + \Pi X_{it} + \epsilon_{it} \end{aligned}$$

where $LaborMarketOutcome_{it}$ stands for labor market outcomes of interest for person i at time

t. The dependent variable could be (log) hourly wages, weekly wages, or annual wages. It could also be any other individual labor market outcome which responds to changes in the demand for an individual's labor. The right hand side of the estimating equation considers the main effect of a continuous measure of health expenses $(HealthExpenses_{it})$ and the main effect of the Affordable Care Act $(PostACA_{it})$ (a binary variable taking on the value of 1 after the Act is announced). The co-efficient on the interaction term in the estimating equation gives a measure of the effect of the Act on the labor market outcomes of individuals as a function of their health expenditure in a year. The estimating equation is completed by allowing for a set of demographic controls X_{it} such as age, sex, education, marital status, race, location, and industry.

The model presented earlier predicts labor market outcomes for workers with higher health care costs will worsen after the Act. The model also highlights that the effects will be strongest at firms who move from not providing coverage to providing coverage (Hypothesis 2*b*). To examine Hypothesis 2*b* the estimating equation above is adjusted to add a third difference between those who work at firms who do and do not provide insurance in the sample period. The estimating equation takes the following form

$$\begin{aligned} LaborMarketOutcome_{it} = \beta_0 &+ \beta_1 HealthExpenses_{it} + \beta_2 PostACA_{it} + \\ &+ \beta_3 HealthExpenses \times PostACA_{it} \\ &+ \beta_4 EmployerOffersInsurance_{it} \\ &+ \beta_5 HealthExpenses \times EmployerOffersInsurance_{it} \\ &+ \beta_6 PostACA \times EmployerOffersInsurance_{it} \\ &+ \beta_7 PostACA \times HealthExpenses \times EmployerOffersInsurance_{it} \\ &+ \Pi X_{it} + \epsilon_{it} \end{aligned}$$

where, again, $LaborMarketOutcome_{it}$ stands for labor market outcomes of interest for person i at time t. In addition to a continuous measure of health expenses $(HealthExpenses_{it})$, the main effect of the Affordable Care Act $(PostACA_{it})$ and the interaction between them, the equation adds a binary indicator that is set to 1 if the observed individual works for a firm that offers health insurance $(EmployerOffersInsurance_{it})$.[18] The co-efficient of interest is then β_7, the triple-difference interaction co-efficient. The term represents the effect on labor market outcomes of interest as a function of health expenses and insurance coverage after the Act is announced. If β_7 is positive, it suggests that firms who offer coverage pay higher wages than firms who do not offer coverage to workers with higher health care expenses. In other words, a positive β_7 highlights that the Act *harms* high cost workers (*helps* low cost).

Finally, Hypotheses 3, predicting higher unemployment and longer unemployment duration is tested using a similar estimating equation as Hypothesis 1. For the unemployment rate, the de-

[18] The worker does not have to accept this insurance for this to be equal to 1. Using this as the measure of insurance availability assumes firms cannot predict who will take up coverage when offered.

pendent variable is a binary variable indicating employment status. For duration, the dependent variable is a count of how many (out of three) MEPS interviews the worker reported being unemployed in that year, acting as a measure of how the Act affects the unemployment duration of workers who would be costlier to insure. Using MEPS data and the estimating equations laid out above, estimates for the Act's effects on higher-cost workers are presented in the next section.

6 Empirical Estimates

In the terminology of the model in Section 3 the impact of the Act is analogous to an exogenous increase in type-d firms. Based upon the model's comparative statics, Hypothesis 1 predicted that an increase in type-d firms would decrease wages for high cost type-B workers, regardless of the firm they work at. The prediction can be examined in a difference-in-differences framework (as laid out in Section 5), comparing the earnings of workers before and after the law change with respect to their annual health care expenses. Table 2 reports the results of such an estimation. The estimates do not show any significant *aggregate* effects on labor market outcomes after the announcement of the new health care law. The first three columns consider the log of annual wages, hourly wages, and an indicator for part-time work regressed on demographic controls (co-efficients not reported), binary indicators for employer-based health coverage, a dummy for the post-Act period, the log of health expenses, and the difference-in-differences interaction term which captures how outcomes have changed with respect to health expenses *after* the Act. The final two columns, presented to aid interpretation, regress the level rather than log of annual wages and hourly wage on the same set of independent variables and controls.[19]

In the first column of Table 2, the difference-in-difference interaction term indicates that the log of annual wages decreased for higher health care cost workers but not significantly. In the specification in column 2 the sign of the estimated co-efficient on how hourly wages change with respect to health care expenses is the opposite sign. The estimates in columns 4 and 5 show the effect on the level of annual and hourly wages rather than log values. While hours worked were not incorporated in the model presented in Section 3, the Affordable Care Act only requires firms to offer coverage to workers who are full time ($\geq$ 30 hours per week). Because of this, the interaction term in column 3 reports estimates where the dependent variable is an indicator for part-time work (<30 hours per week). Again, there seems to be no significant change associated with individual health care costs after the law. Together, the difference-in-difference estimates in Table 2 suggest workers who have higher health care expenses may be no worse off after the Affordable Care Act is announced.

The no-effect finding complements Mathur et al.'s and Garrett and Kaestner's work on the Act's labor market effects. However, the number of firms significantly affected by the law is quite small. These firms already provide coverage and have little incentive to change their hiring practices due to the ACA. As can be seen in the table of summary statistics in Section 4, somewhere

[19] All dollar values in all estimations presented in the paper have been converted to 2013 dollars using the CPI.

between 70% and 75% of workers in the sample had insurance coverage from their employer each year of the sample. As the number of workers who work at firms who are affected by the Act is small, it is not surprising that changes which impact a small sub-group within a small sample do not cause *observable* changes in aggregate labor market outcomes.

Table 2: Difference-in-Differences estimation of the ACA's Aggregate Labor Market Effects

	(1) Log Wages	(2) Log Hourly Wage	(3) Part Time <30 Hours	(4) $ Wages	(5) $ Hourly Wage
Offers Coverage	0.807***	0.419***	-1.454***	22,081***	7.082***
	(0.0219)	(0.0133)	(0.0470)	(680.6)	(0.279)
After ACA	0.0189	0.0167	0.0393	1,117	-0.717*
	(0.0251)	(0.0179)	(0.0856)	(1,079)	(0.434)
Health Expenses	0.00424	0.00369*	0.0562***	292.7**	0.0800
	(0.00292)	(0.00210)	(0.00996)	(129.3)	(0.0533)
After ACA x Health Expenses	-0.00375	0.00266	-0.0135	-125.6	0.0201
	(0.00389)	(0.00278)	(0.0128)	(176.6)	(0.0696)
Observations	12,031	12,031	12,031	12,031	12,031
Race	Y	Y	Y	Y	Y
Education	Y	Y	Y	Y	Y
Marital Status	Y	Y	Y	Y	Y
Age	Y	Y	Y	Y	Y
Region	Y	Y	Y	Y	Y

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Columns 1 and 2 show estimates from a specification where the dependent variables are in log form. Columns 4 and 5 present the same estimations as columns 1 and 2 but the labor market outcomes of interest are not log-transformed. The co-efficients then represent the effect of a unit change in log health expenses in dollar terms. Column 3 provides estimates from a probit estimation where the dependent variable is a binary indicator for part-time work (<30 hours).

Examining the same labor market outcomes while making use of variation in coverage provision at the firm level tells a different story. Recall Hypothesis 2*a* predicted that firms who are forced to provide coverage would employ fewer type-*B* workers. Similarly, Hypothesis 2*b* predicted a fall in wages for any type-*B* worker who worked at a firm forced to provide coverage by the Act. Estimates, focusing on the workers at affected firms are reported in Tables 3 (using a triple-difference estimation) and 4 (using only a difference-in-difference approach). As a brief reminder, the first difference is between workers who have high and low health care expenses. The second difference is between the pre- and post-Act periods. The third difference, when exploited, is between firms who do and do not provide health coverage. The estimations in Tables 3 and 4 are only possible because workers report employer characteristics such as options for health coverage in the MEPS data. As a result, it is possible to determine which workers would be expected to be most affected by examining their employer's pre-existing health care provision choices.

Focusing on changes in wages and hours worked, estimates from the triple-difference estimation are presented in Table 3.[20] Columns 1 and 2 show estimates from a specification where the

[20]The Appendix presents the two difference-in-difference estimations (stratified by insurance coverage) which un-

labor market outcomes of interest are in log form. Columns 4 and 5 present the same estimations as columns 1 and 2 but the outcomes of interest are not log-transformed, so that the co-efficients represent the effect in absolute ($2013) terms. Column 3 provides estimates from a probit estimation where the dependent variable is a binary indicator for part-time work (<30 hours).

Table 3: **Main Results:** Triple-difference estimation of the ACA's Individual-Specific Effects on Wages and Hours Worked

	(1) Log Wages	(2) Log Hourly Wage	(3) Part Time <30 Hours	(4) $ Wages	(5) $ Hourly Wage
After ACA	0.0816	0.0707**	0.0832	2,248	0.525
	(0.0559)	(0.0357)	(0.112)	(1,453)	(0.662)
Offers Coverage x After ACA	-0.0895	-0.0742*	-0.0570	-1,624	-1.720**
	(0.0623)	(0.0412)	(0.160)	(1,989)	(0.849)
Health Expenses	-0.0126	0.00137	0.105***	-65.80	-0.0243
	(0.00853)	(0.00515)	(0.0145)	(239.9)	(0.0960)
Offers Coverage x Health Expenses	0.0211**	0.00319	-0.0885***	458.5	0.128
	(0.00902)	(0.00562)	(0.0188)	(282.2)	(0.114)
After ACA x Health Expenses	-0.0279**	-0.0168**	-0.0339*	-756.6**	-0.311**
	(0.0116)	(0.00678)	(0.0192)	(313.8)	(0.128)
Coverage x ACA x Health Expenses	0.0297**	0.0236***	0.0271	756.0**	0.415***
	(0.0123)	(0.00747)	(0.0256)	(378.9)	(0.153)
Observations	12,031	12,031	12,031	12,031	12,031
Race	Y	Y	Y	Y	Y
Education	Y	Y	Y	Y	Y
Marital Status	Y	Y	Y	Y	Y
Age	Y	Y	Y	Y	Y
Region	Y	Y	Y	Y	Y

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Columns 1 and 2 show estimates from a specification where the dependent variables are in log form. Columns 4 and 5 present the same estimations as columns 1 and 2 but the labor market outcomes of interest are not log-transformed. The co-efficients then represent the effect of a unit change in log health expenses in dollar terms. Column 3 provides estimates from a probit estimation where the dependent variable is a binary indicator for part-time work (<30 hours). The distinction between the estimations presented in Table 2 and 3 is the additional "difference" between firms who do and do not provide health coverage. The triple-difference interaction term shows the difference in the labor market outcome as a function of health expenses after the ACA at firms who already provide health coverage.

The estimates presented in Table 3 are the main empirical findings of this paper. They highlight how labor market outcomes for employees at firms who would have to *begin* providing coverage are impacted by the ACA compared to employees at firms who already provide coverage. As these estimates have used the clean identification strategy afforded by the law's implementation they provide strong evidence that the incidence of employer-provided health coverage can be *individual*-specific rather than merely *group*-specific. The co-efficient of interest in each specification is the triple-difference interaction term. A positive co-efficient indicates that, after the ACA, higher-cost employees at firms who provide coverage earn significantly higher wages than employees at firms who do not provide insurance coverage. In column 1, the estimate of .0297 implies that for

derpin Table 3. These estimations show how the *overall* effect of the Act is allocated among each type of firm.

a positive unit difference in the log of health expenses annual wages are higher by 2.97% at firms who already provided coverage before the ACA.[21] In column 4, as wages are measured in dollars a positive unit difference in the log of health expenses is associated with $756 lower wages due to the ACA. Essentially, firms compensate individuals with lower use of health services relatively more than workers who use more health services. The effect is statistically significant in columns 1 and 4 at the 5% level. Columns 2 and 5 present the analogous result for hourly wages, with a unit difference in health expenses corresponding to a 2.36% or $0.42 per hour lower wage, both significant at the 1% level.

In column 3, the triple-difference co-efficient suggests there is an increase in the likelihood of part-time work for a unit increase in the log of health expenses at firms who already provided coverage before the law.[22] However, the finding is not statistically significant from zero. The estimates in column 3 of Table 3 appear to again confirm the findings of Mathur et al. and Garrett and Kaestner using CPS data. The difference here is that the estimation zeroes in on higher-cost versus lower-cost workers. The CPS data used by Mathur et al. and Garrett and Kaestner cannot zero in on workers with varying expenses, nor can it stratify by employer-based insurance coverage. The estimates from Table 2 and 3 show no sizable effect part time employment for high cost workers, indicating that Mathur et al. and Garrett and Kaestner's work was not accidentally masking a large effect on a small sub-group.

The estimates in Table 3 are also robust to controlling for industry and occupation. They are also unchanged when the mix of controls is altered or allowed to vary after the ACA. That is, repeating the estimation interacting all control variables (Age, Gender, Marital Status, Region, Education, Location, and Industry) with the post-ACA period dummy does not change the size or significance of the effect (see Appendix A for Tables and more details). The robustness of results suggest the effect seen is not concentrated within a certain type of industry or region after the ACA, nor are employers simply using heuristics such as age, race, or gender to implement the change in relative wages seen in Table 3. Instead, it appears firms forced to provide coverage due to the ACA are tailoring compensation to individual health and health expenses in a way that they did not before the ACA.

As reducing hours is just one way to exclude workers from coverage, the estimates in column 3 of Table 3 should be viewed in the context provided by Table 4. In Table 4, the first column presents the difference-in-differences estimates from a probit regression examining employment as a function of health expenses (in log terms) before and after the Act. The dependent variable and outcome of interest is whether a worker is offered employer-based health insurance at their current job. First, the estimates suggest that *all* workers are less likely to have coverage from their job after the ACA. That finding echoes the broad reduction in coverage first seen in the summary

[21] A unit difference in the log of a variable is approximately equal to a 100% difference in annual expenses such as $2,000 versus $4,000 per year in health care expenses.

[22] These estimates are *raw* probit estimates rather than marginal effects. As the estimation considers many categorical variables (race, education, etc.) marginal effects would be complex to produce. As the estimate is not significant, there is little value in the exercise.

statistics presented in Table 1 over the period of the sample. In addition, the co-efficient on the interaction term suggests that higher-cost workers are *more* likely to have a job where employer-based health insurance was already offered after the ACA. In other words, lower cost workers are more likely to work at a firm that does not provide coverage in the period from 2011-2013 compared to 2008-2010. The estimates are not statistically significant but the sign of the effect is as predicted. Column 2 in Table 4 presents the same estimation but uses "z-scores" as a relative measure of health expenses. The co-efficient on the interaction term can then be interpreted as the change in the cumulative density due a one standard deviation difference in health expenses.

Table 4: Difference-in-Differences Probit Estimation of the ACA's Individual-Specific Effects on Employment

	(1) Probit (Offered Coverage)	(2) Probit (Offered Coverage)
ACA	-0.147***	-0.109***
	(0.0537)	(0.0306)
Total Health Expenses (Log)	0.0638***	
	(0.00677)	
ACA x Total Health Expenses (Log)	0.0114	
	(0.00901)	
Total Health Expenses (Z-Score)		0.000183
		(0.0269)
ACA x Total Health Expenses (Z-Score)		0.0974
		(0.0595)
Observations	12,031	12,031
Race	Y	Y
Education	Y	Y
Marital Status	Y	Y
Age	Y	Y
Region	Y	Y

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Column 1 reports the estimates from a probit specification where the dependent variable equals one if the surveyed individual is working at a firm who offers coverage. Column 2 represents the same estimation using a standardized (z-score) measure of health expenses to aid interpretation. The co-efficients on the difference-in-difference term in both specifications shows that the higher an individuals health expenses - after the ACA - the more likely they are to work at a firm who already offers coverage. In other words, the firms most affected by the ACA (those who do not already provide coverage) appear to pivot away from high cost workers.

Hypothesis 3 focuses on the unemployment level and duration effects of the Act. Column 1 of Table 5 estimates a probit model on the binary outcome employed or unemployed. The coefficient on the difference-in-difference interaction term, the effect of higher health expenses after the Act is positive, indicating increased likelihood of unemployment. The effect is statistically significant at the 5% level. Column 2 examines a *qualitative* measure of unemployment duration. The MEPS does not ask respondents how long they have been unemployed so the dependent variable is a simple count of the number of interviews the respondent reported that they were unemployed. Again, the difference-in-difference coefficient is positive. As all of the analysis in this paper focuses on the year-end interview for each panel in their first year in the MEPS (the third interview of five), the number can be zero, one, two, or three. The estimate, significant at the 5% level, suggests that the duration of unemployment after the Act for high cost workers is higher.

Table 5: Difference-in-Differences estimation of the Affordable Care Act's Unemployment Level and Duration Effects

	(1) Unemployed	(2) Duration
ACA	-0.184***	-0.283***
	(0.0456)	(0.0748)
Total Health Expenses (Log)	0.00413	-0.00273
	(0.00521)	(0.00862)
ACA x Total Health Expenses (Log)	0.0143**	0.0246**
	(0.00720)	(0.0119)
Observations	17,225	17,066
Race	Y	Y
Education	Y	Y
Marital Status	Y	Y
Age	Y	Y
Region	Y	Y

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Column 1 reports the estimates from a probit specification where the dependent variable equals one if the surveyed individual is unemployed. Column 2 represents a logit estimation using the count of the number of times a respondent reported being unemployed. The co-efficients on the difference-in-difference term in both specifications shows that the higher an individuals health expenses - after the ACA - the more likely they are to be unemployed and for a longer duration (both significant at the 5% level). Notice the sample size is larger than in Tables 2, 3, and 4 as it is not limited only to those who report they are currently working.

Table 5 suggests workers who would likely be more expensive to cover are *less* likely to be employed after the ACA's announcement and spend longer periods unemployed than their low cost colleagues.

Gathering results, Table 2 suggests there are no overall effects on wages or hours worked that can be associated with higher health care costs after the ACA. However, Table 5 suggests high cost workers are less likely to hold or get a job after the ACA. Focusing on the differences in labor market outcomes at firms that do and do not provide coverage Table 3 shows that higher-cost workers who do have a job will face lower wages at firms most affected by the law. It also suggests they are more likely (but not significantly statistically so) to work part-time at firms who already provided coverage. Lastly, Table 4 suggests firms that did not provide coverage before the ACA appear to favor lower-cost workers in the 2011-2013 period but the effect is not significantly different from zero. Together, the estimates are broadly consistent with the predictions of the job search model presented in Section 3 but are not always statistically significant. However, the consistency in the sign and direction of effects provide evidence that firms can and do condition wages and employment on the actual health care expenses of individual workers.

6.1 Robustness Checks

As Hypothesis 1 was not supported well by the data, robustness checks will focus on Hypotheses 2a, 2b, and 3 - the empirical tests of which are presented in Tables 3, 4, and 5. The first robustness check focuses on the potentially-confounding differences between firms who do and do not provide coverage.

6.1.1 Firm Size

Table 6: Triple-difference Estimation of the ACA's Individual-Specific Effects on Wages and Hours Worked (Restricted to Firms with <300 employees)

	(1) Log Wages	(2) Log Hourly Wage	(3) Part Time <30 Hours	(4) $ Wages	(5) $ Hourly Wage
After ACA	0.0964	0.0722	0.0364	3,281	0.897
	(0.0818)	(0.0513)	(0.164)	(2,194)	(0.776)
Offers Coverage x After ACA	-0.0970	-0.0609	0.229	-1,988	-1.589
	(0.0901)	(0.0578)	(0.223)	(2,827)	(1.023)
Health Expenses	0.00166	0.00798	0.113***	340.7	0.0885
	(0.0114)	(0.00706)	(0.0204)	(320.4)	(0.101)
Offers Coverage x Health Expenses	0.00555	-0.00405	-0.0999***	-14.32	0.00143
	(0.0121)	(0.00761)	(0.0256)	(367.7)	(0.125)
After ACA x Health Expenses	-0.0386**	-0.0147	-0.0459*	-985.6**	-0.238
	(0.0153)	(0.00938)	(0.0267)	(428.7)	(0.146)
Coverage x ACA x Health Expenses	0.0399**	0.0201**	0.0186	908.6*	0.304*
	(0.0163)	(0.0102)	(0.0349)	(510.3)	(0.179)
Observations	6,660	6,660	6,660	6,660	6,660
Race	Y	Y	Y	Y	Y
Education	Y	Y	Y	Y	Y
Marital Status	Y	Y	Y	Y	Y
Age	Y	Y	Y	Y	Y
Region	Y	Y	Y	Y	Y

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Columns 1 and 2 show estimates from a specification where the dependent variables are in log form. Columns 4 and 5 present the same estimations as columns 1 and 2 but the labor market outcomes of interest are not log-transformed. The co-efficients then represent the effect of a unit change in log health expenses in dollar terms. Column 3 provides estimates from a probit estimation where the dependent variable is a binary indicator for part-time work (<30 hours). The distinction between the estimations presented in Table 6 versus Table 3 is only the "small firm" sample restriction that is imposed. Again, the triple-difference interaction term shows the difference in the labor market outcome as a function of health expenses after the ACA at firms who already provide health coverage.

The most striking difference between firms that do and do not offer insurance is *size*. Virtually all firms with 300 employees or more offer health coverage to their employees. What that means is that the estimates in Tables 3 and 4 compare small firms who do not offer insurance to small *and* large firms who do. As a result, the "control" group in the natural experiment set-up of this paper is potentially invalid. Table 6 limits the sample to employees of firms who have fewer than 300 workers. The estimates are produced using the triple-difference estimation strategy detailed in Section 5.

The findings in Table 6 should be compared to those in Table 3. Considering columns 1 and 4, the effect of the Act when using small firms only as a control group is larger in size than the effects seen in Table 3. Focusing on the co-efficients on the triple-difference interaction term suggests a log unit difference in health expenses is associated with a 3.99% difference in annual wages between firms who do and do not provide coverage after the Act is announced. The second column focuses on hourly wages finding a 2.01% fall in hourly wages. The dollar value interpretation in columns 4 and 5 amounts to $908.60 per year or about $0.30 per hour. Again, this is a large pass-through of expenses given employee health expenses are deductible at the marginal corporate tax rate. Column 3 reports probit estimates where the dependent variable is again an indicator that equals one if an individual reports working fewer than 30 hours per week. The triple-difference estimate shows firms that offer coverage are more likely to hire higher-cost workers for part-time positions but the effect is not statistically significant.

Table 7 repeats the analysis of Table 4 but uses the <300-employees sub-sample. Table 4 focused on the likelihood of being employed at a firm that offers coverage as a function of health expenses. Column 1 keeps health care expenses in log form while column 2 uses a standardized (i.e., a z-score) measure. Using the smaller, but potentially more valid sample, the estimates become statistically significant. That means, after the Affordable Care Act is announced, individuals with higher health care expenses are more likely to work at a firm with health coverage. In turn, this suggests higher cost workers are being excluded from the firms *most*-affected by the Act. Each of the estimations controls for demographics and the "main" effect of having health coverage at all (a predictably reliable determinant of health care expenses).

Table 7: Difference-in-Differences Estimation of Effects on Probability of Employment at Firms Who Offer Coverage (Restricted to Firms with <300 Employees)

	(1) Probit(Offered Coverage)	(2) Probit(Offered Coverage)
ACA	-0.204***	-0.101**
	(0.0754)	(0.0397)
Total Health Expenses (Log)	0.0392***	
	(0.00921)	
ACA x Total Health Expenses (Log)	0.0239**	
	(0.0122)	
Total Health Expenses (Z-Score)		-0.0481**
		(0.0211)
ACA x Total Health Expenses (Z-Score)		0.0895*
		(0.0493)
Observations	6,660	6,660
Race	Y	Y
Education	Y	Y
Marital Status	Y	Y
Age	Y	Y
Region	Y	Y

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Column 1 reports the estimates from a probit specification where the dependent variable equals one if the surveyed individual is working at a firm who offers coverage. Column 2 represents the same estimation using a standardized (z-score) measure of health expenses to aid interpretation. The co-efficients on the difference-in-difference term in both specifications shows that the higher an individuals health expenses - after the ACA - the more likely they are to work at a firm who already offers coverage. In other words, the firms most affected by the ACA (those who do not already provide coverage) appear to pivot away from high cost workers. The difference between Table 7 and Table 4 is only the "small firm" sample restriction imposed.

Table 6 and 7 complement the findings presented in Tables 3 and 4. The sign and magnitude of the estimates are consistent with theory regardless of the sample restriction imposed. As a result, there is little reason to be concerned that the findings in Tables 3 and 4 are biased away from zero only by an inappropriately-selected comparison group.

6.1.2 Sensitivity to 50-employee Rule

In all of the estimates presented so far, those who report they work at firms with 50 employees or less are excluded as firms with fewer than 50 employees were not mandated to provide coverage by the ACA. Repeating the estimation presented in Table 3 and 6 but restricting the sample to firms with under 50 employees provides an additional robustness check on the estimates presented in earlier sections. While Tables 3 and 6 suggest firms who do not offer coverage react to the ACA by reducing the wages of higher-cost workers, firms with fewer than 50 employees are not affected at all by the mandate. That means firms who do not provide coverage but have fewer than 50 employees should not react to the new health care law at all. Table 8 presents the estimates from an empirical test of that prediction. The estimation is again a triple-difference where the first difference is a continuous measure of health care costs, the second is before and after the ACA's announcement, and the final difference is between firms who do and do not provide coverage. As the firms who do not provide coverage are not forced to do so by the ACA, there should be essentially no change in how they react to higher versus lower-cost workers relative to before the law and relative to firms who do provide coverage.

In Table 8, the statistically insignificant estimates on the triple-difference interaction term suggest there are no differences between the wage and hiring patterns of very small (<50 employees) firms who do and do not provide coverage in the years following the ACA's announcement. Indeed, the co-efficients on the interaction term in columns 2 and 3 are the opposite sign compared to the corresponding estimates in Tables 3 and 6. As the only difference here is that the estimates are based upon a sample of workers who work at firms with fewer than 50 employees then it follows that the estimates in Tables 3 and 6 cannot simply explained by underlying differences between firms who do and do not provide coverage regardless of size. The differences only appear if the ACA affects the firms who do not provide coverage. If the ACA was not responsible for the estimates seen in Tables 3 and 6, a similar pattern should have been observed in Table 8. As it is not, this robustness check is supporting evidence that the ACA *caused* a deterioration of labor market outcomes for higher-cost workers at firms affected by the Act's mandate.

Table 8: Triple-Difference Estimate of Sensitivity to 50-Employee Mandate Cut-off

	(1) Log Wages	(2) Log Hourly Wage	(3) Part Time <30 Hours
Offers Coverage	0.402***	0.283***	-0.783***
	(0.0361)	(0.0269)	(0.111)
After ACA	-0.0335	0.00887	0.00928
	(0.0319)	(0.0226)	(0.0713)
Offers Coverage x After ACA	0.0540	0.0506	0.0860
	(0.0473)	(0.0353)	(0.145)
Health Expenses	-0.00456	-0.00248	0.0714***
	(0.00491)	(0.00335)	(0.00937)
Offers Coverage x Health Expenses	0.0181***	0.0130***	-0.0751***
	(0.00632)	(0.00452)	(0.0173)
After ACA x Health Expenses	0.000312	0.00583	-0.00265
	(0.00657)	(0.00434)	(0.0125)
Offers Coverage x ACA x Health Expenses	0.00249	-0.00489	-0.0158
	(0.00850)	(0.00602)	(0.0233)
Observations	10,260	10,260	10,260
Race	Y	Y	Y
Education	Y	Y	Y
Marital Status	Y	Y	Y
Age	Y	Y	Y
Region	Y	Y	Y

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Columns 1 and 2 show estimates from a specification where the dependent variables are in log form. Column 3 again provides estimates from a probit estimation where the dependent variable is a binary indicator for part-time work (<30 hours). The distinction between the estimations presented in Table 8 versus Table 6 and 3 is that this estimation is based upon only workers who work at firms that have fewer than 50 employees. These firms do not have to provide coverage under the ACA's mandate and should not be affected. Again, the triple-difference interaction term shows the difference in the labor market outcome as a function of health expenses after the ACA at firms who already provide health coverage. As predicted, the estimates suggest there are no significant differences in the wage and hiring patterns of these firms after the ACA.

6.1.3 Sensitivity to Treatment Date

The estimates presented in this paper use data *after* 2010 as the post-treatment period. Considering 2010 MEPS data as "before" the Act assumes contracts or wage increases were already in place for 2010 before the Act was announced. Considering 2010 as part of the "after"-treatment period would mimic a 2009 announcement date. Such a placebo exercise eliminates the statistical significance of the estimates presented in Table 3 indicating that the effects observed very much depend upon the period *after* but not including 2010. The estimates are not presented here to economize on space. Underlining the importance of the changes that happened in 2010, dropping 2010 data from the analysis altogether - essentially comparing 2008/2009 to 2011-2013 - increases the economic and statistical significance of the main results. Again, these estimates are omitted to conserve space.[23]

6.1.4 Composition Bias and Propensity Score Matching

The data collected by the MEPS forms a *panel* data-set but is transformed into a repeated cross-section in this paper by discarding all but one of the five interviews with survey respondents. Many variables, including medical expenses, are reported only on an annual basis. Additionally, exploiting the fact that there are two year-end surveys for each Panel is not feasible, at least not yet. This is because only one year-end survey is available for Panel 18 as of writing this paper. Any panel-data approach would need to ignore Panel 18 altogether which amounts to discarding about 25% of the available post-ACA data. As the paper's identification relies on the period after 2010, discarding this data is not practical.[24] Moreover, the panel nature of the underlying data in the MEPS could only be fully exploited by focusing on those who switch jobs in the sample. Two immediate econometric issues arise. One, similarly to Levy and Feldman, the decision to move jobs is not random. Two, even if the move were random, so few move - particularly between jobs that do and do not provide coverage - that identification would rely on just a small fraction of the already relatively-small MEPS data-set.

While treating the data as a repeated cross-section does not invalidate the difference-in-difference estimation strategy it introduces composition bias concerns. The estimates for the difference-in-difference co-efficients presented in the earlier tables are not the average change in labor market outcomes for employees at "treated" firms but instead reflect labor market outcomes for employees who happen to *still* work at "treated" firms *after* the ACA is announced. While economic theory would suggest that any composition effects introduced by the law itself would bias estimates towards zero, the MEPS could also simply have observed more high cost, low-wage workers *by chance* after 2010.[25] These composition bias issues and their consequences in a difference-in-difference framework are described in detail by Lee and Kang (2006).

A propensity-score matching exercise can help ease both types of bias concerns. The matching uses observed characteristics to "pair" high and low health care cost individuals who are similar on dimensions such as race, education, age, marital status, insurance coverage, and location. The match then examines how wages for matched pairs differ at firms that do and do not provide coverage before and after the ACA was announced. The estimates from this exercise should help reduce the potential that the results observed earlier are only because the types of workers observed have changed after 2010, either as a result of the ACA or *by chance*. If the ACA *caused* the effects from earlier in the paper, then the matching exercise should report broadly similar effects

[23] Available on request.

[24] The availability of interview three but not five for the most recent MEPS Panel is due to the overlapping-cohort design of the survey.

[25] The ACA appears to reduce the likelihood of a high cost worker being observed working at an affected firm. For the bias introduced by this "attrition" to impact estimates of wages away from zero the unobserved workers (due to attrition caused by the Act) would have to have been positively selected. That is, they would have to be a set of very high-wage individuals to counteract the relative reduction in wages seen in the data.

with affected firms paying higher cost workers less, relative to low cost workers after the Act is announced. Table 9 presents the estimates from the propensity score matching exercise.

Table 9: Average Treatment Effects Using Propensity Score Matching

Period	Firm	Treatment Effect	High-Cost	Low-Cost
Pre-ACA	No Coverage	$602	393	246
	Coverage	$2,345.68	2,629	2,050
Post-ACA	No Coverage	-$3,070.53	514	334
	Coverage	$3,701.12	2,609	2,176

The "Treatment Effect" is the estimated difference in wages between matched high and low health care cost workers in the time period of interest - pre- or post-ACA - at firms who do and do not provide coverage. The first estimate in the table of $602 indicates that high health care cost workers earned $602 *more* per year that low health care cost workers before the ACA at firms that do not provide coverage. After the ACA this estimate reverses sign to show high health care cost workers earn $3,070.53 *less* on average than their lower-cost co-workers.

In the table, the "Treatment Effect" is the estimated difference in wages between *matched* high and low health care cost workers in the two time periods of interest - pre- and post-ACA - at firms who do and do not provide coverage. The matching procedure divides the sample into high and low health care cost employees based on median health care expenses by firm insurance coverage and year.[26] The procedure then matches workers at firms that do and do not offer coverage in each period on fixed observable characteristics (race, education, marital status, age, region) in order to compare "apples to apples." The estimates in Table 9 are based upon nearest neighbor matching. Estimates using alternative matching methods are qualitatively similar.[27]

The first row in the table suggests that high health care cost workers earned $602 *more* per year than low health care cost workers before the ACA at firms that do not provide coverage. After the ACA the estimate reverses sign. The matching exercise suggests high health care cost workers now earn $3,070.53 *less* than their lower-cost co-workers. In stark contrast, the difference between high and low cost workers at firms that do offer coverage changes in the opposite direction. The $3,673 difference between the two estimates is remarkably close to the difference in annual health expenses between those who report excellent health versus poor health presented in Table 13 below. The estimates from the matching exercise show that after the ACA firms that did not offer coverage paid lower-cost workers relatively more than before the ACA. The results of the exercise limits concerns that the earlier difference-in-difference findings were due to causal or random changes in the composition of the MEPS sample across years.

6.1.5 The Impact of the Great Recession

Additionally, there could be a concern with how the financial crisis and Great Recession between 2007 and 2009 impacted the labor market. The potential confounding issue would be that

[26] A worker is considered high cost if they are above the median expense in the year conditional on whether or not the firm offers coverage.

[27] Available upon request.

the recession impacted high and low cost employees at firms who offer and do not offer coverage differently. To address this concern, data from the four years before the Act's announcement can be used to examine how the recession affected labor market outcomes as a function of health expenses at both types of firms. The robustness check is focused on determining that the pre-Act period represents valid "pre-treatment" observations. The empirical approach relies on the same triple-difference estimation used to produce the estimates seen in Tables 3, 6, and 8. It again compares labor market outcomes of higher and lower-cost workers at firms who do and do not offer coverage before and after some key event. In this case, the recession period is that event. The estimates are presented in Table 10 below. They show essentially no differential effects of the Great Recession on the labor market outcomes of higher-cost workers at firms who do and do not offer coverage. The three years 2006, 2007, and 2008 are compared to 2009 and 2010. The results are little changed if 2008 is dropped or considered as after the Great Recession.

Columns 1 and 2 in Table 10 reflect estimates from a specification where the dependent variables are logged. Column 3 provides estimates from a probit estimation where the dependent variable is a binary indicator for part-time work (<30 hours). Again, the triple-difference interaction term shows the difference in a labor market outcome of interest as a function of health expenses at firms who already provide health coverage after the event of interest (the recession). The estimates suggest there is no difference between firms who do and do not provide coverage in the period before versus after the Great Recession. In particular, the triple-difference estimate in column 1 suggests annual wages decreased slightly for higher-cost workers at firms that offer coverage relative to those that do not offer coverage during the recession period. However, column 2 suggests hourly wages, as a function of health expenses, appear to have gone up slightly. Neither estimate is statistically significant. In column 3, the small and statistically insignificant positive coefficient suggests little change in the likelihood of higher-cost workers obtaining part-time work *because* of the recession. The estimates in Table 10 should be contrasted to the significant effects observed in Table 3 and again in Table 6. Table 3 and 6 strongly suggest *something* affected the labor market outcomes of higher-cost workers at firms affected most by the ACA after 2010 compared to before 2010. Table 10 eases concerns that the *something* in question is the Great Recession.[28]

[28] Even though the period *before* the Act appears "okay" - in the sense that firms do not react to the recession by treating workers with varying costs of coverage differently - the 2011/2012 post-recession recovery period may be problematic. The concern would be that firms who did not offer coverage randomly laid-off workers but hired the highest productivity (potentially correlated with health expenses) workers *first* in the 2011-2012 period. To the extent that firms would prefer to *keep* rather than lay-off higher-productivity workers (during the recession) this concern is minimized.

Table 10: Difference-in-Differences Estimation of the Effects of the Great Recession on Wages and Hours Worked as a Function of Expenses and Health Benefits

	(1) Log Wages	(2) Log Hourly Wage	(3) Part Time <30 Hours
After ACA	-0.0551	0.0958**	0.0862
	(0.0710)	(0.0466)	(0.155)
Offers Coverage x After ACA	0.0674	-0.0258	-0.150
	(0.0793)	(0.0546)	(0.216)
Health Expenses	-0.0146	0.00554	0.100***
	(0.0107)	(0.00630)	(0.0186)
Offers Coverage x Health Expenses	0.0290**	0.00716	-0.0898***
	(0.0113)	(0.00703)	(0.0242)
After ACA x Health Expenses	0.00806	-0.00958	0.00187
	(0.0147)	(0.00870)	(0.0254)
Offers Coverage x ACA x Health Expenses	-0.0151	0.00229	0.00782
	(0.0155)	(0.00964)	(0.0333)
Observations	7,892	7,892	7,892
Race	Y	Y	Y
Education	Y	Y	Y
Marital Status	Y	Y	Y
Age	Y	Y	Y
Region	Y	Y	Y

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Columns 1 and 2 show estimates from a specification where the dependent variables are in log form. Column 3 again provides estimates from a probit estimation where the dependent variable is a binary indicator for part-time work (<30 hours). The distinction between the estimations presented in Table 10 is that this estimation is based upon the period from 2006 to 2010 using the end of 2008 as the cut-off for before/after the Great Recession. Again, the triple-difference interaction term shows the difference in a labor market outcome of interest as a function of health expenses at firms who already provide health coverage after the event of interest (the recession). The estimates suggest there are no differences in the wage and hiring patterns of firms who do and do not provide coverage in the period before and after the recession.

6.1.6 Geographic Variation in Health Care Costs

Sheiner (1999) and Jensen and Morrisey (2001) use spatial variation in health care costs to show that older workers receive lower wages in higher health care cost areas. While the MEPS data provide the "region" respondents live in (Northeast, Midwest, South, or West), two workers with similar health care usage in rural Pennsylvania and Washington DC may have very different annual medical expenses. However, the MEPS data considers both as being in the Northeast meaning that controlling for region may not adequately capture spatial variation in health care costs. The type of variation that would confound the findings of the paper would require health care costs to rise faster than wages in some areas within a region and not others. Then, results might be picking up a mechanical association between health expenses and relatively lower wages. However, a mechanical effect of rising relative health care costs should be observed regardless of whether or not a firm offers coverage. As this paper uses difference-in-difference techniques, this source of confounding variation can only be an issue if the MEPS *by chance* sampled relatively more workers at firms who did not provide coverage who also happen to live in areas with a rising health care cost to wage ratio *after* the ACA was announced. Moreover, Table 8 highlights that there were no effects observed at firms with fewer than 50 employees. Even though these firms are not subject to the ACA's mandates, if there is a mechanical explanation for the effects observed at no-coverage firms with more than 50 workers, we would expect it to be observed at firms of all sizes regardless of the ACA's employer mandate.

6.2 Other Measures of Healthfulness and Future Health care Costs

This paper assumes past medical expenditures are a reliable predictor of future health care expenses. While research across academic fields has shown that current health care expenses have been a good predictor of future costs (see Bertsimas et al., 2008 for example) health economists often consider alternative measures of health available in various data sets. As alternatives to medical expenditures, the MEPS provides information on a series of chronic health conditions along with self-reported measures of health. These are potentially much more salient indicators of health to an employer. Leveraging the information provided by self-reported health measures and chronic conditions provides a secondary check on how employers are treating employees who they expect to be more costly to insure.

6.2.1 Chronic Health Conditions and Self-Reported Health

The priority condition enumeration section of the MEPS contains a series of "yes/no" questions on whether the person has ever been diagnosed as having each of several specific conditions that are considered to be chronic in nature. These conditions include high blood pressure, heart disease, stroke, emphysema, chronic bronchitis, high cholesterol, cancer, diabetes, joint pain, arthritis, asthma, and common attention deficit disorders.[29] Using the presence of a chronic disease as a crude alternate estimate of health provides a check on the results presented earlier.

[29]See Machlin et al. (2010) at http://meps.ahrq.gov/mepsweb/survey_comp/MEPS_condition_data.pdf for information on why these conditions are deemed priority.

Table 11: Triple-difference Estimates using Chronic Health Conditions as the Measure of Health

	(1) Log Wages	(2) Log Hourly Wage	(3) Part Time <30 Hours
Offers Coverage	0.773***	0.417***	-1.324***
	(0.0418)	(0.0251)	(0.0835)
After ACA	0.0464	0.0439	-0.0484
	(0.0508)	(0.0297)	(0.0834)
Offers Coverage x After ACA	-0.0449	-0.0160	0.00578
	(0.0534)	(0.0325)	(0.109)
Chronic Condition Reported	-0.0767	0.00348	0.254***
	(0.0612)	(0.0354)	(0.0961)
Offers Coverage x Chronic Condition	0.0581	-0.0168	-0.256**
	(0.0635)	(0.0377)	(0.119)
After ACA x Chronic Condition	-0.184**	-0.103**	-0.159
	(0.0815)	(0.0479)	(0.129)
Coverage x ACA x Chronic Cond.	0.181**	0.115**	0.200
	(0.0849)	(0.0513)	(0.162)
Observations	12,029	12,029	12,029
Race	Y	Y	Y
Education	Y	Y	Y
Marital Status	Y	Y	Y
Age	Y	Y	Y
Region	Y	Y	Y

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Columns 1 and 2 show estimates from a specification where the dependent variables are in log form. Column 3 again provides estimates from a probit estimation where the dependent variable is a binary indicator for part-time work (<30 hours). The distinction between the estimations presented here versus earlier is that this estimation uses the presence of a Chronic Condition as a measure of healthfulness observable to the employer.

The first two columns of Table 11 examine wages in log form annually and hourly at all firms with more than 50 employees in the MEPS data. The third column focuses on the probability of working part-time. The estimates in Table 11 are qualitatively the same as those seen in Table 3. The size of the estimates presented is particularly worthy of note. The triple-difference co-efficient in column 2 suggest the presence of a chronic condition is associated with a 11.5% difference in hourly wages between firms who do and do not offer coverage after the ACA. Put another way, firms who will be forced to provide coverage as a result of the law now pay 11.5% per hour less (on average) to workers with a chronic condition compared to workers with no chronic conditions. On an annual basis, it amounts to an 18.1% difference.

In addition to using chronic health conditions to proxy for "healthfulness," numerous studies have found self-reported health to be a powerful predictor of future health care expenses once appropriate controls for age and gender were included in the analysis (see DeSalvo et al., 2009, Fleishman et al., 2006). Table 12 presents the estimates from a specification which uses poor or very poor self-reported health as a binary measure of (or lack of) healthfulness. The estimates presented do not concord well with Tables 3, 6, and 11, presenting a mixed picture. Specifically, the estimates on wages are of the opposite sign to one another indicating annual wages are higher for "poor health" workers at firms that offer coverage while hourly wages are lower relative to firms that do not offer cover. Neither estimate is statistically different from zero. The lack of significance is not surprising as few individuals report poor or very poor health. The estimate associated with the triple-difference interaction term in column 3 reflects the effect on part-time work at firms that offer coverage seen in earlier sections. Again, the reliability of these estimates given that only 10.8% of the restricted sample report having "poor" health is questionable.

Table 12: Triple-difference Estimates using Self-Reported Health as the Measure of Health

	(1) Log Wages	(2) Log Hourly Wage	(3) Part Time <30 Hours
Offers Coverage	0.793***	0.405***	-1.437***
	(0.0341)	(0.0201)	(0.0655)
After ACA	-0.0119	0.00277	-0.0892
	(0.0425)	(0.0250)	(0.0678)
Offers Coverage x After ACA	0.0177	0.0353	0.0440
	(0.0442)	(0.0268)	(0.0853)
Self-Reported Health = Poor	-0.156*	-0.115**	0.0772
	(0.0865)	(0.0524)	(0.137)
Offers Coverage x Self-Reported Health	-0.0178	-0.00362	0.00789
	(0.0925)	(0.0573)	(0.189)
After ACA x Self-Reported Health	-0.119	-0.00843	-0.287
	(0.120)	(0.0667)	(0.196)
Coverage x ACA x Self-Reported Health	0.0336	-0.0486	0.522**
	(0.130)	(0.0742)	(0.261)
Observations	12,029	12,029	12,029
Race	Y	Y	Y
Education	Y	Y	Y
Marital Status	Y	Y	Y
Age	Y	Y	Y
Region	Y	Y	Y

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Columns 1 and 2 show estimates from a specification where the dependent variables are in log form. Column 3 again provides estimates from a probit estimation where the dependent variable is a binary indicator for part-time work (<30 hours). The distinction between the estimations presented here versus earlier is that this estimation uses poor or very poor self-reported health as a measure of (lack of) healthfulness.

Overall, Tables 11 and 12 do not contradict the notion that the incidence of health care mandates can and does occur at the individual level. A broad concordance between the various measures of healthfulness and their effects on labor market outcomes after the ACA is still visible. The effects echo the correlation between self-reported health, chronic health conditions, and health expenses in the data (see Table 13).

Table 13: Relationship between Self-Reported Health, Chronic Conditions, and Health Expenses

Self-Reported Health	Total Health-care Expenses (Mean)	Number of Chronic Conditions Reported (Mean)	Observations
	$		
Excellent	1,869	0.5	4,323
Good	2,543	0.9	5,971
Fair	3,322	1.3	5,234
Poor	5,429	2.0	1,557
Very Poor	11,851	3.1	330

6.2.2 Risk Scoring and Risk Adjustment

Lastly, many authors have considered the predictive ability of existing diagnoses (see Farley et al., 2006, Meenan et al., 2003 or Perkins et al., 2004) and behavioral indicators (see Pronk et al., 1999, Sturm, 2002, Garrett et al., 2004) to determine which risks *cause* high health care expenditure. The basic idea is to use clustering methods from operations research to group together various medical diagnoses into broader risk categories. Specially designed software "learns" how these codes determine future costs using pre-existing data. It then predicts future relative costs for currently observed individuals. This type of software is used extensively in insurance underwriting and by self-insured firms.

Johns Hopkins HealthCare Solutions at The Johns Hopkins University Bloomberg School of Public Health have kindly provided a research license to the author for their proprietary ACG Health Insurance Risk Score software. The ACG (Adjusted Clinical Groups) system takes medical diagnostic codes (such as the ICD-9 codes in many health-related data-sets including the MEPS) along with demographic and geographic information to produce an estimate of an individual's relative health care "riskiness." These risk scores provide an alternate and highly informative measure of healthfulness. The scores are designed to retain "signal" (long term, expensive medical conditions) and discard "noise" (short-term conditions).[30] However, ACG risk scores must be used with caution. First, they do not have a simple economic interpretation. Second, the MEPS data is not an ideal data set to use as it truncates ICD-9 codes down to a broad three-digit (rather than specific five-digit) level. The truncation to three-digit codes in the public use data discards potentially crucial information. Five-digit codes are available only by gaining access to confidential data files at Research Data Centers. The risk-scoring methodology, the software generating ACG scores, and the interpretation of ACG based results are quite complex. As a result, estimates produced using the ACG software and MEPS data will be presented in a follow-up paper.

[30] An excellent overview of the purpose, strengths and limits of the software are provided by researchers at the Manitoba Centre for Health Policy at the University of Manitoba - http://mchp-appserv.cpe.umanitoba.ca/viewConcept.php?printer=Y&conceptID=1304

7 Conclusion

The Affordable Care Act's pre-implementation period provides a unique opportunity to identify the causal relationship between health expenses and labor market outcomes in a world with employer-provided health coverage. While prior research on mandated benefits shows groups who receive a mandated benefit appear to pay for the benefit provided there are reasons to be skeptical of their findings. Either the mandates studied affect workers and firms simultaneously, or there is insufficient data to examine individual level effects, or both. This paper uses the Affordable Care Act's employer mandate to provide evidence of the individual-specific impacts of a particular type of mandated benefit: Employer-provided health insurance. After controlling for demographic factors associated with health expenses, estimates show firms affected by the ACA's mandates respond by favoring workers with lower health care expenses and ultimately pay lower wages to workers with higher health care expenses.

These findings should not be seen as an indictment of the new health care law, but instead a critique of the institution of employer-provided health insurance. The supposed benefit of employer-based coverage is that groups of workers are ideal risk pools because insurers would have incentives to "screen" out higher-cost individuals if workers had to buy their own coverage. Because firms ultimately pay the health expenses of their employees, they are incentivized to act as the insurer would, lowering the wages of higher-cost employees or excluding applicants from employment altogether. The labor market distortion created by employer provided insurance might be acceptable if it solved the risk-pooling problems it is supposed to. However, as it does not it appears objectively inferior to a world without employer-provided insurance.

The changes the ACA brought to the market for health coverage are used in this paper as a tool to study the behavior of employers in response to the incentives provided by employer-based health insurance. Again, while the Affordable Care Act itself is not what is being analyzed in this paper the findings presented - when combined with the literature on mandated benefits - make the employer mandate in the Affordable Care Act a curious artifact. If individual workers will pay for their care (one way or another) the mandate, at best, seems to arbitrarily restrict workers to a benefits package chosen for them by their employer. At worst, it leaves higher-cost workers unemployed.

What is less clear from this paper is how the *process* of cost-shifting works on a practical level. In a decentralized firm it is not clear why a mid-level team-leader or division manager tasked with hiring a new worker would treat applicants differently based upon expected health expenses. How would it affect the manager one way or another? The robustness check focusing on smaller firms suggested the effects of the Act were larger in economic significance at smaller firms, perhaps where owners are more involved in hiring decisions. However, the mechanism that leads to the outcomes presented in this paper is still something of a black box. Future work will attempt to tackle this question using experimental approaches.

References

Acemoglu, D. and Angrist, J. D. (2001). Consequences of Employment Protection? The Case of the Americans with Disabilities Act. *Journal of Political Economy*, 109(5):915–957.

Baicker, K. and Chandra, A. (2006). The labor market effects of rising health insurance premiums. *Journal of Labor Economics*, 24(3):609–634.

Baicker, K., Finkelstein, A., Song, J., and Taubman, S. (2014). The impact of medicaid on labor market activity and program participation: Evidence from the oregon health insurance experiment. *American Economic Review*, 104(5):322–28.

Baicker, K. and Levy, H. (2008). Employer health insurance mandates and the risk of unemployment. *Risk Management and Insurance Review*, 11(1):109–132.

Bailey, J. (2013). Who pays for obesity? Evidence from health insurance benefit mandates. *Economics Letters*, 121(2):287–289.

Bailey, J. (2014). Who pays the high health costs of older workers? Evidence from prostate cancer screening mandates. *Applied Economics*, 46:32:3931–3941.

Becker, G. S. (1957). *The Economics of Discrimination*. The University of Chicago Press.

Bertsimas, D., Bjarnadóttir, M. V., Kane, M. A., Kryder, J. C., Pandey, R., Vempala, S., and Wang, G. (2008). Algorithmic prediction of health-care costs. *Operations Research*, 56(6):1382–1392.

Bowlus, A. J. and Eckstein, Z. (2002). Discrimination and skill differences in an equilibrium search model. *International Economic Review*, 43(4):1309–1345.

Buchmueller, T. C., DiNardo, J., and Valletta, R. G. (2011). The effect of an employer health insurance mandate on health insurance coverage and the demand for labor: Evidence from hawaii. *American Economic Journal: Economic Policy*, 3(4):25–51.

Card, D. and Krueger, A. B. (1994). Minimum wages and employment: A case study of the fast-food industry in new jersey and pennsylvania. *American Economic Review*, 84(4):772–793.

DeSalvo, K. B., Jones, T. M., Peabody, J., McDonald, J., Fihn, S., Fan, V., He, J., and Muntner, P. (2009). Health care expenditure prediction with a single item, self-rated health measure. *Medical Care*, 47(4):440–447.

Even, W. E. and MacPherson, D. A. (2015). The affordable care act and the growth of involuntary part-time employment. *Working Paper*.

Farley, J. F., Harley, C. R., and Devine, J. W. (2006). A comparison of comorbidity measurements to predict healthcare expenditures. *The American journal of managed care*, 12(2):110–119.

Fleishman, J. A., Cohen, J. W., Manning, W. G., and Kosinski, M. (2006). Using the sf-12 health status measure to improve predictions of medical expenditures. *Medical care*, 44(5):I–54.

Garrett, B. and Kaestner, R. (2015). Recent evidence on the aca and employment: Has the aca been a job killer? *Urban Institute Working Paper*.

Garrett, N. A., Brasure, M., Schmitz, K. H., Schultz, M. M., and Huber, M. R. (2004). Physical inactivity: direct cost to a health plan. *American journal of preventive medicine*, 27(4):304–309.

Gruber, J. (1993). The incidence of mandated maternity benefits. *American Economic Review*, 84(3):622–641.

Gruber, J. (1994). State-mandated benefits and employer-provided health insurance. *Journal of Public Economics*, 55(3):433 – 464.

Gruber, J. and Krueger, A. B. (1991). The incidence of mandated employer-provided insurance: Lessons from workers' compensation insurance. *Tax Policy and the Economy*, 5:111–143.

Jensen, G. A. and Morrisey, M. A. (2001). Endogenous fringe benefits, compensating wage differentials, and older workers. *International Journal of Health Care Finance and Economics*, 1:203–226.

Kolstad, J. and Kowalski, A. (2012). Mandate-based health reform and the labor market: Evidence from the massachusetts reform. *NBER Working Paper Series*, Working Paper 17933.

Lahey, J. N. (2012). The efficiency of a group-specific mandated benefit revisited: The effect of infertility mandates. *Journal of Policy Analysis and Management*, 31(1):63–92.

Lee, M.-j. and Kang, C. (2006). Identification for difference in differences with cross-section and panel data. *Economics letters*, 92(2):270–276.

Levy, H. (1998). Who pays for health insurance? employee contributions to health insurance premiums. Technical report, Princeton University Industrial Relations Section Working Paper 398.

Levy, H. and Feldman, R. (2001). Does the incidence of group health insurance fall on individual workers? *International Journal of Health Care Finance and Economics*, 1:227–247.

Machlin, S., Soni, A., and Fang, Z. (2010). Understanding and analyzing meps household component medical condition data. Technical report, AHRQ.

Mathur, A., Slavov, S. N., and Strain, M. R. (2015). Has the affordable care act increased part-time employment? *Applied Economics Letters*, 0(0):1–4.

Meenan, R. T., Goodman, M. J., Fishman, P. A., Hornbrook, M. C., O'Keeffe-Rosetti, M. C., and Bachman, D. J. (2003). Using risk-adjustment models to identify high-cost risks. *Medical care*, 41(11):1301–1312.

Mitchell, O. S. (1990). Working paper no. 3260: The effects of mandating benefits packages. *NBER Working Paper Series*.

Mortensen, D. (1990). *Panel Data and Labour Market Studies*, chapter Equilibrium Wage Distributions: A Synthesis, pages 279–96. Amsterdam: North-Holland.

Mustard, C. A., Kaufert, P., Kozyrskyj, A., and Mayer, T. (1998). Sex differences in the use of health care services. *New England Journal of Medicine*, 338(23):1678–1683. PMID: 9614260.

Pauly, M. and Herring, B. (1999). *Pooling Health Insurance Risks*. AEI Press.

Perkins, A. J., Kroenke, K., Unützer, J., Katon, W., Williams, J. W., Hope, C., and Callahan, C. M. (2004). Common comorbidity scales were similar in their ability to predict health care costs and mortality. *Journal of clinical epidemiology*, 57(10):1040–1048.

Pronk, N. P., Goodman, M. J., O'Connor, P. J., and Martinson, B. C. (1999). Relationship between modifiable health risks and short-term health care charges. *Jama*, 282(23):2235–2239.

Schmidt, L. (2007). Effects of infertility insurance mandates on fertility. *Journal of Health Economics*, 26(3):431 – 446.

Sheiner, L. (1999). Health care costs, wages, and aging. *Federal Reserve Working Paper Series*, (32).

Siemer, M. (2014). Firm entry and employment dynamics in the great recession.

Sturm, R. (2002). The effects of obesity, smoking, and drinking on medical problems and costs. *Health Affairs*, 21(2):245–253.

Summers, L. H. (1989). Some simple economics of mandated benefits. *The American Economic Review: Papers and Proceedings of the Hundred and First Annual Meeting of the American Economic Association*, 79(2):177–183.

Thurston, N. (1997). Labor market effects of hawaii's mandatory employer-provided health insurance. *Industrial and Labor Relations Review*, 51(1):117–138.

Appendix A - Additional Estimates

In Table 3 (and Table 6 for smaller firms only) of the paper, a triple-difference estimation compares the labor market outcomes of high and low cost workers, at firms that do and do not provide coverage, before and after the ACA was announced. This triple-difference estimator is no more than the combination of two difference-in-difference estimations presented below for completeness. The first three columns of Table 14 present the basic difference-in-difference estimates comparing the labor market outcomes of workers with varying health expenses at firms that do not offer coverage before and after the ACA. The last three columns present the same estimates at firms that do offer coverage.

The co-efficients on the difference-in-difference interaction terms shows that the relative rise in annual and hourly wages at firms that do offer coverage observed in the triple-difference estimation consists mainly of a fall in wages at firms that do not offer coverage. That is, firms *most* affected by the Act reduce wages paid to high cost workers. In contrast, at firms that offer coverage, wages actually *increase* slightly for less-healthy workers relative to healthier workers after the Act. Interestingly, there is a statistically significant reduction in hours worked at firms that do not offer coverage. As there is also a reduction (not statistically significant) in hours worked at firms that do offer coverage the triple difference estimation missed that effect.

Table 14: Difference-in-Difference Estimates for the ACA's Effects by Health Coverage

	(1) Log Wages	(2) Log Hourly Wage	(3) Part Time <30 Hours	(4) Log Wages	(5) Log Hourly Wage	(6) Part Time <30 Hours
After ACA	0.103*	0.0852**	0.0826	-0.0129	-0.00634	0.0305
	(0.0559)	(0.0352)	(0.114)	(0.0275)	(0.0206)	(0.115)
Poor Health	-0.00745	0.00535	0.0943***	0.00757**	0.00372	0.0238*
	(0.00872)	(0.00513)	(0.0149)	(0.00307)	(0.00232)	(0.0125)
After ACA x Poor Health	-0.0272**	-0.0170**	-0.0329*	0.00179	0.00685**	-0.00706
	(0.0116)	(0.00671)	(0.0195)	(0.00416)	(0.00313)	(0.0169)
Observations	1,770	1,770	1,770	10,261	10,261	10,261
Race	Y	Y	Y	Y	Y	Y
Education	Y	Y	Y	Y	Y	Y
Marital Status	Y	Y	Y	Y	Y	Y
Age	Y	Y	Y	Y	Y	Y
Region	Y	Y	Y	Y	Y	Y

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The first three columns focus on firms who do not offer coverage. Columns 1 and 2 show estimates from a specification where the dependent variables are in log form. Column 3 again provides estimates from a probit estimation where the dependent variable is a binary indicator for part-time work (<30 hours). The last three columns focus on firms that do provide coverage. The table shows the effects of the Act are focused on the firms who did not provide coverage before the Act was announced.

In addition, in Table 3 of the paper, estimates presented showed only a single specification, including a series of controls, for the main results. This is because the main result is robust to all specifications that have been examined. Table 15 presents a series of regression estimates which correspond to various alternatives to the specificaiton presented in column 1 of Table 3 in the paper. In each specification the dependent variable is the log of annual wages. The triple-difference interaction term "*Ln (Total Expenses) * After ACA * Offers Coverage*" represents the co-efficients of interest. In all specifications, the estimates are significant and of similar size.

Table 15: Robustness to Specification Changes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent Variable = Log Annual Wages						
Offers Coverage	0.787***	0.777***	0.783***	0.784***	0.696***	0.691***	0.690***
	(0.0471)	(0.0472)	(0.0470)	(0.0471)	(0.0471)	(0.0471)	(0.0476)
After ACA	0.0842	0.0820	0.0734	0.0696	0.0703	0.0691	0.0760
	(0.0552)	(0.0554)	(0.0550)	(0.0550)	(0.0560)	(0.0559)	(0.0555)
Offers Coverage * After ACA	-0.0672	-0.0621	-0.0562	-0.0565	-0.0932	-0.0911	-0.0963
	(0.0629)	(0.0629)	(0.0625)	(0.0623)	(0.0624)	(0.0623)	(0.0618)
Ln (Total Expenses)	0.00220	-8.78e-05	0.000233	0.00100	-0.0125	-0.0126	-0.00575
	(0.00863)	(0.00864)	(0.00867)	(0.00864)	(0.00856)	(0.00852)	(0.00856)
Coverage * Ln (Total Exp.)	0.0248***	0.0253***	0.0256***	0.0244***	0.0212**	0.0211**	0.0162*
	(0.00921)	(0.00922)	(0.00923)	(0.00919)	(0.00906)	(0.00902)	(0.00903)
Ln (Total Expenses) * After ACA	-0.0306***	-0.0295**	-0.0284**	-0.0272**	-0.0287**	-0.0283**	-0.0300***
	(0.0117)	(0.0117)	(0.0117)	(0.0117)	(0.0116)	(0.0115)	(0.0115)
Ln (Total Exp.) * ACA * Coverage	0.0288**	0.0274**	0.0262**	0.0255**	0.0302**	0.0299**	0.0313**
	(0.0126)	(0.0126)	(0.0126)	(0.0125)	(0.0123)	(0.0123)	(0.0122)
Observations	12,111	12,111	12,111	12,111	12,031	12,031	12,031
Age		Y	Y	Y	Y	Y	Y
Region			Y	Y	Y	Y	Y
Race				Y	Y	Y	Y
Education					Y	Y	Y
Marital Status						Y	Y
Industry							Y

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The first column of the table presents a specification with no controls. Each column then adds further demographic controls. The effect size and significance remains almost constant across specifications.

In addition, showing that the effects observed are not simply the result of firms in certain industries or regions reacting to the law more than others or firms using race, gender, and age as a heuristic for health expenses, the estimates presented in Table 16 interact all controls with a post-ACA dummy. The effect size and significance remains almost constant across specifications.

Table 16: Robustness to Post-ACA Period by Firm Location, Industry and Demographic Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent Variable = Log Annual Wages						
Offers Coverage	0.787***	0.779***	0.785***	0.786***	0.700***	0.696***	0.699***
	(0.0471)	(0.0471)	(0.0470)	(0.0472)	(0.0474)	(0.0474)	(0.0487)
After ACA	0.0842	0.125	0.210	0.0523	0.222	0.421	0.715
	(0.0552)	(1.823)	(1.812)	(1.795)	(1.714)	(1.712)	(1.692)
Offers Coverage * After ACA	-0.0672	-0.0646	-0.0575	-0.0572	-0.0981	-0.0988	-0.111*
	(0.0629)	(0.0629)	(0.0626)	(0.0628)	(0.0634)	(0.0632)	(0.0642)
Ln (Total Expenses)	0.00220	0.000359	0.000632	0.00131	-0.0120	-0.0121	-0.00484
	(0.00863)	(0.00864)	(0.00869)	(0.00865)	(0.00858)	(0.00855)	(0.00863)
Offers Coverage * Ln (Total Exp.)	0.0248***	0.0254***	0.0255***	0.0244***	0.0211**	0.0211**	0.0160*
	(0.00921)	(0.00921)	(0.00923)	(0.00919)	(0.00906)	(0.00903)	(0.00909)
Ln (Total Expenses) * After ACA	-0.0306***	-0.0303***	-0.0290**	-0.0274**	-0.0294**	-0.0289**	-0.0314***
	(0.0117)	(0.0118)	(0.0118)	(0.0117)	(0.0117)	(0.0116)	(0.0116)
Ln (Total Exp.) * ACA * Coverage	0.0288**	0.0272**	0.0260**	0.0250**	0.0300**	0.0295**	0.0314**
	(0.0126)	(0.0126)	(0.0126)	(0.0125)	(0.0123)	(0.0123)	(0.0123)
Observations	12,111	12,111	12,111	12,111	12,031	12,031	12,031
Age		Y x ACA	Y x ACA	Y x ACA	Y x ACA	Y x ACA	Y x ACA
Region			Y x ACA	Y x ACA	Y x ACA	Y x ACA	Y x ACA
Race				Y x ACA	Y x ACA	Y x ACA	Y x ACA
Education					Y x ACA	Y x ACA	Y x ACA
Marital Status						Y x ACA	Y x ACA
Industry							Y x ACA

"x ACA" means the control was interacted with the after ACA announcement dummy

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

The first column of the table presents a specification with no controls. Each column then adds further demographic controls but each is also interacted with the post-ACA period dummy. The effect size and significance remains almost constant across specifications suggesting effects are not isolated to specific regions, industries, or demographic groups delineated by age, gender, or race.

Appendix B - Additional Proofs

Flow Conditions

If UE^i is the steady state number of type i unemployed workers and $G^i(w_i)$ is the fraction of type $i = A, B$ workers who earn w_i or less, i.e., the *cdf* of earnings for i. In a steady-state, the flows of type A workers into a firm offering wage w_A and flows out of such a firm must be equal:

$$\begin{aligned} \lambda_0 UE^A + \lambda_1 G^A(w_A)((1-\theta)M - UE^A) &= \delta_A l^A(w_A) + \lambda_1(1-\gamma_d)(1-F_n^A(w_A))l^A(w_A) \\ &\quad + \lambda_1\gamma_d(1-F_d^A(w_A))l^A(w_A) \end{aligned}$$

with $l^A(w_A) = l_n^A(w_A) = l_d^A(w_A)$ indicating the steady state "number" of type A workers at each type of firm being the same. The flow in is the sum of those who are unemployed and receive an offer plus those who are already working but switch in from another firm. The flow out is the sum of the exogenously given rate of job destruction plus those who move out to each of the types of firm.

For type B workers,

$$\begin{aligned}\lambda_0 UE^B + \lambda_1 G^B(w_B)((\theta M - UE^B) &= \delta_B l_n^B(w_B) + \lambda_1(1-\gamma_d)(1-F_n^B(w_B))l_n^B(w_B) \\ &+ k\lambda_1\gamma_d(1-F_d^B(w_B))l_n^B(w_B)\end{aligned}$$

equates the flow in and out of "normal" employers. And for the employers who obtain cost;

$$\begin{aligned}k\lambda_0 UE^B + k\lambda_1 G^B(w_B)((\theta M - UE^B) &= \delta_B l_d^B(w_B) + \lambda_1(1-\gamma_d)(1-F_n^B(w_B))l_d^B(w_B) \\ &+ k\lambda_1\gamma_d(1-F_d^B(w_B))l_d^B(w_B)\end{aligned}$$

From these steady state flows, UE^A, UE^B, $G^A(w_A)$, and $G^B(w_B)$ can be recovered. For steady state unemployment stocks for type A workers, let the flows in and out of unemployment equal one another;

$$\lambda_0(1-\gamma_d)(1-F_n^A(r_A))UE^A + \lambda_0\gamma_d(1-F_d^A(r_A))UE^A = \delta_A((1-\theta)M - UE^A) \tag{24}$$

Note that at all times it must be the case that $UE^A + l^A(w_A) = (1-\theta)M$ with a similar condition for type B workers. Type B workers have steady state unemployment stocks of

overall

$$\lambda_0(1-\gamma_d)(1-F_n^B(r_B))UE^B + k\lambda_0\gamma_d(1-F_d^B(r_B))UE^B = \delta_B(\theta M - UE^B) \tag{25}$$

The flow conditions that relate the offer and earnings distributions for Type A workers

$$\begin{aligned}&[\lambda_0(1-\gamma_d)(F_n^A(w_A)-F_n^A(r_A)) \\ &+\lambda_0\gamma_d(F_n^A(w_A)-F_n^A(r_A))]UE^A = \delta_A G^A(w_A)((1-\theta)M - UE^A) \\ &\qquad + \left[\lambda_1(1-\gamma_d)(1-F_n^A(w_A)) + \lambda_1\gamma_d(1-F_d^A(w_A))\right] \\ &\qquad \times G^A(w_A)((1-\theta)M - UE^A)\end{aligned}$$

and for type B workers

$$\begin{aligned}
&[\lambda_0(1-\gamma_d)(F_n^B(w_B) - F_n^B(r_B)) \\
&+k\lambda_0\gamma_d(F_n^B(w_B) - F_n^B(r_B))]UE^B \quad = \quad \delta_B G^B(w_B)((\theta M - UE^B) \\
&\qquad + \left[\lambda_1(1-\gamma_d)(1-F_n^B(w_B)) + k\lambda_1\gamma_d(1-F_d^B(w_B))\right] \\
&\qquad \times G^B(w_B)((\theta M - UE^B)
\end{aligned}$$

The left-hand side of each equation gives the steady-state number of workers who receive acceptable wage offers below w while unemployed, whereas the right-hand side represents the number of workers with wages below w who exit to unemployment plus those who exit to higher paying employers.

Derivation of Equilibrium Wages, Offers, and Steady State Labor Stocks for Type B workers

Let w_B^i be a utility maximizing wage for firm $i = n, d$. As employers are utility maximizers, using the identities for the reservation wages for each type of worker it must be the case

$$(P_B - w_B^n)l_n^B(w_B^n) \geq (P_B - w_B^d)l_n^B(w_B^d) \tag{26}$$

This is because the expression on each side represents profit per worker times the number of workers. The profit from employing type B workers at "normal" employers must be at least as good as "mimicking" other employers. Similarly, at type d employers;

$$(P_B - d - w_B^d)l_d^B(w_B^d) \geq (P_B - d - w_B^n)l_d^B(w_B^n) \tag{27}$$

indicating that they also must be at least as well off under their chosen strategy as they would by mimicking the normal employers. Some algebraic manipulation will show that:

$$X(w_B^n) = (P_B - w_B^n)l_n^B(w_B^n) - (P_B - d - w_B^n)kl_d^B(w_B^n) \geq (P_B - w_B^d)l_n^B(w_B^d) - (P_B - d - w_B^d)kl_d^B(w_B^d) \tag{28}$$

for any w_B^i that is a utility maximizing wage for firm $i = n, d$. Consider that

$$X'(w_B^n) = (P_B - w_B^n)(1-k)l_n'^B(w_B^n) + kdl_n'^B(w_B^n)l_n'^B(w_B^n) - (1-k)l_n^B(w_B^n) > 0 \tag{29}$$

This expression is *strictly* positive as $(P_B - w_B^n)(1-k)l_n'^B(w_B^n) = (1-k)l_n^B(w_B^n)$. This is because $\pi = (P_B - w_B^n)l_n^B(w_B^n)$ and $\frac{\partial \pi}{\partial w_B^n} = ((P_B - w_B^n)l_n'^B(w_B^n) \times 1) - l_n^B(w_B^n)$ which is zero at a maximum.

Consider $w_B^d \in (\underline{w}_B^n, \overline{w}_B^n)$ where the boundary terms represent the upper and lower limit of the wage offer distribution from type n employers. Given $X'(w_B^n) > 0$ then it must be the case that;

$$(P_B - \underline{w}_B^n) l_n^B(\underline{w}_B^n) - (P_B - d - \underline{w}_B^n) l_d^B(\underline{w}_B^n) < (P_B - w_B^d) l_n^B(w_B^d) - (P_B - d - w_B^d) l_d^B(w_B^d) \tag{30}$$

for $w_B^d > \underline{w}_B^n$. However, this suggests that a profitable deviation from an employer's optimal strategy exists. As this cannot be the case it means $w_B^d \notin (\underline{w}_B^n, \overline{w}_B^n)$ which ensures that the distributions of wage offers from type d and n employers are disjoint and the wage offer distribution takes the form;

$$\begin{aligned}
F_d^B(w_B) &= F_n^B(w_B) = 0 \qquad w_B \leq r_B \\
F_d^B(w_B) &> 0;\ F_n^B(w_B) = 0 \quad r_B < w_B \leq wh_d \\
F_d^B(w_B) &= 1;\ F_n^B(w_B) > 0 \quad wh_d \leq w_B \leq wh_B \\
F_d^B(w_B) &= F_n^B(w_B) = 1 \qquad w_B \geq wh_B
\end{aligned}$$

where wh_B is the highest possible wage to type B workers and $wh_d < wh_B$ represents the max from cost employers. While symbolically complicated, the wage distribution is such that no offers are made below reservation wage (indicating that any job offer that is made will actually be accepted), all type B workers obtain a wage from type d employers in the region $w_B \in [r_B, wh_d]$. Type B workers will receive a wage $w_B \in [wh_d, wh_B]$ at type n firms. Lastly, no type B worker gets more than wh_B.

Gathering results gives

$$l_d^B(w_B) = \frac{k\kappa_{0B}(1+\kappa_{1B}^k)\theta M}{(1+\kappa_{0B}^k)\left(1 + k\kappa_{1B}\gamma_d(1-F_d^B(w_B)) + \kappa_{1B}(1-\gamma_d)\right)^2} \quad r_B \leq w_B \leq wh_B \tag{31}$$

where $\kappa_{iB}^k = \kappa_{1B}(1-\gamma_d) + k\kappa_{iB}\gamma_d$ for $i = 0, 1$. This result is found algebraically by using the fact that $F_d^B(w_B) > 0;\ F_n^B(w_B) = 0\ \ \forall w_B \in (r_B, wh_d]$ in conjunction with the three equations which represent the steady state flows in and out of employment at type d employers;

$$\begin{aligned}
k\lambda_0 UE^B + k\lambda_1 G^B(w_B)((\theta M - UE^B) &= \delta_B l_d^B(w_B) + \lambda_1(1-\gamma_d)(1-F_n^B(w_B)) l_d^B(w_B) \\
&\quad + k\lambda_1\gamma_d(1-F_d^B(w_B)) l_d^B(w_B)
\end{aligned}$$

in and out of unemployment;

$$\lambda_0(1-\gamma_d)(1-F_n^B(r_B))UE^B + k\lambda_0\gamma_d(1-F_d^B(r_B))UE^B = \delta_B(\theta M - UE^B) \tag{32}$$

and the relationship between offers and earning which underpins the equilibrium solution;

$$
\begin{aligned}
&[\lambda_0(1-\gamma_d)(F_n^B(w_B)-F_n^B(r_B)) \\
&+k\lambda_0\gamma_d(F_n^B(w_B)-F_n^B(r_B))]UE^B \quad = \quad \delta_B G^B(w_B)((\theta M - UE^B) \\
&\qquad + \left[\lambda_1(1-\gamma_d)(1-F_n^B(w_B)) + k\lambda_1\gamma_d(1-F_d^B(w_B))\right] \\
&\qquad \times G^B(w_B)((\theta M - UE^B)
\end{aligned}
$$

While the algebra is omitted to economize on space, the solution proceeds by solving for UE^B in the unemployment equation, substituting the resultant expression into the remaining conditions and then solving for the earnings distribution G. Similarly, it can be shown that;

$$
l_n^B(w_B) = \frac{\kappa_{0B}(1+\kappa_{1B}^k)\theta M}{(1+\kappa_{0B}^k)\left(1+\kappa_{1B}(1-\gamma_d)(1-F_n^B(w_B))\right)^2} \quad wh_d \le w_B \le wh_B \tag{33}
$$

To solve for the *offer* distributions consider that employers of a single type must equalize utility at wage offers that satisfy $w_B \in [r_B, wh_d]$. That is, no type d firm should be able to increase profits by mimicking another type d firm. Thus;

$$
(P_B - d - r_B)l_d^B(r_B) = (P_B - d - w_B)l_d^B(w_B) \tag{34}
$$

Similarly, for type n employers;

$$
(P_B - wh_d)l_n^B(wh_d) = (P_B - w_B)l_n^B(w_B) \tag{35}
$$

which means that

$$
l_d^B(w_B) = \frac{(P_B - d - r_B)l_d^B(r_B)}{(P_B - d - w_B)} = \frac{k\kappa_{0B}(1+\kappa_{1B}^k)\theta M}{(1+\kappa_{0B}^k)\left(1+k\kappa_{1B}\gamma_d(1-F_d^B(w_B)) + \kappa_{1B}(1-\gamma_d)\right)^2} \tag{36}
$$

and

$$
l_n^B(w_B) = \frac{(P_B - wh_d)l_n^B(wh_d)}{(P_B - w_B)} = \frac{\kappa_{0B}(1+\kappa_{1B}^k)\theta M}{(1+\kappa_{0B}^k)\left(1+\kappa_{1B}(1-\gamma_d)(1-F_n^B(w_B))\right)^2} \tag{37}
$$

implying;

$$
F_d^B(w_B) = \frac{1+\kappa_{1B}^k}{k\kappa_{1B}\gamma_d} - \left(\frac{1+\kappa_{1B}^k}{k\kappa_{1B}\gamma_d}\right)\left(\frac{P_B - d - w_B}{P_B - d - r_B}\right)^{1/2} \quad r_B \le w_B \le wh_d \tag{38}
$$

because

$$
\left(1+k\kappa_{1B}\gamma_d(1-F_d^B(w_B)) + \kappa_{1B}(1-\gamma_d)\right)^2 \quad = \quad \frac{k\kappa_{0B}(1+\kappa_{1B}^k)\theta M}{(1+\kappa_{0B}^k)l_n^B(wh_d)}\left(\frac{P_B - d - w_B}{P_B - d - r_B}\right)
$$

$$\Rightarrow F_d^B(w_B)) = \frac{1 + k\kappa_{1B}\gamma_d + \kappa_{1B}(1-\gamma_d)}{k\kappa_{1B}\gamma_d} - \frac{1}{k\kappa_{1B}\gamma_d}\left(\frac{k\kappa_{0B}(1+\kappa_{1B}^k)\theta M}{(1+\kappa_{0B}^k)l_n^B(wh_d)}\left(\frac{P_B - d - w_B}{P_B - d - r_B}\right)\right)^{1/2} \quad (39)$$

$$\Rightarrow F_d^B(w_B)) = \frac{1+\kappa_{1B}^k}{k\kappa_{1B}\gamma_d} - \frac{1}{k\kappa_{1B}\gamma_d} \times \left(\frac{k\kappa_{0B}(1+\kappa_{1B}^k)\theta M}{(1+\kappa_{0B}^k)\frac{k\kappa_{0B}(1+\kappa_{1B}^k)\theta M}{(1+\kappa_{0B}^k)(1+k\kappa_{1B}\gamma_d+\kappa_{1B}(1-\gamma_d))^2}}\right)^{1/2}\left(\frac{P_B - d - w_B}{P_B - d - r_B}\right)^{1/2} \quad (40)$$

$$\Rightarrow F_d^B(w_B)) = \frac{1+\kappa_{1B}^k}{k\kappa_{1B}\gamma_d} - \frac{1 + k\kappa_{1B}\gamma_d + \kappa_{1B}(1-\gamma_d)}{k\kappa_{1B}\gamma_d}\left(\frac{P_B - d - w_B}{P_B - d - r_B}\right)^{1/2} \quad (41)$$

where

$$l_n^B(r_B) = \frac{k\kappa_{0B}(1+\kappa_{1B}^k)\theta M}{(1+\kappa_{0B}^k)\left(1+k\kappa_{1B}\gamma_d(1-F_d^B(r_B))+\kappa_{1B}(1-\gamma_d)\right)^2} = \frac{k\kappa_{0B}(1+\kappa_{1B}^k)\theta M}{(1+\kappa_{0B}^k)\left(1+k\kappa_{1B}\gamma_d+\kappa_{1B}(1-\gamma_d)\right)^2} \quad (42)$$

.

Similarly, it can be shown that

$$F_n^B(w_B) = \frac{1+\kappa_{1B}(1-\gamma_d)}{\kappa_{1B}(1-\gamma_d)} - \left(\frac{1+\kappa_{1B}(1-\gamma_d)}{\kappa_{1B}(1-\gamma_d)}\right)\left(\frac{P_B - w_B}{P_B - wh_d}\right)^{1/2} \quad wh_d \le w_B \le wh_B \quad (43)$$

The wage *earnings* distributions can be solved using a similar process of substitution and using the conditions that $F_d^B(wh_d) = 1$ and $F_n^B(wh_B) = 1$. In particular, these conditions give rise to the following two maximum wages at each type of firm

$$wh_d = P_B - d - \left(\frac{1+\kappa_{1B}(1-\gamma_d)}{1+\kappa_{1B}^k}\right)^2 (P_B - d - r_B) \quad (44)$$

and

$$wh_B = P_B - \left(\frac{1}{1+\kappa_{1B}(1-\gamma_d)}\right)^2 (P_B - wh_d) \quad (45)$$

These wages are, as we might expect, functions of productivity, the relative frequency of offers and job destruction, the proportion and max wages of type d employers, and reservation wages. The reservation wage can be solved by taking the reservation wage expression and substituting in the appropriate offer distributions $F_n^B(w_B)$ and $F_d^B(w_B)$;

$$r_B = b + \int_{r_B}^{\infty} \frac{(\lambda_0 - \lambda_1)\left((1-\gamma_d)\left(1-F_n^B(w)\right) + k\gamma_d\left(1-F_d^B(w)\right)\right)}{\beta + \delta_B + \lambda_1\left((1-\gamma_d)\left(1-F_n^B(w)\right) + k\gamma_d\left(1-F_d^B(w)\right)\right)} dw \quad (46)$$

The resulting expression for the reservation wage for type B workers is;

$$
\begin{aligned}
r_B \quad = \quad & \frac{(1+\kappa_{1B}^k)^2 b + \kappa_{1B}(\kappa_{0B}-\kappa_{1B})(1-\gamma_d+k\gamma_d)^2 P_B}{1+\kappa_{1B}^k+\kappa_{1B}(\kappa_{0B}-\kappa_{1B})(1-\gamma_d+k\gamma_d)^2} \\
& - \frac{\kappa_{1B}(\kappa_{0B}-\kappa_{1B})\left((1-\gamma_d+k\gamma_d)^2(1+\kappa_{1B}(1-\gamma_d))^2-(1-\gamma_d)^2(1+\kappa_{1B}^k)^2\right)d}{\left(1+\kappa_{1B}^k+\kappa_{1B}(\kappa_{0B}-\kappa_{1B})(1-\gamma_d+k\gamma_d)^2\right)\left(1+\kappa_{1B}(1-\gamma_d)\right)^2}
\end{aligned}
$$

Armed with an analytical expression for the reservation wage, wh_d, and wh_B along with expressions for $F_n^B(w_B)$, $F_d^B(w_B)$, and knowing that the flow in and out of unemployment is represented by;

$$
\begin{aligned}
& [\lambda_0(1-\gamma_d)(F_n^B(w_B)-F_n^B(r_B)) \\
& +k\lambda_0\gamma_d(F_n^B(w_B)-F_n^B(r_B))]UE^B \quad = \quad \delta_B G^B(w_B)((\theta M-UE^B) \\
& \qquad + \left[\lambda_1(1-\gamma_d)(1-F_n^B(w_B))+k\lambda_1\gamma_d(1-F_d^B(w_B))\right] \\
& \qquad \times G^B(w_B)((\theta M-UE^B)
\end{aligned}
$$

with some substitutions we can recover that

$$
G^B(w_B)=\begin{cases} \frac{\kappa_{0B}}{\kappa_{1B}\kappa_{0B}^k}\left[\left(\frac{P_B-d-w_B}{P_B-d-r_B}\right)^{1/2}-1\right] & r_B\le w_B\le wh_B \\ \frac{\kappa_{0B}}{\kappa_{1B}\kappa_{0B}^k}\left[\frac{1+\kappa_{1B}^k}{1+\kappa_{1B}(1-\gamma_d)}\left(\frac{P_B-wh_d}{P_B-w_B}\right)^{1/2}-1\right] & wh_d\le w_B\le wh_B \end{cases} \tag{47}
$$

Note that $\frac{1+\kappa_{1B}^k}{1+\kappa_{1B}(1-\gamma_d)}=\frac{1+k\kappa_{1B}\gamma_d+\kappa_{1B}(1-\gamma_d)}{1+\kappa_{1B}(1-\gamma_d)}>1$ just acts as a scaling factor. This completes the derivation of the labor stocks in steady state, along with equilibrium wages and offer distributions.

Equilibrium Effects of changes in γ_d

1. The labor stock change at a specific firm who moves from type n to type d.

A firm who becomes type d moves from employing $l_n^B(w_B^n)$ to $l_d^B(w_B^d)$ of type B workers where $w_B^n\neq w_B^d$. Remember that;

$$
l_n^B(w_B^n)=\frac{\kappa_{0B}(1+\kappa_{1B}^k)\theta M}{(1+\kappa_{0B}^k)\left(1+\kappa_{1B}(1-\gamma_d)(1-F_n^B(w_B^n))\right)^2} \tag{48}
$$

$$
l_d^B(w_B^d)=\frac{k\kappa_{0B}(1+\kappa_{1B}^k)\theta M}{(1+\kappa_{0B}^k)\left(1+k\kappa_{1B}\gamma_d(1-F_d^B(w_B^d))+\kappa_{1B}(1-\gamma_d)\right)^2} \tag{49}
$$

It follows that it is only the case that $l_n^B(w_B^n) > l_d^B(w_B^d)$ if

$$1 + k\kappa_{1B}\gamma_d(1 - F_d^B(w_B^d)) + \kappa_{1B}(1-\gamma_d) > k^{1/2}\left(1 + \kappa_{1B}(1-\gamma_d)(1 - F_n^B(w_B^n))\right) \tag{50}$$

Given $F_d^B(w_B^d) = \frac{1+\kappa_{1B}^k}{k\kappa_{1B}} - \left(\frac{1+\kappa_{1B}^k}{k\kappa_{1B}}\right)\left(\frac{P_B-d-w_B^d}{P_B-d-r_B}\right)^{1/2}$ and $F_n^B(w_B^n) = \frac{1+\kappa_{1B}(1-\gamma_d)}{\kappa_{1B}(1-\gamma_d)} - \left(\frac{1+\kappa_{1B}(1-\gamma_d)}{\kappa_{1B}(1-\gamma_d)}\right)\left(\frac{P_B-w_B^n}{P_B-wh_d}\right)^{1/2}$ then

$$1 + k\kappa_{1B}\gamma_d(1 - F_d^B(w_B^d)) + \kappa_{1B}(1-\gamma_d) \tag{51}$$

$$= 1 + k\kappa_{1B}\gamma_d(1 - \frac{1+\kappa_{1B}^k}{k\kappa_{1B}} + \left(\frac{1+\kappa_{1B}^k}{k\kappa_{1B}}\right)\left(\frac{P_B-d-w_B^d}{P_B-d-r_B}\right)^{1/2}) + \kappa_{1B}(1-\gamma_d) \tag{52}$$

$$= 1 + k\kappa_{1B}\gamma_d - \gamma_d(1+\kappa_{1B}^k) + \gamma_d\left(1+\kappa_{1B}^k\right)\left(\frac{P_B-d-w_B^d}{P_B-d-r_B}\right)^{1/2} + \kappa_{1B}(1-\gamma_d) \tag{53}$$

$$= 1 + \kappa_{1B}^k - \gamma_d(1+\kappa_{1B}^k)\left[1 - \left(\frac{P_B-d-w_B^d}{P_B-d-r_B}\right)^{1/2}\right]$$
$$= (1+\kappa_{1B}^k)\left(1 - \gamma_d\left[1 - \left(\frac{P_B-d-w_B^d}{P_B-d-r_B}\right)^{1/2}\right]\right)$$

and similarly,

$$k^{1/2}\left(1 + \kappa_{1B}(1-\gamma_d)(1 - F_n^B(w_B^n))\right) = k^{1/2}\left((1 + \kappa_{1B}(1-\gamma_d))\left(\frac{P_B-w_B^n}{P_B-wh_d}\right)^{1/2}\right) \tag{54}$$

Therefore $l_n^B(w_B^n) > l_d^B(w_B^d)$ if

$$(1+\kappa_{1B}^k)\left(1 - \gamma_d + \gamma_d\left(\frac{P_B-d-w_B^d}{P_B-d-r_B}\right)^{1/2}\right) > k^{1/2}\left((1 + \kappa_{1B}(1-\gamma_d))\left(\frac{P_B-w_B^n}{P_B-wh_d}\right)^{1/2}\right) \tag{55}$$

Since $(1+\kappa_{1B}^k) = 1 + \kappa_{1B}(1-\gamma_d) + k\kappa_{1B}\gamma_d$ then $(1+\kappa_{1B}^k) > 1 + \kappa_{1B}(1-\gamma_d)$. Note that for

$$1 - \gamma_d + \gamma_d\left(\frac{P_B-d-w_B^d}{P_B-d-r_B}\right)^{1/2} > \left(\frac{P_B-w_B^n}{P_B-wh_d}\right)^{1/2} \tag{56}$$

a *sufficient* but not *necessary* condition is for $\left(\frac{P_B-d-w_B^d}{P_B-d-r_B}\right)^{1/2} > \left(\frac{P_B-w_B^n}{P_B-wh_d}\right)^{1/2}$ because $\left(\frac{P_B-w_B^n}{P_B-wh_d}\right)^{1/2} = (1-\gamma_d)\left(\frac{P_B-w_B^n}{P_B-wh_d}\right)^{1/2} + \gamma_d\left(\frac{P_B-w_B^n}{P_B-wh_d}\right)^{1/2}$. For some values of parameters, $l_n^B(w_B^n) > l_d^B(w_B^d)$ even when $\left(\frac{P_B-d-w_B^d}{P_B-d-r_B}\right)^{1/2} < \left(\frac{P_B-w_B^n}{P_B-wh_d}\right)^{1/2}$. That is, it is an empirical question whether or not type a specific firms hires fewer workers if they become type d exogenously.

2. The equilibrium effects on labor stocks

A change in γ_d affects $l_i^B(w_B)$ for $i = d, n$. With labor stocks;

$$l_d^B(w_B) = \frac{k\kappa_{0B}(1+\kappa_{1B}^k)\theta M}{(1+\kappa_{0B}^k)\left(1+k\kappa_{1B}\gamma_d(1-F_d^B(w_B))+\kappa_{1B}(1-\gamma_d)\right)^2} \quad r_B \leq w_B \leq wh_B \tag{57}$$

and

$$l_n^B(w_B) = \frac{\kappa_{0B}(1+\kappa_{1B}^k)\theta M}{(1+\kappa_{0B}^k)\left(1+\kappa_{1B}(1-\gamma_d)(1-F_n^B(w_B))\right)^2} \quad wh_d \leq w_B \leq wh_B \tag{58}$$

then

$$\frac{\partial l_d^B(w_B)}{\partial \gamma_d} = (\Lambda - \Omega) \times \Delta < 0 \tag{59}$$

where

$$\Lambda = (1+\kappa_{0B}^k)\left(1+k\kappa_{1B}\gamma_d(1-F_d^B(w_B))+\kappa_{1B}(1-\gamma_d)\right)^2 \frac{\partial}{\partial \gamma_d}\left[k\kappa_{0B}(1+\kappa_{1B}^k)\theta M\right] \tag{60}$$

$$\Omega = k\kappa_{0B}(1+\kappa_{1B}^k)\theta M \frac{\partial}{\partial \gamma_d}\left[(1+\kappa_{0B}^k)\left(1+k\kappa_{1B}\gamma_d(1-F_d^B(w_B))+\kappa_{1B}(1-\gamma_d)\right)^2\right] \tag{61}$$

and

$$\Delta = 1/\left((1+\kappa_{0B}^k)\left(1+k\kappa_{1B}\gamma_d(1-F_d^B(w_B))+\kappa_{1B}(1-\gamma_d)\right)^2\right)^2 > 0 \tag{62}$$

However, because $k\kappa_{0B}(1+\kappa_{1B}^k)\theta M = k\kappa_{0B}(1+\kappa_{1B}(1-\gamma_d)+k\kappa_{1B}\gamma_d)\theta M$ we have that

$$\frac{\partial}{\partial \gamma_d}\left[k\kappa_{0B}(1+\kappa_{1B}^k)\theta M\right] = k\kappa_{0B}\theta M(-\kappa_{1B}+k\kappa_{1B}) < 0 \tag{63}$$

which implies $\Lambda < 0$. For Ω;

$$\frac{\partial}{\partial \gamma_d}\left[(1+\kappa_{0B}^k)\left(1+k\kappa_{1B}\gamma_d(1-F_d^B(w_B))+\kappa_{1B}(1-\gamma_d)\right)^2\right] \tag{64}$$

$$= (1+\kappa_{0B}(1-\gamma_d)+k\kappa_{0B}\gamma_d) \times 2\left(1+k\kappa_{1B}\gamma_d(1-F_d^B(w_B))+\kappa_{1B}(1-\gamma_d)\right)(k\kappa_{1B}(1-F_d^B(w_B))-\kappa_{1B})$$
$$+\left(1+k\kappa_{1B}\gamma_d(1-F_d^B(w_B))+\kappa_{1B}(1-\gamma_d)\right)^2(k\kappa_{0B}-\kappa_{0B})$$

so we have that

$$\frac{\partial}{\partial \gamma_d}\left[(1+\kappa_{0B}^k)\left(1+k\kappa_{1B}\gamma_d(1-F_d^B(w_B))+\kappa_{1B}(1-\gamma_d)\right)^2\right] < 0 \tag{65}$$

which implies $\Omega < 0$. However, it is still the case that $\frac{\partial l_d^B(w_B)}{\partial \gamma_d} < 0$ because $|\Lambda| > |\Omega|$;[31]

$$\begin{aligned}\Lambda - \Omega \;=\; & (1+\kappa_{0B}^k)\left(1+k\kappa_{1B}\gamma_d(1-F_d^B(w_B))+\kappa_{1B}(1-\gamma_d)\right)^2 k\kappa_{0B}\theta M(-\kappa_{1B}+k\kappa_{1B}) \\ & -k\kappa_{0B}(1+\kappa_{1B}^k)\theta M \\ & \times[(1+\kappa_{0B}(1-\gamma_d)+k\kappa_{0B}\gamma_d) \\ & \times 2\left(1+k\kappa_{1B}\gamma_d(1-F_d^B(w_B))+\kappa_{1B}(1-\gamma_d)\right)(k\kappa_{1B}(1-F_d^B(w_B))-\kappa_{1B}) \\ & +\left(1+k\kappa_{1B}\gamma_d(1-F_d^B(w_B))+\kappa_{1B}(1-\gamma_d)\right)^2(k\kappa_{0B}-\kappa_{0B})]\end{aligned}$$

This expression is positive if

$$(1+\kappa_{0B}^k)(k\kappa_{1B}-\kappa_{1B})+(1+\kappa_{1B}^k)(k\kappa_{0B}-\kappa_{0B})) < \frac{2(1+\kappa_{0B}(1-\gamma_d)+k\kappa_{0B}\gamma_d)(k\kappa_{1B}(1-F_d^B(w_B))-\kappa_{1B})}{1+k\kappa_{1B}\gamma_d(1-F_d^B(w_B))+\kappa_{1B}(1-\gamma_d)} \quad (66)$$

which is true as $(k\kappa_{iB}-\kappa_{iB}) < 0$ for $i = 0, 1$ and all other terms are positive. For the type n firms;

$$l_n^B(w_B) = \frac{\kappa_{0B}(1+\kappa_{1B}^k)\theta M}{(1+\kappa_{0B}^k)\left(1+\kappa_{1B}(1-\gamma_d)(1-F_n^B(w_B))\right)^2} \quad (67)$$

so that

$$\frac{\partial l_n^B(w_B)}{\partial \gamma_d} = (\Gamma - \Psi)\times \Xi > 0 \quad (68)$$

$$\Gamma = (1+\kappa_{0B}^k)\left(1+\kappa_{1B}(1-\gamma_d)(1-F_n^B(w_B))\right)^2 \frac{\partial}{\partial \gamma_d}\left[\kappa_{0B}(1+\kappa_{1B}^k)\theta M\right] < 0 \quad (69)$$

because

$$\frac{\partial}{\partial \gamma_d}\left[\kappa_{0B}(1+\kappa_{1B}^k)\theta M\right] = \kappa_{0B}\theta M(k\kappa_{1B}-\kappa_{1B}) < 0 \quad (70)$$

and

$$\Psi = \kappa_{0B}(1+\kappa_{1B}^k)\theta M \frac{\partial}{\partial \gamma_d}\left[(1+\kappa_{0B}^k)\left(1+\kappa_{1B}(1-\gamma_d)(1-F_n^B(w_B))\right)^2\right] < 0 \quad (71)$$

because

$$\frac{\partial}{\partial \gamma_d}\left[(1+\kappa_{0B}^k)\left(1+\kappa_{1B}(1-\gamma_d)(1-F_n^B(w_B))\right)^2\right] \quad (72)$$

[31]That is, the expression represented by Λ is sufficiently negative to overcome the effect of subtracting a smaller negative number similarly to the following arithmetic $-5-(-4)) < 0$.

$$
\begin{aligned}
&= (1+\kappa_{0B}(1-\gamma_d)+k\kappa_{0B}\gamma_d)\times 2\left(1+\kappa_{1B}(1-\gamma_d)(1-F_n^B(w_B))\right)(-\kappa_{1B}(1-F_n^B(w_B)))\\
&+\left(1+\kappa_{1B}(1-\gamma_d)(1-F_n^B(w_B))\right)^2(k\kappa_{0B}-\kappa_{0B})\\
&< 0
\end{aligned}
$$

and

$$
\Xi = 1/\left[(1+\kappa_{0B}^k)\left(1+\kappa_{1B}(1-\gamma_d)(1-F_n^B(w_B))\right)^2\right]^2 > 0 \tag{73}
$$

For $\frac{\partial l_n^B(w_B)}{\partial \gamma_d} > 0$ it must be that

$$
(1+\kappa_{0B}^k)(k\kappa_{1B}-\kappa_{1B})-(1+\kappa_{1B}^k)\left[\frac{(1+\kappa_{0B}(1-\gamma_d)+k\kappa_{0B}\gamma_d)}{(1+\kappa_{1B}(1-\gamma_d)(1-F_n^B(w_B)))}2(-\kappa_{1B}(1-F_n^B(w_B)))\right]
$$
$$
-(1+\kappa_{1B}^k)(k\kappa_{0B}-\kappa_{0B}) > 0
$$

Given $(1+\kappa_{1B}^k)\left[\frac{(1+\kappa_{0B}(1-\gamma_d)+k\kappa_{0B}\gamma_d)}{(1+\kappa_{1B}(1-\gamma_d)(1-F_n^B(w_B)))}2(-\kappa_{1B}(1-F_n^B(w_B)))\right] < 0$ this means that for $\frac{\partial l_n^B(w_B)}{\partial \gamma_d} > 0$ it must be the case that

$$
(1+\kappa_{0B}(1-\gamma_d)+k\kappa_{0B}\gamma_d)\lambda_1 > \lambda_0(1+\kappa_{1B}(1-\gamma_d)+k\kappa_{1B}\gamma_d) \tag{74}
$$

$$
\lambda_1+\frac{\lambda_0}{\delta_B}(1-\gamma_d)\lambda_1+k\frac{\lambda_0}{\delta_B}\gamma_d\lambda_1-\lambda_0-\lambda_0\frac{\lambda_1}{\delta_B}(1-\gamma_d)-\lambda_0 k\frac{\lambda_1}{\delta_B}\gamma_d > 0 \tag{75}
$$

Since $\frac{\lambda_0}{\delta_B}(1-\gamma_d)\lambda_1 = \lambda_0\frac{\lambda_1}{\delta_B}(1-\gamma_d)$ and $\lambda_0 k\frac{\lambda_1}{\delta_B}\gamma_d = k\frac{\lambda_0}{\delta_B}\gamma_d\lambda_1$ this simplifies down to

$$
\frac{\partial l_n^B(w_B)}{\partial \gamma_d} > 0 \; if \; \lambda_1 > \lambda_0 \tag{76}
$$

which is the case by assumption. Therefore

$$
\begin{aligned}
\frac{\partial l_d^B(w_B)}{\partial \gamma_d} &< 0\\
\frac{\partial l_n^B(w_B)}{\partial \gamma_d} &> 0
\end{aligned}
$$

Note that these are the equilibrium effects on single firm labor stocks at a *given* wage. That is, if there are more type d employers and a firm keeps the same wage, it hires fewer type d workers with the opposite being true for type n employers.

Separation Rates

Firm-worker separation rates are

$$\int_{r_A}^{wh_A} (\delta_A + \lambda_1(1 - F^A(w_A)))dG^A(w_A) = \frac{\delta_A(1+\kappa_{1A})}{\kappa_{1A}} ln(1+\kappa_{1A}) \tag{77}$$

where

$$F^A(w_A) = \frac{1+\kappa_{1A}}{\kappa_{1A}} - \left(\frac{1+\kappa_{1A}}{\kappa_{1A}}\right)\left(\frac{P_A - w_A}{P_A - r_A}\right)^{1/2} \quad r \leq w_A \leq wh_A \tag{78}$$

. and

$$\int_{r_B}^{wh_B} \left(\delta_B + \lambda_1(1-\gamma_d)(1 - F_n^B(w_B))\right) + k\lambda_1\gamma_d(1 - F_d^B(w_B))dG^B(w_B) = \frac{\delta_B(1+\kappa_{1B}^k)}{\kappa_{1B}^k} ln(1+\kappa_{1B}^k) \tag{79}$$

When $\delta_A \leq \delta_B$ it is possible that separation rates for type B workers to be higher. If $\delta_A = \delta_B$, separation rates for type A workers are strictly higher.

Appendix C - Provisions of the Patient Protection and Affordable Care Act

The Patient Protection and Affordable Care Act s a complex response to issues that originate with the decision to exempt fringe benefits such as health insurance from strict wage controls during the Second World War. Employer-based coverage quickly became the norm as employers substituted health care coverage for wage increases. With changing household demographics, decreases in labor force participation, and a rise in part-time employment, the Act attempts to ensure access to affordable coverage is provided to all, rather than just those who are full time employees at larger firms.

To do so, the new health care law makes many changes to the health insurance landscape in the US. While many of these changes do not directly impact the labor market, this paper is focused on changes that are forced upon firms, known collectively as the "employer mandate." The cost of not complying with this "employer mandate" may be quite large. Firms with more than 50 workers face a penalty of $2,000 per full-time employee excluding the first 30 employees for not providing coverage. This means that a firm who did not provide coverage before the new law must decide between paying costly penalties or providing costly coverage.

However, firms who already provide coverage are also given incentives to adjust their behavior. At firms who already provided some form of health benefits, the ACA raises costs on the intensive margin by mandating that all health insurance plans provide Essential Health Benefits which include items and services within *at least* the following ten categories;

1. Ambulatory Patient Services
2. Emergency Services
3. Hospitalization

4. Maternity and Newborn Care
5. Mental Health and Substance Use Disorder Services, Including Behavioral Health Treatment
6. Prescription Drugs
7. Rehabilitative and Habilitative Services and Devices
8. Laboratory Services
9. Preventive and Wellness Services and Chronic Disease Management; along with
10. Pediatric Services, Including Oral and Vision Care.[32]

In addition, plans must provide participants and beneficiaries with a uniform summary of benefits and coverage and must also comply with ACA's requirement to report the aggregate cost of employer-sponsored group health plan coverage on their employees' end of year summary of compensation (referred to as a W-2 in the US).[33,34] They must also comply with the following provisions:

- If dependent coverage is offered, coverage must be available for dependent children up to age 26
- Preventive health services must be covered without cost-sharing ("grandfathered" plans are exempt[35])
- No rescission (removal) of coverage, except in the case of fraud or intentional misrepresentation of material fact
- No lifetime limits on Essential Health Benefits (see below for explanation) *when* offered (self-insured plans do not have to offer these Essential Health Benefits)
- *overall* Improved internal claims and appeals process and minimum requirements for external review (grandfathered plans are again exempt)

[32] Note that states are given some discretion within these items. See https://www.healthcare.gov/glossary/essential-health-benefits

[33] See https://www.cms.gov/CCIIO/Resources/Files/Downloads/uniform-glossary-final.pdf

[34] http://www.ciswv.com/CIS/media/CISMedia/Documents/Self-Insured-Plans-Under-Health-Care-Reform-070312_1.pdf

[35] Grandfathered plans are those that were in existence on March 23, 2010 and have stayed basically the same. But they can enroll people after that date and still maintain their grandfathered status. In other words, even if you joined a grandfathered plan after March 23, 2010, the plan may still be grandfathered. The status depends on when the plan was created, not when you joined it. Grandfathered plans don't have to: Cover preventive care for free, guarantee the insured's right to appeal, protect the insured's choice of doctors and access to emergency care, or be held accountable through Rate Review for excessive premium increases. See https://www.healthcare.gov/what-if-i-have-a-grandfathered-health-plan/.

These plans have been subject to the recent outcry over President Obama's initial claim that "if you like your plan, you can keep it" - see http://www.usatoday.com/story/news/politics/2013/11/11/fact-check-keeping-your-health-plan/3500187/.

It is worth noting that firms who self-insure, under the provisions of the ERISA (1974), are *exempt* from providing the Essential Health Benefits.[36] In addition, self-insured plans are free from Medical Loss Ratio Rules, the Review of Premium Increases regulation, the Annual Insurance Fee, and risk sharing and adjustment charges.

[36] Kaiser Family Foundation website (accessed June 26, 2013) - http://kff.org/interactive/implementation-timeline/