ESSAYS IN EMPIRICAL LABOR ECONOMICS

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This dissertation focuses on furthering our understanding of how labor markets work. Each essay sheds light on how individual, governmental, and institutional factors combine to determine economic outcomes within specific labor markets. The first chapter uses the Affordable Care Act's employer mandate to ask if variation in health expenses at the individual level can affect labor market outcomes. Using a difference-in-difference estimation strategy the essay shows that higher cost workers face lower wages and increased unemployment as a result of the employer mandate. Specifically, each dollar of annual medical expenses is associated with a \$0.30 to \$0.40 wage penalty. The negative changes in labor market outcomes for higher cost workers highlight that firms face the same incentives as an insurer to limit access and price discriminate.

The second chapter focuses on why antebellum slave prices were higher in the deep South. Existing research suggests price differences were due to productivity differences. This essay shows that the chance of escape was a complementary source of price differences. To do so, the paper examines the consequences of the Fugitive Slave Act of 1850. The Act was designed to reduce the chance of successful escape and the paper shows it caused slave prices in northern slave states to increase by between 15 and 30 percent relative to the deeper South. In 1854, reinstated loopholes reverse the effects of the Act. These findings are backed up by advertisements for runaways collected from antebellum newspapers.

The third chapter uses a correspondence study to examine how employers view degrees earned online. Many major universities in the US have online programs that claim to be comparable to a traditional degree. The essay uses fictional resumes to apply for real jobs but randomly varies the degree held. Estimates suggest employers do not value these programs equally: After controlling for co-variates a traditional degree-holder is twice as likely to be called for interview compared to an individual with an online degree.

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1.0 THE INDIVIDUAL-SPECIFIC INCIDENCE OF MANDATED HEALTH INSURANCE

The Affordable Care Act's employer mandate creates a natural experiment which can be used to identify how variation in health care costs at the individual level affect labor market outcomes. Firms affected by the mandate have an incentive to economize along a new margin. Using data from the Medical Expenditure Panel Survey (MEPS), estimates suggest workers with higher health care expenses are less likely to secure employment at firms most affected by the coverage mandate and earn lower wages when they do. The reduction in wages is estimated to be between \$0.30 and \$0.40 in annual earnings for every \$1 difference in annual medical expenses.

1.1 INTRODUCTION

Empirical work on the incidence of health coverage shows a clear trade off between coverage and wages. For example, Gruber (1993) finds mandates requiring maternity coverage result in lower wages for those most likely to benefit. Sheiner (1999) and Jensen and Morrisey (2001) show that elderly workers face lower wages if they live in areas where health care is relatively more expensive. Lahey (2012) finds infertility coverage mandates lead to lower wages for females likely to benefit and Bailey (2014) finds that prostate cancer screening mandates were associated with reductions in wages for older males. Illustrating the same trade-off, Marks (2011) finds that higher minimum wages are associated with reductions in the likelihood of receiving employer-based coverage.

What remains unclear 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 use at the individual level?[1] The level at which cost-shifting occurs matters because individual-specific cost-shifting would undermine the

[1] Without data on individual expenditures it is not possible to separate the two effects. See Levy and Feldman (2001).

risk-pooling benefits of employer-based coverage. Groups of employees are viewed as ideal risk pools because insurers would not extend coverage to risky applicants if coverage was purchased individually. However, attempts to identify where cost-shifting occurs have struggled with two identification issues. One, 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. Two, the decision to offer coverage or to work at a firm that offers coverage is endogenous. Levy and Feldman (2001) illustrate these identification issues. They use job switchers to search for individual-specific cost-shifting and 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 note that clean identification will require exogenous variation in insurance coverage.

The Affordable Care Act (ACA) provides the exogenous variation required.[2] The Act mandates that firms with more than 50 full-time employees provide health coverage for all workers who work more than 29 hours in a usual week. This creates an incentive for many firms to economize on a previously irrelevant dimension: Employee health care costs.[3] Using data from the Medical Expenditure Panel Survey (MEPS) this paper finds that workers with higher health care expenses are less likely to secure employment at firms affected by the employer mandate and earn lower wages when they do. The empirical estimates employ difference-in-difference and triple-difference estimation frameworks and show that between 30 and 40 cents of every \$1 of medical expenses is shifted onto the employee. The size of the pass-through is considerable given (1) employee medical expenses are tax deductible and (2) the pass-through occurs before the workers actually receive any coverage. Consistent with the Act's mandate applying only to full-time employees the data also suggests some firms try to avoid the Act's requirements by reducing hours worked for employees with higher health care expenses.[4] As the estimations presented flexibly control for demographic

[2]The 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.

[3]The Act also mandates coverage that is typically more generous in terms of benefits and eligibility than before. This means that firms who already provide coverage are also be affected by the new law. As these firms are used as a control group in the empirical estimates, the more generous coverage they are required to provide should bias estimates towards zero. For more details on the specifics of the required coverage, see Appendix C.

[4]Again, for the specifics of the Act's mandates see Appendix C.

characteristics both before and after the ACA is announced the paper's findings suggest firms can and do condition wages on health care expenses at the *individual* rather than just at the *group* level.

Aiding identification, the Act gave workers little incentive to alter their behavior until January 2014.[5] 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 2014 eventually arrived.

The paper's findings show that employer-provided coverage transfers the incentive to cherry-pick from insurance companies to employers leading to lower wages for higher-cost employees or exclusion from coverage altogether.[6] This is because the cost wedge between workers caused by employer-provided health coverage is unavoidable. When underwriting coverage, each firm is treated as a single risk pool.[7] Due to firm-specific underwriting, firms face the actual cost of their employees' health care expenses. They could also choose to self-insure, taking the financial risk of expenses on themselves. In either case, they can only reduce the cost of providing a given level of coverage by cherry-picking workers who will be less costly to cover.

Ideally, firms are not supposed to be able to directly observe expenses incurred by specific workers. However, there is plenty of anecdotal evidence to suggest they easily connect the dots. For example, AOL CEO Tim Armstrong caused a firestorm on social media in 2014 when he blamed changes in pension benefits on medical costs incurred by just two specific employees.[8]

[5]Except for workers 26 and under, 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 an 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.

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

[7]Note that the ACA creates a marketplace where firms with fewer than 50 full-time employees can (but are not required to) obtain community-rated coverage.

[8]Armstrong said "We had two AOL-ers that had distressed babies that were born that we paid a million dollars each to make sure those babies were OK in general. And those are the things that add up into our benefits cost. So when we had the final decision about what benefits to cut because of the increased healthcare costs, we made the decision, and I made the decision, to basically change the 401(k) plan."

This paper is not an analysis of the Affordable Care Act itself. The Act consists of many regulatory changes and the paper uses just one of its changes to identify an effect that has previously been difficult to observe. At the same time, the paper's findings raise serious questions about the wisdom of building the ACA 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, firms who already provided insurance coverage likely behave this way, too. A broad mandate on employer-based coverage may ensure that it is impossible for some workers to be profitably employed at any firm that offers health coverage.[9] For workers with higher healthcare costs, the availability of jobs that do not provide health coverage may be crucial to securing gainful employment.

The paper proceeds with some background information on the employer mandate along with a review of the literature on employer-provided coverage and its effects on the labor market. The review highlights that evidence of individual-specific incidence has been elusive to date and illustrates the value of the identifying variation provided by the Affordable Care Act's employer mandate. Section 1.3 provides a theoretical framework to motivate the later empirical analysis. 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. Section 1.6 examines the predictions of the model in difference-in-differences and triple-difference frameworks. The data used to produce the empirical estimates is described in Section 1.4. Section 1.5 details the Act's implementation time-line and why focusing on the pre-implementation period is crucial for clean identification. Section 1.7 concludes.

1.2 BACKGROUND AND 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 the paper called for and sparked a wave of research into the empirical regularities and labor market consequences of mandated benefits. Examples include Gruber and Krueger (1991), Gruber (1993, 1994), Acemoglu and Angrist (2001),

[9] 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).

Baicker and Chandra (2006), and Baicker and Levy (2008). Baicker and Levy examine the potential for health coverage to impact low-wage earners. They find that many positions where wages are close to the federal minimum may not be economically viable if employers were forced to provide health coverage to workers, too. Baicker and Chandra leverage exogenous variation provided by medical malpractice laws across states to show that rising health care premiums reduce employment levels although the effect they find is not intended to be group or individual-specific. Acemoglu and Angrist found that the Americans with Disabilities Act (ADA) reduced labor market opportunities for workers with disabilities. This paper shows that a mandate on health coverage affects workers who would be costlier to cover very similarly.

Gruber (1993) focused on the incidence of mandated maternity benefits. He finds that wages fall, by approximately the expected cost, for the group who would benefit from coverage. Gruber's paper is exactly the type of 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. 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 suggested that wages are actually free to adjust and workers don't pay more than the actual cost of the benefit.[10]

However, data limitations and a lack of exogenous variation in coverage means that how labor market outcomes vary with *individual* health care usage remains an open question. Gruber does not have the data needed to examine if those who have multiple or complicated births face larger wage reductions as a result of their more intensive use of the maternity coverage. Similarly, the data used by Bailey, Lahey, Sheiner, and Jensen and Morrisey does not have the information on actual health care expenses necessary to determine if two otherwise-identical workers are treated differently by employers because of their health expenses. In addition, because many of the mandates used to identify the relationship between wages and coverage affect workers and firms simultaneously, it may be fair to ask if it is worker or firm behavior which causes the observed effects. Clean identification needs all else to remain equal. If, for example, workers substitute towards increased fertility at the margin after gaining maternity coverage then it is not clear that wages are falling *because* of cost-shifting at the individual level.

[10]If workers value the benefit they receive at its cost Gruber's paper minimizes concerns about inefficiencies. 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.

This paper answers the open question surrounding individual-specific cost-shifting using rich MEPS data along with the identifying variation in coverage provided by the ACA. The MEPS data contains individual health care usage and expenses but cannot be used for 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 the imposition of state mandates used for identification in the work of Gruber and others. However, surveys which gather data on wages, insurance coverage, *and* state of residence, such as the CPS, do not collect data on how much health care services an individual consumes. Such data limitations are a major reason why authors have struggled to identify the individual-specific effects of employer-based health coverage.[11] Moreover, the paper provides *cleaner* identification than the existing literature as it focuses on how labor market outcomes change at firms affected by the employer mandate in the period after the announcement of the ACA in 2010 but before its supposed full implementation in 2014. Concentrating on the period between the Act's announcement and implementation is helpful as the Act gave firms time to prepare but gave workers little incentive to change their labor supply behavior in the same period.[12] Indeed, while individuals and the media struggled to wrap their heads around the new health care laws, 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.[13] Highlighting 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.[14]

The cost of not complying with the mandate is significant. From 2014, firms with more than 50 full time 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 a penalty when firms could offer coverage while reducing

[11] The specifics of these effects 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.

[12] It is also necessary to focus on those aged 27 or older as the Act mandated that young adults up to the age of 26 were allowed to remain on parent's coverage as a dependent almost immediately. See Goda et al. (2016) for a review of the effects of that coverage.

[13] 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`

[14] For more information see this *Washington Post* news article: `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`

real wages to cover the cost. Then, at least workers would get something in return for their lower wages in comparison to having to share the burden of the financial penalties without receiving coverage. Because paying the penalty would only make sense as some kind of political protest, a firm who did not provide coverage before the new health care law can be expected to choose to provide cover (or at least to prepare for that possibility). These firms therefore have the strongest economic incentives to institute exclusionary hiring practices, reduce wages, or both.

Underlining the importance of studying the pre-implementation period, a naive approach to this question using data from 2012 to 2016 may find no effect from the employer mandate.[15] Such a finding would be erroneous because adjustments occurred before the stated implementation date of the Act.[16] Other authors, including Garrett and Kaestner (2015), Mathur et al. (2016), and Even and MacPherson (2015) argue that focusing on the pre-implementation period is important to determine how the ACA has affected part-time employment. The analysis in this paper contributes to that literature by examining if the effects of the ACA's mandates are concentrated on particular workers at firms impacted by the employer mandate.

The empirical findings presented in this paper confirm the predictions of Mitchell (1990). Mitchell 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. Mitchell's predictions are supported by a body of work showing that when an identifiable group is affected by a mandate on coverage, labor market outcomes for the group suffers. For example, 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. Thurston (1997) examines the unique experience of Hawaii. Hawaii mandated employer provision of health insurance to full-time workers in 1974. Thurston's findings are confirmed by Buchmueller et al.

[15] As mentioned earlier, confounding variation will be introduced by the distortions created by the heavily-subsidized coverage available on the Act's individual health care exchanges. These exchanges will render clean identification using the employer mandate 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.

[16] Many well-known studies of the impact of labor market policies are open to exactly the same 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 new minimum wage is in place. However, the wage increase the authors study was announced two years *before* its implementation date.

(2011). Kolstad and Kowalski (2012) examine the broad effects of Massachusetts 2006 health care reform.[17] They find that wages at firms who were forced to provide coverage fall by approximately the average cost of coverage compared to firms who already provided coverage.[18] 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, none of these papers, explain if, *within* an affected group, individuals with varying costs of coverage are affected differently.

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.[19] Pauly and Herring (1999), using the 1987 National Medical Expenditure Survey, claim to address the question of whether there is individual-specific cost-shifting. However, their terminology is loose. Their finding is still a group offset, not an individual-specific offset.

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 identification issues and provides a clearer analysis of the impact of the health coverage on individual labor market outcomes.

[17] The Massachusetts reform was viewed by many as a precursor to the Affordable Care Act and their design and implementation are quite similar.

[18] 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 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.

[19] Levy (1998) examines an additional important avenue for cost-shifting, the employee's contribution to employer-provided benefits. Levy finds that worker contributions play an important role in employee sorting and provide employer flexibility to tailor benefit packages to match workers' preferences. The role of employee contributions cannot be examined in this paper as identification relies on firms where no insurance was in place. Data on employee contributions *after* the mandate comes into effect will not be available until late 2017.

1.3 MODELING THE EXPECTED EFFECTS OF THE ACA'S EMPLOYER MANDATE

This section presents a job search model to study the general equilibrium effects of exogenously increasing the amount of firms who offer health coverage. The model is valuable because it shows there exists an equilibrium where some firms offer coverage, some don't, and yet individual workers with high and low health-care costs can find it optimal to work at either type of firm. A perfectly competitive labor market model cannot provide such an outcome. The model builds upon the work of Mortensen (1990) and, particularly, Bowlus and Eckstein (2002). Bowlus and Eckstein develop a search 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 health coverage to workers. These workers can receive wage offers from each type of firm and often the wage offered by a no-coverage firm is optimal in a given period.

For ease of exposition 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 $P_A \geq P_B$.[20]

Employers maximize utility that depends on profits determined by the number of each type of worker they employ and the wages paid to those workers. 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. However, w_B

[20]The reader can think of this increased productivity as due to less absenteeism, greater physical ability, stamina, and so on.

represents the value of wages and health coverage offered to the Type B worker.[21] 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 $\delta_A \leq \delta_B$. 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. The model proceeds by focusing on the decisions facing employers and workers separately and then solving for equilibrium.

1.3.1 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.[22] For type n employers utility is the sum of their profit times the number of each type of worker;

$$U_n(w_A, w_B) = (P_A - w_A)l_n^A(w_A) + (P_B - w_B)l_n^B(w_B)$$

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;

[21] 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. As a result, the worker is assumed to value their overall compensation at w_B whereas it costs the employer an additional amount d to actually provide that package.

[22] Note that d can always be made arbitrarily small enough to ensure that the firm receives positive utility from type B workers.

$$U_d(w_A, w_B) = (P_A - w_A)l_d^A(w_A) + (P_B - d - w_B)l_d^B(w_B)$$

1.3.2 Workers

As in standard job search models, workers choose 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 unknown reservation wage. The value of unemployment is

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

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

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

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;

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

and

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

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;

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

and

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

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.

1.3.3 Solving for 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 and (3) Utility gained by employers is maximized and equal within each type of firm, given other agents behavior. Employers' utility is modeled as additive so that 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;

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

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;

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

and the reservation wage for type A workers is;

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

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

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

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;

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

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

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

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

so that the earnings distribution for type B workers is

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

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 Appendix B.

1.3.4 Properties of Equilibrium

It is relatively easy to show that $G^A(w_A) \le G^B(w_B)$ and $r_B \le r_A$ (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.

1.3.4.1 Expected Earnings

The ratio of earnings between the two types of workers is negatively related to γ_d.[23] This 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.

1.3.4.2 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.[24] 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;

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

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

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

[23] Proof is relegated to Appendix B.
[24] The ambiguity is described in greater detail in Appendix B.

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

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

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

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

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.[25] The model's predictions regarding employment levels provide further testable hypotheses:

> **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:

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

1.3.4.3 Unemployment Rates and Duration Increased health coverage for type B workers introduces unemployment rate and duration effects. In equilibrium all employment offers are accepted and since offers are drawn from a Poisson distribution, expected unemployment durations are

$$\frac{1}{\lambda_0}$$

for type A workers and

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

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.

[25] 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.

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

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 Appendix B for completeness.

1.3.5 Gathering Empirically Testable Predictions

While abstracting from many features of the labor market, the model presented in this section 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 relative earnings will fall even further
2. They will face higher unemployment rates
3. They will search for employment longer

These general equilibrium predictions are encapsulated in Hypotheses 1 and 3. With the Affordable Care Act acting as natural experiment affecting γ_d exogenously, the hypotheses can be tested empirically using earnings, unemployment rates, and measures of unemployment duration as dependent variables in a difference-in-differences framework.

In addition, the model predicts some effects that are limited to firms who are forced to provide coverage by the Act (that is, those moving from being type n to type d). One, they will employ fewer type B workers than before (Hypothesis 2a). Two, the model indicates (Hypothesis 2b) that these employers will pay strictly lower wages to type B workers after they become type d employers. Hypotheses 2a and 2b are tested empirically by exploiting the variation in health insurance provision at the firm level. By comparing 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 coverage on outcomes at the individual level can be observed.

1.4 DATA

The empirical analysis in this paper 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."[26] A new cohort joins the survey each calendar year and respondents participate in five 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 labor market outcomes of individuals with higher coverage expenses because the MEPS provides data on the actual health care expenses of individuals.

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. That is the most recent data available that is suitable to test the hypotheses presented in 1.3. As data on health care expenditures are reported as an annual figure, the analysis cannot meaningfully exploit the panel nature of the MEPS data. Instead, the analysis relies on the third interview with each panel as an independent repeated cross-section. Further, the empirical analysis focuses on working-age adults (ages 27-55). 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 not examined as labor force participation falls after this age, potentially confounding the paper's findings.[27]

Summary statistics for the restricted sample, at firms who do and do not provide coverage, are presented in Table 1.1. Notice in Table 1.1 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. Interestingly, employer-provided coverage has fallen from covering almost 74% of the sample to under 70% of the respondents over the period and the average health expenses follow the same pattern. These are likely related patterns. Overall, the type of worker at

[26]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. See `http://meps.ahrq.gov/mepsweb/`

[27]The findings in the paper do not change significantly by focusing on those aged 27-59 or even 27-64.

firms with coverage appears to be trending towards younger, male workers in the period from 2011 to 2013 compared to the 2008-2010 period.[28]

1.5 ACA IMPLEMENTATION AND EMPIRICAL IDENTIFICATION STRATEGY

1.5.1 Implementation

When the ACA was announced in March of 2010, firms were told they would be subject to an employer mandate on coverage from January 1, 2014.[29] The mandate required firms with more than 50 full-time equivalent (FTEs) employees to have affordable coverage options in place for employees on that date. A firm with 100 part-time employees could have more than 50 FTEs depending on typical hours worked. The time-line for the implementation of the employer mandate is illustrated in Figure 1.1.

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 obtain detailed data on a firm's workforce from the employer. 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 if they wished to minimize the cost of the ACA's mandate.

The Act's implementation time-line illustrates why identification will be clouded after 2014. 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 changes due to the employer and individual mandates. In addition, comparing labor market outcomes before and

[28] 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).

[29] 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.

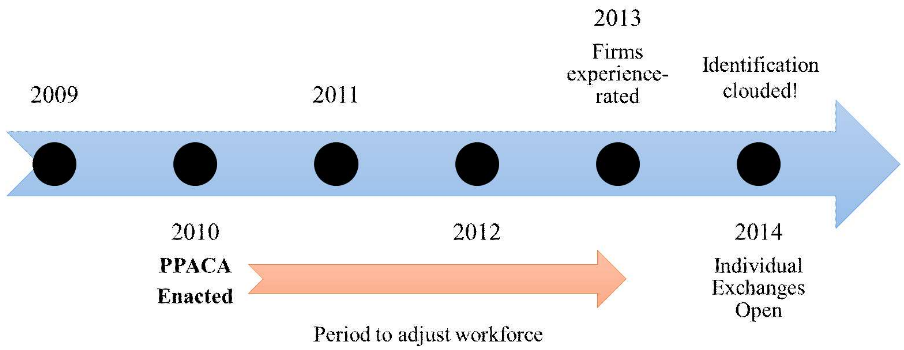
Table 1.1: Summary Statistics MEPS by Year

	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**
Proportion of Sample	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**
Proportion of Sample	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`).

Figure 1.1: The Implementation of the Patient Protection and Affordable Care Act (ACA)



after 2014 would require an assumption that firms did not prepare for or anticipate the mandate in any way. Focusing on the pre-implementation period avoids these identification problems. 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 1.6 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 face increased coverage costs, too, the results presented in Section 1.6 can be viewed as a *lower bound* on the actual effects of the mandate on workers at affected firms.

Comparing the period before 2010 to after 2010 raises concerns with how the Great 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 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 1.6 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, potentially more productive, workers, this potential source of bias will be addressed as a robustness check using MEPS data from 2006-2010.

1.5.2 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 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 much bigger impact at firms who do not provide coverage versus those who already do. In the language of the model in Section 1.3, this means that there are firms who used to be type n but who now act as if they are 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 health care expenses before and after the law but does not account for insurance coverage options. The estimating equation takes the form;

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

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 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 composed of relatively larger changes at firms who move from not providing coverage to providing coverage. To examine the consequences of the Act for workers at firms most impacted estimating equation above is adjusted to add a third difference between those who work at firms who do versus those who do not provide insurance in the sample period. The estimating equation then takes the following form;

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

The equation adds a binary indicator that is set to 1 if the observed individual works for a firm that offers health insurance ($EmployerOffersInsurance_{it}$).[30] 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. A positive sign on β_7 highlights that the Act *harms* the labor market outcomes of workers with higher medical expenses. If β_7 is positive, firms who already offer coverage before the ACA was announced tend to pay higher wages to workers with larger medical expenses than firms who do not offer coverage.

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 dependent 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. Using MEPS data and the estimating equations laid out above, estimates for the Act's effects are presented in the next section.

1.6 EMPIRICAL ESTIMATES

1.6.1 Aggregate Impacts on Annual Earnings, Hourly Wages, and Hours Worked

Based upon Section 1.3's model and the associated 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. That prediction is examined in a difference-in-differences framework, comparing the earnings of workers before and after the law change with respect to their annual health care expenses. Table 1.2 reports the results of the estimation. The estimates show no significant *aggregate* effects on labor market outcomes after the announcement of the new health care law. The first three columns consider employed individuals annual wages (in logs), log hourly wages, and an indicator for part-time work regressed on demographic controls (co-efficients suppressed for space), 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,

[30] 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.

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.[31]

In the first column of Table 1.2, 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 1.3, the Affordable Care Act only requires firms to offer coverage to workers who are full time ($\geq$ 30 hours per week). To examine if firms are avoiding providing coverage to costly workers by reducing their hours, the interaction term in column 3 reports estimates where thedependent variable is an indicator for part-time work (30 hours or less per week). There seems to be no significant changes associated with individual health care costs after the law. Together, the difference-in-difference estimates in Table 1.2 suggest workers who have higher health care expenses may be no worse off after the Affordable Care Act is announced. However, the estimates focus only on outcomes for those currently employed. As a result, if high cost workers are less likely to be observed working due to the ACA, then a composition bias may be why effects are muted.[32]

1.6.2 Disaggregated Impacts on Annual Earnings, Hourly Wages, and Hours Worked

Examining the same labor market outcomes while making use of variation in coverage provision at the firm level tells a very different story. Recall Hypothesis 2a suggested firms who were forced to provide coverage would employ fewer type B workers. Similarly, Hypothesis 2b 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 the most affected firms are reported in Tables 1.3 (using a triple-difference estimation) and 1.4 (difference-in-difference).[33]

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

[32] The no-effect finding complements Mathur et al.'s and Garrett and Kaestner's work on the Act's 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 1.4, 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 perhaps not surprising that changes which impact a small sub-group within a relatively small sample do not cause *observable* changes in aggregate labor market outcomes.

[33] As a 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, is between firms who do and do not provide health coverage.

Table 1.2: Difference-in-Differences estimation of the ACA's Aggregate Impact on Annual Earnings, Hourly Wage, and Hours Worked

	(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).

Focusing on changes in wages and hours worked, estimates from the triple-difference estimation are presented in Table 1.3.[34] Columns 1 and 2 show estimates from a specification where the 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).

The estimates presented in Table 1.3 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*-

[34]Appendix **??**presents the two difference-in-difference estimations (stratified by insurance coverage) which underpin Table 1.3. These estimations show how the *overall* effects in Table 1.3 are allocated among each type of firm.

Table 1.3: 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 1.2 and 1.3 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.

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 already provide coverage earn significantly higher wages than employees at firms who do not already provide insurance coverage. In column 1, the estimate of .0297 implies that for 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.[35] In column 4, as wages are reported in dollars, a positive unit difference in the log of health expenses is associated with a $756 difference in wages. The effect is statistically significant in columns 1 and 4 at the 5% level. Columns 2

[35] 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.

and 5 present the analogous result for hourly wages, with a unit difference in log 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 statistically insignificant increase in the likelihood of part-time work for higher cost workers at firms who already provided coverage before the law.[36] It appears from the data that firms are not attempting to avoid the mandate by shifting towards part-time workers. The CPS data used by Mathur et al. and Garrett and Kaestner cannot zero in on workers with varying expenses, nor do they stratify by employer-based insurance coverage. The estimates from Table 1.2 and 1.3 indicate 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 1.3 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 **??** for estimates 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 1.3. 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 1.3 should be viewed in the context provided by Table 1.4. In Table 1.4, 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 statistics presented in Table 1.1 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, firms that did not provide health coverage from 2011-2013 but would need to do so in 2014 are more likely to have lower-cost workers

[36]The estimates in column 3 and 6 are *raw* probit estimates rather than marginal effects. As the estimation considers many categorical variables (race, education, etc.) marginal effects as calculated by off-the-shelf programs in statistical packages are meaningless. Moreover, as the estimate is not significant, there is no value in the exercise.

compared to the same types of firms from 2008-2010. The estimates are not statistically significant but the sign of the effect is as predicted. Column 2 in Table 1.4 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 1.4: Difference-in-Differences Probit Estimation of the ACA's Individual-Specific Effects on Employment which offers Health Coverage

	(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.

1.6.3 Aggregate Impacts on Employment and Unemployment Levels and Duration

Hypothesis 3 focuses on the unemployment level and duration effects of the Act. Column 1 of Table 1.5 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, as measured crudely by the number of interviews individuals report being unemployed (not currently working, looking for work), after the Act for high cost workers is higher.

Table 1.5: Difference-in-Differences estimation of the Affordable Care Act's Unemployment Level and Quasi-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 1.5 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 these results, Table 1.2 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 1.5 suggests high cost workers are less likely to hold or get a job after the ACA. As a result, Table 1.2's findings may be because wages for those who are not working are not observed. Focusing on the differences in labor market outcomes at firms that do and do not provide coverage Table 1.3 shows that higher-cost workers will face lower wages at firms most affected by the law compared to those at firms who already offered coverage before the Act. Lastly, Table 1.4 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 1.3. The data shows workers with higher healthcare costs face significantly diminished labor market outcomes in at least two areas: they are less likely to hold any job and face lower wages when employed at firms now required to provide coverage for the first time due to the Act. The consistency and robustness (as shown later) of the sign and direction of effects provides evidence that firms can and do react to the actual health care expenses of individual workers.

1.6.4 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 1.3, 1.4, and 1.5. The first robustness check focuses on potentially-confounding differences between firms who do and do not provide coverage. At the same time, it also sheds some light on the mechanisms underpinning the effects observed earlier.

1.6.4.1 Firm Size Effects 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 1.3 and 1.4 compare small firms who do not offer insurance to small *and* large firms who do offer coverage. As a result, the "control" group in the natural experiment set-up of this paper is potentially invalid. Table 1.6 limits the sample

Table 1.6: Triple-difference Estimation of the ACA's Individual-Specific Effects on Wages and Hours Worked (Firms with 300 or Fewer 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 1.6 versus Table 1.3 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.

to workers at firms who have fewer than 300 workers.[37] The estimates are produced using the triple-difference estimation strategy detailed in 1.5. The findings in Table 1.6 should be compared to those in Table 1.3. 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 1.6. 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

[37] Similar effects are observed for 250 and 200 employee restrictions but the sample size becomes quite small.

year or about $0.30 per hour. 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 1.7 repeats the analysis of Table 1.4 but uses only workers at firms with less than 300 employees. Table 1.4 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 (that is, a z-score) measure. Using the smaller, but potentially more valid sample, the estimates become statistically significant. These estimates suggest that after the Affordable Care Act is announced, individuals with higher health care expenses are significantly more likely to work at a firm with health coverage than one without. This means higher cost workers are somehow being excluded from the firms *most* affected by the Act.[38] Each of the estimations controls for demographics and the "main" effect of having health coverage at all which is a predictably reliable determinant of higher health care expenses.

Tables 1.6 and 1.7 complement the findings presented in Tables 1.3 and 1.4. 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 1.3 and 1.4 are biased away from zero only by an inappropriate comparison group. Tables 1.6 and 1.7 also shed light on the mechanisms causing the observed effects. Smaller firms are potentially able to observe changes in health status in ways large decentralized firms are not.[39]

1.6.4.2 Common Trend Assumption Using a triple-difference approach limits the chance that the observed effects would have occurred in the absence of the ACA's mandate. Any labor market trends for higher cost workers, in general, are at least partially controlled for using the triple-difference approach. In addition, Table 1.10 shows that similar effects are not observed in the 2006-2010 period reducing the chance that it is simply a consequence of events occuring during the Great Recession. However, examining the outcomes of higher cost workers at firms unaffected by the ACA provides a third robustness check on the role broad labor market trends may be playing.

[38]Given the data used here it is not possible to examine how this exclusion is occuring. It could be on the hiring side, incentivized or targeted retirement/redundancy programs, or via promotion and advancement restrictions driving workers to leave "voluntarily."

[39]On a monthly basis, firms who offer coverage are provided details of procedures undertaken and costs incurred but employee names are not provided. However, in small firms connections between absences or an observable deterioration in health may make such privacy steps woefully inadequate. See `http://money.cnn.com/2014/02/12/news/economy/employer-health/foradiscussionofarecentrelevantinstanceofsuchconcerns.`

Table 1.7: Difference-in-Differences Estimation of Effects on Probability of Employment at Firms Who Offer Coverage (Firms with 300 or Fewer 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 1.7 and Table 1.4 is only the "small firm" sample restriction imposed.

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 1.3 and 1.6 but restricting the sample to workers who work at firms with under 50 employees provides a strong check on the causal interpretation of the estimates presented earlier. Outcomes for workers at firms who do not provide coverage but have fewer than 50 employees should be unchanged after the new health care law. Table 1.8 presents the estimates from an empirical test of that prediction. The estimation again focuses on a triple-difference term 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 firms who do not provide coverage are not forced to do so by the ACA, there should be 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.[40]

In Table 1.8, 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 interaction term estimates in columns 2 and 3 are the opposite sign compared to the corresponding estimates in Tables 1.3 and 1.6. 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 1.3 and 1.6 cannot simply explained by underlying differences between firms who do and do not provide coverage regardless of size or broad labor market trends.

In sum, the predicted effects of the ACA are *only* observed at firms heavily affected by the ACA. If the ACA was not responsible for the estimates seen in Tables 1.3 and 1.6 then a similar pattern should have been observed in Table 1.8. This robustness check provides additional strong evidence that the ACA *caused* a deterioration of labor market outcomes for higher-cost workers at firms affected by the Act's mandate.

1.6.4.3 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" period would mimic a 2009 announcement date. Such a placebo exercise diminishes the size and statistical significance of the estimates presented in Table 1.3 indicating that the effects observed very much depend upon the period *after* but not including 2010. The estimates are not presented to economize on space.[41]

1.6.4.4 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 until September 2016. Any panel-data approach would

[40] It could be argued that all firms may now have to pay less to higher high-cost workers in equilibrium.
[41] Available on request from the author.

Table 1.8: Triple-Difference Estimate For Workers at Firms Under the 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 1.8 versus Table 1.3 and 1.6 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.

need to ignore Panel 18 altogether which amounts to discarding about one-third of the available post-2010 data. As the paper's identification relies on that period, discarding this data is not practical.[42] 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 issues arise. One, similarly to Levy and Feldman, the decision to move jobs is not random. Two, even if the move

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

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 does introduce 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.[43] The composition bias and econometric issues caused by using repeated cross-section data in a difference-in-difference estimation 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 observable 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 with affected firms paying higher cost workers less, relative to low cost workers after the Act is announced. Table 1.9 presents the estimates from the propensity score matching exercise.

In Table 1.9, 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 insurance coverage and year.[44] The procedure then matches workers at firms that do and do not offer coverage in each period on observable characteristics (race, education, marital status, age, region) in order to compare "apples

[43]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.

[44]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.

Table 1.9: 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.

to apples." The estimates in Table 1.9 are based upon nearest neighbor matching where matches are allowed to be many to one with replacement. Alternative matching methods provide similar estimates.[45]

The first row in Table 1.9 suggests that high health care cost workers earned $602 *more* per year than matched low health care cost workers before the ACA at firms that do not provide coverage. After the ACA the estimate reverses sign dramatically. The matching exercise suggests high health care cost workers now earn $3,070.53 *less* than their lower-cost co-workers. In 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 1.13 later. 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 limit concerns that the earlier difference-in-difference findings were due to causal or random changes in the composition of the MEPS sample across years.

1.6.4.5 The Effects of the Great Recession As mentioned a little earlier, there could be a concern with how the financial crisis and Great Recession between 2007 and 2009 impacted the labor market for certain types of workers. The potential confounding issue would be that the recession impacted high and low cost employees at firms who offer and do not offer coverage differently.

[45] Available upon request.

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 1.3, 1.6, and 1.8. 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 1.10 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 1.10 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 1.10 should be contrasted to the significant effects observed in Table 1.3 and again in Table 1.6. Table 1.3 and 1.6 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 1.10 eases concerns that the something in question is the Great Recession.[46]

[46] 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 1.10: 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)	(2)	(3)
	Log Wages	Log Hourly Wage	Part Time <30 Hours
Offers Coverage	0.629***	0.393***	-0.860***
	(0.0572)	(0.0410)	(0.159)
After 2008	-0.0551	0.0958**	0.0862
	(0.0710)	(0.0466)	(0.155)
Offers Coverage x After 2008	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 2008 x Health Expenses	0.00806	-0.00958	0.00187
	(0.0147)	(0.00870)	(0.0254)
Offers Coverage x 2008 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 1.10 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.

1.6.4.6 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 status 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 the costs of receiving care. The type of variation that would confound the findings of the paper would require healthcare 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 healthcare cost to wage ratio *after* the ACA was announced. Moreover, Table 1.8 highlights that there was no corresponding effect observed at firms with fewer than 50 employees. If there is a mechanical explanation for the effects observed, it should be observed at firms of all sizes.

1.6.5 Other Measures of Healthfulness and Future Healthcare Costs

This paper relies on past medical expenditures to be a reliable predictor of future healthcare expenses. While research across academic fields has shown that current healthcare 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.

1.6.5.1 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. Just under 49% of the 27-55 year old sample used for the main estimates has at least one chronic condition. 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.[47]

[47] 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 1.11: 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 1.11 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 1.11 are qualitatively the same as those seen in Table 1.3. 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.

Table 1.12: 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.

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 and Fleishman et al., 2006). Table 1.12 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 present 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 coverage. Neither estimate is statistically different from zero. 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. However, the reliability of these estimates given that only 10.8% of the restricted sample report having "poor" health is questionable.

Overall, Tables 1.11 and 1.12 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 1.13).

Table 1.13: 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

This table shows the mean of healthcare expenses, the number of chronic conditions, and the number of observations reported for those whose self-reported health was each of five self-explanatory categories laid out in the first column.

1.7 CONCLUSION

The ACA's employer mandate is used to study the behavior of employers under the broader incentives provided by employer-based health coverage. The Act's pre-implementation period provides a unique opportunity to identify a 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 the cost there are reasons to be cautious regarding 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 clean identification of the individual-specific impacts of a particular type of mandated benefit: Employer-provided health insurance.

After flexibly 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. The effects observed might be best viewed as a lower bound as they would be 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, once the ACA is fully implemented identifying the effect of the employer mandate separately from the rest of the Act's provisions will be impossible.[48]

These findings should not be seen as an indictment of the ACA, but instead viewed as the consequence of a basic flaw in 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 act as an 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.

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.

[48]The effects on labor supply decisions may be much stronger than those observed by Baicker et al. (2014) in the wake of Medicaid expansion in Oregon. This is because their sample may represent a group with less attachment to the labor force to begin with.

2.0 SLAVE ESCAPE, PRICES, AND THE FUGITIVE SLAVE ACT OF 1850

Prior studies use agricultural productivity to explain the positive relationship between antebellum slave prices and distance from the North. This paper examines the risk of escape as a complementary explanation. To do so, it uses the 1850 Fugitive Slave Act as a natural experiment. The Act altered the chance of successful escape but had effects that were arguably a function of distance from the North. Using data from Fogel and Engerman's probate appraisal data-set, estimates suggest the 1850 Fugitive Slave Act increased prices in slave states closer to the North by 15% to 30% more than in states further south. These estimates are robust to changes in sample restrictions, spatial composition effects, and placebo tests on the Act's implementation date. The contention that the Act had an effect on escape is supported by a reduction in rewards offered and the frequency of advertisements for runaways from newspaper advertisements at the time.

2.1 INTRODUCTION

In the antebellum South, slaves in states closer to the North persistently sold for less than slaves located further south. Figure 2.1 illustrates the north-south price-distance relationship. The figure uses slave values from Fogel and Engerman's Probate Appraisal Data-set (1974) and plots them as a function of distance from the county of observation to the Pennsylvania component of the Mason-Dixon Line.[1] The north-south price difference observed in the figure is typically attributed to differences in agricultural productivity (Fogel and Engerman, 1974; Evans, 1962).[2] Olmstead and Rhode (2008) confirm these north-south productivity differences using hand-collected plantation-

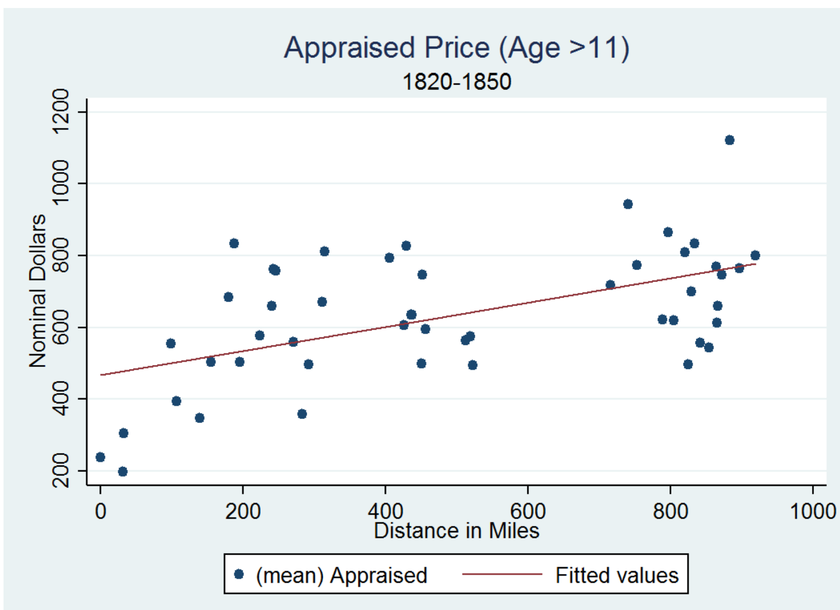
[1] As explained in Section 1.2, successful escape required crossing into Pennsylvania due to legal restrictions on Free blacks in states such as Texas, Iowa, Ohio, Illinois, and Indiana.

[2] They claim the longer growing season, increased hours of sunlight, and better soil quality boosted productivity in the deeper South.

level data. They show that slaves picked much more cotton per day in the deep South compared to the North.[3]

The agricultural productivity explanation for north-south slave price differences assumes that willingness to pay is a function of the present value of the slave's stream of marginal revenue product (marginal physical product times marginal revenue). Given marginal physical product was greater in the deeper south, this paper does not dispute the role of productivity in determining prices. Instead, it examines if slave escape was a complementary cause of the observed price-distance gradient. Escape could cause price differences to emerge because marginal physical product is zero if a slave escapes. As a result, escape could limit willingness to pay in states closer to the Free northern states.

Figure 2.1: Probate Appraisal Values by County Distance from Mason-Dixon Line 1820-1850



Source: Fogel and Engerman's Probate Appraisal Data-Set (1974). The figure highlights how average prices varied by county. The measure of distance used in the figure is the minimum distance between the most northern point of a county and closest point along the Pennsylvania portion of the Mason-Dixon Line. The lack of observations around 600-miles reflects few observations for counties in Alabama and northern Mississippi.

Clouding identification, productivity and differences in likelihood of escape are both correlated with distance from the Free northern states. Causally relating regional price differences and escape requires an exogenous change in the likelihood of successful escape that leaves productivity

[3]Olmstead and Rhode's work additionally provides new insights intoa long-running debate on the source and extent of slave efficiency ignited by Fogel and Engerman (1977).

unchanged. The 1850 Fugitive Slave Act, at least on paper, provides this kind of change creating a natural experiment that can be used to test how prices were impacted by variation in the risk of escape.

The 1850 Act closed legal loopholes, mandated Federal and State officials to assist recapture efforts, allowed bounty hunters to cross into the North to recover slaves, and imposed a fine of $500 (in 1850 dollars) on anyone who assisted fugitive slaves. The Act replaced the 1793 Fugitive Slave Act which had been nullified by a series of legislative and judicial decisions in Free states. As a result, fugitive repatriation was costly and difficult for slave-owners.

The use of the Act as a natural experiment relies on a contention that the Act had a greater effect closer to the Mason-Dixon line. That is, while the Act represents a *de jure* improvement in property rights for slave-owners, its *de facto* effects are solely an empirical concern. Indeed, many have argued escape was so uncommon that the Act was little more than political grandstanding. This paper, via a review of the literature surrounding slave escape and political events before and after the Act, shows that actual and threatened slave escape was an issue in states closer to the Free northern states. In addition, using hand-collected data from antebellum newspapers, this paper highlights a significant drop in the number of advertisements for fugitive slaves in the years following the Act.

As a result, the empirical relationship between the Act and slave prices can be viewed as causal. Estimates of the effect of the Fugitive Slave Act on slave prices are produced using the probate slave appraisal data-set gathered by Fogel and Engerman (2006). The estimation uses a difference-in-difference approach, comparing prices for slaves in two locations before and after the Fugitive Slave Act. The data-set contains appraisal values and basic demographic information for thousands of slaves in eight southern states for many years. Estimates suggest the 1850 Fugitive Slave Act was indeed a boon to slave-owner property rights in states closest to the north. The data shows a relative increase in prices in states closer to the North of between 15% and 30% compared to the deeper South depending on specification. The estimates are robust to alternate specifications, time periods, sample restrictions, and a series of placebo tests.

It is always possible that the observed effects are due to some other contemporaneous change that also happened to have a stronger effect on the price of slaves located closer to the Mason-Dixon line. Such a claim cannot be falsified with the given data. Nor can it be dealt with by appealing to variables that may explain slave prices such as crop prices (tobacco or cotton), land prices, or labor rental rates as these are endogenously determined.

Instead, the contention that the Act caused the observed changes in the price-distance gradient is further supported by an analysis of pre-trends before the Act was announced along with reward prices from the advertisements data mentioned earlier. In addition, the paper appeals to a second instance of regulatory change affecting the likelihood of escape. Despite initial support for the Act, Free states implemented new Personal Liberty laws protecting fugitives who escaped. The data shows that these laws, which make increase the chance of successful escape, reverse a significant portion the Fugitive Slave Act's effects on prices.

Finding that property rights matter will surprise few. However, scholars have argued that the Fugitive Slave Act was neither necessary nor relevant. They argued slave-owners' property rights were already strong because relatively few slaves escaped before or after the law. This restricts slave agency by implying they could not use the threat of escape to their benefit. Instead, because the Fugitive Slave Act had a large impact, it shows property rights were weaker than previously thought and that the Act was not merely a perfunctory nod to Southern interests. Such a finding is consistent with slaves having complete agency. Moreover, the dismissal of the Act in the existing slavery literature is puzzling. If slave-owners' property rights were strong, slaves didn't escape, and slave-owners' did not worry about losing valuable assets, why would not one, but two Acts of Congress designed to limit successful escape be required?

Section 2.2 explains the content and development of the 1850 Fugitive Slave Act and presents slavery as an economically rational institution. To do so, the paper examines the relevant literature on slave profitability, escape, and inter-regional trading. Applying economic analysis to the market for slaves only makes sense if economic forces were at play.[4] Section 2.3 describes Fogel and Engerman's probate appraisal data set while Section 2.4 presents estimates of the Act's impact on prices. Section 2.4 also explores the effect of stronger Personal Liberty laws enacted from 1854 onward in Free states in an attempt to undermine the 1850 Act.[5] Section 2.5 considers the robustness of these estimates and presents additional data on runaway frequency and rewards gathered manually from newspaper announcements in southern cities before and after the Act. Section 2.6 concludes.

[4]It is necessary to establish that slavery was profitable, prices reflected economic conditions, and slaves used the few bargaining chips they had at their disposal to ensure economic analysis is appropriate.

[5]Curiously, many Northern states supported the Act, issuing declarations of affirmation and promises of enforcement. These appear to have been politically motivated in order to dampen talk of secession. See Section 2 for more details.

2.2 DID ESCAPE MATTER?

Arguing that slave escape mattered contradicts much of the existing literature. For example, Geyl (1951) and McPherson (1988) argue that the Fugitive Slave Act was little more than political grandstanding. Geyl claimed "[s]outherners clung to the law because they desired to have from the North an acknowledgment of their right rather than because of the material advantage." However, the evidence offered to support such a claim is questionable. Firstly, the frequency of runaways was estimated using census data. The 1850 and 1860 censuses asked slaveowners how many of their slaves were currently fugitives (see Hummel and Weingast (2006) for more details). The census data is unreliable because it is a self-reported point-in-time estimate rather than an objective cumulative estimate of how many slaves have ever run away. More importantly, the data is uninformative as it does not consider the effect of the *threat* of escape. If the threat of escape forced slave-owners in the Upper South to treat slaves less harshly and required additional private resources to be used for monitoring and security, escape could play a major role in the determination of market prices without any escapees being observed, at all.

Hummel and Weingast (2006) and Freehling (1990), stand out as rare examples of discord. Freehling argues that runaways would be a greater threat for slaveholders in states bordering the North.[6] Hummel and Weingast agree with Freehling and demonstrate that runaways were much more common in *border* states. To do so, they calculate the ratio of runaways to slave population by state. Their approach illustrates runaways were more likely in states bordering the North.[7] Hummel and Weingast highlight that Delaware, Maryland, Virginia, Kentucky, and Missouri combined to account for more than half of all runaways listed in the 1850 and 1860 censuses.[8] These five states contained less than a fourth of the total slave population in 1860. However, even if productivity was equal *and* census data showed runaways were equally prevalent in the Deep and Upper South, the threat of escape could still cause the observed price differences. This is because, in the spirit of

[6]Freehling also contended that the vulnerability of border state slave-owners contributed to a retreat of slavery toward the Deep South and also created a powerful special interest group who demanded Northern states comply with the Fugitive Slave Acts of 1793 and 1850.

[7]There are a number of caveats to Hummel and Weingast's approach. If slaves were more valuable in the Deep South (due to productivity differences) then we might expect more resources to be devoted to recapture in the Deep South. Greater monitoring, both public and private, in the Deep South would ensure runaways in the deep south remained fugitives for shorter periods than in border states. In other words, the data used by Hummel and Weingast to suggest more slaves ran away in border states may simply reflect differences in the time-to-recapture in the two regions rather than differences in the frequency of runaways.

[8]Hummel and Weingast consider Kentucky and Missouri as being next to "Free" states but do not appear to be aware of the Black laws enacted in states such as Iowa, Ohio, and Indiana excluding free blacks from entry to those states.

Fogel and Engerman , a slave-owner who lived a few miles from the Pennsylvania border may have to treat a slave differently compared to a slave-owner in Louisiana, all else equal. The literature has not adequately examined the relationship between slave-owner and slave through this lens and has therefore missed how the potential for escape affected prices in the various regions of the South. Put simply, a border state slave-owner concerned about runaways would take steps to ensure staying was the relatively more attractive option (via the stick of increased monitoring or the carrot of concessions). The Fugitive Slave Act significantly altered the terms of this implicit negotiation by reducing the chance of successful escape for a slave.

A 1793 Fugitive Slave Act should have protected slaveowners' property rights. However, the Act failed to determine whether state or federal officials were responsible for the return of escaped slaves (Rosenberg, 1971). Free states pounced upon this ambiguity and undermined the Act with what were known as "Personal Liberty" laws ensuring that a slave who made it across the Mason-Dixon line would rarely be sent back to the South.[9] In response, an enhanced Fugitive Slave Act made its way through Congress as part of the Compromise of 1850. The Compromise admitted California to the Union and created a Free state majority. The Fugitive Slave Act of 1850 was a concession to Southern interests to compensate for the new imbalance.[10] It allowed slave-owners to hire bounty-hunters to recover runaways, mandated state *and* federal officials to assist slave-owners in recovering fugitive slaves, and levied harsh punishments for interfering in re-capture. The Act also closed all of the loopholes which had undermined the first Fugitive Slave Act. Moreover, despite the Act being a response to the behavior of Free states, northern legislatures initially supported the Act. Strother (1962) reports how in February of 1851 Democrats in Hartford, Connecticut announced "[t]hat we hold in undiminished veneration the Constitution of the United States - that we will abide in good faith by all its Compromises - and that we have no sympathy with those who, to evade its provisions, appeal to a "higher law" that teaches discord and disunion, and sectional hatred, and the violation of that Constitution under which this country has arrived at its present greatness and power."

[9]Within this institutional reality, the Underground Railroad helped thousands of slaves escape to the North many years prior to President Lincoln's emancipation proclamation. The Underground Railroad gained its name by using the language of the railroad rather than because it used a specific method of transportation. Fugitive slaves would find safe harbor at "stations" which were run by "stationmasters." Financial supporters were "stockholders, and a "conductor" moved fugitives from one station to the next. For further details on the Underground Railroad, see Snodgrass (2008), Still (1968), or Blockson (1987).

[10]Again, the fact the Fugitive Slave Act was viewed as political grandstanding - given it was the countervailing concession which allowed a Free state majority to occur *for the first time* - is surprising. In addition, the Compromise ensured there would be no federal restrictions on slavery in the territories of Utah and New Mexico. See `http://www.ushistory.org/us/30d.asp` for more details.

Despite initial support, enforcement of the Act caused conflict, particularly after President Franklin Pierce took office in March of 1853. Pierce took a heavy-handed approach to fugitive slaves such as Anthony J. Burns. The events surrounding Burns' recapture (along with other similar cases as explained by von Frank, 1998) advanced the abolitionist political agenda and lead to enhanced Personal Liberty laws in Northern states. In 1854, Connecticut reinstated protections for fugitive slaves via new Personal Liberty laws. The laws nullified many of the provisions of the 1850 Fugitive Slave Act.[11] Connecticut was joined by Rhode Island later in 1854. Massachusetts, Maine, and Michigan followed in 1855 while Wisconsin, Ohio, and New Hampshire passed similar laws in 1857. Vermont was the last to pass a Personal Liberty law in 1858 (see Hur, 2012 for an in-depth treatment of these laws). Connecticut's Personal Liberty law of 1854, because it created a new safe haven for fugitives, increasing the likelihood of successful escape, can be exploited as a *reverse* experiment.

The Fugitive Slave Acts of 1850 and 1793 along with Northern states' legislative responses could be taken alone as sufficient evidence that escape was a significant issue. Providing further evidence, authors such as Deyle (2005) explored the concerns individual slaveholders had about escape. A particularly enlightening passage taken from correspondence between Thomas Copes of Illinois and his brother Joseph, who lived in Mississippi, reads;

> "The sole object in disposing of [the slave] is the danger of loosing [sic] him here. We are on the edge of the state of Illinois, and [slaves] can make their escape across that state to Canada. And do do it every day."[12]

Campbell (1989), focusing on slavery in Texas, highlights the dilemma of a slave-owner close to Mexico. Slave-owners, such as a certain "N.B. Hawkins," were afraid to "chastize" slaves as they were "right on the line where they could cross into Mexico and be free" (pp. 179-180). In addition to the direct loss associated with escape, Deyle provides evidence to show that slaves regularly used escape as a bargaining chip to their advantage across all slave states. He highlights that the threat of escape was one of the main ways slave families managed to remain together. Given any hint that

[11] Johnston (1884) notes that the new Personal Liberty laws generally "prohibited the use of the state's jails for detaining fugitives; provided state officers ... to act as counsel for persons alleged to be fugitives; secured to all such persons the benefits of *habeas corpus* and trial by jury; required the identity of the fugitive to be proved by two witnesses; forbade state judges and officers to issue writs or give any assistance to the claimant; and imposed a heavy fine and imprisonment for the crime of forcibly seizing or representing as a slave any free person with intent to reduce him to slavery."

[12] Thomas P. Copes to Joseph Copes, Oct. 31, 1846, Copes Papers, Tulane University Library: Special Collections. More information available at `http://specialcollections.tulane.edu/archon/?p=collections/findingaid&id=736&q=&rootcontentid=125323#id125323`.

they may be separated by sale, such as a visit from a slave trader, Deyle reports that slave families responded by escaping or with threats of violence.[13]

Curiously, the route of escape for slaves in states such as Louisiana, Alabama, and Georgia was almost always to proceed north, rather than south to Mexico. This is because Texan law and institutions were particularly unfavorable to slaves attempting to runaway to Mexico. In 1846, the Texas legislature created an incentivized patrol system granting slaveholders power to search places suspected of harboring escaped slaves. The rewards for capturing escaped slaves were divided among patrol members and "paterollers" became feared by slaves. For slaves who were not indentured in Texas, the long journey through Texas from other states would have been close to impossible because "free persons of color" were prohibited from entering the state in 1840. Under the law, a slave who wanted to escape to Mexico would be re-enslaved immediately in Texas, if caught, *regardless* of their actual status.

In addition, states such as Ohio, Indiana, and Illinois, while technically Free, were not slave-friendly. Each had laws ensuring a free slave could not enter the state (see Farnam, 1938) by requiring free blacks to produce documents proving that they were not enslaved and a good behavior bond. Ohio's "Black Laws" enacted in 1804 and 1807, required a bond of $500, a prohibitively large sum for the time. Similar laws came into effect in Illinois in 1819, 1829, and again in 1853. In Indiana, such laws were enacted in 1831 and again in 1852. Michigan, Iowa, and Oregon also had laws effectively prohibiting persons of color from entering the state. As a result, the only non-slave states where an escaped slave could actually reside were in the Atlantic north-east. Because these states are reached most easily by crossing the Mason-Dixon line along the southern border of Pennsylvania, it is distance from that notional location that is use to examine the effects of the Fugitive Slave Act.

Given that Deyle finds evidence that sales occurred in order to minimize the chance of loss from runaway slaves and that both Campbell and Deyle report instances where slaves used the threat of escape to improve their situation it is easy to imagine that escape concerns might explain a portion of the difference between prices in the Upper and Deep South. Despite this, price differences between areas have been ascribed to productivity differences. Of course, any price gap between regions would be expected to close due to trade. However, the risks associated with moving slaves in the 19th century were not trivial. Transportation was so arduous that it had to be managed by specialized

[13]Deyle notes that slave traders would place advertisements in newspapers highlighting their discretion and how their unique appearance would not raise suspicion that they were a slave trader.

slave traders. These traders traveled to the Upper South, purchased slaves to form a "lot," and made their way back to the deep South with the slaves connected by chains in a "coffle." Daily progress was painstakingly slow: coffles frequently featured 100 or more slaves chained together and it took "7 to 8 weeks to travel from the Chesapeake to Mississippi in good weather" (Deyle, p. 99). On a daily basis, success and safety were threatened not only by abolitionists, theft, and the elements, but also by the risk of slaves engineering their own escape or becoming violent. In addition, before and during their departure to the south slave traders expenses would be significant. Slaves who were to be transported had to be housed in pens and the slave trader had to finance food and lodging for the slaves and wages paid to their employees during the journey south.[14]

Due to the challenges of moving slaves southwards, it is not surprising that the price of slaves in the Upper South were permanently lower than prices in the Deep South. The movement of slaves was laborious, financially risky, and physically dangerous to both the trader and to the slave. The gains to those who successfully managed to transport slaves southward were significant, but the costs were not trivial. The Fugitive Slave Act itself would have had an ambiguous effect on the quantity of slaves transported between regions. On one hand, price increases in border states may make transporting slaves less attractive. On the other, stronger property rights may have reduced the risks borne by the trader when moving slaves southward.

This paper relies on a sophisticated response to the Fugitive Slave Act by participants in the slave trade, including enslaved individuals. If slavery was irrational and unprofitable it may be fair to question if the participants can be relied upon to change their behavior in response to economic incentives. Many early slavery scholars maintained that the institution was indeed unprofitable, inefficient, and barbaric.[15] Phillips (1918) provided early empirical evidence on profitability. He gathered data from plantation documents, probate records, and bills of sales, and provided quantitative evidence in support of his propositions. Based upon this work, Phillips claimed (p. 391-392) that;

> "by the close of the [eighteen] 'fifties it is fairly certain that no slaveholders but those few whose plantations lay in the most advantageous parts of the cotton and sugar districts and whose managerial ability was exceptionally great were earning anything beyond what would cover their maintenance and carrying charges."

[14] Many larger slave trading operations eventually formed and used railand sea to transport slaves quicker albeit at increased cost. Again, see Deyle, p. 106-111.

[15] The idea of slavery being unprofitable stretches back to the principles of liberty espoused by Adam Smith in 1776. Smith's argument was that the work of free men "comes cheaper in the end than the work performed by slaves."

Flanders (1930) claimed that "[i]t cannot be denied that slave labor was expensive and inefficient." Bancroft (1931) was less certain the enterprise was unprofitable but was convinced that the institution was barbaric.[16] However, modern econometric methods have shown the slave trade, prices, management, and ownership to be quite rational and profitable even if it was perhaps not "optimal."[17] Controversially, some authors have claimed that slavery was not as barbaric as Bancroft maintained. The first of these claims appears to have been made by Conrad and Meyer (1958) who compile evidence on the costs of maintenance and purchase prices, and compare slavery to alternative investments such as bonds and stocks. They conclude that slavery was profitable for the whole South rather than just pockets as claimed by Phillips. They also suggest the institution was stable rather than self-destructive (as claimed by Bancroft) and that slave territories would have expanded in the absence of the Civil War rather than collapsing. Lastly, Conrad and Meyer suggest if the profitability or suitability of cotton fell, slaves would simply have been put to other work.

In *Time on the Cross*, Fogel and Engerman furthered these claims. Combining census data, transcripts of oral interviews, and plantation records they discover slaves' material lives were quite similar to free laborers.[18] Morally repugnant practices such as slave "breeding" were found to be uncommon with slaves regularly being allocated plots, encouraged to maintain families, and given significant plantation responsibilities beyond manual labor. The approach and findings of Fogel and Engerman and others such as Kotlikoff (1979) who examine slavery in an economically rational light seems to have struck a cord.[19]

Indeed, Whaples (1995), in a survey of areas of agreement (and disagreement) among Economic Historians, found near *unanimity* among members of the profession that Slavery was not a system

[16]Via correspondence with those who were actively involved in the slave trade he attempted to expose the underbelly of the slave trade. Bancroft detailed the "breeding" and "rearing" of slaves for future sale. Bancroft also discredited the notion that traders were social outcasts. Instead, he found that major traders were part of the highest classes of Southern society.

[17]Evans (1962) explains that the long-running argument over the profitability of slavery in the antebellum South was clouded by the difficulty in establishing what counted as profit, what expenses a slave-owner incurred over the life of the slave, how slave labor compared to freely provided labor, and how the market for cotton determined the demand for and thus the price of slaves. By clearly defining the costs and benefits to slave ownership, and examining data on slave markets and the returns to alternative investments, Evans argued that the rate of return on expenditure on slaves was more than adequate and that slavery was booming right up until the Union army prevailed in the American Civil war.

[18]They calculated that between "the value of housing, clothing, food and other benefits received by the slaves", the "slave field hand received approximately ninety percent of the income produced." Fogel and Engerman's mathematically rigorous approach and their associated findings were highly controversial and Gutman (1975) was one of many to critique Fogel and Engerman's methods, data collection, and conclusions.

[19]Kotlikoff, using data on slave auctions in New Orleans, focused only on how slave prices were determined rather than directly on the profitability of the institution and concluded that "[t]he pricing of slaves in New Orleans suggests a highly competitive and economically 'rational' market differing in few respects from a market in live stock."

irrationally kept in existence by plantation owners and that the slave system was not economically moribund on the eve of the Civil War.[20]

Lastly, the literature has hesitated to exploit the regulatory changes brought about by the Fugitive Slave Act (and the subsequent Personal Liberty laws). This is odd only because so many authors have attempted to "rationalize" slavery and the Fugitive Slave Act of 1850 provides an ideal test for rational behavior by all parties to the institution. The fact that slave-owners clamored for a Fugitive Slave Act which reinforced the institution suggests slaves were exploiting any advantages that they could: They had agency. They were not inhuman creatures, unable and unwilling to act in their own interest, but instead were resourceful, rational, and tenacious in the face of extreme adversity.

2.3 PROBATE APPRAISAL DATA

The Inter-university Consortium for Political and Social Research (ICPSR) hosts a data-set of probate-related slave sales and appraisals that took place from 1775 to 1865 in eight states: Virginia, Maryland, North Carolina, South Carolina, Louisiana, Tennessee, Georgia, and Mississippi.[21] Virginia, Maryland, and North Carolina are considered the Upper South due to their proximity to the Free states. The remaining states are considered as the Deep South. The consequences of this subjective division are examined later in the paper. In total, 76,785 records from 1775-1865 appear in the data digitized and first used by Fogel and Engerman. The data-set documents slaves' locations (county and state), sale or probate appraisal values, as well as age, sex, skills, and sometimes their health. There are records for 43,670 males and 32,726 females. 389 records where the sex of the slave was not recorded or was unknown were dropped completely from the analysis.

As this analysis is focused on the effect of the 1850 Fugitive Slave Act in a difference-in-difference framework, the focus is on the period immediately before and after the Act (8 years from 1846 to 1853 inclusive). This restriction identifies over 14,000 probate records. While the majority of slaves

[20] Moreover, the behavior of individuals undermines the argument against the profitability of slavery. First, slaves were traded for considerable amounts in what appears to be a very active and well-organized market for many decades (if not centuries). Second, planters expended significant resources and managed to have Federal laws enacted to help them re-capture escaped slaves. Thirdly, Southern planters were willing to secede from the Union and fight a civil war to maintain the institution. It would be surprising if even one, never mind all, of these events occurred in the absence of an unprofitable institution.

[21] `http://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/07421/version/3` The data are a digitized version of physical records on deposit at the Genealogical Society Library of the Church of Jesus Christ of Latter-Day Saints in Salt Lake City, Utah.

were appraised for probate purposes, many have no appraisal record. For some of these, there is a listed sale price which can potentially be used as a substitute for appraisal value. For the 179 records that have neither a sale nor appraisal value, the record was dropped from the analysis leaving just over 13,000 records.[22]

In addition, a number of observations were eliminated due to a "defect." At least 30 different defects were reported in the data. The claimed defects range from being a "girl", a "fellow", an "orphan" or being "small" to having cancer or being deaf. To avoid making a judgment on which of these defects should be considered valid they are all eliminated. Lastly, only those who were 10 or older at the time of appraisal are considered because the appraisal of children is not likely to represent meaningful information.[23] The proportion of small children in the sample is consistentwith the demographics of the slave population on the eve of the civil war. Few slaves lived into old age and women birthed many children, many of whom did not make it to adulthood. In 1860, almost half of the black population (of which the majority were slaves) were under 16 years of age.[24] Table 2.1 presents summary statistics for the remaining observations by state and region for males and females. North Carolina appears as an Upper South state in the table despite appearing more similar to its neighbors South Carolina and Tennessee.[25]

In Table 2.1 there are relatively few slaves in the Upper South who meet the sample selection criteria. There is a similar ratio of male to females in the two areas but Upper South slaves appear to be younger, in general, than those in the Deep South. Census records indicate that in 1850 there were a total of 834,921 slaves living in Maryland, Virginia, and North Carolina. In the states of South Carolina, Louisiana, Tennessee, Georgia, and Mississippi, there were 1,106,163 slaves. As a result, the 7:1 ratio of observations is well in excess of the population ratio and raises concerns about the representativeness of the data There are many plausible explanations for the observed ratio. First, Fogel and Engerman (among others) note that the largest plantation operations were in the Deep South. The number of slaves subject to probate appraisal upon the death of an average slave-holder is dependent upon the size of their plantation. As the largest plantations were in the

[22]One extreme outlier, with a reported value of $525,000, was also eliminated as all others had an appraised value of less than $2,000.

[23]Numerous states had laws prohibiting the separate sale of slaves under 10 years of age (see Deyle, p. 52). Moreover, at such a young age, successful escape was likely not a consideration for a child. The appraised value of the child was also likely to be hard to separate from that of the child's parent. As Deyle notes, it was the case that "young children were more of a liability than an asset."

[24]Based on the 1% 1860 census extract, available at IPUMS (see Ruggles et al., 2010).

[25]The subjective classification of North Carolina as Upper South suggests a natural robustness check would be to examine the effect of excluding North Carolina from the analysis completely or at least considering it as part of the Deep South.

Table 2.1: Summary Statistics by State 1846-1853

State	Observations	Rel. Freq	% Male	Age (Female)	Age (Male)	Price (Female)	Price (Male)
North Carolina	275	20.0%	61.8%	22.9	24.2	$ 500.66	$ 673.73
Maryland	790	57.4%	57.3%	27.8	27.9	$ 308.80	$ 437.04
Virgina	312	22.7%	60.3%	22.9	28.2	$ 443.06	$ 522.50
Upper South Weighted Total	1377	100.0%	58.9%	25.7	27.2	$ 377.54	$ 503.67
South Carolina	465	5.7%	60.9%	28.1	26.7	$ 436.87	$ 606.29
Louisiana	5479	66.8%	59.8%	27.9	30.4	$ 503.06	$ 672.84
Tennessee	30	0.4%	66.7%	19.9	26.0	$ 526.30	$ 611.35
Georgia	860	10.5%	54.0%	28.2	28.3	$ 469.70	$ 640.99
Mississippi	1368	16.7%	53.1%	29.1	29.7	$ 554.04	$ 682.05
Deep South Weighted Total	8202	100.0%	58.2%	28.1	29.8	$ 504.40	$ 667.04

Source: Fogel and Engerman's 1974 Probate Appraisal Data-Set (ICPSR, 2006).

Deep South, more slaves would be appraised in probate records from that area. Second, while arguing about the exact number who were moved, the existing slave trade literature indicated many slaves migrated south *with* their owners. Given that the scale of operations was shown to matter for the profitability of a plantation, that the Deep South would lure slave-owners who had a relatively large number of slaves. Thirdly, the relationship between the number of slaves owned and the age of the slaveholder can be expected to be at least weakly positive. If the lure of the south was strongest for relatively larger (in terms of slave-holdings), older slave-owners then slave-owner deaths and associated probate records will be stacked towards the Deep South. Slave-owners who are older than those who *do not* move will simultaneously cause more probate records to appear in the Deep South and fewer in the areas that they moved from. The potential relationship between the age of slaveholders and migration, the scale advantages of larger plantations, and the already larger slave population in the Deep South combine to explain why the number of slaves observed in probate records does not reflect the distribution of the slave population.

Despite concerns about representativeness, one benefit of using probate records is that the records are less likely to be affected by the selection effects examined by the Choo and Eid (2008) and Greenwald and Glasspiegel (1983). Greenwald and Glasspiegel explain how slaves sold were adversely selected. Choo and Eid examine an Alchian and Allen (1964) explanation for price differences based upon origin. According to Alchian and Allen, slaves with highly valued characteristics would be more likely to be shipped from farther away regions (which could cause selection effects over time, even in probate data).[26] Choo and Eid found no support for this theory in their data suggesting that slave prices in Maryland or Virginia are not lower simply due to selection effects.

However, the reliance on appraisal rather than market-determined prices is potentially concerning as only prices determined in a market can be expected to efficiently reflect all available information.The data-set does report a *sale* price for close to 10% of observations. Using a ten-year window around 1850 (five years each side), a regression of appraisal/sale prices on observable characteristics and an indicator for a "sale" shows that sale prices and appraisal prices in the Upper South are not different. That is, the coefficient on the "sale" indicator is not statistically different from zero. However, sales in the Deep South show tend to be for prices higher than than appraised slaves (all else equal) during the ten year period.[27] However, the informativeness of such an exercise is questionable. Is a slave who is sold likely to be representative of slaves who are observationally similar in the sample? What theoretical relationship, if any, would there be between being sold and changes in market prices or political events? Would the relationship be expected to be orthogonal or the same in all locations at all times? Ultimately, even though the data suggests there is no difference in sale and appraisal prices in the Upper South (but some evidence of a difference in the Deep South) it is not clear that inference can be made on a broader relationship. The fact that sale prices in the Deep South were higher than the typical appraisal value is not itself evidence that the appraisal prices are lower than the market price unsold slaves would command. Similarly, *observing* no difference in the Upper South does not mean appraisal prices there are error-free.

2.4 IDENTIFICATION AND ESTIMATION

Using the data described in Section 2.3, a difference-in-difference approach can used to determine how the risk of slave escape contributed to regional slave price differences. The identification strategy relies on the Fugitive Slave Act of 1850 affecting states closer to the pro-abolition North differently to those in the Deep South and requires no other forces to be acting on slave prices in a similar spatial manner. The question central to this paper is, given the changes brought about by the Fugitive Slave Act, does the price gap between the regions *actually* fall? If not, then the

[26]The Alchian-Allen theorem states that if substitutable goods of differing quality have a fixed transportation cost, the higher quality item will be relatively cheaper after transport than before.

[27]Similar patterns can be observed in the broader sample from 1820-1860. These estimates are all available from the author upon request.

risk of escape was likely not a determinant of north-south price-distance gradient. The estimating equation is of the form;

$$slave\ price = \beta_0 + \Pi X + \beta_1 D_1(1 = after\ 1850) + \beta_2 D_2(1 = Upper\ South)$$
$$+ \delta D_3(1 = after\ 1850 \times Upper\ South) + \epsilon$$

Here, Π is a vector of coefficients $\pi_1, ..., \pi_n$ corresponding to the effect of individual characteristics $x_1, ..., x_n \in X$. The difference-in-difference estimator $\hat{\delta}$ represents the differential effect of the Fugitive Slave Act on slave prices in states close to the Mason-Dixon line where it coincides with Pennsylvania's southern border. Identification using a difference-in-difference approach requires an assumption that there would be parallel trends across the South in the absence of the Fugitive Slave Act. However, violations of such an assumption are possible in either direction. On one hand, slave prices in states closer to the North may have fallen relative to those in the Deep South in the absence of the 1850 Fugitive Slave Act: Changing attitudes towards slavery combined with manumission and abolition movements may have made the slave trade less attractive the Upper South. This would bias estimates towards zero, working against finding any significant effect of the Act.

Alternatively, the assumption of parallel trends could be violated in ways that bias the empirical estimates away from zero. In particular, other factors (such as the value of commodities produced using slave labor) that affected slave prices in a specific place at a particular time could be varying in such a way as to render the effects attributed to the Act as spurious. Robustness checks will attempt to address these concerns as carefully as possible but directly accounting for such events poses endogeneity problems: If the Fugitive Slave Act caused slave prices to rise, then prices of commodities that were produced using slave labor would rise, all else equal. Any increase in commodity prices would be relatively swift as supply curves reflect all opportunity costs of production including holding onto rather than selling a slave whose sale price increases.[28]

2.4.1 Main Empirical Estimates

Table 2.2 presents OLS estimates which use the log and dollar price of a slave as the dependent variable in alternate columns using the estimation strategy laid out earlier in this Section. Columns

[28]With data observed at a higher frequency, tests for Granger causality would separate the two effects but with such a short time period and only yearly observations it is not feasible.

3 and 4 omit slaves who have a "sale" price as described earlier. As can be seen, their omission or inclusion has an effect but (as discussed earlier) there is no way to determine if sale prices actually provide information on the accuracy of appraisal values. In the estimation presented, data is limited to 4 years either side of Fugitive Slave Act. The four years before January 1, 1850 are considered as before treatment and the four years from January 1, 1850 to December 31, 1853 as after treatment.[29]

Table 2.2: OLS Diff-in-Diff Estimates for Full 1846-1853 Sample - Males and Females 10 and older

	(1) Log of Slave Price	(2) Appraised Slave Price	(3) Log of Slave Price *excl. Sale Prices*	(4) Appraised Slave Price *excl. Sale Prices*
Male	0.290***	148.7***	0.285***	140.8***
	(0.01)	(3.97)	(0.01)	(3.81)
Age	0.0692***	22.21***	0.0696***	22.41***
	(0.00)	(1.20)	(0.00)	(1.16)
Age Squared	-0.00129***	-0.395***	-0.00129***	-0.394***
	(0.00)	(0.02)	(0.00)	(0.02)
Upper South	-0.501***	-188.6***	-0.535***	-196.0***
	(0.02)	(5.62)	(0.03)	(5.74)
After Fugitive Slave Act (FSA)	0.223***	131.9***	0.208***	119.1***
	(0.01)	(4.67)	(0.01)	(4.33)
Upper South x After FSA	0.254***	69.31***	0.312***	93.61***
	(0.03)	(8.93)	(0.03)	(9.07)
Observations	9,579	9,579	8,681	8,681

*** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.

The estimates in Table 2.2 indicate that the effect of the Fugitive Slave Act (labeled FSA) on slave prices in the Upper South was statistically and economically significant. The first column of the table reports the difference-in-difference OLS estimates using the log of slave price. The column suggests prices were 50.1% lower in the Upper South across the period with a 22.3% rise everywhere after the law was enacted. The difference-in-difference coefficient of interest (Upper South $\times$ After Fugitive Slave Act) corresponds to a 25.4% additional increase in prices in the Upper South compared to the Deep South after the Act.[30] The second column of Table 2.2 uses

[29] The new law did not come into effect until September of 1850. Considering the treatment date to be from January 1st 1850 allows for anticipated effects to preempt the law. Moving the "treatment" date to 1851 (as the data is not stratified by month) will be a robustness check on the data.

[30] Coefficients in a log-linear model indicate that for a unit change in the independent variable there is a $100 * \beta\%$ change in the dependent variable.

the level of the dependent variable (in dollars) and suggests that prices in the Upper South were \$188.60 lower than in the Deep South. After the Fugitive Slave Act, prices rose across the South by an average of \$131.90 but by an additional \$69.31 in the Upper South - eliminating a significant proportion of the \$188.60 price gap between the regions.[31] Given the Fugitive Slave Act only reduced the chance of successful escape rather than eliminating it, it is possible escape could have driven even more of the price differences than observed here.

The main concern with the estimations in Table 2.2 is a bias that may be introduced by the aggregation of states in the two regions. In particular, the Upper South consists of three states - Maryland, Virginia, and North Carolina - while the Deep South consists of five states - Georgia, Louisiana, Mississippi, Tennessee, and South Carolina. Table 2.1 shows that different states had different prices and a composition bias could be driving the results if the data contains *more* observations from higher-priced states in the Upper South (or relatively more from lower-priced states in the Deep South) after 1850. Table 2.3 splits the eight-year 1846-1853 period into two four-year windows (1846-1849 and 1850-1853) and details the number of observations and the average price of a slave in each state in each period. It also provides weighted average prices for the regions based on the relative frequency of observations from a state in a given region. In the table it is easy to see that the data suffers from a composition bias in the Upper South. While the total number of observations in each time period for each region is stable there are more observations from North Carolina and Virginia post-1850 relative to pre-1850. Because these states already have higher prices before 1850, the fact that there are more of them in the sample after 1850 means that the average price in the Upper South is *mechanically* higher post-1850. Recalculating the average post-1850 price in the Upper South using the pre-1850 frequency weights would give an average price of just \$505.79 rather than the \$545.57 observed. As a result, the estimates provided in Table 2.2 are biased upwards as it represents the joint effects of the Fugitive Slave Act and changing sample composition.

While there are changes in the composition of the sample, the effect of the Fugitive Slave Act is still easily observed. For example, in Maryland the increase in the average price between the two periods is over \$180 (a 60% increase compared to the 1846-1849 price). Also, as mentioned earlier, North Carolina may be misclassified if considered as part of the Upper South. Indeed, some parts of North Carolina (particularly the important trading port of Wilmington) are further

[31] Estimating the effect separately for males and females shows the Act's effects were not limited to either gender but perhaps slightly higher for females. These estimations are available from the author upon request.

Table 2.3: Prices and Relative Frequency by State before and after 1850

State	1846-1849	Observations	Rel. Freq	1850-1853	Observations	Rel. Freq
North Carolina	$ 419.19	45	8%	$ 644.51	230	29%
Maryland	$ 301.78	434	73%	$ 480.53	356	45%
Virgina	$ 391.19	113	19%	$ 547.56	199	25%
Upper South Total	$ 327.77	592	100%	$ 545.57	785	100%
South Carolina	$ 436.50	118	3%	$ 575.17	347	8%
Louisiana	$ 542.98	2747	71%	$ 666.43	2732	63%
Tennessee	$ 453.12	16	0%	$ 731.43	14	0%
Georgia	$ 478.29	334	9%	$ 615.35	526	12%
Mississippi	$ 540.07	679	17%	$ 702.69	689	16%
Deep South Total	$ 533.33	3894	100%	$ 658.85	4308	100%

Note: Regional price is the weighted average price.

This table splits the eight-year 1846-1853 period into two four-year windows and details the number of observations and the average price of a slave in each state in each period. It also provides weighted average prices for the regions based on the relative frequency of observations from a state in a given region.

from the Mason-Dixon line than significant portions of Tennessee and South Carolina. In addition, North Carolina prices are similar to South Carolina and Tennessee before 1850, are higher than South Carolina after 1850, and North Carolina has just 45 observations before 1850. Given North Carolina slaves may face escape probabilities more similar to South Carolina and Tennessee an alternate specification removing North Carolina from the analysis is worth examining.[32]

The first column of Table 2.4 shows that the effects of the Fugitive Slave Act (the co-efficient on the difference-in-difference term) are paradoxically increased by the removal of North Carolina observations from the sample. The estimates of the Act's impact on Upper South prices now increase to 28.2% and over $82. This is because the inclusion of North Carolina as an Upper South state reduced the overall price difference between the areas over the period of the sample. As a result, dropping North Carolina ensures the effects of the Act become more pronounced. Table **??** also illustrates the effect of removing Virginia so that Maryland is the only state considered

[32]Moving North Carolina to the Deep South would deal with the escape probability issue but would leave the composition bias issues so that relatively more observations in North Carolina after 1850 would lead to an underestimation of the Act's effects.

as “Upper South.” These specifications reduce the likelihood that composition effects are driving these estimates.

Table 2.4: OLS Diff-in-Diff Estimates with Sample and Specification Restrictions

	Excluding NC		**Excluding NC and VA**	
	(1)	(2)	(3)	(4)
	Log of Slave Price	Appraised Slave Price	Log of Slave Price	Appraised Slave Price
Male	0.291***	149.3***	0.291***	150.6***
	(0.01)	(4.09)	(0.01)	(4.18)
Age	0.0687***	22.10***	0.0689***	22.19***
	(0.00)	(1.23)	(0.00)	(1.25)
Age Squared	-0.00128***	-0.393***	-0.00128***	-0.393***
	(0.00)	(0.02)	(0.00)	(0.02)
Upper South	-0.540***	-202.5***	-0.594***	-213.4***
	(0.03)	(5.80)	(0.03)	(6.28)
After Fugitive Slave Act (FSA)	0.223***	131.8***	0.223***	131.7***
	(0.01)	(4.67)	(0.01)	(4.67)
Upper South x After FSA	0.282***	82.64***	0.324***	95.20***
	(0.03)	(9.95)	(0.04)	(11.30)
Observations	9,114	9,114	8,802	8,802

Columns 1 and 2 report the co-efficient estimates from a difference-in-difference specification where North Carolina was dropped for the log of slave prices and prices in dollars. North Carolina was dropped as it introduces significant composition effects due to the change in the number of observations from the state before and after 1850. Columns 3 and 4 report the co-efficient estimates from a difference-in-difference specification where North Carolina and Virginia were both dropped to illustrate how the effects of the Act were strongest in Maryland, the state where the median town is closest to the Mason-Dixon line. *** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.

Additionally, Table 2.5 examines how the inclusion of state-level fixed effects alters the estimated co-efficients (while restoring NC and VA to the data). This removes the binary indicator for “Upper South” in the analysis. Instead, the specification allows the intercept to vary for each state across the period. The estimates in Table 2.5, while smaller in economic size, are consistent with the thesis that the effect of the Fugitive Slave Act was to increase prices for slaves in the states closest to the Mason-Dixon Line.

2.4.2 A True Spatial Effect?

Neither Table 2.4 nor Table 2.5 truly address the spatial dimension of the Fugitive Slave Act’s impacts. Table 2.4 shows that the Act’s effects were strongest when focusing only on states actually

Table 2.5: OLS Diff-in-Diff Estimates with State-level Fixed Effects

	(1) Log of Slave Price	(2) Appraised Slave Price
Male	0.290***	148.6***
	(0.01)	(3.93)
Age	0.0689***	22.08***
	(0.00)	(1.19)
Age Squared	-0.00129***	-0.393***
	(0.00)	(0.02)
After Fugitive Slave Act (FSA)	0.226***	134.0***
	(0.01)	(4.69)
Upper South x After FSA	0.164***	38.57***
	(0.03)	(9.24)
Observations	9,579	9,579

Columns 1 and 2 above report the co-efficient estimates from a difference-in-difference specification which allows for differences in the mean price of a slave by state across the period. As this soaks up some of the change in the states closer to the Mason-Dixon line, the reduction in the size of the effect on prices is to be expected. *** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.

bordering the Mason-Dixon Line and Table 2.5 showed that composition bias is not the sole driver of the observed effects. Such exercises ease concerns about bias introduced by the data but do not preclude other events, such as rising demand for slave-produced commodities and crops, causing the observed effect. Ideally, including controls for crop prices by year and state (if available) would deal with these concerns. Endogeneity problems prevent such an approach: Using a variable which is directly affected by changes in the dependent variable as a control is redundant. However, even if the endogeneity issues of using commodity prices by year and state were minimal, commodity prices by year and state would simply be a linear combination of year by state fixed effects. However, including year by state fixed effects in a difference-in-difference estimation where the treated entity is a group of states and the before and after treatment periods are a group of years means that the estimated co-efficient on an indicator for “post-1850 $\times$ Upper South” will not provide an unbiased estimate of the Act’s effect.[33]

What is feasible are a number of empirical exercises designed to seriously examine the contention that the Act had an impact that was a function of distance from the Pennsylvania portion of the Mason-Dixon Line after 1850. First is an examination of a difference-in-difference specification

[33] Year by state fixed effects would introduce serial correlation bias (see Bertrand et al., 2004).

with controls for prices by state before and after the Fugitive Slave Act of 1850. If the post-1850 effect grows smoothly with distance to the Mason-Dixon line it is supportive evidence that the Act *caused* the observed empirical findings. While other events could cause an increase in prices in the Upper South after 1850, it would be an unlikely coincidence if the event's effects were correlated with distance from the Mason-Dixon line (particularly, *within* the Upper South). The estimates from this exercise are presented in Table 2.6. In the estimation, each state is given a state-specific intercept before and after the Fugitive Slave Act. Maryland is the omitted state. As a result, the "After 1850" estimated co-efficient represents the effect of the Act in Maryland and each of the interaction co-efficients reflects differences between that state and Maryland. The estimates show the effect of the Act on prices diminishes when slaves are located further from the Mason-Dixon Line.

Secondly, Figure 2.2 provides an examination of pre-trends leading up to 1850. If the pre-trend between the regions is similar, then time and location specific events are less likely to have caused the observed findings. Figure 2.2 provides the post-estimation predictions of the effect of being in the Upper South across the period from 1846 to 1853. The estimation this is based upon examines appraisal prices as a function of demographic controls, year fixed effects, region-specific effects, plus an interaction between year and region fixed effects. The value of the marginal effect of being in the Upper South is then plotted by year to see if the effect is stable before the Fugitive Slave Act. As can be seen the effect was quite stable (maybe even declining slightly) in the years leading up to the 1850 Act. Figure 2.2 also demonstrates that something happened in 1850 to cause a jump in prices in the Upper South region.

2.4.3 County-Level Analysis

Some states, such as Virginia and South Carolina, have quite a distance between their northern and southern extremities. Fortunately, in addition to providing state and year of appraisal, Fogel and Engerman's Probate records contain the county the record originates from. Using distance from the Mason-Dixon Line to the county of origin provides a more granulated check on the thesis that the Fugitive Slave Act had a spatial impact on prices that varied with distance to the Mason-Dixon Line. Summary statistics are presented in Table 2.7 for counties from seven states in the data in the four years before and after 1850.[34] The measure of distance is constructed as miles to the

[34] There are no Tennessee observations post 1850 that met the sample restrictions.

Table 2.6: OLS Estimates of State-specific Changes in Slave Prices Post-1850

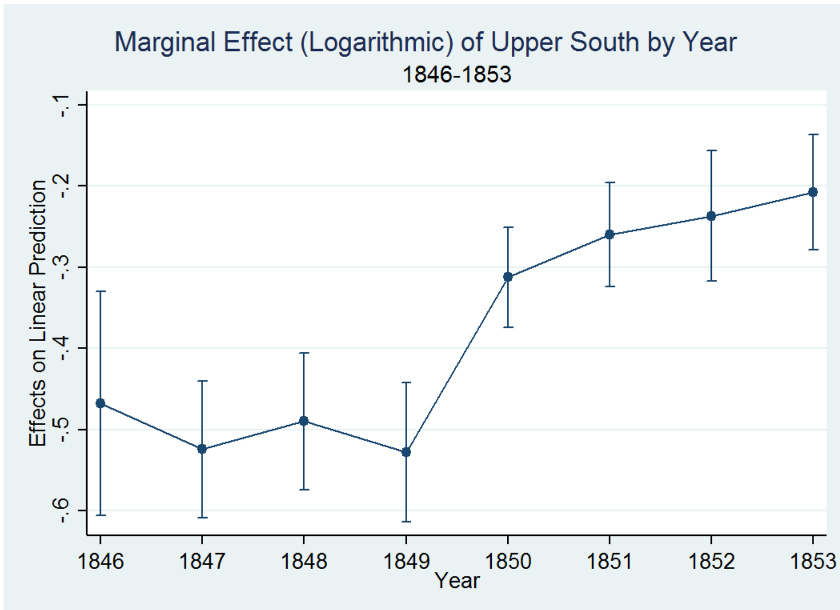
	(1) Log of Slave Price	(2) Appraised Slave Price
After 1850	0.491***	197.4***
	(0.05)	(11.59)
Virginia	0.346***	76.10***
	(0.04)	(11.72)
North Carolina	0.504***	131.3***
	(0.05)	(18.32)
South Carolina	0.321***	90.85***
	(0.07)	(14.16)
Georgia	0.559***	190.1***
	(0.04)	(9.22)
Mississippi	0.696***	231.7***
	(0.03)	(7.76)
Louisiana	0.641***	224.8***
	(0.03)	(6.35)
Virginia * After 1850	-0.170***	-29.93
	(0.06)	(19.85)
North Carolina * After 1850	-0.224***	-11.33
	(0.07)	(24.67)
South Carolina * After 1850	-0.173**	-53.59***
	(0.08)	(20.08)
Georgia * After 1850	-0.228***	-70.12***
	(0.06)	(15.62)
Mississippi * After 1850	-0.255***	-31.54**
	(0.05)	(15.21)
Louisiana * After 1850	-0.267***	-62.78***
	(0.05)	(12.84)
Observations	9,177	9,177

The regression also flexibly controls for age and sex. In the regression, each state is given its own state-specific intercept before and after the Fugitive Slave Act. The omitted state is Maryland. As a result, the "After 1850" term represents the effect of the Act on slave prices in Maryland while the co-efficients on each State × After 1850 interaction reflects differences between that state and the effect in Maryland. As can be seen, the positive effect of the Fugitive Slave Act is smallest in the deeper south. *** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.

Pennsylvania border as measured from the most northerly point in a given county. As can be seen, the data is quite noisy with many counties having large differences between the number of slaves observed in each period.

Table 2.8 provides the results of a difference-in-difference estimation of the Fugitive Slave Act's effect on prices using the county-specific measure of distance. Column 1, where the dependent variable (slave price) is logged, suggests prices increase by 6.6% for every 100 miles from the Mason-Dixon Line. Using a continuous measure of distance controls for pre-existing differences in

Figure 2.2: Examination of Trends by Region of the South



The figure provides the post-estimation predictions of the effect of being in the Upper South across the period from 1846 to 1853. The figure is produced by regressing appraisal prices on demographic controls, year fixed effects, plus an interaction between year and Upper South. The value of the marginal effect of being in the Upper South is then plotted (and 95% confidence intervals) by year to see if the effect is stable or changing before the Fugitive Slave Act was announced. Although stable from 1846 to 1849, it is clear something happened in 1850.

prices by area across the period of interest and mitigates sample composition concerns. After the Act, there is an increase of 46% everywhere but the effect of distance on prices diminishes by 2.84% for every 100 miles in the period after 1850. In other words, the relationship between distance from the Mason-Dixon Line and prices becomes much smaller after 1850. This finding is consistent with the idea that the Act reduced the north-south price gap by reducing the likelihood of escape in the Upper South. This is also reflected in the raw data in Table 2.7 where states in the Upper South had the largest increases as a proportion of pre-existing prices. Column 2 in Table 2.8 uses the dollar value of a slave as the dependent variable. The estimation includes controls for the same demographic variables as in earlier estimations although those co-efficients are not reported.

As a final robustness check on the validity of the spatial effect contention, Table 2.9 repeats the estimation in Table 2.8 but progressively drop Louisiana and Mississippi then further eliminates Georgia and Tennessee. The third column of the table removes North and South Carolina leaving just Maryland and Virginia. The negative co-efficient on the difference-in-difference term in each

Table 2.7: Summary Statistics by County

	State	Distance From Mason-Dixon Line	N	Appraisal Price in Dolalrs	%Male	N	Appraisal Price in Dolalrs	%Male
				1846-1849			1850-1853	
Queen Annes	MD	31	197	264.44	53%	25	301.60	64%
Anne Arundel	MD	33	215	343.17	53%	316	502.92	61%
Albemarle	VA	99	64	424.30	70%	79	627.85	62%
Essex	VA	107	23	347.83	65%	69	529.93	57%
Henrico	VA	139	17	397.06	47%	20	421.50	45%
Halifax	NC	223	17	426.47	47%	26	529.69	54%
Nash	NC	243	1	500.00	100%	34	777.21	65%
Edgecombe	NC	246	3	433.33	33%	123	654.12	67%
Johnstone	NC	271	16	428.98	50%	33	609.86	55%
Richmond	GA	436	169	489.94	50%	184	601.01	53%
Charleston	SC	451	114	440.85	50%	335	582.19	64%
Jefferson	GA	456	192	563.27	51%	13	676.92	46%
Troup	GA	512	85	482.11	52%	122	650.11	64%
Rankin	MS	716	20	516.25	65%	65	797.32	52%
Hinds	MS	740	37	547.70	57%	133	579.47	53%
East Carroll	LA	753	194	552.71	52%	359	653.76	50%
Tensas	LA	789	376	517.65	50%	126	616.28	52%
Union	LA	797	26	534.62	81%	73	685.62	55%
Ouchita	LA	804	156	494.81	57%	142	615.56	57%
Adams	MS	820	505	561.01	49%	214	727.83	59%
St. Mary	LA	829	371	624.66	71%	738	777.69	75%
St. Helena	LA	829	101	543.66	66%	237	605.00	64%
Wilkinson	MS	833	91	480.33	51%	245	748.39	56%
Orleans	LA	841	251	518.35	57%	304	526.86	55%
Avoyelles	LA	864	42	682.33	74%	21	943.00	57%
Plaquemines	LA	866	96	545.83	63%	102	638.73	51%
Natchitoches	LA	872	140	482.16	64%	300	704.27	62%
De Soto	LA	896	145	475.88	51%	245	653.83	60%

The table shows summary statistics for the counties that appear in the data in the four years both before and after 1850. The data is noisy and there are large changes in sample composition in almost every county.

specification reflects how the Act had a smaller positive impact on slaves located further from the North, even *within* the states closest to the North. Moreover, the effects of the Fugitive Slave Act as a function of distance from the Mason-Dixon Line become larger when states further from Pennsylvania are removed from the analysis. This suggests that the Fugitive Slave Act had its strongest impact on the price-distance gradient within states closer to the Free northern states. Such a finding is strong evidence that the Fugitive Slave Act *caused* slave prices in the Upper South

Table 2.8: Estimation of the Fugitive Slave Act's Impact as a Function of Miles from the Mason-Dixon Line

	(1) Log of Slave Price	(2) Slave Price in Dollars
Distance in Miles	0.000666***	0.251***
	(0.00)	(0.01)
After 1850	0.463***	186.6***
	(0.03)	(8.68)
After 1850 * Distance	-0.000284***	-0.0567***
	(0.00)	(0.01)
Observations	9,177	9,177

The table reports the results of a difference-in-difference estimation of the Fugitive Slave Act's effect on prices using distance from a county to the Mason-Dixon Line in miles. *** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.

to increase relative to the Deep South and that concerns about escape considerably impacted slave prices.

Together, Tables 2.6, 2.8, and 2.9 show that post-1850 price increases as a function of distance were largest not just for but also *within* states closer to the Mason-Dixon Line. Figure 2.2 shows the pre-trend between the two regions of the South was stable. If this was not the case it would lead to concerns that location and time specific events were causing variation in slave prices between regions. Instead, the evidence presented suggests that for the effects observed in Tables 2.2, 2.4, and 2.5 to be unrelated to the Fugitive Slave Act there would have to be some event that occurs only after 1850 that has the same spatial impact as the Fugitive Slave Act not only as a function of distance across states but also within states. Separately identifying the effect of such an event, if it were present, from the Fugitive Slave Act itself is not feasible.

2.4.4 Reverse Experiment

The Fugitive Slave Act led to renewed "Personal Liberty" laws in northern states. As described in Section 2.2, these laws were first implemented in Connecticut and Rhode Island in 1854. The changes created by these laws allow for a second examination of the impact of regulatory changes which alter the likelihood of successful escape. The difference with the Personal Liberty laws is

Table 2.9: Estimation of the Act's Impact on Prices as Measured by Distance from the Mason-Dixon Line using Sub-Samples of Data

	(1) Log of Slave Price	(2) Log of Slave Price	(3) Log of Slave Price
Distance in Miles	0.00110***	0.00254***	0.00420***
	(0.00)	(0.00)	(0.00)
After 1850	0.541***	0.578***	0.627***
	(0.04)	(0.05)	(0.06)
Distance in Miles * After 1850	-0.000644***	-0.00160***	-0.00282***
	(0.00)	(0.00)	(0.00)
Observations	2,569	1,300	1,044
Maryland	Y	Y	Y
Virginia	Y	Y	Y
North Carolina	Y	Y	
South Carolina	Y	Y	
Tennessee	Y		
Georgia	Y		

The table reports the results of a difference-in-difference estimation of the Fugitive Slave Act's effect on prices using distance from a county to the Mason-Dixon Line in miles. Relative to Table 2.8, Column 1 drops Louisiana and Mississippi from the estimation. Column 2 drops Georgia and Tennessee. Column 3 then drops North Carolina and South Carolina. The negative co-efficient on the difference-in-difference term reflects how the Act had a smaller positive impact on slaves located further from the North. *** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.

that, by providing a safe harbor for fugitives, they made successful slave escape *more* likely. If the chance of escape truly affects prices, the 1854 Personal Liberty laws should undo some or all of the price increases observed in the Upper South in response to the 1850 Fugitive Slave Act. Focusing on the period from 1852 to 1856, and using 1854 as the treatment date, the effect of these enhanced Personal Liberty laws are presented in Table 2.10.

The negative coefficient on the interaction term between the Upper South and post-1854 suggests the Personal Liberty laws had the expected effect albeit somewhat smaller in absolute magnitude and measured with less precision than the effect of the Fugitive Slave Act. This lack of precision manifests itself as highly significant effects when the dependent variable is in dollars but a lack of significance in the log specification. The imprecision is perhaps not surprising as the new Personal Liberty laws varied in their timing across the Free states. The difference between the estimates using the log and absolute (dollar) value highlights that there are also data issues with changes in the composition of the sample before and after the law. Accounting for these using state-level fixed

Table 2.10: OLS Diff-in-Diff Estimates of the Effect of 1854 Personal Liberty Laws (1852-1856)

			With State Fixed Effects	
	(1)	(2)	(3)	(4)
	Log of Slave Price	Appraised Slave Price	Log of Slave Price	Appraised Slave Price
Male	-0.0341	95.08***	0.273***	185.1***
	(0.03)	(13.10)	(0.01)	(5.29)
Age	0.0620***	25.31***	0.0672***	26.77***
	(0.00)	(1.27)	(0.00)	(1.29)
Age Squared	-0.00128***	-0.500***	-0.00127***	-0.497***
	(0.00)	(0.02)	(0.00)	(0.02)
Upper South	-0.198***	-101.4***		
	(0.02)	(8.53)		
After 1854	0.160***	135.6***	0.141***	124.7***
	(0.01)	(6.44)	(0.01)	(6.47)
Upper South * After 1854	-0.0226	-62.24***	-0.0542**	-66.75***
	(0.03)	(11.78)	(0.03)	(12.33)
Observations	8,189	8,189	8,189	8,189

*** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.

effects (to again net out the effect of a changing composition of states with different average prices in the sample) gives the estimates in columns 3 and 4. This restores statistical significance in the log specification but the overall effect of the re-instituted Personal Liberty laws is still relatively small. While \$67.75 is not a trivial sum in 1850, the average price of a slave by the mid-1850s was above \$740 in the sample. Note that the increase in the size of the effect suggests that composition effects were biasing the effect downwards.

In addition, using the county-level data tells a similar story with positive co-efficients in both specifications but statistical significance only when focused on the dollar value specification. The estimates are not presented here but the effect of the enhanced Personal Liberty laws after 1854 is an \$8.45 decrease in prices for each 100 miles closer to the Mason-Dixon Line. This is a significant reversal of the effects of the Fugitive Slave Act. Taken together, the effects of the Fugitive Slave Act and their reversal due to new Personal Liberty laws suggest regulatory changes which made slave escape harder and then easier had predictable effects on prices, effects that have been largely ignored in the literature to date.

2.5 ADDITIONAL ROBUSTNESS CHECKS

2.5.1 Narrower and Wider Event Windows

The estimates presented in Section 2.4 were unaffected when the sample was restricted to just males or just females (not reported). In addition, allowing for a wider, 10-year window (1845-1854) does not change the magnitude of the effect of the Act but tightens confidence intervals due to additional data points. A very narrow four-year window (1849-1852) reduces the size of the estimated effect relative to the eight-year window in Section 2.4. Table 2.11 reports the estimates from these the narrower and tighter windows, repeating the same difference-in-difference estimation as laid out in Section 2.4.

The effect of the Act on slave prices in the Upper South using the narrower time period is presented in *Panel A*. Focusing on log prices, the estimated effect of the law falls to close to a 22% increase attributable to the law. Taking a wider time period, *Panel B* estimates a similar increase. Columns 3 and 4 of each panel include state fixed-effects to ease concerns about the changing composition of the sample across time. These indicators for each state reduce the size and significance of estimated co-efficients, particularly in the narrow window where so few valid observations occur in the Upper South (of the 4,905 observations, fewer than 1-in-7 are observed in the Upper South in the narrow four-year window).

2.5.2 Sensitivity to Treatment Date

September 1, 1850 was the official implementation date of the Act. However, in Section 2.4 the Act was considered as affecting all observations after *and* including 1850. Unfortunately, the data cannot be stratified into any finer time periods than "year of appraisal." As a result, treating the date the law comes into effect as 1850 potentially overestimates any effect while treating it as 1851 would underestimate it. Either choice results in an unknown number of slaves appraised in 1850 being classified as pre-treatment when they were actually appraised after the new laws or as post-treatment when they were actually appraised before the law was enacted. Table 2.12 reports the estimates from the same 1846-1853 time period but considers observations occurring in 1850 as before the Act rather than after, essentially moving the treatment date. The table shows the change reduces the magnitude of the empirical effect of the Fugitive Slave Act to 21.5% in the log specification. Overall, Table 2.12 highlights that the exact implementation date of the law was less

Table 2.11: Robustness to Time Period Changes

Panel A - Narrow Window (1849-1852)

	(1) Log of Slave Price	(2) Appraised Slave Price	(3) Log of Slave Price	(4) Appraised Slave Price
Male	0.286***	147.2***	0.287***	147.5***
	(0.01)	(3.62)	(0.01)	(3.57)
Age	0.0681***	21.80***	0.0679***	21.69***
	(0.00)	(1.06)	(0.00)	(1.05)
Age Squared	-0.00129***	-0.393***	-0.00128***	-0.391***
	(0.00)	(0.02)	(0.00)	(0.02)
Upper South	-0.455***	-177.8***	*State FEs*	*State FEs*
	(0.02)	(4.68)		
After FSA	0.294***	167.0***	0.294***	167.8***
	(0.01)	(4.33)	(0.01)	(4.34)
Upper South x After FSA	0.220***	59.72***	0.130***	24.31***
	(0.02)	(8.02)	(0.03)	(8.42)
Observations	11,986	11,986	11,986	11,986

Panel B - Wide Window (1845-1854)

	(1) Log of Slave Price	(2) Appraised Slave Price	(3) Log of Slave Price	(4) Appraised Slave Price
Male	0.284***	137.8***	0.285***	138.0***
	(0.01)	(5.13)	(0.01)	(5.06)
Age	0.0659***	20.24***	0.0653***	19.98***
	(0.00)	(1.53)	(0.00)	(1.51)
Age Squared	-0.00125***	-0.362***	-0.00124***	-0.358***
	(0.00)	(0.02)	(0.00)	(0.02)
Upper South	-0.504***	-192.5***	*State FEs*	*State FEs*
	(0.03)	(7.61)		
After FSA	0.144***	86.64***	0.145***	86.97***
	(0.02)	(6.21)	(0.02)	(6.20)
Upper South x After FSA	0.216***	47.23***	0.0853**	1.713
	(0.04)	(11.17)	(0.04)	(11.22)
Observations	4,905	4,905	4,905	4,905

*** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.

important than the difference it caused between the periods 1846-49 and 1851-53. As in Table 2.4, the third and fourth columns report estimates using state-level fixed-effects to net out the effects of a changing sample composition. In addition, dropping 1850 from the analysis leaves estimates essentially unchanged.

Table 2.12: OLS Diff-in-Diff Estimates using 1851 as "Treatment" Date

	(1) Log of Slave Price	(2) Appraised Slave Price	(3) Log of Slave Price	(4) Appraised Slave Price
Male	0.293***	150.2***	0.293***	150.0***
	(0.01)	(3.97)	(0.01)	(3.93)
Age	0.0695***	22.32***	0.0691***	22.16***
	(0.00)	(1.21)	(0.00)	(1.20)
Age Squared	-0.00129***	-0.396***	-0.00129***	-0.393***
	(0.00)	(0.02)	(0.00)	(0.02)
Upper South	-0.438***	-173.4***	*State FEs*	*State FEs*
	(0.02)	(5.40)		
After FSA	0.225***	131.0***	0.227***	132.9***
	(0.01)	(5.11)	(0.01)	(5.12)
Upper South x After FSA	0.215***	68.29***	0.129***	42.54***
	(0.03)	(9.73)	(0.03)	(9.92)
Observations	9,579	9,579	9,579	9,579

*** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.

2.5.3 Additional Evidence from Newspaper Advertisements

Runaway slaves were often advertised in local newspapers and the frequency of unique (and repeated) advertisements along with information on rewards can provide another source of evidence on the Fugitive Slave Act's effects. Newsbank's American Newspaper Archives provides digitized editions of historical daily newspapers from across the U.S, search-able by keyword.[35] The advertisements typically provide a description of the slave, perhaps record the county from which the slave fled, detail the name of the slave owner, and give a dollar value and the terms of the reward offered for the capture of the runaway. Examples are provided in Figure 2.3.

The advertisements provide data that can also be used to examine if slave escape was an important economic aspect of the institution of slavery. While the advertisements do not report slave values, the reward offered in the advertisements can help test if the findings presented in Section 4 and 5 are actually driven by fewer runaways and stronger property rights for slave-owners in the

[35]Available with subscription via `readex.com`.

Figure 2.3: Typical Runaway Advertisement

ONE HUNDRED DOLLARS REWARD.—Ran away from the subscriber, on Sunday, the 30th ult., Negro MAN GEORGE, calls himself George Henry Duppin, about 30 years of age, 5 feet 9 or 10 inches high, has a large scar on one side of his neck, occasioned by a scrofulus affection when a boy. The clothing which he wore consisted of a drab box coat and pantaloons, fur hat and long coarse boots; he, however, took other clothing with him, and will probably change them. Fifty dollars will be paid for his apprehension within the State, or the above reward if taken beyond the State, and secured so that I get him again.

GASSAWAY WINTERSON,

fe2-8t* West River.

$100 REWARD.—Ran away from the subscriber, living at Merry Point, Va., a NEGRO GIRL, about 16 years of age, somewhat likely. She was owned by Richard Hartnal, of Northumberland county, at the time I bought her, and was in my possession but a short time when she ran away; and is supposed to have come to Baltimore with some free negroes who left in December, 1850. She went by the name of Minna, and is about 5 feet high. The above reward will be given for her arrest and delivery to SOLOMON KING, Baltimore; or to LEWIS H. DIX, Merry Point, Lancaster county, Va. 04-3t*

Source: America's Historical Newspapers. Available with subscription from `readex.com`.

Upper South. To collect the advertisements data, a search is completed of Newsbank's America's Historical Newspapers for all advertisements containing the words "abscond*", "runaway*", "ran away*", "run away*", or "apprehen*" for the period 1849-1852, where * represents a wild card. The search returns tens of thousands of results from newspapers during the antebellum period. Due to both the number of records to be codified and the less-than-perfect quality of the digital images, gathering this data is an arduous process. To economize on data collection efforts, only advertisements from four states (Louisiana, Georgia, Maryland, and Virginia) are examined. This reduces the number of search results to about 12,000. As not all of these turn out to be actual advertisements for runaways the sample eventually contains just over 6,000 observations.[36] In addition, many turn out to be repeated advertisements for the same escapee. The number of repeated notices is recorded and can be used to crudely examine how "quickly" slaves were recaptured. Of the remaining unique observations some were illegible or were missing crucial information such as the slave's age, sex, or details of a monetary reward. In the valid sample, there are just under 1,000 *unique* observations.

[36]In fact, given the search terms, the reader may have guessed that quite a few of the false positive search results are notices regarding lost dogs.

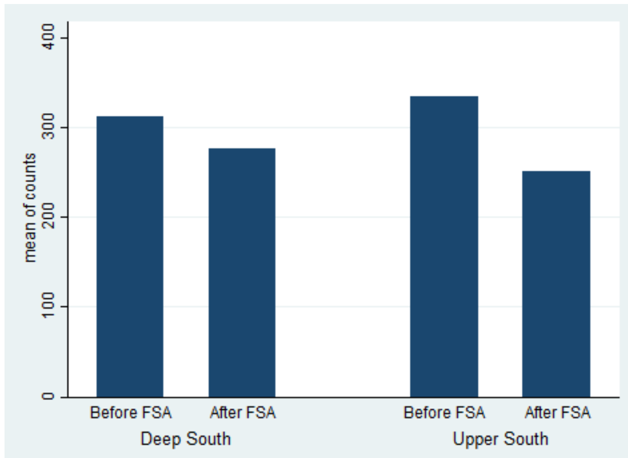
The frequency of advertisements before and after September 1850 is presented in Figure 2.4 for the Upper and Deep South. Fewer runaways are observed in both locations but the decline is more significant in the Upper South. However, the actual number of advertisements observed in each area before and after the Act provides less information than at first glance. This is because the effect the Act would have on the actual number of attempted runaways and advertisements is not predictable. For buyers and sellers of slaves the strengthening of property rights would cause prices in the Upper South to increase. However, if slave-owners then treat their slaves worse because they were assured that the slave can no longer escape so easily, it might make sense if *more* slaves tried to run away. On the other hand, if slaves were aware of the Act's provisions, they may be *less* motivated to try to escape as they are less likely to succeed. Additionally, even if there is no change in the number of runaways the incentives to advertise may be changed after the Act.

On the other hand, if the paper's main findings are explained by stronger property rights afforded by the Fugitive Slave Act then rewards offered for similar slaves in the Upper South should fall (relative to the Deep South) after the Act comes into effect. Such an effect should be expected regardless of the actual *number* of advertisements observed. This is because, conditional on a slave-owner advertising a runaway, slaves with similar observable characteristics should command larger rewards after prices increased in the Upper South, all else equal. No assumption about the Fugitive Slave Act's role is necessary. However, it would mean that observing falling rewards would be strong evidence of the Fugitive Slave Act's impact.

If rewards fall even though slave prices are rising it is evidence that the Fugitive Slave Act represented a significant strengthening of slave-owners property rights. If rewards offered do not fall, it does not mean the Act had no impact. It could simply mean that the increasing price of slaves overwhelmed the increased likelihood of recapture. It would also be possible that slave prices the Upper South were increasing for reasons unrelated to slave-owner property rights and would support the contention that the Fugitive Slave Act was unnecessary and irrelevant.

Estimates generated from the advertisements data, using the same difference-in-differences approach as earlier, suggest rewards offered in the Upper South drop dramatically after the Act. Table 2.14 provides these estimates which are based upon the data presented in Table 2.13. The table provides summary statistics from the advertisements data by state and year. It can be seen that advertisements for runaways were most often for male slaves and were generally in their mid to late twenties. The small number of observations (1852 had no valid observations) from Georgia

Figure 2.4: Frequency of Advertisements for Two Years Before and After the Fugitive Slave Act (FSA) by Region



Source: Data gathered by the author from advertisements provided by Newsbank's American Historical Newspapers collection. Data available with subscription to readex.com.

ensures noisy summary statistics.[37] However, even states with more observed advertisements are quite noisy.

The coefficient on the difference-in-difference term in the second column of Table 2.14 shows that after the Act, rewards in the Upper South fell by $19.08 relative to advertised rewards in the Deep South. In the first column, the dependent variable has been logged to allow the coefficient to be interpreted as a percentage change. It suggests rewards fell by 21.3% in the Upper South relative to the Deep South after the Fugitive Slave Act came into place. The sign of the estimate is consistent with improved property rights for slave-owners. Addtionally, the third column suggest the repetition of advertisements also decreases post-1850.

What makes the estimates in Table 2.14 such strong evidence is that the rewards offered and advertisement repetition *fall* relative to the Deep South while we know market prices for slaves were rising. Rising rewards would be consistent with some other event causing slave prices to rise in the Upper South but falling rewards is additional evidence to suggest that the Act explains the patterns in the data and that escape was a significant factor in the spatial variation in prices observed in the antebellum South.

[37]There were no valid observations for Georgia in 1852. There were a number of advertisements in that year but they were for a slave under 12 or were missing gender, age, or specific details of a reward.

Table 2.13: Summary Statistics: Advertisements Data

	Male	Age	$ Reward Offered	# of Repeats	# of Observations
Georgia					
1849	92%	29.6	65.56	5.1	12
1850	80%	23.5	16.43	1.7	10
1851	71%	25.4	17.14	3.0	7
1852					
Louisiana					
1849	78%	24.8	31.6	5.0	165
1850	68%	26.5	33.45	5.5	77
1851	70%	28.4	60.59	11.2	99
1852	68%	27.0	35.9	11.1	130
Maryland					
1849	82%	23.7	70.99	2.8	101
1850	80%	22.2	85.99	2.8	115
1851	83%	23.6	83.84	3.2	71
1852	81%	20.3	53.07	3.7	103
Virginia					
1849	88%	27.9	27.88	2.6	26
1850	77%	25.7	39.33	2.5	31
1851	95%	35.1	36.9	3.9	21
1852	95%	26.3	44.25	2.2	20

Data collected by the author from advertisements for runaways found by searching through Newsbank's digitized repository of American Newspapers (available with subscription at readex.com). The table presents summary statistics for each state and year for percent male, mean age, mean reward offered and number of times the advertisement was repeated. The data is restricted to valid observations which were those listing a monetary reward, the gender of the slave, and were for a slave who was at least 12 years old. Note that 1852 contained no observations for Georgia.

One concern with this approach and the estimates presented are issues of selection and composition bias. Easing concerns about composition bias, the sample includes all available advertisements in the states of Lousiana, Georgia, Maryland, and Virginia from 1849-1852. At the same time, there are relatively fewer observations for Maryland in 1851 relative to earlier years and as rewards are lower there before the FSA (see Table 2.13), this could be driving the effects observed. However,

Table 2.14: OLS Diff-in-Diff Estimates of Changes in Rewards Offered using Advertisements Data

	(1) Log Reward	(2) Reward	(3) # of Repeated Advertisements
Upper South	0.569	37.94*	-2.185***
	(0.248)	(15.05)	(0.325)
After FSA	0.200**	13.21**	5.751***
	(0.0389)	(2.464)	(0.154)
Upper South x After FSA	-0.213**	-19.08**	-5.090***
	(0.0566)	(3.727)	(0.187)
Observations	929	929	988
Controls for Age	Y	Y	Y
Controls for Sex	Y	Y	Y

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

the change in composition is small enough to be safely dismissed as the cause of the effects. For example, re-running the estimation in Table 2.14 with the addition of state fixed-effects leaves the estimates virtually unchanged in the dollar specification. At the same time, the log specification remains close to a 20% fall in rewards offered but, partly due to the small sample size, fails to reach standard measures of statistical significance. See Table 2.15 below for more details.

Aside from composition bias, the data collected could still be censored by the decisions of slave-owners: The Fugitive Slave Act may have changed the incentive to advertise runaways or eliminated the need to advertise at all. However, such censoring could be expected to work against finding any empirically significant results. This is because the decision to incur the cost to advertise should be positively related to the value of a slave. For the estimates in Table **??** to represent the consequences of censoring or selection, the opposite would have to be happening: Slave-owners in the Upper South would have to react to the law by deciding to only advertise in the event of escape of a "lower value" slave. Alternatively, perhaps the data *before* the Act was left-censored and slave-owners did not advertise if lower value slaves escaped but began to do so after the Act. As age and sex were strong predictors of price, Table 2.16 might help shed some light on both types of censoring issues.

In Table 2.16, the *Age* column reports the mean age of runaways for the Upper and Deep South before and after the Act. The data shows advertisements tended to be for older slaves in both

Table 2.15: OLS Diff-in-Diff Estimates of Changes in Rewards Offered using Advertisements Data:Adding State Fixed-Effects

	(1) Log Reward	(2) Reward	(3) # of Repeated Advertisements
After FSA	0.187**	12.57**	5.567***
	(0.0755)	(5.212)	(1.083)
Upper South * After FSA	-0.209	-19.03**	-4.919***
	(0.128)	(7.752)	(1.139)
Observations	929	929	988
Controls for Age	Y	Y	Y
Controls for Sex	Y	Y	Y

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

regions after the Act. This would actually tend to increase rewards offered all else equal (slave prices increased with age up to a point). However, only the difference in age in the Deep South is statistically different from zero. The proportion of males in the sample increases in the Upper South after the Act but not significantly. At the same time, there is a decrease in the proportion of males in the sample in the Deep South after the law passed. Together, this means that, after the Act, there are no statistically significant changes in the age or sex of advertised slaves in the Upper South. In the Deep South, advertisements tended to feature older slaves but more females. The differences are statistically significant but would have contradictory impacts on the rewards offered. Observing advertisements for older slaves suggests selection may be biased towards higher value slaves but observing more advertisements for females suggests the opposite.[38] Because of the lack of significance in the Upper South and contradictory changes in the Deep South Table 2.16 should reduce concerns about how censoring and selection biases the presented estimates.[39]

[38]Before the Act, the data suggest a 25 year old and 28 year old slave would have mean rewards of \$37.89 and \$40.32. After the Act, these values change only slightly to \$37.80 and \$40.24. Similarly, a male slave would have a reward approximately \$2 higher on average than a female slave. These values are produced using the co-efficient estimates from Table 2.14.

[39]Despite the usefulness of Table 2.16 in helping us understand the changes in the demographics of advertised runaways and how it relates to rewards, the table cannot ease concerns about how selection would affect an analysis of the *number* of advertisements in each region and time period. This is because the decision to advertise is a function of at least two important factors: The likelihood of recapture and the market value of the slave. As the Fugitive

Table 2.16: Mean Age and Percent Male Runaways from Advertisements Data

	Age (years)		***Percent Male***	
	Before FSA	After FSA	Before FSA	After FSA
Upper South	23.76	25.23	82.35%	86.84%
t-stat under $H_0 : before = after$	-1.32 (n=238)		$-.9455$ (n=233)	
Deep South	25.05	27.89	74.88%	62.02%
t-stat under $H_0 : before = after$	-3.41 (n=331)		2.49 (n=328)	

Note that the number of observations for each test in each region differs slightly due to differences in missing values. The t-statistic reported represents a comparison of means test under a null of no difference. The t-statistic and number of observations used for the test is reported. In all four tests, the alternative hypothesis is that the means are indeed different.

2.6 CONCLUSION

Differences in antebellum slave prices across slave states have generally been attributed to differences in slave productivity across regions. This paper uses data on slave prices from probate appraisals plus hand-collected data from newspaper advertisements to examine how prices were also affected by the risk of escape. To do so, the Fugitive Slave Act of 1850 is exploited as a natural experiment using a difference-in-difference approach to estimation. After the 1850 Act, the gap in regional slave prices diminishes markedly. The findings are robust to alternate treatment dates and wider and narrower event windows. The paper's findings are significant as only scattered contributions have suggested slave escape as an issue and none provide strong empirical evidence to back their claims.

The main empirical findings are supported by repeating the same estimations using county-level measures of distance from the Mason-Dixon Line, estimates from a reverse experiment, and data from newspaper advertisements for runaways. Using county-level data provides stronger controls for distance and shows the Act had the strongest effects on the price-distance gradient within

Slave Act affected both of these factors but also reduced the likelihood of attempting to escape, it is not clear what would happen to the quantity of advertisements.

states closest to Pennsylvania.[40] The reverse experiment shows that when Free states later enacted laws to undermine the Fugitive Slave Act, the price increases seen in the Upper South due to stronger property rights were reversed. Data from newspaper advertisements for runaways provide corroborating evidence that the Fugitive Slave Act had an impact on the frequency of runaways. This data is a valuable check against alternate explanations of the pattern seen using the probate data-set. The fact that rewards offered and re-runs of the same advertisement decline suggest property rights were enhanced by the law, reducing the chance of successful escape. If rewards had increased, it would suggest slave prices were increasing in the Upper South for reasons unrelated to the Act or slave-owners' property rights.

Together, the empirical work presented here suggests a much more important role for slave escape than previously assumed by authors in the Fogel and Engerman tradition. Paradoxically, if slave escape was more important than previously thought, it strengthens Fogel and Engerman's overall thesis: Slave-owners and slaves had a much more complex master-slave relationship than had been considered by earlier slavery scholars.

The estimates presented are potentially an underestimate of the importance of escape and slave-owner property rights. This is because difference-in-difference estimates can be biased upwards or downwards in the absence of a common trend in treatment and control groups. In this paper, the common trend assumption could be violated in such a way as to reduce the likelihood of finding any empirical effect. The downward bias is possible due to both the Act *reducing* rather than totally *eliminating* the chance of escape and because of abolition and manumission efforts in the North.[41]

In sum, the evidence suggests that slave prices varied across regions not only due to productivity differences but also due to the perils associated with owning human beings who can act and choose for themselves in ways livestock and inanimate objects cannot. This finding is complementary rather than contradictory to prior explanations of the regional price gap and shows that slave agency played an important role within the Peculiar Institution.

[40] This, again, is due to the so-called black laws in Texas, Ohio, Illinois, and Indiana. As a result, the important route of escape was to cross the Mason-Dixon Line into Pennsylvania. That is not to say slaves did not try to go elsewhere, but that the Act's effects can be broadly expected to be a function of distance from the Mason-Dixon Line.

[41] If influential in the area, these would have reduced demand for slaves in border states, all else equal.

3.0 ARE ONLINE DEGREES SUBSTITUTES FOR TRADITIONAL DEGREE PROGRAMS?

This chapter examines how online degrees affect labor market outcomes using a correspondence study. The study involves sending fictional resumes to real job openings while experimentally varying the medium of instruction reported on the resume used. In the preferred specification, callback rates for individuals who have a traditional degree were double those for online degree-holders, all else equal. These findings are important for understanding the future of college education. In particular, they suggest a move towards online instruction is less predictable than suggested by research focused on learning outcomes.

3.1 INTRODUCTION

Allen and Seaman (2014) find that, in the Fall of 2012, 33.5% of all post-secondary students in the U.S. were taking at least one class online. In 2002, this figure was just 9.6%. Such striking growth begs the question: what is higher education going to look like in 20 years time? Will all students be taking at least some classes online? Will students still attend any face-to-face classes?

The answer to these questions critically depends on whether or not online classes are preferred by students. Curiously, the literature seems to be stuck on measuring how online programs affect learning outcomes. Bowen et al. (2014), Figlio et al. (2013), Ary and Brune (2011), Bennett et al. (2007) and Hernandez-Julian and Peters (2012) are just a few examples. Each considers how student learning is affected by the move to various types of technology-enhanced instruction or assignment completion. Their work finds that learning (measured in a variety of ways) is much the same regardless of the medium of instruction. If online education can provide comparable learning at arguably a lower cost (see Bowen et al.) it is tempting to conclude that the move towards online college degrees is inevitable.

What is missing from these studies is a consideration of the individual choices that will determine the future of university education. Learning outcomes are of crucial importance to university administrators and scholars of education and pedagogy. Students may also be pleased to hear that learning outcomes are not affected by taking classes online. However, many students may also be concerned about how taking classes online affects their chances in the labor market.

To try to answer the question of how labor market outcomes are affected by pursuing an online degree, this paper presents the findings of a carefully-controlled correspondence study. In a correspondence study, resumes which vary a single piece of information are sent to employers and callbacks for interview are tracked. Correspondence studies allow researchers to determine causal relationships between individual characteristics and how many interviews an individual receives. While callbacks are not a perfect measure of labor market success, the assumption underlying these studies is that applicants with more interviews will face shorter spells of unemployment and higher wages.

Despite the effort these studies require, the correspondence study method is popular as it tends to provide convincing evidence on how labor markets work and what determines differences in outcomes. Typical examples include Bertrand et al. (2004) who vary applicants' names to indicate race and ethnicity and Neumark (1996) who examines the effect of gender. Others include Kroft et al. (2013) who tackles the effect of longer spells of unemployment, Pager (2003) who examines how having a criminal record effects callbacks, and Lahey (2008) who considers how age affects outcomes.

The popularity of these studies comes from the fact that a researcher can truly randomize the characteristic of interest, rendering it orthogonal to other observed worker characteristics. In contrast, labor market survey data is tainted by selection, measured with error, struggles to account for the effects of joint labor supply decisions, and may not report important characteristics of interest. Using a correspondence study to examine the impact of online education also avoids those same problems. Indeed, labor market surveys do not currently ask about the medium of instruction for surveyed individuals rendering an observational approach unfeasible. Even if they did ask about the medium of instruction, the decision to pursue online education is endogenous rendering causal identification impossible. Creating fictional resumes and then randomly assigning some to report online education provides clean identification of the effect on callbacks for interview.

The experimental set up, described in greater detail in 3.3, gathers real resumes from online job search websites, anonymizes them by changing dates, names, contact information, resume format and style, the applicant's address, cover letter, and previous work names and locations. Each resume is assigned a unique cover letter, a realistic but fictional address, along with their own email address and a contact phone number. Resumes are then randomly assigned to either convey that the degree was obtained via a specific University's online "wing" or in a traditional classroom setting.

Once the bank of resumes is created, they are randomly used to apply to many real jobs across a number of major U.S. cities. After the resumes are sent, requests (calls or emails) for interview are tracked.[1] Note that the experimental variation is not designed to compare selective traditional universities to for-profit schools like the University of Phoenix or DeVry. Instead, the focus is on the effect of having an online versus a traditional degree from an established four-year university. Many large schools have a significant online presence offering several online degrees including Arizona State, Ohio State, Penn State, Northeastern University, and many more.[2]

While telling the employer that the applicant has an online degree may seem contrived, a 2010 survey by the Society of Human Resource Managers found that only 17% of Human Resource Professionals had never seen an applicant clearly indicate an online degree.[3] In the five years since the survey, the number of people completing online classes and degrees has risen considerably: the National Center for Education Statistics reports that the percent taking exclusively online programs rose from 3.7% in 2007-2008 to 12.5% in 2012-2013.[4]

A statistically significant negative effect on callbacks is observed. Marginal effects from the preferred probit specification show that traditional degree holders are called back much more often, an almost 2:1 ratio. The difference in callbacks amounts to 7.44 percentage points. In other words, applicants who reported having a traditional degree were almost three times more likely to be called for interview. These estimates are based on 734 unique job applications for 29 different resumes. To provide some context, the magnitude of this difference is larger than the size of the biggest effects seen in in correspondence studies who examine callback rates by race, gender, or age. The

[1] Note that if a request for interview is received by the researcher the employer is contacted as soon as is feasible to thank them and politely decline the request.

[2] For example, Penn State University offers 24 completely online bachelor's degree programs that students can complete from anywhere in the world.

[3] See http://www.shrm.org/research/surveyfindings/articles/pages/hiringpracticesandattitudes.aspx.

[4] See http://nces.ed.gov/pubs2014/2014023.pdfandhttps://nces.ed.gov/fastfacts/display.asp?id=80 for more details.

positions applied to were randomly assigned to receive an online or traditional degree application.[5] This study's limited sample size is in contrast to the breadth of options for applications and resumes Bertrand et al. (2004) and others have. Interested in the effect of black sounding names on callbacks, Bertrand and Mullainathan apply to 1,300 unique jobs. They send four different resumes to each of these for a total of almost 5,000 observations. The jobs they apply to are typically retail, clerical, sales management, or administrative positions. These jobs rarely require a degree and Bertrand and Mullainathan are not limited to sending resumes which represent younger applicants with a bachelor's degree but with little experience in their field. In fact, Bertrand and Mullainathan can easily ensure that all of their applicants are well-qualified, generating enough callbacks to obtain statistical power.

Section 3.2 reviews the literature on the relative merits of online education along with the benefits and drawbacks of using correspondence studies to understand the labor market. It also relates this work to the literature on school reputation effects. This paper is in some ways an examination of the within-school reputation differences created by having brand-name universities offering in-person and online degrees. Section 3.3 describes the correspondence study procedure. Section 3.4 provides summary data and checks on the experimental randomization. 3.5provides the main estimates and considers their robustness. Section 3.6 concludes.

3.2 LITERATURE

Lack (2013) provides an exhaustive literature review of the available research on learning outcomes in online education through the end of 2012. The review details large and small scale studies examining how students in accounting (Rich and Dereshiwsky, 2011), management (Daymount and Blau, 2008), economics (Bennett et al., 2007), engineering (Enriquez 2010), sociology (Driscoll et al., 2012) and more fare in online settings. Unfortunately, the conclusions that can be drawn from these studies are complicated by wide differences in research methods. They are also affected

[5] On average, just over 25 applications were completed for each resume but there is some variation in the total number of applications sent for each individual. As all of the resumes had to potentially represent someone who has a degree earned *wholly* online, the resumes represent fictional individuals who have graduated recently and have limited experience in their field. This lack of experience means there are sometimes not enough advertised openings in a particular field that meet the study's selection criteria. To ensure enough jobs met the criteria, the key characteristic that would need to be changed is work experience. However, it would make no sense to create a resume which lists a degree earned *online* in 1995, or even 2005. In addition, all jobs had to *require* at least but *no more than* a bachelor's degree. These restrictions make finding suitable openings extremely time-consuming.

by subject attrition, treatment and control group cross-contamination, small sample sizes, different populations of interest, along with each study having a unique institutional setting and time-frame. What the review does find is that there is no solid evidence that students, controlling for observable characteristics, learn less effectively when the medium of instruction is online rather than in-person.

Since Lack's review a number of newer studies with larger samples sizes and true experimental variation have been published. One of these is Figlio et al. (2013). They examine of the effects of watching online rather than attending introductory economics lectures at a large selective research institution in the United States. Unlike the majority of studies in Lack's review, the authors find mild support for the hypothesis that learning outcomes are inferior in online settings. In the study, students were randomly assigned to taking introductory microeconomics online versus in-person. The authors observe that regardless of sex or race, average test scores were higher for those who were assigned to the face-to-face instruction. However, the effects were modest and not always statistically different from zero. Figlio et al.'s work is not without limitations. In particular, their results rely on the comparison between just 215 students assigned to online lectures and 97 assigned to traditional classes. In addition, the online instruction they compare to is primitive. The live lecture is recorded and the "treated" students watch it online. Many would argue this set up is not the relevant comparison: Students watching a recording of a live lecture with no other changes is perhaps not what people mean when they refer to taking online classes.

Bowen et al. (2014), in a larger-scale study, allow for online instruction to be augmented by a new interactive learning platform. They perform their experiment at six large universities, randomly assigning undergraduates into traditional and hybrid statistics classes. The hybrid classes met once per week. Students accessed sophisticated machine-guided instruction instead of a second weekly class meeting. Bowen et al. find that "students in the hybrid format are not harmed by this mode of instruction in terms of pass rates, final exam scores, and performance on a standardized assessment of statistical literacy."

While the research on learning outcomes in single classes suggests that it may be a valid alternative to traditional in-person instruction, no study has been able to examine how a purely online degree program compares to a traditional degree. Indeed, if learning outcomes are comparable in a single introductory class, that doesn't necessarily mean that an entire degree program can be effectively delivered online. To validly examine the outcomes from entirely online programs versus traditional degrees, experimental variation at the program level would be required. Of course, randomly assigning students into an entire degree program would be problematic. First, volunteer subjects would likely form a small and unique group. Secondly, students who were randomly assigned into online degrees may take other steps to mitigate its effects over a period of four or more years. This is aside from the fact that such a design, given the significance of the intervention it entails, would be unlikely to obtain approval from an Institutional Review Board.

What is unusual about all of the research discussed is that none consider how labor market outcomes may be affected by the move towards online coursework. To be fair, in the context of the early studies described by Lack and the more rigorous work of Figlio et al. and Bowen et al., labor market outcomes would be impossible to convincingly relate to a change in the mode of instruction of a single college course. At the same time, if the conclusion drawn from these studies is that college degrees could be delivered mostly via technology-enhanced instruction in the future, it seems reasonable to wonder how students who pursue online degrees will be treated in the labor market.

Rechlin and Kraiger (2012) appear to be the only authors who have even considered this issue. Their study examines the attitudes of employed Industrial-Organization (I-O) Psychologists towards applicants who have completed their degree online who do and do not have internship experience. Their goal was only to inform the educational investment decisions of fellow I-O Psychologists. The survey finds that "applicants with online degrees are viewed less favorably than are applicants with traditional degrees." Rechlin and Kraiger's method, however, is open to criticism. Firstly, they present estimates from just 23 survey respondents in a highly-specialized profession. These respondents viewed just a one-sentence description of each hypothetical individual and their education. Respondents viewed a total of eight sentences, where each sentence varied the level of education (M.A. vs Ph.D.), its delivery (online versus traditional), or whether or not the individual had internship experience. The one sentence descriptor for an applicant with an online education was typically "this applicant received his/her degree in I-O psychology from an online program and did not complete an internship during graduate school." This was compared to "this applicant received his/her degree in I-O psychology from a traditional terminal degree program and did not complete an internship during graduate school."[6] Given these descriptions, the study's respondents are left to make an inference not only on the medium of education but also the institution itself. If the respondent considers applicants with online degrees likely to have graduated from less-selective and lower-ranked schools, then it is unclear if the survey respondent is reacting to the medium of instruction or the inferred institutional prestige. Moreover, as all respondents viewed all eight

[6]Note that these sentences are paraphrased.

potential descriptors and were asked how likely they were to offer the hypothetical applicant a position, demand effects could be driving the results.[7]

The lack of discussion and rigorous research on the impact of online education on labor market outcomes motivates this paper. The fact that information on the medium of instruction is not recorded by labor market surveys means that a correspondence study is likely the only way to causally relate the attitudes of real employers towards potential employees to degrees that are earned online. Correspondence studies are a reliable solution when crucial information available to employers is not available to or cannot be controlled for by researchers. Bertrand and Mullainathan provide the ideal example of the value and purpose of such studies. The authors were interested in the perennial question of how race affects labor market outcomes. One particular channel they are interested in is how employers respond to names which indicate the applicant's race. The assumption being that employers screen resumes using indicators for race which then causes wage and employment gaps to persist. Their paper's title "Are Emily and Greg More Employable than Lakisha and Jamal?" provides the clearest illustration of their approach. Crucially, labor market survey data cannot shed any light on this question as respondents' names are redacted. Bertrand and Mullainathan find that white sounding names received 50 percent more callbacks for interview, holding all else equal.

An even larger-scale correspondence study involving 12,000 applications by Kroft et al. (2013) examines the role of unemployment duration in call backs for interview. They find that fictional applicants who report being unemployed for eight months have 45% fewer callbacks compared to those who were unemployed for just a single month. While labor market survey data could, in theory, be used to examine this question, researchers would have to find extraordinarily creative ways to deal with issues of selection. A naive approach would regress the probability of returning to employment in some time period on the duration of unemployment and observable characteristics. Such an approach would not provide causal estimates mainly because while applicants with longer and shorter spells of unemployment may appear similar to researchers, they likely look quite different to potential employers. Kroft et al.'s approach provides tight control and allows causal relationships to be accurately determined. This paper uses a correspondence study to obtain the benefits of both Bertrand and Mullainathan's and Kroft et al.'s studies. Labor market survey data

[7] If respondents weren't being implicitly asked to compare online and traditional programs for research purposes, they may have reacted differently.

does not provide the required detail on the medium of instruction and even if it did, the choice to pursue an online degree is endogenous and difficult to control for.

Lastly, while not directly discussing the effects of college reputation on labor market outcomes, this paper is a contribution to that literature. Dale and Krueger (2014) provide a detailed analysis of the literature in that area and update the analysis of an earlier paper. They still find that reputation, proxied by selectivity, positively affects labor market outcomes in regressions. However, after controlling for selection *into* selective colleges, the effects fall dramatically, consistent with the existing literature on the topic. Research on this topic is usually based on differences *between* schools. On the other hand, this paper can be viewed as examining *within*-school variation in reputation created by offering online degree programs.[8]

3.2.1 Limits of Correspondence Studies

Correspondence studies are an excellent way to uncover the attitudes of employers towards specific employee characteristics. However, a number of caveats apply. Firstly, callbacks for interviews do not pay bills, and it is not definitively clear from these studies that fewer callbacks actually translates to lower wages and higher unemployment. Instead, the information transmitted to employers via the resume may improve matching, reducing wasteful and unnecessary interviews that would not result in a job offer anyways.

In addition, the revelation of some less favored characteristic perhaps allows an applicant to gain access to an interview they would not have otherwise earned that ultimately leads to a good job offer. Some employers, due to their own personal experiences may be seeking black, female, older, or homosexual workers, or that some might take pity (for lack of a better word) on those with longer spells of unemployment if they themselves faced a similar spell of unemployment. Secondly, given employers review resumes so quickly, the experimental variation may not be as strong as it seems in the study set up. If employers fail to notice the experimental variation it would limit differences in call back rates between applicants. Thirdly, applying for jobs posted in newspapers and online is only one way to secure employment. Social networks and connections, internships, and personal recommendations may compensate for or exacerbate the effects seen in correspondence studies. It is not clear that an individual who fares poorly in a correspondence study couldn't improve their potential job opportunities via alternative approaches to job search.

[8] To make a more direct contribution to this literature, an extension to the paper could vary where a fictional applicant earned their online degree and use multiple versions of the same resume to study the effect.

Additionally, correspondence studies are designed to study the consequences of an immutable characteristic (at least temporarily). While coming close, they rarely actually study what it is they intend to. Take Bertrand and Mullainathan's paper as an example. Their paper claims to study the effect of having a Black-sounding name compared to an identical resume with a White-sounding name. Instead the paper studies the effect of having a Black-sounding name, reporting it without alteration (Jamal Jones could easily present himself on his resume as Jay or J. Jones) *and* having a resume that does not reflect changes that an employer may *expect* to see given that variation. If resumes from otherwise similar whites and blacks are systematically different in the population, then an employer receiving a resume that is not like other black applicants' resumes ensures that those differences become part of the experimental variation. In that case, the effect reported in the paper is the combined effect of having a black-sounding name but having a resume that doesn't seem like the other resumes from black people.

An example may clarify: suppose that black people know their resumes are often discarded based on their name, their neighborhood of residence, or listed education (perhaps they went to a traditionally black high school or college) and that a typical black applicant takes action to counteract that by working harder on their cover letter, improving their resume's content, clarity, and format, oversells or inflates their experience and skills, perhaps obtains a PO Box in a different zip code, or takes some other action intended to counteract the effect of the bias against them. Suppose also that employers are aware of this. Then, even if names are ignored, a black worker with similar listed experience and education may receive fewer callbacks than a similar white worker as the employer does not view the resume to be as accurate or truthful as a resume from other applicants. Essentially, the same worker on paper is treated differently not because of their race but because the employer *interprets* similar resumes differently for each group based on prior experience.[9] This example is not chosen at random: Bertrand and Mullainathan find that white applicants experience a much higher return to increased resume quality. This suggests employers may be skeptical of high-quality black resumes.

Kroft et al.'s work is subject to a similar critique. The authors are identifying not just the effect of unemployment duration but the the combined effect of being unemployed and being foolhardy enough to not have a good (even if contrived or completely fabricated) *explanation* for the spell of unemployment. Employers may be much more receptive to resumes that have a brief explanation of the reason for the gap in employment. While being little more than cheap talk, addressing the issue

[9] This is nothing more than a complicated form of statistical discrimination.

shows that the worker is aware that such gaps raise red flags with employers. Kroft et al. report that of the resumes they found online 75% of the resumes for unemployed workers listed the month and date when the worker last worked. Of these, Kroft et al. report that 95% do not explain why they have not worked since the listed date. They appear to take this as evidence that resumes they submit listing an unexplained period of unemployment will not seem out of the ordinary. That is a quite a leap given they do not have access to the cover letters these workers used when applying for jobs.

Unfortunately, this paper is subject to the same unavoidable criticism. The estimated effects reported later in the paper should be accurately seen as the impact on callbacks for interview from having an online degree *and* telling the employer about it. However, the consequences of this distinction are less concerning compared to Kroft et al.'s paper. This is because individuals can fabricate a reason for their spell of unemployment in their application and at interview that may satisfy an employer's concerns. In contrast, an entire online four-year college degree is not so easily side-stepped. Even if it is not mentioned in the resume, the issue will likely come up during an interview.[10] This means that while the effects of unemployment duration on callbacks and eventual employment may perhaps be eliminated with a simple one sentence explanation, the effect of online degrees on labor market success is less avoidable. Alternatively, mimicking concerns with Bertrand and Mullainathan's approach, employers may be used to applicants with online degrees taking steps to compensate for their perceived deficiency via improvements in other areas. While the resumes may appear equivalent to the researcher, employers may have priors that vary for these kinds of candidates. This is considered in Section 3.5.

These methodological caveats, while relevant, do not completely invalidate the correspondence study method nor the causal relationships uncovered. Instead, they should be viewed as qualifications, adding a disclaimer that delineates what it is that is being explained and how it might be interpreted differently under alternative circumstances.

[10]Consider an applicant who lists work experience coincident with their college degree, perhaps in another state. Alternatively, the fact a degree was completed online likely will arise when the candidate is asked to answer campus or location-specific questions.

3.3 EXPERIMENTAL PROCEDURE

The procedure to generate resumes is much the same across correspondence studies. For authenticity, a bank of resumes is created from real resumes researchers can find online. These are then anonymized so that the original person can no longer be identified.[11] The resumes are then sent to real jobs advertised online or in newspapers that the resume is qualified for. Critically, before the resumes are sent, they are randomly assigned one of n possible variations in a characteristic of interest. The researcher then tracks callbacks for interview.

Because the randomization is completely orthogonal, by construction, to the resume's other reported characteristics, differences in callbacks can be considered causal. This paper uses a similar approach but differs in an important dimension. Correspondence studies tend to focus on clerical, retail, and administrative roles to ensure they can apply to many job openings with multiple resumes. This study is focused on differences in callbacks for those who have bachelor's degrees. As a result, entry-level clerical and retail jobs are not realistic options. Instead, the paper focuses on several early career positions suitable for recently graduated degree holders in the business, engineering, and medical professions. These fields were chosen because these positions represent the types of programs offered online, a bachelor's degree in these fields is linked to employment in a particular well-defined job (such as software engineer, nurse, accountant, or business analyst), and there are typically lots of jobs advertised in these fields.[12] A disclaimer that the findings may not generalize to other situations and professions applies, even though there is no immediate reason to suspect that is the case.

3.3.1 Resume Generation

Step one was to search a major job-hunting website for publicly-posted resumes of recent graduates of degree programs in business administration, marketing, accounting, nursing, along with software, mechanical, or manufacturing engineering (for the reasons laid out immediately above). Only resumes representing those who are recent graduates (obtained their BA/BS in 2012 or later) were

[11] Even if they could still be identified, the information used was posted publicly.

[12] U.S. News ranks online programs in Accounting, Business Administration and Management, Business Technology Management, Communication, Computer Science, Criminal Justice, Cybersecurity, Dietetics, Early Childhood Education, Electrical Engineering, Elementary Education, Engineering, Environmental Science, Family and Human Development, Finance, Graphic Design, Health Care Administration and Management, Health Science, Homeland Security, Information Technology, Interior Design, Liberal Studies, Marketing, Network Administration, Nursing, Paralegal Studies, Political Science, Psychology, Public Safety Administration, and Special Education. See http://www.usnews.com/education/online-education/bachelors

selected. All the resumes selected reflect someone who was currently employed in a job that matched their educational background. That is, a Registered Nurse with a nursing degree was working as a nurse, and software engineers selected were currently working in software development or some other information technology-related position. The resumes chosen varied in almost every way one can imagine. The individuals lived in a variety of locations, had different work experience, attended different colleges throughout the US, had various degree titles (even within the same field), many listed internships or part-time employment in college, and some used personal statements and listed "headline" keywords while others did not. For practical reasons, the resumes chosen were limited to those currently employed (and therefore having at least *some* experience) to allow a sufficient number of applications to be completed. Openings suitable for recent graduates with *no* experience are rare whereas those requiring at least one year of experience are relatively plentiful.

Once the set of resumes were chosen, they were anonymized by altering names, state of residence, dates and places of employment, college attended, graduation dates, listed GPAs, and other characteristics. All of these changes preserved the overall quality of the resume. The perceived gender, title of the degree held (but not institution), reported current job title (but not firm), and years of work experience reported on the resume were not altered. Additionally, a resume only reported that a student attended a university where the listed degree was offered in both a traditional and a completely online format. As an example, Penn State University offers 24 degree programs that can be completed online from anywhere in the world.[13] Interestingly, their FAQ page emphasizes that the transcript will not be any different to the transcript of those who completed their degree on-campus.

Next, appropriate email addresses were created (generally: first name, middle initial, last name "at" some internet domain, or a slight variation if that was not available).[14] These email addresses were then associated with virtual phone numbers and voice-mail services. The outgoing voice-mail message was left as the default computerized greeting. The message an employer heard was the same regardless of resume received and only differed in the individual's phone number.

Lastly, after this process was complete, the details of each resume (name, location, years of experience, gender, U.S. News College Selectivity, listed GPA) were input into a spreadsheet. A random number between zero and one was then assigned to each resume. Those whose random

[13] The programs offered at Penn State's World Campus can be accessed here: `http://www.worldcampus.psu.edu/degrees-and-certificates/directory/undergraduate/bachelor`

[14] As the paper is not focused on racial gaps all of the names used are associated with Caucasian males and females in the United States.

number turned out to be below the median in the list were assigned to have earned their education online. The remaining resumes were not altered to reflect an online education. The effectiveness of this randomization is discussed later.

The fact that a college degree had been obtained online was conveyed to potential employers by just one word following the name of the university or college the individual graduated from. On a resume this appeared as "[Name of University or College] - Online." That is the only difference potential employers would see on the resume. As mentioned earlier, while this may seem contrived, in 2010 just 17% of human resource professionals reported never seeing someone state their degree was earned online.

3.3.2 Cover Letter Generation

Applications for positions typically request and almost always allow a cover letter. For each resume, a cover letter was created which slightly varied in content but not in organization or intent. All cover letters contained four paragraphs, no more or less. The first expressed interest in "the advertised position." The second explained the candidate's current role, responsibilities, length of tenure, and expressed a desire to utilize and improve their skills in a new position. That paragraph drew its information from the resume. The third paragraph explained why the candidate would be a good fit for the available position but was not tailored to each job in any way. Instead, it reminded the reader of the candidate's education along with their excellent technical, analytical, communication, and other skills as relevant to the field.[15] The final paragraph thanked the employer for their consideration, reiterated the candidate's interest in the position, and expressed a desire to discuss the candidate's suitability for the position at interview. As each of these letters followed the same structure but varied specifics to match the details of the resume, their effect cannot (and should not) be separated from a resume-level fixed effect.

3.3.3 Applying to Open Positions and Monitoring Callbacks

Random numbers generated by a spreadsheet program assigned online and traditional degree status to each of the resumes. That assignment remains constant in the data presented.[16] A list of

[15] For example, all nurse cover letters suggested the candidate was kind, caring, and considerate. Software engineers were technically and analytically adept, and so on. These paragraphs drew from samples online for these types of positions. They did not vary appreciably across individuals with the same career/degree.

[16] A matched-pairs approach will be used in subsequent versions of this paper.

positions suitable for all the fictional candidates with the same degree was also created using a combination of applicants' job titles and experience. The text of the advertisement was studied to ensure all candidates were minimally qualified in terms of required experience. Positions also had to have been advertised in the previous *two* business days. This time-frame helps to ensure resumes are not submitted *after* the firm had received many suitable applications. This maximizes the chance of callbacks, providing greater statistical power. A randomly chosen fictitious resume and associated cover letter was then used to apply to each of the jobs the search returned.

In addition, to avoid bias, only open positions which asked for information explicitly available in the existing cover letter and resume were used. This resulted in many abandoned applications as many job application systems require more than a resume and cover letter to be submitted. Unfortunately, it is rarely clear what will be asked when beginning a job application. Applications often appear to request just a resume and cover letter be uploaded (or the information to be pasted into a firm-specific format) but upon clicking "submit" the system brings the applicant to another page of questions which can include basic personality tests or short essays specific to the firm, location, industry, or background of the potential applicant. To avoid the potential for bias from such essays and tests, these applications were all abandoned. Around 33-40% of applications had to be abandoned for this reason.

Only calls and emails requesting an interview were recorded. This is easy as each resume has its own email address and an assigned virtual phone number. While each resume reports a postal address the address is entirely fictitious (although it appears realistic) and any contact via postal mail would be missed. Bertrand and Mullainathan were concerned about this and contacted several human resources managers who suggested postal requests for interview was extremely rare. Given that Bertrand and Mullainathan's experiment was undertaken over 13 years ago the potential for bias introduced by requests for interview via postal mail can safely be ignored.

3.4 ESTIMATION AND DATA OVERVIEW

3.4.1 Estimation

As the assignment to an online degree is random, the estimate of δ from a regression of the following form provides causal estimates of the difference in callback probability between applicants who have online rather than traditional degree programs:

$$y_{i,k} = \beta X_i + \delta D_i + \epsilon_i$$

In the estimation, $y_{i,k}$ takes on the value of 1 if firm k calls applicant i for interview. This binary outcome is predicted by person-specific characteristics X_i and a dummy D_i to represent the result of the randomization. In this paper X_i includes GPA, years of experience, a measure of college selectivity (as used by Dale and Krueger), gender, and a binary indicator for the industry the person is in (business, engineering, or nursing). The dummy D_i takes on the value of 1 if the randomization selects that individual as having an online degree.

A negative $\hat{\delta}$ would suggest the likelihood of getting a callback is reduced for online degree holders, even after accounting for other factors. Such an empirical approach would not be feasible using labor market survey data due to concerns about endogeneity and omitted variable bias. These concerns cannot be driving the results seen later in this paper as the randomization of D_i avoids that problem by construction.

The approach in this paper is subtly different to the matched-pairs approach of Bertrand and Mullainathan. However, it is similar to the Kroft et al. approach in the sense that they use unique resumes which are then assigned a randomized unemployment duration. For Kroft et al. four resumes are created for each MSA-job pair using simple rules for their research assistants to follow. The actual contents of each resume are drawn from a pool of over 1,200 real resumes. There is the potential that an identical resume was created by the same or different research assistants and sent to more than one job but the authors do not track this. Instead, they have their assistants record the objective facts of the resumes and note the duration of unemployment it was assigned. Then, the assistant moves on to the next MSA-job pair, creating another four unique resumes. Kroft et al. explicitly thank 14 research assistants. This labor intensive approach provides a bounty of data but is not feasible for an individual researcher.

In contrast, the matched-pairs approach creates two (or more) versions of each resume and then examines the difference in callback rates as a function of the varied characteristic. Such an approach completely avoids concerns that results could be driven by systematic differences between the resumes which are separated into groups by randomization. For large enough samples true randomization ensures the estimated $\hat{\delta}$ would be the same but the empirical interpretation is different in a minor way. This paper's set up requires $\hat{\delta}$ to be interpreted as the difference in the mean callback rate between the group of people randomly assigned to have an online degree rather than a traditional degree. Formally, if the group of people assigned to online is group O and those assigned to traditional education are group T then

$$\hat{\delta} = \overline{y}_{i \in O} - \overline{y}_{j \in T}$$

Where $\overline{y}_{i \in O} = E_{i \in O}[\hat{y}_{D_i=1}]$ and $\overline{y}_{j \in T} = E_{j \in T}[\hat{y}_{D_j=1}]$ and $i \neq j$. This approach computes the mean of all callback rates, conditional on other characteristics, of all the members of each group. The approach leaves the possibility that differences between groups could drive the effects *if* the randomization fails. If the study was set up like Bertrand and Mullainathan, $\hat{\delta}$ would instead be the average of *differences* in callbacks for each individual when they are switched from an online to a traditional degree;

$$\hat{\delta} = E_{i \in I}[\hat{y}_{D_i=1} - \hat{y}_{D_i=0}]$$

Bertrand and Mullainathan's approach essentially computes a pair predicted callback rates for each (fictional) individual: One for each value of the varied characteristic. Then, the estimated coefficient represents the average difference for all individuals. It's similar to doing a medical study on twins. In contrast, the results in this paper report the average difference between two groups of individuals who are assumed to be no different on average because of randomization. This is similar to a randomized trial with treatment and control groups who are not explicitly paired or matched to someone such as a twin. With valid randomization, the assumption is that the differences between the group outcomes are only related to the treatment. The two approaches are closely related but will only be identical if randomization is perfect, leaving all covariates completely orthogonal to the randomization.

The benefits of the matched pairs approach are clear. First, it is not subject to any concerns about randomization. Second, the matched pairs approach significantly economizes on researcher effort. This is because instead of creating N resumes, associated email addresses, phone numbers, contact details, and cover letters, and then randomizing across them, the researcher can create an equivalent sample size by merely creating N/M resumes and having M versions of each resume, where $M = 2$ is typical. The only downsides of the matched-pairs approach is that caution must be taken to never send both versions of the resume to the same employer. Mainly this is to avoid raising suspicion and undermining the outcome of the study. In addition, sending multiple different resumes to the same employer creates the potential for equilibrium effects with one of the submitted resumes crowding out another, potentially in a biased manner unless care is taken. The administrative and clerical burden with these studies is significant for the researcher and having to check each application has not been sent before would add further steps, and the potential for bias-inducing error, into the process.[17]

This study's initial set up was chosen as it allows the study to pivot to a matched-pairs design by reusing the same resumes while switching of the type of degree reported. Data collection has begun for the matched-pairs phase but not enough has been collected to provide valid inference.

3.4.2 Data

This version of the paper is based on the outcomes of a total of 734 job applications completed for 29 different resumes.[18] As detailed in Section 3.3 the experimental approach was to search for suitable positions advertised in each of the various careers (nursing, business administration, engineering, and so on) and then randomly assign a resume to that position from the appropriate career group. This method resulted in 102 callbacks from the 734 applications completed, a raw callback rate of 13.9%. This overall callback rate is higher than many correspondence studies. For example, Kroft et al. have a callback rate of just 4.7%, while Bertrand and Mullainathan have a rate of 8.05%. The higher overall callback rate here is likely because the resumes are quite well-matched to the available positions. The applicant always has a degree in the field, a GPA greater than 3.0 (GPAs on the resume were always listed as higher than 3.0 to help achieve higher overall callback rates), is currently working in a position with exactly the same job title, and meets (and often exceeds) all of the minimum requirements for the posted opening. In addition, the openings applied to were less than 48 hours old in all cases. Table 3.1 provides summary statistics on the (fictional) demographic characteristics of the resumes used to apply for positions. These statistics explain how the randomization actually fared (comparing how balanced characteristics are across degree-types) and guides the selection of the preferred empirical specification in Section 3.5.

3.4.3 Did the Experimental Randomization Work?

As detailed in Section 3.3, the paper relies on a treatment and control group design (like Kroft et al.) rather than a matched-pairs approach (like Bertrand and Mullainathan). While Kroft et al.

[17]Even if dates are restricted when searching for available openings, advertisements are often re-posted a few days later.

[18]More will be added in time.

have 14 research assistant and complete thousands of applications, a sole researcher cannot achieve such large sample sizes in a reasonable time frame. Table 3.1 provides the summary statistics for the characteristics for Online and Traditional degree holders. Despite randomization, traditional degree holders are slightly more likely to be male, have slightly more years of experience, and are more likely to have attended a less selective college (as measured using U.S. News undergraduate admission rates so that higher numbers indicate less selectivity.).[19]

Table 3.1: Summary Statistics by Degree Held

Type of Degree	**Statistic**	**Callback Rate**	**Sex**	**GPA**	**Selectivity**	**Experience**
Traditional Degree	Mean	0.20	0.47	3.50	0.65	1.63
(N=15)	Std. Dev.	(0.41)	(0.52)	(0.21)	(0.17)	(0.81)
Online Degree	Mean	0.07	0.43	3.49	0.57	1.43
(N=14)	Std. Dev.	(0.27)	(0.51)	(0.21	(0.12)	(0.55)

Table reports summary statistics for the characteristics for Online and Traditional degree holders. Despite randomization, traditional degree holders are more likely to be male but more likely to have attended a less selective school. Selectivity is measure using U.S. News Undergraduate Admission Rates. Higher values indicate less selective institutions.

The randomization was broadly successful. However, important differences remain across the two groups of resumes. As a result, controls for experience, GPA, college selectivity and so on will be necessary to ensure observed callback rates are not being driven by differences in group characteristics.

3.5 EMPIRICAL ESTIMATES

As detailed earlier, the paper's empirical approach is to estimate the difference between callback rates for two groups: those whose resumes convey that they have an online degree compared to those with a traditional face-to-face degree. The estimating equation is of the form $y_{i,k} = \beta X_i + \delta D_i + \epsilon_i$. To aid intuition, the empirical approach is equivalent to a difference-in-difference estimation where the randomization of the treatment ensures that the pre-existing difference between treatment and control groups is zero *by construction.*

[19]See `http://colleges.usnews.rankingsandreviews.com/best-colleges`

The correspondence study set up, with multiple observations for a number of individuals, results in data that forms an unconventional panel data-set: One that has repeated observations for each individual but no time component. As the data can be considered as a panel a fixed or random effects specification could be estimated in addition to a pooled-OLS. Typically, if there is reason to believe that differences *across* entities have an influence on the dependent variable then a random effects model should be used.[20]. Random effects estimations assume that the error term is not correlated with the independent variables to allow for values that are fixed for each individual to play a role as explanatory variables.[21] Upon estimating a random effects model a Breusch-Pagan Lagrange Multiplier (LM) test is typically completed. This test examines if treating the data as a panel is appropriate. In particular, the null hypothesis in the LM test is that variances across entities is zero. That is, there is no significant difference across units of observation: No panel effect. Based upon the data collected so far, the Breusch-Pagan null hypothesis cannot be rejected.[22] As the LM test suggests that the data can be treated as cross-sectional, OLS estimation with standard errors clustered at the person ID level is appropriate. Table 3.2 reports the empirical estimates of the coefficient of interest using various OLS Probit specifications. The outcome of interest is whether or not a callback was received and a Probit specification is ideal as the outcome takes on only the values zero and one. The table adds controls with each estimation. The preferred specification in the sixth column includes controls for all factors likely to effect callback rates: Sex, experience, college selectivity, career/field, and GPA.

While the effect of an online degree is negative in all specifications in Table 3.2, it is not until controls for experience and career/field (nursing, engineering, and so on) are added that the effect becomes significantly different from zero. In the estimation, experience is measured as the difference between the actual number of years of experience and the minimum required experience for a particular job-applicant pair. As one of the issues with the randomization was that selectivity was higher for the schools of those taking online degrees, it is not surprising to observe that controlling for this increases the statistical significance of the effect of online education considerably. Table 3.2 first reports raw Probit estimates and associated t-statistics. These raw estimates do not have straightforward economic interpretations. To aid intuition, the table then reports post-estimation marginal effects. The marginal effects are percentage point differences. That is, in the specification

[20]See the excellent notes of Oscar Torres-Reyna at http://www.princeton.edu/ otorres/Panel101.pdf

[21]Random effects specifications typically cause concerns about omitted variable bias but that is less of a concern here as there are no missing variables by construction.

[22]The panel-probit random effects estimations (reflecting similar findings to Table 3.2 as suggested by the test) and the LM test statistics are reported in Appendix D.

Table 3.2: Probit Callback Rate - OLS Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Online Degree (=1)	-0.355	-0.330	-0.282*	-0.354**	-0.361***	-0.380***
	(-1.85)	(-1.72)	(-2.27)	(-3.20)	(-3.62)	(-3.89)
Marginal Effect	-7.53%	-6.98%	-5.62%	-7.01%	-7.08%	-7.44%
Observations	734	734	734	734	734	734
Experience		Y	Y	Y	Y	Y
Career/Field			Y	Y	Y	Y
College Selectivity				Y	Y	Y
GPA					Y	Y
Sex						Y

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The table reports the co-efficient of interest from a Probit estimation with controls added sequentially. The co-efficient estimates on those controls are not reported. Standard errors are clustered at the Person ID level. The marginal effects reported should be interpreted as percentage point differences. That is, in the specification in column six, there is a 7.44 percentage point difference between traditional and online degree holders, all else equal.

in column six, the estimation reports that there is a 7.44 percentage point difference in callback rates between traditional and online degree holders, all else equal.[23] A 7.44 percentage point difference reflects a 2:1 ratio of callbacks for the two types of resume. That is, a resume reflecting a traditional degree appears to receive twice as many callbacks for interview as a resume reporting an online degree. As can be seen, the size of the estimate is consistent across specifications (although specifications with more controls lead to sharper point-estimates of the treatment effect). To give these estimates some context, Bertrand and Mullainathan found whites were about 1.5 times more likely to receive a callback for interview as blacks, and Kroft et al. found that someone who is

[23]Note that marginal effects are provided only to aid intuition. There are several well-known issues with computing marginal effects when the estimation involves a number of dummy variables. The main problem is that the effects crudely consider the effect of the variable of interest at the average of variables that have no such interpretation. For example, the marginal effects estimated reflect a process which sets the value of gender to its average value in the data, a meaningless number. The command does the same for the dummy variables for Career/Field.

just one month unemployed is about 1.8 times more likely to receive a callback for interview than someone who has been unemployed for eight months.

3.5.1 A Clear Causal Relationship?

As mentioned in Section 3.2, a potential concern with audit studies is that results are driven not by the experimental variation but by how the resumes compare to their subjective competition. Take Bertrand and Mullainathan's findings that blacks receive fewer callbacks. If identical black and white resumes are interpreted differently, perhaps because employers have learned that one of the groups tends to oversell or fabricate their experience and skills more than another, then the effects Bertrand and Mullainathan find could be simply an artifact of a labor market norm they (and perhaps employers, subconsciously) are unaware of. In that case, employers don't call blacks with a given resume quality but do call whites because they expect a black applicant to overstate their abilities in order to *combat* expected discrimination. The signal of ability the employer takes from each resume is different not because the employer is discriminating but because of the actions of other similar applicants. This is something that a researcher cannot control. As Bertrand and Mullainathan find that employers respond only slightly more often to higher-quality Black resumes, an employer skepticism explanation is a potential concern with their findings.

A similar concern arises in this paper. An employer may be used to seeing a person who has an online degree having other compensating characteristics. When they do *not* see this, they infer something about the candidate's ability that the researcher is, again, not able to control for. This problem is caused by attempting to hold all else equal when the changes made should not result in all else remaining equal. In this paper, such concerns would lead to lower returns to resume quality for online degree holders. As an example, if employers expect to see online degree holders having compensating characteristics such as more experience or higher GPAs, then callback rates for online-degree holders will not increase as much with increases in GPA or years of work experience. Econometrically, this means that a regression interacting experience or GPA with an indicator for having an online-degree will report a negative effect. Examining this concern requires a basic difference-in-difference estimation approach, illustrating how different years of work experience or a different GPA effects online and traditional degree holders differently. Table 3.3 reports the estimates from the following estimation;

$$y_{i,k} = \beta X_i + \lambda D_i + \gamma Exp_i + \delta D_i * Exp_i + \epsilon_i$$

where Exp_i is the difference between the actual and required experience for the job-applicant pair, and all else is as described in Section 3.4.

The coefficient of interest is the interaction between having an online degree ($D_i = 1$) and more experience (Online Degree $\times$ Experience). A non-zero estimate for δ would suggest that there is a concern that the returns to the same characteristics across degree types are different. In the table it appears that firms are reacting to more than just the study's controlled experimental variation. As mentioned earlier this may be because their experience with online degree holders changes how they view the rest of the resume. Employers appear to view experience as a positive in all specifications (although estimates are often not statistically significant) but for those with an online degree the effect of experience is wiped out (again, not statistically different from zero in most specifications). Notice that the additional demands this puts on the data causes the coefficient on for the dummy for online degree to fall below standard levels of statistical significance in most specifications). Repeating the same analysis but interacting the online degree dummy with GPA, Gender, or Career/Field reports no statistically significant interactions. With more data, further examination of how online degrees effect males and females and various labor markets (nurse, engineers, and so on) would be feasible.

The consistently negative estimate on the interaction term in Table 3.3 suggests employers are responding less to additional years of experience for online degree holders. This raises a concern - but does not immediately imply - that the findings in Table 3.2 could be *caused* by more than just the simple experimental variation that was created. If this was the case, results are being driven not by having an online degree, but by having an online degree and a resume that does not have what others with online degrees tend to have, such as greater experience or other compensating characteristics. Unfortunately, this is a limitation of correspondence studies as discussed earlier. One way to control for this effect would be to use difference-in-differences. A researcher would need access to actual representative resumes of those who have online degrees and those who have traditional degrees. Then, the underlying source of the resumes provides a control for the way the two types of resumes may be interpreted by employers. However, gaining access to actual resumes of actual job applicants is challenging. It would be tempting to just use resumes posted online that report online degrees, but those resumes are likely not representative of actual applicants for open positions. Without a clear solution to this potential concern, the effects reported in this

Table 3.3: Callback Rate - Interaction with Years of Experience

	(1)	(2)	(3)	(4)	(5)
Experience	0.163	0.187	0.170	0.228*	0.210*
	(1.69)	(1.93)	(1.71)	(2.25)	(2.16)
Online Degree	-0.226	-0.205	-0.286*	-0.247	-0.264
	(-1.23)	(-1.47)	(-1.99)	(-1.73)	(-1.84)
Online Degree × Experience	-0.157	-0.123	-0.103	-0.167	-0.173
	(-1.07)	(-0.76)	(-0.61)	(-0.96)	(-0.98)
Observations	734	734	734	734	734
Career/Field		Y	Y	Y	Y
College Selectivity			Y	Y	Y
GPA				Y	Y
Sex					Y

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The table reports the co-efficients of interest from a Probit estimation with controls added sequentially. The co-efficient estimates on control variables are not reported. Standard errors are clustered at the Person ID level.

paper should properly be viewed as the effect on callbacks of both having an online education and doing nothing to your resume and background to mitigate the effects that it may cause in the labor market. The estimates presented can then be viewed as suggesting that callback rates for interview will be lower for those who pursue an online education *and* do not equip themselves with other compensating characteristics to counteract the effect of that decision.

3.6 CONCLUSION

To date, studies of the potential move towards online undergraduate education have focused on learning outcomes. Under the assumption that costs are lower, it appears that authors believe if learning outcomes can be shown to be at least as good, then online education will become the norm. To these authors, the future of higher education is a function purely of production costs and learning outcomes. There is no space at the table for market forces to work their magic. In contrast, this paper considers that the future of higher education may emerge via an interaction between producers (universities and colleges) and consumers (students and perhaps their families). Labor market outcomes from online degree programs are a critical factor of the future of higher education.

To examine the relationship between online degrees and labor market outcomes, the paper presents the findings of a correspondence study. It compares one metric of labor market success, callbacks for interview, for individuals with online degrees relative to callbacks for those with traditional degrees. Despite a relatively small data set, the empirical estimates strongly suggest that employers are skeptical of online degree programs. The difference in callbacks is slightly larger than the gap in callbacks found in similar studies on the effects of race and unemployment duration. The estimates suggest that a future where college educations are delivered to students sitting in coffee shops or their parents' basements is not a foregone conclusion.

An important caveat is that the estimates suggest that callback rates will be lower for those who pursue an online education *and* do not take any steps to counteract that decision. At the same time, this still implies employers are not yet ready to consider individuals with online degrees as being as attractive as those with traditional degrees. Indeed, they may never do so.

An additional but somewhat moot caveat is that it is not clear what aspect of a traditional college education employers are responding favorably to. They may believe human capital formation is diminished in online programs relative to traditional degrees, they may believe the individual will be less socially adept, are inferring some socioeconomic characteristics, or they may feel a traditional college education gives students something more than just grades written on a piece of paper. While understanding why students with online degrees fare poorly in the labor market is important, it is not the focus of this paper. This paper is agnostic about why and only cares about if labor market outcomes are affected by how a degree was earned. Until labor market outcomes are comparable, demand for traditional face-to-face learning from a professor in a classroom setting on existing college campuses will likely remain quite high. Moreover, allowing for an even broader conception of how market forces might play out, a reduction in the cost of completing some introductory college classes may simply increase demand for more advanced or graduate-level classes which may be less amenable to routinized online instruction.

Ultimately, the interaction between producers and consumers of college education, and not learning outcomes or administrative costs alone, will determine how college education will be delivered in the future.

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APPENDIX A

ADDITIONAL ESTIMATES FOR CHAPTER ONE

In Table 1.3 (and Table 1.6 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 A1 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.

Table A1: 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.

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.

In addition, in Table 1.3 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 A2 presents a series of regression estimates which correspond to various alternatives to the specificaiton presented in column 1 of Table 1.3 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 A2: Robustness to Alternate Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variable = Log Annual Wages							
Offers Coverage	0.787***	0.777***	0.783***	0.784***	0.696***	0.684***	0.681***	0.675***
	(0.0471)	(0.0472)	(0.0470)	(0.0471)	(0.0471)	(0.0465)	(0.0465)	(0.0473)
After ACA	0.0842	0.0820	0.0734	0.0696	0.0703	0.0737	0.0728	0.0762
	(0.0552)	(0.0554)	(0.0550)	(0.0550)	(0.0560)	(0.0551)	(0.0551)	(0.0551)
Offers Coverage * After ACA	-0.0672	-0.0621	-0.0562	-0.0565	-0.0932	-0.0968	-0.0954	-0.0973
	(0.0629)	(0.0629)	(0.0625)	(0.0623)	(0.0624)	(0.0613)	(0.0613)	(0.0612)
Ln (Total Expenses)	0.00220	-8.78e-05	0.000233	0.00100	-0.0125	-0.00243	-0.00286	-0.000286
	(0.00863)	(0.00864)	(0.00867)	(0.00864)	(0.00856)	(0.00858)	(0.00855)	(0.00858)
Offers Coverage * Ln (Total Exp.)	0.0248***	0.0253***	0.0256***	0.0244***	0.0212**	0.0194**	0.0194**	0.0165*
	(0.00921)	(0.00922)	(0.00923)	(0.00919)	(0.00906)	(0.00904)	(0.00902)	(0.00904)
Ln (Total Expenses) * After ACA	-0.0306***	-0.0295**	-0.0284**	-0.0272**	-0.0287**	-0.0270**	-0.0268**	-0.0281**
	(0.0117)	(0.0117)	(0.0117)	(0.0117)	(0.0116)	(0.0115)	(0.0115)	(0.0115)
Ln (Total Exp.) * ACA * Coverage	0.0288**	0.0274**	0.0262**	0.0255**	0.0302**	0.0282**	0.0281**	0.0292**
	(0.0126)	(0.0126)	(0.0126)	(0.0125)	(0.0123)	(0.0122)	(0.0122)	(0.0122)
Observations	12,111	12,111	12,111	12,111	12,031	12,031	12,031	12,031
Age		Y	Y	Y	Y	Y	Y	Y
Region			Y	Y	Y	Y	Y	Y
Race				Y	Y	Y	Y	Y
Education					Y	Y	Y	Y
Sex						Y	Y	Y
Marital Status							Y	Y
Industry								Y

"x ACA" = 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. 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 A3 interact all controls with a post-ACA dummy. The effect size and significance remains almost constant across specifications.

Table A3: Robustness to Post-ACA Period by Firm Location, Industry and Demographic Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variable = Log Annual Wages							
Offers Coverage	0.787***	0.779***	0.785***	0.786***	0.700***	0.690***	0.689***	0.692***
	(0.0471)	(0.0471)	(0.0470)	(0.0472)	(0.0474)	(0.0468)	(0.0468)	(0.0484)
After ACA	0.0842	0.125	0.210	0.0523	0.222	-0.161	0.0264	0.133
	(0.0552)	(1.823)	(1.812)	(1.795)	(1.714)	(1.691)	(1.691)	(1.686)
Offers Coverage * After ACA	-0.0672	-0.0646	-0.0575	-0.0572	-0.0981	-0.107*	-0.108*	-0.125*
	(0.0629)	(0.0629)	(0.0626)	(0.0628)	(0.0634)	(0.0623)	(0.0623)	(0.0639)
Ln (Total Expenses)	0.00220	0.000359	0.000632	0.00131	-0.0120	-0.00298	-0.00317	0.000693
	(0.00863)	(0.00864)	(0.00869)	(0.00865)	(0.00858)	(0.00860)	(0.00860)	(0.00866)
Offers Coverage * Ln (Total Exp.)	0.0248***	0.0254***	0.0255***	0.0244***	0.0211**	0.0195**	0.0195**	0.0150*
	(0.00921)	(0.00921)	(0.00923)	(0.00919)	(0.00906)	(0.00904)	(0.00902)	(0.00909)
Ln (Total Expenses) * After ACA	-0.0306***	-0.0303***	-0.0290**	-0.0274**	-0.0294**	-0.0253**	-0.0254**	-0.0286**
	(0.0117)	(0.0118)	(0.0118)	(0.0117)	(0.0117)	(0.0116)	(0.0116)	(0.0116)
Ln (Total Exp.) * ACA * Coverage	0.0288**	0.0272**	0.0260**	0.0250**	0.0300**	0.0274**	0.0273**	0.0308**
	(0.0126)	(0.0126)	(0.0126)	(0.0125)	(0.0123)	(0.0122)	(0.0122)	(0.0123)
Observations	12,111	12,111	12,111	12,111	12,031	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	Y x ACA
Region			Y x ACA	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	Y x ACA
Education					Y x ACA	Y x ACA	Y x ACA	Y x ACA
Sex						Y x ACA	Y x ACA	Y x ACA
Marital Status							Y x ACA	Y x ACA
Industry								Y x ACA

"x ACA" = 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

PROOFS FOR CHAPTER ONE

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,

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

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

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

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;

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

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

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

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

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

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.

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

$$(P_B-w_B^n)l_n^B(w_B^n) \geq (P_B-w_B^d)l_n^B(w_B^d)$$

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;

$$(P_B-d-w_B^d)l_d^B(w_B^d) \geq (P_B-d-w_B^n)l_d^B(w_B^n)$$

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:

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

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

$$X'(w_B^n)=(P_B-w_B^n)(1-k)l_n'^B(w_B^n)+kdl_n'^B(w_B^n)l_n'^B(w_B^n)-(1-k)l_n^B(w_B^n)>0$$

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;

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

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;

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

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

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

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;

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

in and out of unemployment;

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

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

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

While the algebra involved here is omitted in order 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;

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

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;

$$(P_B - d - r_B) l_d^B(r_B) = (P_B - d - w_B) l_d^B(w_B)$$

Similarly, for type n employers;

$$(P_B - wh_d) l_n^B(wh_d) = (P_B - w_B) l_n^B(w_B)$$

which means that

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

and

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

implying;

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

because

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

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

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

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

where

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

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

.

Similarly, it can be shown that

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

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

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

and

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

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 $F_d^B(w_B)$;

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

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

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

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.

1. Expected Earnings and γ_d

Consider the mean earnings of type A workers given by Mortensen;

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

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

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

and that if $\gamma_d = 0$ then

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

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 $\partial E^B(w_B)/\partial\gamma_d < 0$.

2. 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;

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

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

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

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

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

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

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

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

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

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

and similarly,

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

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

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

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

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

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.

3. Equilibrium Effects on Labor Stocks

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

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

and

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

then

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

where

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

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

and

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

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

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

which implies $\Lambda < 0$. For Ω;

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

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

so we have that

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

which implies $\Omega < 0$. However, it is still the case that $\frac{\partial l_d^B(w_B)}{\partial \gamma_d} < 0$ because $|\Lambda| > |\Omega|$;[1]

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

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

This expression is positive if

$$(1+\kappa_{0B}^{k})(k\kappa_{1B}-\kappa_{1B})+(1+\kappa_{1B}^{k})(k\kappa_{0B}-\kappa_{0B}))$$

$$<$$

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

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;

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

so that

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

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

because

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

and

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

because

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

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

and

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

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

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

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

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

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

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

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

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.

Firm-worker separation rates are

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

where

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

. and

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

When $\delta_A\le\delta_B$ 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

ACA COVERAGE MANDATES

The Patient Protection and Affordable Care Act is 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.[1]

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).[2] 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[3])
- 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)

It is worth noting that firms who self-insure, under the provisions of the ERISA (1974), are *exempt* from providing the Essential Health Benefits.[4] 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.

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

[2]See this article: `https://www.cms.gov/CCIIO/Resources/Files/Downloads/uniform-glossary-final.pdf`

[3]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/`.

[4]Kaiser Family Foundation website (accessed March 12, 2016) - `http://kff.org/interactive/implementation-timeline/`

APPENDIX D

LM TEST FOR PANEL EFFECTS FOR CHAPTER THREE

As mentioned in Section 3.5, the data created by the correspondence study can be considered as either a panel dataset or treated as cross-sectional. The determination of which approach should be used is typically made based upon a post-estimation Breusch-Pagan Lagrange Multiplier Test. The test examines the null of ”no panel effect.”

Table D1: Probit Callback Rate - Panel Data Random Effects Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Online Degree (=1)	-0.384*	-0.356*	-0.297*	-0.347**	-0.349***	-0.359***
	(-2.20)	(-1.99)	(-2.43)	(-3.09)	(-3.57)	(-3.85)
Observations	734	734	734	734	734	734
Experience		Y	Y	Y	Y	Y
Career/Field			Y	Y	Y	Y
College Selectivity				Y	Y	Y
GPA					Y	Y
Sex						Y

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The table reports the co-efficient of interest from a Random Effects Probit estimation with controls added sequentially.The co-efficient estimates on control variables are not reported. Notice that the co-efficient of interest is again (as in Table 3.2)consistent in size and gains statistical significance as controls are added.

Repeating the analysis of Table 3.2 using the random effects panel probit specification, gives the estimates in Table D1. Using the specification in column five as the basis for the test, the Breusch-Pagan Lagrange Multiplier test statistic does not recommend rejecting the null of no panel effect (χ^2=.87 with a p-value of .17). Failure to reject suggests that treating the data as cross-sectional and using OLS is the right approach to econometric estimation. Easing any concerns about which approach to use, the point estimates from the panel probit and estimates from the OLS probit with clustered standard errors are very similar in size. Indeed, in Table D1 the co-efficient of interest is (as in Table 3.2) consistent in size and gains statistical significance as controls are added.