

Essays on how health and education affect the labor market outcomes of
workers

by

Sheryll Namingit

M.A., University of the Philippines, 2006

M.A., Kansas State University, 2013

AN ABSTRACT OF A DISSERTATION

submitted in partial fulfillment of the
requirements for the degree

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Abstract

This dissertation consists of three essays on how health and education affect the labor market outcomes of workers. Health and education issues have been key determinants of labor demand and supply. In light of increasing incidence of health problems and the rapid growth of post-baccalaureate certificates in the US, this dissertation seeks to answer questions about labor market outcomes of workers with poor health history and with post-baccalaureate certificates.

The first essay which I co-authored with Dr. William Blankenau and Dr. Benjamin Schwab uses a résumé-based correspondence test to compare the employment consequences of an illness-related employment gap to those of an unexplained employment gap. The results of the experiment show that while the callback rate of applicants with an illness-related employment gap is lower than that of the newly unemployed, applicants with illness-related employment gaps are 2.3 percentage points more likely to receive a callback than identical applicants who provide no explanation for the gap. Our research provides evidence that employers use information on employment gaps as additional signals about workers' unobserved productivity.

Co-authored with Dr. Amanda Gaulke and Dr. Hugh Cassidy, the second essay tests how employers perceive the value of post-baccalaureate certificates using the same methodology in the first essay. We randomly assign a post-baccalaureate certificate credential to fictitious résumés and apply to real vacancy postings for managerial, administrative and accounting assistant positions on a large online job board. We find that post-baccalaureate certificates are 2.4 percentage points less likely to receive a callback than those without this credential. However, this result is driven by San Francisco, and there is no effect in Los Angeles or New York. By occupation, we also find that there is only significant negative effect in administrative assistant jobs, and there is none in managerial or accounting assistant jobs.

A typographical error made in the résumés of certificate holders regarding the expected year of completion of the certificate may also contribute to negative effects of a certificate.

Using NLSY79 data, the third essay tests whether the source of health insurance creates incentives for newly-diagnosed workers to remain sufficiently employed to maintain access to health insurance coverage. I compare labor supply responses to new diagnoses of workers dependent on their own employment for health insurance with the responses of workers who are dependent on their spouse's employer for health insurance coverage. I find that workers who depend on their own job for health insurance are 1.5-5.5 percentage points more likely to remain employed and for those employed, are 1.3-5.4 percentage points less likely to reduce their labor hours and are 2.1-6.1 percentage points more likely to remain full-time workers.

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Approved by:

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Soli Deo gloria!

Dedication

When life gives you lemons, make lemonade.

So I did.

This is for you, Michael Dean Wales, my hero.

Chapter 1

Sick and Tell: A Field Experiment

Analyzing the Effects of an

Illness-Related Employment Gap on

the Callback Rate

1.1 Introduction

Poor health may lead to a temporary gap in employment. Such joblessness can occur if health problems result in reduced ability to function in the workplace or if treatment interferes with work activities. For example, a worker may be diagnosed with cancer and exit the workforce for treatment. Even upon recovery and receiving a positive prognosis, labor market consequences of the illness may persist. Both the gap in the employment record and the health issue itself may be relevant to employers in deciding how to respond to a worker's application as she re-enters the labor market. In this paper, we present the results of a résumé-based correspondence test designed to explore the effects of an illness-related employment gap on the probability that an applicant will receive a callback upon applying for a job.

Recent research confirms that employers are less likely to make callbacks to applicants with an employment gap on their résumés. However, little work has been done to explore how this propensity changes when the gap results from an illness. On one hand, applicants with illness-related employment gaps may be particularly risky. Employers may be concerned that individuals with illness-related gaps will have lower current and future productivity given weaker physical strength, greater routine medical needs, and a higher likelihood of being sick in the future. There may be concerns as well that human capital depreciates more quickly in employment gaps for those dealing with health issues, as treatment takes precedence over job market considerations.¹ Moreover, employees with a history of poor health may impose higher health care costs on employers who offer health insurance.

A strand of the literature provides evidence that employers are indeed sensitive to the health status of workers. In jobs where employers offer employer-sponsored health insurance, high health risk workers such as women, those who are obese and those who smoke, tend to receive lower wages to compensate for the higher health insurance premiums paid by employers.²

On the other hand, an unexplained gap may provide a different sort of negative signal to employers. Employer screening models suggest that employment gaps are negatively correlated with unobserved productivity.³ As a result, employers may feel that the unemployed are less productive on average. This may be reinforced by the suspicion that companies who have interviewed the applicant for other positions have found unfavorable indicators that prevented job offers.⁴ Anecdotal evidence of such concerns is not difficult to find. Some employers even explicitly state in job ads that they do not consider unemployed job applicants.⁵

If the gap is explained as resulting from an illness, the applicant may be exempt from

¹Human capital models suggest that skills of potential workers depreciate through periods of joblessness. See [Acemoglu \(1995\)](#); [Ljungqvist and Sargent \(1998\)](#).

²See [Bhattacharya and Bundorf \(2009\)](#); [Cowan and Schwab \(2011a, 2016\)](#).

³See [Vishwanath \(1989\)](#); [Lockwood \(1991\)](#).

⁴See [Oberholzer-Gee \(2008\)](#).

⁵Legal experts say that the practice probably does not violate discrimination laws because unemployment is not a protected status, like age or race. However, New Jersey recently passed an anti-discrimination law against the unemployed and other states are considering similar legislation ([Rampell \(2011\)](#)).

these conditional assessments. Illness strikes productive and unproductive workers alike. Moreover, a worker currently re-entering the labor market after an illness has not been subject to the scrutiny of other employers to the same extent as those who have been looking for work since the separation from previous employment. The expected productivity of a worker with an illness-related employment gap may be closer to that of the general worker population than to that of the long-term unemployed. For job applicants who experience a health shock, providing information on the reason for the employment gap may mitigate, if not eradicate, the unemployment bias.

We begin our study by developing a theoretical framework that helps to disentangle these competing effects. The model shows under what conditions researchers should expect that revealing the cause of a gap will increase or decrease the callback rate. The key to the results is that productivity is negatively correlated with experiencing an employment gap, but uncorrelated with becoming ill. Employment costs related to health issues, in contrast, are correlated with prior illness and uncorrelated with productivity. As a result, an unexplained gap gives negative information about expected productivity and an explained illness-related gap gives negative information about expected health costs. The clarity of these signals and the distributions of productivity and health costs determine the relative callback rates.

We then turn to an experiment that explores callback rates contingent on employment gaps that are either explained or unexplained. In our field experiment, carefully prepared résumés and corresponding cover letters were sent to employers who advertised vacancies in online job boards. For each vacancy, we sent three types of résumés. One résumé contained an explained illness-related employment gap while another contained an unexplained employment gap. These were in contrast to a third résumé where the applicant was newly unemployed (no gap). For illness-related and unexplained employment gaps, the résumés showed no employment over the previous seven months or more.

To signal an illness-related employment gap, a phrase in the cover letter explained that the employment gap was due to a physical illness followed by a full recovery. An additional signal on medical history was sent via information in the résumé that indicates involvement in a

cancer recovery support group. The corresponding cover letters of résumés with unexplained gaps did not provide any explanation for the gap. For the résumé of newly unemployed applicants, the length of the gap is limited to less than two months. Based on the literature, this is too short a gap to bring about adverse effects. The corresponding cover letter of newly unemployed applicants notes that the applicant left the last job because her family had to move from another state and that she is currently looking for a new job. We chose this control as our ‘no gap’ group because applicants who are currently working tend to have fewer callbacks ([Kroft et al. \(2013\)](#)). From March to September, 2016, we sent 3,771 résumés to 1,257 sales, administrative, and accounting assistant jobs.

Outcomes are measured in terms of differences in the callback rate of each type of résumé. The results of the experiment show that newly unemployed applicants had the highest callback rate (27.4%). Consistent with previous studies, résumés with an employment gap received lower callback rates, indicating that such gaps negatively affect hiring outcomes. However, résumés with an explained illness-related gap received a higher callback rate than résumés with an unexplained gap (25.6 % versus 23.3%). Within the context of our theoretical model, these results suggest that the negative productivity signal of an unexplained gap outweighs undesirable factors associated with poor health history.

Our work contributes to the literature that examines how hiring probabilities depend on the duration of unemployment (i.e. duration dependence). Results from studies using non-experimental methods are mixed. In a review of the literature, [Machin and Manning \(1999\)](#), find little evidence supporting duration dependence. In contrast, a separate set of studies concludes that duration dependence plays a significant role in labor market outcomes.⁶ As pointed out by [Oberholzer-Gee \(2008\)](#), non-experimental studies of duration dependence suffer endogeneity problems. The effects of employment gaps can be difficult to separate from the effects of other important worker characteristics that determine employment prospects.

Results from studies that use correspondence tests, where identification is derived from experimentally induced variation, provide more consistent results. Several such studies find that employment gaps beyond a threshold duration negatively affect the likelihood of being

⁶See [Lynch \(1989\)](#); [Van den Berg and Ours \(1996\)](#); [Imbens and Lynch \(2006\)](#); [Shimer \(2008\)](#).

invited for an interview.⁷ Some refinements of these findings have been considered. [Eriksson and Rooth \(2014\)](#) find that while contemporary employment gaps negatively affect the likelihood of getting a callback, past employment gaps do not. [Kroft et al. \(2013\)](#) show that duration dependence is stronger when the labor market is tighter. [Ghayad \(2013\)](#) shows that positive traits such as work experience can compensate for employment gaps.

To our knowledge, [Baert et al. \(2016\)](#) is the only research that investigates whether the effects of long illness-related gaps are distinct from equally long unexplained employment gaps. However, the paper only focuses on the effects of mental illness (i.e. depression) and do not include physical illness. Results from this smaller-scale correspondence study show no significant effect of an employment gap caused by mental illness relative to an unexplained employment gap in Belgium.

Studies that investigate illness-related employment gaps are important, in part, because health issues affect a large part of the potential labor force in the US. For example, in 2014, the number of working age adults who were not in the labor force because of illness or disability reached 13 million, or 6.4 percent of the US population.⁸

Our results provide an additional refinement by showing that explaining the gap as resulting from a non-mental medical issue can dampen duration dependence in the US. While not explicitly stated, the applications received by employers implied that each health-related gap was due to cancer. We chose cancer since its characteristics are consistent with our experimental design. Cancer treatment plausibly causes an employment gap for treatment. Patients who receive a good post-treatment prognosis can return to the labor market with undiminished productivity. Cancer is perceived to be onset-uncontrollable, which prevents employers from forming inferences about productivity from the diagnosis. Moreover, there is a chance that former patients may impose costs to employers through a future relapse or related healthcare issues. Thus, cancer per se, is not the focus of the paper but rather a reasonable proxy for health-related gaps more generally.

Nonetheless, by explaining the gap as related to cancer, we provide evidence on another

⁷Specifically, 9 months for [Eriksson and Rooth \(2014\)](#), 18 months for [Oberholzer-Gee \(2008\)](#), 6 months for [Kroft et al. \(2013\)](#) and 6 months for [Ghayad \(2013\)](#).

⁸Based on the 2015 Annual Social and Economic Supplement to the Current Population Survey.

poorly understood issue: the true impact of cancer survivorship on employment. Several studies confirm that cancer patients often are temporarily unable to work. Many also find high-rates of long-term unemployment (Bradley et al. (2007b)). While much of that literature has focused on supply-side issues (e.g. lingering effects of chemotherapy), our paper is the first to demonstrate the role of employers in the struggle of some cancer survivors to find work.

The remainder of the paper proceeds as follows. In Section 1.2, we give more detail on the theoretical model. In Section 1.3, we describe the experimental design. Section 1.3 explains the empirical strategy while Section 1.4 reports the results and interpretation. Section 1.6 concludes and provides suggestions for future studies.

1.2 Theoretical Framework

Before presenting the experimental design, we provide a theoretical framework that helps clarify the role of employment gaps in providing signals of productivity and health costs. We establish a simple framework to provide an example of how these signals can result in callback probabilities ordered: no gap, explained gap, unexplained gap. The key to this result is that productivity is correlated with the likelihood of getting a job offer but uncorrelated with the likelihood of becoming ill, while health costs⁹ are correlated with illness and uncorrelated with job offers. As a result, an unexplained gap gives negative information about productivity and an explained gap gives negative information about health costs. The clarity of these signals and the distributions of productivity and health costs determine the relative callback rates.

Let θ be the expected benefit of a callback from the point of view of the employer. This expected benefit will depend on items such as the probability that the employer will want to hire the applicant after the callback, that the applicant will take the job, that she will do well in the job, stay in the job, etc. We refer to such items collectively as productivity. For

⁹For simplicity, we refer to all potential negative consequences of hiring unhealthy workers as “health costs.”

simplicity we let θ be uniformly distributed such that

$$f_{\Theta}(\theta) = \begin{cases} 1 & \text{for } \theta \in [0, 1] \\ 0 & \text{otherwise.} \end{cases} \quad (1.1)$$

The productivity measure θ is not observed by the employer. The employer does observe a signal $S \in \{n, i, u\}$ which may be informative about θ . A signal n means the applicant has no employment gap, i means an illness related gap, and u means an unexplained gap. By Bayes' law, the distribution of θ conditional on $S = s$ is

$$f_{\Theta}(\theta|S = s) = \frac{P(S = s|\Theta = \theta) f_{\Theta}(\theta)}{P(S = s)}. \quad (1.2)$$

The signal depends on two shocks to the applicant in the previous period. A positive health shock means that the applicant was physically available to work. The probability of a positive health shock is independent of productivity and set to ω . A positive employment shock means that the worker, if healthy, had a job. Because good workers tend to remain employed, this is correlated with productivity. For simplicity the probability of a positive shock is set to θ . Given this, $s = n$ if in the previous period the applicant received a positive health shock and a positive employment shock, $s = u$ if the applicant had a positive health shock but a negative employment shock, and $s = i$ if the applicant received a negative health shock. That is:

$$P(S = n|\Theta = \theta) = \omega\theta \quad (1.3)$$

$$P(S = u|\Theta = \theta) = \omega(1 - \theta) \quad (1.4)$$

$$P(S = i|\Theta = \theta) = (1 - \omega). \quad (1.5)$$

Given these conditional probabilities and equation (1.1) we can find the unconditional

probability of each type of shock:

$$P(S = n) = \frac{\omega}{2} \quad (1.6)$$

$$P(S = i) = (1 - \omega) \quad (1.7)$$

$$P(S = u) = \frac{\omega}{2}. \quad (1.8)$$

Equations (1.1) and (1.3)-(1.8) into (1.2) give the following conditional distributions

$$f_{\Theta}(\theta|S = n) = 2\theta$$

$$f_{\Theta}(\theta|S = i) = 1$$

$$f_{\Theta}(\theta|S = u) = 2(1 - \theta)$$

with support $\theta \in [0, 1]$. From this and equation (1.1), the conditional expected productivities are

$$\begin{aligned} E(\theta|S = n) &= \frac{2}{3} \\ E(\theta|S = i) &= \frac{1}{2} \\ E(\theta|S = u) &= \frac{1}{3}. \end{aligned}$$

Note that $E(\theta|S = u) < E(\theta) = \frac{1}{2} = E(\theta|S = i)$. That is the expected θ conditional on $S = u$ is less than the unconditional expectation for θ while the expected θ conditional on $S = i$ is equal to the unconditional expectation. This reflects that employers learn something about productivity from applicants without illness related gaps but an illness related gap gives no information regarding productivity.

A firm may receive many applications for a position, However from the perspective of the researcher, each firm receives a triplet of applications. With a one-to-one correspondence between firms and triplets we use k to index both. Upon receiving this triplet, the problem

for the firm is to maximize the expected net return to making callbacks, given by V_k . To maximize this, the firm chooses a callback strategy $C_k = \{c_{n,k}, c_{i,k}, c_{u,k}\}$, where $c_{n,k}, c_{i,k}, c_{u,k} \in \{0, 1\}$ and $c_{s,k} = 1$ means make a callback to the applicant from triplet k with signal $S = s$. Finally, $c_{s,k} = 0$ means do not call that applicant back.

The value of V_k is equal to the sum of expected productivity and an idiosyncratic shock less an expected health care expenditure on the applicant if hired and a reservation level of returns. The shock $\varepsilon_{s,k}$ is known to the employer before making callbacks. It represents idiosyncratic items unobservable to the researcher that increase or decrease the value of a callback from the employer's perspective. Let $E(H|S = s)$ be the expected health related cost from hiring an agent given signal s . This could be insurance costs, costs due to lost work days in the future, lost productivity, a higher probability of future separation, etc. Let R_k be a reservation level of expected returns which must be exceeded in order to warrant a callback. This can reflect the cost of a callback, recruitment, training, etc. We can write the firm's problem in dealing with this triplet as

$$\max_{C_k} V_k = \max_{C_k} \sum_{s \in \{n, i, u\}} [E(\theta|S = s) + \varepsilon_{s,k} - E(H|S = s) - R_k] \quad (1.9)$$

Stated differently, the firm decides independently whether to respond to each applicant in the triplet.

We normalize health costs such that $E(H|S = n) = E(H|S = u) = 0$ and set $E(H|S = i) = hE(\theta|S = i) = \frac{h}{2}$. Given this, $E(\theta|S = i) - E(H|S = i) = \frac{1-h}{2}$. We economize on notation by setting $T_{s,k} \equiv \varepsilon_{s,k} - R_k$. Then equation (1.9) simplifies to

$$V_k = \max_{C_k} \left[\frac{2}{3} - T_{n,k} \right] + \left[\frac{1-h}{2} - T_{i,k} \right] + \left[\frac{1}{3} - T_{u,k} \right].$$

The optimal strategy for the firm is

$$C_k = \begin{cases} \{1, 1, 1\} & \text{if } T_{n,k} \leq \frac{2}{3}, T_{i,k} \leq \frac{1-h}{2}, T_{u,k} \leq \frac{1}{3} \\ \{1, 1, 0\} & \text{if } T_{n,k} \leq \frac{2}{3}, T_{i,k} \leq \frac{1-h}{2}, T_{u,k} > \frac{1}{3} \\ \{1, 0, 1\} & \text{if } T_{n,k} \leq \frac{2}{3}, T_{i,k} > \frac{1-h}{2}, T_{u,k} \leq \frac{1}{3} \\ \{1, 0, 0\} & \text{if } T_{n,k} \leq \frac{2}{3}, T_{i,k} > \frac{1-h}{2}, T_{u,k} > \frac{1}{3} \\ \{0, 1, 1\} & \text{if } T_{n,k} > \frac{2}{3}, T_{i,k} \leq \frac{1-h}{2}, T_{u,k} \leq \frac{1}{3} \\ \{0, 1, 0\} & \text{if } T_{n,k} > \frac{2}{3}, T_{i,k} \leq \frac{1-h}{2}, T_{u,k} > \frac{1}{3} \\ \{0, 0, 1\} & \text{if } T_{n,k} > \frac{2}{3}, T_{i,k} > \frac{1-h}{2}, T_{u,k} \leq \frac{1}{3} \\ \{0, 0, 0\} & \text{if } T_{n,k} > \frac{2}{3}, T_{i,k} > \frac{1-h}{2}, T_{u,k} > \frac{1}{3}. \end{cases}$$

To simplify even more, we let $T_{n,k}, T_{i,k}, T_{u,k}$ be *i.i.d.* $u[0, 1]$. This allows us to easily find the probability that a firm chooses any strategy C_k ; i.e. $P(C_k = \{c_{n,k}, c_{i,k}, c_{u,k}\})$. This will in turn be equal to the share of firms having this strategy in the population of firms $P(C = \{c_n, c_i, c_u\})$. In particular we find

$$\begin{aligned} P\{1, 1, 1\} &= \frac{1-h}{9} \\ P\{1, 1, 0\} &= \frac{2(1-h)}{9} \\ P\{1, 0, 1\} &= \frac{1+h}{9} \\ P\{1, 0, 0\} &= \frac{2(1+h)}{9} \\ P\{0, 1, 1\} &= \frac{1-h}{18} \\ P\{0, 1, 0\} &= \frac{1-h}{9} \\ P\{0, 0, 1\} &= \frac{1+h}{18} \\ P\{0, 0, 0\} &= \frac{1+h}{9}. \end{aligned}$$

We now turn attention to the percentage of instances in which each signal is favored. An application in a triplet with signal $S = s$ is favored if the application receives a callback while at least one other application in the triplet does not. Let $P(F|S = s)$ be the probability that

an application with signal s is favored. For example, $P(F|S = n) = P\{1, 1, 0\} + P\{1, 0, 1\} + P\{1, 0, 0\}$. Given this

$$P(F|S = s) = \begin{cases} \frac{5+h}{9} & \text{if } s = n \\ \frac{7(1-h)}{18} & \text{if } s = i \\ \frac{2+h}{9} & \text{if } s = u \end{cases}$$

so

$$P(F|S = n) > \max\{P(F|S = i), P(F|S = u)\}$$

and

$$P(F|S = i) > P(F|S = u) \text{ iff } h < \frac{1}{3}.$$

This shows that so long as h is not too large, applicants with no gap are most likely to be preferred, followed by an illness gap and an unexplained gap. However, all will be preferred in some share of the cases.

To find the probability of a callback, we add $P\{1, 1, 1\}$ to $P(F|S = s)$. This gives

$$P(C|S = s) = \begin{cases} \frac{2}{3} & \text{if } s = n \\ \frac{1-h}{2} & \text{if } s = i \\ \frac{1}{3} & \text{if } s = u \end{cases}$$

so that

$$P(C|S = n) > \max\{P(C|S = i), P(C|S = u)\}$$

and

$$P(C|S = i) > P(C|S = u) \text{ iff } h < \frac{1}{3}.$$

This shows that so long as h is not too large, applicants with no gap are most likely to get a callback followed by an illness gap and an unexplained gap.

1.3 Experimental Design

The correspondence test methodology has been used to provide insights on hiring discrimination based on race, ethnicity, immigration, gender, sexual orientation and age.¹⁰ The method involves sending similar job applications to employers posting jobs with the only difference being a characteristic that signals membership to a group. We employ this methodology to study how employers respond to a job applicant’s illness-related employment gap compared to unexplained gap or no gap.

The experiment was carried out between March, 2016, and September, 2016. Over this period, we surveyed eligible employment ads from multiple online job boards. For each job ad, we customized fictitious résumés and sent them to employers. We then measured employers’ responses to our fictitious job seekers’ application.¹¹

We chose the following occupations: sales and customer service, clerical/administrative assistant and accounting assistant jobs.¹² We targeted these jobs for several reasons. First, these types of jobs do not require complex skills and are fairly similar across firms which allows us to easily create suitable generic résumés. Second, there are enough numbers of available jobs in online job boards in these fields to conduct a sufficiently powered study. We limited our sample to job ads that required 6 or fewer years of work experience. We restricted our experiment to 15 of the most populous cities of the United States.¹³ We chose jobs that allowed direct uploads of résumés and cover letters to apply. We eliminated any ad where applicants were asked to call or appear in person or that required résumés to be submitted to external websites.

We recorded available information about the job, including the date the job ad was

¹⁰See [Bertrand and Mullainathan \(2004\)](#), [Arai et al. \(2015\)](#), [Arceo-Gomez and Campos-Vazquez \(2014\)](#), [Baert et al. \(2015\)](#), [Carlsson and Rooth \(2007\)](#), [\(Drydakis and Vlassis, 2010\)](#) and [Neumark et al. \(2015\)](#). It has been extended to identify hiring penalties associated with motherhood, physical unfitness, obesity and criminal record. For examples, see [Correll et al. \(2007\)](#), [Rooth \(2011\)](#), [Rooth \(2009\)](#) and [Pager \(2003\)](#).

¹¹The experimental protocol was reviewed and approved by the Institutional Review Board at Kansas State University.

¹²As in [Bertrand and Mullainathan \(2004\)](#) and [Kroft et al. \(2013\)](#).

¹³New York, NY; Los Angeles, CA; Chicago, IL; Houston, TX; Philadelphia, PA; Phoenix, AZ; San Antonio, TX; San Diego, CA; Dallas, TX; San Jose, CA; Jacksonville, FL; Indianapolis, IN; San Francisco, CA; Austin, TX; Columbus, OH.

posted, position, company name, company address, telephone number and job requirements (education level and skills required). We also recorded whether the ad explicitly stated that the employer required physical capacity to lift objects and whether it required a stable job history. Moreover, we also collected information on whether the ad stated that the employer provided employer-sponsored health insurance and other benefits. This information was used to create the résumés and where relevant, was used in the statistical analysis. We collected jobs and sent résumés by batch. More specifically, the size of a batch depends on the available jobs at the particular time of data collection and ranges from about 30 to 150 jobs. We prepared and sent the résumés for one batch of job ads before collecting a new batch of job ads.

Three equally qualified artificial résumés and corresponding cover letters were customized for each job ad. These three résumés sent to a single job ad constitute one triplet.¹⁴ Using the résumé randomizer developed by [Lahey and Beasley \(2009\)](#), we then randomly assigned treatments and other résumé details to each type of résumé.¹⁵

All of the résumés that we sent indicated a contemporary employment gap. The résumés differed in terms of the duration of the employment gaps and assignment of the explanation for the gap.¹⁶ Thus if applicable, the type of employment gap was explicitly explained in the cover letter and an additional signal was sent using the interest section of the résumé. By differing the employment gaps and gap explanation (or the lack of it) in each of the résumés in a triplet, we can identify the effects of the types of employment gaps on the employment prospects of job seekers. Each résumé in the triplet belonged to one of the following three treatment groups.

A résumé in treatment group 1 signaled an applicant who was newly unemployed. We used newly unemployed, rather than currently employed, as [Kroft et al. \(2013\)](#) and [Eriksson and Rooth \(2014\)](#) find that a currently employed worker is less likely to be called back for an

¹⁴Accordingly, we have 1,257 triplets since we applied to 1,257 jobs.

¹⁵We exported these characteristics assignment to a spreadsheet, which was used as input to résumé creation in Microsoft Word using the Mail Merge function.

¹⁶Non-employment duration appeared on the résumé in the form of an end date for the applicant's most recent job. For example, if the résumé is assigned a 8-month employment then the end date of the applicant's last job is 8 months from the date the résumé was sent.

interview than a newly unemployed individual. Kroft et al. (2013) suggest that employers may perceive individuals who engaged in on-the-job search as less loyal and prone to job hopping. In addition, some jobs require workers to start immediately which may be typical of the sample of jobs in our experiment. To minimize these effects, the corresponding cover letter indicated that the applicant resigned from her last employment due to a family decision to relocate from Seattle.¹⁷ Applicants with this type of gap are said to have no relevant employment gap.

A résumé in treatment group 2 contained a signal that the applicant had an illness-related gap for a notable period of time. For most of the sample this was 7 to 12 months. For 5% of the sample this was 20 to 22 months. In this treatment, a phrase in the cover letter explained that the gap is due to a medical illness followed by a complete recovery. An additional signal on the medical history was sent via information in the résumé which indicated involvement in a support group for cancer survivors. This implied that the illness associated with the employment gap was cancer, though this was not stated explicitly.

We chose cancer because cancer treatment is more likely to cause an employee to stop working, causing an employment gap. Cancer patients who receive a good post-treatment prognosis can return to the labor market with the comparable level of productivity as workers with no poor health history. Cancer is also perceived to be onset-uncontrollable, which prevents employers from forming inferences about productivity from the diagnosis. The possibility of relapse is also a concern for job applicants with cancer history. Further, health insurance costs for employers are likely to increase, since previous episodes of cancer are treated as a pre-existing condition that raise premiums.

A résumé in treatment group 3 contained an employment gap that is comparably long with treatment group 2. However, no explanation is provided for the employment gap in the résumé and cover letter. The employer, then, was free to assume the underlying reason behind the applicant's spell of joblessness.

In order to provide a signal of one's health issues in the résumé of treatment group 2,

¹⁷The explanation is necessary to prevent employers from assuming that the applicant is not readily available for work or prone to job-hopping.

we indicated that the applicant is a Member/Organizer of a cancer survivor group. To balance the three groups, we also assigned an alternative activity in the interest section of the résumés of treatment group 1 and treatment group 3. The applicant is either a volunteer for the “Watch the Wild” program or is interested in drawing, painting and running.

We chose common first names in 1990 and last names that were most likely to signal that the applicant was Caucasian to prevent any name-based employment discrimination from influencing the results. For females, we used Jessica Smith, Ashley Johnson and Rachel Miller. For males, we chose Joshua Smith, Andrew Johnson and Ryan Miller. Each name was assigned a corresponding telephone number and email address. To easily track the call-back, we assigned the name, a corresponding telephone number and e-mail address based on the treatment type. Those who were assigned treatment 1, for example, were assigned the name Rachel Miller and the corresponding email address and telephone number. The email addresses were all gmail accounts. We used Vumber to get three online telephone numbers by city, one for each treatment group. These did not appear any different than regular phone numbers to the employer, but had the benefit that the calls and voicemails were recorded in an online account and no physical phones were required. Residential addresses on the résumés were selected carefully to ensure that they were realistic. We used Zillow.com to get real addresses but we changed the housing/apartment number/letters to generate fictitious addresses.

Since we targeted jobs that required 6 years or less of experience, we designed the work histories such that the total years of experience was about 6 years. Each résumé was designed such that the applicant had two jobs, no unemployment since high school graduation, but were currently not employed. We created job histories by first randomly assigning the length of contemporary unemployment. As mentioned above, the length of the employment gap is conditional on the treatment assignment. We derived the end-dates of the last job by subtracting the length of contemporary unemployment from the date the résumé was planned to be sent. We then randomly assigned the tenure in the last job (12, 24 or 36 months) which then determined the start date of the last job and the end date of the first job. We assigned the tenure in the second job such that the number of years of work experience in

the first and second job added up to about 6 years. The tenure in the first job determined the start date of the first job and the date of high school graduation.

We collected a sample of acceptable job histories from real résumés downloaded in job search sites. Based on these histories, we selected three options for the following fields for each type of job: first job and job responsibilities, and second job and job responsibilities. We randomly assigned the first and second job to the résumés in each triplet. As in [Neumark et al. \(2015\)](#), we followed a defined profile of responsibility, showing a progression of jobs from lower to higher-level jobs.¹⁸ We added employer names and addresses to each job on our final job histories. We used employers that were active at the time and in the region listed.

We designated half the résumés to be high skilled (or high quality), and half to be low skilled. This would enable us to see if the employment consequences of illness-related gaps vary by quality. We combined the measures used by [Kroft et al. \(2013\)](#) and [Neumark et al. \(2015\)](#) to define a measure of quality for each résumé. All low-quality résumés had typographical errors and had no additional signals of productivity/skills. We had three types of high quality résumés and each type of high quality résumé had different additional signals of productivity. High quality type 1 résumé had an extra level of education, additional proficiency such as proficiency in *Quickbooks* software and indicated fluency in Spanish as a second language. An extra level of education means that if the job required high school completion, we listed an associate degree or if the job required an associate degree, we listed a bachelor degree. High quality type 2 résumés had acquired a certificate, an “*Employee of the Month*” award and did not have typographical error. The third high quality résumé had academic honors, notable achievement in previous work and had an an additional skill different from the additional skill of type 1 high quality résumés. All the résumés in a triplet were of same quality.

We created three résumé templates. Templates were randomly assigned to each résumé

¹⁸For example, in retail sales, the first job starts with the lower responsibility job like a cashier position and then the applicant works his/her way through becoming sales associate (hoping to step up to management positions). For administrative assistants, workers start as a receptionist before working their way to an administrative officer position.

created. There are no same templates used in a triplet to prevent the employers from detecting the experiment. For each job ad, the résumé randomizer assigned whether the triplet would be all male or all female.

For each city, we selected universities and corresponding degrees, community colleges and corresponding degrees, and high schools by city listed on each résumé. We selected universities that do not fall on the tier 1 to tier 3 categories defined by [Hersch \(2014\)](#). We randomly assigned these based on the education levels. To be consistent with our story that the treatment 1 applicant just moved from Seattle, we always assigned a school from Seattle for treatment group 1.

Once the résumés were generated, we converted the files to PDF formats. We named the files based on the name or initials of the applicant. We made sure that the filename style differs within triplet (e.g. “Rachel Miller”, “R.Miller”, or “Miller, Rachel”) to minimize chances of detecting the experiment. Appendix A provides a sample résumé and cover letter for each of the treatment groups. The first two pages in Appendix A show a sample cover letter and résumé of a newly unemployed applicant. The next two pages show a sample cover letter and résumé of an applicant with an illness-related employment gap. These are followed by a sample cover letter and résumé of an applicant with an unexplained gap. In sending the résumés, we randomly assigned the order by which the résumés were sent. For each triplet, the second résumé is sent at least a day after the first résumé was sent. We recorded the day the résumé was sent in the database.

We measured whether a given résumé elicits a callback, textback or e-mailback for an interview. For each phone, text or e-mail response, we used the content of the message left by the employer (name of the applicant, company name, telephone number for contact) to match the response to the corresponding résumé/job ad pair.¹⁹ We defined a callback as a personalized phone or e-mail contact by a potential employer. Usually the callback was a request for an interview, but employers also contacted applicants asking for more information or stated that they have a few questions. After hearing from employers, we sent a message

¹⁹Any attempt by employers to contact applicants via postal mail cannot be measured in our experiment since the addresses are fictitious.

to them that the applicant is no longer available for the job.

1.4 Empirical Strategy

Our empirical strategy consists of comparing the average callback rates across treatment groups and conducting various regression analyses to analyze the data we gathered from the experiment. Our regression analyses start with checking whether our results are consistent with previous studies showing that contemporaneous employment gaps adversely affect employment prospects. To do this, we estimate the following equation:

$$C_{jk} = \alpha + \delta G_{jk} + \mathbf{R}'_{jk}\Gamma + \mathbf{E}'_k\Lambda + \epsilon_{jk} \quad (1.10)$$

where C_{jk} is a callback indicator that equals 1 if applicant j who applied for job k received an invitation to a job interview, G_{jk} is a dummy variable that equals 1 if applicant j who applied for job k has an employment gap (i.e. illness-related employment gap or an unexplained employment gap), \mathbf{R} is vector of résumé attributes and \mathbf{E} is a vector of employer/job advertisement attributes.²⁰ Given that the résumé characteristics are randomized across treatment groups, δ gives the unbiased estimate of the impact of having any employment gap on the callback rate relative to the newly unemployed since the omitted variable is the dummy variable for the newly unemployed. The vectors Γ and Λ provide the effects of résumé and job advertisement attributes on the callback probability, respectively.

Our main goal in this paper is to analyze the effect of explaining an illness-related employment gap on workers' likelihood of being invited to a job interview. Thus the next thing we do is to estimate the following equation where instead of pooling all employment gaps into one variable, we separately estimate the effect of each type of employment gap:

$$C_{jk} = \alpha + \beta_1 I_{jk} + \beta_2 U_{jk} + \mathbf{R}'_{jk}\Gamma + \mathbf{E}'_k\Lambda + \epsilon_{jk} \quad (1.11)$$

²⁰ \mathbf{R} includes dummy variables for tenure, résumé quality, résumé template, order of sending within the triplet, gender of the applicant etc. \mathbf{E} includes dummy variables for occupation, required education level and experience, full-time jobs, commission-based jobs, employer-sponsored health insurance, physical requirements and location of the job.

I_{jk} is a dummy variable that equals 1 if applicant j who applied for job k has an illness-related employment gap, U_{jk} is a dummy variable that equals 1 if applicant j who applied for job k has an unexplained employment gap and the rest of the variables are the same as in Equation 1.10. Given that the résumé characteristics are randomized across treatment groups, β_1 gives the unbiased estimate of the mean impact of explaining an illness-related employment gap relative to the hiring rate of the newly unemployed and β_2 gives the unbiased estimate of the mean impact of not explaining an illness-related employment gap relative to the hiring outcome of the newly unemployed. Again, the omitted variable is the dummy variable for newly unemployed.

As a robustness check, we estimate Equation 1.11 using OLS estimation as well as probit estimation (and provide marginal effects). Moreover, we estimate the following fixed effects model to control for fixed effects at the job ad level:

$$C_{jk} = \alpha + \beta_1 I_{jk} + \beta_2 U_{jk} + \mathbf{R}'_{jk} \Gamma + \mu_k + \epsilon_{jk} \quad (1.12)$$

where μ_k represents the fixed effect of k^{th} job.

Since a small sample of the résumés that were assigned an illness-related gap and unexplained gap have an employment gap of more than 20 months, it is also informative to modify and estimate Equations 1.10 to 1.12 to include an additional dummy variable for the employment gap that lasted for more than 20 months. The coefficient of this additional variable can be interpreted as the mean impact of having more than 20 months in gap, given that the applicant has either an illness-related gap or unexplained gap. This coefficient tells us if having a longer employment gap matters once we control for more information on the gap.

1.5 Results

In this section, we present the overall descriptive statistics and then turn to the results. Column 1 of Table 1.1 presents the overall callback rate for the sample. Included in brackets under each rate is the number of résumés sent in that cell. We sent a total of 3,771

résumés to 1,257 job ads. The overall callback rate is 25.5 percent. Unlike the [Deming et al. \(2016a\)](#), we find that the callback rates did not differ between low and high quality résumés. The callback rate is higher for female-sounding names compared to male-sounding names. Applications sent to sales jobs received higher callbacks compared to administrative and accounting assistant jobs.

In the following subsections, we compare the callback rates among treatment groups and subgroups and discuss the regression results and interpretation.

1.5.1 Comparing the Mean Callback Rates

Column 2 to 4 of Table 1.1 show the average callback rate of applicants who are newly unemployed, with illness-related gaps and with unexplained employment gaps. Overall, there is evidence of negative duration dependence. Row 1 shows that 27.4 percent of the newly unemployed applicants received a callback compared to the average callback rate of 24.5 percent for applicants with employment gap (illness-related or unexplained). This holds true by type of quality, gender and occupation.

When employers are given more information about the type of employment gap, they appear to consider this additional information in their callback decisions. Comparing columns 3 and 4, we see that the average callback is lower for applicants with an unexplained employment gap (23.3 percent) compared to applicants with an explained illness-related employment gap (25.6 percent). This represents a difference in callback rates of 2.3 percentage points, or 9 percent reduction relative to the average callback rate of 25.5 percent. Except for low quality type résumés and accounting jobs, the observation of lower callback rates for unexplained gap can be seen in most sub-groups in Table 1.1 .

As in [Bertrand and Mullainathan \(2004\)](#), we tabulate the distribution of callbacks at the firm or triplet level. In each of the cells in columns 2 and 3 of Table 1.2, the first row indicates the firm’s callback strategy, $C_k = \{c_{n,k}, c_{i,k}, c_{u,k}\}$ where $c_{n,k}, c_{i,k}, c_{u,k} \in \{0, 1\}$ and $c_{s,k} = 1$ means the k^{th} firm made a callback to the applicant with signal s and $c_{s,k} = 0$ means the k^{th}

firm did not call that applicant with signal s back.²¹ The second and third rows under each cell in columns 2 and 3 respectively contain the percentage and the number of firms with row 1 callback strategy. Equal treatment occurs when either no applicant gets called back or all the types of applicants in a triplet receive a callback. The newly unemployed applicant is favored when either only the newly unemployed gets called back, or the newly unemployed is one of the two applicants in the triplet who received a callback. Similarly, the applicant with an illness-related employment (unexplained employment) gap is favored when either only the applicant with an illness-related employment (unexplained employment) gets called back, or the applicant with an illness-related employment (unexplained employment) is one of the two applicants in the triplet who received a callback.

In column 1 of Table 1.2, we report the percentage and number of firms that showed equal treatment and the same statistics of firms who favored each treatment group. Equal treatment occurs for about 77.2 percent of the ads but most of that is due to the high fraction of ads for which no callbacks are recorded (62.9 percent of the ads). Approximately, 14 percent of job ads call all the applicants in the triplet. Newly unemployed applicants are favored by 13.2 percent of the employers. Applicants with an explained illness-related gap, on the other hand, are favored by only 11.4 percent of employers while applicants with unexplained employment gap were the least favored with only 9.1 percent of employers favoring this group.

Using the test of proportion, we test the null hypotheses that there is symmetry in : 1) favoring of newly unemployed over applicants with an illness-related gap ($H_0 : nF = iF$); 2) favoring of newly unemployed over applicants with an unexplained gap ($H_0 : nF = uF$); and 3) favoring of applicants with an illness-related gap over applicants with an unexplained gap ($H_0 : iF = uF$). Given a p -value of 0.169, we do not reject $H_0 : nF = iF$ which suggests that the difference between the fraction of employers favoring newly unemployed and the fraction of employers favoring applicants with illness-related gap is not statistically different from each other. However, we reject the $H_0 : nF = uF$ and $H_0 : iF = uF$

²¹For example, callback strategy, $C_k = \{c_{n,k}, c_{i,k}, c_{u,k}\} = \{1, 1, 0\}$ means that for triplet k , the employer called the applicants with signals n (no gap) and i (illness-related employment gap) and did not call the applicant with signal u (unexplained employment gap).

Table 1.1: Mean Callback Rates by Type of Employment Gap

	(1) All	(2) Newly Unemployed	(3) Illness-related gap	(4) Unexplained gap
Overall	25.5 [3771]	27.4 [1257]	25.6 [1257]	23.3 [1257]
Low Quality	25.5 [1956]	26.5 [652]	23.8 [652]	26.2 [652]
High Quality	25.4 [1815]	28.4 [605]	25.0 [605]	22.8 [605]
Male	23.3 [2025]	24.6 [675]	23.6 [675]	21.8 [675]
Female	27.9 [1746]	30.8 [582]	28.0 [582]	25.1 [582]
Retail	36.0 [1566]	37.9 [522]	37.0 [522]	33.0 [522]
Administrative Assistant Jobs	18.2 [1200]	19.5 [400]	18.0 [400]	17.0 [400]
Accounting Assistant Jobs	17.8 [1005]	20.6 [335]	15.8 [335]	17.0 [335]

Note: The number of résumés sent in each particular group is provided in the brackets.

because the test gives p – values of 0.001 and 0.057, respectively. Rejecting $H_0 : nF = uF$ suggests that there is statistical difference between the rate by which employers favor the newly unemployed relative to the rate by which employers favor applicants with unexplained employment gap. Rejecting $H_0 : iF = uF$ suggests that there is statistical difference is also observed if we compare the rate by which employers favor applicants with illness-related gaps and the rate by which employers favor applicants with unexplained employment gaps.

1.5.2 Regression Results

In this subsection, we present the regression results of the empirical models represented by Equations 1.10 to 1.12. Our main results show that applicants with explained illness-related

Table 1.2: Distribution of Firms' Callback Strategy, $C_k = \{c_{n,k}, c_{i,k}, c_{u,k}\}$

Equal Treatment 77.2 [970]	{0, 0, 0} 62.9 [791]	{1, 1, 1} 14.2 [179]	
Newly Unemployed Favored (nF) 13.2 [166]	{1, 0, 0} 5.3 [67]	{1, 1, 0} 4.8 [60]	{1, 0, 1} 3.1 [39]
Illness-related Employment Gap Favored (iF) 11.4 [143]	{0, 1, 0} 3.7 [46]	{1, 1, 0} 4.8 [60]	{0, 1, 1} 2.9 [37]
Unexplained Employment Gap Favored (uF) 9.1 [114]	{0, 0, 1} 3.0 [38]	{1, 0, 1} 3.1 [39]	{0, 1, 1} 2.9 [37]

Note: The first line in each of the cells in columns 2 to 4 represents the callback strategy of the firm, $\{c_{n,k}, c_{i,k}, c_{u,k}\}$ while the first line in column 1 sums up the relevant callback strategies to determine the share of firms that showed equal treatment and unequal treatment. Across all cells in the table, the second line is the percentage of firms while the third row contains the number of firms.

employment gaps receive higher callbacks than otherwise identical applicants who offer no explanation for the gap.

Our experiment finds negative duration dependence that is consistent with studies closely related to our paper. Table 1.3 presents a regression analyses of the effect of having a contemporaneous employment gap (relative to newly unemployed applicants) on the probability of getting hired. These estimates pool Treatment 2 and Treatment 3 together. Columns 1 to 3 are estimated using OLS estimation, probit estimation²² and linear estimation with fixed effects, respectively. Relative to applicants who are newly unemployed, the effect of having an employment gap that is at least 7 months is negative and significant at the 1 percent level. The callback rate is reduced by 12 percent (0.03/0.255) for applicants with any type of employment gap. The results are robust across linear specifications (with and without job advertisement fixed effects) and non-linear specification.

²²Coefficients reflect marginal effects.

A small proportion of workers were assigned employment gaps of more than 20 months. We include an additional dummy equal to 1 when the gap is greater than 20 months in columns 4 to 6. The changes in the coefficient of the employment gap are small and not significant and the coefficient of the additional dummy is also not significant. As a result, we see no evidence that employment gaps longer than 12 months further reduce employment prospects. This is consistent with the results of [Kroft et al. \(2013\)](#) and [Eriksson and Rooth \(2014\)](#) which suggest that callbacks decline sharply for mid-long spells (up to around 9 months) but is flat for unemployment durations thereafter. After a length threshold is reached, additional length of employment gaps does not explain the adverse effect of employment gaps.

Table 1.3: The Effect of Having an Employment Gap on the Callback Rate

Dependent Variable: Callback Dummy	(1)	(2)	(3)	(4)	(5)	(6)
Employment Gap	-0.030*** (0.010)	-0.031*** (0.010)	-0.030*** (0.010)	-0.034*** (0.010)	-0.035*** (0.011)	-0.029*** (0.010)
Gap Duration>20				0.066 (0.040)	0.059 (0.037)	-0.023 (0.034)
OLS	X			X		
Probit		X			X	
Fixed Effects: (Job Ads)			X			X
Callback Rate Ave. (%)	25.5	25.5	25.5	25.5	25.5	25.5
Observations	3,771	3,771	3,771	3,771	3,771	3,771
R-squared	0.090		0.012	0.091		0.012
Number of Job Ads			1,257			1,257

Note: The dependent variable is the callback dummy. Control variables include dummy variables for tenure, résumé quality, résumé template, order of sending within the triplet, gender of the applicant, for occupation type, required education level and experience, full-time jobs, commission-based jobs, employer-sponsored health insurance, physical requirements and location of the job. Columns 1 to 3 were estimated without controlling for gaps greater than 20 months. Columns 4 to 6 include a dummy controlling for gaps greater than 20 months. Columns 2 and 5 give the results of a probit regression using dprobit command in Stata 12. The coefficients reported in these columns (2 and 5) are estimated marginal changes in the probability for estimated discrete changes for the dummy variables. Robust standard errors are in parentheses and are clustered at the job vacancy level: *** p<0.01, ** p<0.05, * p<0.1.

To fully control for résumé and job characteristics, we estimate Equations 1.11 and 1.12. Columns 1 and 2 of Table 1.4 show estimates of Equation 1.11 using OLS and probit while column 3 shows the linear estimation result controlling for the fixed effects at the job ad level (Equation 1.12). The coefficients of the illness-related gap and the unexplained gap are negative and significant at the 1 and 10 percent levels, respectively, indicating that applicants with any type of employment gap have worse employment prospects relative to newly unemployed applicants.

In order to answer our main research question, we compare the magnitude of the coefficients for an illness-related gap and an unexplained gap to show if the effect of explaining a gap mitigates its negative effect on employment. Indeed, relative to the newly unemployed, applicants with an unexplained gap receive fewer callbacks (4.2 percentage points less) than the those with an illness-related employment gap (1.9 percentage points less). Relative to the mean callback rate (25.5 percent), an illness-related employment gap reduces the callback rate by approximately 7 percent. An unexplained employment gap, however, reduces the callback rate by 16 percent. Results from the F-test reject the null hypothesis that the marginal effect of the two types of employment gaps are the same. These results are robust across several specifications, including controlling for the job ad fixed effects. Our main results show that applicants with explained illness-related employment gaps fare better in attracting callbacks than otherwise identical applicants that offer no explanation for the gap.

The results and the theoretical framework presented in Section 1.2 suggests that employers are not as concerned about the cost of having poor health, h , as they are about to the cost of hiring workers with unexplained gap (i.e. lower expected productivity).²³ In a model with heterogeneous workers and uncertainty, employers do not directly observe productivity of job applicants. Given a positive correlation of general employment gaps and low productivity, firms take observed employment gaps as signals of productivity.

²³The theoretical framework in this paper can provide explanation to the results of Baert et al. (2016) which show that the mean callback rate of the recently unemployed candidate is 14.2% compared to the 10.1% average callback of applicants who have unexplained employment gap and the 9.4% callback rate of applicants with employment gaps because of depression implies that employers. The framework implies that the healthcare cost associated with mental illness is not significantly higher than the reduction in productivity associated with those who have unexplained employment gaps.

Table 1.4: The Effect of Having an Explained Illness-related Gap and an Unexplained Gap on the Callback Rate

Dependent Variable: Callback Dummy	(1)	(2)	(3)	(4)	(5)	(6)
Illness-related Gap	-0.019* (0.011)	-0.019* (0.011)	-0.018* (0.011)	-0.022* (0.012)	-0.022* (0.012)	-0.019* (0.011)
Unexplained Gap	-0.042*** (0.011)	-0.042*** (0.011)	-0.041*** (0.011)	-0.046*** (0.012)	-0.046*** (0.012)	-0.038*** (0.012)
Gap Duration>20				0.074 (0.058)	0.069 (0.055)	-0.062 (0.047)
Unexplained Gap x Gap Duration >20				-0.017 (0.085)	-0.019 (0.068)	0.075 (0.068)
<i>F (Illness-related = Unexplained)</i>	<i>0. 0.038</i>	<i>0.036</i>	<i>0.035</i>	<i>0.046</i>	<i>0.044</i>	<i>0.090</i>
Ave. Callback Rate	25.5	25.5	25.5	25.5	25.5	25.5
OLS	X			X		
Probit		X			X	
Fixed Effects: Job Ad			X			X
Observations	3,771	3,771	3,771	3,771	3,771	3,771
R-squared	0.091		0.013	0.092		0.014
Number of Job Ads			1,257			1,257

Notes: The dependent variable is the callback dummy. Control variables includes dummy variables for tenure, résumé quality, résumé template, order of sending within the triplet, gender of the applicant, for occupation type, required education level and experience, full-time jobs, commission-based jobs, employer-sponsored health insurance, physical requirements and location of the job. Columns 1 to 3 were estimated without controlling for a gap duration greater than 20 months. Columns 4 to 6 include a dummy controlling for gaps greater than 20 months. Columns 2 and 5 give the results of a probit regression using dprobit command in Stata 12. The coefficients reported in these columns (2 and 5) are estimated marginal changes in the probability for an estimated discrete change. The F(Illness-related Gap = Unexplained Gap) provides the F-statistic needed to test the null hypothesis that the coefficient of an illness-related gap is equal to the coefficient of the unexplained gap. Robust standard errors are in parentheses and are clustered at the job vacancy level: ***p<0.01, ** p<0.05, * p<0.1.

1.5.3 The Role of the Physical Requirements of the Job and Employer-Sponsored Insurance

To further explore our results, we analyze how job-related physical requirements and employer-sponsored insurance (ESI) affect the relative callback probability of each treatment group. We do this in order to determine if any evidence exists for possible alternative mechanisms underlying our results.

First, employers might take an illness-related gap as a signal for weaker physical ability that may interfere with current or future productivity. To test the influence of perceived physical limitations, we use data from the job advertisement on the physical requirements of the job to create a dummy variable, “Physical Requirements”, which is equal to 1 if the job advertisement explicitly mentions that the job entails physical strength such as standing and lifting objects. We interact this variable with the treatment group dummies.

Table 1.5 shows that the coefficient of the interaction of an illness-related gap and physical requirements of the job is negative but not significant. However, the size of the coefficient (-0.013 for column 1) is large relative to the impact of an illness employment gap in jobs without a physical requirement (-0.021 for column 1). By contrast, there is no additional callback reduction in jobs with physical requirements for those with unexplained gaps.

Second, employers that offer employer-sponsored insurance may want to avoid applicants with an illness-related gap because of their higher expected medical costs. We use data from the job advertisement on the offer of employer-sponsored insurance to create a dummy variable, “Health Insurance”, which is equal to 1 if the job advertisement explicitly mentions that the employer provides employer-sponsored insurance. Table 1.5 shows that the coefficient of the interaction of the dummy variable for illness-related gaps and “Health Insurance” is positive and not significant. The coefficient of the interaction of the dummy variable for unexplained gaps and “Health Insurance” is negative and significant at 10 percent level. Neither estimate suggests that the offer of health insurance negatively affects job prospects of applicants with poor health histories. However, our ability to make a stronger conclusion about the role of employer sponsored insurance in explaining our results is tempered by two

Table 1.5: Job Characteristics and Effect of Employment Gaps on Callback Rate

Dependent Variable: Callback Dummy	(1)	(2)	(3)	(4)	(5)	(6)
Illness-related Gap	-0.021* (0.012)	-0.021* (0.012)	-0.018 (0.012)	-0.025* (0.013)	-0.026** (0.013)	-0.022* (0.013)
Unexplained Gap	-0.045*** (0.012)	-0.046*** (0.013)	-0.039*** (0.012)	-0.033** (0.013)	-0.034** (0.014)	-0.025* (0.013)
Health Insurance	-0.012 (0.024)	-0.014 (0.025)		-0.000 (0.029)	-0.003 (0.029)	
Physical Requirements	-0.013 (0.046)	-0.017 (0.040)		-0.017 (0.037)	-0.021 (0.033)	
Illness-related Gap x Physical	-0.013 (0.043)	-0.013 (0.038)	-0.012 (0.043)			
Unexplained Gap x Physical	-0.000 (0.039)	0.001 (0.037)	0.013 (0.039)			
Illness-related Gap x Health Insurance				0.011 (0.026)	0.014 (0.027)	0.009 (0.026)
Unexplained Gap x Health Insurance				-0.047* (0.027)	-0.045* (0.025)	-0.050* (0.027)
OLS	X			X		
Probit		X			X	
Fixed Effects: Job Ad			X			X
Observations	3,771	3,771	3,771	3,771	3,771	3,771
R-squared	0.092		0.014	0.092		0.017
Number of Job Ads			1,257			1,257

The dependent variable is the callback dummy. Control variables includes dummy variables for tenure, résumé quality, résumé template, order of sending within the triplet, gender of the applicant, for occupation type, required education level and experience, full-time jobs, commission-based jobs, employer-sponsored health insurance, physical requirements and location of the job. Columns 3 and 6 gives the results of a probit regression using dprobit command in Stata 12. The coefficients reported in columns 3 and 6 are estimated marginal changes in the probability for estimated discrete changes for the dummy variables. Robust standard errors are in parentheses and are clustered at the job vacancy level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

important weaknesses. Firstly, we rely on explicit reports of insurance availability in a job advertisement, and thus might misclassify some jobs that offer insurance but do not advertise as such. Secondly, the offer of health insurance is strongly correlated with firm size, which may confound the estimates if firm size affects perception of gaps for reasons unrelated to health costs.

In summary, our results on the effects of the presence of physical requirements of the job and employer-sponsored insurance suggest that these factors do not play a significant role in determining the differential impact of an unexplained gap and an illness-related gap. However, we exercise caution in ruling out such factors altogether, as our study was not specifically powered to test these hypotheses.

1.6 Conclusion

As of 2015, nearly one-third of the the US population suffered from some chronic health condition. Unsurprisingly, a large portion of potential workers (nearly 13 million adults in 2014) exited the labor force as a result of illness or disability. The loss of productivity from these workers who exited the labor market adds to the overall cost of health problems to society. One way to minimize the cost is to ensure that workers with poor health history, but the ability to return to work, are able to do so. However, their employment gaps may adversely affect their employment prospects. Further, the recent recession has refocused on how increased spells of joblessness affect the prospect of re-entry into the workforce.

Our paper aims to shed light on the relative prospects of job applicants with different types of gaps in their employment records. We compare the effects of illness-related employment gaps, no employment gaps and unexplained employment gaps. We have two main findings. First, job applicants with illness-related employment gaps have slightly worse callback rates than newly unemployed applicants, which indicates some degree of unemployment stigma, even for those who report being forced out of work for illness-related reasons. Second, applicants with explained illness-related employment gaps fare substantially better in attracting callbacks than otherwise identical applicants that offer no explanation for the gap.

We provide a theoretical model showing how employers may rank workers with different types of employment gaps. In this model, heterogeneous workers and employers do not directly observe productivity of job applicants. Firms, however, can observe employment gaps and take these as signals of productivity and healthcare costs. When the cost of hiring workers with health shocks is lower than the cost of hiring workers with lower expected productivity, we show that employers may prefer to hire job applicants with illness-related employment gaps over applicants with unexplained gaps.

Our paper is limited to the effects on callback rates and cannot provide evidence on the impact on final job-finding rates. Despite these limitations, we believe that our study provides insights on employer behavior that underlie the documented employment gap. In particular, our model and results suggest that the productivity signalling value of long spells of joblessness likely play a larger role than other explanations, such as human capital depreciation, in the increased difficulty faced by these workers in finding a new job.

Future work can determine whether the effects of health-related employment gaps found here differ across types of physical illness. Future work might also explore the behavioral transmission mechanisms underlying the effects of illness-related employment gaps on employment prospects. For example, are illness-related gaps that are perceived to be caused by controllable factors treated differently from those perceived to be caused by uncontrollable factors? We are also unable to determine the extent to which the relatively favorable job market conditions at the time of our study influenced the large relative penalty faced by applicants with unexplained gaps. A simple extension of the model may suggest a more strongly negative signal from an unexplained gap in high employment settings than in times of widespread unemployment.

Chapter 2

The Effect of Post-Baccalaureate Certificates on Job Search: Results from a Correspondence Study

2.1 Introduction

There has been a dramatic growth in the receipt of post-baccalaureate certificates.¹ As shown in Table 2.1, data from the Integrated Post-Secondary Education Data System (IPEDS) show that certification completion reached more than one million in 2014, which is 91.7 percent increase from 1999. Certificates were about a quarter of all post-secondary awards in 2014.² Although most of these certificates are sub-baccalaureate, post-baccalaureate certificates constitute one of the fastest-growing areas in higher education. Post-baccalaureate certificates registered the highest percentage growth at 178 percent in the last 15 years (1999 to 2014) compared to 88 percent growth rate for sub-baccalaureate certificates in the same period. Post-baccalaureate certificates divert from traditional post-secondary programs in

¹Post-baccalaureate certificates are interchangeable with the following terms: graduate certificate, graduate diploma, certificate of graduate study (COGS), certificate of advanced graduate study (CAGS) and professional development program.

²Bosworth (2010) and Carnevale et al. (2012) provide more detailed description on the landscape of post-secondary certificate programs.

that they tend to be short-term, highly specific and have more easily defined student objectives. Post-baccalaureate programs are also often offered online which give the student more flexibility.

The growth in post-baccalaureate certificate programs highlights the need to examine if getting such certificate is beneficial in the job application process, especially given concerns about the perceived usefulness of graduate certificates. To date, little is known about the actual benefits associated with such programs. Most of the research done on the value of certificates focuses on the returns of sub-baccalaureate certificates. In this paper, we fill this gap in the literature by focusing on post-baccalaureate certificates. Specifically, our goal is to establish the causal relationship between having a post-baccalaureate certificate and the chances of getting a request for an interview.

Table 2.1: Certificate Completion, in thousands

	1999	2004	2009	2012	2014	% Change	
						1999-2009	1999-2014
Sub-baccalaureate	546	720	840	1,030	1,007	53.6	88.4
Post-baccalaureate	19	33	47	55	57	145.4	178.4
TOTAL	566	754	887	1,085	1,065	56.7	91.7

Notes: Post-baccalaureate includes post-master.

Source: Integrated Postsecondary Education Data System (IPEDS) Completions Survey.

To better understand post-baccalaureate certificate programs, [Patterson \(1999\)](#) surveyed deans, associate deans of graduate schools, and other graduate school officers affiliated with the Council of Graduate Schools (CGS) and the University Continuing Education Association (UCEA). The surveys show that post-baccalaureate certificates are more common at research and doctoral universities than at master’s and bachelor’s institutions. Most of the certificate programs offered are in the business, health, and information science disciplines. The admissions process for each type of program varies considerably. These certificates may act as a stepping stone to a graduate degree as the graduate credits may be transferable to a master’s or doctoral program.

For background information on business certificates specifically, we spoke with someone

who works in a business school at a large, research university. Business certificates are aimed at students who majored in fields not related to business as undergraduates. Often, the business certificate is a way to try out whether an MBA program is right for the student, as some of the courses can transfer over to an MBA program. There is variation in how long students wait after graduating with a bachelor's degree to enroll. Since certificates are non-degree seeking programs, the students are not eligible for financial aid unless they are also enrolled in a degree seeking program. Most enrollees are working full-time or are full-time students in other degree seeking programs. The school did not keep track of post-certificate outcomes such as getting a promotion, getting a new job or getting a raise. The representative said that non-degree granting programs are not required to keep track of what happens after the program ends.

Notwithstanding the increasing popularity, there are concerns about the perceived usefulness of graduate certificates. The first concern pertains to the lack of a clear definition of a post-baccalaureate certificate ([Houston and Marksburly \(2003\)](#)). The term “degree” – associate's, bachelor's, master's or doctor's – is used to recognize a study over known time, known depth (subjects studied) and known standards while “certificates” is a less clear term. Some universities also issue certificates for persons who receive continuing education unit (CEU) while other certificates are issued for non-credit courses, for undergraduate coursework or for professional service. Also, the term certificate is often confused with the term “certification.”³ Thus the meaning of a post-baccalaureate certificate to an employer may be unclear ([Patterson \(1999\)](#)).

This lack of standardization is further exacerbated by the lack of quality control in the market for certificates. Due to the profitability of such programs the field has attracted numerous for-profit firms and encouraged aggressive marketing of certificate programs. [Deming et al. \(2012\)](#) look at outcomes of for-profit students and find that they are more likely to complete short-term programs like certificates but have higher unemployment and lower earnings six years after entering the program. [Deming et al. \(2016b\)](#) use a correspondence study to

³Normally, universities certify that students have demonstrated competence in a certain discipline, and in some cases, the document attesting to this fact is called a certificate. (In other cases, the document may be called a “degree,” “diploma,” or even, colloquially, a “sheepskin.”) ([Patterson \(1999\)](#))

find that for people applying for health care related jobs that have no certificate requirement, having a certificate from a for-profit college decreases the likelihood of a callback compared to having a certificate from a public institution. Thus, if students are getting credentials from for-profit schools, they may have little value in the labor market.

On the other hand, post-baccalaureate certificates may work as a signal in the labor market that the worker is more productive than a similar worker without a certificate. [Gaulke \(2017\)](#) shows that people who enroll in post-baccalaureate training are quite different from those who do not enroll. An unordered multinomial logit model finds that higher ASVAB score (a proxy for ability) is associated with an increase in the odds of enrolling in a post-baccalaureate program versus not enrolling with a p-value of .054. Even within majors, there tends to be a pattern of those with post-baccalaureate training program having higher average ASVAB scores than those who do not enroll. The main exception to this pattern is in the health field. However, in that paper, training programs do not include business certificates due to the lack of popularity at the time of the surveys.

To address the concern that people who self-select into the post-baccalaureate certificate program are different from those who do not, this paper uses a correspondence study. Fictitious résumés are sent to prospective employers to estimate the effect of a post-baccalaureate certificate on the probability of receiving a callback for a job. A correspondence study allows us to control for observable characteristics while randomly assigning whether a candidate has a post-baccalaureate certificate or not. We find that for jobs posted in San Francisco, having a certificate actually significantly decreases the likelihood of a callback. In Los Angeles and New York, there is not significant effect from having the certificate. However, there are two reasons why this might occur. The first is that certificates do not benefit those looking for a new job. The other is a typographical error in which the expected date of the certificate is May 2016 instead of May 2017 which may have harmed application of those with certificates.⁴

The remainder of the paper proceeds as follows. In Section [2.2](#), we give more detail on the related literature. In Section [2.3](#), we describe the experimental design. Section [2.4](#)

⁴Future work, which fixes this error will help us provide clearer answer.

reports the results and interpretation. Section 2.5 concludes and provides suggestions for future studies.

2.2 Related Literature

While there has been a paucity of work related to post-baccalaureate certificates, research on the labor market effects of pre-baccalaureate certificates has been done both nationally and at the state level. Pre-baccalaureate certificates are awarded to those individuals with at least a high school diploma, but less than a baccalaureate degree by community colleges, technical institutes, proprietary and vocational schools.

There are inconsistent findings at the national level. Research before the year 2000 used either the National Longitudinal Study of the High School Class of 1972 (NLS72), the National Adult Literacy Survey (NALS) or the Survey of Income and Program Participation (SIPP) data to analyze the effects of certificates on earnings or wages. Papers that used NLS72 or NALS found certificates yield zero returns (Grubb (1995), Hollenbeck (1993), Rivera-Batiz (1998) and Surette (1997)). Meanwhile, research that used SIPP indicate significant returns to sub-baccalaureate certificates (Grubb (1995) and Ryan (2005)). Inconsistencies in results persist even with the use of the more recent longitudinal surveys from the US Department of Education (Bailey et al. (2004) and Marcotte et al. (2005)). Small sample sizes and a lack of data on program length are concerns raised with this literature (Bosworth (2010)).

In contrast to national level studies, the findings of some state-specific research studies that have used administrative data (i.e. unemployment insurance records) are more consistent. Some of these studies provide information on the value of programs by length and field of study (Bosworth (2010)). Certificate attainment overall has a positive earnings results (For example, see Friedlander (1996) for California and Jepsen et al. (2014) for Kentucky). Field of study is a key determinant of labor market returns of certificates. Jacobson and Mokher (2009) show that, in Florida, median earnings of certificate completers approximate or even surpass median earnings for associate degree completers (especially those in pre-

baccalaureate non-occupational fields who do not leverage their associate’s degree into a bachelor’s degree). With administrative data providing information about program length and program of study, researchers have found that long-term certificates are more valuable in the labor market than short-term certificates. In addition to wage benefits, [Jepsen et al. \(2014\)](#) also show that certificates were found to contribute positively to the probability of employment.

Even with administrative data, self-selection concerns have hampered the ability to make causal conclusions. More recently, research on the effects of education credentials on the labor market outcomes of workers have used résumé-based correspondence studies to circumvent these problems. In these field experiments, researchers prepare fictitious résumés and apply to real jobs online. The characteristics of job applicants including academic credentials are randomly assign in the résumé so that on average, the résumés are identical except for their assignment of education attainment. [Darolia et al. \(2015\)](#) and [Deming et al. \(2016b\)](#) use this method to examine how employers value job applicants with certificates (or degrees) from for-profit colleges. The former does not show any significant effect of certificates while the latter find that employers hiring for health jobs with no certificate or license requirements (primarily medical assistant jobs) strongly prefer applicants with certificates from public institutions, compared with applicants with a for-profit certificate or no credential at all.

2.3 Experimental Design

As described in the first essay, the correspondence study has been used to test for hiring discrimination based on race, ethnicity, immigration, gender, sexual orientation and age. Additionally, the method has been extended to identify hiring penalties associated with motherhood, physical unfitnes, obesity and criminal record.⁵ The method involves applying to job vacancies by sending equally qualified résumés whose only difference is a characteristic

⁵See [Bertrand and Mullainathan \(2004\)](#), [Arai et al. \(2015\)](#), [Arceo-Gomez and Campos-Vazquez \(2014\)](#), [Baert et al. \(2015\)](#), [Carlsson and Rooth \(2007\)](#), [\(Drydakis and Vlassis, 2010\)](#) and [Neumark et al. \(2015\)](#), [Correll et al. \(2007\)](#), [Rooth \(2011\)](#), [Rooth \(2009\)](#) and [Pager \(2003\)](#).

that signals membership to a group.⁶ This paper employs this methodology to study how employers respond to a job applicant's holding of a post-baccalaureate certificate compared to applicants who have none.

While there are benefits to using an experimental design, there are downsides to our approach as well. By using a correspondence study, we only learn about perceived benefits to post-baccalaureate certificates instead of actual productivity gains. Additionally, we can only compare callback rates and not rates of job offers. Also, our applicants are applying for online job postings without using any networking contacts. While this may not be the experience for every job candidate, [Carnevale et al. \(2014\)](#) report that there is better online job ad coverage among high-skilled, white-collar jobs. Given that all of our applicants are college educated, we believe the coverage rate should be a minor issue for this study. A 2015 survey by the Pew Research Center ([Smith \(2015\)](#)) found that 79 percent of individuals say they used online resources and information in their latest job search and 34 percent said that was their most valuable resource. Thus, we believe using online jobs postings will not distort our findings.

That said, there are always external validity concerns with experiments. Our study focuses on relatively new college graduates and thus does not apply to individuals with longer work histories. Additionally, since our outcome of interest is a callback, the study cannot make any conclusions regarding raises or promotions within a company in which a person would not be applying online. Additionally, if certificates are valuable because they help people who are self-employed or help people start their own businesses, that will not be picked up in comparing callback rates.

The first phase of this experiment was implemented between January, 2017, and April, 2017. Over this period, we surveyed eligible employment ads from multiple online job boards. We prepared a pool of résumés, and for each job ad, we drew and sent a pair of fictitious résumés from our pool and sent them to employers. We then measured employers' responses to our fictitious job seekers' application.⁷

⁶The experimental protocol was reviewed and approved by the Institutional Review Board at Kansas State University.

⁷The experimental protocol was reviewed and approved by the Institutional Review Board at Kansas

The first step in our experiment is surveying for eligible job ads. As in the first essay, we chose the following occupations: managerial, clerical/administrative assistant and accounting assistant jobs. We targeted these jobs for several reasons. First, these types of jobs do not require complex skills and are fairly similar across firms which allow us to easily create suitable generic résumés. Second, there are enough numbers of available jobs in online job boards in these fields to conduct a sufficiently powered study. We limited our sample to entry job ads for fresh college graduates. We restricted our experiment to three cities: New York, Los Angeles, and San Francisco. These cities were chosen based on geographic proximity to certificate granting schools. We chose jobs that allowed direct uploads of résumés to apply. We eliminated any ad where applicants were asked to call or appear in person or that required résumés to be submitted to external websites. We recorded available information about the job, including the date the job ad was posted, position, company name, company address and job requirements (education level and skills required). We collected jobs and sent résumés by batch. More specifically, the size of a batch depends on the available jobs at the particular time of data collection and ranges from about 30 to 50 jobs. We prepared and sent the résumés for one batch of job ads before collecting a new batch of job ads.

The next step is to prepare the résumés. Using the résumé randomizer developed by [Lahey and Beasley \(2009\)](#), we first created our pool of résumés by randomly assigning résumé details, most importantly the treatment/control assignment, to each résumé.⁸ Each résumé in the pool has a corresponding partner résumé and the two of them constitute one pair. Each résumé in the pair either have a post-baccalaureate certificate or none. Résumés that were randomly assigned the post-baccalaureate certificate belong to the treatment group while résumés that were randomly assigned none belong to the control group. For each job ad, a pair of résumés is drawn and sent.⁹ By differing the presence of post-baccalaureate certificate in each of the résumés in each pair, we can identify the effects of post-baccalaureate certificates on the employment prospects of job seekers. The rest of the details were randomly

State University. The protocol is closely patterned to the experimental design in the first chapter of this dissertation.

⁸We exported these characteristics assignment to a spreadsheet, which was used as input to résumé creation in Microsoft Word using the Mail Merge function.

⁹Accordingly, we have x pairs since we applied to x jobs.

assigned to each résumé and designed so that there would be no repetition of exact details for each pair of résumés and so that each treatment group is very similar on average.

All of the résumés in the pool indicated a completion of a bachelor's degree from a university. To not send any additional signals such as race or religious affiliation we focused on public universities. We further restricted the schools to be ranked by the U.S. News and World Reports as a top fifty to top one hundred and fifty national university. National universities should be well known, and the ranking restriction should reduce variation in callback rates across the university of bachelor's degree completion. We designed the assignment so that 75 percent of the time, the résumé would be assigned a university from the same state as the location of the position advertised and 25 percent of the time a university outside the state. We also designed the assignment of bachelor's degree so that the distribution is close to the distribution of degrees awarded in national surveys.

Since we targeted jobs that required 1 year or 2 years of experience, we designed the work histories such that the total years of experience was approximately 2 years. Each résumé was designed such that the applicant has only one job and were employed at the time of application. For each type of occupation (i.e. manager, accounting assistant or administrative assistant), their current job was related to the job description. Tenure in the applicant's current (and sole) employment is either one or two years. This, in turn, determined the year of graduation from college. For example, if the résumé was assigned a tenure of two years, the start date of the first job was two years before the current date (when the job application was sent). Details in the skills and the objective career sections were related to the position applied for and were randomly assigned across résumés.

As in the first essay, we chose common first names in 1990 and last names that were most likely to signal that the applicant was Caucasian to prevent any name-based employment discrimination from influencing the results. For females, we used Jessica and Lisa. For males, we chose Michael and Robert. Each name was assigned a corresponding telephone number and email address. To easily track the callback, we assigned the name, a corresponding telephone number and e-mail address based on the treatment type. Those who were assigned

treatment, for example, were assigned the name Lisa and Robert and the corresponding email address and telephone number. We again used Vumber to get two online telephone numbers by city, one for each treatment group. These did not appear any different than regular phone numbers to the employer, but had the benefit that the calls and voicemails were recorded in an online account and no physical phones were required. Residential addresses on the résumés were selected carefully to ensure that they were realistic. We used Zillow.com to get real addresses, but we changed the housing/apartment number/letters to generate fictitious addresses. We created two résumé templates. Templates were randomly assigned to each résumé created. The same template was not used in a pair to prevent the employers from detecting the experiment. For each job ad, the résumé randomizer assigned whether the pair would be all male or all female.

Once the résumés were generated, we converted the files to PDF formats. We named the files based on the name or initials of the applicant. We made sure that the filename style differs within pair to minimize chances of detecting the experiment. Appendix B provides a sample résumé for each of the treatment groups. The first page in Appendix B show a sample résumé of an applicant without a certificate. The next page shows a sample résumé of an applicant with a post-baccalaureate certificate. In sending the résumés, we randomly assigned the order by which the résumés were sent. For each pair, the second résumé is sent at least a day after the first résumé was sent. We recorded the day the résumé was sent in the database.

We measured whether a given résumé receives a response from potential employers. For each phone, text or e-mail response, we match the response to the corresponding résumé/job ad pair based on the content of the message left by the employer (name of the applicant, company name, telephone number for contact).¹⁰ As in the first experiment, we defined a callback as a personalized phone or e-mail contact by a potential employer. Usually, the callback was a request for an interview, but employers also contacted applicants asking for more information or stated that they have a few questions. After hearing from employers,

¹⁰Response by employers via postal mail cannot be measured in our experiment since the residential addresses are fictitious.

we sent a message to them that the applicant is no longer available for the job.

2.4 Results

The descriptive statistics, overall and separately by city, are shown in Table 2.2. Note that, while half of the résumé pairs are women, less than half of the sample are women, due to some jobs expiring either before any applications can be sent, or after the first application is sent but before the second can be sent. The overall callback rate is 11.5 percent, with San Francisco having the highest rate, at 18.6 percent.

Table 2.2: Descriptive Statistics

	(1) All mean/sd	(2) New York mean/sd	(3) San Francisco mean/sd	(4) Los Angeles mean/sd
Callback	0.115 (0.320)	0.086 (0.280)	0.186 (0.390)	0.102 (0.302)
Female	0.468 (0.499)	0.475 (0.500)	0.466 (0.499)	0.464 (0.499)
Occupation				
Manager	0.368 (0.482)	0.383 (0.486)	0.291 (0.455)	0.401 (0.490)
Acct. Assist.	0.274 (0.446)	0.269 (0.444)	0.243 (0.429)	0.299 (0.458)
Admin. Assist.	0.358 (0.480)	0.349 (0.477)	0.466 (0.499)	0.299 (0.458)
City				
New York	0.396 (0.489)			
San Francisco	0.237 (0.425)			
Los Angeles	0.368 (0.482)			
Observations	2,088	826	494	768

Notes: Each observation is an application. Numbers represent fractions.

Table 2.3 shows the callback rate by the control group (no certificate) and the treatment group (certificate), as well as by other groups. The main result is the overall difference in the callback rates between the control and treatment groups: 12.5 percent for the no certificate group, and 10.6 percent for the certificate group. In other words, job applicants with a

Table 2.3: Callback Rate, No Certificate (Control) versus Certificate (Treatment), by Group

	(1) No Certificate mean	(2) Certificate mean
Total	0.125	0.106
Gender		
Male	0.112	0.086
Female	0.139	0.129
City		
New York	0.090	0.082
San Francisco	0.215	0.158
Los Angeles	0.104	0.099
Occupation		
Manager	0.156	0.125
Admin. Assist.	0.131	0.096
Acct. Assist.	0.073	0.094
Observations	1,044	1,044

Notes: Each value is the callback rate of that group, which is the fraction of applications that received a response from employers. Column (1) shows callback rates for the group without a certificate (control), while column (2) shows callback rates for the group with a certificate (treatment).

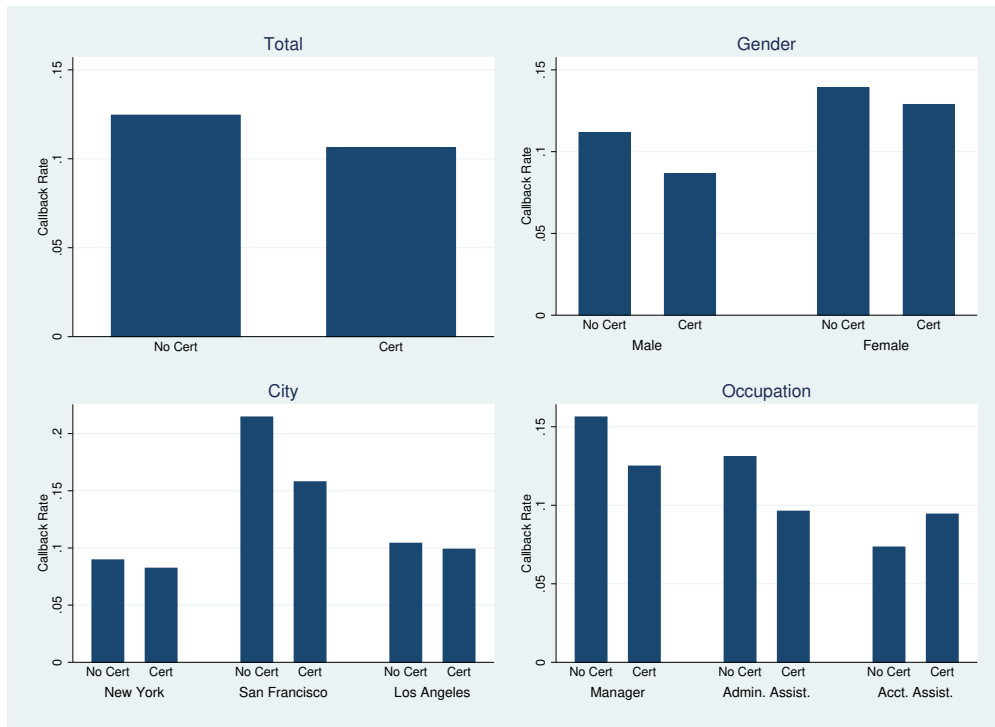
certificate are 1.9 percentage point less likely to receive a callback relative to job applicants with no certificates. Given the overall rate of 11.5 percent, this represents a 16.5 percent lower probability of a callback. Thus, it appears that post-baccalaureate certificates may, in fact, be harmful to job seekers.

Table 2.3 also shows the callback rate by gender, city, and occupation, again separately by control versus treatment groups. These results are also shown graphically in Figure 2.1. The penalty for having a certificate is common across all groups, to various degrees, except for the accounting assistant occupation, where a positive effect of having a certificate is observed. The differences in the callback rates between the control and treatment groups are 2.6 percentage points and 1.0 percentage points for men and women, respectively, which indicates that the presence of a certificate appears to impact men’s callback rates more so than women’s. In all cities, we observe that job applicants with certificates have a smaller chance of receiving a call relative to those with no certificates. However, this difference is significantly more pronounced in San Francisco (5.7 percentage points difference) compared to the differences observed in Los Angeles (0.5 percentage point) and New York (0.8 percentage point). The results are also different by occupation. We notice negative effects of certificates

in managerial and administrative assistant jobs but a positive effect in accounting assistant jobs.

An advantage of our methodology - sending pairs of résumés to the same job ad - is that it allows us to look at how frequently neither, both, or just one job applicant received a callback, and whether that varies by certificate. In Table 2.4, we compare the relative proportions of the following four groups of outcomes for each application pair: 1) neither control nor treatment application received a callback; 2) only the control received a callback; 3) only the treatment received a callback; and 4) both treatment and control received a callback. The most common outcome, at 83.1 percent, is neither application to receive a callback. 6.2 percent of application pairs are in the final group, i.e. where both treatment and control receive a callback. In the event of only a single callback in the pair (the middle two groups), the application without the certificate is 40.9 percent more likely to have received the callback than the application with the certificate. This result highlights the apparently negative impact of holding a certificate on the probability of receiving a callback.

Figure 2.1: Callback Rate, No Certificate versus Certificate, by Group



We more thoroughly test the effect of having a certificate on the probability of a callback

by estimating a series of linear probability regressions of the following form:

$$Y_{iomc} = \beta * Cert_i + X_{iomc} * \rho + \gamma_o + \lambda_c + \delta_m + \alpha_t + \epsilon_{iomc} \quad (2.1)$$

where i refers to the individual, o refers to occupation, m refers to major, and c refers to city. Y_{iomc} equals one if the applicant received a callback, and zero otherwise, while $Cert_i$ equals one if the applicant is in the treatment group (certificate), and zero otherwise. Our main outcome of interest is β which is the coefficient on having a post-baccalaureate certificate. We also include controls for the month the job was posted, whether the application was the first or second of the pair sent, the number of days between when the job ad was posted, and the application was sent, undergraduate university, and undergraduate degree type.

The results are shown in Table 2.5. Consistent with the previous discussion, we see evidence of a negative effect of having a post-baccalaureate certificate on the probability of receiving a callback. Relative to the control group, the treatment group has a 2.4 percentage point, and thus a 20.9 percent lower probability of receiving a callback. To test whether this effect varies by gender, column (2) interacts the treatment variable (Certificate) with a female dummy variable. While the interaction term is positive, suggesting that the penalty of having a certificate is lower for women than men, the difference is insignificant at conventional levels. We repeat the estimate from column (1) separately for men and women, in columns (3) and (4), respectively. The treatment variable is negative for both genders though larger

Table 2.4: Distribution of Application Pairs Across Callback Types

	Number	Percent
No Callbacks	868	83.1
One Callback - No Cert	65	6.2
One Callback - Cert	46	4.4
Two Callbacks	65	6.2
Total	1,044	100.0

Notes: Table shows the distribution of application pairs across four types: 1) neither control nor treatment application received a callback; 2) only the control received a callback; 3) only the treatment received a callback; and 4) both treatment and control received a callback.

Table 2.5: Linear Probability Regressions, Callback

	All		Men	Women
	(1)	(2)	(3)	(4)
Certificate	-0.024** (0.011)	-0.034** (0.014)	-0.034** (0.014)	-0.021 (0.017)
Cert X Female		0.021 (0.021)		
Female	0.041** (0.017)	0.030 (0.021)		
City				
San Francisco	0.142*** (0.035)	0.142*** (0.035)	0.120*** (0.042)	0.173*** (0.061)
Los Angeles	0.051* (0.029)	0.051* (0.030)	0.045 (0.036)	0.074 (0.052)
Occupation				
Admin. Assist.	-0.035* (0.021)	-0.035* (0.021)	-0.065** (0.026)	-0.006 (0.032)
Acct. Assist.	-0.049** (0.022)	-0.049** (0.022)	-0.067** (0.029)	-0.039 (0.034)
Observations	2,088	2,088	1,110	978
R ²	0.039	0.039	0.054	0.047

Notes: Dependent variable is binary, and equals one if the applicant received a callback, and zero otherwise. Omitted categories are *Manager* for occupation and *New York* for city. Columns (1) and (2) include the full sample, while column (3) includes only men and column (4) includes only women. All specifications include controls for month job was posted, undergraduate degree type, undergraduate school, days between job posting and when application was sent, and whether the application was the first of the pair sent.

Standard errors in parentheses, and are clustered at the job level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

in magnitude for men, at -0.034 versus -0.021 for men versus women, respectively, and it is significantly different from zero at the 5 percent level for men though not significantly different from zero at conventional levels for women.

To test whether the certificate impacts the callback rate differently across occupations, the previous results are repeated separately by occupation. The results are shown in Table 2.6. For both managers and administrative assistants (columns 1 and 2, respectively), having

Table 2.6: Linear Probability Regressions, Callback, by Job Type

	(1) Manager	(2) Admin. Assist.	(3) Acct. Assist.
Certificate	-0.029 (0.018)	-0.043** (0.018)	0.013 (0.021)
Female	0.007 (0.031)	0.069** (0.027)	0.046* (0.026)
City			
San Francisco	0.095 (0.070)	0.176*** (0.053)	0.120** (0.058)
Los Angeles	-0.033 (0.057)	0.070 (0.047)	0.129** (0.056)
Observations	768	748	572
R ²	0.043	0.073	0.069

Notes: Dependent variable is binary, and equals one if the applicant received a callback, and zero otherwise. Omitted category is *New York*. Results shown separately by job opening type, with managers in column (1), administrative assistants in column (2), and accounting assistants in column (3). All specifications include controls for month job was posted, undergraduate degree type, undergraduate school, days between job posting and when application was sent, and whether the application was the first of the pair sent.

Standard errors in parentheses, and are clustered at the job level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

a certificate is related to a substantial reduction in the probability of a callback relative to the probability of receiving a callback by those who have no certificate: for managers, the reduction is 2.9 percentage points (i.e. 18.6 percent relative to the overall manager response rate), while for administrative assistants, the reduction is 4.3 percentage points (i.e. 32.8 percent relative to the overall manager administrative assistant response rate). The effect is only statistically significant for administrative assistants, however. While the effect of having a certificate is positive for accounting assistants, the coefficient is relatively small and insignificant at conventional levels.

Finally, we test the impact of holding a certificate separately by city, with the results shown in Table 2.7. Only in the San Francisco sample is there a statistically significant effect of holding a certificate on callback rate, though the effect is quite large, at 7.6 percentage

Table 2.7: Linear Probability Regressions, Callback, by City

	(1) New York	(2) San Francisco	(3) Los Angeles
Certificate	-0.007 (0.013)	-0.076*** (0.027)	-0.013 (0.019)
Female	0.035 (0.024)	0.058 (0.043)	0.038 (0.026)
Occupation			
Admin. Assist.	-0.077*** (0.030)	-0.068 (0.057)	-0.004 (0.032)
Acct. Assist.	-0.058* (0.033)	-0.215*** (0.066)	0.034 (0.034)
Observations	826	494	768
R ²	0.037	0.064	0.044

Notes: Dependent variable is binary, and equals one if the applicant received a callback, and zero otherwise. Omitted category is *Manager*. Results shown separately by city, with New York in column (1), San Francisco in column (2), and Los Angeles in column (3). All specifications include controls for month job was posted, undergraduate degree type, undergraduate school, days between job posting and when application was sent, and whether the application was the first of the pair sent. Standard errors in parentheses, and are clustered at the job level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

points (i.e. 35.3 percent, relative to the overall rate in that city). This negative effect also does not appear to be driven by a particular certificate-granting school: within application pairs, for each certificate school in San Francisco, the application with the certificate received a lower callback than the application without the certificate. Thus, it appears that there may be some aspect of the labor market in that city that makes these certificates of significantly negative value. Future work that fixes this error will give us a clearer answer to the cause of this negative effect.

Overall, our results point to a somewhat surprising conclusion: job applicants who hold a post-baccalaureate tend to have significantly lower callback rates than otherwise similar applicants without certificates. The overall negative effect of post-baccalaureate certificates is driven by the large negative effect observed in San Francisco and the negative effect observed in administrative jobs. We do not observed significant effects of post-baccalaureate

certificates in New York and Los Angeles as well as in managerial jobs. Only in accounting jobs do we find what might be expected, which is that these post-baccalaureate certificates improve callback rates. The caveat to this finding is that there was a typo in the résumés with certificates such that the expected date of completion was listed as May 2016 instead of May 2017. Thus, it is unclear whether the effect is being driven by the perceived value of the certificate or whether the typo is sending a negative signal about the applicant's quality.

2.5 Conclusion

This paper estimates the causal effect of post-baccalaureate business certificates on the probability of getting a callback for recent college graduates. A correspondence study is used to find that a résumé randomly assigned a post-baccalaureate business certificate is significantly less likely to receive a callback in San Francisco. In the other cities in the study, there are no significant differences between the two groups. Thus, the glowing testimonials reported by schools are likely due to positive self-selection into getting the certificate. This matches the anecdotal evidence provided by our contact in the business school who said the students who enroll tend to be the most motivated students. Thus, the claims made by schools that these programs yield strong positive results are not substantiated by this experiment. However, a typo in the expected date of certificate completion is a competing explanation as to why we find null or negative results depending on the city. Going forward, this mistake has been corrected, and we will be able to examine the effect of certificates in a résumé without a typo.

Chapter 3

The Short-Run Effects of Employer-Sponsored Health Insurance on the Labor Market Supply of Ill Workers

3.1 Introduction

When diagnosed with a chronic health condition, workers who have employer-sponsored health insurance (ESI) benefits face a labor supply dilemma. On the one hand, newly diagnosed workers may want to reduce labor supply or stop working altogether because they may need to take time off from work for treatment and convalescence. They may also want to reduce work hours as work stress can exacerbate their health problems. On the other hand, workers with chronic health conditions also face increased risk of health care expenses and subsequently, increased demand for health insurance. Since a majority of workers in the U.S. obtain health insurance from full-time employment¹, most workers with health problems

¹In 2008, 61.1 percent of workers had coverage through their own employer (Rho and Schmitt (2010)). Most of these workers have to be full-time workers to be eligible for health insurance benefits.

must remain sufficiently employed to maintain health insurance coverage. This incentive to work may constrain labor supply changes desired by workers with chronic health conditions.

While workers with no ESI may obtain health insurance in the individual markets, such option is more expensive compared to prices paid if workers obtain it through their own employment. Participants in the individual markets are worse off than workers with ESI for a number of reasons. Because insuring firms pool the risks of their employees, employers pay lower premiums from reduced adverse selection and lower administrative expenses. Moreover, the health insurance costs of employers are also tax-deductible, further reducing the cost of ESI relative to insurance from individual markets.

Prior to the implementation of the Patient Protection and Affordable Care Act (otherwise, known as the Affordable Care Act (ACA)), the price gap between individual markets and pooled markets was greater for individuals with pre-existing conditions.² For firms offering health insurance to their workers, the worker and the employer pay a share of the health insurance premiums of the worker. The premiums paid by workers whether they have pre-existing conditions or not are the same because the Health Insurance Portability and Accountability Act (HIPAA) prohibits employers from charging more based on medical conditions, claims experience, receipt of health care services, genetic information or disability. Pre-ACA, individuals with pre-existing conditions who did not have health insurance through their employer had difficulty finding insurance companies that offered affordable individual insurance plans because no law prevented insurance companies from charging higher rates for pre-existing conditions.

Regaining much-needed coverage through new employment is also challenging. First, employees with health problems may have a lower likelihood of finding a job in firms offering health insurance. Discrimination against less healthy individuals can arise from insurers'

²The Kaiser Family Foundation studied the efforts that seven hypothetically ill individuals would have to make to find health insurance. The average annual premium offered was \$3,996, a significant increase from the standard average annual rate of \$2,998 (Pollitz et al. (2001)). With the ACA, there are some reports that individual rates may be even lower than group rates because individual health plans must cover all individuals regardless of health and new subsidies are available for qualifying employees. (For example, see <https://www.zanebenefits.com/blog/bid/322844/why-individual-health-insurance-is-more-affordable-than-group-health-insurance>.)

practice of experience rating when setting a company's health insurance premiums. This matters more for small firms wherein one major illness may increase the group's insurance costs. Although the American Disabilities Act (ADA) prohibits screening for health in hiring, the law does not prohibit insurers from charging higher rates to account for expected higher medical expenses for workers with a poor health history. To avoid high cost, firms may refuse to hire workers with pre-existing conditions. Second, even if workers with pre-existing conditions get employed by businesses offering health insurance, coverage is subject to length of service requirements. Although this is only temporary, immediate coverage is essential for workers who require medicines and/or medical services.

Legal provisions set in place to temporarily protect workers from losing their health insurance coverage along with their employment do not guarantee protection. The Consolidated Omnibus Reconciliation Act of 1985 (COBRA) mandates previous employers to provide coverage for recently unemployed workers for 18 months. However, only a few take continued coverage from previous employers because the employee must pay the full cost of the coverage, which is defined as 102 percent of the average cost of providing coverage. Doty, Rustgi, Schoen and Collins (2009) found that although most unemployed workers are eligible, fewer than one in ten extends coverage under this option for cost reasons.

The HIPAA also provides little additional protection. After exhausting coverage from COBRA extension, the HIPAA enables workers to convert their group coverage into a renewable individual policy without exclusion for pre-existing conditions. However, this option may not be desirable as HIPAA does not cap the premium that the offering insurer may charge. With the HIPAA, employees may also add to their insurance policy a spouse or other dependent who loses job-related coverage, without waiting until the next open enrollment cycle. This protection, however, is not beneficial to workers with medical conditions whose spouses are not employed and those with employed spouses whose employer does not offer health insurance coverage for family members.

The Family Medical Leave Act (FMLA) also entitles eligible employees of covered employers to take 12 workweeks of unpaid, job-protected leave for specified family and medical reasons with continuation of group health insurance coverage (United States Department

of Labor, Wage and Hour Division, 2012). Although the Family Medical Leave Act (FMLA) may help workers feel less constrained to stay at work and remain insured, not all workers are covered and covered workers may not want to use FMLA for fear of adverse effects in the workplace.³ In summary, these legislative measures may not keep ill workers from being locked into their jobs to keep their health insurance.

To see whether employment-sponsored health insurance creates incentives for ill workers to supply labor to maintain access to health insurance coverage, this paper compares the labor supply changes made after a diagnosis by workers who depend on their employers for health insurance to the changes in labor supply made by workers who are dependent on their spouse's employment for their insurance. I use the latter as the comparison group instead of workers with no health insurance because these workers with coverage through their own employer and workers with coverage through their spouse's employer are more likely to be homogeneous in terms of observable and unobservable attributes. I focus on the following labor supply changes made 0, 6, 12 and 24 months after the diagnosis: changes in hours worked, the probability of remaining employed and the probability of moving from full-time to part-time.

The results of the paper confirm most of the findings of previous studies that ESI creates incentives for workers with chronic health conditions to remain employed at a sufficient level to maintain access to health insurance. Workers with their own employer-sponsored insurance are 2.5 percentage points more likely to stay employed than workers who are dependent on their spouse's employer for their health insurance at the time of diagnosis. This likelihood persists up to 24 months post-diagnosis and is significant 6 to 12 months after diagnosis. For workers who have remained employed after their diagnosis, workers who are dependent on their spouse's employer for their health insurance reduce their labor hours by more than workers with their own employer-sponsored insurance. This difference in percentage reduction is 3.6 percent for the time of diagnosis, and it increases with time to 5.4 percent 24 months after diagnosis. There is also evidence that the likelihood of employees

³According to the U.S. Department of Labor, employees are eligible to take FMLA leave if they have worked for their employer for at least 12 months, have worked for at least 1,250 hours over the previous 12 months, and work at a location where at least 50 employees are employed by the employer within 75 miles.

who have insurance through their own employer are less likely to become part-time workers.

This paper addresses one of the limitations of the previous studies. First, the scope of earlier studies was limited to either a single state, a single disease or a particular gender or age group which limits the generalizability of conclusions to other settings. This paper addresses such limitation by using the national NLSY data which reports information on diagnosis of various illnesses. A large percentage of the diagnoses reported in NLSY were hypertension, diabetes, arthritis, and mental health problems, which compared to previous studies is more reflective of the national distribution of chronic health conditions faced by residents in the US. These conditions were not included in any of the previous studies done on this topic. The information from the NLSY dataset enables the examination of ESI effects by illnesses although insufficient observations for other diseases poses a challenge. Since most of the diseases in the NLSY are not life-threatening compared diseases investigated in previous studies, this paper contributes analysis for the effects of ESI for workers with a diagnosis of less severe conditions.

The rising prevalence of chronic illness in the workplace and the strong dependence of these workers on their employers for health insurance highlights the importance of the incentives and possible pitfalls that ESI creates.⁴ This paper aims to help policy makers understand some of these and the effectiveness of existing legislation in muting or exacerbating the effects of ESI on how workers make labor supply adjustments in response to health problems.

The remainder of the paper proceeds as follows. In Section 3.2, I present a review of related literature. This is followed by Section 3.3 which explains the empirical strategy. Section 3.4 which describes the data while Section 3.5 reports the results and interpretation. Section 3.6 concludes and provides suggestions for future studies.

⁴The incidence of chronic illness in the workplace is estimated by Hoffman and Schwartz (2008) to have grown by 25% over the past 10 years to a total of nearly 58 million in 2006.

3.2 Review of Related Literature

This investigation is related to following three strands of the literature: 1) research on the labor supply of individuals who have ESI; 2) research on the effects of health on labor supply; 3) research on the effects of the interaction of health status and ESI.

The first strand of literature focuses on testing for “job lock” or the hypothesis that workers are locked into their job to keep their health insurance. [Gruber and Madrian \(2002\)](#), in their review of the literature on ESI, labor supply, and job mobility, conclude that there is convincing evidence that health insurance plays a major role in job mobility decisions. Older workers, for example, would not opt to retire unless there is an alternative coverage after retiring from continued health insurance from employers or Medicaid.⁵ There is also evidence that such job immobility is exhibited by married women. [Buchmueller and Valletta \(1999\)](#), [Olson \(1998\)](#), [Wellington and Cobb-Clark \(2000\)](#) and [Schone and Vistnes \(1997\)](#), among others, find that the availability of spousal health insurance reduces labor participation and has other effects on job choice decisions of married women.

This paper is also broadly related to research on the labor supply effects of health which, for the most part, finds that poor health decrease labor supply.⁶ For example, results of [Bradley et al. \(2007b\)](#) show that women with breast cancer, except those having in situ cancer, were less likely to work 6 months following diagnosis relative to a control sample of women. In the case of workers with psychiatric disorders, [Ettner et al. \(1997\)](#) find that such disorders significantly reduced employment among men and women. This result has also been observed in workers with diabetes ([Vijan et al. \(2004\)](#)).

Most closely related to this study is the third strand which directly analyzes the changes in labor supply after a health shock. Previous studies show that the interaction of employment-based health insurance (ESI) and health shocks constrain labor supply in the short-run. Using Cox proportional hazard models, [Stroupe et al. \(2001\)](#) find that chronic illness reduces job mobility of workers in Indiana who relied on their employer for coverage by about 40

⁵For examples, see [Gruber and Madrian \(1996\)](#), [Gruber and Madrian \(1995\)](#), [Gustman and Steinmeier \(1994\)](#), [Rogowski and Karoly \(2000\)](#) and [Blau and Gilleskie \(2001\)](#).

⁶[Currie and Madrian \(1999\)](#) provide a review on the effects of health on labor hours.

percent as compared with otherwise similar workers who did not rely on their employer for coverage. [Bradley et al. \(2012\)](#) and [Bradley et al. \(2007a\)](#) collect primary data from a sample of married women who were newly diagnosed with breast cancer in Virginia and Detroit, respectively. Both studies find that women with ESI were significantly more likely to remain employed and less likely to cut weekly hours worked than were women with insurance from spouse's employer. [Tunceli et al. \(2009\)](#) examine the labor market supply changes of older men and women from Pennsylvania diagnosed with all types of cancer from 1997 through 1999 compared to a non-cancer sample drawn from the Health and Retirement Study (HRS). They report higher employment rates after a cancer diagnosis for those with ESI compared to those who had an alternative source of health insurance or who were uninsured. [Bradley et al. \(2012\)](#) focus on older married men and study the effects of cancer, congestive heart failure, stroke, lung disease, stroke and angina on labor supply. The study finds that for some specifications of health shocks ESI encourages continued employment of men, although not of women.

In this handful of studies that analyze the changes in labor supply after a health shock, the scope of earlier studies is limited to either a single state, a single disease or a certain gender or age group which limits the generalizability of conclusions to other settings. The main contribution of the paper is that it uses the NLSY dataset which includes more medical conditions and has national distribution. This paper focuses its analysis on the effects of ESI for workers with benign and/or more common chronic diseases like hypertension, arthritis and mental conditions.

3.3 Empirical Strategy

In this paper, I study the effects of having ESI before diagnosis on the labor supply outcomes of newly diagnosed workers 0, 6, 12 and 24 months after diagnosis. I consider a sample of workers who are fully employed at the baseline (pre-diagnosis) period. One is considered a full-time worker if he or she works at least 32 hours per week.⁷ The labor market outcomes

⁷As in [Bradley et al. \(2007a\)](#).

considered are employment status (E), change in mean weekly hours worked (CH) and change from full-time to part-time work ($FT2PT$). E_{it} is equal to 1 if worker i is employed $t = 0, 6, 12, 24$ months after diagnosis and 0 otherwise. CH_{it} is the growth rate of H_{it} or the average work hours of worker i at time $t = 0, 6, 12, 24$ months after diagnosis relative to H_{ib} or the average work hours of worker i at the baseline period b . CH_{it} is calculated using the following equation:

$$CH_{it} = \frac{H_{it} - H_{ib}}{H_{ib}} * 100 \quad (3.1)$$

$FT2PT_{it}$ is equal to 1 if worker i was fully employed at the baseline period b but is partially employed t months after diagnosis and 0 otherwise.

The labor supply outcomes are modeled as functions of worker i 's source of health insurance in the baseline period (ESI_{ib}), a vector of control variables observed in the baseline period (X_{ib}) and unobserved influences (ϵ_{it}). ESI_{ib} is equal to 1 if worker i has ESI at the baseline period b or 0 if worker i depends on spouse's employer for health insurance at the baseline period. In particular, the following equation:

$$Y_{it} = \alpha + \beta ESI_{it} + X_{ib}\gamma + \epsilon_{it} \quad (3.2)$$

estimates the effects of ESI (and other exogenous variables) for labor supply outcome, Y , of workers who are fully employed ($FT_{ib} = 1$) and have insurance either from their own or their spouse's employer in the baseline period. I separately estimate Equation 3.2 for different labor market outcomes, $Y = E_{it}$, CH_{it} and $FT2PT_{it}$ at each period t (0, 6, 12, 18 months after the diagnosis). For $Y = CH_{it}$ and $FT2PT_{it}$, I estimate Equation 3.2 conditional on the worker being employed post-diagnosis. This condition is imposed to separate any employment effects on these two labor supply indicators. Equation 3.2 is estimated as a linear probability model.

I am interested in the coefficient on ESI, β . For estimated models with Y equal to E_{it} and Y equal to CH_{it} , a positive and significant β indicates significant effects of ESI in locking workers with a diagnosis to employment and to a certain amount of work hours in

order to secure health insurance. For estimated models with Y equals to E_{it} , a positive β implies that workers with ESI tend to remain employed relative to workers with no ESI. For estimated models with Y equals to CH_{it} , a positive β implies that the reduction in mean hours worked is smaller for workers with ESI than for workers with no ESI if both groups of workers reduced their labor hours. For estimated models with Y equal to $FT2PT_{it}$, a negative and significant β indicates significant effects of ESI in preventing workers with a diagnosis from becoming part-time workers in order to secure health insurance.

To control for observable differences between workers with ESI and workers insured through their spouse, I include the following control variables in X_{it} : year of diagnosis, gender, race, an indicator of whether there are any children in the household and its interaction with gender, marital status, age, age squared, education level measured by highest grade completed and AFQT score which is a proxy for ability.⁸

Jobs that offer ESI may employ workers who are more dedicated and are more likely to remain at work regardless of their cancer diagnosis. To account for variation in hours worked in the pre-diagnosis period, I include the mean labor hours worked during the baseline period in the vector of control variables and estimate the model using full-time workers alone.⁹ Workers with ESI may also tend to have lower income and/or are more likely to be the breadwinner and hence, their labor supply may decrease less compared to a comparatively ill worker who does not have ESI. To control for this, I also include pre-diagnosis household income in 2002 dollars and pre-diagnosis share of worker i 's annual income to household income.

To control for any differences in job characteristics, I also include job tenure, location of residence (urban or rural), number of employees at the workplace and industry category. It is possible that jobs that offer health insurance benefits require less physical strength. If this is the case, workers with ESI may appear to reduce labor supply less than workers with no ESI. This difference may be incorrectly attributed to the difference in the source of health insurance. In view of this, I also include occupation category in the vector of control

⁸Most of these covariates were used in wage models used by [Cowan and Schwab \(2011b\)](#) which also used the NLSY79 dataset.

⁹As in [Bradley et al. \(2007a\)](#).

variables, X_{ib} .

The severity of a chronic illness may limit the kind or amount of work that a person can do and subsequently, the labor supply response. To see whether the labor supply response differential associated with ESI varies by severity, I also estimate the following equation:

$$Y_{it} = \alpha + \beta ESI_{ib} + \delta limit_{it} + \mu limit_{it} * ESI_{ib} + X_{ib}\gamma + \epsilon_{it} \quad (3.3)$$

using a sample that only includes respondents who have no work limitation before diagnosis and include interactions of an indicator of post-diagnosis work limitation and ESI. The proxy indicator for severity of illness is a dummy variable, $limit_{i,after}$ that is set equal to 1 if the respondent experienced any current work limitation. The empirical model shows the main and interactive effects of severity of the condition and health insurance source on labor supply indicators t months following diagnosis. As usual, β measures the effect of having ESI for workers with no limitation while δ measures the effect of having work limitation (that is likely caused by the severity of illness). The coefficient, μ , measures the interactive effect of having work limitation and ESI.

Using the following equation:

$$Y_{it} = \alpha + \beta ESI_{ib}^{only} + \delta ESI_{ib}^{dependents} + X_{ib}\gamma_{yt} + \epsilon_{it} \quad (3.4)$$

I also test whether newly diagnosed workers whose employers also cover insurance of their spouse and/or child are more locked into their jobs than workers who have ESI but no dependents. $ESI_{ib}^{dependents}$ is equal to 1 if worker i has other family members who have the same ESI coverage and 0 otherwise and ESI_{ib}^{only} is equals to 1 if worker i has no other family members who have the same ESI coverage and 0 otherwise. The omitted variable is the dummy variable which is equal to 1 if the worker has coverage through spouse's employer. If δ is statistically significantly greater than β , then this means that workers with dependents are more locked in their jobs than workers who have ESI but no dependents. I also use the F-test to test the null hypothesis, $\delta \neq \beta$. If the null hypothesis is rejected and δ is greater

than β , then there is evidence of greater job/hour lock effects of ESI for ill workers with dependents than ill workers with no dependents.

3.4 Data

I use data from the National Longitudinal Survey of Youth (NLSY) which is a nationally representative sample of 12,686 people aged 14-22 years in 1979. The NLSY was administered annually until 1994, and biennially through 2014. Respondents were asked to answer the first health module upon reaching 40 years old and another health module upon reaching 50 years old. These modules ask whether the respondent was ever diagnosed with a chronic illness and if yes, when (month and year) he or she was diagnosed. The chronic diseases covered in the modules were hypertension, diabetes, arthritis, cancer, mental health problems, heart attack, heart failure and stroke. By 2014, all the respondents had reached 50 years old and had answered these health modules. As of 2014, about 4,892 respondents reported having one of the diagnoses mentioned above. There were multiple diagnoses to other respondents, but I restrict my sample to the first chronic illness diagnosis.

The NLSY asks whether respondents have health insurance policy and if yes, the source of the policy. The source of the policy could be from the current employer, previous employer, spouse's current employer, spouse's previous employer, individual markets, government or others. Unfortunately, only take-up data is available, and there is no information on whether the respondent or the spouse was offered any health insurance coverage from other sources.

I only use post-1988 data because the earlier years of the survey did not include questions on health insurance status. Since I am only interested in the comparative effects of ESI and insurance from spouse's employer on the labor supply of workers with diagnosis, I limit my main analysis to the sample of workers who get their coverage through their own employer or their spouse's employer.¹⁰ After these restrictions, I am left with 2,336 respondents with

¹⁰Survey respondents can indicate more than one of the following sources of coverage: current employer, past employer, current spouse's employer, previous spouse's employer, private insurance, government programs and others. For those respondents who have ESI and insurance from spouse's employer, I consider them to have spouse's employer.

a diagnosis.

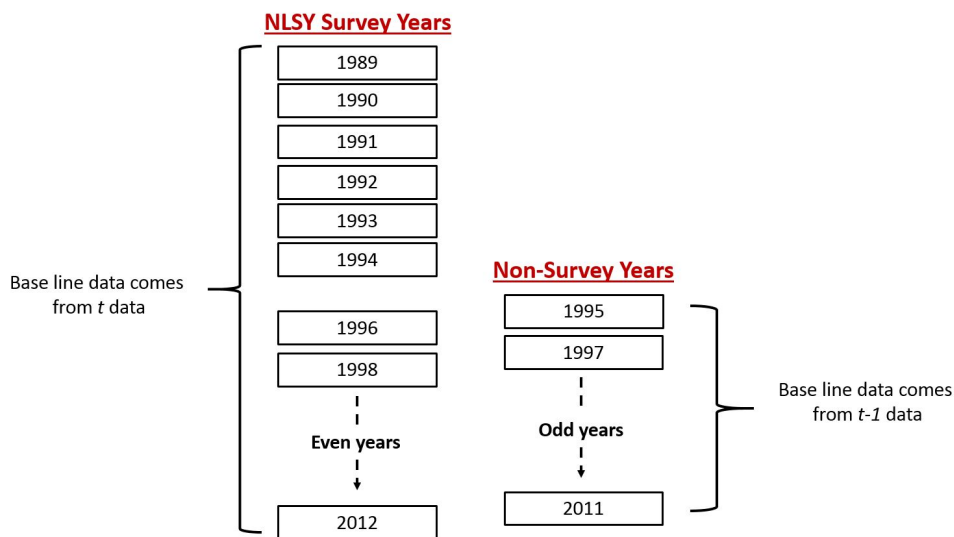
The NLSY provides weekly data from 1979 to 2014 on the respondent’s employment status and hours worked. I collapse the weekly data on hours worked into monthly data. From this data, I am able to derive the outcome variables, CH_{it} , E_{it} and $FT2PT_{it}$. CH_{it} is the growth rate of hours worked from baseline to t months after diagnosis. The baseline period is 6 months before diagnosis. As mentioned earlier, I restrict the sample to individuals who are fully employed 6 months before diagnosis with full employment defined as working at least 32 hours per week. The respondent is considered employed for the month (i.e. E_{it} is equal to 1) if he or she was employed at least a week of that month. If the respondent was partially employed t months after diagnosis, then $FT2PT_{it}$ is set equal to 1 and 0 otherwise.

The NLSY also provides a rich source of control variables. As mentioned earlier, I control for the year of diagnosis, gender, race, marital status, whether the worker has children, age, education level, AFQT score, location of residence, household income, share of annual income to household income, firm size, job tenure, industry, occupation categories and labor hours supplied in the baseline period. Unfortunately, except for hours worked and the employment status that is provided by the the NLSY on a weekly basis, the rest of the variables needed for this study are given on a yearly basis from 1989 to 1994 and biennially, after that. As proxies for the baseline values of the control variables, I use the values of the controls at the year of diagnosis or one year before diagnosis. For workers who were diagnosed during the time the NLSY was administered, I use information from that survey year.¹¹ For workers who were diagnosed when the NLSY was not administered, I use information from the previous year (See Figure 3.1).

After exclusions for missing data for control variables and key study variables, the “Full” sample includes 1,383 observations. I also construct a “Married Only” restricted to married individuals alone. This alternative sample contains 892 observations.

¹¹I can also use prior year data to get pre-determined baseline values. However, it is not very recent and may not capture most recent insurance status.

Figure 3.1: Source of Baseline Values of Control Variables



Notes: $t=1989, \dots, 2012$ in this diagram. This figure does not pertain to household income and annual income variables.

3.5 Results

3.5.1 Descriptive Statistics

Table 3.1 contains information on the distribution of diagnoses received by respondents in the “Full” and “Married Only” samples. In the “Full” sample, there are 1,384 eligible respondents who had a first diagnosis. 45.4 percent of the sample had been diagnosed with hypertension, 21.9 percent had been diagnosed with arthritis, 16.7 percent had been diagnosed with any mental health condition¹² and 10.5 percent had diabetes diagnosis. A small share (5.5%) had cancer, heart problems or stroke. Most of the health problems in the sample are mild chronic illnesses but may lead to more severe conditions. Since most are mild illnesses, this sample can be used to examine ESI effects on workers with more common chronic illnesses like hypertension, mental illness, and arthritis which were not included in past studies. The distribution of the “Married Only” sample mirrors that of the “Full” sample.

Figure 3.2 also shows the distribution of the year of diagnosis. 97% of the respondents in the sample were diagnosed on or before 2010, the year the ACA was signed by President

¹²Emotional, nervous or psychiatric problems.

Barack Obama into law. Since the ACA was not fully implemented until 2014, it is then safe to assume that the effects estimated are not convoluted by the significant change in health policies during the Obama administration.

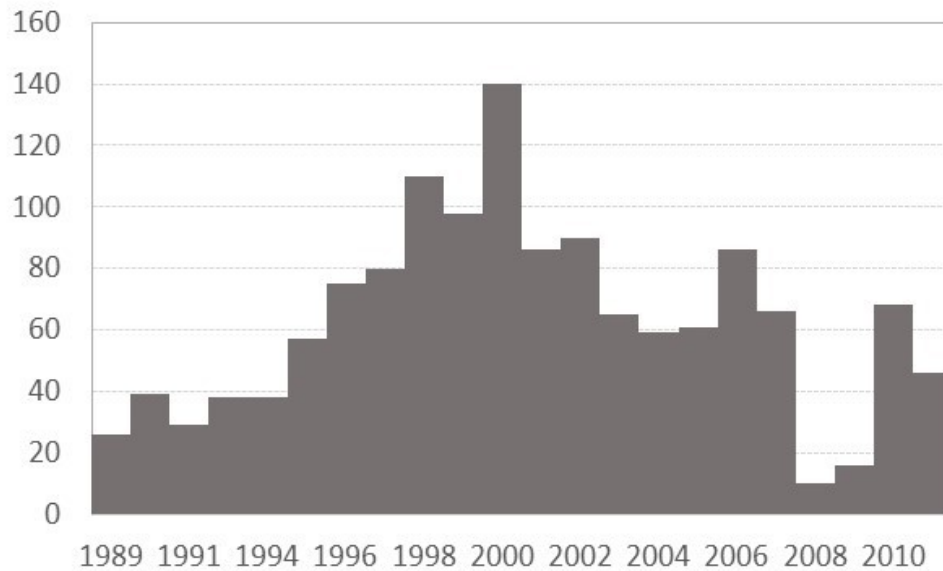
The summary statistics of labor supply indicators are presented in Table 3.2. Column 2 describes the “Full” sample while column 4 describes the “Married Only” sample. Columns 3 and 4 compare respondents with and without ESI for the “Full” sample while columns 6 and 7 do the same for the “Married Only” sample. To be able to make causal inferences from the samples, the group with ESI and the group without ESI should only differ in terms of health insurance and should be similar in terms of other pre-diagnosis unobservable and observable characteristics.

Table 3.1: Sample Distribution of Diagnoses, by Source of Health Insurance

	Full Sample			Married Only Sample		
	Any Ins N=1,383	ESI N=1,117	Spouse ESI N=266	Any Ins N=892	ESI N=633	Spouse ESI N=259
<i>% Share</i>						
Hypertension	45.4	47.8	35.3	45.2	49.1	35.5
Diabetes	10.5	10.4	10.9	11.0	10.9	11.2
Cancer	3.5	3.3	4.5	3.4	3.0	4.2
Mental	16.7	15.1	23.3	16.9	14.4	23.2
Arthritis	21.9	21.2	24.8	21.6	20.4	24.7
Heart Failure	0.3	0.4	0.0	0.1	0.2	0.0
Heart Attack	1.1	1.2	0.8	1.2	1.4	0.8
Stroke	0.6	0.6	0.4	0.6	0.6	0.4
Total	100.0	100.0	100.0	100.0	100.0	100.0

Notes: Any Ins refers to Any Insurance.

Figure 3.2: Number of Diagnoses, by Year



Notes: The sample only includes the first diagnosis of a chronic illness of each eligible respondent.

For the “Full” sample, 81 percent have health insurance from their own employer (i.e. ESI=1), and 19 percent have coverage through their spouse’s employer. For the married sample, 71 percent have health insurance from their own employer, and the other 29 percent are covered through their spouse’s employment-based insurance. This distribution is about the same as the national distribution.

On average, respondents in the “Full” and “Married Only” samples work 46 hours per week 6 months before diagnosis. This number is the same as the reported average number of hours worked per week by full-time employees in a 2014 Gallup Survey (see [Saad \(2014\)](#)). The mean work hours are almost the same for those with ESI and those with insurance from their spouse’s employer. This indicates that workers in both groups have the same labor market supply before diagnosis. This similarity of labor supply at the baseline bodes well with the choice of using these two groups who differ in the source of insurance to detect any differential effect of insurance source.

Table 3.2: Mean Values of Labor Supply Indicators

	Full Sample			Married Only Sample		
	Any Ins N=1,383	ESI N=1,117	Spouse ESI N=266	Any Ins N=892	ESI N=633	Spouse ESI N=259
Has own ESI	0.81	1.00	0.00	0.71	1.00	0.00
Hrs wrked 6 mos bef	45.95	45.98	45.84	46.25	46.35	46.01
Employed=1						
0 mos after diagnosis	0.96	0.97	0.95	0.97	0.97	0.95
6 mos after diagnosis	0.93	0.94	0.88	0.93	0.96	0.88
12 mos after diagnosis	0.92	0.92	0.89	0.93	0.95	0.89
24 mos after diagnosis	0.91	0.91	0.89	0.92	0.93	0.89
% Change in hrs worked						
0 mos after diagnosis	-5.31	-4.10	-10.38	-5.32	-3.25	-10.37
6 mos after diagnosis	-7.14	-5.66	-13.37	-6.74	-4.11	-13.16
12 mos after diagnosis	-8.81	-7.59	-13.93	-7.29	-4.48	-14.17
24 mos after diagnosis	-11.02	-9.81	-16.11	-10.03	-7.45	-16.35
% Change in hrs worked, Conditional						
0 mos after diagnosis	-1.83	-0.91	-5.78	-2.14	-0.58	-6.02
6 mos after diagnosis	-0.29	-0.02	-1.52	-0.25	0.16	-1.35
12 mos after diagnosis	-0.69	-0.07	-3.40	-0.49	0.61	-3.35
24 mos after diagnosis	-1.94	-0.94	-6.24	-1.89	-0.20	-6.21
Changed from FT to PT						
0 mos after diagnosis	0.08	0.06	0.16	0.09	0.06	0.16
6 mos after diagnosis	0.10	0.08	0.18	0.09	0.06	0.17
12 mos after diagnosis	0.12	0.10	0.19	0.11	0.07	0.19
24 mos after diagnosis	0.15	0.13	0.23	0.14	0.10	0.23
Changed from FT to PT, Conditional						
0 mos after diagnosis	0.05	0.03	0.11	0.05	0.03	0.12
6 mos after diagnosis	0.03	0.03	0.06	0.03	0.02	0.06
12 mos after diagnosis	0.04	0.03	0.09	0.04	0.02	0.09
24 mos after diagnosis	0.06	0.04	0.13	0.06	0.03	0.13

Notes: Any Ins refers to Any Insurance. Except for “Hrs wrked 6 mos bef”, “Change in hrs worked” and “Change in hrs worked, Conditional”, the variables are all dummy variables. Conditional means conditional on being employed.

At the time of diagnosis (0 month after diagnosis), about 97 percent of workers with ESI in the “Full” sample remained employed compared to 95 percent for workers with insurance from spouse’s employer. Two years after diagnosis, this proportion decreases to 91 percent for the former and 89 percent for the latter. The difference is slightly greater in the “Married Only” sample where almost all (97%) of the workers with ESI remained employed compared to 95 percent for workers with insurance from spouse’s employer at the time of diagnosis.

Without accounting for employment effects (i.e., workers who are no longer employed during and after diagnosis and hence, supply zero work hours), the mean hours worked for workers in the “Full” sample increasingly declines post-diagnosis. The mean hours worked of workers with ESI is lower by 4.1 percent during the time of diagnosis, lower than the 10.4 percent decline for workers with coverage from spouse’s employer. Mean hours worked drops further 6, 12 and 24 months after diagnosis, reaching 9.8 percent 24 months post-diagnosis for the workers with ESI and 16.1 percent for workers without ESI. The 9.8 percent decline is equivalent to about 5 hours per week relative to the baseline mean labor hours and the 16.1 percent is equivalent to 8 hours relative to the respective baseline value. The difference is more defined in the married only sample.

Conditional on being employed and hence, accounting for employment effects, the decline of mean work hours between workers with ESI and workers with no ESI is now lower at any period (0 to 24 months after diagnosis). At the time of diagnosis, the 0.9 percent decline for workers with ESI is equivalent to only half an hour decline relative to the baseline mean hours worked, and the 5.8 percent for the other group is equivalent to 2 hours relative to the respective baseline value. The decline is more defined 24 months after diagnosis.

Conditional on being employed post-diagnosis, 11 percent of respondents who get coverage from spouse’s employer changed from being fully employed to partially employed, almost 4 times the proportion of respondents with ESI who became part-time workers (3%). The gap in the fraction of people who became partially employed between these two groups does not narrow down even two years post-diagnosis. This pattern in the change of labor hours and the transition from being a full-time to a part-time worker, conditional and unconditional to being employed post-diagnosis, is also observed in the “Married Only” sample.

Without controlling for other factors that may affect labor supply outcomes, the summary statistics indicates that workers’ labor supply tends to be lower for workers with non-ESI than workers with ESI. Figures 3.3 to 3.5 also show that workers with ESI compared to workers with non-ESI tend to remain employed and for those who have remained employed,

Figure 3.3: Share of Employed Workers in Sample

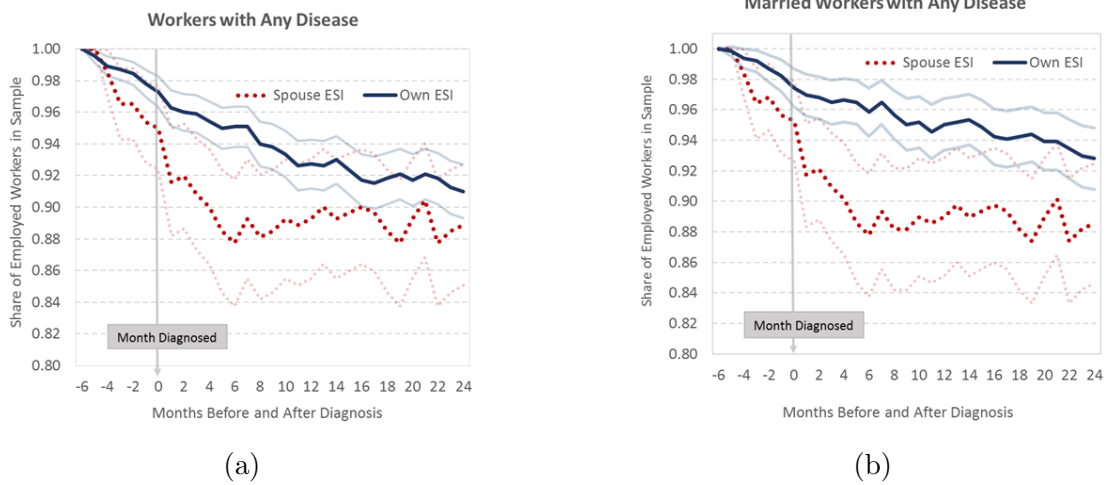


Figure 3.4: Mean Weekly Hrs Worked, Conditional on Being Employed

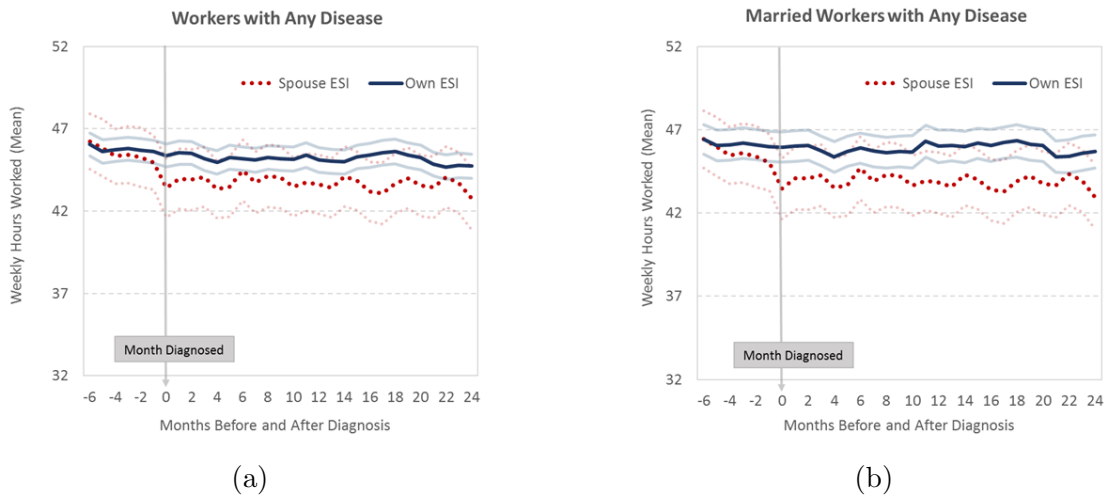
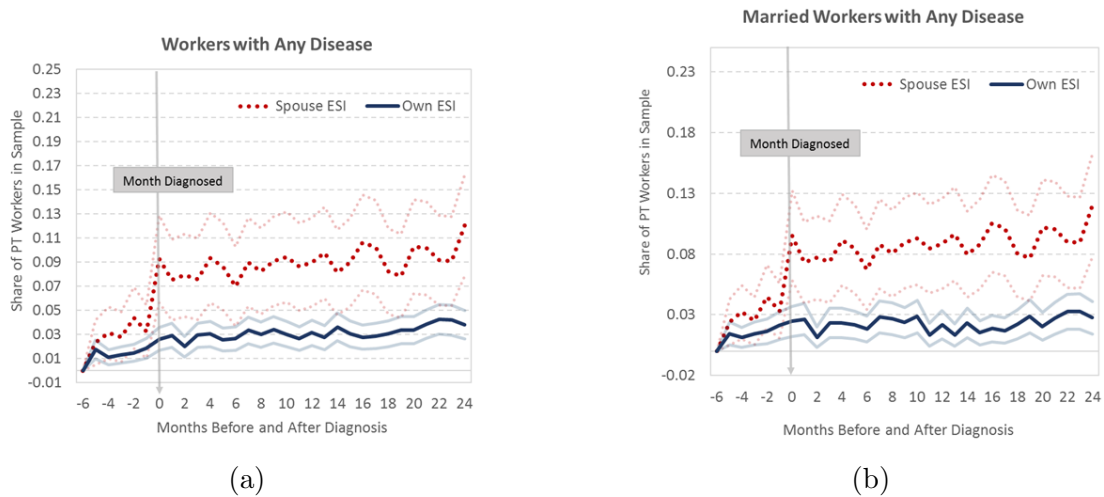


Figure 3.5: Share of PT Workers in Sample, Conditional on Being Employed



Notes: Dark blue solid line refers to the ratio of part-time employees with ESI to the total number of workers with ESI at baseline; area below and above the light blue lines is the 95% confidence interval. Dark red dashed line refers to the ratio of part-time employees with spouse ESI to the total number of workers with spouse ESI at baseline; area below and above the light red dashed lines is the 95% confidence interval.

tend to have lower change in mean hours worked and to be less likely partially employed.

Table 3.3 presents the summary statistics of the control variables. On average, workers in the “Full” and “Married Only” samples received their first diagnosis at the age of 39, with no significant difference in age at diagnosis between workers with ESI and workers with spouse employment-based insurance. While almost half of the full sample are women, the proportion of women in the group of workers with ESI is smaller than the proportion of women in the group of workers with coverage from spouse’s employer. In terms of race, the fraction of black workers in the group with ESI is greater than the proportion in the other group. In the “Full” sample, the proportion of workers who have children is greater for the group with coverage from spouse’s employer. However, in the “Married Only” sample, this proportion is the same. In the full sample, 56 percent of workers with ESI are married, 24 percent were formerly married, and about 20 percent were never married. The other group of workers who depend on their spouse’s employer for coverage, by definition of the group, consist of almost 100 percent married individuals (with about 2 percent formerly married). In terms of educational attainment, the proportion of workers with higher education is slightly higher for the group of workers with ESI compared to workers with non-ESI.

In the “Full” and “Married Only” samples, workers with ESI tend to have longer tenure in their pre-diagnosis job compared to workers with no ESI. 53% respondents with ESI work 6+ years in their pre-diagnosis job, significantly higher than the proportion of respondents without ESI with the same tenure. Also, workers with ESI tend to work in firms that are relatively bigger in terms of the number of employees compared to workers with no ESI. In the full sample, workers with ESI are more likely to have lower pre-diagnosis household annual income relative to workers depending on spouse employer for coverage. However, for the married sample, the distribution of household income between the two groups are very similar. A more striking difference between the group of workers with ESI and the group of workers with no ESI is the share of worker’s income to household income. Income of workers with spouse insurance tends to have a lower share of household income compared to the

share of workers with ESI.

Table 3.3: Summary Statistics of Control Variables

	Full Sample			Married Only Sample		
	Any Ins N=1,383	ESI N=1,117	Spouse ESI N=266	Any Ins N=892	ESI N=633	Spouse ESI N=259
Age	39.28	39.37	38.89	39.24	39.38	38.89
Female=1	0.50	0.46	0.66	0.45	0.37	0.65
Race: Black	0.29	0.30	0.23	0.22	0.22	0.23
Race: Hispanic	0.17	0.17	0.16	0.17	0.18	0.16
Race: White	0.54	0.52	0.61	0.60	0.60	0.61
Any children in HH=1	0.67	0.63	0.83	0.82	0.81	0.84
Never married=1	0.16	0.19	0.01	0.00	0.00	0.00
Formerly married=1	0.20	0.24	0.02	0.00	0.00	0.00
Married==1	0.64	0.57	0.97	1.00	1.00	1.00
Education: <9	0.01	0.01	0.02	0.01	0.01	0.02
Education: 9-12	0.47	0.47	0.50	0.48	0.48	0.49
Education: 13 and over	0.52	0.52	0.49	0.51	0.52	0.49
AFQT: 0-25	0.30	0.30	0.29	0.27	0.26	0.29
AFQT: 25-50	0.28	0.28	0.30	0.27	0.26	0.31
AFQT: 50-75	0.22	0.22	0.21	0.24	0.24	0.21
AFQT: 75-100	0.20	0.21	0.19	0.22	0.23	0.19
Job Tenure: 0-1 years	0.11	0.09	0.19	0.11	0.08	0.19
Job Tenure: 1-3 years	0.20	0.18	0.28	0.20	0.16	0.28
Job Tenure: 3-6 years	0.21	0.20	0.24	0.21	0.20	0.24
Job Tenure: 6+ years	0.49	0.53	0.29	0.48	0.56	0.29
Urban residence=1	0.73	0.74	0.70	0.69	0.69	0.70
Employer size: 0-9	0.14	0.11	0.26	0.17	0.13	0.26
Employer size: 10-24	0.12	0.11	0.17	0.13	0.11	0.16
Employer size: 25-49	0.12	0.11	0.14	0.12	0.11	0.14
Employer size: 50-999	0.47	0.51	0.33	0.45	0.50	0.34
Employer size: 1000+	0.14	0.15	0.10	0.13	0.14	0.10
HH Income: \$0 - \$30K	0.17	0.20	0.05	0.07	0.07	0.05
HH Income: \$30,001 - \$50K	0.27	0.28	0.19	0.21	0.23	0.18
HH Income: \$50,001 - \$100K	0.44	0.41	0.55	0.55	0.54	0.56
HH Income: >\$100K	0.13	0.11	0.21	0.18	0.16	0.21
Annual Inc <50 of HH Inc	0.28	0.20	0.64	0.41	0.31	0.65
Annual Inc >50 of HH Inc	0.72	0.80	0.36	0.59	0.69	0.35

Notes: Any Ins refers to Any Insurance. HH Income and Annual Income are in 2002 dollars.

3.5.2 Regression Results

Probability of Remaining Employed

Panel A of Table 3.4 reports the various estimates of β , the coefficient on ESI in Equation 3.2 when the dependent variable is the probability of remaining employed 0, 6, 12 and 24 months after diagnosis. The specific β for workers diagnosed with diabetes, cancer, heart attack, heart failure and stroke are not estimated because these sub-groups have insufficient

observations.

Table 3.4: Estimated Coefficient of ESI, By Type of Diagnosis

	Full Sample				Married Only Sample			
	0 Mo	6 Mos	12 Mos	24 Mos	0 Mo	6 Mos	12 Mos	24 Mos
<i>Panel A. Dependent Variable: Employment</i>								
(1) All Diagnoses	0.0245 (0.0154) 1,383	0.0553*** (0.0209) 1,383	0.0402* (0.0226) 1,383	0.0149 (0.0239) 1,383	0.0185 (0.0156) 892	0.0513** (0.0215) 892	0.0415* (0.0221) 892	0.0208 (0.0243) 892
(2) Hypertension	0.0259 (0.0234) 628	0.0669** (0.0296) 628	0.0548 (0.0347) 628	0.0640* (0.0368) 628	0.0186 (0.0248) 403	0.0641** (0.0308) 403	0.0680** (0.0343) 403	0.0793** (0.0393) 403
(3) Mental	0.0244 (0.0489) 231	0.0803 (0.0581) 231	0.0244 (0.0599) 231	0.0106 (0.0644) 231	-0.0287 (0.0553) 151	0.0939 (0.0650) 151	0.0373 (0.0659) 151	0.0269 (0.0722) 151
(4) Arthritis	0.0208 (0.0339) 303	0.0293 (0.0478) 303	0.00788 (0.0548) 303	-0.0141 (0.0511) 303	0.0464 (0.0348) 193	0.0654 (0.0478) 193	0.0289 (0.0556) 193	0.0169 (0.0517) 193
<i>Panel B. Dependent Variable: % Change in Hours Worked, Conditional</i>								
(1) All Diagnoses	3.573** (1.603) 1,334	1.273 (1.838) 1,288	2.926 (1.926) 1,270	5.439*** (2.052) 1,255	3.756** (1.730) 863	1.350 (1.812) 834	3.069 (1.999) 831	4.669** (2.137) 818
(2) Hypertension	3.763 (2.473) 609	1.667 (3.092) 597	2.764 (3.063) 583	1.219 (3.146) 577	3.654 (2.706) 391	1.939 (3.021) 384	2.711 (3.294) 379	1.164 (3.285) 372
(3) Mental	3.038 (3.928) 216	0.255 (4.032) 209	-1.487 (4.055) 209	0.386 (5.193) 205	6.289 (4.353) 141	0.952 (3.980) 137	-2.831 (4.153) 137	-1.118 (5.622) 132
(4) Arthritis	7.150** (3.219) 294	1.417 (4.025) 282	5.191 (4.718) 277	15.49*** (5.454) 279	8.045* (4.110) 188	-0.479 (4.890) 181	3.029 (5.463) 179	17.53*** (6.390) 179
<i>Panel C. Dependent Variable: Change from Full-time to Part-time, Conditional</i>								
(1) All Diagnoses	-0.0454** (0.0178) 1,334	-0.0208 (0.0154) 1,288	-0.0286* (0.0173) 1,270	-0.0605*** (0.0204) 1,255	-0.0423** (0.0197) 863	-0.0261* (0.0152) 834	-0.0313* (0.0179) 831	-0.0597*** (0.0219) 818
(2) Hypertension	-0.0469* (0.0260) 609	-0.0241 (0.0239) 597	-0.0433* (0.0246) 583	-0.0489* (0.0274) 577	-0.0373 (0.0285) 391	-0.0305 (0.0229) 384	-0.0523** (0.0251) 379	-0.0393 (0.0281) 372
(3) Mental	-0.0793 (0.0516) 216	-0.0497 (0.0430) 209	-0.0172 (0.0493) 209	0.00840 (0.0673) 205	-0.101* (0.0569) 141	-0.0879** (0.0409) 137	0.00931 (0.0543) 137	0.0229 (0.0801) 132
(4) Arthritis	-0.0769** (0.0371) 294	-0.0236 (0.0329) 282	-0.0408 (0.0375) 277	-0.0872* (0.0466) 279	-0.101** (0.0458) 188	-0.0330 (0.0456) 181	-0.0367 (0.0465) 179	-0.108* (0.0564) 179

Notes: For each item in each panel, the number in first row is the estimated coefficient. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The second row contains the standard errors (in parentheses) while the third row contains the number of observations. Control variables are included in the estimation.

For the “Full” sample, β is positive across almost all sub-groups, consistent with the hy-

pothesis that newly diagnosed workers with ESI are more likely to remain employed to keep ESI relative to workers who are less dependent on their own employer for health insurance. Looking at “Full” sample with all types of diagnoses (Panel A (1)), the estimates imply that workers with ESI are 2.5 percentage points more likely to be employed than workers with health insurance through their spouse’s employer at the time of diagnosis, 5.5 percentage points more likely 6 months after diagnosis before declining to only 1.5 percentage points more likely 24 months after diagnosis. However, the coefficients are only significant 6 and 12 months after diagnosis.

The estimates are lower than the estimates of [Bradley et al. \(2007a\)](#) and close to the estimates of [Bradley et al. \(2013\)](#) for women with breast cancer. In [Bradley et al. \(2007a\)](#), women with ESI were 10 percentage points more likely to be employed at six months following diagnosis, but this estimate was not statistically significant. At 12 months following diagnosis, women with ESI were 13 percentage points more likely to be employed than women without ESI. The difference in employment lock effects compared to [Bradley et al. \(2007a\)](#) is probably caused by the composition of health conditions in the samples used. Most of the health conditions in the sample used in this paper are relatively milder conditions relative to the cancer sample used in [Bradley et al. \(2007a\)](#) and [Bradley et al. \(2013\)](#). Since cancer requires a more intense treatment regimen, workers in the cancer sample are more likely to stop working altogether upon and after diagnosis. Nevertheless, I see significant effects, albeit at a smaller magnitude, for workers diagnosed with milder illnesses.

The effects of having ESI on remaining employed are mostly positive across all types of diagnoses except for married workers with mental conditions (at the time of diagnosis) and for workers with arthritis (24 months after diagnosis). For hypertension, there seems to be a significant effect a few months after diagnosis. While the sign of the coefficients support employment effects of ESI for workers with mental and arthritis, the effects are not significant at the conventional levels.

Change in weekly hours worked

Panel B of Table 3.4 provides various estimates of β which represent the effects of ESI on percent changes in weekly hours worked for workers with health problems 0, 6, 12, and 24 months following diagnosis, conditional on remaining employed during the period. The descriptive statistics in Table 3.2 show that the mean labor hours worked declines during and after diagnosis for workers with ESI and workers without ESI (except at 6 and 12 months after diagnosis in the “Married Only” sample). Hence, a positive β means that the reduction is smaller for workers with ESI relative to workers with no ESI. The coefficients from the conditional model are positive in most samples and sub-samples which means that while workers on average tend to reduce their weekly labor hours, the reduction tends to be lower for workers with ESI than workers with no ESI. This implies that, relative to workers with insurance from spouse’s employer, newly diagnosed workers with ESI are more locked into a certain amount of work hours to keep insurance. For the “Full” and “Married Only” samples, the percentage reduction in mean labor hours worked per week is significantly lower by more than 3.5 percentage points for workers with ESI relative to workers dependent on their spouse’s employer at the time of diagnosis (see Panel B (1)). This coefficient drops 6 months post-diagnosis and starts to increase 12 and 24 months after.

Conditional on remaining employed, the effects of ESI for workers with arthritis diagnosis on changes in labor hours worked are relatively larger (see Panel B (2)). The effects on workers with hypertension are positive but insignificant (see Panel B (3) and (4)). The immediate effects of ESI for workers with mental conditions are positive but it turns negative 12 and 24 months after (for married workers). This means that workers with mental problems and ESI who have remained employed will more likely reduce their hours by more 12 months after diagnosis.

Change from Full Time to Part-time Work

Panel C of Table 3.4 shows the ESI effects of getting diagnosed on the probability of becoming partially employed (i.e. working less than 32 hours per week) conditional on being employed post-diagnosis. A negative coefficient of ESI means that workers with ESI tend to remain fully employed compared to workers with coverage from spouse's employer. The results show that the coefficients estimated for "Full" and "Married Only" samples for workers with all types of diagnosis are negative. The coefficients are significant at conventional levels across all periods (except for "Full" sample 6 months after diagnosis). Since full employment is usually required to be eligible for ESI, this supports the hypothesis that workers with ESI tend to remain fully employed to keep health insurance.

In summary, results from Table 3.4 provides stronger evidence for intensive effects of ESI compared to extensive effects (employment effects) of ESI.

Controlling for Limitation

Table 3.5 presents the estimates for Equation 3.3 using all workers with all types of diagnoses. It shows the effects of having ESI for those with and without limitations on the three labor supply indicators.

For the "Full" sample, workers with ESI and no limitation are 2.1, 5.2, 3.7 and 1.7 percentage points more likely (relative to workers with non-ESI) to remain employed 0, 6, 12 and 24 months after diagnosis, respectively (See Tables 3.5 Panel A (1)). As expected, workers with limitation are less likely to remain employed (Table 3.5 Panel A (2)). The sign of the interactive effects of ESI and limitation is mostly negative and relatively large compared to the the coefficient on ESI. This means that workers with ESI and limitation are less likely to remain employed (Table 3.5 Panel A (3)). The pattern, however, is different for married workers wherein the interactive effects of ESI and limitation is mostly positive and relatively large to the coefficient of ESI. This means that married workers with limitation and ESI are more likely to be employed at least for the first 12 months after diagnosis.

For those workers who remained employed after the diagnosis, workers with ESI and no limitation still tend to exhibit a smaller reduction in labor hours worked (Table 3.5 Panel B (1)). Limitation significantly and negatively affects labor hours (Table 3.5 Panel B (2)). The coefficient of the interaction of ESI and limitation is mostly positive and large relative to the coefficient of ESI. This implies that workers with ESI and limitation even tend to have a smaller reduction in labor hours relative to workers with ESI and no limitation. (Table 3.5 Panel B(3)).

Table 3.5: Estimated Coefficient of ESI With Work Limitation Control

	Full Sample				Married Only Sample			
	0 Mo	6 Mos	12 Mos	24 Mos	0 Mo	6 Mos	12 Mos	24 Mos
<i>Panel A. Dependent Variable: Employment</i>								
(1) ESI	0.0216 (0.0158)	0.0518** (0.0209)	0.0369 (0.0231)	0.0174 (0.0244)	0.0164 (0.0160)	0.0417* (0.0213)	0.0303 (0.0226)	0.0168 (0.0247)
(2) Limitation	-0.149** (0.0591)	-0.00472 (0.0863)	-0.0225 (0.0909)	-0.00369 (0.0569)	-0.156*** (0.0756)	-0.0120 (0.0801)	-0.0268 (0.0875)	-0.0115 (0.0833)
(3) ESI*Limitation	0.0899 (0.0677)	-0.0982 (0.0892)	-0.0890 (0.0987)	-0.120 (0.104)	0.0349 (0.0739)	0.0674 (0.0982)	0.0307 (0.104)	-0.0409 (0.114)
N	1,300	1,300	1,300	1,300	834	834	834	834
<i>Panel B. Dependent Variable: % Change in Hours Worked, Conditional</i>								
(1) ESI	3.658** (1.666)	1.265 (1.919)	3.734* (2.025)	5.598*** (2.142)	3.882** (1.804)	1.028 (1.894)	3.873* (2.118)	4.581** (2.206)
(2) Limitation	-23.83*** (6.822)	-16.44** (7.271)	-7.428 (7.680)	-1.878 (8.069)	-23.98*** (7.071)	-17.22** (6.885)	-9.209 (7.666)	-4.152 (7.938)
(3) ESI*Limitation	17.80** (7.688)	18.02** (8.401)	2.998 (8.925)	-1.175 (9.434)	21.02** (9.077)	22.17** (8.758)	2.925 (9.905)	0.149 (10.45)
N	1,256	1,220	1,199	1,188	808	786	780	770
<i>Panel A. Dependent Variable: Change from Full-time to Part-time Employment, Conditional</i>								
(1) ESI	-0.0458*** (0.0173)	-0.0144 (0.0157)	-0.0319* (0.0177)	-0.0583*** (0.0210)	-0.0441** (0.0191)	-0.0162 (0.0154)	-0.0354* (0.0188)	-0.0592*** (0.0223)
(2) Limitation	0.400*** (0.0710)	0.288*** (0.0594)	0.151** (0.0672)	0.00173 (0.0791)	0.415*** (0.0750)	0.295*** (0.0558)	0.161** (0.0680)	0.000739 (0.0803)
(3) ESI*Limitation	-0.334*** (0.0800)	-0.279*** (0.0687)	-0.116 (0.0781)	0.0341 (0.0925)	-0.376*** (0.0962)	-0.340*** (0.0710)	-0.0580 (0.0878)	0.114 (0.106)
N	1,256	1,220	1,199	1,188	808	786	780	770

Notes: For each item in each panel, the number in first row is the estimated coefficient of ESI. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The second row contains the standard errors (in parentheses). Control variables are included in the estimation.

The effects of ESI for workers with no limitation on the likelihood of becoming partially-

employed are all negative, implying that workers with ESI tend to become partially employed during and after diagnosis (Table 3.5 Panel C (1)). This is significant 0, 12 and 24 months after diagnosis. Again, the effects of having limitation are opposite the effects of ESI and strongly significant 0, 6 and 12 months post-diagnosis in both samples. For workers who have remained employed and have a limitation, having ESI seems to constrain them from becoming partially employed since the interactive effects of ESI and limitation are negative and significant (except for the 24th month after diagnosis).

Controlling for Intensity of Dependence on ESI

Table 3.6 shows the estimates for Equation 3.4 which tests whether newly diagnosed workers whose employers also cover insurance of their spouse and/or child are more locked into their jobs than workers who have ESI but no dependents. The estimated coefficients on ESI for those with or without dependents are all positive when the dependent variables are the probability of remaining employed and change in mean work hours while the estimated coefficients are mostly negative when the dependent variable is the probability of becoming a part-time worker.

The signs of the coefficients in Table 3.6 support the findings discussed earlier that having ESI incentivizes workers – with or without dependents– to remain employed to keep health insurance. Using an F-test, I test the null hypothesis that there is no statistical difference between the estimated coefficient for workers with no dependents and the estimated coefficient for workers with dependents. The p-values for almost all of the F-tests in Table 3.6 is greater than 10%. Thus, I do not reject the null hypothesis. This means that there is no difference in “job/hour lock” effects of ESI between ill workers with dependents and ill workers with no dependents.

Table 3.6: Estimated Coefficient of Indicators of ESI Dependency Level

	All				Married			
	0 Mo	6 Mos	12 Mos	24 Mos	0 Mo	6 Mos	12 Mos	24 Mos
<i>A. Dependent Variable: Employment</i>								
(1) Only Worker Depends ESI	0.0185 (0.0186)	0.0299 (0.0253)	0.0464* (0.0272)	0.0161 (0.0289)	0.0201 (0.0202)	0.0261 (0.0278)	0.0405 (0.0286)	0.0218 (0.0314)
(2) Spouse/Child also depend on ESI	0.0222 (0.0162)	0.0448** (0.0220)	0.0408* (0.0237)	0.0275 (0.0251)	0.0199 (0.0165)	0.0456** (0.0227)	0.0407* (0.0233)	0.0310 (0.0257)
N	1,383	1,383	1,383	1,383	892	892	892	892
Ho: (1) = (2)								
F-test p-value	0.32	0.24	0.09	0.58	0.32	0.35	0.16	0.49
<i>B. Dependent Variable: % Change in Hours Worked, Conditional</i>								
(1) Only Worker Depends ESI	2.731 (1.924)	1.701 (2.218)	1.587 (2.326)	4.084 (2.489)	0.869 (2.231)	1.277 (2.360)	0.934 (2.580)	0.765 (2.773)
(2) Spouse/Child also depend on ESI	2.283 (1.682)	0.776 (1.920)	3.135 (2.023)	4.196* (2.161)	2.966 (1.832)	0.867 (1.907)	3.299 (2.108)	3.704 (2.258)
N	1,334	1,288	1,270	1,255	863	834	831	818
Ho: (1) = (2)								
F-test p-value	0.16	0.44	0.50	0.10	0.70	0.59	0.72	0.78
<i>C. Dependent Variable: Change from Full-time to Part-time, Conditional</i>								
(1) Only Worker Depends ESI	-0.0472** (1.924)	-0.0207 (2.218)	-0.0254 (2.326)	-0.0182 (2.489)	-0.0331 (2.231)	-0.0260 (2.360)	-0.0316 (2.580)	0.00324 (2.773)
(2) Spouse/Child also depend on ESI	-0.0360* (1.682)	-0.0153 (1.920)	-0.0283 (2.023)	-0.0478** (2.161)	-0.0349* (1.832)	-0.0192 (1.907)	-0.0272 (2.108)	-0.0495** (2.258)
N	1,334	1,288	1,270	1,255	863	834	831	818
Ho: (1) = (2)								
F-test p-value	0.03	0.27	0.22	0.46	0.19	0.19	0.17	0.91

Notes: For each item in each panel, the number in first row is the estimated coefficient of ESI. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The second row contains the standard errors (in parentheses). Other control variables are included in the estimation.

3.6 Conclusion

Despite the existence of legislative measures that were intended to protect workers from the dangers of losing ESI, this paper finds evidence that the current system wherein most of the workers (and their family members) are dependent on their employers for their health coverage creates incentives for ill workers to remain fully employed and to reduce their hours by less relative to workers who get their health insurance through their spouse's employer. The evidence is weaker for employment effects of ESI compared to the evidence for intensive effects of ESI. This paper covers mostly mild illnesses (unlike in past research that focuses

on severe illnesses) and thus, results may imply that severe cases tend to have even more severe consequences.

The main advantage of this study over previous work is that it is not confined to a single state and single illness which does not limit generalizability. However, there are also limitations to this study. Since the source of health insurance is not randomly assigned, dissimilarities between workers with ESI and coverage from spouse’s employer may still occur, and I cannot completely rule out the possibility of having biased estimates. Possible sources of bias comes from endogeneity of ESI. Workers who have more labor market attachment tend to anticipate chronic illness and may self-select into having ESI prior to diagnosis. I assumed that the baseline values of ESI, marital status and other control variables do not change 0, 6, 12 and 24 months after diagnosis. This may not be necessarily true shown in Table 3.7 which tabulates insurance source and marital status of workers in the sample in the survey years before and during/after diagnosis. Differences in jobs with ESI and jobs with non-ESI that are not insurance-related may also confound results. Jobs with ESI may be more desirable and have better job quality that workers with ESI tend to be able to work more after diagnosis.

Table 3.7: Percent Share Before and After Diagnosis, by Insurance Status and Marital Status

Yr of Diagnosis	Before Diagnosis		
	ESI	Spouse ESI	Missing Info
ESI	12.0	5.3	2.0
Spouse ESI	9.8	61.0	10.0
	Never Married	Married	Others
Never Married	15.7	0.0	0.1
Married	0.9	60.3	3.3
Others	0.1	2.7	16.9

As the debate on the US health insurance system continues to be on center stage, research on the effects of ESI is timely and can shed light on the advantages and disadvantages of the current system. “Job lock” and/or “hours lock” may be disadvantageous if it prevents employees from doing the necessary adjustments in response to their health conditions. In the long run, workers who became locked up may be less productive relative to workers

who were not locked up. This potential loss in productivity may be more costly from the perspective of the employer and for society, as a whole.

Areas for future study include verifying the long-run existence of job lock as well as long-run effects of job lock on the health of workers. Since the ACA prohibits premium rating based on pre-existing conditions in the individual markets, it is also worth investigating how the ACA changes the effect of ESI on sick workers' willingness to remain employed sufficiently.

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Appendix A

Sample Résumés & Corresponding Cover Letters for Essay 1

Treatment Group 1: Newly Unemployed

September 22, 2016

Company W
Brooklyn, NY

Dear Sir/Madam:

I am writing in regards to the open Sales Associate position that your company recently advertised online.

I am an experienced job applicant who is adept in dealing with customers and selling products. I am a strong team player who is able to work in any diverse & fast-paced commercially driven environment.

I resigned from my last job because our family had to move here from Seattle.

I would love to have an opportunity to be associated with your company. Please find attached my current resume for your careful consideration.

I look forward to hearing from you at your earliest convenience.

Yours sincerely,

Ryan Miller
2348 Midland Ave
Staten Island, NY 10306
Email: millerryan238@gmail.com
Telephone: 646.766.9116

Treatment Group 1: Newly Unemployed

Ryan Miller

Address line 1
Staten Island, NY 10306

Telephone: 646.766.9116
Email: millerryan238@gmail.com

OBJECTIVE

Seeking a position where my sales skills and experience can be use to contribute to a company that provides opportunities for professional advancement

SKILLS

I have great customer service skills, computer skills (including cash register operation), and soft skills such as teamwork and communication skills. I am also a quick learner.

EDUCATION

High School 3, Seattle, WA, 2010

EXPERIENCE:

Company R, Seattle, WA

September 2012 - September 2016

Retail Sales Associate

- Assessed customer needs and concerns and offered product solutions
- Provided accurate processing for all customer transaction
- Maintained selling floor presentations, and restocked them as needed
- Handled all returns courteously and professionally

Company P, Seattle, WA

September 2010 - September 2012

Service Clerk

- Greeted and assisted guests in finding appropriate departments, aisles, services and products
- Organized merchandise products and count store inventory
- Installed and maintained store displays and signage to match company standards and accurately reflect weekly sales

COMMUNITY WORK

I have volunteered for the Watch the Wild program.

Treatment Group 2: Illness-related Gap

September 21, 2016

To whom it may concern:

I am writing in response to your advertisement for the position of Sales Associate, and would like to submit my resume for the position. I believe I will be able to contribute to the success of your company. I have 6 years of work experience. I was most recently associated with Company X in Brooklyn, NY, where I gained important lessons and skills in achieving sales target and providing high-quality customer service.

I stopped working because I had a medical issue. It is now taken care of and I am ready to get back to work.

I have attached my resume so that you can see my professional skills and qualifications in greater detail. I hope you will grant me an opportunity to meet you in person to discuss my application further. I am looking forward to hearing from you.

Sincerely,

Joshua Smith

Treatment Group 2: Illness-related Gap

Joshua Smith

Address Line 1
Queens, NY 11428
Email: smith.joshua.work@gmail.com
Cell: 347-851-8963

OBJECTIVE

To work as a sales associate in an environment that allow for professional growth opportunities

WORK EXPERIENCE

Sales Specialist

Company X

Brooklyn, NY

December 2012 - December 2015

- Engaged with customers to quickly identify and meet their needs
- Marketed new sales and promotions
- Assisted with store inventory, merchandising, and display organization
- Opened and closed cash registers, tallied daily totals, and processed money deposits

Customer Service/Sales Associate

Company Y

Brooklyn, NY

December 2009 - December 2012

- Used POS cash registers to complete transactions and process returns
- Helped customers find merchandise within the store
- Helped customers with the in-store kiosk and in placing orders from the Staples website
- Ensured that the assigned section were neat and tidy for the following day

CORE COMPETENCIES

Excellent customer services skills; Proficient in Data Entry; Proficient in Microsoft Word, Excel and PowerPoint; POS system

EDUCATION

High School 1, New York, NY, 2009

INTEREST

Organizer/Member, Cancer survivors' group

Treatment Group 3: Unexplained Gap

September 20, 2016

Human Resource Staff
Company W
Brooklyn, NY

Dear Sir/Madam:

I would like to apply for the position of Sales Associate at Company W.

As a Sales Associate with Company S, I gained extensive experience in sales/customer service. I also enjoy helping and interacting with customers which has helped me succeed in my job. My personality and qualifications make me a suitable candidate for the position.

Please contact me at your earliest convenience to discuss how I may fit in at your company. I look forward to hearing from you and thank you for your time.

Warmly,

Andrew Johnson

Treatment Group 3: Unexplained Gap

Name	Andrew Johnson
Address	Address line 1 Brooklyn, NY 11224
Telephone number	(585) 209-5129
E-mail	andrewethanjohnson@gmail.com

Objective	To obtain a sales associate position within an established company that can offer an opportunity for career advancement
Education	High School 2, New York, NY, 2009
Experiences	<p>Sales Associate</p> <p>Company S, Brooklyn, NY, January 2013 - January 2015</p> <p>Responsibilities: Understood shoppers' needs and provided options and advice on meeting those needs; Maintained knowledge of current sales, promotions, policies regarding payment and exchanges as well as security practices; Conducted sales transaction using the POS system; Cleaned and organized the store, including the checkout desk and displays</p> <p>Server</p> <p>Company T, Brooklyn, NY, January 2009 - January 2013</p> <p>Responsibilities: Ensured that every guest felt important and welcome; Presented the menu, answered questions, and made suggestions regarding food and beverage and took orders; Followed all cash handling policies and procedures; Pre-bused tables, maintained table cleanliness, and bused tables</p>
Professional Skills	Strong customer service skills. Cash handling. Proficient in Internet Explorer and Microsoft Office Word/Excel. Team player. Strong interpersonal skills. Easily manage multiple priorities/tasks
Other Activities	Drawing, Running and Photography

Appendix B

Sample Résumés for Essay 2

Control Group: No Certificate

Michael Baker

Address 1
Address 2
Email: michaelbaker@gmail.com
Cell: 111-222-3333

OBJECTIVE

To contribute managerial skills and experience to your firm in a management capacity

WORK EXPERIENCE

Store Manager

X Company

Los Angeles, CA

February 2015 - present

- Responsible for development of team to accomplish store's business objectives through recruitment, selection, coaching, investment, engagement, retention, and motivation.
- Directed merchandise presentation, restocking, and recovery to maximize productivity.
- Performed opening and closing procedures including maintaining registers, preparing bank deposits, preparing store for next day's business and accepting and receiving merchandise shipments.

SKILLS

Computer proficient in Microsoft Office: Word, Excel, PowerPoint, Access; Adobe InDesign

EDUCATION

BS Chemistry, University of _____, 2015

LEADERSHIP

- Student Diplomat, Office of Admission
 - Served as student representative for prospective students and their families.
- Elected to positions to generate interest in multicultural organizations.
- Mentored at-risk junior-high students to help them improve their grades.
- Helped organize and coordinated events during the International Week

Treatment Group: With Certificate

Robert Nelson

Address 1
City, State Zipcode

Telephone: 444-555-6666
Email: robertnelson@gmail.com

OBJECTIVE

Seeking a managerial position

SKILLS

Microsoft Word, PowerPoint, Excel, SPSS

TRAINING AND CERTIFICATION

Online Business Administration Certificate, University of _____, Expected May 2016

EDUCATION

BA Psychology, _____ University, 2014

EXPERIENCE:

Sales Consultant/Sales manager

Y Company

Los Angeles, CA

Aug 2014 - present

- Analyzed applicant's financial status, credit, and property evaluation to determine feasibility of granting loans.
- Adhered to all federal and state compliance guidelines relative to mortgage lending.
- Executed the loan origination process including appraisals, credit reports, ordering title, and closing procedures.

INTERNSHIP

- Special Events Intern, Company Z
 - Coordinated receptions and business meetings.
 - Helped design and distribute monthly employee newsletters.

EXTRACURRICULAR ACTIVITIES

- Developed and implemented class programs and spearheaded efforts to raise funds for nonprofit organizations.
- Generated funds for events and promoted alcohol-abuse awareness campus-wide.
- Volunteered with the local Boys and Girls Club.