Essays in Health and Labor Economics

by

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Abstract

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In the United States, health insurance is often necessary for access to regular, affordable health care. With only eight of every hundred Americans buying private insurance plans on the individual market, the main sources for health insurance traditionally have been employers and the government. As new laws are being debated and introduced to reform an expensive health care industry in which nearly one-sixth of the population is uninsured, research is needed in order to evaluate the costs and benefits of these policy changes and to predict their success. To this end, in addition to understanding how likely individuals are to adopt new health insurance policies, we also should be interested in knowing how the demand for health insurance and changes in its accessibility will affect non-medical decisions. Specifically, labor market choices have been theorized to be directly related to decisions involving insurance coverage. If the availability of health insurance distorts a workers' job-related decisions, then the changing the landscape for how to access insurance may reverberate in employment outcomes.

My dissertation focuses on understanding the factors that influence the demand for health insurance and the role that health insurance plays in an individual's decision to work, where to work, and how much to work. Specifically, I focus on the following three related questions: how does the demand for insurance affect labor market decisions such as when to exit unemployment? what drives insurance demand, and in particular, what motivators work best to increase demand for health coverage among the uninsured? and lastly, what are the supply-side employment responses to the provision of free or reduced-cost public health insurance?

My first chapter explores how the demand for health insurance can change re-employment decisions among the unemployed, as well as the speed at which individuals return to work. Past research on this issue focuses on job-to-job switches and "job lock" but has yet to focus on individuals looking for work. This chapter uses data on laid-off individuals from the Medical Expenditure Panel Survey to compare the job search behavior and outcomes of individuals who differ in their demand for health insurance. I use three proxies for demand, based on spousal health and past insurance offer take-up decisions. Although each is poten-

tially confounded by unobserved determinants of job search, I use a difference-in-differences and propensity score designs to isolate plausibly causal effects. I find consistent patterns across all three proxies (despite different potential omitted variables biases). Overall unemployment durations do not vary with demand for insurance, but this masks variation in the types of jobs taken. Individuals with higher demand for insurance have higher hazards for exiting unemployment into a job with insurance, but lower hazards for exiting to a job without insurance. This points to effects of insurance demand on both search effort and reservation wages, and to potentially important distorting effects of employer-linked health insurance.

Whereas the first chapter takes variation in demand for insurance as a given, my second chapter digs deeper into the basis for this variation and whether it can be affected. In this chapter, I investigate the reasons the uninsured choose to forego insurance coverage and the impact of different messages on their insurance demand. Working with Enroll America, a large non-profit dedicated to decreasing the number of uninsured Americans, I conducted a stratified experiment to determine the best communication strategies to encourage participation in the healthcare exchanges.[1] We test a combination of the following behavioral and information treatments: a risk treatment that emphasizes the average financial risk for someone without health insurance; a norms treatment that alerts our participants that staying uninsured will be against the law; a savings treatment that highlights the average savings available at the exchanges; a wording treatment where we refer to the Affordable Care Act (ACA) as "Obamacare"; and lastly, a cost-calculator treatment that allows individuals to explore the likely cost of insurance based on their own characteristics. Among the uninsured, we find that the cost-calculator treatment, the risk treatment, and the mandate are most effective in increasing intention to purchase insurance. The cost-calculator and the risk treatment increase informedness among this population, but the cost-calculator (when paired with the savings treatment) is the only treatment that increases willingness to pay for insurance. We use the information on willingness to pay to construct sub-group price elasticities of demand to compare to previous work interested in the demand for health insurance. Overall, the results of this chapter highlight the importance of informational campaigns to increase awareness of the costs and benefits of health coverage, particularly after large changes such as those implemented by the ACA.

My third chapter continues by looking at the changes that have been introduced as a result of the ACA. Specifically, it explores whether expanding access to government-provided insurance affects individuals' decisions regarding employment and overall hours of work. Recent findings have suggested that increasing access to health insurance outside of employment has a sizable, negative impact on labor force participation. Along these lines, the Congressional Budget Office predicted that the expansion of Medicaid and private health insurance will cause a 1.5 to 2% reduction in hours worked in the first ten years. Comparing states by whether they chose to expand Medicaid under reforms introduced by the ACA, I look at changes in the probability a childless adult receives Medicaid, as well as changes in

[1] Berkeley IRB determined that IRB approval was not necessary for the analysis of this data.

this group's employment likelihood and hours of work. Using household survey data from the CPS monthly survey and ASEC Supplement, I confirm a marked increase in the percent of childless adults insured by Medicaid but find no statistically significant changes in employment outcomes. I compare these results to other estimates of "employment lock" in recent literature. These results, though imprecise, align with the findings in Chapter 1 which suggest that overall employment is not drastically affected by insurance demand.

For my family, who made this all possible.

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Hooray!!

Chapter 1

Health Insurance Demand, Unemployment, and Re-Employment Job Characteristics

1.1 Introduction

Unique among industrialized countries, the U.S. organizes its health insurance system around employers. Although people can also use the private market to purchase individual insurance policies, these are generally costlier, of worse quality, and more difficult to access. Moreover, before the passage of the Patient Protection and Affordable Care Act (PPACA or ACA) in 2010, individuals with negative medical histories could be denied coverage. Thus, an individual who does not qualify for government assistance could be left with weak or no outside alternatives to accessing expensive and often cost-prohibitive medical services. As a consequence of this absence of quality alternatives, over half of all Americans, and 90% of those not receiving government coverage, receive insurance coverage through an employer.

The dependence of access to health insurance coverage on employment has the potential to distort the labor market. One commonly recognized possibility is that individuals without alternate coverage options may be "locked" into a job by their need for health insurance, making them unwilling to leave for other opportunities. Madrian (1994), Gruber and Madrian (1994), and Stroup, Kinney, and Kneiser (2000) find evidence that this "job lock" is quantitatively important. Another effect of this distortion may be on decisions on labor force participation (Dague et al., 2014; Garthwaite et al., 2013; Heydari et al., 2014; Chou and Staiger, 2001).

This paper focuses on a previously unstudied type of distortion. The fact that not all jobs, or even all full-time jobs, extend insurance to employees may distort the search and matching process for those who have decided to engage in search. An unemployed job seeker might search harder if motivated by a need to obtain health insurance coverage, but might also decline an otherwise attractive job offer that lacks insurance. Little is known about either effect and how each affects unemployment duration and the efficiency of matches in the job-market.

The impact of health insurance demand on the likelihood of exiting unemployment is not obvious ex ante. Current or anticipated demand for health insurance from a job may drive unemployed individuals to search harder for a job because it is riskier or costlier for them to remain unemployed. All things equal, this should raise the job-offer arrival rate and speed of reemployment. Moreover, higher demand for insurance should reduce job seekers' reservation wages for job offers that come with insurance, again reducing the time to reemployment. But these effects might be offset by higher reservation wages for jobs without insurance. Thus, the effect of insurance demand on overall unemployment duration is ambiguous.

In this paper, I use data on the job-match outcomes of unemployed workers to measure the presence and magnitude of these distortions to the search and matching process. Specifically, I look at how increased demand for health insurance among the laid-off changes a) the type of jobs that people take and b) the time to re-employment. The extent to which search and unemployment duration may be influenced by fringe benefits, such as health insurance, has not been explored previously. I find that the overall exit rate from unemployment does not vary with demand for insurance, but there is variation by insurance demand in the rate at which individuals leave for specific types of jobs. Individuals with higher demand for insurance have higher hazards for exiting unemployment into a job with insurance, but lower hazards for exiting to a job without insurance. These exit patterns signal an effect of insurance demand on the search behavior and reservation wages among the displaced.

I formalize the displaced worker's problem in a simple search model. An unemployed individual chooses how much to search and sets a reservation wage for any offers that come in. I modify standard models by introducing heterogeneity in jobs (some offer insurance, some do not) and in demand for insurance, modeled as a flow disutility of being uninsured. In this case, job searchers set two reservation wages: one for jobs with insurance and a higher one for jobs without.

This model yields a number of testable predictions regarding the relationship between taste for health insurance and search outcomes. As sketched above, individuals with higher demand for insurance will search harder than those with lower demand. They have lower reservation wages for jobs with insurance, and thus higher hazards for exiting to such a job. But they have higher reservation wages for jobs that do not offer insurance. Due to the offsetting effects of higher search yet higher reservation wages, the effect of demand for insurance on the hazard for exiting to such a job is ambiguous, as is the effect on the overall hazard for exiting unemployment.

The bulk of the paper attempts to assess these predictions empirically. The primary challenge in such an assessment is to isolate useful variation in demand for health insurance among comparable individuals that is not confounded by other characteristics (e.g., skill) that themselves affect search outcomes. I propose three proxies for differences in health insurance demand, and three corresponding strategies for controlling for the effects of demand from any other characteristics that may be correlated with demand. My proxies take advantage of thorough health and coverage data available in the Medical Expenditure Panel Survey (MEPS) from 1996 to 2012 [1]. By including detailed information about a person's last job, the MEPS allows me to richly control for a person's previous employment outcomes.

[1]This range excludes the majority of reforms imposed under the ACA

My primary analysis uses spousal health risk as a driver of insurance demand. All else equal, people whose spouses are sicker should have higher demand for health insurance, but only if the spouse does not have his or her own coverage. This motivates a difference-in-differences-like design in which I difference across both spousal health status and spousal access to coverage, thereby controlling for the effects of spousal health risk and coverage on unemployment. The identifying assumption is that spousal health affects the unemployed worker's employability similarly when the spouse does and doesn't have her own coverage.

My second and third approaches are based on previous insurance offer take-up decisions. The second approach proxies for the unemployed worker's demand for insurance on the job with a couple's joint decision for which partner's insurance policy to accept. Restricting my attention to married unemployed workers who were offered insurance in their past job, I argue that a decision to decline a spousal insurance offer and retain an own-offer - as opposed to decline the own-offer and take up the spouse's - likely reflected a decision to rely on the focal worker for coverage, and thus, implies an increased preference for a job that offers such coverage. My third approach identifies high-demand individuals as those who accepted a past employer's insurance offer (as distinct from the low-demand group, who declined such an offer).

In these secondary analyses, I control for an extensive set of observables available in the MEPS to absorb any association between the past take-up decisions (which might in part reflect the copay or other unobserved characteristics of the prior insurance offer) and workers' employment options. The identifying assumption is that conditional on these observables the decision to take up or reject an offer is associated with future job search outcomes only through its association with worker preferences for health insurance. Furthermore, by using a propensity score estimator that trims observations that do not match observably similar cases in the comparison group, I ensure that the identified effects are not the result of comparing dissimilar individuals.

The three research designs, each of which draws on rich information about previous employment of the now-displaced individuals that is available in my data, allow for plausibly unbiased estimates of the effects of individual preferences on job search outcomes. Moreover, any remaining selection bias likely has different likely effects on the different comparisons, so consistency of results across the three comparisons suggests that this bias is minimal.

My empirical analyses begin with an investigation of the likelihood of having insurance in the post-unemployment job. In all three strategies, I find that higher-demand individuals are more likely to have jobs that offer insurance. In two of the three approaches, however, I find that demand is uncorrelated with the duration of the unemployment spell. To understand this, I implement a competing risks model that treats exit to a job with insurance and exit to a job without insurance as distinct ways for an unemployment spell to end. I find that higher demand is associated with a higher hazard for the first type of exit and a lower hazard for the second type. This result is hard to reconcile with selection bias but is entirely consistent with my search model in which individuals with higher demand search harder, lower their reservation wages for jobs with insurance, and increase their reservation wages for jobs without insurance. The tradeoff between increased search and decreasing reservation wages for insurance-offering jobs is counterbalanced by increasing reservation wages for jobs without insurance offers to yield no overall effects on unemployment duration.

The issues explored here have important implications for understanding the impact of the American health insurance system on labor supply and on unemployment. It has long been recognized that the American practice of tying health insurance to jobs can reduce job-to-job mobility (Madrian, 1994; Gruber and Madrian, 2002), reduce rates of self-employment (Gumus and Regan, 2014; Fairlie et al., 2011; DeCicca, 2010), and influence labor force participation decisions (Buchmueller and Valletta, 1999; Yelowitz, 1995; Boyle and Lahey, 2010). My analysis adds a new kind of distortion, affecting the re-employment of those who have been displaced [2]. In particular, this implies higher-demand individuals likely expend more search effort, may accept fewer job offers without health insurance, and, likewise, reject fewer job offers with health insurance.

The rest of the paper is organized as follows: after discussing the institutional setting in Section 2, I outline past related research in Section 3. In Section 4, I describe a simple search model with predictions for how higher demand for health insurance will affect unemployment exit hazards and insurance offer probabilities. In Section 5, I describe the data I use and, in Section 6, I describe the approaches I use to isolate the individuals with relatively higher demand for employer-sponsored health insurance. In Section 7, I present the results from the different methods, and in Section 8, I conclude and discuss policy implications.

1.2 Institutional Setting: Employer-Sponsored Health Insurance in the US

In this section, I describe the relevant features of the US health insurance system and its relationship to the labor market. With reasons dating back to World War II[3], health insurance in the US is predominately provided through and administrated by public and private employers. Over 60% of covered individuals in the country are insured through an employer - the rest receive coverage through means-tested or eligibility-restricted government programs such as Medicaid, Medicare, SCHIP, Veteran's coverage, etc (30%) or through privately purchased contracts, also known as small-group plans (8%). Thus, outside of the government options - which are not available to all - nearly 90% of people with private insurance are on plans obtained through an employer.

The high employer-based coverage rates come from the fact that a majority of jobs offer insurance and that these offers often extend coverage to employee dependents. There are a variety of reasons that firms may choose to provide health insurance coverage to an employee. Firstly, similar to other fringe benefits, insurance can be an attractive way to increase compensation and attract workers. This is because there are economies of scale that

[2]The ACA itself will likely affect the degree to which health insurance choices distort labor market outcomes.

[3]Insurance was used as a way around income caps that were in place to encourage participation in war related industries (Buchmueller and Monheit, 2009).

come with purchasing plans on the group market and, particularly for large firms, further savings that come through bargaining power with insurance companies. These factors make it possible for a firm to provide a valued good (insurance) to an employee for less than the employee's valuation of the good (Gruber and Madrian, 2002). Secondly, firms are further encouraged to provide insurance by tax subsidies which often only apply if a firm offers the majority of its workers health insurance. These features contribute to the current landscape in the US for the wide provision of private insurance through firms.

Outside of employers and government programs, individuals can purchase private plans on individual markets. These markets, however, have been plagued by adverse selection, lack of transparency and low information, and the ability to price discriminate highly based on health and demographic characteristics. These factors make private market plans a costly and difficult-to-access outside option to employer-provided plans. Possibly most importantly, before the passage of the ACA, insurers in these private markets could reject applicants based on pre-existing conditions or individual-level risk factors. Thus, unhealthy individuals or people with sick dependents may not have had any outside options to employer-sponsored health insurance (ESHI). Though employers may also desire to not enroll these high-cost cases, many are less likely to do so for the following reasons: firstly, many firms are insuring large pools of employees and are thereby able to pool health risk so that that the average cost per employee does not fluctuate much with a high-risk employee; secondly, some tax benefits and the Health Insurance and Portability and Accountability Act require all employees of a given type within a firm to be offered insurance, thereby reducing the ability to discriminate on the insurance offer. It may still be the case that these employers discriminate on the hiring margin (Kapur, 2004).

Whereas, the vast majority of large employers and a substantial amount of smaller firms offer health insurance plans to their employees, the conditions of these offer vary widely. Similarly, even if a firm offers to heavily subsidize an employee's health insurance cost, this same subsidization may not be extended to the employee's dependents. In 2013, the average employee contribution to an employer-sponsored health insurance policy for a single individual was found to be \$90 a month, compared to \$402 for families, where the full (unsubsidized) monthly cost of these policies (employer and employee contributions) was estimated at \$502 for singles and \$1403 for families.(KFF, 2014). Data from 2009 and 2010 show the average monthly premiums paid by individuals for private plans are significantly higher than employer-sponsored costs: estimates range from \$249-\$301 on average for single policies and \$521-\$592 for family policies (AHIP, 2009; KFF, 2010). Importantly, conditional on price, private plans are often more restrictive (i.e. - cover fewer benefits) and have higher out-of-pocket costs (KFF, 2004).

Dependents on Employer-Sponsored Health Insurance Though employers are not required to offer dependent coverage [4], a vast majority of employer plans make this type of coverage available[5]. Enrollment in a plan is usually constrained to open-enrollment periods

[4] Some states mandate certain types of dependent coverage.

[5] In 2014, 96% of firms with fewer than 200 workers and 99% of larger firms that offered benefits to at

but can occur outside of these periods with the occurrence of a qualifying life event such as marriage/divorce, birth/adoption, and loss of alternate coverage due to events like spousal job loss. The rules for rejecting a dependent are similar to those regarding coverage of employees: a firm cannot reject dependent coverage on an individual-basis but instead must treat all covered employees within an employee-type consistently. Even without employer contributions to the dependent's portion, the marginal increase in an employee's premium is often cheaper, for a given level of quality, than insurance secured on the private market.

Employer-Sponsored Health Insurance Offer and Take-up Rates As discussed above, not all jobs offer insurance to an employee. In Figure 1.1, I plot insurance offer and acceptance rates over time for the employed in the Medical Expenditure Panel Survey (MEPS, discussed in more detail below). From 1996 to 2013, the percent of employees receiving an employee-insurance offer shrunk slightly from 66% to 62%, though acceptance rates have decreased more in the same time period. Importantly, these average rates are related to significant variation in ESHI-offer rates across firms, by firm characteristics, and across individuals. For example, larger and older firms are much more likely to offer insurance to at least some of their employees than smaller, newer firms. The likelihood of being offered insurance varies dramatically, even among full time positions. Table 1.1 illustrates this variation in the MEPS across employer firm size for full- and part-time jobs.

An individual may choose to reject an employer's insurance offer for a variety of reasons, all of which I take as a signal of lower demand for employer-sponsored health insurance. Low-cost sharing on the part of the employer, high-costs of an offered policy, outside coverage, and low valuation for insurance are all contributors to the reasons individuals choose to decline an employer's health insurance. In a Census survey conducted in 2010, over 30% of rejectors cited high costs or no need as a reason for not accepting an insurance offer , whereas 66% claimed to have health insurance through another source (Janicki, 2013).

In the MEPS, rejection rates are around 20% during the time period studied here; though, as mentioned above, this rate varies depending on employee characteristics. For example, firms with younger workers have lower insurance-offer acceptance rates (ex. in firms with a higher proportion of younger workers, 30% of the offers are not accepted). Table 1.1 shows how offer and take-up rates vary by full-time status and key individual characteristics such as age, sex, and education. As may be expected, younger people and less educated people are much less likely to be in a job with an insurance offer and are similarly less likely to take-up insurance offers. Males are less likely to be in jobs with insurance offers but more likely than females to accept a given offer - likely pointing to the fact that men are still more likely to be the primary insurance provider for families.

COBRA Coverage while Unemployed The insurance-demand shifters I explore below assume that unemployed individuals can only receive insurance through a spouse. How-

least some employees extended this coverage to employee spouses and a slightly smaller share (92%) offered coverage to other dependents, such as children (KFF, 2014). Starting in 2010, the ACA required all plans covering dependents to cover children up to the age of 26. However, the ACA does not govern whether or not a plan covers employee dependents overall.

ever, in reality, employees who leave jobs with insurance at firms with 20 or more employees can remain insured under their old plans for up to 18 months under the Consolidated Omnibus Budget Reconciliation Act (COBRA). "Group health coverage for COBRA participants is usually more expensive than health coverage for active employees, since usually the employer pays a part of the premium for active employees while COBRA participants generally pay the entire premium themselves. It is ordinarily less expensive, though, than individual health coverage."[6] Though COBRA provides temporary insurance for those searching for new jobs, it is not heavily used[7]. While COBRA may reduce the demand for health insurance while unemployed, it does not provide indefinite coverage making it more likely that the demand for employer-sponsored insurance play a role in job search and job choice.

1.3 Related Literature

The questions in this paper relate to previous work on whether employer-tied health insurance limits job-market transitions. Referred to as job-lock, this theory supposes that individuals with higher demand for health insurance will be less likely to leave a job with insurance for outside options without coverage (other jobs, self-employment, retirement, or unemployment). If the alternative options do not fully compensate the individual for the lost value of insurance coverage, possible productivity increasing transitions will not occur because of the decrease to individual welfare.

The evidence on job-lock is mixed. A handful of studies have found effects ranging from 10% to 40% reduction in job-mobility due to job-lock (Madrian, 1994; Gruber and Madrian, 1994; Stroupe et al., 2001), though other studies have failed to replicate these results and find insignificant effects on transition rates (Kapur, 1998; Buchmueller and Valletta, 1996; Monheit and Cooper, 1994). As summarized by Gruber and Madrian (2001) in an extensive review of this literature, the approaches try to exploit variation in likely demand for employer-sponsored health insurance among individuals in jobs with coverage. These demand shifters have included: family size, pregnancy, as well as detailed information on a family's chronic conditions[8].

[6] http://www.dol.gov/ebsa/faqs/faq_compliance_cobra.html. Whereas the employer is not required to pay the monthly premium, the plan expense cannot be more than 102 percent of the cost of the plan for individuals still at the firm.

[7] Gruber and Madrian find that COBRA extensions increase the likelihood of having insurance by 2.5 percentage points (Gruber and Madrian, 1997). Low COBRA take-up may be because of the prohibitive cost of paying full monthly premiums and the fact that enrollment in a COBRA plan can be retroactive for up to 60-100 days thereby encouraging individuals to enroll only when they have high confidence they will need insurance, suspect they will be uninsured for a long time, or have already incurred medical expenses they want covered.

[8] Whether unemployment is affected by health insurance demand comes from the same fundamentals as the arguments predicting job-lock and effects on overall labor supply. However, null results for transitions with a job does not imply there will be null results for the unemployed, and vice versa. If the unemployed are not optimizing with respect to insurance provision on their next job, we could still find job-lock if those that "luck" into a job with insurance transition less because of this benefit. On the other hand, if we take the

Other research is concerned with the question of whether health insurance coverage itself affects the decision to work overall or the decision of how many hours to work (Boyle and Lahey, 2010; Dague et al., 2014; Garthwaite et al., 2013; Heydari et al., 2014). These papers focus on the tradeoffs between the disutility of working and the value of insurance. Using a variety of policy changes or program caps, some recent work seems to point to large extensive margin responses to either the provision or elimination of health insurance through government programs. This large employment response has implications for what we may expect to find with respect to how insurance demand affects an individual's likelihood to exit from unemployment. However, other work using present-day reforms has found limited effects of insurance provision on labor supply (Baicker et al., 2014). Given that the elasticity of labor supply is argued to be higher for secondary earners in a household, many researchers have focused on identifying how the provision of health coverage affects extensive and intensive labor supply margins among married women (Boyle and Lahey, 2014; Chou and Staiger, 2001; Buchmueller and Valletta, 1999). Focusing on the responses of married women, these papers estimate that access to coverage through a spouse can change the labor force participation of women by 1.5 to 12 percent amount.

Many researchers have set out to estimate the degree to which wages compensate for on-the-job benefits. In perfectly competitive markets, we would expect jobs to bid up wages so that the jobs with insurance pay lower wages that account for the cost of insurance provision to workers (any firm paying less than this amount would lose workers). Finding this offset in wages in different data has proved difficult. Simon finds a wrong signed compensating differential among the laid off (Simon, 2001). On the other hand, using differences in maternity benefits, Gruber finds a decrease in wages among childbearing-aged women and Kolstad and Kowalski (2012) structurally estimate lower wages for individuals remaining in jobs with insurance after the passage of the Massachusetts healthcare act (Kolstad and Kowalski, 2012). I also look for evidence of compensating differentials in hourly wages and present these results below.

Importantly, the underlying tradeoffs are different among the unemployed and among those considering leaving a job. Whereas workers on the job evaluate the tradeoff between a current and prospective jobs' wages, benefits, and amenities, the unemployed must weigh another day without insurance against job offers with given wage and compensation packages. In the only other study that uses variation in current coverage while unemployed, Gruber and Madrian (1997) use state differences in COBRA limits to look at unemployment duration and job matches. They find that unemployment duration increases as do wages on the next job (Gruber and Madrian, 1997). These results point to increases in productive search but do not touch on whether the types of jobs or unemployment differs for those with higher demand for insurance in the future. Most importantly, Gruber and Madrian do not study the question of how higher demand for health insurance both in the present *and* in the future affect unemployment. In other words, though COBRA extensions alleviated present

null job-lock results as our priors, we may still expect to find effects on unemployment if everyone perfectly sorts into the jobs that satisfy their preferences for insurance.

demand for insurance, because it eventually expires, individuals will still be motivated by their future need for insurance (expected health costs out of unemployment). This demand will likely vary across individuals and I study how it changes unemployment and job choice. Furthermore, I restrict my attention to individuals who have been laid-off from their jobs so as to reduce concern that any effects are being driven by selection into unemployment.

Using the same dataset as in this paper, Kanika et al. (2008) investigate whether individuals are less likely to be at small firms and, particularly, small firms without insurance. By exploiting the detailed health information in the MEPS, the authors find that workers with sick families are less likely to be new hires at small firms and, among small firms, less likely to be hired in positions that offer health insurance (Kapur et al., 2008). They confirm this pattern in overall stock employment at small firms with insurance; though they find ambiguous results on employment separations by firm type.

1.4 Model

In a perfectly competitive labor market, jobs with health insurance would have lower wages than those that do not provide health insurance - with the difference in wages exactly offsetting the cost of health insurance to the employer. In this environment, an individual would be choosing between a job with lower wages and health insurance and a job with higher wages but no health insurance[9]. Individuals will prefer the job with insurance when their valuation of health insurance exceeds the offset in wages; where the value of insurance to an individual is determined by the cost of outside health insurance, individual health and income, and preferences. However, as pointed out by Gruber and Madrian (2001), not all firms have the same cost of insurance provision and not all workers value insurance equivalently. This implies that people will sort into jobs where their valuation of the benefits is higher than the firm-level reduction in wages for those benefits, and firms will offer benefits if the cost of providing insurance is less than or equal to the market-clearing wage offset.

These factors will be present for the unemployed who must choose whether to accept a job offer or reject one for the promise of a better offer in the future. Thus, in addition to affecting the type of job someone might accept, there is a role for insurance demand to affect unemployment duration. Below, I present a simple search model to characterize the decisions facing unemployed individuals with differing demands for health insurance. The model is meant to illustrate the tradeoffs involved in searching and in accepting realized job offers. In addition, the model describes the behavior of key factors driving unemployment duration and the likelihood of accepting a job: search intensity and insurance-specific reservation wages.

My model assumes that in each period an unemployed person chooses how much to search, s, where $s \in [0, 1]$, given that there is a cost to search due to necessary effort expended, opportunity cost of leisure, pecuniary costs related to search, etc. The cost of

[9]This simplified framework ignores the regular out-of-pocket costs of maintaining the insurance. This can just be assumed to be a part of the decrease in wages without changing any of the conclusions below.

search is a convex function of the amount of search itself and is denoted by $c(s)$[10]. I assume that individuals differ in terms of their taste for insurance where individuals with higher insurance demand face a higher utility cost to not being insured through an employer; this cost is represented by γ. By searching, an individual proportionally increases her job-offer arrival rate; for simplicity, s also directly represents the job-offer arrival rate. When an offer arrives, the offer comes from a pool of jobs where a proportion π are jobs with insurance and $1-\pi$ are jobs without insurance. These jobs are assumed to have the following wage distributions, respectively, $f_{HI}(w)$ and $f_{NHI}(w)$. Lastly, I take ρ to be the discount rate and b are any benefits received while unemployed, where $b \in (\gamma, \overline{w})$.

Under this setup, a person's value of being unemployed is given by the following expression, where an individual chooses search to maximize the value of being unemployed:

$$\begin{aligned} V_U &= max_{s\geq 0}\left\{b - c(s) - \gamma + \frac{1-s}{1+\rho}V_U + \right. \\ & \left. \frac{s}{1+\rho}\left(\pi E_{F_{HI}}\left[max\left\{V_E(W), V_U\right\}\right] + (1-\pi)E_{F_{NHI}}\left[max\left\{V_E(W-\gamma), V_U\right\}\right]\right)\right\} \end{aligned}$$

The first expression is an individual's flow utility which comes from unemployment benefits minus the cost of searching and a person's demand for insurance. The second term is the discounted value of being unemployed in the event that the individual does not receive a job offer (an event with probability $1-s$). The last term represents the expected value of accepting a job given that the individual receives a job offer with probability s (the amount she searches). The individual will only accept a given job if its present discounted value is higher than that of being unemployed where the present discounted value of taking a job forever is given by $V_E(\cdot)$.

By definition, an individual's reservation wage is the wage at which she is exactly indifferent between remaining unemployed and accepting a job offer. Therefore, we have the following indifference conditions that yield a reservation wage, w^*_{HI}, and a reservation wage, w^*_{NHI}:

$$\begin{aligned} V_E(w^*_{HI}) &= V_U; V_E(w^*_{NHI}-\gamma) = V_U \\ \Rightarrow \ w^*_{HI} &= w^*_{NHI} - \gamma \end{aligned}$$

which, since the worker is assumed to keep an accepted job forever, yields

$$V_U = \sum_{t=0}^{\infty}(\frac{1}{1+\rho})^t w^*_{HI} = \frac{1+\rho}{\rho}w^*_{HI}$$

[10] I assume that $c'(\cdot) > 0, c''(\cdot) > 0, c(0) = 0, c'(0) = 0$, and $c'(1) = \infty$

Thus, with some re-arranging of our initial function for the value of unemployment, we have

$$\begin{aligned} w^*_{HI} &= max_{s\geq 0}\{b - c_u(s) - \gamma + \\ &\quad \frac{1}{\rho}s\left(\pi\int_{w^*_{HI}}^{\infty}(w - w^*_{HI})dF_{HI}(w) + (1-\pi)\int_{w^*_{NHI}+\gamma}^{\infty}(w - w^*_{NHI})dF_{NHI}(w)\right)\right\} \end{aligned}$$

Taking first order conditions and using the envelope theorem, I find that the optimal solution$(s^*, w^*_{HI}, w^*_{NHI})$ is given by the following system of equations

$$c'(s^*) = \frac{1}{\rho}\left(\pi\int_{w^*_i}^{\overline{w}}(w - w^*_{HI})\, f_{HI}(w)\, dw + (1-\pi)\int_{w^*_{NHI}}^{\overline{w}}(w - w^*_{NHI})\, f_{NHI}(w)\, dw\right)$$

$$\begin{aligned} w^*_{HI} &= b - \gamma - c(s^*) + \\ &\quad \frac{s^*}{\rho}\left\{\pi\int_{w^*_i}^{\overline{w}}(w - w^*_{HI})\, f_{HI}(w)\, dw + (1-\pi)\int_{w^*_{NHI}}^{\overline{w}}(w - w^*_{NHI})\, f_{NHI}(w)\, dw\right\} \end{aligned}$$

$$w^*_{NHI} = w^*_{HI} + \gamma$$

To summarize, the first equation shows that the marginal cost of search needs to equal the marginal benefit of search, which is given by the expected discounted difference between wage offer received and the reservation wage at each type of job. The second equation sets the reservation wage for a job with insurance to be equal to the value of being unemployed. The last equation describes the reservation wages such that a worker is indifferent between a job with insurance and a job without insurance. In the next section, I explore how demand for insurance impacts these optimal choices.

Comparative Statics

This model yields the following comparative statics (with the details derived in the Appendix), at the optimal (s^*, w^*_i, w^*_o). As the demand for insurance, γ, increases: 1) search increases, $\frac{\partial s}{\partial \gamma} > 0$; 2) the reservation wage for jobs with insurance decreases, $\frac{\partial w^*_{HI}}{\partial \gamma} < 0$; 3) the reservation wage for jobs without insurance is decreases, $\frac{\partial w^*_{NHI}}{\partial \gamma} > 0$; 4) the exit hazard to jobs with insurance increases, $\frac{\partial H_{HI}}{\partial \gamma} > 0$; 5) the exit hazard to jobs without insurance can either increase or decrease, $\frac{\partial H_{NHI}}{\partial \gamma} \geq 0$ or $\frac{\partial H_{NHI}}{\partial \gamma} < 0$; and, 6) the overall exit hazard from unemployment is also ambiguous, $\frac{\partial H}{\partial \gamma} \geq 0$ or $\frac{\partial H}{\partial \gamma} < 0$. Below, I explain the intuition behind these predictions.

The effect of higher demand on overall unemployment duration can be ambiguous for the following reasons: on the one hand, present demand for health insurance will increase search effort. This is because individuals without access to outside health insurance find it costlier to remain uninsured while unemployed. This increases the difference between the flow utility from being employed at a job with insurance and that from remaining unemployed, thereby increasing the value (expected return) of search. By increasing search effort, an individual increases the offer arrival rate and therefore increases the likelihood of receiving a job offer above their reservation wage. On the other hand, a high-demand type individual will be more likely to reject job offers that arrive without health insurance. This will have an offsetting effect of higher search, as increased search also increases the likelihood of receiving an uninsured offer from a part of the wage distribution that is too low to compensate for the cost of remaining uninsured. These two offsetting forces may thereby yield an ambiguous effect on unemployment duration.

Finally, as the demand for insurance increases, the reservation wage for jobs providing insurance declines. This is because the lifetime utility from a job without insurance is decreasing significantly and therefore the value of the outside options has decreased. Therefore, the unemployed prefers a job with insurance and the reduced reservation wages for these jobs further drives up search effort. The contrary effect is found on reservation wages for jobs without insurance. Since a person must pay their insurance disutility forever when taking this job, reservation wages must rise to make an individual indifferent between taking a job without insurance to one with insurance.

Figures 1.8 and 1.9 plot the overall exit hazard and hazard rates for each type of job for different values of π (the proportion of jobs with insurance). As described above, the model predicts that insured reservation wage decreases and uninsured reservation wage increase in demand, search probability increases in demand, and the insured hazard rate increases, while the uninsured hazard rate can be increasing or decreasing.

In the empirical analysis below, I exploit three measures that proxy for variation in γ, the model's parameter for demand for health insurance. I cannot observe search effort or job offers for individuals but I have information on the type of job someone accepts and how long the person was unemployed. From these I can test the model's predictions on insurance offer probabilities, exit hazards, and wages.

1.5 Data

MEPS

The samples I construct and analyze in this paper come from the Medical Expenditure Panel Survey (MEPS)[11]. The MEPS is a panel survey that collects information regarding an individual's health, health insurance status, and health costs over time. The MEPS began collecting information in 1996 and its 15 overlapping panels currently span through 2012. The MEPS draws a random subsample of 30,000 households from the larger, monthly National

[11] Other possible data sources that could be used include the CPS, the SIPP, the PSID, and the NLSY. Among these, only the NLSY has information on whether an individual is offered health insurance through an employer. I tried to construct a corresponding sample of unemployed, married individuals by linking March-to-March CPS interviews but this approach yielded too few observations to study the effect of spousal health on unemployment outcomes.

Institute of Health Survey. In the MEPS, each individual in a household is interviewed five times (with each interview referring to a specified period of time before the interview date) for a total of two years[12]. The majority of the MEPS is publicly available though certain identifiers, such as state of residence, are only available through restricted-use access.

The MEPS collects detailed information on each individual in a household. Most relevant to this study, the interviews collect information on an individual's employment in a round, as well as characteristics about the job - such as the industry, occupation, wage, firm size, hours of work, and the benefits provided by the job. The information regarding health insurance benefits distinguishes the MEPS from other data sources which also collect data on whether an individual has health insurance through her job. In addition to recording the source of an individual's health insurance coverage, the MEPS surveys whether the job offers health insurance to the individual and/or to other employees. This distinction is important and will be used to identify individuals with lower and higher demand as described in Section 6.

The second reason the MEPS lends itself to answering questions about insurance and unemployment is that it collects employment information extending before the first interview round. If an individual is not employed or started a new job in the first round, information is collected about the last job the individual had, including when and why the job ended and whether the job offered health coverage. Similarly, each job's start date is recorded. Knowing the last job's end date and the new job's start date (when applicable) allows me to construct non-employment durations for individuals who are identified as not working or who start at a new job in the interview round[13].

Unfortunately, unlike other more employment focused data, the MEPS does not ask about search behavior. In other words, I cannot tell whether an individual is explicitly looking for work when they are not employed. Given that the distinction between the non-employed and unemployed has been contested (Shimer, 2012) and the question posed in this paper about the return to work can extend to both groups, I refer to the groups interchangeably throughout this paper. Importantly, the MEPS collects information on why an individual left her last job. In this paper, I exclusively focus on the individuals who have been laid off from their jobs, in order to avoid any differences that selective entry into non-employment may cause on spell duration and re-employment decisions.

[12]Since not all individuals in a round are interviewed on the same date, each round spans longer than two years by roughly six months.

[13] This feature likely allows me to identify shorter unemployment spells than most surveys which only capture the *currently* unemployed. On the other hand, people may be less likely to remember short unemployment spells. In Table 1.2, we see that the average weeks unemployed for my sample is 5 weeks higher than that reported in the CPS; however, this may be due to the top-code for this variable put in place by the CPS at 99 weeks.

Sample Construction

I pool together all available MEPS data from 1996 through 2012 and keep all individuals above the age of 18 and below 65, this leaves me with 208,147 individuals (for a combined 847,231 person-round observations). Among these, by using information on a person's last day of work at previous job, I identify 69,225 cases where a person either switched jobs or had an identifiable unemployment spell[14]. Among those with identifiable transitions and unemployment spells, around 34% are classified as exogenously unemployed yielding a sample of 23,370 unemployment spells due to layoff[15]. Ultimately, each observation in my data represents an unemployment spell that is terminated for one of two reasons: either the individual finds a new job or the individual is censored due to dropping out of the survey or reaching the survey end date. Of the 23,370 laid-off unemployment spells I observe, 40.5% are censored - implying I observe a total of 13,112 individuals exit unemployment. I implement further restrictions to define my samples of interest which I describe in more detail below.

To summarize, I focus on individuals who have lost their jobs due to layoff ($N = 23,370$). Of these, 13,112 are observed taking new jobs before the end of the panel: 4,996 to a job with a health insurance offer (38%). Of the unemployed who end up finding jobs, I see the following transitions into and out of jobs with health insurance (see table below). Roughly 43% of those who were previously in jobs with health insurance transition to jobs without health insurance and 24% of those who previously did not have health insurance find new jobs out of unemployment with health insurance. This does not appear to be purely driven from transitions out of full-time employment, as can be seen in the following sub-table where the sample is restricted to those coming from and moving to a full-time job. In the results section, I explore how higher demand for health insurance affects individuals' likelihood to work full-time or part-time.

[14]Anyone who is currently unemployed or recently started a new job in the first round will be included in my data. I allow individuals to to be associated with more than one unemployment spell if we see them transition more than once during the panel.

[15] I classify the laid-off as those who state they left their jobs due to either: layoff, downsizing, or business termination. I also include anyone who is seen collecting unemployment benefits. Individuals who did not provide a response to why they are no longer employed at their last job are not included in the sample. The 23.4k unemployment-spell observations in my sample are associated with 20k individuals, with 12.4% individuals representing two unemployment spells and 2% of individuals corresponding to more than two unemployment spells.

Among Laid-Off:

	New Job Does Not Offer HI	New Job Offers HI	Total
Past Job Did Not Offer HI	43.4	13.5	56.8
Past Job Offered HI	18.5	24.6	43.2
Total	61.9	38.1	100.0

N = 13,112

Restricting to only Full-Time Workers:

	New Job Does Not Offer HI	New Job Offers HI	Total
Past Did Not Offer HI	34.1	12.6	46.7
Past Offered HI	18.9	34.4	53.3
Total	53.1	46.9	100.0

N = 7905

Thus, we see that there are individuals that switch from jobs with insurance to those without and vice versa. Along these lines, Figure 1.3 plots the predicted probability that an unemployed individual will be in a job that offers her insurance. These probabilities are based off of a regression on a sample of all employed individuals in the MEPS from 1996-2012 and their observable characteristics and interactions of these variables. The mass for this distribution is concentrated around 0.7, the proportion of jobs that offer insurance. Encouragingly, there is significant spread across the probability distribution - suggesting that, based on their observable characteristics, most individuals have a non-negligible probability to be eligible for a job with insurance.

Sample Summary Statistics

Table 1.2 shows the summary statistics for my sample of unemployed and laid-off. I compare this sample to the laid-off from the March CPS surveys from 1996-2012. We see that the MEPS sample is more likely to be female, Hispanic, and has a slightly lower average number of weeks unemployed[16]. The samples look more balanced in terms of age and education. I control for all of these covariates in all of the specifications below.

Figure 1.2 shows the unemployment survival curve for my population of interest. The median weeks unemployed is 11.3 and 5% of my sample transitions to new work immediately (i.e. - has 0 weeks of unemployment). These individuals are not included in the Kaplan Meier Survival curve or the duration models that estimate exit hazards below and none of the results that condition on exit from unemployment are affected by their inclusion. 71% of those who find a job exit unemployment in 6 months and 90% before 1 year.

[16] In addition to differences in survey methodology regarding the collection of unemployment information, this may be due to the fact that the CPS maximum number of weeks for unemployment is set to 99.

Demand Proxies

Demand Proxy I: Spousal Health Risk My primary analysis uses the health risk of an unemployed person's spouse as a proxy for the demand for a job with health insurance. I describe the assumptions behind this approach below in Section 6.1. In this section, I describe what the measure is and how it is constructed.

Using actuarial models, each MEPS respondent is assigned a relative health-risk score that predicts an individual's expected annual health care resource use for the upcoming year. This score is based on an extensive and thorough list of health conditions that are collected in the initial MEPS survey. The health-risk score predicts expenditure on health relative to the average national expense. The score is on a logarithmic scale where 1 corresponds to the national average and a $.x$ increase in the score corresponds to a $x\%$ predicted increase in expected costs, with respect to the national average. Scores are computed to correspond to spending under private insurance, Medicaid, Medicare, or no insurance (irrespective of an individual's actual coverage). I focus on the uninsured score so as to represent an individual's expected out-of-pocket costs if they were to treat all reported conditions without insurance. The health-risk score has been found to be correlated to future inpatient stays, emergency room visits, and even mortality[17].

Figure 1.4 plots the distribution for spousal health risk for observations in my laid-off and married sample. Below, I create a dummy variable to represent whether an individual is in the top two-thirds of the health-risk distribution to distinguish between those with very low expected scores and those with higher scores. The cutoff for the bottom-third of the distribution is 0.57. More details by the MEPS health-risk measure are included in Appendix A1.

Perhaps surprisingly, an individual's health risk is not highly correlated to her spouse's health risk. I find that the correlation between spouses' health risk is only .11. Among the employed, the correlation across spousal health risk is only slightly higher at .14. The average health for a displaced individual given a spouse's health is presented in Figure 1.5.

Demand Proxy II: Spousal Insurance Offer Rejection My second demand proxy is constructed using information on an individual's and spouse's insurance take-up decisions. I restrict my sample to everyone who has a working spouse with an insurance offer and also received an insurance offer at her pre-displacement job. I then compare the outcomes of the individuals who previously accepted their policies while their spouses rejected their offers to laid-off individuals who had rejected their own offers but whose spouses accepted their offer. The idea behind this comparison is to compare individuals who were more likely to be their family's primary insurance provider to those who are more likely to be covered by their spouse's employer policy.

In order to limit the effect of endogenous decisions made while unemployed, I use the earliest information we have regarding the spouse's employment and insurance take-up decision. In the case of individuals who we see laid-off, I use the spouse's labor market conditions at the

[17] http://meps.ahrq.gov/mepsweb/data_stats/download_data/pufs/h92/h92doc.shtml

time of layoff and keep these fixed when looking at other points in the unemployment spell. In all other cases in which individuals already enter our sample unemployed or were previously unemployed, I use the spouse's characteristics and job-details from the first record available. Due to minimal transitions observed on the side of the spouse, the use of this snapshot should not differ significantly from the initial-unemployment case.

Demand Proxy III: Own Previous Insurance Take-Up Along with information on the last day at a previous job, respondents are asked whether their past job offered insurance, and if so, whether it had been taken up. As discussed in more detail below, I use differences in previous take-up to signal differences in demand for ESHI. Of those laid-off, 40% were offered insurance in their previous posts and 30% of those offered rejected the insurance offer.

1.6 Research Design

Identifying Higher Relative Demand Groups

As explained above, I consider three proxies for health-insurance demand. In this section I discuss the strategies I use to identify the effect of differences in demand, holding other determinants constant. Given that none of my approaches exploit random assignment, the main concern in these comparisons is that differences in demand are correlated with unobservable characteristics that may also affect search outcomes. We want to ensure that our results are not being driven by omitted variable bias. To do so, I implement two different empirical strategies to reduce the role of potential selection bias in our estimates. I argue that these three research designs, which draw on rich information about previous employment of the now-displaced individuals, allow for plausibly unbiased estimates of the effects of individual preferences on job-search outcomes. Using these alternative approaches to detect the effects of higher demand for insurance serves as a a way to test the consistency of the results and reduces the concern of selection bias influencing our overall conclusions.

The first approach I consider is based on the premise that individuals with sicker spouses should have a higher valuation for health insurance because of their higher expected family health-care use. This higher valuation should, in theory, only affect the search decisions and job choices of those whose spouses do not have their own employer-provided insurance. I thus adopt a difference-in-differences design, using outcomes of individuals with covered spouses to control for any spurious correlates of spousal health risk.

The second method I use of identifying differences in demand relies on comparing past take-up choices among couples with joint insurance offers through their employers. I study the outcomes of those who took up their previous insurance offer but whose spouse declined their own health insurance to the outcomes of displaced individuals who declined their offer but whose spouse accepted his own. The premise here is that married individuals with spouses who have rejected their insurance offers have reduced outside options for health insurance and, by likely having been the family's sole insurance provider, will have higher demand for ESHI on the next job. I use detailed controls for the person's last job and their spouse's employment to account for any differences that may come from individual-level heterogeneity.

The third conceptual experiment is based on revealed preferences observed by an individual's past decision for whether to accept or reject a job's insurance offer. Among individuals without a spouse with a ESHI offer, individuals who chose not to take up a past insurance offer should be less likely to require that their next job offer include insurance. After conditioning on individual-level characteristics and past job attributes, I argue that preferences are the main driver for an individual's past decision to accept insurance on their job[18].

I assume that my second and third proxies, conditional on a long list of observables, are not confounded by unobserved determinants of employment outcomes. Importantly, I can control for attributes of the pre-unemployment job, lending more justification to my assumption of unconfoundedness. Furthermore, for these secondary demand differences, I implement a propensity score estimator to control flexibly for the observables and to trim each sample so as only to compare observably similar cases in the higher and lower-demand groups. This approach allows me test the degree to which lack of common support across groups is driving results. Below, I describe the three different methods and corresponding research designs in more detail.

Method 1: Spousal Coverage and Spousal Health Risk

My principal approach takes advantage of information on the health of a displaced worker's spouse and the spouse's access to health coverage. I argue that individuals with high spousal health risk but no existing health insurance[19] have a higher valuation, and therefore higher demand, for employer-sponsored health insurance than those with healthier spouses, due to their higher predicted expenditures in health. Specifically, I restrict my sample to married individuals who have been laid-off and have a non-missing measure for their spouse's health risk. For this approach to work, the health risk score needs to align with people's expectations for their dependent's future health costs. This seems to be justified in that the health score is modeled off of a person's age, sex, and her self-reported health conditions.

In the MEPS, responses to self-reported "need" for insurance, which I use as a proxy for a person's demand for insurance, do indeed vary with own, as well as spouse's health risk. In Table 1.3, we see that an individual with higher health risk is 10 percentage points more

[18] Though drivers of health insurance demand, such as risk-preferences and health, may be correlated with individual demographics and other observables, it is important to control for these characteristics to be able to argue that differences in preferences are driving differences in outcomes rather than these individual-level characteristics.

[19] I restrict health care coverage to those through an employer. The results are robust to including individuals covered under government insurance. The reason behind not including these individuals in the covered group is that there is reason to believe this could bias the results upwards, as individuals with spouses on Medicaid or Medicare are less likely to secure jobs with insurance due to lower employability and lower skills.

likely to claim she has a high need for health insurance. Similarly, individuals with higher spousal health risk are 6 percentage points more likely to put themselves in this high-need category. In the analysis below, I restrict my attention to variation in spousal health risk since there are fewer concerns for endogeneity affecting the job market outcomes[20].

We may worry that individuals with sicker spouses may also be different in other ways that will affect their unemployment behavior and employability (other than just their demand for insurance). In other words, it is likely that spousal health is not randomly assigned and this introduces concern about the effect of health risk on unemployment outcomes through its correlation with unobservable individual characteristics[21]. By using information on individuals with unhealthy spouses with insurance coverage, I can implement a difference-in-differences-like approach where I control for the non-demand related effects of poor spousal health on job outcomes. A similar design using an individual's chronic conditions was used to study the effect employer insurance provision has on job-leaving rates (Kapur, 1998).

This setup motivates the following estimation equation where I use a spouse's predicted health risk and expenditure as a demand shifter for individuals with no spousal coverage [22]:

$$Y_i = \beta_1 NoSpCoverage_i + \beta_2 SpHlthRisk_i + \beta_3 SpHlthRisk_i \times NoSpCoverage_i + \mathbf{X}_i'\Gamma + \epsilon_i$$

where $\mathbf{X}_i$ represents a vector of individual and spouse characteristics and job-attributes (where past job is used for the unemployed individual), $SpHlthRisk_i$ is a dummy that is set to one if an individual's spouse is in the top two-thirds of the health-risk distribution[23], $NoSpCoverage_i$ is a dummy that is equal to one if an individual's spouse does not have employer-sponsored health insurance, and Y_i is a specific unemployment outcome. In this equation, we are interested in estimating β_3 as it describes the effect of the demand for insurance on unemployment outcomes, while controlling for any selection or actual effects of not having spousal coverage, β_1, and of having a sicker spouse throughβ_2. Our coefficient of interest, β_3, will be unbiased when:

$$\begin{aligned} &E[\epsilon_i|SpHlthRisk_i = 1, NoSpCoverage_i = 0, \mathbf{X}_i'] \\ &= E[\epsilon_i|SpHlthRisk_i = 1, NoSpCoverage_i = 1, \mathbf{X}_i'] \end{aligned}$$

meaning that, once we control for the main effect of spousal health risk and the main effect for not having spousal coverage, there should be no differences in the potential outcomes for the group with both high spousal health risk and no spousal coverage. This requires spousal health affects the unemployed worker's employability similarly when the spouse does and does not have her own coverage. The key confound here is if a worker's employability is

[20] My regressions control for own-health risk as a way to verify expected results. The results do not change with the exclusion of own-health risk.

[21] For example, those with sick spouses may be those with lower job skills.

[22] The basis for health score is discussed in Section 5.4 and in the Appendix

[23] There appears to be a non-linear effect of spousal health risk on job outcomes, with the effect decreasing for very high values of spousal health risk. I look at a variety of specifications (such as a squared term, terciles, and quartiles, and cutoffs and present this one for simplicity.

correlated with our high-demand group (those with non-covered and unhealthy spouses). Below, we explore the extent to which this may be the case as evident through differences in observables[24].

Ideally, our high-demand group would no look different, in terms of observables, from the other groups implied by the difference-in-differences strategy once we control for any differences due to not having spousal coverage and to having a sicker spouse. In Appendix Table 1.13, I explore the balance in covariates across the difference-in-differences groups and in particular, for the main group of interest (the group with both spousal health risk and no spousal coverage). The table presents the p-value for the difference in means for the high-demand group from a regression that additively controls for the average difference for those with unhealthy spouses and for the average difference for those without spousal coverage[25]. We see that most covariates are balanced, except for the percent that are White, college educated, older, and across past hourly wage.

In order to evaluate how these covariate differences influence the overall likelihood of getting a job with insurance out of unemployment, I construct a measure for the probability that a person is in a job with insurance based on the characteristics of all employed individuals in the MEPS. I then use this measure to compare the predicted likelihood of being offered insurance in a job across the four groups. The differences in average characteristics for the non-insured unhealthy spouse group combine yield a 1.4 percentage points increase in the predicted likelihood to be offered insurance (above what is predicted by the spousal health effect and no coverage effect combined). Though this signals slight differences in key observables for this group, this difference is much smaller than the effect sizes we find below in the results on insurance coverage secured out of unemployment[26].

Method 2: Spousal Insurance Rejection

For my next two methods, I restrict my attention to comparing the outcomes of unemployed workers with a similar choice sets but different enacted choices. I take advantage of infor-

[24] In Appendix Table 1.12, I compare how key insurance and health measures differ during unemployment across the four groups implied by difference-in-differences. These statistics are only available from the restricted set of those in the sample who were unemployed at the time of a survey. We see that COBRA rates are fairly uniform across all four groups and, as expected by construction, the likelihood that someone is a dependent or is insured in the non-covered spouse group is much lower. This group is also more likely to be covered by Medicaid or Medicare. Also, by construction, the main group of interest - those with high spousal health risk and uncovered spouses - have spouses with higher health risk measures, as well as higher own-health risk and age (since these measures are positively correlated). This effect should, in theory, work against finding any effects on the likelihood to be re-employed at job with insurance since these workers are likely less productive, as well as costlier to insure. Lastly, as confirmed above, individuals with sicker spouses are less likely to state that they have no need for HI and more likely to claim they have a high need.

[25] The p-value presented is for a t-test on β_4 in the following regression: $Y_i = \beta_1 + \beta_2 NoSpCov_i + \beta_3 SpRisk_i + \beta_4 NoSpCov_i \times SpRisk_i$, where Y_i is an individual-level covariate, such as gender, race, education, etc.

[26] I also run specifications where I control for this out-of-sample predicted likelihood of securing a job with insurance and find no differences in the effects.

mation on past-offer and, in the case of married individuals, spouse-offer status to define my comparison groups[27]. In addition to the likely differences in observables, own and spousal, we cannot argue that a married individual who rejects her insurance offer to be otherwise covered by a spouse's plan has the same demand for ESHI as a married individual rejecting insurance without a spousal offer of insurance. I separate these groups so as to compare individuals with the same options but different choices.

My second approach restricts the sample to married, laid-off individuals who had an insurance offer at their previous job and have a working spouse who also has an employer-provided insurance offer. I then compare the individuals who took up their ESHI while their spouses rejected their own offers to individuals who rejected their offers while whose spouses accepted. In other words, the idea is to compare individuals who were more likely to be the primary insurance providers through their jobs to those who were more likely to be dependents on a spouse's policy. In both cases, there is a working member of the family with insurance. I classify those with spouses who *rejected* their own insurance offers as the high-demand types since it is more likely that this group will want a job to replace their previously accepted health insurance policy. In the opposing case, individuals are likely to remain covered by their spouse's policy while unemployed and, possibly, even after finding a job out of unemployment. I explore this evidence using my data below.

When considering joint offers, we expect families to choose the cheaper policy, conditional on quality. Thus, it is important to consider why plan terms and prices may vary. The worry may be that individuals who become their family insurance providers are more likely to also be in better jobs with better benefits. For this reason, I control extensively for past job characteristics and spousal job characteristics, as well as own- and spouse-descriptors. The idea behind this approach, therefore, is that once we control for the type of job our displaced worker had and what type of worker they are, the variation in plan quality should be unrelated to a worker's type, employability, and ultimately unemployment outcomes[28,29]. For example, we may know that administrators are more likely to have better insurance policies, and, for that reason, we want to compare across two people who were administrators. Similarly, I control for a spouse's job's attributes so as to factor in the expected differences in the types of offers that come with different types of jobs, as well as how these spousal job characteristics may affect the re-employment of the unemployed worker.

In Appendix Table 1.14, we see that the individuals with previous dual-coverage are much more likely to be dependents on an insurance policy when unemployed; therefore, indicating that this group may also be more likely to have a better outside option for health insurance, further lowering demand.

[27]By using within group comparisons, we can reduce the difference in both observables and (presumably) unobservables.

[28] It is also the case that some employers do not allow individuals to forego insurance. This may help introduce variation so that couples are not always choosing the best policy.

[29]Though I exclude cases in which both a spouse and the focal individual accept their insurance offers, the results hold up if we include these cases as having lower demand for ESHI than the couples in which the unemployed individual was the sole insurance holder in the couple.

Thus, among married, laid-off workers who have employed spouses with insurance offers, past take-up is used as a signal of demand for insurance in the past and spousal take-up is used to shift availability of health insurance outside of employment.[30] I use the following estimation equation to characterize the effect of higher-insurance demand on job outcomes:

$$Y_i = \beta_1 SpReject + X_i'\Gamma + \epsilon_i \tag{1.1}$$

where we are interested in the sign and magnitude of β_1 as the effect of higher demand for insurance on unemployment outcomes. In this case, $SpReject_i$ is a dummy for whether the spouse rejected the insurance offer and is set to zero when the unemployed individual rejected her insurance offer (where $SpReject = 1$ represents the high-demand group). Y_i represents job characteristics for the individuals we observe exit unemployment. For this effect to be unbiased, we need for:

$$E[\epsilon_i | SpReject_i = 1, \mathbf{X}_i'] = E[\epsilon_i | SpReject_i = 0, \mathbf{X}_i']$$

This means that, conditional on observables, there are no differences in the potential outcomes between individuals whose spouses chose to reject versus accept their previous insurance offers. In other words, once we control for any selection on observables, we can isolate differences in demand.

Spousal choices, however, are not randomly assigned and, therefore, introduce the concern of omitted variable bias influencing the results. For example, we may be worried that spouses that accept their insurance offers have better jobs and that these differences are related to differences in the unemployed worker's employability. In order to reduce these concerns, I use a rich set of controls about the displaced individual, her past job[31], her spouse, and her spouse's job[32]. By controlling for this variation in observables, I propose that we can isolate plausibly exogenous variation in preferences for insurance.

In Appendix Tables 1.14 and 1.15, I describe average differences in observables across the high and low-demand groups outlined here. As we would expect, the low-demand group is much less likely to be on COBRA coverage while unemployed and significantly more likely to be a dependent on another person's policy. Appendix Table 1.15 presents the p-value for the likelihood that the averages in demographic and person-level characteristics are statistically different across our comparison groups. Here, we can see that there are observable differences

[30] I do not include the group of individuals who rejected their past insurance offers and have spouses with an insurance offer. This is because it is not clear whether those who rejected their insurance offers, but whose spouses did not, have a lower demand for ESHI than those where both partners rejected the insurance offer (a strong sign of low demand). For the same reason, I do not include the group with individuals who were not offered health insurance while their spouse was. Similar to the case above, it is not obvious that the group where the spouse declined insurance should have a higher demand for insurance because of past revealed preference for not having ESHI.

[31] Including past full-time status, self-employment status, hours per week, industry and occupation dummies, firm size, and past hourly wage.

[32] Including hours per week, type of job, type of additional benefits offered (such as bonuses, paid vacation, overtime pay, retirement, etc), firm size, industry and occupation dummies, hourly wage, etc.

in our groups pointing to fact that a person's demand type does not look as if it were distributed randomly[33] for all characteristics, particularly past work attributes. However, these demographic differences do not yield large differences in the predicted likelihood for each group to be in a job with an insurance offer where the predicted probability is based off the observable characteristics of those working in the MEPS. Nonetheless, below I present a propensity score method to explicitly ensure overlap in the covariate distributions across the demand types.

Propensity Score Controls and Trimming: Likelihood that Spouse Rejects Offer I introduce a propensity score method as 1) an alternative and possibly more flexible way to control for the selection on observables into the high-demand type, and as 2) a measure to restrict my sample to observably-similar observations and thereby ensure equivalency across covariate means. By controlling for a fourth-order polynomial of the predicted likelihood that an observation is a high-demand type, I can confirm that any identified effects remain under a more parsimonious specification of the effect of observables on our outcome of interest. Moreover, we can study whether there is any heterogeneity in the effect of having higher demand by comparing across the distribution of observations with the same predicted likelihood of being high-demand.

To implement this design, I construct a propensity score to predict an individual's likelihood to be in the high-demand group. To do so, I estimate a logistic regression to model the likelihood that a couple chooses to take-up the the now-displaced worker's previously-offered policy over the spouse's HI policy versus the opposing case of taking up the spouse's policy over the displaced worker's. I model this probability among the set of married and unemployed individuals who had ESHI offers at the job they lost and have working spouses with insurance offers. This regression controls for main effects and interactions of individual characteristics, past job features, spouse characteristics, spouse job features, and region, year, and panel fixed effects[34]. The propensity score is the predicted probability from this regression. Figure 1.6 shows the predicted distribution for the propensity to be in the high-demand group. Here, we see that the propensity score model commits few Type-I errors; meaning, few of the low-demand observations are misclassified as having a high propensity score. On the other hand, the high-demand group is distributed fairly evenly across the propensity score.

[33] Insurance theory would not predict these demand groups to be identical in all dimensions except for their preferences. For example, we assume that increases in an individual's current and expected medical outlays will outwardly shift her demand for insurance. Similarly, individuals that are more risk-averse will be more likely to choose an insurance contract at a given price than less risk-averse individuals. Thus, we should find different health conditions and different incomes across our demand groups. Though these differences in observables may align with differences in relative insurance demand, we want to control for the effects of these differences on unemployment outcomes to be able to isolate the effect of insurance demand from the effect of these (correlated) observable characteristics. Once controlling for differences in observables, we can argue that any identified effects are driven by demand differences rather than heterogeneity across groups.

[34] The full list of variables included are listed in Appendix A3.

We may be worried that, even after controlling flexibly for the estimated propensity score, the across-group differences in observables imply that I am not comparing equivalent subjects and any results are being driven by inference on subjects without similar counterparts. Lack of overlap in individual characteristics prevents the ability to control for differences in observables and, in this way, can introduce bias in the estimates. In other words, by estimating the effect of having high-demand in regions of the propensity score distribution where there is minimal overlap, we reduce the precision at which we can identify true effects and may be identifying effects that are due to differences in covariates rather than demand. To make sure I am not estimating the effect off of cases without common support, I use the propensity score to evaluate the comparability of the demand types and to test the degree to which my estimate may be biased by extrapolating out of sample.

Assuming unconfoundedness[35], we know that by controlling for the propensity for an observation to be a high-demand type, we can ensure overlap in the joint distribution of observables across demand types and recover unbiased estimates of the effects of insurance demand (Rosenbaum and Rubin, 1983). However, this requires ignorability, i.e. we need for there to be overlap in the probability of being a high-demand type such that:

$$0 < P(HD_i|X_i) < 1$$

This states that the probability that an individual has high-demand is not deterministic and implies that we can balance covariates across demand types. To ensure this, I propose trimming my sample to areas of common support across the propensity score. By trimming the sample in this way, we omit observations that are more dissimilar and do not have equivalent counterparts. This allows us to be more confident we are achieving unbiased estimation of our effect of interest (Dehejia and Wahba, 1999) because we can fully control for the effects of the underlying covariates.

The degree of trimming is based on having sufficient overlap between the propensity scores of the two demand types. I implement the rule suggested by Crump et al. (2009) where I trim at least as much as .1 off of each tail of the distribution (where scores are distributed from 0 to 1) (Crump et al., 2009). In Figure 1.6, we see that the right-tail of the distribution (the part predicting a high likelihood of being a high-demand type) lacks a large degree of overlap and so I extend the proposed range to exclude this region as well. In the figure, I show the cutoffs I use to trim this sample[36] where the results presented below are not sensitive to the cutoff threshold. As we see in Appendix Table 1.15, in columns 4 and 8, once I trim the sample by excluding the tails of the propensity score distribution and controlling flexibly for the propensity score, all statistical differences in means by demand type go away[37]. This fact gives support to the argument that I am addressing heterogeneity which might influence results.

[35] $(Y_i(1), Y_i(0)) \perp HD_i|X_i) \; \forall \; i$

[36] The exact cutoffs I use are the following: in the spousal take-up case, I restrict the propensity scores to be in [0.1, 0.6].

[37] Trimming the sample does not impact this result. Statistical differences in the covariate averages across demand groups can be rejected after controlling flexibly for the propensity score.

Controlling for the propensity score flexibly and trimming motivates the following regression specifications where I compare job outcomes (Y_i) across higher and lower demand groups:

$$Y_i = \alpha + \beta HD_i + \gamma_1 \hat{p}_i + \gamma_2 \hat{p}_i^2 + \gamma_3 \hat{p}_i^3 + \gamma_4 \hat{p}_i^4 \ if \ \hat{p}_i \in T \tag{1.2}$$

where HD_i is a higher-demand dummy, $\hat{p}_i$ is the predicted propensity score for an individual, and T is a restricted range of the propensity score in which there is overlap. This specification is reported with bootstrapped standard errors since the propensity score is an estimated object and therefore Eicker-White standard errors do not apply. In the results, I present the effect of health insurance demand on the outcomes of interest under a regression controlling for observables (Equation 1) and a regression controlling for a fourth-degree polynomial of the propensity score on the trimmed sample (Equation 2). These regressions will estimate the effect of higher relative demand on our specified outcomes assuming that high demand for insurance is not correlated with unobserved determinates of Y_i, conditional on X_i.

Method 3: Revealed Preference

For this last approach, I define my high-demand group to be the set who accepted an insurance offer from their employer in their last job (i.e. the job before unemployment) as opposed to individuals who rejected their employer's insurance offer. In all cases, individuals who rejected insurance offers in their past jobs are deemed to have lower relative demand for ESHI as signaled by their past choices.

The sample in this case is comprised of laid-off individuals whose last job had an insurance offer and who do not have spouses with insurance offers. This includes all individuals without the possibility of spousal coverage: single individuals, married individuals with non-employed spouses, and married individuals whose working spouses did not receive insurance offers from their jobs. In Appendix Table 1.14, we see that those identified as high demand are significantly more likely to be on a COBRA plan while unemployed (this is somewhat by construction, since you can only have a COBRA plan if you were previously covered while on the job) and are significantly less likely to be uninsured. A strong reason for declining past coverage becomes apparent in this comparison: 26% of the low-demand group are on Medicaid or Medicare compared to 8% of the high-demand group. Similarly, the high-demand group is less likely to state they have no need for insurance and more likely to state they have a high need for insurance. Ultimately, we are interested in the following estimation:

$$Y_i = \beta_1 PastTakeUp_i + \mathbf{X}_i'\Gamma + \epsilon_i$$

where we are interested in the sign and magnitude of β_1 and $PastTakeUp_i$ is a dummy if an individual previously chose to take up insurance at the last job she had. Here, for β_1 to be unbiased, we assume that:

$$E[\epsilon_i | PastTakeUp_i = 0, \mathbf{X}_i'] = E[\epsilon_i | PastTakeUp_i = 1, \mathbf{X}_i']$$

which translates to assuming that there is no selection into whether someone accepts or rejects a past insurance policy, after we control for a detailed set of individual-level information.

Ultimately, we might be worried that individuals who previously rejected their insurance offer are very different from those who accepted or that insurance offers are rejected by this group because they come from worse-quality, lower-paying jobs. If there is unobservable heterogeneity that is correlated with our proxy for high demand, our results may be biased. In Appendix Table 1.15, I explore the differences in characteristics across past-insurance rejectors and acceptors. As may also be expected, the high-demand group (the acceptors) is older than the low-demand group and is more educated.

In order to account for the effect of any related differences in an individual's employability and skill or a past job's offer conditions, I control for extensive past-job characteristics[38] and individual-level descriptors. Once conditioning on individual and past-job characteristics, the effect of a past take-up decision is more likely to be driven by preferences for health insurance demand than differences in previous insurance terms or individual employability. This approach will work only if there are sufficient cases with these characteristics across both demand types. Otherwise, the results may be biased by the inability to control for how differences in observables may affect outcomes. For this reason, I introduce a propensity score approach, like the one discussed above, to ensure comparability across demand types and to test whether my estimates are biased by comparing cases with unbalanced covariates.

Propensity Score Controls and Trimming: Likelihood that Individual Accepted Past HI Offer Similar to above, I introduce a propensity score method to verify the results on a trimmed sample and with a specification that controls flexibly for the propensity to be in the high-demand group. As seen by the covariate means presented in Appendix Table 1.15, the low- and high-demand groups for the preferences comparison look fairly different in their average demographic characteristics. As discussed above, our comparison across relative demand must be made across similar individuals so that our estimates are not biased by incomparable differences. Ideally, we want to restrict our analysis to individuals who have the same underlying propensity to take up insurance, but differed only in their choices. If they are similar in all other respects, we can be more confident that differences in outcomes are driven by differences in the taste for insurance.

Like the approach for spousal insurance rejection above, I propose a propensity score specification that is meant to evaluate the degree of comparability of observations in each demand type and to identify a sample of observations with credible matches. I model the likelihood that someone will accept versus reject a past job's insurance plan as an individual's likelihood of being in the high-demand group. The propensity score for being in the high-demand group is constructed using a logistic regression based on available individual and

[38] Such as past hourly wage, hours of work, type of employment, firm size, and industry and occupation dummies.

family covariates and their interactions[39], as well as past-job descriptors. Figure 1.7 shows the resulting propensity score distribution by demand type, as well as the cutoffs I use for this sample[40]. In this case, we see that there are few observations in the left tail of the distribution but substantial overlap in the rest of the distribution. Just like in the preceding case, I compare my results across a regression with linear controls and a regression controlling for a quartic of the propensity score on a trimmed sample with overlap.

1.7 Results

Effect on Likelihood of Being Offered and Being Covered on Next Job

I am interested in testing whether individuals sort into different jobs and health insurance out of unemployment because of differences in demand for health coverage. As a first pass, I restrict my analysis to those who exit unemployment though by not accounting for censoring this may not represent the overall effect. As the difference-in-differences results in Table 1.4 attest, individuals with spouses with higher expected expenditures and no coverage are more likely to exit unemployment to a job with a health insurance offer than individuals with healthy spouses and no coverage or than individuals with unhealthy spouses but with coverage [41]. Comparing across individuals with covered and uncovered spouses allows me to control for the effect of spousal health on the job outcomes of interest. As we would expect, the effect of higher spousal health risk on the likelihood to find a job with health insurance is negative. This covariate may be capturing unobservables about an individual's likelihood to get a job with insurance or it may represent the forces (such as income constraints) that push individual's with sicker spouses to take any job above waiting for one with health insurance. Similarly, in the first specification without controls, the effect of not having spousal coverage is negative - likely capturing unobserved positive assortative correlation among partners. This effect decreases substantially when I add in the set of rich controls on individual and spousal demographics, as well as detailed information on an individual's past job, such as firm size, industry, occupation, hours of work, and whether an individual received coverage on the last job.

As a validation for this proxy and the results on an exit job's likelihood of a having an insurance offer, I also examine the effect of higher spousal health risk and no coverage on the likelihood that an individual takes up an offer, unconditional on whether or not she receives one, as well as the likelihood of take-up conditional on offer (among the smaller sample of individuals who exit to a job with insurance). Both cases show that the health-risk proxy for higher demand for employer-provided insurance is associated with increased rates of take-up, both conditional and unconditional. Even among those who take jobs with insurance out of unemployment, individuals with spouses with higher expected health expenditures and

[39] The inputs to the propensity score model are listed in Appendix A3.

[40] Here, I restrict an observation's propensity score to be between [0.2, 0.9] in my trimmed sample.

[41] I have confirmed that these effects are present if the specification is changed to a logistic regression instead. The average marginal effects are roughly similar in magnitude.

no spousal coverage are much more likely to accept a given insurance offer than individuals with a more healthy spouse and no spousal coverage. Similarly, as we would expect, we see that individuals with poor health and no spousal coverage are also more likely to take up health insurance. Thus, the finding on sorting out of unemployment is further strengthened by the significantly higher take-up of insurance plans amongst the higher relative insurance demand types; thereby corroborating the identification of the high-demand groups as well as providing additional support for the idea that ESHI was a driver in job choice.

Table 1.5 presents the results for the additional methods I propose to isolate variation in demand for employer-based health insurance. For the two methods (Spousal Rejection and Past Preferences), I compare my results across two specifications. The first is a linear probability model for a job outcome with an extensive set of individual descriptors and past job details. Similar to above, all regressions are restricted to those who exit unemployment. The second specification restricts the sample further to individuals with propensity score in restricted range of overlap. In the second specification, I control flexibly for the propensity to be in the high-demand group with a fourth-order polynomial of the propensity score. Figure 1.10 explores how these effects vary across the distribution of propensity scores for being in the high-demand group.

As is evident from this table, the sorting results coincide with those presented above using spousal health risk as a demand shifter. We see that in all cases, individuals classified as having higher demand for ESHI are 11 to 14 percentage points more likely to leave unemployment for jobs that offer insurance. The results are unchanged in the trimmed propensity score specifications. In terms of take-up, both groups also demonstrate increased take-up though the statistical significance disappears for the spousal take-up comparison in the second specification which restricts the sample to more observationally similar cases. This does not invalidate the approach if the sorting is happening by job type and is not also occurring on the take-up margin.

Exiting Unemployment and Unemployment Duration

Econometric Approach

I want to be able to test whether individuals with higher demand for insurance find jobs quicker, slower, or at the same rate as similar individuals with lower demand for employer-sponsored health insurance. To explore whether there is an effect on overall unemployment duration, one may want to run a regression similar to those in Section 7.1, with the weeks it takes for an individual to find a job on the left hand side and demand shifters on the right hand side. However, this approach does not take into account the limited panel nature of my data. Because the MEPS does not follow individuals indefinitely, some observations are censored and, therefore, I cannot observe total weeks of unemployment before finding a job[42]. This fact can bias our estimates, particularly, when there is non-random censoring.

[42]Since I cannot identify transitions out of the labor force, I assume everyone is either working or unemployed

In order to avoid this source of bias, we also want include the information on the amount of time unemployed for the censored observations, up to the point that they are censored. In other words, instead of only considering unemployment duration for those I see exit unemployment, the proper object of interest should be the hazard for exiting unemployment among the individuals that are still at risk of exiting unemployment (i.e. - those that are still in the survey and still unemployed at a given time).

A Cox proportional hazards model estimates how the exit rate varies with model explanators when accounting for censoring. A Cox model assumes that exit hazards (in this case, the probability of exiting unemployment at a given point conditional on having been unemployed up to that point) take the following functional form:

$$\lambda(t, x_i) = \lambda_0(t)e^{X_i'\beta}$$

where t represents the amount of time that has elapsed, X_i is a vector of observation-varying covariates, and $\lambda_0(t)$ represents the baseline exit hazard when X_i is a vector of 0's that depends only on the time an individual has been unemployed, t. These models are semi-parametric in that the baseline hazard is left unspecified and, instead, the model identifies how covariates proportionally affect this exit hazard at all points in time.

Though the model presented above does not make predictions for how overall unemployment duration is affected by differences in the taste for insurance, it unambiguously predicts that the exit hazard to jobs with insurance should be positively correlated with demand for ESHI. So, we would like to look at the differential rate at which individuals with higher demand exit to a specific type of job. My data allow me to observe two competing risks for exiting unemployment: I see whether an individual leaves unemployment for a job which offers health insurance or for one that does not offer health benefits. We want to be able to identify the joint distribution of latent failure times for each type of exit, though I only see the minimum time to a particular exit for each observation. Similar to identifying an effect on overall duration, we want to account for the fact that some observations are censored and therefore, do not provide information for which type of exit is eventually completed[43]. To summarize, I have the following information for each non-censored observation:

$$(t, i) = (min(t_i), argmin_i\{t_i\})$$

where i is the reason of exit for an observation, which can include exit due to censoring, and t is the time of exit.

One approach to dealing with competing risks is to use a Cox model to estimate the effects of covariates of interest on one type of exit, while treating the other type of failure as censored. This approach is valid when the object of interest is the likelihood of a given type of failure, in the complete (theoretical) absence of the other type. By treating the competing

[43]As mentioned above, there are only two types of exit because I do not have information for whether someone chooses to leave the labor force altogether

failure as censored, we allow for the observation to remain theoretically at risk of the other failure[44].

A competing risks framework accounts for censoring, while also allowing an observation to "fail" for more than one reason; thereby, permitting us to look at the incidence of exiting to a type of job over time. In a competing risks framework, we isolate the sub-hazard for a specific type of failure. Unlike in a Cox model, in a competing risks model we calculate the sub-hazard over all observations that have not been censored or failed to the exit of interest (Fine and Gray, 1999). The sub-hazards in a proportional competing risks model are assumed to take similar functional forms as in a Cox model:

$$\lambda_k(t, x_i) = \lambda_{0k}(t)e^{X_i'\beta_k}$$

where $\lambda_k(t, x_i)$ represents the sub-hazard for a specific type of exit, k, and $\lambda_{0k}(t)$ is the baseline sub-hazard for exiting to this type of job (still left unspecified) that depends only on the amount of time an individual has been unemployed, and β_k represents the proportional effects of covariates on the conditional sub-hazard. The competing risks model I implement assumes the failure types are independent and that there is no unobserved heterogeneity within demand types.

Exit Hazard Results

In Figures 1.9, 1.10, and 1.11, I plot the lowess regression lines for the empirical monthly sub-hazard for exiting to a specific type of employment for each comparison group. Here, the exit rate is determined by evaluating the observations still at risk of leaving unemployment at a certain point in time. In these cases, I calculate the rate at which groups leave unemployment among all non-censored and still unemployed individuals. The sub-hazards shown here do not control for any individual-level covariates but they show the general pattern I find in the results discussed below.

In Figure 1.9a, I plot the lowess empirical sub-hazards for exiting to a job with insurance across all four groups in the difference-in-differences approach. Here, we see that not having coverage is associated with a substantial negative effect on the exit likelihood to jobs with ESHI offers. Those with healthy spouses, those without coverage are significantly less likely to exit for a job with insurance than the group with coverage. My approach exploits spousal health variation within this group to understand the effects of higher insurance demand, while controlling for the effect of health variation in the covered group. We see that higher spousal health risk does not appear to have much of an effect on the sub-hazards for jobs with HI, but this negligible effect is not present for the group with sick spouses lacking coverage: those with higher expected demand for insurance. This group is dramatically more likely to leave unemployment for a job with health insurance than their healthy-spouse counterparts. I explore this difference below in a competing risks model where I can control for the effects of

[44]The results for this type of specification are similar, if less precise, to those found using a competing risk model.

individual characteristics, as well as spousal health, and lack of coverage on the sub-hazards. The corresponding panel (Figure 1.9b) depicts exits to jobs without insurance, among those still at risk of exiting to this type of job. Here, we see the opposite result, spousal health risk appears to increase the likelihood of exiting to a job without insurance, likely representing some unobserved characteristics of being unhealthy and motivating the proposed difference-in-differences research design. However, the group with unhealthy and uncovered spouses are just as likely to exit to jobs without insurance as those with healthy spouses - thereby, pointing to a decreased effect of increased insurance demand on the exit hazard to jobs with insurance.

Figures 1.10 and 1.11 show roughly similar patterns. Both higher demand groups have unambiguously higher exit rates to jobs with insurance over months of unemployment. For the spousal take-up comparison, the exit sub-hazard for the high-demand group (those with spouses that rejected their employer-insurance offer) is below that of the low-demand group for most months of unemployment. Likewise, for the revealed preferences comparison, we see that the high-demand group exits at a faster rate to jobs with insurance and at a slower rate to jobs without insurance.

The Cox and competing risk results for the main approach exploiting spousal health are presented in Table 1.6, where columns 2 and 3 present the sub-hazard ratios for exiting to a specific type of employment. Again, we see the clear pattern that individuals with higher spousal health risk and no coverage have exit hazards to jobs with insurance that are higher than those with unhealthy spouses with coverage. Conversely and implying the result on overall unemployment duration, these high-demand individuals exit at a much lower rate to jobs that do not offer insurance than other observably similar individuals with lower projected demand. The underlying effects of spousal health risk and no spousal coverage appear to work in the opposite direction by decreasing the exit sub-hazards to jobs without insurance and increasing the exits to jobs without insurance. The coefficient for the overall unemployment exit hazard, in column 1, is centered at one - the baseline hazard, though the standard errors are fairly large and we cannot reject true effects in either direction. This result implies that there does not seem to be any statistically significant effect on the likelihood to exit unemployment by an individual with higher demand, thereby implying similar overall unemployment duration among the demand groups.

In Table 1.7, I present the results from the additional two demand distinctions. I present the coefficients from two models: one in which the propensity score is controlled for using a polynomial of the fourth degree and one in which the sample is trimmed to exclude areas of non-overlap in the propensity score. The same pattern is present for the competing exits to jobs by insurance offer. However, for the spousal rejection case, we see that overall unemployment durations seem to be lower for individuals with higher demand. This result is not corroborated by the third approach I use, in which overall unemployment duration seems to be unaffected by insurance demand, as a result of the offsetting effects on exits to the two competing types of jobs.

Wages and Other Job Characteristics

In addition to explicit sorting by insurance offer, I look at how demand for insurance may affect other characteristics of the jobs taken out of unemployment. My model describes diverging reservation wages for jobs with and without insurance: as the demand for insurance increases, reservation wages for jobs with insurance should decline and reservation wages for jobs without insurance should increase. Thus, I look at whether wages are different for my higher-demand groups within given insurance-offer job types. In Table 1.8, I examine how log wages at a new job differ by demand groups from my difference-in-differences approach. We see that wages for those with unhealthy and uncovered spouses appear to be higher; but, importantly, this increase seems to be coming from jobs that do not offer insurance. Wages from jobs without insurance, on the other hand, do not appear to be lower as the model would predict.

I then explore whether the same wage effects are present for the other two approaches I consider. In the first case, wages do not appear to differ for the high-demand group. However, there appears to be an insignificant drop in wages for those exiting to jobs without insurance. These results are very noisy due to the limited number of observations I have with wage information on the next job. For the third approach, wages appear to increase overall for those with higher demand and, in particular, in jobs with insurance - contradicting the predictions from the model. This calls into question the degree of selection which may be present for these groups.

I also look at other job-characteristic margins which insurance demand may ultimately influence through job choice. For example, given that small firms[45] are significantly less likely to offer insurance to all employees, we may expect there to be an effect on a person's likelihood of being at a small-firm. Similarly, if a person is also choosing how much to work, we may see fewer part-time workers among those with higher-demand. Lastly, given that the self-employed do not have access to large-group insurance markets, there may also be effects on the likelihood of becoming self-employed, given that a person exits unemployment.

For our health shifter, I do not see any significant effects across any of these characteristics (see Table 1.10). Among the other two groups, evidence in Table 1.11 suggests that high demand individuals are around 3 points more likely to work full-time (among those who did not previously reject an insurance offer). However, this result is not found among the group of individuals whose spouses rejected their own insurance offers.

1.8 Discussion and Conclusion

The evidence presented in this paper points to a large degree of job sorting out of unemployment by demand for insurance. Across three different proxies for insurance demand, I find that the laid-off individuals with higher demand for insurance have higher hazards for exiting unemployment into a job with insurance. For two of the approaches, there is a

[45] I define these as firms with fewer than 50 employees.

distinctly lower rate of exit to jobs without insurance. These two effects combine to yield no significant effects on overall unemployment duration.

The results align with previous findings on job-lock, to the extent that they demonstrate a distortion in individuals' choices for where to work based on their provision of non-pecuniary benefits. Since individuals with higher demand for insurance may consider fewer jobs, we can imagine this resulting in worse quality job-matches thereby introducing efficiency costs to a system based around employer-provided health insurance. In other words, individuals may forego productive opportunities to instead take a job with health benefits. Though this paper does not attempt to quantify the efficiency-losses related to this distortion, we can imagine a variety of policies (some of which have been introduced in the ACA) to lessen the costs of this design.

Though this paper does not find an effect on overall unemployment duration, this may be in part to pre-existing policies already in place to serve individuals transitioning out of jobs with healthcare. COBRA allows individuals to extend their employer-provided health insurance plans for up to a year and a half after leaving an employer for any reason. However, as the MEPS data shows: of the laid-off individuals, all of which had coverage in their previous positions only 11.3% took up COBRA. A leading reason for this low-take up has to do with the fact that these plans are no longer subsidized through cost-sharing on the part of the employer. Thus, unemployed individuals who are liquidity constrained may choose to remain uninsured while searching for a job to replace their past insurance. Regardless, COBRA remains a recourse to individuals while they are searching for employment, thereby possibly lessening the pressures to quickly secure insurance through a job.

Given that the results of this paper are conditional on the presence of COBRA, this may imply that job-sorting is particularly driven by the demand for health insurance in the future or that COBRA is not an effective policy to aid the uninsured searching for work and insurance. An additional policy intervention to lessen the labor market distortions that may be caused by the need to access health insurance is to increase the outside options for health insurance by making individual plans less costly (increase the quality, lower deductibles, increase coverage networks). The ACA introduced health insurance exchanges and income-based subsidies to increase competition among private insurance plans and increase their accessibility to certain income groups. Furthermore, as mentioned before, the ACA struck down insurer's ability to deny individual's coverage based on a person's health history. In addition to the effect of lowering costs, this should lessen the dependence on an employer plan for health coverage. A question remains over whether there will be convergence in quality and plan terms, in addition to price, across private and employer plans. Guided by the results in this paper, these changes should expand the job choices faced by those with higher demand for insurance.

The results in this paper suggest that demand for insurance can be large enough to affect the job taken out of unemployment. This can be due to both immediate demand for health insurance (i.e. a lower utility from unemployment while uninsured) as well as demand for future coverage. Disentangling these two is left to further work, as is measuring the efficiency costs of a system that is reliant on the employer provision insurance like in the US.

I find that overall unemployment is not affected by demand for insurance, though exit hazards to jobs with insurance are higher for those with higher demand and exit hazards to jobs without insurance are lower. These combined effects cause there to be no difference in overall unemployment duration. The results on unemployment duration imply that though individuals with cheaper and more outside options for health insurance will be less likely to reject non-insurance job offers, they may also respond by searching less since being unemployed is not as costly and because they have more acceptable job options available.

Table 1.1: HI Offer and Take-Up by Individual and Employer Characteristics

	Offered		**Accepted — Offered**	
	Full-Time	Part-Time	Full-Time	Part-Time
Sex				
Males	0.71	0.23	0.87	0.63
Females	0.77	0.31	0.83	0.56
Age				
18-29	0.64	0.22	0.81	0.47
30-39	0.74	0.31	0.85	0.59
40-49	0.76	0.33	0.86	0.60
50-59	0.79	0.33	0.88	0.65
60-65	0.76	0.34	0.89	0.70
Education				
No Schooling Info	0.71	0.26	0.84	0.54
< than HS	0.45	0.17	0.78	0.55
Some HS	0.56	0.19	0.78	0.46
HS	0.73	0.30	0.84	0.57
Some College	0.78	0.31	0.85	0.57
College or Above	0.86	0.38	0.90	0.67
Firm Size				
No Size Info	0.72	0.25	0.87	0.61
1-9	0.41	0.14	0.82	0.60
10-25	0.69	0.27	0.79	0.47
26-49	0.79	0.33	0.82	0.49
50-100	0.85	0.41	0.84	0.54
101-500	0.91	0.49	0.88	0.62
501-1000	0.93	0.53	0.90	0.68
1000 up	0.95	0.61	0.92	0.71
Total	0.74	0.29	0.85	0.58

Notes:
Mean HI Offer and Take-Up rates for employees by sex, age-group, education, and firm-size. Sample is all employed individuals in the MEPS from 1996-2012 (N=556,243).

Table 1.2: Summary Statistics for the Laid-Off and Unemployed

	Mean	SD	Min	Max	CPS
Female	0.47		0	1	0.35
White	0.47		0	1	0.76
Black	0.19		0	1	0.16
Single	0.52		0	1	0.56
Age: 18-29	0.38		0	1	0.27
Age: 30-39	0.25		0	1	0.25
Age: 40-49	0.21		0	1	0.25
Age: 50-59	0.13		0	1	0.18
Age: 60-64	0.04		0	1	0.04
No Schooling Info	0.18		0	1	-
< than HS	0.08		0	1	0.06
Some HS	0.16		0	1	0.14
HS	0.28		0	1	0.40
Some College	0.19		0	1	0.25
College or Above	0.12		0	1	0.14
Northeast	0.14		0	1	0.21
Midwest	0.20		0	1	0.23
South	0.37		0	1	0.27
West	0.29		0	1	0.29
Past Full-Time Status	0.74		0	1	-
Past Self-Employment	0.06		0	1	-
Past Hours per Week	36.4	12.4	1	168	-
Past Hourly Wage*	10.77	6.11	2.1	97.5	-
Weeks Not Emp.	26.7	29.9	0	154.4	21.4**
Observations	23370				47926

Notes:
Summary statistics for the full set of unemployed, laid-off individuals in the MEPS from 1996-2012. CPS averages taken for laid-off and unemployed in March samples from 1996 - 2012.
* Past Hourly Wage on limited sample of N=13,124. ** CPS Weeks Unemployed capped at 99 weeks.

Table 1.3: Relationship between High Health Risk and Stated Need for Insurance

	High Need for HI	
Own High Risk	0.11***	(0.02)
Sp. High Risk	0.06***	(0.02)
Female	0.12***	(0.02)
White	0.12***	(0.02)
Black	0.17***	(0.03)
Hispanic	0.03	(0.03)
No Schooling Info	-0.09	(0.06)
< than HS	-0.06**	(0.03)
Some HS	-0.06**	(0.02)
HS	-0.01	(0.02)
Some College	-0.02	(0.02)
Age: 30-39	0.07***	(0.02)
Age: 40-49	0.07**	(0.03)
Age: 50-59	0.13***	(0.03)
Age: 60-64	0.13***	(0.05)
Observations	6852	

Notes:
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Robust standard errors reported in parentheses.
Regression is an LPM for relationship between health risk dummies and self-reported high need for HI. Regression controls for individual characteristics, panel and region fixed effects. Sample is laid-off, married individuals.

Table 1.4: Likelihood of HI Offer and Take-Up, Conditional on Exiting Unemployment

	Offered		Take-Up		Take-Up—Offer	
	NoControls	Controls	NoControls	Controls	NoControls	Controls
No Spouse Cov.	-0.218***	-0.133***	-0.147***	-0.081**	-0.018	-0.023
	(0.037)	(0.036)	(0.03)	(0.034)	(0.051)	(0.053)
Sp. High Risk	-0.056**	-0.069**	-0.037	-0.064***	-0.021	-0.067*
	(0.026)	(0.027)	(0.023)	(0.024)	(0.035)	(0.037)
Sp. High Risk x No Spouse Cov.	0.108***	0.091***	0.102***	0.094***	0.094**	0.092**
	(0.032)	(0.031)	(0.029)	(0.028)	(0.047)	(0.046)
Own High Risk	-0.014	-0.054*	-0.106***	-0.117***	-0.191***	-0.179***
	(0.030)	(0.030)	(0.027)	(0.028)	(0.040)	(0.043)
Own High Risk x No Spouse Cov.	0.050	0.043	0.145***	0.129***	0.221***	0.214***
	(0.035)	(0.033)	(0.033)	(0.031)	(0.050)	(0.050)
Observations	4606	4606	4606	4606	2037	2037

Notes:
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.
Sample is married, laid-off individuals who exit unemployment. MEPS: 1996-2012. Regressions are linear probability models for whether a job offers HI, a person takes-up HI, and takes-up HI conditional on offer. Specifications with controls include individual and spouse demographics, past job characteristics, year, panel, and region fixed effects.

Table 1.5: Alternate Approaches: Offer and Take-Up, Conditional on Exiting Unemployment

	Offered	**Take-Up**	**Take-Up — Offer**
Spouse Rejected HI			
Controls	0.142***	0.359***	0.419***
	(0.047)	(0.047)	(0.061)
Obs.	755	755	494
Pscore Trim	0.0957*	0.305***	0.368***
	(0.051)	(0.051)	(0.057)
Obs.	440	440	302
Preferences: Own Take-Up			
Controls	0.129***	0.308***	0.473***
	(0.019)	(0.016)	(0.025)
Obs.	4133	4133	2255
Pscore Trim	0.127***	0.295***	0.466***
	(0.019)	(0.015)	(0.023)
Obs.	3372	3372	1752

Notes:
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.
Regressions are LPM for job outcome. Bootstrapped standard errors reported for propensity-score regression. Samples restricted to laid-off individuals who exit unemployment to a job. The first specification controls for own an spousal demographics, own and spouse education, past job characteristics, year, panel, and region fixed effects. Second specification controls for up to fourth-order polynomial of propensity score on a sample trimmed by propensity score. Trimmed set for the Spouse HI Rejection group are observations with propensity scores between [0.1,0.9] and, for the Preferences group, between [0.2,0.9].

Table 1.6: Cox Proportional Hazards and Competing Risk Models

	(1) All	(2) Exit to Job with HI	(3) Exit to Job without HI
No Spouse Cov.	1.04	0.76**	1.45***
	(0.09)	(0.10)	(0.17)
Sp. High Risk	0.97	0.82**	1.35***
	(0.06)	(0.07)	(0.12)
Sp. High Risk x No Spouse Cov.	1.03	1.37***	0.69***
	(0.08)	(0.15)	(0.07)
Own High Risk	0.98	0.88	1.06
	(0.07)	(0.09)	(0.11)
Own High Risk x No Spouse Cov.	0.99	1.06	1.01
	(0.08)	(0.13)	(0.11)
Observations	6852	6852	6852

Notes:
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.
Exponentiated coefficients shown; significance relative to difference from 1. Sample is married, laid-off individuals. Regression models control for individual and spouse demographics, past job characteristics, year, panel, and region fixed effects.

Table 1.7: Cox Regression: Proportional Hazard for Exiting Unemployment

	Sp. Rejected HI			**Past Preferences**		
	Overall	Jobs with HI	Jobs without HI	Overall	Jobs with HI	Jobs without HI
Controls Flexibly for Propensity Score						
High Demand	1.25*	1.27**	0.92	0.97	1.28***	0.74***
	(0.15)	(0.12)	(0.20)	(0.04)	(0.08)	(0.04)
Observations	1088	1088	1088	6516	6516	6516
Propensity Score Trim						
High Demand	1.24**	1.33***	0.81	0.98	1.32***	0.73***
	(0.13)	(0.19)	(0.17)	(0.05)	(0.08)	(0.04)
Observations	610	610	610	5423	5423	5423

Notes:
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.
Exponentiated coefficients shown; significance relative to difference from 1. Regression models control for individual and spouse demographics (for the Sp. Take-Up comparison), past job characteristics, year, panel, and region fixed effects. Bootstrapped standard errors reported for regression controlling for estimated propensity score.

Table 1.8: Log Hourly Wage, conditional on Exiting Unemployment

	Log Hourly Wage	Job with HI	Job without HI
No Spouse Cov.	-0.105***	-0.027	-0.136***
	(0.029)	(0.040)	(0.043)
Sp. High Risk	-0.049**	-0.028	-0.049
	(0.021)	(0.029)	(0.032)
Sp. High Risk x No Spouse Cov.	0.055**	0.004	0.062*
	(0.025)	(0.036)	(0.036)
Own High Risk	-0.027	0.006	-0.032
	(0.025)	(0.035)	(0.039)
Own High Risk x No Spouse Cov.	0.031	0.017	0.032
	(0.028)	(0.039)	(0.041)
Observations	3851	1846	2005

Notes:
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.
Sample restricted to married, laid-off individuals who exit unemployment to a job and have reported a non-missing wage. The regressions control for own and spousal demographics, education, spousal employment, past job characteristics (including past wages), year, panel, and region fixed effects.

Table 1.9: Additional Approaches: Log Hourly Wage, Conditional on Finding a Job

	(1) All	(2) Jobs w/ HI	(3) Jobs w/o HI
Sp. Rejected HI			
Controls	0.013	-0.004	-0.123
	(0.039)	(0.049)	(0.12)
Obs.	666	456	209
PScore and Trim	0.004	-0.012	-0.129
	(0.062)	(0.070)	(0.098)
Obs.	388	278	110
Preferences			
Controls	0.0608***	0.0618***	0.0312
	(0.013)	(0.018)	(0.019)
Obs.	3666	2092	1559
PScore and Trim	0.065***	0.043*	0.041*
	(0.017)	(0.025)	(0.024)
Obs.	3007	1621	1373

Notes:
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.
The regressions control for own and spousal demographics (when applicable), education, spousal employment, past job characteristics (including past wages), year, panel, and region fixed effects. Bootstrapped standard errors reported for regression controlling for estimated propensity score.

Table 1.10: Main Approach: Firm Characteristics, Conditional on Exiting Unemployment

	Big Firm	**Full-Time**	**Self-Emp**
No Spouse Cov.	-0.050	-0.051*	-0.021
	(0.038)	(0.028)	(0.024)
Sp. High Risk	-0.020	-0.030	-0.011
	(0.028)	(0.022)	(0.016)
Sp. High Risk x No Spouse Cov.	0.043	0.020	0.021
	(0.032)	(0.026)	(0.019)
Own High Risk	-0.019	-0.073 ***	-0.026
	(0.032)	(0.022)	(0.020)
Own High Risk x No Spouse Cov.	0.010	0.045*	0.019
	(0.035)	(0.025)	(0.022)
Observations	4254	4555	4604

Standard errors in parentheses

Sample is married, laid-off individuals with spouses with health risk information.

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

Notes:

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

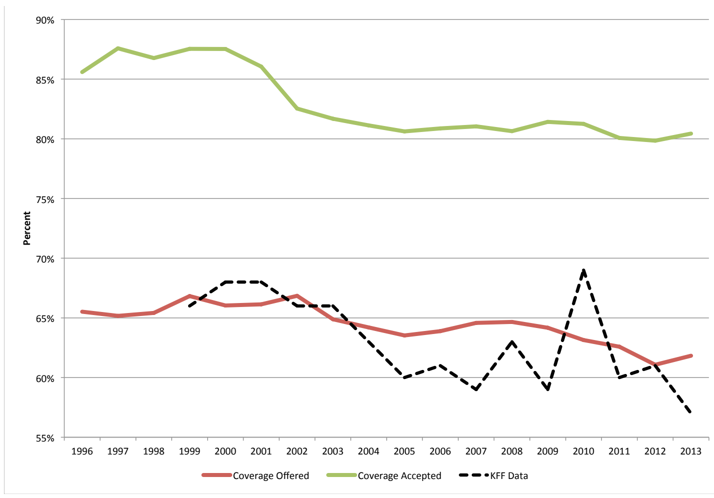
Robust standard errors in parentheses. Sample restricted to married, laid-off individuals who exit unemployment to a job. Regressions are LPMs for job outcome. The regressions control for own and spousal demographics, education, spousal employment, past job characteristics, year, panel, and region fixed effects.

Table 1.11: Alternate Approaches: Firm Characteristics, Conditional on Exiting Unemployment

	Big-Firm	**Full-time**	**Self-Emp**
Spouse Rejected HI			
Controls	-0.002	0.025	-0.005
	(0.051)	(0.036)	(0.025)
Obs.	706	770	756
Pscore Trim	0.039	0.026	-0.010
	(0.057)	(0.038)	(0.026)
Obs.	414	446	440
Preferences			
Controls	0.018	0.038**	-0.007
	(0.019)	(0.015)	(0.009)
Obs.	3820	4309	4159
Pscore Trim	0.015	0.039**	-0.006
	(0.020)	(0.017)	(0.010)
Obs.	3583	4041	3899

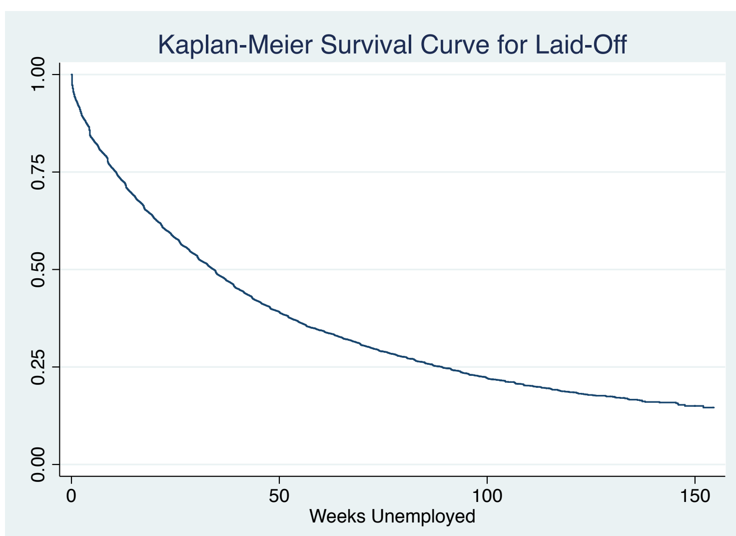
Notes:
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Bootstrapped standard errors reported for propensity-score regression. Samples restricted to laid-off individuals who exit unemployment to a job. Regressions are LPMs for job outcomes. The first specification controls for own and spousal demographics, own and spouse education, past job characteristics, year, panel, and region fixed effects. Second specification controls for up to fourth-order polynomial of propensity score on a sample trimmed by propensity score.

Figure 1.1: MEPS: ESHI Offer and Take-Up Rates over Time



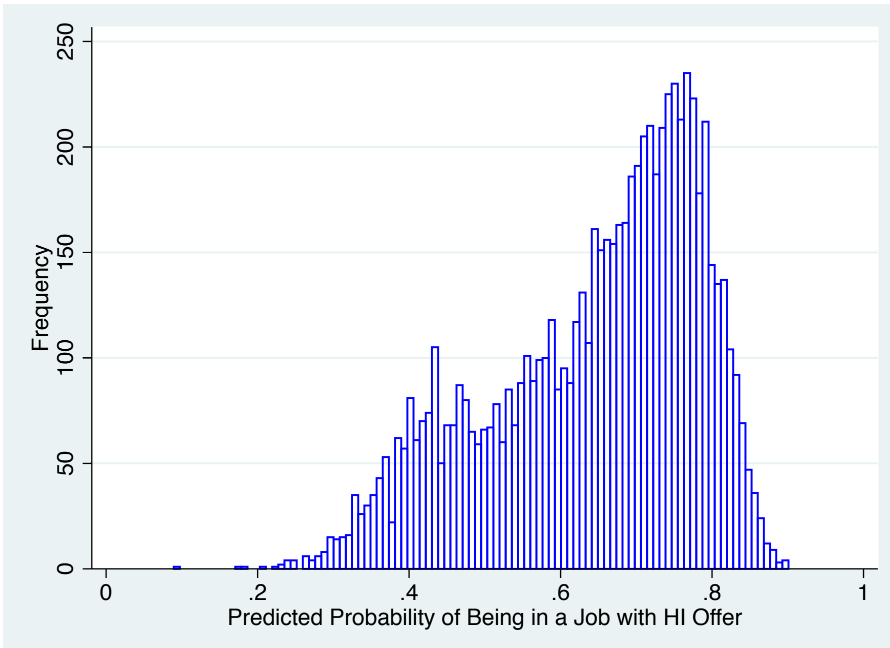
Notes: This figure shows insurance offer and take-up rates for employed individuals over time in the MEPS. I compare the offer rates to external data collected yearly KFF surveys of employers.

Figure 1.2: Kaplan-Meier Survivor Curve for Laid-Off Sample



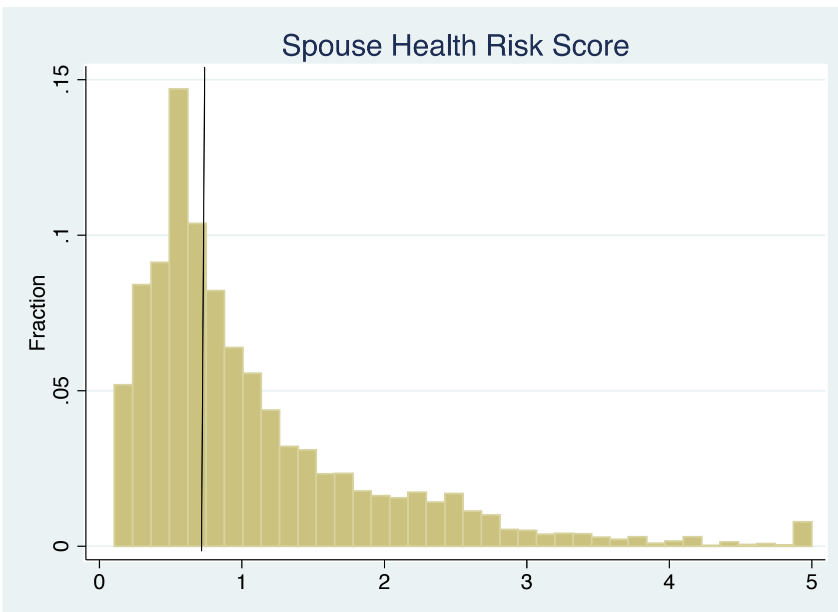
Notes: This figure shows the empirical survival curve for weeks unemployed among the laid-off in the MEPS from 1996-2012.

Figure 1.3: Probability of being in a Job with HI Offer based on Individual Observables



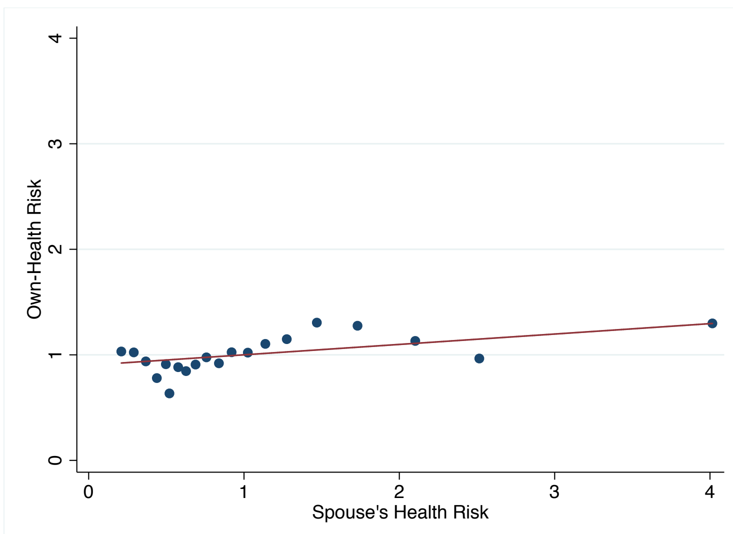
Notes: This figure shows the distribution of predicted probabilities for having a job with insurance among my unemployed sample. The predicted probabilities are based on a regression using the observable characteristics and their interactions of employed individuals in the MEPS from 1996-2012.

Figure 1.4: Distribution for Expected Spousal Health Expenditure



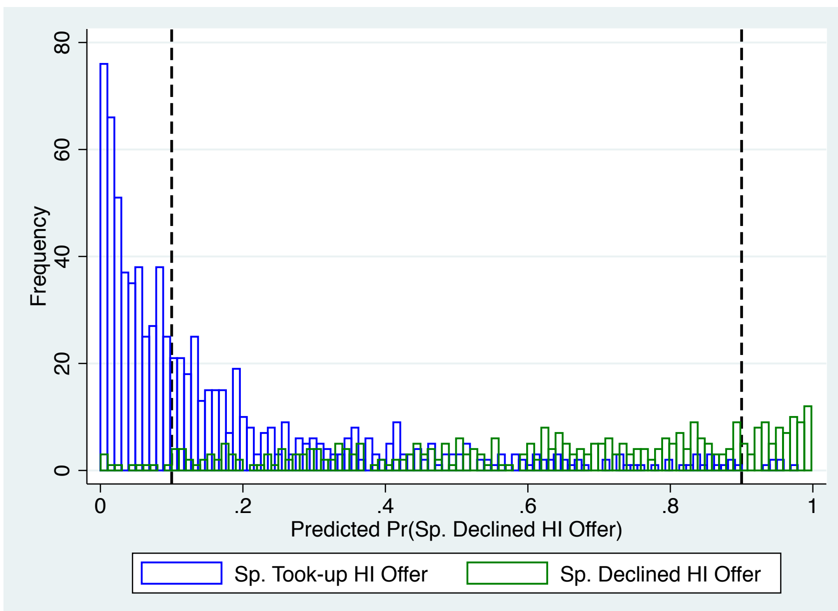
Notes: This figure shows the distribution of the health risk measure among spouses of the married and laid-off in my sample. I cap all spouses with scores above 5 to that value for the purposes of this figure only. The median spousal health risk is represented with a black line at 0.78.

Figure 1.5: Relationship between Spousal Health Expenditure and Own Health Expenditure



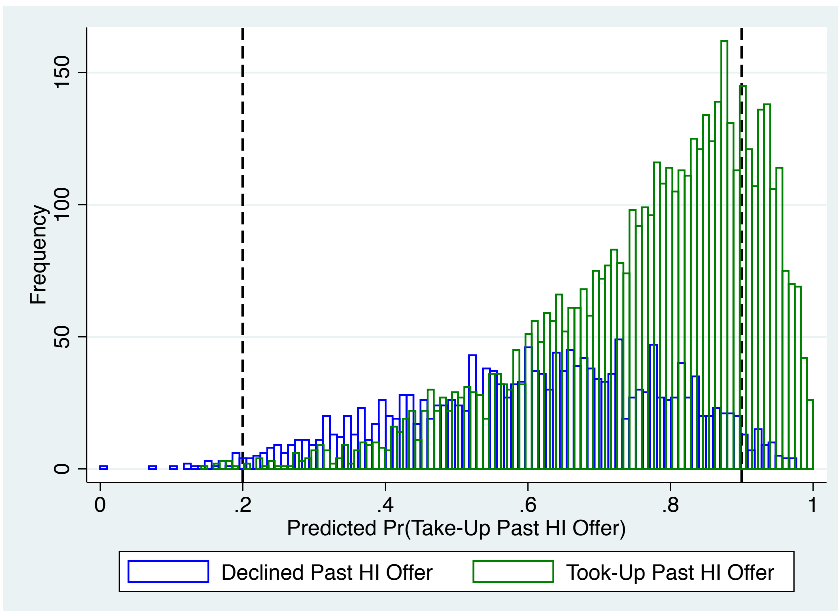
Notes: This figure shows the average individual health risk across five-percent bins of the spousal health risk measure. The correlation between the two scores, among couples with an unemployed individual, is 0.11.

Figure 1.6: Approach 2: Propensity Score Distribution for Spousal Rejection of HI Offer



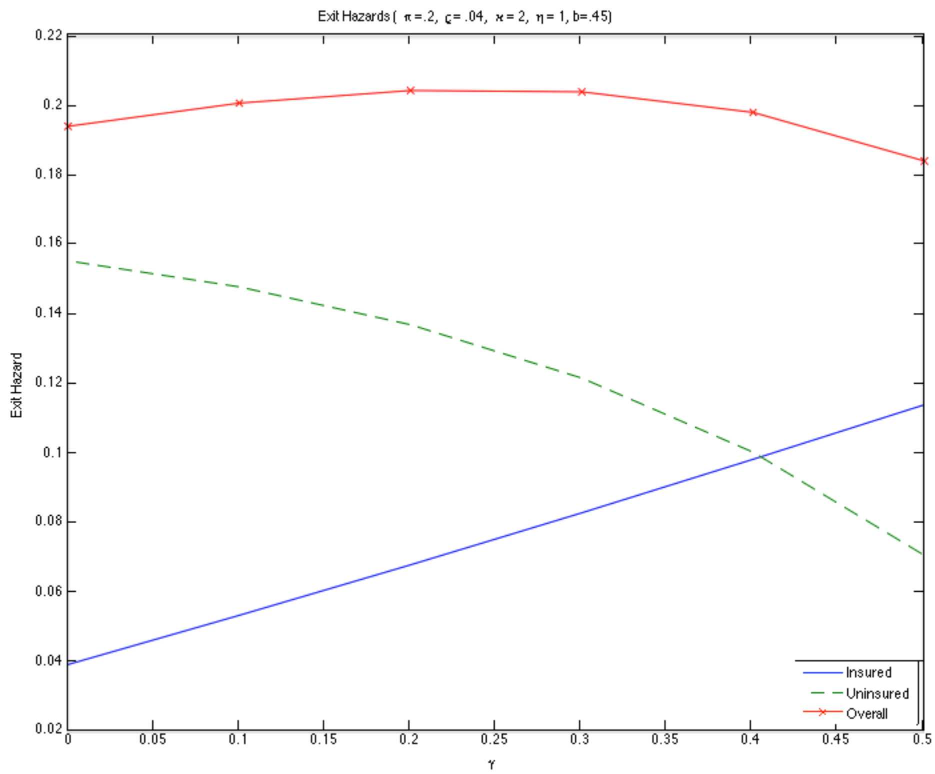
Notes: This figure shows the modeled propensity score among laid-off, married individuals with previous offers of job-based HI and with spouses that also have an employer insurance offer (N = 1175). I predict the likelihood that an individual will be in the high-demand group; that is, that a person accepts her offer and her spouse rejects his/her insurance offer vs. the spouse accepting the offer and the unemployed rejecting her offer. The propensity score is predicted by a logistic regression with detailed controls about a person's last job, her spouse's job, as well as individual characteristics, panel, year and region fixed effects. The trimmed sample is defined as those with propensity score between 0.1 and 0.9 (N=652).

Figure 1.7: Approach 3: Propensity Score Distribution for Own Take-Up of HI Offer



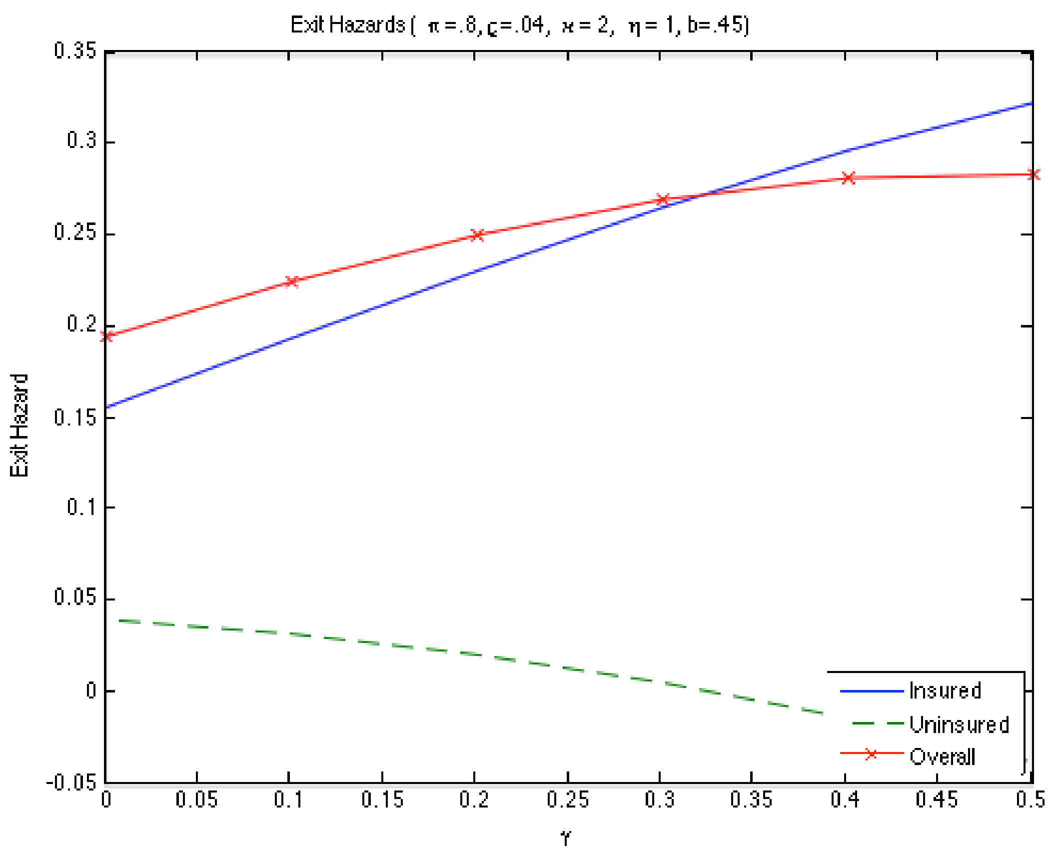
Notes: This figure shows the modeled propensity score among laid-off individuals with previous insurance offers on the last job. Sample restricted to those without spouses with insurance offers including both single and married individuals (N = 6950). I predict the likelihood that an individual will be in the high-demand group; that is, that a person rejected a past insurance offer. The propensity score is predicted by a logistic regression with detailed controls about a person's last job, her spouse's job, as well as individual characteristics, panel, year and region fixed effects. The trimmed sample is defined as those with propensity score between 0.2 and 0.9 (N=5748).

Figure 1.8: Model Exit Hazards over γ: $\pi = 0.2$

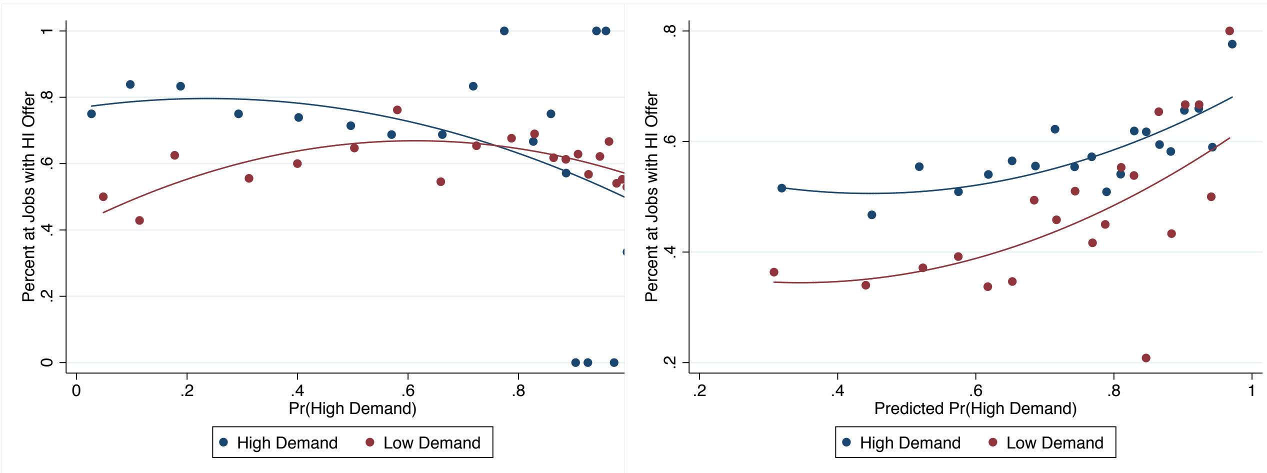


Notes: This figure plots the overall exit hazard, the exit hazard to jobs with insurance, and the exit hazard to jobs without insurance over γ, the taste for insurance, under the case where only 20% of jobs offer insurance. The hazards are derived by solving for the optimal level of search, the optimal reservation wage for jobs with insurance, and the optimal reservation wage for jobs without insurance for each γ. Other parameters in the model are set as follows: $\rho = 0.4, \kappa = 2, \eta = 1$ and $b = 0.45$.

Figure 1.9: Model Exit Hazards over γ: $\pi = 0.8$



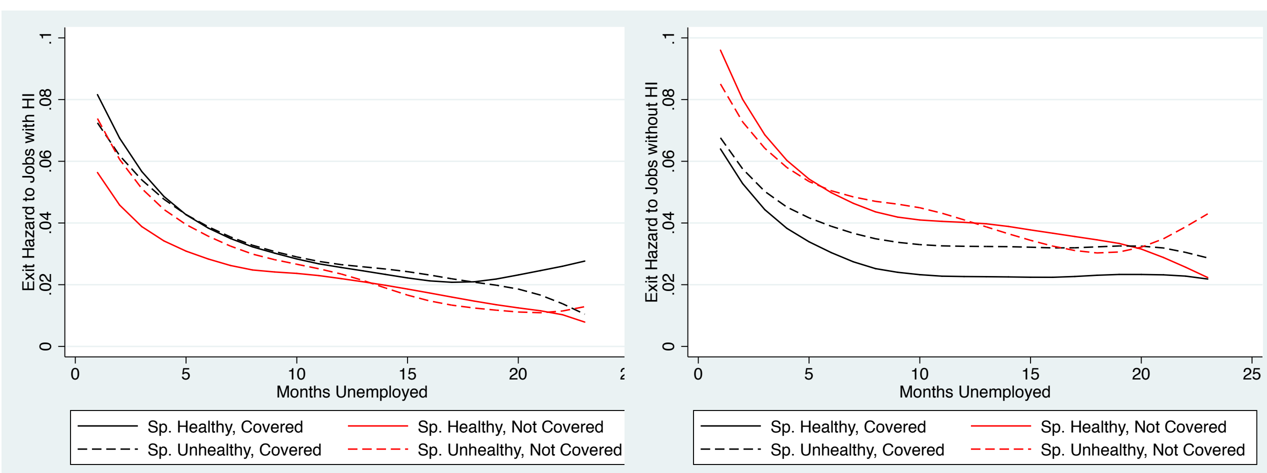
Notes: This figure plots the overall exit hazard, the exit hazard to jobs with insurance, and the exit hazard to jobs without insurance over γ, the taste for insurance, under the case where only 80% of jobs offer insurance. The hazards are derived by solving for the optimal level of search, the optimal reservation wage for jobs with insurance, and the optimal reservation wage for jobs without insurance for each γ. Other parameters in the model are set as follows: $\rho = 0.4, \kappa = 2, \eta = 1$ and $b = 0.45$.



(a) Spousal HI Rejection Comparison (b) Preferences Comparison

Figure 1.10: Effect of High Demand over Propensity Score

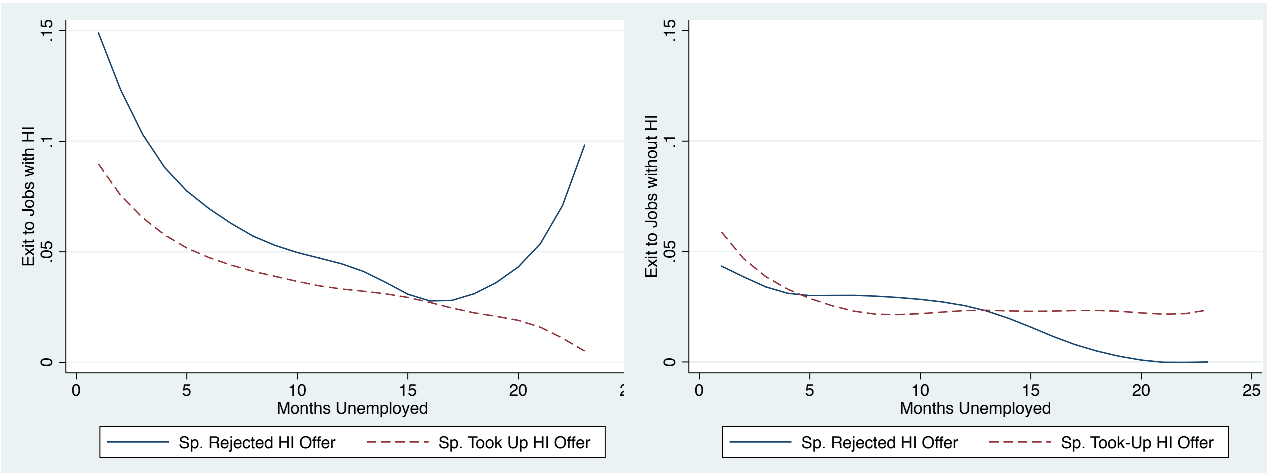
Notes: These figures plot the percent of individuals that take a job with HI within 5 percent bins of the propensity score, across the high- and low-demand groups.



(a) A Exit Hazard to Jobs with HI (b) A Exit Hazard to Jobs without HI

Figure 1.11: Competing Risks Exit for Difference-in-Differences Groups

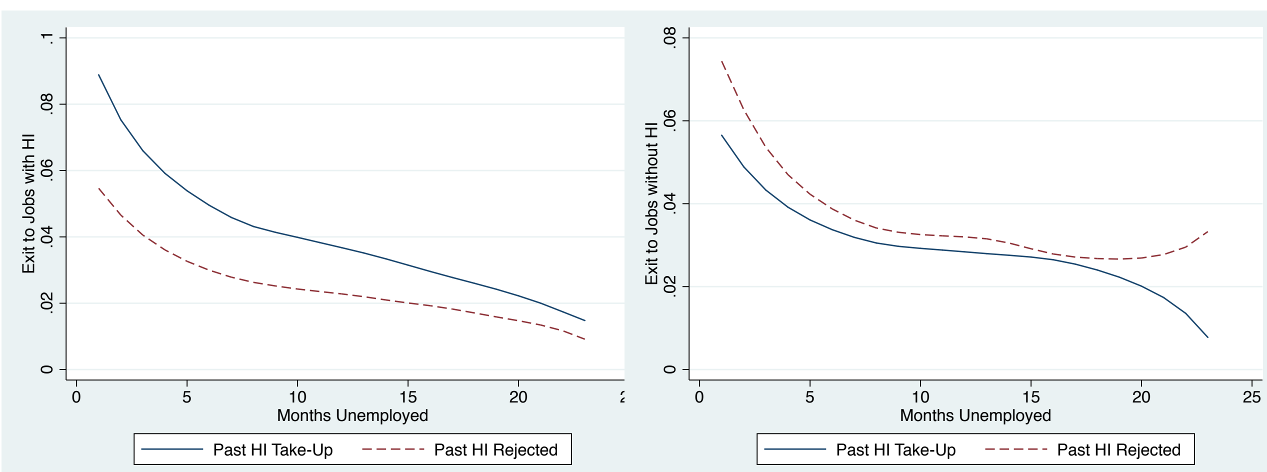
Notes: These figures plot the exit hazards by months spent unemployed across demand-types. The exit hazards are represented with a lowess regression line.



(a) A Exit Hazard to Jobs with HI (b) A Exit Hazard to Jobs without HI

Figure 1.12: Competing Risks Exit for Spousal HI Rejection Approach

Notes: These figures plot the exit hazards by months spent unemployed across demand-types. The exit hazards are represented with a lowess regression line.



(a) A Exit Hazard to Jobs with HI (b) A Exit Hazard to Jobs without HI

Figure 1.13: Competing Risks Exit for Preferences Approach

Notes: These figures plot the exit hazards by months spent unemployed across demand-types. The exit hazards are represented with a lowess regression line.

Appendix

A1. Explanation of MEPS Risk Score Design

The health-risk score used in this paper is referred to by the MEPS as relative risk scores based on hierarchical condition categories (variable name: **HCCPV**) [46]. "The relative risk score is a summary of disease burden and expected annual health care resource use at the individual level." The models used provide a score to predict prospective future health costs for each individual and have been shown to validate over time: "Studies have shown that people with higher RRS scores go on to use more inpatient hospital services, ER services and home care, and to experience higher mortality. These scores are widely employed in health policy studies, budgeting, payment, pricing, negotiation, provider profiling, disease management reconciliation, and resource planning." The scores are constructed based on information from reported health conditions collected in the surveys. Using national samples, future health expenditures are then predicted using health conditions, age and sex, and interactions of health with age and sex. The specifics behind the model and individual scores are not disclosed by the MEPS.

In terms of understanding the scores and their magnitude, a score of 1 represents a prediction of average health expenditures, where the average is based off of a nationally representative sample. Each .01 increase in a score represents a 1% increase in expenditures so that a score of two represents predicted expenditures 100% above average. I use the scores that have been normalized to the national average, rather than the sample average. To back out dollar expenditures, one must multiply a score by the average expenditure by panel provided by the MEPS, below. Thus, a one unit increase in a relative risk score represents an increase of roughly $2,300 on average for an individual in Panel 7 (2002-2003). Because the scores are normalized across the population to the average expenditure in a given year, they account for inflation in health maintenance costs.

Average Expenditure by Panel for Privately Insured

Panel	Private
1	$1,727.24
2	$1,726.42
3	$1,680.42
4	$1,569.08
5	$1,776.17
6	$2,117.51
7	$2,329.69
8	$2,575.74
9	$2,928.18
10	$2,774.81
11	$3,033.68
12	$3,338.40
13	$3,226.53
14	$3,390.57

The MEPS includes four different risk scores for each individual predicting expected costs under four differing insurance scenario: 1) having private insurance, 2) being uninsured, 3) being enrolled

[46] A full description of these scores and their construction can be found at http://meps.ahrq.gov/mepsweb/data_stats/download_data/ pufs/h140/h140doc.shtml%

in Medicaid, and 4) being enrolled in Medicare. I use the the first score as this one is the most extensive in terms of predicting future expenditures and is the basis of the uninsured relative risk score. The correlation between the private insurance risk score and the uninsured score is .98.

A2. Model Derivations

Effect of insurance demand on reservation wages:

$$w_i^* = b - \gamma - c(s^*)\frac{s^*}{\rho}\left\{\pi\int_{w_i^*}^{\overline{w}}(w-w_i^*)f^i(w)\,dw + (1-\pi)\int_{w_o^*}^{\overline{w}}(w-w_o^*)f^o(w)\,dw\right\}$$

$$\frac{dw_i^*}{d\gamma} = -1 + \frac{s^*}{\rho}\left\{-\pi\left[1-F(w_i^*)\right]\frac{dw_i^*}{d\gamma} - (1-\pi)\left[1-F(w_o^*)\right]\frac{dw_o^*}{d\gamma}\right\}$$

and

$$\frac{dw_o^*}{d\gamma} = \frac{dw_i^*}{d\gamma} + 1$$

$$\Longrightarrow$$

$$\frac{dw_i^*}{d\gamma} = -1 + \frac{s^*}{\rho}\left\{-\pi\left[1-F(w_i^*)\right]\frac{dw_i^*}{d\gamma} - (1-\pi)\left[1-F(w_i^*+\gamma)\right](\frac{dw_i^*}{d\gamma}+1)\right\}$$

$$\frac{dw_i^*}{d\gamma} = \frac{-1-(1-\pi)\frac{s^*}{\rho}[1-F(w_i^*+\gamma)]}{1+\frac{s^*}{\rho}\left(\pi\left[1-F(w_i^*)\right]+(1-\pi)\left[1-F(w_i^*+\gamma)\right]\right)}$$

$$\Longrightarrow -1 < \frac{dw_i^*}{d\gamma} < 0$$

$$\Longrightarrow 0 < \frac{dw_o^*}{d\gamma} < 1$$

Effect of insurance demand on search behavior:

$$\rho c'(s^*) = \pi\int_{w_i^*}^{\overline{w}}(w-w_i^*)f^i(w)\,dw + (1-\pi)\int_{w_o^*}^{\overline{w}}(w-w_o^*)f^o(w)\,dw$$

$$\begin{aligned}
\rho c''(s^*)\frac{ds^*}{d\gamma} &= -\left[\pi\left[1-F(w_i^*)\right]\frac{dw_i^*}{d\gamma} + (1-\pi)\left[1-F(w_i^*+\gamma)\right](\frac{dw_i^*}{d\gamma}+1)\right] \\
&= -(1-\pi)[1-F(w_i^*+\gamma)] - \left(\pi\left[1-F(w_i^*)\right]+(1-\pi)\left[1-F(w_i^*+\gamma)\right]\right)\frac{dw_i^*}{d\gamma} \\
&= \frac{\pi\left[1-F^i(w_i^*)\right]}{1+\frac{s^*}{\rho}\left(\pi\left[1-F^i(w_i^*)\right]+(1-\pi)\left[1-F^o(w_i^*+\gamma)\right]\right)} > 0
\end{aligned}$$

Lastly, we want to look at the effects of increasing γ on the exit hazards. The overall exit hazard is given by:

$$H = s^*[\pi(1-F(w_i^*)) + (1-\pi)(1-F(w_o^*)]$$

whereas the exit hazard for jobs with insurance is:

$$H^i = s^*[\pi(1 - F(w_i^*))]$$

and, similarly, the exit hazard for jobs without insurance is:

$$H^o = s^*[(1 - \pi)(1 - F(w_o^*)]$$

so that:

$$\begin{aligned}\frac{\partial H}{\partial \gamma} &= \frac{\partial s}{\partial \gamma}[\pi(1 - F(w_i^*)) + (1 - \pi)(1 - F(w_o^*)] + \\ &\quad s[\pi f(w_i^*)\frac{\partial w_i^*}{\partial \gamma} + (1 - \pi)f(w_o^*)\frac{\partial w_o^*}{\partial \gamma}]\end{aligned}$$

whether the second term in this expression is positive negative depends on whether: and

$$\frac{\partial H^i}{\partial \gamma} = \frac{\partial s}{\partial \gamma}[\pi(1 - F(w_i^*)) - s\pi f(w_i^*)\frac{\partial w_i^*}{\partial \gamma}] > 0$$

and

$$\begin{aligned}\frac{\partial H^o}{\partial \gamma} &= \frac{\partial s}{\partial \gamma}[(1 - \pi)(1 - F(w_o^*))] - s(1 - \pi)f(w_o^*)\frac{\partial w_o^*}{\partial \gamma} \\ &= (1 - \pi)[\frac{\partial s}{\partial \gamma}(1 - F(w_o^*)] - sf(w_o^*)\frac{\partial w_o^*}{\partial \gamma}]\end{aligned}$$

A.3 Inputs into Propensity Score Model

A.3.1 Approach 1

The covariates included in the propensity score model for the spousal take-up sample are as follows (interactions between covariates not listed here):

Spouse gender, spouse race dummies, spouse schooling dummies, own schooling dummies, spouse age, own age, spouse industry dummies, spouse occupation dummies, spouse full-time status, spouse hours of work, spouse self-employment status, spouse temporary job status, spouse salaried, spouse earns bonus, spouse earns overtime, spouse has retirement plan, spouse type of employment (government or private), spouse firm size, spouse hourly wage, spouse wage imputation dummy, spouse has sick leave, spouse has paid vacation, number of kids, own past industry dummies, own past occupation dummies, own past firm size, own past type of employment, own past hours per week, own past hourly wage, own wage imputation dummy, own past full-time status, own past self-employment status, own past union membership, and year, panel, and region fixed effects.

Table 1.12: Main Approach: Information on Insurance and Health while Unemployed

Among the Unemployed	NoSpRisk x Cov Mean	SpRisk x Cov Mean	NoSpRisk x NoCov Mean	SpRisk x NoCov Mean
Cobra	0.05	0.07	0.05	0.10
Dependent	0.75	0.77	0.10	0.13
Uninsured	0.20	0.15	0.56	0.46
Medicare/Medicaid	0.06	0.02	0.19	0.14
Holder of Policy in Round	0.13	0.16	0.14	0.26
Health Risk	1.19	1.33	0.99	1.26
Sp. Health Risk	0.37	1.30	0.37	1.63
Age	36.9	47.8	37.0	46.5
No Need for HI*	0.30	0.20	0.41	0.26
High Need for HI*	0.51	0.63	0.40	0.59
Observations	625	1109	784	1397

Notes: * Means calculated over a smaller subsample with non-missing values for these fields. Sample consists of married, laid-off individuals who were unemployed during survey rounds.

A.3.2 Approach 2

The covariates included in the propensity score model for the revealed preferences sample are as follows (interactions between covariates not listed here):

family type (married with working spouse, married with non-working spouse, or single), gender, schooling dummies, age, number of kids, own past firm size, own past type of employment, own past hours per week, own past hourly wage, own wage imputation dummy, own past full-time status, own past self-employment status, own past union membership, and year, panel, and region fixed effects.

A.4 Appendix Tables

Table 1.13: Summary Statistics for Spousal Health and Coverage Sample

	NoSpRisk x Cov Mean	SpRisk x Cov Mean	NoSpRisk x NoCov Mean	SpRisk x NoCov Mean	SpRisk x NoCov P-Value
White	0.54	0.66	0.35	0.54	0.01
Black	0.12	0.12	0.08	0.08	0.70
Female	0.76	0.45	0.56	0.28	0.14
< than HS	0.076	0.059	0.22	0.18	0.18
Some HS	0.10	0.094	0.20	0.16	0.17
HS	0.33	0.33	0.29	0.32	0.22
Some College	0.25	0.27	0.17	0.17	0.46
College or Above	0.24	0.24	0.11	0.16	0.02
Age: 18-29	0.23	0.08	0.28	0.14	0.59
Age: 30-39	0.41	0.25	0.39	0.26	0.22
Age: 40-49	0.29	0.33	0.27	0.28	0.25
Age: 50-59	0.06	0.28	0.053	0.24	0.12
Age: 60-64	0.00	0.05	0.00	0.07	0.08
Sp. < than HS	0.08	0.05	0.23	0.16	0.04
Sp. Some HS	0.09	0.08	0.19	0.18	0.98
Sp. HS	0.35	0.34	0.31	0.33	0.44
Sp. Some College	0.24	0.24	0.15	0.18	0.24
Sp. College or Above	0.24	0.28	0.11	0.14	0.82
Past Full-Time	0.75	0.76	0.80	0.83	0.37
Past Self-Emp	0.08	0.09	0.08	0.09	1.00
Past Hrs. per Wk.	36.5	37.2	37.9	39.4	0.21
Past Hrly. Wage*	11.8	12.5	10.1	12.0	0.01
Weeks Not Emp. **	28.6	28.7	26.5	24.4	
Predicted Likelihood of HI Offer	0.66	0.71	0.57	0.64	0.03
Observations	939	1943	1409	3141	

Notes: * Means for these variables are from a limited sample.
** P-Values for these statistics not calculated.
The table shows summary statistics for the sample of laid-off, married individuals. The groups are split by whether a spouse has high health risk and by whether a spouse has HI coverage. High health risk is defined as being in the top two-thirds of the health risk distribution. Column 5 shows the p-values for the differences in covariate averages for our group of interest. Each line is a separate regression where the covariate is regressed on a dummy for whether someone's spouse has high health risk, a dummy for whether the spouse is covered, and the interaction of these two effects. The p-value for this last coefficient is presented here.

Table 1.14: Additional Approaches: Information about Insurance Status and Health while Unemployed

Among the Unemployed	Sp. HI Rejection		Preferences	
	High-Demand	Low-Demand	High-Demand	Low-Demand
	Mean	Mean	Mean	Mean
Cobra	0.31	0.02	0.21	0.01
Dependent	0.28	0.87	0.05	0.10
Uninsured	0.15	0.11	0.26	0.55
Medicare / Medicaid	0.01	0.01	0.08	0.26
Holder of Private Ins. in Round	0.65	0.015	0.55	0.05
Own Health Risk	1.03	1.47	1.19	1.21
Spouse Health Risk	0.89	1.07		
Age	43.6	46.1	42.2	38.2
No Need for HI*	0.34	0.24	0.28	0.33
High Need for HI*	0.51	0.56	0.54	0.49
Observations	147	487	2363	1059

Notes: * Means calculated over a smaller subsample with non-missing values for these fields. Sample consists of married, laid-off individuals who were unemployed during survey rounds.

Table 1.15: Summary Statistics for Sp. HI Rejection and Preferences Methods

	Spouse Rejected HI				Preferences			
	High-Demand	Low-Demand		Trimmed w/ PScore	High-Demand	Low-Demand		Trimmed w/ PScore
	Mean	Mean	P-Value	P-Value	Mean	Mean	P-Value	P-Value
White	0.63	0.64	0.76	0.94	0.53	0.43	0.00	0.33
Black	0.15	0.13	0.58	0.65	0.19	0.24	0.00	0.87
Female	0.32	0.54	0.00	0.16	0.43	0.51	0.00	0.95
< than HS	0.03	0.03	0.44	0.93	0.05	0.06	0.03	0.94
Some HS	0.08	0.07	0.51	0.45	0.10	0.16	0.00	0.72
HS	0.31	0.31	0.65	0.62	0.30	0.30	0.86	0.71
Some College	0.25	0.24	0.83	0.61	0.21	0.19	0.04	0.92
College or Above	0.23	0.22	0.97	0.94	0.18	0.09	0.00	0.65
Age: 18-29	0.12	0.14	0.19	0.84	0.30	0.40	0.00	0.91
Age: 30-39	0.32	0.30	0.23	0.51	0.26	0.25	0.47	0.87
Age: 40-49	0.33	0.30	0.54	0.30	0.22	0.19	0.01	0.60
Age: 50-59	0.20	0.22	0.64	0.08	0.17	0.11	0.00	0.70
Age: 60-64	0.04	0.05	0.32	0.36	0.05	0.04	0.01	0.63
Past Full-Time	0.96	0.85	0.00	0.52	0.91	0.82	0.00	0.88
Past Self-Emp	0	0	0.72	0.74	0.02	0.01	0.00	0.89
Past Hrs. per Wk.	42.7	39.7	0.00	0.96	40.9	37.4	0.00	0.96
Past Hrly Wage	19.2	14.9	0.00	0.35	15.3	11.4	0.00	0.70
Sp. < than HS	0.03	0.02	0.61	0.93				
Sp. Some HS	0.08	0.06	0.10	0.30				
Sp. HS	0.31	0.30	0.60	0.79				
Sp. Some College	0.25	0.21	0.21	0.64				
Sp. College or Above	0.23	0.24	0.36	0.82				
Weeks Not Emp.	21.1	29.5			22.4	28.9		
Predicted Likelihood of HI Offer	0.71	0.69	0.01	0.97	0.68	0.63	0.00	0.55
Observations	366	809	1175	652	5056	1895	6951	5748

Notes:
The table shows summary statistics for the samples defined by my second and third approaches. The Spousal HI Rejection approach looks at laid-off with previous on-the-job coverage and spouses with HI offers. The high-demand group are those whose spouses rejected an insurance offer. The Preferences approach looks at laid-off individuals who were previously offered HI on the job. The high demand group is the set that accepted a past HI offer. For each group, the table shows the p-values for the differences in covariate averages across high and low-demand individuals. Each cell is a p-value from a separate regression where in column 3 and 7, the covariate is regressed on a dummy for whether someone is in the high-demand group. In column 4 and 8, the covariate is regressed on a high-demand dummy and a quartic of the propensity score among the trimmed set of observations where the trimming is determined by regions of overlap on the propensity score distribution.

Chapter 2

A Spoonful of Sugar: Experimental Evidence on the Demand for Health Insurance

with Peter Backof

2.1 Introduction

The success of President Obama's legislation to revamp the American health insurance system depends in part on the response of the 42 million individuals who began 2014 without health insurance (Smith et al., 2014). Under the Patient Protection and Affordable Care Act (ACA), starting in 2014, every person is legally required to have insurance coverage or pay a fine that is a fixed percentage of their income, instead. Health experts and the media have warned that without the involvement of healthy individuals that use less healthcare, the costs of insurance could spiral out of control - especially given the new provision that insurance companies can no longer deny coverage to individuals based on their health conditions. This raises the question of what will drive the previously uninsured to purchase insurance, and, more specifically, what arguments and tools can be used to increase a person's likelihood of taking up insurance coverage?

Generally, individuals that do not qualify for substantial subsidies to their insurance premiums will find it cheaper to remain uninsured and pay a yearly fine than to pay for an insurance plan (not accounting for any savings that come through health care use while insured). Thus, the key to predicting insurance coverage rates and to motivating take-up among the uninsured relies on understanding the factors that determine the demand for health insurance.

This paper presents the findings from a large-scale online messaging experiment run in December of 2013 to determine the effectiveness of different messages intended to encourage visiting an insurance marketplace and insurance enrollment. With Enroll America (EA)[1], we tested a combination of five different treatments and measured their impact on self-reported outcomes related to the demand for an insurance plan on the exchanges. Explained in more detail in Section 3, the messages emphasized either the expected savings; expected costs; expected financial risk of remaining without insurance; the new mandate; or referred to the

[1]Enroll America is a non-partisan non-profit dedicated to maximizing the number of Americans who are enrolled in and retain health coverage.

upcoming changes with the more politically motivated term “Obamacare”. For each of these messages, we measured message effectiveness on intention to enroll, likelihood to purchase a plan for a given price, and interest in receiving more information.

In addition to testing the reception of a host of informational messages, our data allows us to recover demand curves and price-elasticities for an insurance policy for an individual on the marketplaces. By polling both insured and uninsured respondents, we present some of the first comparisons of elasticities among these two groups and compare our findings to other estimates for the price-elasticity of demand among the uninsured.

Among the uninsured, we find that our cost-calculator treatment, the risk treatment, and the mandate treatment increased intention to purchase insurance on the healthcare exchanges. This makes sense as these treatments were the most relevant to the choice to remain uninsured. We find that, with the exception of our mandate treatment, our messaging treatments did not have any effects on the insured. This lends credence to our interpretation that our messages actually affect an individual's likelihood to purchase insurance, rather than just affecting informedness or the way people responded to our survey questions, in general, after being exposed to a treatment.

Similarly, we explore how the treatments affected other outcomes such as willingness to pay, informedness, and an individual's likelihood of signing up for more information by email. The cost-calculator increased self-reported informedness, while decreasing the likelihood that individuals were willing to provide their emails for continued contact. The only other treatment to increase informedness was the risk treatment. The case for the cost-treatment is ultimately made by its additional effect on willingness to pay for insurance. When paired with the savings treatment, the cost-treatment increased the uninsured's willingness to pay at all prices below and including $350 per month.

Lastly, using demographic information and self-reported willingness to pay, we construct elasticities of demand for These elasticities are larger than those found in past literature, but generally align in terms of signaling which demographic sub-groups have more inelastic demand for health insurance.

The paper is structured as follows: in Section 2, we present related research; Section 3 summarizes the experiment and its design; Section 4 describes our online sample and how it compares to other commonly used national samples; and, lastly, Section 5 presents the experimental results and findings from the data we collected on respondents and Section 6 concludes.

2.2 Related Literature

There is a large base of work that focuses on identifying the price elasticity of demand for health insurance take-up; these papers take advantage of observational data (Marquis and Long, 1995), imputed insurance prices (CBO, 2005; Polsky et al., 2005), program changes and policy interventions (Gruber and Poterba, 1994; Gruber and Washington, 2003), and, occasionally, experimental interventions (Kronick et al., 2008; Royalty and Hagens, 2005; Manning et al., 1987) to recover how much demand increases with decreases in the price of insurance. These estimates differ based on the population studied as well as the context in which individuals are choosing whether to buy insurance. As a recent description of this literature points out, the estimates for own-price elasticity of health insurance on the non-group market point to inelastic demand for health coverage, ranging from -0.1 to -0.8 (Liu and Chollet, 2006).

In general, the populations studied have been individuals with employer-provided health insurance options - thereby limiting the extent to which we can apply these estimates to contexts like the new health insurance exchanges for private non-group policies and to the policy question of how much prices need to be lowered to increase insurance rates among the types of individuals that make-up the majority of the uninsured. There appears to be significant variability in the price-elasticities of demand for different groups of individuals. For example, previous findings that have been able to explore heterogeneity suggest that low-income individuals are more price sensitive than higher-income groups (CBO, 2005). However, there is very limited knowledge about the demand among individuals with limited means and less access to insurance (Liu and Chollet, 2006).

In some of the only work that focuses specifically on the uninsured, Krueger and Kuziemko (K-K) design a survey to recover elasticities for subpopulations of the uninsured (Krueger and Kuziemko, 2013). Their study finds price elasticities that are much larger than those found in most previous studies. K-K argue that past estimates are biased downwards due to measurement error that comes from a) imputing price of insurance b) comparing different insurance policies as if they were all the same. Secondly, by focusing on the uninsured, like we do, their sample is substantially poorer and is ultimately facing a different decision with respect to insurance take-up than those receiving employer offers. In terms of expected adverse selection on the insurance marketplaces, K-K find that less healthy individuals have lower price elasticities but find no evidence that they would be more likely to enroll in new insurance plans, with or without the mandate. Our paper uses the elasticities estimated in that paper as a point of comparison.

In addition to understanding how price limits the demand for health insurance, this paper is interested in exploring other reasons individuals remain uninsured. We can then target interventions to address these determinants (many of which affect willingness to pay a given price for insurance). In other contexts, it is commonly recognized that Medicaid and SCHIP take-up is and has remained unexpectedly low (Remler and Glied, 2003; Currie, 2004). One commonly discussed theory for the low take-up rates argues that individuals do not have enough information about the programs, their benefit, and how to enroll. The costs to gaining this knowledge may often be high enough to deter enrollment (Currie, 2004). Along these lines, there have been studies that have demonstrated that information can increase the receipt of public benefits like Food Stamps (Daponte et al., 1999). On the other hand, Chernew et al. (2007) estimate a modest \$20 value of health insurance plan report cards among individuals. Along these lines, they find that the information does not cause many individuals to switch out of their current plans; though this, in part, may be due to large switching costs (Chernew et al., 2007). This is consistent with Jin and Sorenson's findings that new-enrollees were most affected by plan information (Jin and Sorensen, 2006).

Others cite evidence for the mis-appreciation of the value of health coverage and health benefits (Baicker et al., 2012; Pauly and Blavin, 2008), mis-perception of risks, and undervaluation of the potential costs to remaining uninsured (McGuire, 2012). For example, Chernew et al. (2008) find that reducing the cost of already cheap, highly-effective, and necessary medication to treat chronic illnesses increases their adoption and their continued use(Chernew et al., 2008). This research points to the sensitivity costs in the healthcare context, as well as possible mis-evaluation of the benefits of treatments. In a context with great uncertainty, like health, individuals who do not appropriately calculate the value of medical care will also mis-measure the benefits of buying health insurance (Arrow, 1963).

We are also interested in how motivations about the benefits of having insurance, its cost, and the risks to not having it compared to the ACA's newly-imposed individual mandate, a legal requirement that individuals who can afford insurance be covered by a health plan or pay a fine. Previous research has found that the mandate had a significant causal role in improving risk-selection in insurance plan enrollment, particularly compared to other interventions like offering subsidized, community-rated insurance (Chandra et al., 2011).

With this past research in mind, our messages, which are explained in detail below, were designed with the intention of increasing awareness and information of the health-care exchanges; updating beliefs on the risks to remaining uninsured as well as costs of insurance on the new healthcare marketplaces; and emphasizing the legal requirement to have insurance (the mandate). Overall, the messages serve to test the value of different types of information among the uninsured and towards the goal of increasing insurance rates in the US.

2.3 Experiment

The online experiment described below was designed to gain insight into what communication strategies most effectively motivate marketplace-eligible uninsured and under-insured individuals to become interested in purchasing health insurance. In the experiment, respondents were randomly shown one of several different messaging frames and were asked a series of questions on their interest in purchasing health insurance and learning more about how to do so. The responses were analyzed in comparison to a control group that saw no message frame.

We collected data from December 11 – 29, 2013. The experiment fell a month into the first four-month open enrollment period for the new insurance exchanges.[2] The samples were recruited from one of two sources (MechanicalTurk, a.k.a MTurk, or SSI). MTurk is Amazon's online task forum in which employers can advertise "jobs" with a description and expected duration for a given wage. In the past few years, MTurk has become a popular way to conduct online experiments due to its ease of use and ability to recruit large and relatively diverse

[2]The open enrollment period began on October 1st and ended on March 31st, 2014. This means that some individuals surveyed may have already been aware, visited, and possibly signed up for insurance at the marketplaces. In fact, 44% of our survey respondents had been online and tried to sign up for healthcare coverage; only 24% of those who had tried had succeeded by the time of the experiment.

samples for low costs.[3] The other survey platform we used is Survey Sampling International (SSI), which serves as a survey platform and sample source.[4] Though more expensive per survey response, SSI allows you to specify the composition of your sample. We used SSI in order to increase the representation of the uninsured and of minorities.[5] This is because EA wanted the experiment to yield a message-frame to use to contact the uninsured in the US and because it was interested in having enough precision to determine the effects of two of the messages on minorities.[6]

As a result of needing to test some of the treatments only on minority respondents, the experiment was designed as a stratified experiment where an individual's likelihood of being in a treatment depended on their race. In other words, treatments were randomized conditional on a respondent's race. Whereas non-minorities were randomized into seven possible cases (including a control case), minorities were only randomized into four possible treatments (two of which overlap with non-minorities). Table 2.4 presents a respondent's theoretical likelihood of being treated by each message based on his or her race; as well as the actual treatment distribution among respondents. [7].

All individuals were guided through the same introduction and then presented with their randomly assigned message treatment (the exact wording for the introduction and messages is presented in the Appendix). The full set of treatments is described below, with the message text presented in the Appendix. Survey takers that were randomized into the control case were taken directly to the questions. In order to test whether interacting treatments had additional effects on an individual's interest in becoming insured, three of the treatments were combinations of two of the individual treatments. Thus, aside from the control scenario, the full set of treatments used among non-minorities were the:

- **Cost-Calculator:** Developed in-house by Enroll America, the calculator gave people specific, personalized price information based on their location, income, and family-size about the subsidies they were eligible for under the ACA and their expected out-of-pocket cost of insurance for a plan on the insurance exchanges. [8].

- **Savings Treatment:** The savings message frame presented insurance purchased through the exchange as a good deal financially and highlighted new expected savings.

[3] MTurk been found to elicit more demographically diverse samples than other internet recruitment techniques, though not necessarily more representative (Buhrmester et al., 2011; Berinsky et al., 2012)

[4]http://www.surveysampling.com/

[5]In this paper, minorities are defined as blacks and Hispanics.

[6]Though the treatment messages differed for minorities and non-minorities, to test treatment effects we pool all observations and test for treatment effects among the full sample, while controlling for race.

[7]The survey platform itself didn't let you develop random assignments beyond integers, and there were 7 distinct treatment branches for the non-minority population. As a result, we chose to assign 16% of respondents to the control and 14% to each of the other 6 treatments

[8]The cost-calculator gave individuals a range of prices: the high end of the cost range is based on the premium for the second-lowest-cost silver plan available in the rating area associated with the ZIP code provided. The low end of the cost range is based on the premium for the lowest-cost bronze plan available in the rating area associated with the ZIP code provided

The motivation behind this message was to highlight the reduced cost of insurance for most people and to test the effect of showing people how much money they could expect to save in general, relative to what they would pay without financial assistance before the ACA.

- **Risk Treatment:** The risk message frame explained that tens of millions of Americans will spend at least $5,000 in health care expenses every year. The idea behind this message was to motivate uninsured people to get health insurance by both informing them of their average expected costs and their potential liability if they do get sick or injured.

- **Mandate Treatment:** The mandate message frame informed respondents of the existence and specific details of the mandate. It was designed to make a strong case for the mandate by mentioning specific dollar amounts and making explicit the trade-off between paying the penalty and purchasing health insurance.

- **Combination of Cost-Calculator and Savings Frame Treatment:** In this treatment, respondents were exposed to both the savings message frame, as well as the cost calculator. The idea behind this combined treatment was to test whether the savings frame could be reinforced by explicit information on an individual's expected costs of insurance.

- **Combination of Risk Frame and Mandate Treatment:** In this treatment, we combined both "negative" message frames which outlined the potential costs of remaining uninsured.

In addition to the control scenario, minorities were randomized into the following three treatments:

- **Cost-Calculator:** Described above.

- **Obamacare Treatment:** The Obamacare treatment explains the Affordable Care Act, but rather than describing "health insurance marketplaces" the language instead refers to "Obamacare". The motivation was to test whether some individuals might have greater recognition and favorability towards "Obamacare" than to other terms for the ACA.

- **Combination of Cost-Calculator and Obamacare Treatment:** In this treatment, respondents were exposed to both the cost calculator, as well language referring to the ACA as Obamacare.

The four outcomes we collected are described below with the exact wording presented in the Appendix:

- **Interest in purchasing insurance:** Respondents were asked how likely they were to buy insurance through the marketplaces, without having to select a price they would pay (Almost Certainly, Probably Will, 50-50 Chance, Probably Will Not, and Definitely Will Not).
- **Willingness to pay for insurance:** Respondents indicated the most they would be willing to pay for health insurance through the marketplaces, with the smallest possible amount set at $83.
- **Signing up for our email:** We presented respondents with information about Get Covered America[9] and asked if they were interested in signing up for an email list providing more information. We interpret this outcome as demonstrating interest in receiving more information and as a possible step towards buying insurance on the exchanges. On the other hand, not signing up for the email list could represent feeling sufficiently informed or could be because individuals with high intentions to purchase insurance on the exchanges go directly to these websites or search for their questions directly. Thus, this outcome is relevant for information campaigns that are trying to grow their email lists to eventually re-contact and possibly follow individuals.
- **Awareness of the ACA's impact:** Respondents were asked if they felt informed about how the ACA affects them and their families. By recording self-reported awareness of the ACA and its requirements, this outcome serves as another way to measure impact by looking at whether subjects walk away feeling more informed about the new regime. Furthermore, it serves to explore whether effects on email signup are at all related to how informed a message makes an individual feel.

2.4 Sample

We used two sources to recruit an online sample of 12,353 respondents aged 18 to 64, of which 7,276 (59%) reported being uninsured and another 1,519 described being under-insured (i.e., individuals who responded yes to the question: "Are you concerned about keeping your current insurance over the next year?").[10] All information provided in the surveys, including insurance status, is self-reported and unverified. Given that we did not want to encourage false participation in the survey or bias responses by signaling our population of interest, we did not restrict our survey to only uninsured respondents. In some cases below, we compare the effects on the insured to those of the uninsured group.

The summary statistics for the samples from each survey source are presented in Table 2.1, as well as how the characteristics compare to the average individual in the Current Population Survey. We also present the summary statistics for our uninsured group. While

[9]Get Covered America is Enroll America's campaign to raise awareness and engage consumers about the changes in their health insurance options as a result of Obamacare.

[10]Sample sources are described above in Section 3

the respondents in this survey differ from the general uninsured population—in particular over-representing the unemployed, those with lower-income, and those who rate their health fair or poor—the large sample size allows for detailed comparisons of enrollment experiences between various subgroups within the sample. Our samples significantly under-represent Hispanics and overrepresent whites.[11] The uninsured in our online samples look more similar to the average uninsured respondent in the CPS. We focus most of our results below on those reporting to be uninsured.

We see that MTurk is significantly more likely to survey uninsured individuals than a traditional national sample - making it a useful platform to get information from this population group.[12] The demographics represented in our sample are consistent with the those commonly observed to populate MTurk (Buhrmester et al., 2011; Ipeirotis, 2010). Furthermore, while the sample was not designed to be representative and therefore should not be generalized to the full adult population, several results from this survey closely track those from a nationally representative poll of uninsured adults conducted by PerryUndem for Enroll America during the same time period.

Treatment balance by individual characteristics are presented in Appendix Tables 2.14 and 2.15. These tables confirm that the online treatment randomization worked and individual demographics are not overly represented among any one treatment. The cases in which certain characteristics dominate in a treatment are few, represent very small increases in the probability of being treated by the given treatment, and their frequency falls within the bounds of normal statistical chance.

Before presenting respondents with a treatment, we asked several factual questions about the ACA. In addition to helping us measure a basic level of information among key target populations. We found that the uninsured were in the dark about the ways the ACA would impact them. Over 30% of those without insurance did not know there would be financial assistance (i.e. - subsidies) to help low-income individuals buy insurance, 18% were not aware of the healthcare exchanges (compared to 10% of those with insurance), 13% were not aware there was a mandate to purchase insurance (compared to a similar rate among the insured), and, startlingly, only 18% knew that the deadline to secure insurance was in March. These statistics demonstrate the importance of knowing how best to reach and inform the uninsured about how the ACA and the changes it imposes.

Our data allow us to explore how insurance status and individual characteristics affect an individual's stated likelihood to purchase private health insurance through the insurance marketplaces. Table 2.2 illustrates that the uninsured and the underinsured are significantly more likely to state positive intention to purchase insurance on the exchanges. We compare the likelihood to purchase insurance for all respondents and just for the control group, so as to not conflate the effects of our messaging treatments on intention to buy insurance. Though the uninsured signal more interest in the exchanges, this is not the case for individuals who

[11]This is one of the main reasons we resorted to collecting additional responses from SSI. SSI allows you to increase sampling weights for specific individual characteristics.

[12]The higher rate of uninsured in the SSI sample is by construction.

self-report as unhealthy. In fact, we see a slight negative correlation between poor health and likelihood of purchasing insurance through the new insurance marketplaces - thereby, suggesting that adverse selection may not have driven the large increases in enrollment seen since 2014.[13]

Our cost treatment was designed with the assumption that there is wide mis-information about how much insurance actually costs and that unrealistic expectations about costs may prevent individuals from visiting the healthcare exchanges or from becoming insured. As a result, we collected information about individual's beliefs about the average cost of an annual insurance policy for an individual policy for a person in earning \$30,000 a year.[14] Using an OLS regression to control for correlated attributes among individuals, Table 2.3 demonstrates how expectations about the cost of insurance vary with individual characteristics. Tellingly, the unhealthy, uninsured, and older predict insurance rates to be higher by an average of \$60-70 per year. Household size increases predicted rates; whereas, household income dramatically decreases predicted rates. Minorities estimate the cost of insurance to be lower, as do men.

As theorized, we see that beliefs about the costs of insurance influence intention to purchase insurance. Table 2.5 demonstrates that, among the control group, those with lower expectations for the cost of a family insurance policy are much more likely to answer that they will almost certainly buy health insurance and much less likely to say they will definitely not buy insurance on the exchanges (and vice versa for those with higher expectations). This is evidenced by the decreasing coefficients as the cost beliefs rise in columns (1) and (3); and the increasing (though not purely monotonic) coefficients in columns (2) and (4). Notably, these effects appear to be even stronger among the uninsured, possibly indicating more price sensitivity. We explore these issues further below when we study the cost-calculator treatment, which informed people of their actual costs, and price-elasticities of demand for different sub-groups of our sample.

2.5 Results

Intent to Purchase, Receive More Information, and Informedness

Below, we present the results from the insurance messaging experiment. The linear probability models we run take the following form:

$$S_i = \beta_t T_{ti} + \Gamma X_i + \epsilon_{ti} \tag{2.1}$$

[13]Among the uninsured, this negative difference remains and is significant at the ten percent level when we control for individual attributes. Among the full sample, the difference disappears with controls.

[14]Given that this person earns around 250% of the federal poverty level, he or she would be eligible for subsidies on the exchanges which would result in a monthly cost of insurance around \$208. 42% of respondents estimated a cost higher than this amount. 36% of respondents estimated a monthly cost that was half of the correct amount. Overall, over 65% were off by more than \$100 (either above or below), demonstrating the wide variation in beliefs about insurance costs preceding the first year of Obamacare. Individuals earning around this amount were only slightly more likely to guess near the correct value.

where T_{ti} is a dummy variable representing whether person i received treatment t and β_t represents the effect of this message, t, on a given survey outcome, S_i (coded as a dummy). These regressions control for individual characteristics[15], X_i, as well as survey-date and survey-source controls. Since this is a randomized experiment, we have that:

$$E[T_{ti}|\epsilon_{ti}] = 0$$

which ensures that β_t represents the causal effect on an outcome of seeing a given message.

As mentioned above, treatments were randomized, conditional on a respondent's race. Given that minorities and non-minorities were represented in both the control group and the cost treatment, we can pool all observations and identify treatment effects as well as the effect of being either black or hispanic on an outcome. We can analyze the effect of the treatments in a pooled regression if we assume that the effect of being black or hispanic does not vary by treatment (in other words, that there is no treatment heterogeneity by race).

Intent to Enroll

In Table 2.6, we present the overall effects of the experiment on an individual's stated intention to "almost certainly" buy insurance at the new healthcare marketplaces.[16] The results on the overall sample are compared to the effects on the uninsured, our main population of interest.

Table 2.6 shows that, among everyone surveyed, the most effective message treatment was the mandate treatment. Being exposed to this treatment increased an individual's likelihood of stating the highest intention to buy health insurance by 26 percent (2.2 percentage points). When we focus on the uninsured, our population of interest, we see that the cost-calculator and risk treatments were also effective at increasing an individual's likelihood to "almost certainly" purchase insurance. This makes sense given that the cost of a plan on the exchanges and the financial risks of not having insurance are significantly more relevant to this population than to those who already receive or pay for coverage. Among this set of respondents, the effects of the two treatments (cost-calculator and the risk treatment) are around 3 percentage points (roughly a 25% increase). The effects of all treatments are not statistically different from each other. In these two regressions, we see the fairly general finding that combining treatments is not more effective than exposing an individual to a singular message. This is important given that there may be additional time and money

[15]All regressions control for the following individual characteristics: sex, race, age buckets, household income buckets, household size, and region.

[16]Given that this outcome has five possible values, testing effects on survey response can take many forms. Here, we look at the effect on the strongest intention to enroll in insurance as we expect this to correlate the most with actual purchase behavior (as self-reported voting intention correlates highly with actual vote turnout). It is also possible to study the effect in different ways such as measuring changes in the likelihood of responding that a person "Definitely Will Not" enroll in health insurance on the marketplaces (particularly important to measure if any strategies have adverse effects) or measuring changes in other parts of the response distribution (i.e. - more people in the top three responses, top two, etc.).

costs to delivering more than one message - and these results do not point to any increased effectiveness from doing so.

We can use the data on the price quotes individuals received through the cost-calculator to explore whether learning about prices is uniformly positive. In Table 2.7[17], we see that among the full set of respondents the increase in purchase intentions comes exclusively from individuals who were quoted lower than average prices.[18] Individuals who were given high price quotes do not vary their purchasing intentions. However, among the uninsured, this does not appear to be the case. Though the coefficient is not precise, uninsured individuals who received a high quote for their cost of insurance appear no less likely to state that they will go buy insurance on the exchanges. Thus, providing information to individuals about insurance prices does not appear to be counterproductive.

Interest in Receiving More Information & Feeling Informed

Another method of measuring message responsiveness and commitment to becoming insured is an individual's interest in joining Get Covered America's email list to receive more information.[19] This outcome also serves as a way to measure changes in behavior that may signal increased demand rather than focusing solely on changes in stated intentions and opinions. Beyond measuring active interest, we were interested in learning which types of messages are best at getting people to provide their contact information so as to encourage repeated involvement and track individuals after their first contact. Though email sign-up demonstrates strong interest in learning more and receiving reminders, it is also possible that individuals who are convinced they will buy insurance on the exchanges will actually be less likely to sign up for email messages because they feel they no longer need more information on how to enroll or because they plan to act immediately and do not value reminders. We test this possibility by also collecting information on how informed individuals respond being after seeing their message.

Columns 3 and 4 in Table 2.6 show the effects of our treatments on email sign-up - the results are the same for our full sample and just the uninsured. Here, we see that being exposed to individualized cost estimates for insurance based on a person's age, income and household size decreases interest in signing up for informational emails by three percentage points (a roughly a 15% decrease). Combining the cost-calculator treatment with language referring to the ACA as Obamacare counteracts this decrease - however, we cannot say

[17]We do not have the price quotes (or price quotes someone would have seen had they been in the treatment) for everyone in our sample. For this reason, our sample is smaller for this regression.

[18]The cost calculator generally referred an individual to a range of prices they could expect on the exchanges. We define a low-price quote as having the mid-point of the range be below $225 (the average mid-point in the sample).

[19]The appendix shows the exact wording of this question. If an individual answered affirmatively, they were re-directed to a page asking them for their email address. SSI does not allow you to collect personally identifying information on individuals. We found this out after we had begun the survey and therefore were not able to collect emails from all individuals contacted through SSI who demonstrated interest. For this reason, we use the survey response as the outcome of interest, instead of actual email sign-ups.

with statistical statistical confidence that it increases email signups. Table 2.7 explores this finding by testing whether the decrease is coming specifically from individuals who were quoted high insurance prices. The point estimates are negative for both low and high price quotes, implying that finding out about costs could come at a cost - particularly among those quoted above-average prices for their expected cost of insurance.

Information campaigns may also be interested in knowing whether individuals feel more informed after being exposed to a treatment as a measure for the reception of a message. To this end, we also collected information on whether our respondents feel that they have enough information to understand the new marketplaces. The results in column 5 of Table 2.6 indicate that the risk treatment and the cost-calculator treatment increase uninsured individual's self-reported level of information by 4 to 6 percentage points (roughly, a 13% increase). This result cautions the interpretation of lower email sign-ups resulting from the cost-calculator treatment as a negative signal for message effectiveness. It may be the case that individuals who feel more informed may have less use for informational emails and reminders from EA.

Price Sensitivity

Willingness to Pay

Similar to Krueger and Kuziemko (K-K), we collect information about the most our survey respondents would be willing to pay for an insurance plan on the open-enrollment exchanges (Krueger and Kuziemko, 2013). Our question did not describe any details about the policy other than specifying that the prices were for an individual plan. Unlike K-Ks approach in which they explicitly decrease prices across three questions, our survey design presented individuals with the range of prices all at once. Specifically, we asked our respondents to "Please select a category below that best describes the most you would be willing to pay to purchase an individual insurance plan provided by these new health insurance marketplaces." The menu of prices ranged, in decreasing order, over \$5,000 a year (\$417/month) to \$1000 a year (\$83/month), or none of the above (see the Appendix for an example of what respondents saw on for this question).

Given concerns that observing a low-price could influence an individual's choice for their maximum willingness to pay, we tested a variant of our survey question among 10% of our respondents. In this variant, individuals were presented with an additional price at the bottom of the price spectrum.[20] The results from this test are presented in the appendix. The fact that we do find an effect of presenting a low-price calls into question the direct comparability to K-K's results. Furthermore, our samples differ on demographics compared

[20] In other words, when asked "Please select a category below that best describes the most you would be willing to pay to purchase an individual insurance plan provided by these new health insurance marketplaces.", instead of seeing the following choices: "\$5,000 a year, or \$417 a month, \$4,000 a year, or \$333 a month, \$3,000 a year, or \$250 a month, \$2,500 a year, or \$208 a month, \$2,000 a year, or \$167 a month, \$1,500 a year, or \$125 a month, \$1,000 a year, or \$83 a month, and None of the above" we showed individuals an additional price of "\$500 a year, or \$42 a month".

to their national sample, and our sample is poorer since it is self-selected based on people who choose to answer online questions for money. This may be the reason we find a much lower share of our respondents willing to pay $2000 per year for insurance (less than 20% compared to K-K's 60%). Below, when we test treatment effects on willingness to pay, we drop the ten percent of the sample that was shown the lowest price.

We use the control group to explore the "untreated" relationship between intention to purchase insurance, willingness to pay, and expectations for the cost of insurance. In table 2.9, we explore the relationship between willingness to pay and intent to purchase. We can imagine that an individual's decision about whether to visit the exchanges to buy insurance will be highly influenced by their willingness to pay for insurance (and their expectations for the cost of insurance). Table 2.9 confirms that individuals with higher willingness to pay are more likely to state near-certain intentions to purchase health insurance on the healthcare exchanges (and vice versa). This relationship appears to be stronger for the uninsured. Similarly, in Figures 2.3 and 2.4, we see that individuals with higher a priori estimated costs for an individual insurance are willing to pay more for insurance. Except for individuals who think an individual policy costs over $5,000 a year (¿$417/month), we see that individuals are more likely to be willing to pay more the more that they identify insurance costs to be. This trend appears to be apparent among the uninsured, but is starker among the insured. This suggests that willingness to pay is determined by the information individuals have about the prices of individual policies, and likely, prices of purchased policies. This is not the case for individuals who quote the lowest and highest prices for the price of insurance - reflecting less willingness to pay among individuals with less realistic price expectations.

Table 2.8 examines the effects of our treatments on a respondent's willingness to pay for insurance at a given price. Given that treatments could affect willingness to pay in different parts of the price distribution and in either direction, we analyze treatment impact by defining our outcomes as whether a person is willing to pay equal to or above each offered price (i.e. willing to pay above $83/month, willing to pay above $125, etc.). By coding the results this way, we can gain some insight into which parts of the price distribution affected individuals may be coming from. We present the results for the uninsured respondents since willingness to pay among the uninsured will factor in significantly in the reduction in the uninsured rate. In one of the rare cases of interacted messages having an effect, the cost-calculator paired with the savings treatment appears to push individuals from not being willing to pay a listed price or only being willing to pay $83/month to being nearly 5 percentage points more likely to be willing to pay $125/month and 3 percentage points more likely to be willing to pay above $350/month for an individual policy. Most surprisingly, the Obamacare treatment also appears to have increased willingness to pay at these higher price ranges; with a 4 percentage point increase in the willingness to pay above $167/month. Opposingly, the risk treatment decreases the uninsured's likelihood of being willing to pay $83/month or more for insurance by 6.6 percentage points. In addition to the mandate treatment, this treatment put an estimate on expected financial costs of remaining uninsured. The quote of $5,000 that some Americans end up paying may have appeared worth the gamble for individuals in the low willingness to pay range.[21]

Demand Curves

Using this information, we compare how demand differs across different segments of our sample. Across different subsamples, we plot the share willing to buy insurance against the log of the the monthly price presented in the survey question. For these figures, we include all observations regardless of treatment given the results above that our messaging treatments did not have large effects on willingness to pay. Figure 2.1(a) plots the breakdown for all of the respondents in our sample and, as expected, we find that the share willing to purchase increases with decreases in the price. The remaining figures compare demand across different subgroups of interest. In figure 2.1(b), we compare willingness to pay by insurance status. We find that uninsured individuals are less likely to demand insurance at a given price. In figure 2.2(a), we see that a higher share of individuals that report having higher income are willing to pay higher prices for insurance (i.e. - have a higher demand for insurance). Lastly, we look at how self-reported health status affects demand in figure 2.2(b). Here, the unhealthy report having lower demand of health insurance at all prices.

In these graphs, we do not control for the effect of other individual attributes on demand. This may be leading to some of these unexpected findings. We continue to explore this in the next section where we focus on the elasticity of demand - the relevant policy instrument for increasing the share of people with insurance.

Elasticities

Table 2.12 presents the raw price elasticities for the demand for health insurance at different price points among the control group.[22] These elasticities are calculated using the unadjusted share of individuals from the control group stating willingness to pay at given prices, without controlling for any individual characteristics. Using this basic approach, we find elasticities that are much higher than those found by Krueger and Kuziemko's data - the only directly comparable estimates for willingness to pay for insurance among the uninsured.

We follow an approach similar to Krueger and Kuziemko's to construct sub-group specific elasticities. We expand our data so that we have an observation per individual per price point (restricting the price points to the ones that are common to K-K's). We then run the following regression to recover percentage changes in likelihood to purchase for \$1000 increases in the annual price of a policy:

$$Y_{ip} = \beta Price_{ip} + \Gamma X_i + \epsilon_{ip} \tag{2.2}$$

[21]Interestingly, mentioning the cost of remaining uninsured under the mandate appeared to have lowered willingness to pay among insured individuals in this range

[22]As discussed below, because the treatments were not found to have significant effects on willingness to pay, these elasticities do not differ from those found when including treated observations as well.

where Y_{ip} is a dummy for whether individual i would purchase health insurance at price p and X_i is a vector of individual characteristics. We are interested in β, which represents the elasticity of demand while controlling for the effect of individual characteristics on overall demand. We present these results in Table 2.10, which confirms some of the patterns seen the demand curves, which did not control for other person-level covariates. For example, income increases demand; whereas puzzlingly being unhealthy or being older do not. Similar to K-K, we find that females have lower demand whereas African-Americans have higher levels of demand for insurance.

For sub-group elasticities, you can run the same regression among only individuals in that sub-group and include fixed effects to capture unobserved characteristics which may affect demand. We run:

$$Y_{ip} = \beta_k Price_{ip} + \epsilon_{ip} \text{ if } Subgroup_k = 1. \tag{2.3}$$

Table 2.11 presents the results and implied elasticities from these regressions. We see that our coefficients are very much in line with the magnitudes found by Krueger and Kuziemko. However, since a lower share of respondents in our sample were willing to pay \$2,000 a month for health insurance, our elasticities are higher than their estimates (as is discussed in (CBO, 2005)). This may be due to the fact that our question frame differed by showing all prices at once and lower prices than their survey question. It may also be due to the fact that our sample appears to have lower-income (particularly among the insured) and be much less likely to be employed - thereby contributing to the larger signs of price sensitivity. We confirm that the uninsured are more price-elastic than the insured. Although they differ in magnitude, for the most part our findings follow the same pattern as K-K. African Americans are less price sensitive, as are richer individuals. Our findings differ for those above 50 years old and those who identify as unhealthy. We find these groups to be more price elastic. This may be because these regressions do not control for the effect of other characteristics on price-sensitivity. For example, both the unhealthy and older respondents in our sample are much more likely to report lower household income and this may be contributing to the higher elasticities.

2.6 Conclusion

There appears to be significant misinformation about the cost of insurance, particularly about the introduction of new subsidies, the inability to use health characteristics to price-discriminate, and the introduction of the new health insurance exchanges. We find that the uninsured are motivated to purchase insurance after being exposed to three messages: the cost calculator, a message about the financial risk of remaining uninsured, and a message informing them of the new law requiring everyone to have insurance. The treatment messages less consistently affect willingness to pay for insurance (i.e., the lowest price at which someone would buy insurance) with the cost-calculator and savings combination yielding the most consistent increases in willingness to pay. The strong effects of the Obamacare treatment in affecting willingness to pay are surprising and highlight the importance of using commonly used terms to grab the attention of individuals.

This experiment highlights the value of exposing uninsured individuals to the price of insurance on the marketplaces. Individuals appear to react positively to receiving this information by increasing their intention to purchase insurance, as well as their likelihood to be willing to pay certain prices for insurance. We explore whether this effect is concentrated among those who received low price-quotes and find suggestive evidence that there is response heterogeneity by exposed price.

Overall, our findings highlight the importance and impact of informational campaigns to increase informedness and likelihood of purchasing insurance. Our largest limitation is that we were unable to track individuals following our survey to measure whether self-reported intentions map onto behavioral choices, as well. This is left to further studies.

Table 2.1: Summary Statistics for All Survey Respondents and Uninsured Respondents

	All				Uninsured			
	MTurk	**SSI**	**All**	**CPS**	**MTurk**	**SSI**	**All**	**CPS**
Hispanic	0.06	0.14	0.12	0.18	0.07	0.12	0.11	0.36
White	0.74	0.56	0.62	0.62	0.73	0.64	0.65	0.44
Black	0.09	0.24	0.19	0.11	0.11	0.19	0.17	0.13
Female	0.47	0.66	0.60	0.52	0.45	0.66	0.63	0.48
Age	31.1	41.7	38.0	40.8	32.5	41.8	40.2	38.0
Healthy	0.84	0.76	0.79	0.89	0.78	0.74	0.75	0.89
Household Income (1000s)	53.10	36.65	42.09	82.01	37.88	32.83	33.71	46.03
Employed	0.41	0.45	0.44	0.72	0.53	0.51	0.51	0.66
Full-time	0.24	0.21	0.22	0.63	0.29	0.22	0.24	0.55
Northeast	0.20	0.13	0.15	0.19	0.13	0.12	0.12	0.13
South	0.35	0.44	0.40	0.32	0.41	0.44	0.44	0.38
Midwest	0.21	0.20	0.20	0.22	0.21	0.20	0.20	0.18
West	0.23	0.22	0.22	0.27	0.24	0.22	0.22	0.31
Republican	0.15	0.16	0.16	-	0.11	0.17	0.16	-
Democrat	0.42	0.40	0.41	-	0.41	0.37	0.38	-
Uninsured	0.30	0.76	0.61	0.19	1	1	1	1
Under-Insured	0.18	0.09	0.12	-	0	0	0	0
Observations	4209	8144	12353	119423	1249	6027	7276	23023

Notes: Sample summary statistics presented by sample source (MTurk or SSI) and compared to average responses in the CPS in 2013.

Table 2.2: Intention to Purchase Insurance by Insurance Status and Health

All Respondents	(1) Insured Pct	(2) Uninsured Pct	(3) UnderInsured Pct	(4) Unins. Healthy Pct	(5) Unins. Unhealthy Pct
Almost Certain	2.54	11.48	7.17	12.06	9.61
Probably Will	4.67	16.64	13.54	17.29	14.93
50-50 Chance	12.77	31.55	28.27	31.41	32.12
Probably Will Not	34.81	22.99	28.50	21.96	26.03
Definitely Will Not	45.21	17.33	22.52	17.27	17.31
Total	100.00	100.00	100.00	100.00	100.00
Observations	2913	6741	1270	5008	1675
Among Control Group					
Almost Certain	1.35	11.60	6.56	12.77	8.05
Probably Will	2.69	13.32	9.84	14.29	10.92
50-50 Chance	11.45	29.94	22.95	28.57	33.33
Probably Will Not	37.71	26.02	33.61	25.11	28.74
Definitely Will Not	46.80	19.12	27.05	19.26	18.97
Total	100.00	100.00	100.00	100.00	100.00
Observations	297	638	122	462	174

Notes: Intent to purchase health insurance on individual exchanges by respondent insurance and health status. Response breakdown for all respondents and only control group respondents.

Table 2.3: Estimate for Annual Cost of Insurance by Respondent Characteristics

	Estimated Annual Cost of Insurance in $	
Unhealthy	71.6*	(38.3)
Uninsured	68.9*	(36.9)
Age < 30	-61.6*	(34.6)
Female	48.1	(32.0)
Fulltime	-0.2	(42.8)
Employed	57.7	(40.7)
Household Size	31.7***	(10.9)
SSI	311.3***	(39.8)
$15K <HH Income < $25K	-48.0	(52.5)
$25K < HH Income <$40K	-250.9***	(50.0)
$40K < HH Income < $65K	-271.9***	(50.0)
HH Income > $65K	-325.6***	(55.0)
White	181.1***	(60.5)
Black	-242.2***	(68.7)
Hispanic	-63.7***	(73.2)
Northeast	-22.8	(51.1)
South	-23.5	(40.6)
West	-149.2***	(46.6)
Observations	11587	

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ OLS regression predicting respondent's beliefs for the cost of insurance based on individual characteristics. Standard errors in parentheses.

Table 2.4: Treatment Probabilities by Race: Design and Implementation

	Theoretical Prob. of Treatment		Actual Prob. of Treatment		
	Non-Minorities	Minorities	Non-Minorities	Minorities	Total
Control	16	25.0	16.1	25.0	18.8
Mandate Treatment	14	0.0	15.0	0.0	10.4
Risk Treatment	14	0.0	13.5	0.0	9.4
Mandate and Risk Treatment	14	0.0	13.6	0.0	9.5
Cost-Calculator Treatment	14	25.0	13.4	25.8	17.1
Savings Treatment	14	0.0	14.6	0.0	10.2
Cost-Calc and Savings Treatment	14.2	0.0	13.9	0.0	9.7
Obamacare Treatment	0.0	25.0	0.0	24.5	7.4
Cost-Calc and Obamacare Treatment	0.0	25.0	0.0	24.8	7.5
N	-	-	8614	3739	12353

Table 2.5: Effect of Price Expectations on Stated Likelihood of Purchasing Insurance

	All		Uninsured	
	Definitely Buy	Definitely Won't Buy	Definitely Buy	Definitely Won't Buy
	(1)	(2)	(3)	(4)
Cost Belief =\$125 / month	0.024	-0.043	0.046	-0.047
	(0.019)	(0.030)	(0.029)	(0.034)
Cost Belief = \$167	0.020	-0.017	0.057*	-0.011
	(0.023)	(0.035)	(0.034)	(0.040)
Cost Belief = \$208	-0.015	-0.10***	-0.024	-0.085**
	(0.022)	(0.035)	(0.032)	(0.038)
Cost Belief = \$250	-0.045**	-0.013	-0.067**	0.029
	(0.022)	(0.034)	(0.032)	(0.037)
Cost Belief = \$333	-0.046*	-0.059	-0.053	-0.011
	(0.025)	(0.039)	(0.036)	(0.042)
Cost Belief = \$417	-0.031	-0.033	-0.070**	0.020
	(0.024)	(0.037)	(0.033)	(0.038)
Cost Belief >\$417	-0.020	0.062*	-0.047	0.13***
	(0.022)	(0.034)	(0.030)	(0.035)
Observations	2117	2117	1283	1283

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.
Sample restricted to respondents in control group. LPM for likelihood of stating will definitely buy insurance on the exchanges based on individuals' estimated cost for insurance. Omitted category is ¡\$83 per month.

Table 2.6: Effect of Treatments on Key Survey Outcomes

	Definitely Buy		Email Sign-Up		More Informed	
	All	Uninsured	All	Uninsured	All	Uninsured
	(1)	(2)	(3)	(4)	(5)	(6)
Cost-Calculator Treatment	0.014	0.026**	-0.033***	-0.035**	0.062***	0.058***
	(0.0089)	(0.013)	(0.012)	(0.017)	(0.015)	(0.020)
Savings Treatment	-0.016	-0.011	0.0086	0.017	0.022	0.022
	(0.011)	(0.016)	(0.015)	(0.021)	(0.019)	(0.024)
Cost-Calc and Savings Treatment	0.016	0.011	-0.0018	-0.0071	-0.016	0.0055
	(0.015)	(0.022)	(0.021)	(0.029)	(0.025)	(0.033)
Mandate Treatment	0.022**	0.031*	0.015	0.029	0.026	0.024
	(0.011)	(0.016)	(0.015)	(0.021)	(0.018)	(0.023)
Risk Treatment	0.018	0.037**	0.013	0.0066	0.032*	0.041*
	(0.011)	(0.016)	(0.015)	(0.022)	(0.019)	(0.025)
Mandate and Risk Treatment	-0.014	-0.034	-0.032	-0.035	-0.019	-0.030
	(0.016)	(0.024)	(0.022)	(0.032)	(0.028)	(0.036)
Obamacare Treatment	0.015	0.0014	-0.012	-0.036	0.055**	0.039
	(0.013)	(0.020)	(0.018)	(0.026)	(0.022)	(0.029)
Obamacare and Cost-Calc Treatment	-0.018	-0.015	0.058**	0.075**	-0.095***	-0.069*
	(0.017)	(0.025)	(0.023)	(0.033)	(0.028)	(0.037)
Observations	10910	6549	11818	6947	11837	6957

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Regressions control for date, race, gender, age, household size, household income buckets, self-reported health, region, and survey source.

Table 2.7: Effect of Learning Cost of Insurance in Cost-Calculator Treatment on Intent to Purchase

	Definitely Buy	Email Sign-Up
All		
Cost-Calculator Treatment	0.023	-0.037*
	(0.014)	(0.019)
High Quoted Price	0.0055	-0.019
	(0.012)	(0.016)
Cost-Calculator Treatment and High Price Quote	-0.027*	-0.029
	(0.015)	(0.021)
Observations	5907	6323
Uninsured		
Cost-Calculator Treatment	0.029	-0.054**
	(0.021)	(0.027)
High Quoted Price	-0.0020	-0.010
	(0.018)	(0.024)
Cost-Calculator Treatment and High Price Quote 1	-0.010	-0.047
	(0.024)	(0.031)
Observations	3416	3603

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Regressions controls for message shown, date, race, gender, age, household size, household income buckets, self-reported health, region, and survey source.

Table 2.8: Effect of Treatments on Willngness to Pay (WTP) for Uninsured

	(1) WTP>83	(2) WTP>125	(3) WTP>167	(4) WTP>208	(5) WTP>350	(6) WTP>417
Cost-Calculator Treatment	-0.019	-0.0075	-0.0051	-0.00017	-0.0055	0.0026
	(0.020)	(0.017)	(0.014)	(0.012)	(0.0096)	(0.0068)
Savings Treatment	0.011	-0.030	-0.019	-0.0038	-0.0044	0.00098
	(0.024)	(0.021)	(0.017)	(0.014)	(0.011)	(0.0082)
Cost-Calc and Savings Treatment	0.023	0.049*	0.044*	0.034*	0.032**	0.0072
	(0.033)	(0.029)	(0.023)	(0.019)	(0.016)	(0.011)
Mandate Treatment	-0.039	-0.025	-0.015	-0.0020	-0.0028	0.00027
	(0.024)	(0.021)	(0.017)	(0.014)	(0.011)	(0.0081)
Risk Treatment	-0.067***	-0.015	-0.016	-0.017	-0.013	-0.0022
	(0.025)	(0.022)	(0.017)	(0.014)	(0.012)	(0.0085)
Mandate and Risk Treatment	0.097***	0.035	0.036	0.028	0.031*	0.0055
	(0.036)	(0.031)	(0.025)	(0.021)	(0.017)	(0.012)
Obamacare Treatment	0.025	0.034	0.039*	0.033*	0.024*	0.016
	(0.030)	(0.026)	(0.021)	(0.017)	(0.014)	(0.010)
Obamacare and Cost-Calc Treatment	-0.024	-0.021	-0.030	-0.014	-0.0061	-0.00040
	(0.038)	(0.033)	(0.027)	(0.022)	(0.018)	(0.013)
Observations	6266	6266	6266	6266	6266	6266

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Regression controls for date, race, gender, age, household size, household income buckets, self-reported health, region, and survey source.

Table 2.9: Relationship between Willingness to Pay and Intention to Purchase Health Insurance

	All		Uninsured	
	Definitely Buy	Definitely Won't Buy	Definitely Buy	Definitely Won't Buy
WTP = \$83/month	0.051***	-0.21***	0.072***	-0.17***
	(0.015)	(0.023)	(0.020)	(0.023)
WTP = \$125/month	0.047**	-0.19***	0.081**	-0.21***
	(0.021)	(0.032)	(0.032)	(0.037)
WTP = \$167/month	0.072***	-0.26***	0.15***	-0.23***
	(0.026)	(0.041)	(0.041)	(0.048)
WTP = \$208/month	-0.00048	-0.18***	0.011	-0.11*
	(0.029)	(0.046)	(0.055)	(0.064)
WTP = \$250/month	0.051	-0.19***	0.12**	-0.23***
	(0.032)	(0.051)	(0.057)	(0.066)
WTP = \$333/month	0.089**	-0.19***	0.17*	-0.15
	(0.044)	(0.069)	(0.10)	(0.12)
WTP > \$417/month	0.14***	-0.27***	0.20**	-0.27***
	(0.051)	(0.080)	(0.088)	(0.10)
Observations	1908	1908	1148	1148

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

Table 2.10: Price Elasticity for $1000 Increase: Controlling for Respondent Characteristics

	Willingness to Purchase Insurance	
	All	Unins
$1000/mo Price Increase	-0.12***	-0.089***
	(0.0025)	(0.0028)
Female	-0.026***	-0.023***
	(0.0043)	(0.0049)
African American	0.0013	0.027***
	(0.0078)	(0.0086)
Hispanic	-0.00086	0.0094
	(0.0086)	(0.0096)
$15K < HH Income < $25K	-0.0082	0.0044
	(0.0071)	(0.0071)
$25K < HH Income < $40K	0.033***	0.041***
	(0.0068)	(0.0069)
$40K < HH Income < $65K	0.087***	0.093***
	(0.0069)	(0.0074)
HH Income > $65K	0.17***	0.17***
	(0.0075)	(0.0089)
Unhealthy	-0.027***	-0.014***
	(0.0052)	(0.0054)
Under 30	0.018***	0.033***
	(0.0047)	(0.0053)
Uninsured	-0.12***	-
	(0.0050)	-
Employed	0.021***	0.015**
	(0.0055)	(0.0058)
Fulltime	0.023***	0.034***
	(0.0058)	(0.0065)
SSI	0.0050	0.019***
	(0.0054)	(0.0063)
HH size	-0.0066***	-0.0140***
	(0.0015)	(0.0017)
Observations	31314	18798

Notes:
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Regression includes control for treatment assignment.

Table 2.11: Demand Elasticities for Health Insurance

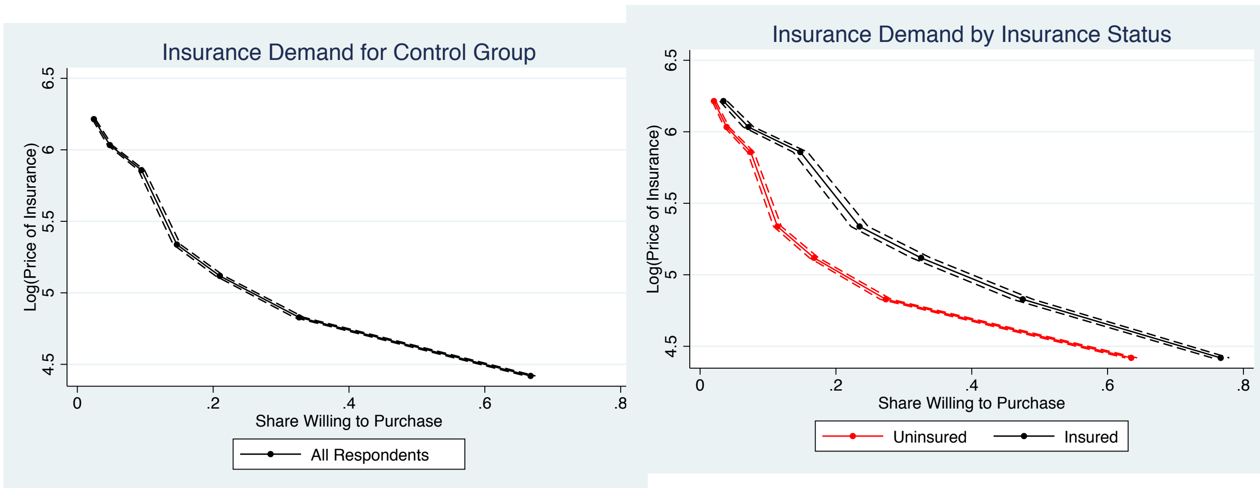
Price per Month	Share	Elasticity	K-K Elasticity
$417	4.02	-	
$333	8.60	-2.43	
$250	13.87	-3.03	-1.12
$208	20.01	-2.11	
$167	31.64	-1.88	-1.11*
$125	65.62	-1.71	

Notes:
Arc Elasticity from $417 to $125 is 1.39. Sample limited to control group.
*Arc elasticity from $250 to $167.

Table 2.12: Elasticities

					Uninsured			
	Insured	Uninsured	Females	Over50	AfAm	65kt160k	Employed	Unhealthy
Decrease in Demand for 1k Increase	-0.16***	-0.088***	-0.081***	-0.083***	-0.096***	-0.15***	-0.11***	-0.074***
	(0.0031)	(0.0020)	(0.0024)	(0.0036)	(0.0050)	(0.0074)	(0.0030)	(0.0037)
Share WTP $2k								
	47.81	22.89	20.69	20.93	26.67	42.46	27.93	18.61
Observations	4242	6439	4027	1844	1106	716	3255	1585
Elasticity								
	-1.52	-1.87	-1.93	-1.96	-1.68	-1.67	-1.83	-2.00
K-K Estimates								
	-	-1.07	-1.07	-1.10	-0.76	-1.11	-1.07	-0.95+

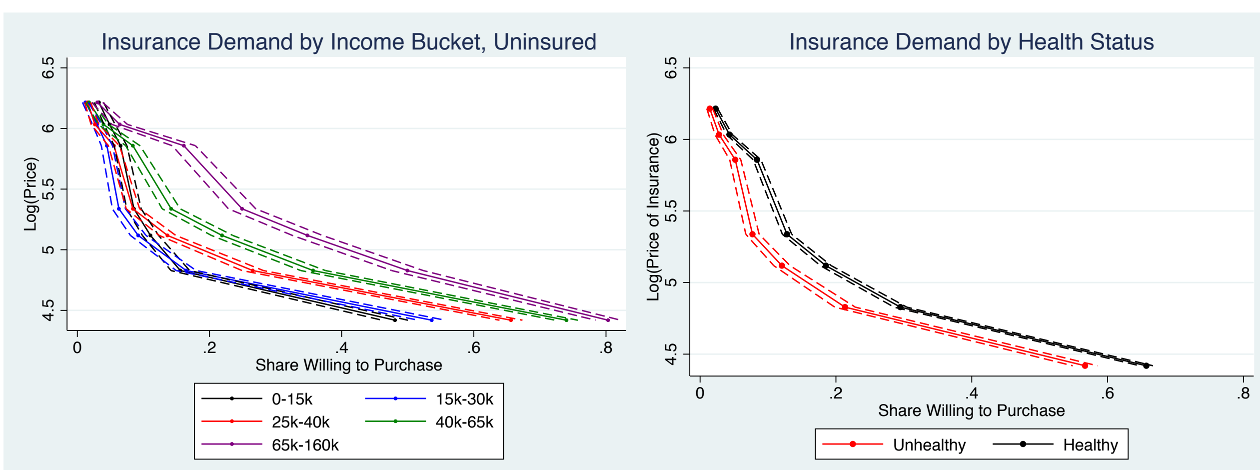
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. + Questions differ determining health status from our survey to theirs.



(a) Demand for Insurance among All Respondents (b) Demand for Insurance among Everyone and by Insurance Status

Figure 2.1: Demand Curves for All Respondents and by Insurance Status

Notes: The plots show the percent of individuals that responded that they would be willing to pay a given price per month for an individual insurance plan. The y-axis is the log of the monthly price and the x-axis is the share of respondents willing to pay that amount. Dashed lines represent 90% confidence intervals.

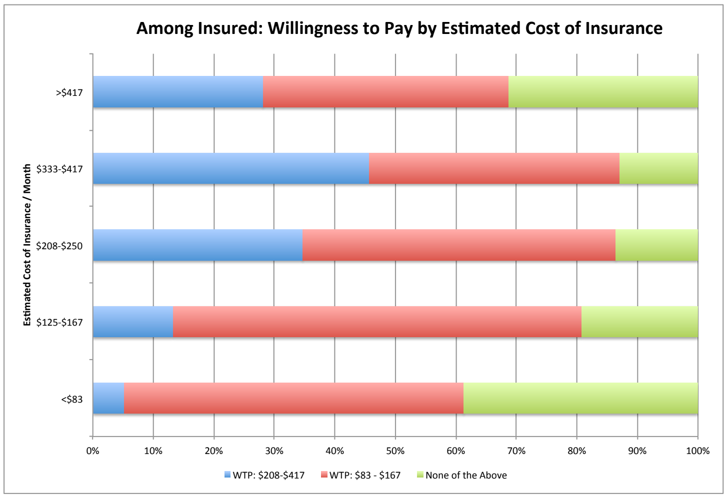


(a) Demand for Insurance by Income Group (b) Demand for Insurance by Income and Health

Figure 2.2: Demand Curves by Sub-group

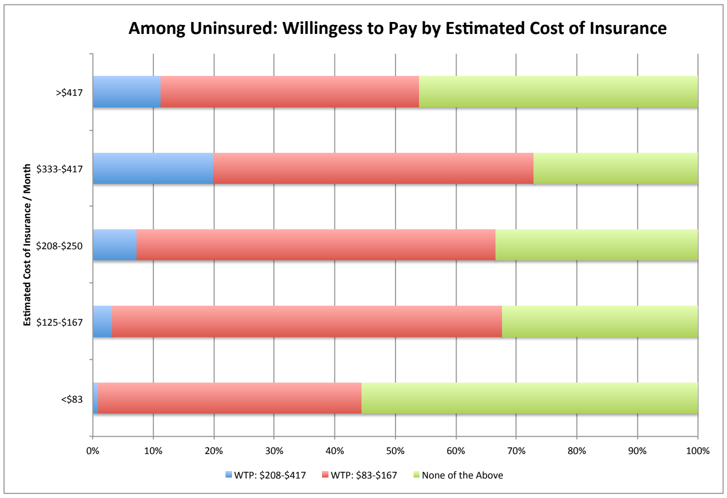
Notes: The plots show the percent of individuals that responded that they would be willing to pay a given price per month for an individual insurance plan. The y-axis is the log of the monthly price and the x-axis is the share of respondents willing to pay that amount. Dashed lines represent 90% confidence intervals.

Figure 2.3: Among Insured: Willingness to Pay by Estimated Cost of Insurance



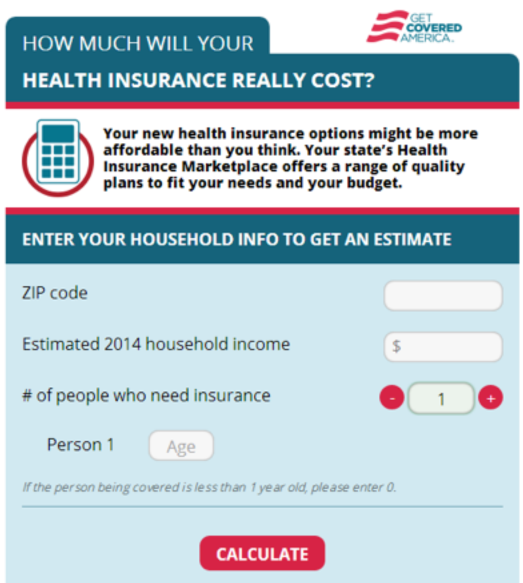
Notes: This figure shows the share, among the insured, willing to pay different monthly prices for insurance based on their initial beliefs about the cost of insurance.

Figure 2.4: Among Uninsured: Willingness to Pay by Estimated Cost of Insurance



Notes: This figure shows the share, among the uninsured, willing to pay different monthly prices for insurance based on their initial beliefs about the cost of insurance.

Figure 2.5: Example of Cost-Calculator



Notes: This is an example of the cost-calculator a respondent in the cost-calculator treatment would have seen and been asked to fill in.

2.7 Appendix

Non-Response Rate

It appears that those in the control group were more likely to not answer our question for intention to purchase insurance on the exchanges (all of our other outcome questions had very low non-response rates). The majority (55%) of those who did not answer this question were insured. Appendix Table 2.13 outlines which treatments increased the likelihood that an individual would respond to this outcome question. Though this finding introduces questions concerning our treatment results explored above, our results do not change significantly when we set all of these missings as 0's (i.e. observations that will not definitely buy insurance on the exchanges). When we impute all these missings as non-buyers, the risk treatment actually becomes significant at the ten percent level for the full sample, with an effect size of 1.72 percentage points).

Survey Text and Questions

Common Introduction

Aside from those randomized into the "Obamacare" treatment, all individuals saw the following introduction:

"Many Americans get health care through their employer, spouse, Medicare, Medicaid, the VA, or other sources of insurance that help pay the costs of medical care. Health insurance plans like these help people pay for the regular care they need to stay healthy and cover the costs of unexpected health problems.

This year new health insurance marketplaces became available to the millions of Americans that still need access to high-quality, low-cost health insurance. These new health insurance marketplaces have now opened to help people shop for health insurance and compare prices. All of the plans offered through the health insurance marketplaces will have to cover the basics, like doctor visits, emergency rooms, medication, and wellness care."

Treatment Text

Cost Calculator Treatment

"Please use the healthcare cost calculator below to get an estimate of how much you could expect to pay through the new health insurance marketplaces." The healthcare calculator, and the information requested, is shown above in Figure 2.5.

Based on the information entered into the calculator, individuals received either a price estimate of an insurance plan on the marketplace, or a notice informing them they might be eligible for completely subsidized insurance programs like Medicaid and CHIP. Survey takers were not asked to enter their email address and were not redirected to Enroll America's website.

Savings Treatment

"The health insurance marketplaces provide financial assistance to some people based on their household income. This assistance could reduce the price someone pays every month for insurance by about 30% on average for those who qualify, compared to what the same plan would normally cost. That level of savings could mean hundreds of extra dollars in a year."

Risk Treatment

"Health insurance plans offered through the new marketplaces will help offset medical costs and protect you from huge medical bills. Huge medical bills are more common than you might think. Over 57 million Americans will spend more than $5,000 on medical costs this year. Insurance reduces the risk of large medical bills wrecking your finances."

Mandate Treatment

"If you decide to not purchase insurance, a penalty will be added to your taxes. The penalty will be 1% of your income. For example, someone making $28,000 would have to pay a penalty of $280. The penalty could rise to a minimum of 2.5% in future years, or $700 for someone making $28,000. While everyone can decide to pay this penalty, that decision will cost money without providing any of the benefits of health insurance."

Obamacare Treatment

"This year new health insurance marketplaces, which are also known as "Obamacare", became available to the millions of Americans that still need access to high quality, low-cost health insurance. These new health insurance marketplaces have now opened to help people shop for health insurance and compare prices. All of the plans offered through Obamacare will have to cover the basics, like doctor visits, emergency rooms, medication, and wellness care. The health insurance marketplaces are designed to make health insurance more affordable than it currently is for most people."

Combination of Savings Message Frame with Cost Calculator

"The health insurance marketplaces provide financial assistance to some people based on their household income. This assistance could reduce the price someone pays every month for insurance by about 30% on average for those who qualify, compared to what the same plan would normally cost. That level of savings could mean hundreds of extra dollars in a year.

Please use the healthcare cost calculator below to get an estimate of how much you could expect to pay through the new health insurance marketplaces."

Combination of the Risk Message Frame with the Mandate Treatment

"Health insurance plans offered through the new marketplaces will help offset medical costs and protect you from huge medical bills. Huge medical bills are more common than you might think. Over 57 million Americans will spend more than $5,000 on medical costs this year. Insurance reduces the risk of large medical bills wrecking your finances.

If you decide to not purchase insurance, a penalty will be added to your taxes. The penalty will be 1% of your income. For example, someone making $28,000 would have to pay a penalty of $280. The penalty could rise to a minimum of 2.5% in future years, or $700 for someone making $28,000. While everyone can decide to pay this penalty, that decision will cost money without providing any of the benefits of health insurance."

Outcome Questions

The following questions were used to measure the impact of the message frames.

Chance of Buying Insurance on Exchanges

"What are the chances that you will buy a health insurance plan in the new marketplaces that opened this year?"

- Almost certain
- Probably will
- 50-50 chance
- Probably will not
- Definitely will not

Intent to Buy Insurance on Exchange at a Given Price

"Please select a category below that best describes the most you would be willing to pay to purchase an individual insurance plan provided by these new health insurance marketplaces."

- $5,000 a year, or $417 a month
- $4,000 a year, or $333 a month
- $3,000 a year, or $250 a month
- $2,500 a year, or $208 a month
- $2,000 a year, or $167 a month
- $1,500 a year, or $125 a month
- $1,000 a year, or $83 a month
- None of the above

Have Enough Information About the Exchanges

"Many Americans stand to benefit from more accessible and affordable health care because of these new health insurance marketplaces. Do you feel you have enough information about these new health insurance marketplaces to understand how they will impact you and your family, or not?"

- Yes, I have enough information
- No, I do not have enough information

Interested in Joining Our Email List

"Get Covered America is an independent non-profit organization working to make sure that people know about the new health coverage options. The new health insurance marketplaces, where you may find a quality health care plan that fits your budget needs, are now open. At the health insurance marketplaces:

- You can shop and compare private health insurance plans displayed in simple language. There's no fine print.
- All plans must cover the important benefits so if you or a family member gets sick, you won't have to worry about big medical bills.
- Low cost and free plans are available.
- No one can be denied coverage because they have a pre-existing condition

Are you interested in staying informed by signing up for email updates from Get Covered America? This decision is totally optional and does not affect your completion of the survey. You will not have to provide an email address to continue."

- Yes, I would like to sign up for email updates
- No, I would not like to sign up for email updates

Cost Expectations

"This year new health insurance marketplaces opened up to provide a new option for people that don't currently have insurance. The plans offered in these new health insurance marketplaces are designed to make health insurance more affordable than it currently is for most people. Based on what you know right now, think about health insurance costs for an average individual with a household income of $30,000 a year. For that person, what do you think the basic insurance plan from the new health insurance marketplaces will generally cost?"

- More than $5,000 a year
- $5,000 a year, or $417 a month
- $4,000 a year, or $333 a month
- $3,000 a year, or $250 a month
- $2,500 a year, or $208 a month
- $2,000 a year, or $167 a month
- $1,500 a year, or $125 a month
- Less than $1,000 a year

Appendix Tables

Table 2.13: Effect of Treatments on Non-Response

	No Response to Purchase Intention	
	All	Uninsured
Cost-Calculator Treatment	-0.016*	-0.0023
	(0.0083)	(0.0095)
Savings Treatment	-0.028***	-0.010
	(0.010)	(0.011)
Cost-Calc and Savings Treatment	0.043***	0.040**
	(0.014)	(0.016)
Mandate Treatment	-0.0057	-0.00019
	(0.010)	(0.011)
Risk Treatment	-0.015	-0.00040
	(0.010)	(0.012)
Mandate Treatment and Risk Treatment	-0.0015	0.00040
	(0.015)	(0.017)
Obamacare Treatment	0.012	0.014
	(0.012)	(0.014)
Obamacare and Cost-Calc Treatment	0.010	0.00072
	(0.015)	(0.018)
Observations	11846	6960

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Standard errors in parentheses. Regressions controls for date, race, gender, age, household size, household income buckets, self-reported health, region, and survey source.

Table 2.14: Balance Test across Treatments for Non-Minorities

Non-Minorities	(1) Control	(2) Risk	(3) Mandate	(4) Risk-Mandate	(5) Cost	(6) Savings	(7) Cost-Savings
White	0.022	0.0032	0.0018	-0.014	0.0022	-0.013	-0.0016
	(1.58)	(0.25)	(0.13)	(-1.09)	(0.18)	(-1.00)	(-0.13)
Female	-0.0052	0.0035	-0.0036	0.0019	0.0016	0.012	-0.010
	(-0.61)	(0.45)	(-0.44)	(0.24)	(0.21)	(1.42)	(-1.25)
Republican	0.0079	-0.0048	0.0012	0.010	0.00090	-0.020	0.0042
	(0.71)	(-0.46)	(0.11)	(0.97)	(0.09)	(-1.83)	(0.41)
Democrat	0.017	0.0071	-0.0089	0.0021	0.0019	-0.021*	0.0012
	(1.85)	(0.81)	(-0.98)	(0.24)	(0.23)	(-2.30)	(0.14)
Age	0.00013	-0.00032	-0.00011	0.00050	-0.000445	0.00012	0.00013
	(0.36)	(-0.98)	(-0.32)	(1.53)	(-1.41)	(0.35)	(0.40)
Healthy	0.0081	-0.012	0.022*	-0.0039	-0.0087	-0.0011	-0.0043
	(0.79)	(-1.30)	(2.25)	(-0.41)	(-0.93)	(-0.11)	(-0.45)
Uninsured	0.016	-0.020	0.0029	0.0043	0.0055	-0.018	0.0085
	(1.37)	(-1.81)	(0.25)	(0.39)	(0.50)	(-1.52)	(0.77)
Under-insured	0.022	-0.0012	-0.0044	-0.0064	-0.012	-0.0094	0.012
	(1.51)	(-0.09)	(-0.32)	(-0.48)	(-0.94)	(-0.67)	(0.90)
HH Income (1000s)	-0.00015	0.00017	-0.00017	-1.89e-05	0.00014	-0.00014	0.00018
	(-1.13)	(1.38)	(-1.43)	(-0.16)	(1.21)	(-1.16)	(1.50)
Full-time	-0.015	0.0085	-0.022	-0.0051	0.0052	0.011	0.018
	(-1.21)	(0.72)	(-1.82)	(-0.44)	(0.45)	(0.94)	(1.50)
Employed	0.0069	0.0017	0.015	0.0068	-0.0085	-0.0072	-0.015
	(0.63)	(0.17)	(1.45)	(0.67)	(-0.86)	(-0.69)	(-1.48)
Northeast	0.029*	-0.0049	-0.0046	-0.018	-0.0044	0.0039	-0.00089
	(2.15)	(-0.40)	(-0.36)	(-1.43)	(-0.36)	(0.30)	(-0.07)
South	0.0062	0.0090	-0.0048	-0.0042	0.0018	-0.0093	0.0013
	(0.57)	(0.90)	(-0.46)	(-0.42)	(0.18)	(-0.89)	(0.13)
Midwest	0.023	-0.0043	0.0049	0.0040	-0.012	-0.013	-0.0027
	(1.91)	(-0.38)	(0.42)	(0.35)	(-1.12)	(-1.08)	(-0.24)
SSI	-0.012	0.015	0.00015	-0.022*	0.0074	0.011	0.00017
	(-1.04)	(1.50)	(0.01)	(-2.16)	(0.73)	(1.00)	(0.02)
Constant	0.12***	0.14***	0.15***	0.14***	0.14***	0.18***	0.13***
	(5.13)	(6.82)	(6.66)	(6.74)	(6.92)	(8.15)	(6.05)
Observations	8152	8152	8152	8152	8152	8152	8152

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ T-statistics in parentheses.

Table 2.15: Balance Test across Treatments for Blacks and Hispanics

	(1) Control	(2) Cost	(3) ObamaCare	(4) Cost-ObamaCare
Hispanic	0.036*	-0.014	-0.024	0.0015
	(2.17)	(-0.84)	(-1.45)	(0.09)
Female	0.0078	-0.0055	-0.012	0.0092
	(0.49)	(-0.35)	(-0.74)	(0.58)
Republican	0.021	-0.049	-0.011	0.039
	(0.68)	(-1.59)	(-0.37)	(1.26)
Democrat	0.025	-0.028	-0.0081	0.011
	(1.50)	(-1.66)	(-0.49)	(0.64)
Age	0.00069	0.00081	-0.00099	-0.00052
	(1.12)	(1.32)	(-1.63)	(-0.84)
Healthy	-0.013	-0.00037	0.020	-0.0060
	(-0.71)	(-0.02)	(1.06)	(-0.32)
Uninsured	-0.019	0.011	-0.0094	0.017
	(-1.00)	(0.59)	(-0.52)	(0.93)
Under-insured	-0.023	-0.029	-0.0032	0.055*
	(-0.96)	(-1.19)	(-0.13)	(2.28)
HH Income (1000s)	-0.00033	0.00000025	-0.00000023	0.00000030
	(-1.47)	(1.15)	(-1.06)	(1.38)
Full-time	-0.032	-0.012	0.037	0.0057
	(-1.39)	(-0.51)	(1.68)	(0.25)
Employed	-0.0048	0.0099	-0.0050	-0.00014
	(-0.25)	(0.51)	(-0.26)	(-0.01)
Northeast	-0.0057	0.0023	-0.020	0.023
	(-0.22)	(0.09)	(-0.78)	(0.90)
South	0.029	-0.0074	-0.032	0.010
	(1.49)	(-0.38)	(-1.67)	(0.53)
Midwest	-0.0045	0.015	-0.017	0.0065
	(-0.18)	(0.58)	(-0.67)	(0.26)
SSI	-0.011	-0.024	0.0087	0.026
	(-0.51)	(-1.14)	(0.42)	(1.23)
Constant	0.24***	0.26***	0.31***	0.20***
	(5.55)	(6.03)	(7.36)	(4.67)
Observations	3464	3464	3464	3464

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ T-statistics in parentheses.

Chapter 3

The Impact of Public Insurance Expansion on Labor Supply

3.1 Introduction

Since the end of 2013, enrollment in Medicaid and the Children's Health Insurance Program has grown by over 11 million people. This dramatic 19% increase in program size has been driven primarily by the 9.5 million new beneficiaries living in states that expanded Medicaid under the Patient Protection and Affordable Care Act (ACA) (CMS, 2015). The debate around the ACA and its passage included significant disagreements about the bill's expected effects on the economy; in particular, the strains it would put on employment (**?**). Though many in the debate focused on the effects the mandates would have on an employer's decision to hire and retain full-time workers, the expansion of Medicaid rolls was also targeted as a potential productivity killer. Some theorized that increasing the qualifying income threshold for Medicaid would lower the incentives of the newly eligible to work or work full-time (**??**)

While Medicaid previously existed for a subset of the population with qualifying incomes and characteristics, the new law extended access to a large previously excluded part of the population: childless adults. Overall, labor supply theory predicts a negative employment response to increased insurance access for these eligible adults who were uncovered under state policy prior to the expansion. The theory posits that newly qualifying individuals are enabled by this alternative coverage either to move to another job or reduce their hours of work without the risk of becoming uninsured. Furthermore, for those who were working without insurance, the income effect from receiving government benefits might introduce downward pressure on their hours of work. Lastly, the state-based income caps determining Medicaid eligibility introduce large implicit marginal tax rates through the loss of coverage eligibility that comes from having income above the threshold. These increased marginal tax rates could introduce further downward pressure on hours. As a result, individuals may restrict their labor supply in order to qualify for benefits or stay within the eligible income range. [1]

Recent work by Garthwaite et al., Dague et al., and Baicker and Lahey has suggested

[1] Possible positive long-term effects of expanding Medicaid would be any health benefits that come from insurance receipt. We may see increases in labor force measures over time if access to government health insurance encourages improvements in health that enable individuals to work or work more than they previously could.

that there are large negative employment responses [2] to broader access to public health insurance (Garthwaite et al., 2013; Boyle and Lahey, 2010, 2014; Dague et al., 2014). This paper sets out to further explore the relationship between access to health insurance and labor supply. Specifically, I focus on whether broadening eligibility limits for government health insurance through the recent Medicaid expansion affects the decision to work and how much to work. In addition to speaking to prior research, understanding the labor supply effects of expanding access to health insurance is necessary to be able to account properly for the costs and benefits of these programs.

To study the effect on labor supply of the recent large-scale expansion of Medicaid, I take advantage of three empirical approaches. To start, I implement a difference-in-differences approach that compares extensive and intensive margin responses[3] among childless adults who newly qualify for Medicaid across expansion and non-expansion states and over time. [4] The difference-in-differences specification may be flawed if the employment outcomes for expansion states are trending differently from non-expansion states or if they receive shocks to their outcomes that coincide with the expansion. To address these concerns, I also implement two triple-differences approaches. The additional variation for these specifications comes from comparing individuals based on whether their past income is above or below the established Medicaid income threshold. I argue that past income can serve as a useful metric to differentiate those who are more likely to be affected by the new access to Medicaid in expansion states. The assumptions behind these approaches, as well as their strengths and weaknesses, are described in more detail below.

Based on the fifteen months of data available since the expansion went into effect, the three approaches I use find increases in Medicaid coverage among childless adults in Medicaid expansion states. Conversely, my results do not provide strong evidence that employment or full-time rates decreased among the newly eligible due to this policy change. However, the employment responses are not measured with enough precision to reject that there were large changes in labor force outcomes among childless adults in states with broadened access to Medicaid.

3.2 Details on Medicaid and Medicaid Expansion

Initiated under President Johnson in 1965, Medicaid is a joint federal and state program that provides free and low-cost healthcare to low-income Americans. A key provision in the

[2] Or large positive employment responses from opposing decreases in insurance coverage

[3] I look at employment, as well as overall labor force participation, the decision to work full-time vs. part-time, and hours worked per week.

[4] The decision to focus on childless adults is due to two factors: 1) a smaller share of adults with children were made newly eligible under the Medicaid expansion, making it harder to detect overall effects and 2) though labor supply may decrease for parents with incomes in the newly qualifying range of 100 to 138% of the federal poverty level (FPL), we may see a counterbalancing increase for those previously in the 0 to 100% range as the threshold increases now allow these individuals to expand their income up to a certain point and remain insured.

passage of the Patient Protection and Affordable Care Act (a.k.a the ACA and Obamacare), in 2010, established new rules determining Medicaid eligibility. Prior to this change, access to Medicaid benefits was set individually by the states and generally granted to individuals – almost exclusively families with children – that had incomes that were 100% or less of the federal poverty level (FPL). In early 2010, the ACA mandated[5] that benefits be extended to *anyone* with family income up to 138% of the FPL beginning in 2014. In addition to increasing the maximum qualifying income, this expansion opened Medicaid to a previously almost-universally denied group of individuals: childless adults.

In June of 2012, the Supreme Court ruled the Medicaid portion of Obamacare unconstitutional by deciding that states could not be forced to provide state-benefits and bear part of the costs of these expanded services. As a result, the increased threshold for Medicaid eligibility was no longer required to be applied universally across states. Instead, each state was given the choice of whether to change the terms of their programs to those set by the ACA and accept federal dollars for doing so. By 2015, 29 states (including the District of Columbia) had chosen to expand their Medicaid rolls - introducing Medicaid access to childless individuals with income from 0% to 100% of the FPL. [6]

As seen below in Table 3.1, the choice to expand fell largely across political lines, with most states with Democratic governors at the time choosing to implement the new eligibility terms proposed in the ACA. Almost 90% of states that did not expand Medicaid were governed by a Republican and almost 70% of those expanding were governed by a Democrat. The fact that states may have acted based on partisanship reduces some of the concern that the decision to expand Medicaid was made endogenously with a state's labor force and economy in mind. As a result, there may be less reason to worry about endogenous selection affecting the comparison of state outcomes based on their Medicaid expansion status. The triple-difference approaches that I propose further reduce the worry that state-specific differences are affecting the results.

Figure 3.2 presents Medicaid eligibility rules for childless adults before and after 2014, the first year the majority of states implemented these changes. In most of the expansion states, Medicaid benefits were awarded beginning on January 1st, 2014 - though individuals

[5]The Medicaid provision in the ACA grants states near complete financial support for covering these new enrollees in the first few years: 100% federal reimbursement from 2014 through 2016, 95% in 2017, 94% in 2018, 93% in 2019, and 90% going forward (Angeles, 2012); though it stipulates that states are required to cover 10% of the costs of this expansion after 2020.

[6]Whereas most of these 29 states chose to expand in 2012 and 2013, a few states made the decision to expand after the broad Medicaid expansion in January of 2014. New Hampshire and Pennsylvania chose to expand their Medicaid program under ACA guidelines in 2014 with benefits starting in August of 2014 and January of 2015, respectively. Michigan also joined late, expanding in April of 2014. Indiana decided to expand in January of 2015. Wisconsin chose not to expand Medicaid under the terms required to receive federal dollars, but did allow childless adults with income 100% or below the FPL to enroll in BadgerCare starting in 2015. In this paper, I classify Medicaid expansion states based on their 2014 expansion status, thereby excluding Indiana and Wisconsin. I account for the differences in the start-date for the other states in my program start definitions.

could enroll starting in October of 2013. [7] Unlike private insurance, individuals can enroll in Medicaid at any time in the year, given their income is below the qualifying threshold.

In addition to choosing whether to expand Medicaid, expansion states could set higher income thresholds than those proposed by the ACA and they were given the opportunity to enact these program changes before the 2014 implementation of other ACA components [8] (such as the federal mandate for every individual to be covered by insurance, the opening of the federal or state-based marketplaces to sell individual insurance plans, simplified eligibility and applications for Medicaid, etc.). Six states (California, Connecticut, DC, Minnesota, New Jersey, and Washington) opened Medicaid enrollment to those with qualifying incomes some time between 2010 and 2014. Though these states chose to expand early, these programs were limited in size and impact. In Connecticut, only childless adults with income below 56% of the FPL were allowed to enroll prior to 2014; the threshold then increased to 138% in 2014. Similarly, the income limit was set to only 23% of the FPL in New Jersey [9] for previously ineligible childless adults. Only in 2014 did Governor Christie then decide to adopt the full expansion, adding over 320,000 new enrollees to Medicaid. Some of the other early actors, like Minnesota, used the early expansion to transfer over 90% of their Medicaid enrollees over from pre-existing local programs, covering only roughly 10,000[10] new individuals prior to 2014 (Sommers et al., 2014). Washington similarly used the early expansion to transfer previously covered low-income adults onto Medicaid so as to receive federal dollars for these claims. Lastly, California used the early expansion to start county-driven programs to expand enrollment with county-imposed income caps (for example, in Sacramento childless adults had to have family income levels between 0 and 67% of the FPL). This program, referred to as the Low Income Health Program (LIHP), required counties to establish the infrastructure to incorporate new enrollees and, therefore, was implemented to varying degree.[11]

I document the change in Medicaid and other insurance coverage rates for the six early expansion states in Appendix Table 3.10. As we might expect from the limited rollout of the early Medicaid expansion, Table 3.10 confirms that these states did not have large increases in their Medicaid coverage rates of childless adults before 2014. For example, we know that from 2010 through December 2013 California added around 350,000 people to Medicaid; however, this jump is small compared to the over 2.2 million people it added in in 2014. [12]

The broader national expansion in 2014 brought significantly larger increases in Medicaid enrollment in these states than any prior years. This is likely due to the fact that many of the

[7]Michigan's benefits did not go into effect until April 1st, 2014 and Pennsylvania and New Hampshire did not change access to Medicaid until August of that year. Michigan, Pennsylvania, and New Hampshire are assigned different start dates in all the regressions below.

[8]These state-specific details are listed in Figure 3.2.

[9]http://www.healthinsurance.org/new-jersey-medicaid/

[10] This translates to around 5% of the total number of new enrollees that later joined in 2014.

[11]This implied "assigning enrollees to a medical home, and providing a minimum array of medical and behavioral health services." http://healthpolicy.ucla.edu/programs/health-economics/projects/coverage-initiative/low-income-health-program/Pages/default.aspx

[12]http://www.latimes.com/opinion/op-ed/la-oe-gorman-medi-cal-obamacare-california-20140812-story.html

early-expansion state programs increased their eligibility limits significantly in 2014, as well as ramped up publicity for the ACA and the possibility for people to enroll easily in Medicaid through the new exchanges. For this reason, I include these states in the 2014 expansion cutoff but also run the same regressions excluding observations from early expansion states to confirm that the results are unaffected. I remark on any differences in the results caused by including these states. For the vast majority of the analysis below, the results are not sensitive to this inclusion.

As of today, Medicaid enrollment has grown by over 11 million people, with over 85% of this growth occurring in expansion states. While there are aggregate reports for how much Medicaid enrollment grew in each state after 2014 (see Figure 3.3), there is less information about specific sub-populations. The types of people expected to have opted-in to Medicaid as a result of the expansion are described as more likely to be young males without children and either white or Hispanic (Kenney et al., 2012). Unlike many previous studies on the matter (Boyle and Lahey, 2014; Yelowitz, 1995; Blank, 1989; Meyer and Rosenbaum, 1999; Moffitt and Wolfe, 1990; Winkler, 1991), this policy variation directly affects the types of individuals that historically have had higher labor force attachment rates. For this reason, the Medicaid expansion in over half of the states in the US provides an interesting context to study the effect of government health insurance expansions on employment and labor force attachment.

3.3 Related Literature

The focus on how health policy may affect labor force decisions is not new to the ACA. Yelowitz (1995) investigates the risk of losing public health insurance on single mothers' labor supply. By taking advantage of changes in Medicaid eligibility across states, he measures whether and how much more mothers work when not confronted with strict earnings thresholds that determine Medicaid access. Also using the Current Population Survey to detect changes in employment, Yelowitz finds that women increase their labor supply by nearly one percentage point with increases in Medicaid earnings caps (Yelowitz, 1995) . Other previous studies on the effect of Medicaid on labor supply decisions provide conflicting evidence. Blank (1989) and Meyer and Rosembaum (2001) do not find any effects of the value of Medicaid on welfare participation or employment (Blank, 1989; Meyer and Rosenbaum, 1999); whereas, looking at the same policy change, Moffit and Wolfe (1992) and Winkler (1991) find similar effects to Yelowitz on employment of around -1 percentage point (Moffitt and Wolfe, 1990; Winkler, 1991).

More recently, there have been a handful of studies that have taken advantage of policy changes in public health insurance programs to evaluate the extensive and intensive margin responses of specific populations to health insurance access or dis-enrollment. Utilizing a mid-1990s expansion of health insurance for U.S. veterans, Boyle and Lahey provide evidence on the effects of public insurance availability on the labor supply of veterans and their spouses (Boyle and Lahey, 2010, 2014). Using data from the CPS to look at the veterans themselves, they find that older workers are significantly more likely to decrease work both on the extensive and intensive margins after receiving access to non-employer based insurance. As we might expect, when focusing on the spouse response, they find that the labor supply of spouses actually increases because this group did not qualify for the new benefits. Looking at another program's variation, Wisconsin's expansion of public insurance to non-disabled, childless adults similarly was found to affect the probability of employment - though the results yield a wide effect range of 0.9 to 10.6 percentage point decreases among those newly enrolled. (Dague et al., 2014).

The 2005 Tennessee Medicaid contraction, which revoked access to approximately 170,000 people, was used to look at the opposing effect of *losing* access to government health benefits. Using this policy variation, Garthwaite et al. find that the large program dis-enrollment was associated with an large increase in employment, particularly among people previously working at least 20 hours a week and receiving employer-provided health insurance (Garthwaite et al., 2013). On the other hand, a recent experiment providing public insurance did not find any strong effects on labor supply: in a limited expansion of Oregon's Medicaid program, qualifying individuals were randomly chosen to receive Medicaid coverage. This new access to Medicaid was not found to be associated with changes to either employment or earnings (Baicker et al., 2014). Other studies have similarly pointed to null employment effects from policies that increased access to health insurance (Antwi et al., 2012; Depew, 2012).

Specifically focusing on changes implemented through the ACA, the Urban Institute determined that there was a small increase in part-time work in 2014 but that this increase in part-time work can be attributed fully to an increase in involuntary part-time work rather than intentional shifts related to increased insurance access. The authors argue that this increase is due to a slow recovery in full-time jobs rather than pressure from the ACA (Garrett, 2014). This finding highlights the importance of controlling for other changes occurring around the time of the ACA when studying these questions.

Figure 3.1 provides the range of labor supply estimates from previous studies that take advantage of policy changes that altered access to health insurance. In the results section below, I discuss how I compare the results in this study to these findings.

3.4 Data

The data used in this paper come from the Current Population Survey (CPS). The CPS is a monthly survey of 60,000 nationally representative households conducted by the Census. Individuals within a sampled household are surveyed consecutively for four months, not followed for the next eight months, and then re-contacted and surveyed for four additional months. Each month, the CPS administers its base questionnaire to collect information on, among other things, every adult's labor force participation, hours of work, full-time status, type of work, hourly wages, household composition, and bucketed income group. The CPS also collects additionally useful information in its Annual Social and Economic supplement (a.k.a. March supplement). Most relevant to this study, this survey reports information regarding an individual's health insurance coverage in the previous year and their detailed annual income.[13]

Throughout the analysis, I restrict my sample to childless adults aged 18 to 64. [14] Table 3.1 describes characteristics of my CPS sample in 2012 by whether a state eventually expanded Medicaid. I choose to compare the demographic characteristics of states in 2012 since most states decided whether to expand in late 2012 and 2013. As shown in this table, expansion states had slightly higher unemployment rates and were already covering more individuals under Medicaid to begin with (this is also true when the early expansion states are excluded from the average). On the other hand, Medicaid expansion states appear to have a slightly smaller share of childless adults under 138% of the federal poverty level[15], possibly contributing to a state's decision for whether to expand or not. Importantly, the governor's political party also appears to strongly align with whether a state expanded. This signals that the decision to expand may have been more related to exogenous factors such as political affiliation, rather than endogenous characteristics such as the employment rate[16].

In addition to using the monthly data to study how employment statistics have evolved over time in these states, I use the latest available data from the 2014 March supplement to study how insurance coverage has changed in expansion states following the introduction of the ACA. Data from the March supplement also serves to calculate the share of childless adults with family income under 138% of the federal poverty level - the qualifying Medicaid threshold.[17] I use this information in order to isolate the "treated" population and its responses to the policy change. In other words, by knowing an individual's family income in the last year, I am able to identify childless adults who are more likely to have been affected by the Medicaid expansion. The research designs that take advantage of this detailed individual-level information are proposed below in the section describing my empirical approaches.

By using information from the March supplement, I introduce an incongruity in the timing of my outcome measures. Whereas the base-file employment responses all refer to the interview week and the expansion status dummies refer to the month in question, the health insurance variables from the March supplement refer to the previous year. [18] So, for

[13]Though this survey is primarily conducted with individuals that are being interviewed in month of March, it is also be administered in other months in order to increase the sampling of certain under-represented populations. Roughly 26% of observations in the March supplement are surveyed in other months (the preceding November, February, and following April) (Bureau, 2015). Since the surveys all reference the same calendar year, the non-March observations should not affect the integrity of the results presented below.

[14]An individual is categorized as childless if he or she is not a parent or does not have any own children under the age of 18 in the household.

[15]This percent is calculated off of individuals in the March CPS for which we can calculate their family's adjusted gross income

[16]Of course, the party in power may in fact be determined by a state's economic characteristics

[17]Medicaid eligibility is determined by a family's Modified Adjusted Gross Income (MAGI): AGI plus any tax-exempt Social Security income plus tax-exempt interest income and plus tax-exempt foreign income. After aggregating across family members' individual adjusted gross income (AGI), my measure may still be an underestimate since I do not account for social security income, interest income, or foreign income.

[18]An individual's AGI is also collected with respect to last year, but this is ok since last year's income

example, in 2013, for the employment question in the base survey individuals were asked "(THE WEEK BEFORE LAST/LAST WEEK), did you do ANY work for (pay/either pay or profit)?"; whereas, with respect to their insurance status in the March supplement, individuals were prompted with "At any point in 2012, were you covered by Medicaid / 'state program name'?'" (Bureau, 2015).

We may be worried that as a result of this timing issue, the data will be unable to measure the "first-stage" response of the expansion on Medicaid coverage rates in 2014. In actuality, it appears that individuals are prone to answer last year's insurance question with respect to their current insurance status. Swartz (1986) confirms this hypothesis by comparing CPS responses on health insurance coverage to three other national surveys which ask about *current* insurance status (Swartz, 1986). She finds that the trends in aggregate statistics align with the CPS time-series only when you assume that individuals in the CPS are answering with respect to their insurance status at the time of the interview. Furthermore, it has been documented that Medicaid coverage data from the March supplement undercount actual Medicaid receipt - with some studies finding rates in the CPS that are 43% lower than official sources (Davern et al., 2009). For this reason and the above, I treat the insurance data from the March questionnaire as a lower bound for Medicaid coverage rates among childless adults in a survey respondent's interview year. In the section outlining my results below, I discuss how this mis-measurement may affect the estimates of interest.

The most recent data available extends through March 2015 for the CPS base-file and extends through March 2014 for the March supplement. In Table 3.2 I document the average rates of insurance and public assistance coverage in the CPS among childless adults by whether a state expanded its Medicaid program. As seen above for the 2012 data, in the years before Obamacare was implemented Medicaid expansion states were already covering a higher share of childless adults through Medicaid. [19] This table excludes the states [20] that expanded Medicaid after March of 2014 so as to identify program impact in places where the expansion had already occurred by the time of the survey. In 2014, we see an increase of 1.7% in Medicaid receipt in expansion states, compared to an increase of 0.8% in non-expansion states. [21]

This data is represented graphically in Figure 3.4. In panel A, I plot the Medicaid coverage rates from the March supplement over time. As we can see, the percent of people covered by Medicaid by expansion category move together closely over time until 2014. The strong increase in Medicaid rates in the Medicaid expansion states points to the likely fact that many individuals respond to the insurance question using their current insurance

determines this year's Medicaid eligibility and I am purposefully interested in past income since it is less likely to have been affected by manipulation on the part of individuals trying to qualify for Medicaid. I discuss the assumptions required for the integrity of this measure in the empirical section below.

[19] Roughly 2/3 of childless adults on Medicaid before 2014 are either receiving Social Security Income or have a reported disability. The remaining one-third come from states that already covered some childless adults or is due to misclassification and other forms of measurement error.

[20] These states are Michigan, New Hampshire, and Pennsylvania.

[21] I confirm that there is not an increase in disability rates that could explain this difference.

status instead of last year's. Similar to the Medicaid coverage rates over time, the percent of childless adults under the qualifying poverty threshold also appear to co-move until 2014, at which point expansion states experience a surge in the percent under 138% of the FPL and non-expansion states experience a decline. This is surprising as this income measure is based off a family's adjusted gross income in the last year and should therefore be less likely to respond to a policy enacted in 2014 and proposed in most states in mid-to-late 2013. Below, in the results section, I explore whether this increase is statistically significant in a regression framework when controlling for other state characteristics.

3.5 Empirical Approach

This paper relies on three different approaches to compare the labor force outcomes in states that expanded Medicaid to those in states that refrained from expanding. These three approaches rely on data from the Current Population Survey, which has employment information through March of 2015 and Medicaid coverage data through March of 2014[22], as described above in Section 3.4.

In all three approaches, I look at the effect of the Medicaid expansion on Medicaid receipt in the March Supplement sample and separately on employment outcomes in the base sample. My first approach consists of a difference-in-differences specification in which the effect of the Medicaid expansion is identified based on differences in whether an individual's state of residence state participated in the program and on differences over time among these groups (i.e. before and after the policy change). This approach assumes that states are comparable across expansion status and would have remained comparable in the same way over time absent the Medicaid policy intervention. Thus, I model outcomes (Y_{sti}) in a given state, s, at time t, for individual i as determined by the following process:

$$Y_{sti} = \delta Exp_s \times \{\mathbf{I}(t = 2014)\} + \alpha Exp_s + \gamma_t + \mathbf{X}_{sti}\beta + \epsilon_{sti} \tag{3.1}$$

where Exp_s is a dummy for whether a state is a Medicaid expansion state, $\{\mathbf{I}(t = 2014)\}$ is a dummy that turns on in and after 2014 (or, when using monthly data, the month in 2014 when a state expanded Medicaid), γ_t are time-dummies, and $\mathbf{X}_{sti}$ is a vector of individual characteristics. We are interested in δ, which captures any effect on labor supply outcomes in Medicaid expansion states once the programs have been enacted in 2014. All regressions cluster standard errors by state so as to absorb any state-level serial correlation in the errors. The underlying assumption here (and below) is that:

$$E[\epsilon_{sti} | Exp_s, \{\mathbf{I}(t = 2014)\}, \gamma_t, \mathbf{X}_{sti}] = 0,$$

implying that the decision to expand is unrelated to other labor force determinants in Medicaid expansion states. In other words, this formulation assumes that absent any policy

[22] As discussed above, roughly a quarter of observations in the March supplement come from other months. These observations are included in the regressions below

changes, any differences in labor supply outcomes across Medicaid and non-Medicaid states can be explained by overall state differences, changes in state demographics, and a random error.

I examine the basis for this assumption by comparing unconditional employment rates, labor-force status, full-time status, and hours per week over time across expansion status (see Figures 3.5 - 3.6). Full-time rates and average hours per week appear to move much more similarly over time across expansion status than employment and labor force participation rates. From these graphs, we notice that, following 2014 and among expansion states, employment rates do not not fall and the percent of those out of the labor force does not rise.

Because I have monthly time-series data before the policy change, I can also look at pre-expansion employment responses in the expansion states to study common pre-trends.[23] In other words, I model:

$$Y_{sti} = \delta_{-\tau} Exp_s \times \{\mathbf{I}(7/2013 \leq t \leq 12/2013)\} + \delta Exp_s \times \{\mathbf{I}(t = 2014)\} \qquad (3.2)$$
$$+\alpha Exp_s + \gamma_t + \mathbf{X}_{sti}\beta + \epsilon_{sti}$$

where, I evaluate differences in the pre-expansion period for 6 months. So, $\delta_{-\tau}$ captures any changes in employment outcomes in Medicaid expansion states in the 6 month before expansion and δ represents the effects once Medicaid was actually expanded. [24]

The ability to identify an increase in Medicaid coverage rates in the data depends entirely on the extent to which, when responding about their insurance status, individuals refer to their current coverage versus their past year's. The higher the share of individuals that responds with their current insurance status, the closer δ will be to capturing the actual effect of the Medicaid expansion on Medicaid coverage rates. [25] Also, if the CPS consistently undercounts Medicaid coverage rates then δ could be biased downwards. As seen below in the results section, I am able to identify an increase in Medicaid take-up rates but, for the reasons just discussed, this should be seen as a lower bound for the true percentage point increase due to the high-likelihood of mis-measurement of Medicaid rates in the CPS.

Given that we may be worried about state-specific changes over time jeopardizing the common-trends assumptions inherent in a differences-in-differences approach [26], for my sec-

[23]We cannot do this for the insurance coverage data since that comes from the March supplement and, therefore, is yearly.

[24]While people could sign up for Medicaid before January of 2014, benefits were not provided until the first day of the year. Though individuals may wait to leave their jobs until they have other health benefits, there may also be pre-emptive moving. This clouds our interpretation of changes in the employment outcomes before 2014. Similarly, individuals may be adjusting their labor supply in order to qualify for health insurance.

[25] This is obviously not the case for any of the ASEC respondents that are actually interviewed in the prior year. The 6.6% that are interviewed in the previous November will decrease my ability to detect a program effect in Medicaid expansion states in 2014 since they will either report their current insurance status (2013) or their insurance status in 2012.

[26]For example, if the Medicaid expansion states received an economic shock in 2014, a difference-in-differences regression will interpret this shock as an effect of the Medicaid expansion.

ond approach, I introduce a triple-differences (DDD) set-up in which I can control for within-state changes over time by comparing outcomes among those most likely to be affected by the Medicaid expansion to those of unaffected individuals in the same state.

This specification necessitates a counterfactual group whose Medicaid eligibility is unchanged even within states that expanded their Medicaid programs. I consider anyone whose family AGI is below 138% of the federal poverty level in the last year (most states' eligibility cutoffs[27]) as the "treated" group and refer to the treatment dummy to as $\{\mathbf{I}(Under138\%)\}$. [28]

I use past income to identify the individuals who are most likely to be affected by increased access to insurance because past income is highly correlated to current income but is less likely to be endogenous to the policy change. In other words, because current income is used to determine Medicaid eligibility, past income should not be related to whether or not a person lives in an expansion state and wants to access Medicaid.

For this strategy to work, it must be true that individuals with past income above the eligibility limit will not make employment choices with the new Medicaid rules in mind. So, employment changes for this group should represent choices that are independent of and unaffected by the policy change. Though the Medicaid expansion makes it possible for individuals earning well above 138% of the FPL to drop out of the labor force and remain uninsured, it seems implausible that high-earning individuals would drastically reduce their income (by way more than the value of insurance) in order to access Medicaid. The introduction of subsidies for insurance and the inability to discriminate against pre-existing condition, provide even less reason for individuals with high incomes to try and qualify for Medicaid. However, this may be less true for individuals who are very close to the earnings threshold.

We may be worried that people right above the cutoff will change their behavior so as to land below the cutoff and qualify for Medicaid. This response will bias my results downward, since these individuals are classified in the counterfactual "untreated" group. Given that the number of individuals near this threshold is likely to be very low compared to the number of individuals with which they are grouped, this type of endogenous response should not cause too much worry. Secondly, The results are not sensitive to where we set the income cutoff. In other words, when looking at higher thresholds, like 150% or 200% of FPL, I am still able to detect effects on Medicaid coverage rates in Medicaid expansion states and unable to detect differences in employment responses. But, it is still the case that this approach ultimately will not account for any employment or labor force changes coming from individuals with high past income that truly are due to the new low-income eligibility windows.

Furthermore, I assume that income in period $t-1$ is not influenced by upcoming policy changes. This seems legitimate since the majority of expansion states did not choose whether to expand until the second half of 2013 (see Figure 3.2) and individuals receive

[27]For states with higher cutoffs, I use their own higher limit instead

[28]I could also consider using adults with children as the counterfactual group. However, the eligibility thresholds for his group were also changing in the majority of states that expanded Medicaid.

Medicaid based on their current income. However, my estimates could be biased downwards if individuals responded to future Medicaid expansions by adjusting their household income or employment the year before the Medicaid expansion. This is because some of the adjustments, though due to the policy change, would have happened prior to 2014 and would therefore not be attributed to the policy dummy of interest, δ, which turns on in 2014.

I use the following triple-differences specification that takes advantage of the information on an individual's prior income:

$$\begin{aligned} Y_{gsti} = {} & \delta Exp_s \times \{\mathbf{I}(Under138\%)\} \times \{\mathbf{I}(t=2014)\} \\ & +\lambda_1 \{\mathbf{I}(Under138\%)\} \times \{\mathbf{I}(t=2014)\} \\ & +\lambda_2 \{\mathbf{I}(Under138\%)\} \times Exp_s + \lambda_3 Exp_s \times \{\mathbf{I}(t=2014)\} \\ & +\lambda_4 \{\mathbf{I}(Under138\%)\} \\ & +\alpha Exp_s + \gamma_t + \mathbf{X}_{gsti}\beta + \epsilon_{gsti} \end{aligned} \tag{3.3}$$

where as mentioned above $\{\mathbf{I}(Under138\%)\}$ defines my within-state comparisons group and is equal to 1 when an observation's prior year adjusted family income is below 138% of the FPL. As before, $\{\mathbf{I}(t = 2014)\}$ is a dummy for observations in Medicaid expansion states after Medicaid was expanded and Exp_s is a dummy for whether a state is a Medicaid expansion state. The vector $\mathbf{X}_{gsti}$ is for individual-level characteristics and γ_t introduces year dummies.

The information on an individual's prior adjusted gross family income comes from the March supplement and, thus, the frequency of the independent variable that provides the third level of difference is yearly. I correspond this measure to the employment outcomes collected in March. For this reason, the sample is limited to individuals in the March sample.

I introduce a third approach which does not restrict the triple-difference regressions to observations from the March supplement. In order to do the same analysis with individuals in all months of the CPS, I construct a model to predict last year's income threshold (above or below 138% of FPL) in the March supplements based on static demographic information and I apply this prediction to the observations in the monthly CPS base sample. [29] This way, I can look at how employment trends vary in months other than just March for those predicted to be in the lower income strata.

Thus, I use the following specification to run a triple-differences model on the employment data in the monthly base sample:

$$\begin{aligned} Y_{gsti} = {} & \delta Exp_s \times \widehat{Under138}\% \times \{\mathbf{I}(t=2014)\} \\ & +\lambda_1 \widehat{Under138}\% \times \{\mathbf{I}(t=2014)\} \end{aligned} \tag{3.4}$$

[29] The model used is a logistic regression that predicts whether someone's family income was equal to or below 138% of the FPL in the last year. The model takes advantage of demographic information such as sex, age, race; as well as education-level and marital status.

$$+\lambda_2 Exp_s \times \{\mathbf{I}(t = 2014)\} + \lambda_3 \widehat{Under138\%} \times Exp_s$$
$$+\lambda_4 \widehat{Under138\%}$$
$$+\alpha Exp_s + \gamma_t + \mathbf{X}_{gsti}\beta + \epsilon_{gsti}$$

where, now, our within-state comparison groups are defined by the predicted probability (ranging from 0 to 1) that someone had family income below 138% of the FPL last year ($\widehat{Under138\%}$). Since this is an estimated independent variable, I bootstrap the standard errors for all regressions using this third approach. As mentioned above, this approach allows us to extend the triple-differences outside of the observations from March supplement to the full base sample and not rely on the basic difference-in-differences. The D-in-D results may reflect unrelated changes in Medicaid expansion states at the time of the expansion and only capture the aggregate effects of the expansion on employment, rather than the effect on the newly-eligible.

3.6 Analysis

Effects on Insurance and Employment Outcomes

I begin by confirming the findings from the aggregate statistics in Table 3.2. The increase in Medicaid take-up in expansion states is apparent in a difference-in-differences (D-in-D) specification that uses a sample of observations from the March supplements from 2010 through 2014. I use a linear probability model (specification (1) above) to predict insurance status at the individual level. These regressions control for individual characteristics, which could vary over time within states, as well as state- and quarter-fixed effects. As the regression results in Table 3.3 show, Medicaid expansion states experienced a 1.5 percentage point (16%) increase in Medicaid coverage rates, significant at the 1% level. [30]

The other regressions in this table (Columns 2 through 5) look at the changes in other insurance outcomes in order to understand whether people substituted Medicaid coverage for other forms of insurance. We see that the increase in Medicaid coverage is paired with a slight decrease in Medicare coverage (less than half a percentage point). In some cases, Medicaid covers drugs and care that Medicare does not cover but it is possible to be covered by both Medicare and Medicaid and the CPS does not preclude you from being covered by both in its survey, either. This makes the decrease in Medicare rates a curious finding. It might be the case that individuals mistook Medicaid for Medicare and that this effect is due in reality to the salience of Medicaid in Medicaid expansion states (or that some individuals that say that they are covered by Medicare are in reality covered by Medicaid and a higher share of those in Medicaid expansion states corrected their mistakes). Nonetheless, this cautions interpreting the full increase in Medicaid coverage rates as coming from new enrollees. Furthermore, these

[30] This corresponds to slightly more than half of the average percentage increase in reported enrollment presented in Figure 3.3. These enrollment figures include increases in enrollment among non-childless adults and children as well.

results do not show any large changes in the likelihood of being covered by an employer, as a dependent, or of being uninsured.

Lastly, Column 6 identifies a statistically significant increase in the likelihood that an individual is below 138% of the federal poverty level. This increasing trend in the share of low-income childless adults in Medicaid expansion states was apparent in the unconditional averages presented in Figure 3.4(b) and roughly matches the increase in individuals covered by Medicaid in these states. This introduces worry as to whether individuals were adjusting their income in order to preemptively qualify for Medicaid.

I now transition to look at employment outcomes by Medicaid expansion status in the monthly CPS. The time trends for the outcomes I examine are presented in Figures 3.5 and 3.6, with the outcomes averaged across states based on whether a state expanded Medicaid by 2014. [31] These figures plot the average percent of individuals employed, average percent out of the labor force, average percent working full-time (given that a person is working), and the average hours per week (again, given a person is working). The difference-in-differences assumption of common trends before the policy changes appear to be reasonable when examining full-time rates and average hours per week; however, we see that this assumption may be less acceptable for employment rates and the percent of individuals out of the labor force. Below, I discuss ways in which I address this possibly unrealistic assumption.

In Table 3.4, I examine employment effects using a D-in-D specification. The regression framework allows us to control for sample demographics (i.e. - the composition of each state's sample), as well as account for the states that expanded their Medicaid programs after January of 2014. All standard errors are clustered by state and the regressions for full-time status and hours of work only include individuals who are employed. There are no apparent statistically significant effects of the Medicaid expansion on overall employment, being out of the labor force, likelihood to be working full-time, or hours of work.

In the bottom half of Table 3.4, I run the basic D-in-D specification but I also include an interaction with a state's expansion status and a dummy for 6-months before the expansion, in order to test our assumption of common trends across Medicaid expansion status (specification (2) above). This specification shows that Medicaid expansion states had decreasing trends going into 2014 for employment, labor force participation, and full-time rates. These trends either signal pre-expansion adjustment in employment choices or common preceding shocks to Medicaid expansion states. Even when accounting for differences 6 months prior to 2014, we still do not see any effects on these outcomes once Medicaid coverage kicked in.

In addition to the problematic evidence for differences in pre-trends, the basic differences-in-differences does not allow us to look at employment changes among the Medicaid eligible specifically. Instead, the approach presented in Table 3.4 will also capture any aggregate effects of the Medicaid expansion on employment. So, for example, if more individuals are hired in the health care industry in order to serve the new Medicaid enrollees, we may

[31] For the purpose of these graphs, the three Medicaid expansion states that expanded after March of 2014 are not included in these figures but are included in the regressions below where I can account for the different points at which they expanded their programs.

identify an increase in overall employment but this change will not reflect the effect of an individual's new eligibility for Medicaid. I am interested in the causal response to new health insurance access, rather than any effects from the overall program implementation or increased spending on Medicaid.

Given that we want to study the group of individuals that were affected by the expansion and that there may be reason to doubt the assumption of common trends over time across expansion and non-expansion states, I introduce a third level of variation across which to study the effects of the Medicaid expansion on labor supply. As described in Section 3.5, by introducing a sub-group that is not affected by the Medicaid expansion I can control for common within-state changes to labor supply.

In order to isolate the Medicaid eligible, I identify individuals with family incomes that were below 138% of the federal poverty level in the past year. The idea behind this cutoff is that individuals who earned far above the eligibility cutoff will not qualify for Medicaid and are substantially less likely to manipulate their earnings so as to qualify. In other words, this approach is plausible since we do not expect high-earners to drastically lower their incomes in order to receive Medicaid benefits. This group can therefore serve to control for the general macro-shocks to the state's employment conditions. Assuming individuals did not respond to the policy change before it was enacted, by using past income, instead of current income, I do not have to worry about capturing endogenous income responses to the policy change. We know from the regressions above that there may have been employment shifts prior to the enactment of the policy in order to qualify for Medicaid. These intentional shifts would violate the assumptions of the triple-differences (DDD) approach. I present the results from this specification (equation 3) below.

Using outcomes from 2010 through 2014, the DDD regressions in Table 3.5 illustrate that low-income individuals in expansion states experienced a 1.9 percentage point increase in Medicaid coverage in 2014 (significant at the 10% level). As we might expect, individuals with income above the qualifying threshold are not found to have any increase in Medicaid take-up (0.4 percentage points and not significant). Similar to the regression results from the basic D-in-D, we see a similarly sized decline in Medicare rates among the bottom income group (1.8 percentage points, significant at the 1% level). These results do not present much evidence for crowd-out of employer-provided coverage. Instead the point estimates also suggest that the increase in coverage may have come came previously uninsured individuals, with a large statistically significant decline in the percent of people unemployed (2.2 percentage points). However, the combined effects among these two groups are larger than the increase in Medicaid coverage

Compared to the D-in-D, the DDD regression finds larger changes in Medicaid coverage among those with lower income in the last year in Medicaid expansion states. I now use this treatment group to examine the DDD results for employment outcomes. The results in Table 3.6 coincide largely with the results presented in both of the previous specifications. There are no apparent effects on employment, likelihood to be out of the labor force, full-time rates, or hours for the triple-interaction group. The point estimates are all insignificant and smaller in magnitude than the percentage point increases in coverage. Interestingly, there do appear to be decreases in the likelihood of being employed and increases in the likelihood of being out of the labor force for the counterfactual group in Medicaid expansion states. Assuming the assumptions behind the triple-differences approach are valid, these differences provide justification for controlling for the underlying trends over time in expansion states.

As mentioned before, we may be worried that the income measure is endogenous and therefore does not provide a good proxy to define affected and unaffected groups within expansion states. As a robustness test, I run the same regression using income from two years ago on the limited sample of individuals we see from one year to the next in the March supplement. These results are nearly identical to those in Table 3.5 and Table 3.6. This is reassuring since income from 2012 should be significantly less likely to be adjusted endogenously in order to qualify for Medicaid benefits in 2014. These results can be provided upon request.

As discussed in my description of the empirical approach, part of the reason I may not be finding effects in the triple-differences regressions is that I am limited to observations from the March supplement that provide information on an individual's previous year's family income. In order to try to increase the power of these regressions I implement a third strategy that allows me to look at employment outcomes in the monthly base file. Using demographic information such as gender, race, age, marital status, and education, I model an individual's likelihood of being below the qualifying income threshold in the March supplement and I use this model to predict this likelihood for all childless adults in the CPS base file. By predicting last year's family income using individual-level information that is less likely to be changing in relation to policy changes, I reduce the concerns for endogeneity bias in using income as a treatment indicator. However, this approach introduces less precision in the third-level of variation and, therefore, possibly less precision to identify effects.

Table 3.7 shows how the estimated probability performs in predicting Medicaid take-up for those with a higher probability of being lower income. We see that for every 0.1 increase in the probability of having income below 138% of the FPL, individuals in Medicaid expansion states were 0.3 percentage points more likely to have newly received Medicaid benefits (the predicted probability of being low-income among the March supplement observations ranges from 0 to 1 with a mean of 0.22). Though this effect is not significant at the 10% level, I proceed to analyze employment outcomes below to verify that there are also no apparent effects while using this measure for a triple DDD approach.

Table 3.8 analyzes the employment outcomes using the predicted probability of being under 138% of the FPL. Similar to the prior DDD regression using actual income, individuals with attributes that increase their likelihood of qualifying for Medicaid are not less likely to be employed. They may be less likely to work full-time but this effect is not significant and the effect on hours of work is less than a one half-hour decrease (also insignificant). However, it appears that predicting income levels on the CPS base sample does not yield enough precision to be wholly confident that there were not 1:1 disemployment effects (meaning, I cannot say for certain whether every person that took-up Medicaid under the new Medicaid expansion laws stopped working).

As we see, individuals predicted to be above this qualifying income threshold in Medicaid expansion states are more likely to be out of the labor force but more likely to work full-time, conditional on working. This signals the existence of other economy-wide effects of the Medicaid expansion and the importance of finding relevant ways to identify those affected by increased access as a way to measure their specific employment responses.

Employment Response Ratio

Given my estimates for the expansion-related increases in Medicaid coverage and small and statistically insignificant changes in employment, I calculate employment response ratios to assess how my results compare to previous estimates in the literature. This ratio comes from dividing the employment response for childless adults living in Medicaid expansion states by the increase in Medicaid take-up attributed to the expansion among this group. As a result, this measure represents the percentage change in employment as a share of the percentage increase in Medicaid coverage for childless adults and, in theory, should range from -1 to 1. I construct this ratio for my three approaches, as well as past findings from other studies, in order to standardize employment effects by their associated increase in insurance take-up.

My three estimates for the employment response ratio come from the following regressions: one from the basic D-in-D regression that exploits variation across time and expansion status, one from the DDD regression using observations from the March supplement and introduces variation by past-income, and one from the DDD regression on the monthly CPS sample where the third difference (the low-income group) comes from a predicted probability of being low income. The estimates for the three Medicaid take-up rates, employment changes, and implied ratios (as well as their standard errors) are listed in Table 3.9. Unlike previous estimates in the literature, I only find a negative employment response in one of three cases (the difference-in-differences approach). However, as mentioned above, the basic difference-in-differences approach does not account for state-specific trends which were occurring around the time of the expansion and may be incorrect as a result.

As mentioned above when describing the approaches taken, my employment ratios are likely upwardly biased in absolute magnitude due to a likely downward bias in Medicaid coverage rates in the CPS. In other words, because my estimates most likely do not reflect the full extent of Medicaid take-up by childless adults, the denominator in my employment response ratio will be too low. Overall, this problem is made somewhat irrelevant due to the fact that I do not find any strong employment responses.

I construct the standard errors for the ratios using the Delta Method. [32] As can be seen below, the implied standard errors are very large and therefore do not allow us to reject any of the estimates in prior work. The standard errors all span a range greater than the possible estimates ([-1,1]). This is due to the fact that I am unable to estimate the employment effects with a lot of precision and to the fact that the employment ratio is being constructed with a denominator that is close to 0. Using 1600 bootstrapped samples to construct empirical distributions of the ratio yields similarly large confidence intervals

[32]For simplicity, I assume that the covariance between the coefficients for the Medicaid and employment responses is 0. This is not true given that some of the same individuals are included in each sample.

in both of the triple-difference specifications. In the case of the difference-in-differences specification, the bootstrapped 95% confidence interval is [-0.77, -0.17]; however, we know this estimate may capture broader differences between the states expanding Medicaid, as well as general effects of the expansion on employment.

3.7 Conclusion

The expansion of Medicaid to childless adults has been in place for over a year in roughly 29 states (including Washington, D.C.). This paper sets out to determine whether the resulting large increase in enrollment has resulted in any changes in the extensive or intensive labor supply margins among this affected group. This paper demonstrates that there was a detectable effect on Medicaid receipt in 2014 in Medicaid expansion states among childless adults. I find an overall 1.5 percentage point increase in Medicaid coverage in expansion states in 2014. Among my low-income subgroup in the March supplement, I identify a 1.9 percentage point increase in Medicaid receipt. When using predicted low-income status on the base-sample, I find an (imprecise) 3.0 percentage point increase.

Though I am able to identify relatively precise increases in Medicaid coverage in two of my three approaches, I do not find similarly large or consistent effects for changes in the likelihood to work, likelihood to be out of the labor force, likelihood to work full-time, or hours of work. Of the three approaches I use, none detect any statistically significant changes in employment and only one of the three coefficients points to a small decline in the employment rate of childless adults. The other two approaches yield small positive coefficients for changes in the employment rate. On the other hand, all three approaches find small (statistically insignificant at the 10% level) decreases in the likelihood that a childless adult will work full-time, conditional on working.

Unfortunately, these approaches do not provide enough power to reject the hypothesis of large employment responses by childless adults to the increased access to Medicaid introduced by the Medicaid expansion. As a result, I am unable to reject prior estimates from the literature based on these findings. Though we expect the biggest enrollment increases to have occurred within this first year, continued growth is expected. For this reason and if individuals adjust slowly, lagged effects of the program expansion may continue to develop. It would be valuable to revisit these analyses as more time has passed, as well as explore these effects in other datasets.

Table 3.1: State Summary Statistics by 2014 Expansion Status

	Non-Expansion	2012 Expansion	Expansion States excluding Early States
% Employed	70.8	70.4	70.7
% Employed among Childless Adults	68.1	67.8	68.1
% Unemployed	6.7	7.3	7.1
% Out of LF	23.4	23.3	23.4
% between 18 and 44	55.6	55.6	54.5
% between 45 and 64	44.4	44.4	45.5
% Female	51.6	51.4	51.2
% Black	11.9	8.6	8.6
% Hispanic	11.8	13.7	9.8
% Married	56.3	53.3	53.8
% with only High School	30.3	28.3	30.14
% with College Degree	27.4	32.2	30.9
% of Childless Adults with Medicaid*	7.3	9.0	9.3
% of Childless Adults w/ Private Ins.*	67.0	71.3	72.0
% of Childless Adults w/ Employer Ins.*	41.1	42.7	43.0
% of Childless Adults w/o Ins.*	21.0	17.2	16.2
% of Childless Adults w/ Fam. AGI < 138% FPL in LY*	24.8	22.4	22.9
Republican Gov.	87.0	32.1	36.4
States	23	28	22

Notes: Yearly averages taken across 12 months of 2012 for each state. Averages taken for adults 18-65 in the CPS. Early expansion states include California, Connecticut, DC, Minnesota, New Jersey, and Washington. Expansion status determined by 2014 decisions. AGI stands for Adjusted Gross Income. * Taken from March supplement.

Table 3.2: Yearly Insurance, Medicaid, and Medicare Rates by Expansion Status: 2009-2014

Medicaid Expansion States						
	2009	2010	2011	2012	2013	2014
Covered by Medicaid	7.1	8.2	8.5	9.2	9.2	10.9
Covered by Medicare	3.6	3.6	3.8	4.4	4.0	3.3
Covered by Private Insurance	73.3	70.5	70.8	70.9	71.2	71.4
Covered by Employer Health Plan	46.2	44.1	43.2	42.7	42.5	42.3
Covered as a Dependent	23.6	22.7	23.2	23.3	23.4	22.4
Uninsured	17.0	19.0	18.3	17.5	17.3	15.4
Under 138% FPL	20.9	22.6	23.2	22.7	22.8	23.9
Obs	25					
Non-Expansion States						
	2009	2010	2011	2012	2013	2014
Covered by Medicaid	6.4	6.6	6.7	7.3	7.2	8.0
Covered by Medicare	4.5	4.3	4.5	4.8	5.0	4.5
Covered by Private Insurance	69.1	67.0	66.5	67.0	67.1	67.5
Covered by Employer Health Plan	43.8	42.1	41.2	41.1	40.7	40.9
Covered as a Dependent	21.5	20.9	20.5	20.6	20.7	20.0
Uninsured	20.1	21.6	21.9	21.0	21.3	19.1
Under 138% FPL	22.8	23.8	25.2	24.8	24.6	23.9
Obs	23					

Notes: Percentage covered among childless adults in CPS March Supplements, 18-64. Medicaid expansion state averages exclude Michigan, New Hampshire, and Pennsylvania which all expanded after March of 2014.

Table 3.3: Difference-in-Differences LPMs for Effect of Expansion on Medicaid, Medicare, Insurance Rates

	(1) MedAid	(2) MCare	(3) Emp. Ins.	(4) Dep. Ins.	(5) Unins.	(6) Under138FPL
Medicaid Expansion State	0.022**	-0.0035	0.036***	0.036***	-0.036***	-0.012
	(0.0084)	(0.0023)	(0.0090)	(0.0069)	(0.010)	(0.0080)
Post-2014	0.015***	0.00080	0.014**	0.0062	-0.032***	-0.003
	(0.0031)	(0.0017)	(0.0060)	(0.0047)	(0.0039)	(0.0046)
MedAid Exp. State X Post-2014	0.015***	-0.0042**	-0.0088	-0.0095	-0.0026	0.017**
	(0.0048)	(0.0018)	(0.0076)	(0.0065)	(0.0052)	(0.0066)
Observations	401703	401703	401703	401703	401703	401703

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Standard errors in parentheses and clustered by state. Sample is childless adults (18-64) in March supplements from 2009-2014. Difference-in-differences regressions include controls for sex, race, age, education, marital status, and year fixed effects. Year effects with respect to omitted year (2009). The results do not change if we exclude observations from early expansion states or if we run the regressions with state fixed effects instead of the medicaid expansion state dummy.

Table 3.4: CPS Monthly Observations: LPM for Effect of Medicaid Expansion on Employment Outcomes over Time

	Employment (1)	Out of LF (2)	FullTime (3)	Hours (4)
Differences-in-Differences				
Medicaid Expansion State	-0.012	0.0061	-0.019***	-0.79***
	(0.011)	(0.0089)	(0.0061)	(0.21)
YR=2014	0.011***	0.014***	0.0047*	0.54***
	(0.0031)	(0.0028)	(0.0024)	(0.079)
YR=2015	0.013***	0.018***	0.0093***	0.81***
	(0.0039)	(0.0020)	(0.0037)	(0.081)
MedAid Exp. State X Post-2014	-0.0045	0.0041	-0.00061	-0.017
	(0.0043)	(0.0039)	(0.0032)	(0.087)
Observations	3428186	3428186	2326478	2245143
Differences-in-Differences Including 6 Month Pre-Trend Dummy				
Medicaid Expansion State	-0.0090	0.0038	-0.017***	-0.78***
	(0.011)	(0.0090)	(0.0062)	(0.21)
YR=2014	0.012***	0.013***	0.0032	0.54***
	(0.0031)	(0.0028)	(0.0027)	(0.079)
YR=2015	0.015***	0.017***	0.0082***	0.82***
	(0.0040)	(0.0037)	(0.0027)	(0.084)
MedAid Exp. State X Post-2014	-0.0062	0.0056	-0.0016	-0.021
	(0.0040)	(0.0036)	(0.0030)	(0.086)
MedAid Exp. State X 6-mo before MedAid Exp.	-0.028***	0.024***	-0.016*	-0.074
	(0.010)	(0.0089)	(0.0093)	(0.14)
Observations	3428186	3428186	2326478	2245143

Notes: Sample is childless adults, 18-64 in monthly CPS from January 2009 - March 2015. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses and clustered by state. Difference-in-differences regressions control for sex, age, race, education, marital status, quarter and year fixed effects. Pre-Medicaid Expansion dummy defined as six months before Medicaid expansion. Results similar when excluding early expansion states.

Table 3.5: March Supplements: Effects of Medicaid Expansion on Insurance

Insurance Outcomes	MedAid (1)	MCare (2)	Through Emp (3)	Dep. Ins. (4)	Unins (5)
Medicaid Expansion State	-0.0049	0.00086	0.0041**	-0.052***	0.050***
	(0.0050)	(0.0019)	(0.0019)	(0.0027)	(0.0042)
Post-2014	0.0063**	-0.0058***	0.0031	-0.0041	-0.016***
	(0.0026)	(0.0015)	(0.0053)	(0.0041)	(0.0036)
Under 138% LY	0.12***	0.072***	-0.29***	-0.16***	0.19***
	(0.013)	(0.0038)	(0.0052)	(0.0059)	(0.0097)
Post-2014 X Under 138% LY	0.0069	-0.0021	-0.0030	0.0055	-0.029***
	(0.0068)	(0.0045)	(0.0039)	(0.0045)	(0.0074)
MedAid Exp. State X Under 138% LY	0.043**	-0.0070	-0.0028	-0.018**	-0.025*
	(0.017)	(0.0061)	(0.0069)	(0.0072)	(0.014)
MedAid Exp. State X Post-2014	0.0042	0.00081	-0.0064	-0.0018	0.00024
	(0.0044)	(0.0018)	(0.0059)	(0.0056)	(0.0052)
MedAid Exp. State X Post-2014 X Under 138% LY	0.019*	-0.018***	0.0099	0.0069	-0.022**
	(0.011)	(0.0051)	(0.0079)	(0.0074)	(0.010)
Observations	401703	401703	401703	401703	401703

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses and clustered by state. Triple-difference LPMs for individual outcomes, controlling for for sex, race, age, marital status, disability, education. Data from March supplements from 2009 through 2014. When excluding early-expansion states, the coefficient for Medicaid increase in 2014 decreases slightly to 0.013 with a p-value of 0.3.

Table 3.6: March Supplements: Effects of Medicaid Expansion on Employment Outcomes

Employment Outcomes	Employed (1)	OutofLF (2)	FullTime (3)	Hours (4)
Medicaid Expansion State	0.039***	-0.027***	0.0039**	-0.87***
	(0.0033)	(0.0036)	(0.0016)	(0.073)
Post-2014	0.017***	-0.0075***	0.0047	0.21**
	(0.0026)	(0.0027)	(0.0034)	(0.086)
Under 138% LY	-0.35***	0.31***	-0.20***	-4.74***
	(0.0065)	(0.0064)	(0.0083)	(0.27)
Post-2014 X Under 138% LY	-0.025***	0.034***	-0.0015	-0.27
	(0.0082)	(0.0060)	(0.011)	(0.32)
MedAid Exp. State X Under 138% LY	-0.0039	-0.00026	-0.019*	-0.27
	(0.0097)	(0.011)	(0.011)	(0.32)
MedAid Exp. State X Post-2014	-0.0064*	0.0078*	0.00020	-0.042
	(0.0034)	(0.0039)	(0.0040)	(0.12)
MedAid Exp. State X Post-2014 X Under 138% LY	0.0048	-0.0083	-0.0069	0.19
	(0.013)	(0.0099)	(0.016)	(0.42)
Observations	401703	401703	281372	260614

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses and clustered by state. Triple-difference LPMs for individual outcomes, controlling for for sex, race, age, marital status, and education. Data from March supplements from 2009 through 2014.

Table 3.7: Medicaid Take-Up by Medicaid Expansion Status and Predicted Poverty Status

	Medicaid Coverage
Medicaid Expansion State	0.038***
	(0.0070)
$\widehat{Under\ 138\%\ LY}$	0.13***
	(0.025)
Post-2014	0.015***
	(0.0039)
MedAid Exp. State X $\widehat{Under\ 138\%\ LY}$	0.080***
	(0.029)
Post-2014 X $\widehat{Under\ 138\%\ LY}$	0.0053
	(0.014)
MedAid Exp. State X Post-2014	0.0046
	(0.0058)
MedAid Exp. State X Post-2014 X $\widehat{Under\ 138\%\ LY}$	0.030
	(0.021)
Observations	401687

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors bootstrapped and clustered by state. This is triple-difference regression where I compare Medicaid take-up, over time, across expansion status, and by last year's predicted income level (above or below 138% of the FPL). Regression includes controls for sex, race, age, marital status, and education. Sample limited to childless adults under 65 in the March Supplements from 2010-2014.

Table 3.8: Predicted Poverty Status: Effects of Medicaid Expansion on Insurance and Employment Outcomes

Employment Outcomes	Employed (1)	OutofLF (2)	FullTime (3)	Hours (4)
Medicaid Expansion State	-0.0018	-0.0069	-0.019***	-0.78***
	(0.012)	(0.0095)	(0.0057)	(0.17)
Post-2014	0.0038	0.0048	0.00034	0.035
	(0.0037)	(0.0031)	(0.0033)	(0.12)
$\widehat{Under\ 138\%}\ LY$	0.18***	-0.18***	0.35***	9.24***
	(0.028)	(0.025)	(0.013)	(0.40)
Post-2014 X $\widehat{Under\ 138\%}\ LY$	0.010	0.013	0.025**	1.09***
	(0.012)	(0.010)	(0.011)	(0.34)
MedAid Exp. State X $\widehat{Under\ 138\%}\ LY$	-0.025	0.033	0.0064	0.00020
	(0.033)	(0.029)	(0.014)	(0.48)
MedAid Exp. State X Post-2014	-0.0024	0.012***	0.0086**	0.34***
	(0.0054)	(0.0047)	(0.0043)	(0.13)
MedAid Exp. State X Post-2014 X $\widehat{Under\ 138\%}\ LY$	0.012	-0.011	-0.017	-0.38
	(0.018)	(0.017)	(0.013)	(0.43)
Observations	3428186	3428186	2326478	2245143

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Bootstrapped standard errors (500 draws) in parentheses and clustered by state. LPMs for individual outcomes, controlling for for sex, race, age, marital status, education. Medicaid expansion state dummy absorbed by full set of state fixed effects. Data from CPS base sample from 2009 through 2014. This is triple-difference regression where I compare employment outcomes, over time, across expansion status, and by last year's predicted income.

Table 3.9: Estimates for Employment Ratio and Calculated Standard Errors

	Medicaid Take-Up	Emp. Response	Emp. Ratio
DD	0.015***	-0.0062	-0.43
	(0.0048)	(0.004)	(3.082)^
DDD	0.034*	0.0048	0.14
	(0.018)	(0.013)	(376.11)^
Pred. DDD	0.021	0.012	0.58
	(0.027)	(0.018)	(8.88)^

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. The employment ratio is calculated as the estimated employment response (percentage point change) divided by the estimated increase in Medicaid coverage (percentage point change) due to the Medicaid expansion. ^denotes standard errors for the employment ratio constructed using Delta-Method. 95% confidence intervals for the employment ratio from 1600 bootstrapped iterations are [-0.77,-0.17], [-1.78, 3.48], and [-3.69,4.91], respectively. Estimates for the Medicaid take-up and employment responses from results presented in regression tables 3.3, 3.4, 3.5, 3.6, 3.7, and 3.8.

Figure 3.1: Previous Research on Labor Supply Effects of Changes in Insurance Access

Study Authors	Data Source	Population	Time Period	Change in Coverage (pp)	Extensive Margin (pp)
Garthwaite et al., 2015	CPS	170k Tennessee Adults; primarly, childless	2005	-7.3 (1.7)	4.6 (2.0)
Baicker et al., 2014	Proprietary	30,000 Oregon Low-Income Adults	2008	25.6 (0.35)	-1.6 (1.4)
Depew, 2014	ACS	Young Adults under 19-29	2001-2010	6.3 (1.3)	-0.10 (0.22)
Heim, Lurie, and Simon, 2014	Tax Data	Young Adults	2008-2012	-	0.5++
Antwi et al., 2013	SIPP	Young Adults 19-25	2010	7.02 (0.69)	-0.58 (0.62)
Dague et al., 2013	Proprietary	Childless adults in Wisconsin	2009	100	-10.5 to -0.9 (4.45) to (0.2)
Dave et al., 2013	March CPS	Unmarried Pregnant Women	1980s-1990s	20	7***
Strumpf, 2011	March CPS	Single Mothers	1966-1970	x	2.41 (1.89)
Meyer and Rosenbaum, 2001	March and ORG CPS	Single Mothers	1984-1996	x	No Change
Yelowitz, 1995	March CPS	Single Mothers	1986-1991	100+	4.4 (1.56)+
Moffitt and Wolfe, 1992	SIPP	Female headed households	1986	34.5%+	-5.5
Winkler, 1991	March CPS	Female headed households	1986	10%	-1.3 to -0.9

Notes: This table presents estimates from previous work that use policy changes in insurance coverage to evaluate the effect of insurance access on employment. Standard errors noted in parentheses, when available. Otherwise, significance denoted using conventional stars. When percentages are shown rather than percentage points, I denote this with a %. + denotes simulation and ++ denotes insignificant effect. (Garthwaite et al., 2013; Baicker et al., 2014; Depew, 2012; Heim et al., 2014; Antwi et al., 2012; Dague et al., 2014; Dave et al., 2013; Strumpf, 2011; Meyer and Rosenbaum, 1999; Yelowitz, 1995; Moffitt and Wolfe, 1990; Winkler, 1991).

Figure 3.2: State Medicaid Eligibility Changes

State Regulations: Pre- and Post-Medicaid Expansion
% FPL that Qualifies for Medicaid for Adults w/ No Dependents

State Name	2014 Expansion Status	Gov: D/R (in 2013 or at time of decision)	Pre-Expansion Not Working	Pre-Expansion Working	Post Expansion All	Post Expansion Signed into Law	# of Uninsured w/ Income <138% FPL
ALABAMA	0	R	N/A	N/A	0%	-	254,000
ALASKA	0	R	N/A	N/A	0%	-	29,000
FLORIDA	0	R	N/A	N/A	0%	-	1253000
GEORGIA	0	R	N/A	N/A	0%	-	682,000
IDAHO	0	R	N/A	N/A	0%	-	90,000
INDIANA	0	R	N/A	N/A	0%	1/27/15	320,000
KANSAS	0	R	N/A	N/A	0%	-	126,000
LOUISIANA	0	R	N/A	N/A	0%	-	289,000
MAINE	0	R	N/A	N/A	0%	-	38,000
MISSISSIPPI	0	R	N/A	N/A	0%	-	200,000
MISSOURI	0	D	N/A	N/A	0%	-	293,000
MONTANA	0	D	N/A	N/A	0%	-	55,000
NEBRASKA	0	R	N/A	N/A	0%	-	72,000
NORTH CAROLINA	0	R	N/A	N/A	0%	-	593,000
OKLAHOMA	0	R	N/A	N/A	0%	-	246,000
SOUTH CAROLINA	0	R	N/A	N/A	0%	-	292,000
SOUTH DAKOTA	0	R	N/A	N/A	0%	-	34,000
TENNESSEE	0	R	N/A	N/A	0%	-	352,000
TEXAS	0	R	N/A	N/A	0%	-	1,186,000
UTAH	0	R	N/A	N/A	0%	-	116,000
VIRGINIA	0	R	N/A	N/A	0%	-	235,000
WISCONSIN	0	R	N/A	N/A	100%+	-	167,000
WYOMING	0	R	N/A	N/A	0%	-	19,000
ARIZONA	1	R	100%	100%	138%	6/17/13	336,000
ARKANSAS	1	D	N/A	N/A	138%	4/23/13	196,000
CALIFORNIA	1	D	N/A	N/A	138%	6/27/13	2,113,000
COLORADO	1	D	10%	20%	138%	5/13/13	238,000
CONNECTICUT	1	D	55%	70%	138%	Early	80,000
DELAWARE	1	D	100%	110%	138%	6/1/13	21,000
DC	1	D	200%	211%	215%	5/13/10	12,000
HAWAII	1	D	133%	133%	138%	6/28/12*	27,000
ILLINOIS	1	D	N/A	N/A	138%	7/22/13	572,000
IOWA	1	R	N/A	N/A	138%^	12/12/13	91,000
KENTUCKY	1	D	N/A	N/A	138%	5/9/13*	262,000
MARYLAND	1	D	N/A	N/A	138%	5/5/13	160,000
MASSACHUSETTS	1	D	N/A	N/A	138%	7/5/13	70,000
MICHIGAN	1	R	N/A	N/A	138%	9/16/13	443,000
MINNESOTA	1	D	75%	75%	138%~	2/-/13	122,000
NEVADA	1	R	N/A	N/A	138%	12/-/12	174,000
NEW HAMPSHIRE	1	D	N/A	N/A	138%^^	3/27/14	35,000
NEW JERSEY	1	R	N/A	N/A	138%	6/28/13	285,000
NEW MEXICO	1	R	N/A	N/A	138%	1/9/13	147,000
NEW YORK	1	R	100%	100%	138%	6/28/12*	631,000
NORTH DAKOTA	1	R	N/A	N/A	138%	4/-/13	19,000
OHIO	1	R	N/A	N/A	138%	10/21/13	454,000
OREGON	1	D	N/A	N/A	138%		224,000
PENNSYLVANIA	1	R	N/A	N/A	138%	8/28/14	379,000
RHODE ISLAND	1	I	N/A	N/A	138%	7/3/13	34,000
VERMONT	1	R	150%	160%	138%		10,000
WASHINGTON	1	D	N/A	N/A	138%	6/30/13	292,000
WEST VIRGINIA	1	D	N/A	N/A	138%	5/-/13	110,000

Notes: R stands for Republican, D is for Democrat, and I is for Independent. Sources: http://www.advisory.com/daily-briefing/resources/primers/medicaidmap and * Date support was signaled in a public statement. ** For purposes of this paper, Indiana is classified as not expanding though they will be in 2015. ^ denotes cost sharing for beneficiaries. Population based on Census estimations for 2013. Average percent of population that is uninsured and under 138% of FPL is 4.5% for non-expansion states and 5% for expansion states. ^^ Effective in August of 2014. + Gov. Walker decided to Expand BadgerCare to childless adults starting on April 1st, 2014.

Figure 3.3: Medicaid Enrollment Growth by State

Total Monthly Medicaid and CHIP Enrollment by Oct 2014

State	Pre-ACA Enrollment*	Post-ACA** Enrollment	Percent Change	Expanded Medicaid
United States	57,787,483	68,529,576	16.8%	-
Alabama	799,176	861,776	7.8%	0
Alaska	120,946	125,735	4.0%	0
Arizona	1,201,770	1,494,777	24.4%	1
Arkansas	556,851	815,675	46.5%	1
California	9,157,000	11,500,000	25.6%	1
Colorado	783,420	1,143,585	46.0%	1
Connecticut	-	767,157	-	1
Delaware	223,324	230,680	3.3%	1
DC	235,786	255,707	8.4%	1
Florida	3,104,996	3,375,405	8.7%	0
Georgia	1,535,090	1,733,367	12.9%	0
Hawaii	288,357	291,505	1.1%	1
Idaho	251,926	285,660	13.4%	0
Illinois	2,626,943	3,062,543	16.6%	1
Indiana	1,120,674	1,198,070	6.9%	0
Iowa	493,515	569,181	15.3%	1
Kansas	378,160	400,646	5.9%	0
Kentucky	606,805	1,040,548	71.5%	1
Louisiana	1,019,787	1,051,248	3.1%	0
Maine	-	291,930	-	0
Maryland	856,297	1,077,179	25.8%	1
Massachusetts	1,296,359	1,493,702	15.2%	1
Michigan	1,912,009	2,151,130	12.5%	1
Minnesota	873,040	1,164,750	33.4%	1
Mississippi	637,229	698,505	9.6%	0
Missouri	846,084	843,692	-0.3%	0
Montana	148,974	159,365	7.0%	0
Nebraska	244,600	237,921	-2.7%	0
Nevada	332,560	558,934	68.1%	1
New Hampshire	127,082	158,347	24.6%	0
New Jersey	1,283,851	1,645,149	28.1%	1
New Mexico	572,111	748,832	30.9%	1
New York	5,678,417	6,212,308	9.4%	1
North Carolina	1,595,952	1,737,525	8.9%	0
North Dakota	69,980	82,014	17.2%	1
Ohio	2,341,481	2,838,379	21.2%	1
Oklahoma	790,051	811,979	2.8%	0
Oregon	626,356	1,028,198	64.2%	1
Pennsylvania	2,386,046	2,420,980	1.5%	0
Rhode Island	190,833	260,815	36.7%	1
South Carolina	790,229	888,326	12.4%	0
South Dakota	115,501	116,100	0.5%	0
Tennessee	1,244,516	1,390,324	11.7%	0
Texas	4,441,605	4,679,930	5.4%	0
Utah	294,029	276,963	-5.8%	0
Vermont	127,162	184,867	45.4%	1
Virginia	935,434	941,639	0.7%	0
Washington	1,117,576	1,590,657	42.3%	1
West Virginia	354,544	529,725	49.4%	1
Wisconsin	985,531	1,034,850	5.0%	0
Wyoming	67,518	71,296	5.6%	0

Notes: See http://kff.org/health-reform/state-indicator/total-monthly-medicaid-and-chip-enrollment/# for sources. * Average enrollment from October 2013 to October 2014. ** Medicaid / CHIP enrollment. The average percentage increase in Medicaid expansion states was 30.3% compared to 6.2% in non-Medicaid expansion states.

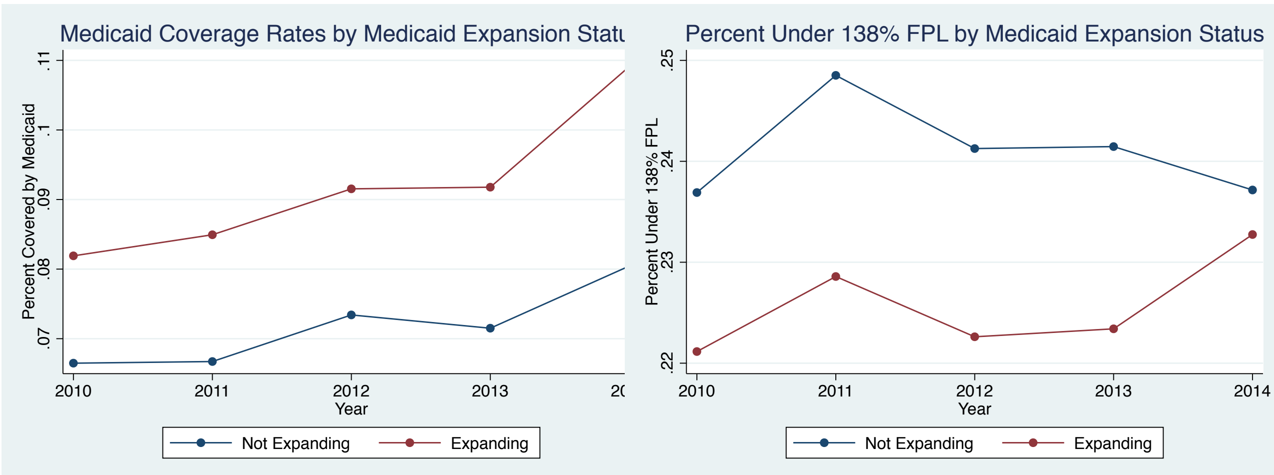


Figure 3.4: Time trends for Medicaid and FPL Outcomes

Notes:
Data from March supplement of CPS. Sample restricted to childless adults, 18-64, and excludes states that did not expand Medicaid before March 2014.

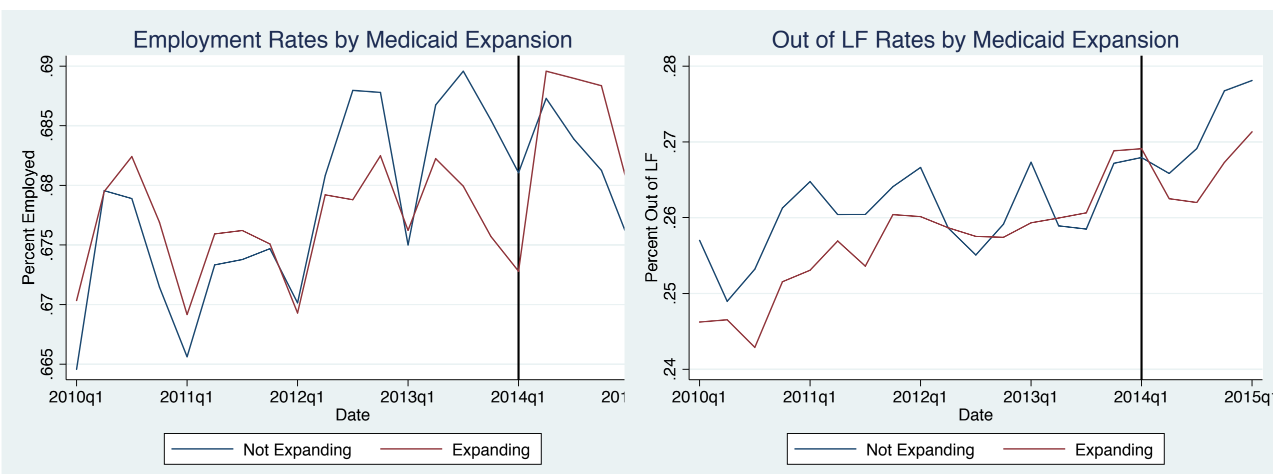


Figure 3.5: Time trends for Employment and Out of LF Rates

Notes:
Sample restricted to childless adults, 18-64, and excludes states that did not expand Medicaid in 2014.

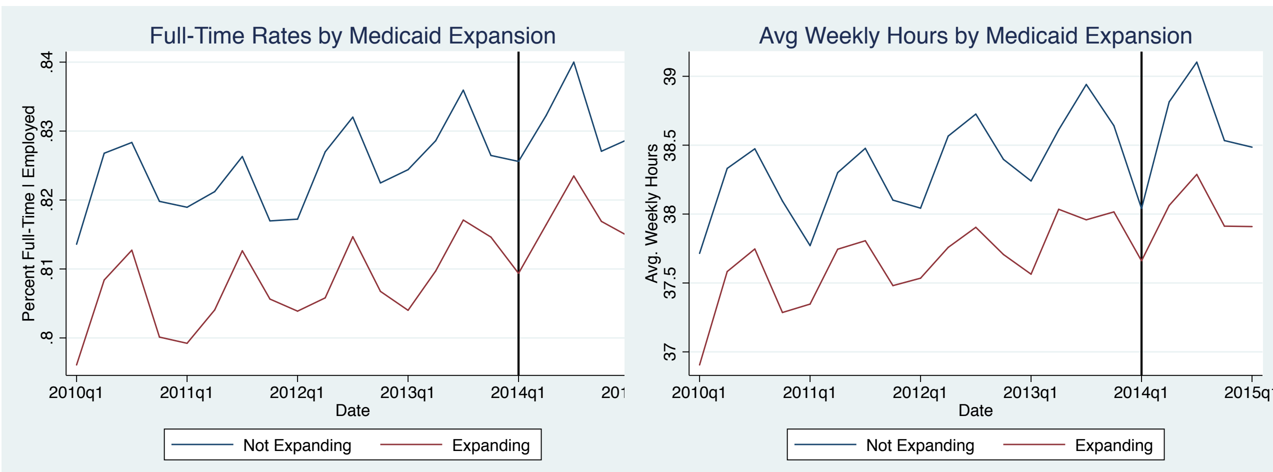


Figure 3.6: Time trends for Full-Time Rates and Avg. Hours / Week

Notes: Full-Time rates and Hours per Week only for those working. Sample restricted to childless adults, 18-64, and excludes states that did not expand Medicaid in 2014.

A1 Appendix

Table 3.10: Yearly Insurance and Eligibility Rates for Childless Adults in Early Expansion States: 2009-2014

	California					
	2009	2010	2011	2012	2013	2014
Covered by Medicaid	6.9	8.3	8.7	8.5	8.9	12.3
Covered by Medicare	2.4	2.8	2.9	3.6	3.0	2.7
Covered by Private Insurance	68.8	65.4	66.9	65.0	66.8	66.6
Covered by Employer Health Plan	42.2	40.1	39.7	38.1	37.7	38.0
Covered as a Dependent	21.8	20.9	21.4	20.7	23.0	21.7
Uninsured	22.7	24.4	23.1	24.5	22.8	19.7
Under 138% FPL	21.9	24.1	25.4	24.0	23.2	24.3
	Connecticut					
	2009	2010	2011	2012	2013	2014
Covered by Medicaid	5.7	3.6	4.9	5.8	10.7	10.3
Covered by Medicare	4.2	4.2	3.2	5.0	3.6	3.2
Covered by Private Insurance	77.1	76.6	78.1	79.5	78.5	79.6
Covered by Employer Health Plan	46.6	46.4	45.3	45.5	45.7	45.5
Covered as a Dependent	28.1	30.3	29.5	30.5	28.7	29.8
Uninsured	20.1	21.6	21.9	21.0	21.3	19.1
Under 138% FPL	17.2	16.7	14.3	16.1	16.5	18.6
	District of Columbia					
	2009	2010	2011	2012	2013	2014
Covered by Medicaid	9.0	11.1	10.7	14.4	14.9	16.0
Covered by Medicare	4.7	3.9	4.2	4.9	2.9	2.5
Covered by Private Insurance	75.9	73.8	71.3	71.8	73.3	73.0
Covered by Employer Health Plan	55.9	56.6	53.0	49.9	54.2	51.3
Covered as a Dependent	13.5	11.2	14.4	13.6	12.8	11.8
Uninsured	12.5	13.5	15.9	11.7	10.0	9.8
Under 138% FPL	29.3	29.7	31.2	30.9	27.6	28.1
	Minnesota					
	2009	2010	2011	2012	2013	2014
Covered by Medicaid	9.6	10.5	9.6	10.2	8.5	8.3
Covered by Medicare	3.1	2.3	3.8	3.4	3.0	2.8
Covered by Private Insurance	78.6	76.8	75.6	76.5	78.9	80.1
Covered by Employer Health Plan	50.1	45.8	46.9	48.0	48.4	48.4
Covered as a Dependent	23.9	22.7	23.3	24.0	23.0	20.5
Uninsured	10.8	12.5	13.8	12.5	11.4	9.7
Under 138% FPL	18.0	18.1	20.2	16.7	16.6	20.6
	New Jersey					
	2009	2010	2011	2012	2013	2014
Covered by Medicaid	4.6	6.0	5.8	6.1	6.6	9.7
Covered by Medicare	3.5	3.5	3.8	3.9	4.2	3.0
Covered by Private Insurance	77.4	71.5	75.0	73.7	74.2	76.0
Covered by Employer Health Plan	46.7	42.5	42.7	42.7	42.1	42.7
Covered as a Dependent	29.3	27.4	29.1	28.3	30.2	30.2
Uninsured	16.7	21.6	17.5	19.5	19.2	15.9
Under 138% FPL	15.9	16.7	16.4	15.9	16.7	17.1
	Washington					
	2009	2010	2011	2012	2013	2014
Covered by Medicaid	5.7	6.9	5.4	5.5	6.3	7.1
Covered by Medicare	2.7	2.3	2.2	3.2	3.8	3.2
Covered by Private Insurance	75.1	75.1	74.4	73.6	70.1	73.8
Covered by Employer Health Plan	47.9	48.1	45.8	46.1	45.1	44.9
Covered as a Dependent	23.6	22.4	24.3	23.0	21.8	22.5
Uninsured	15.3	15.8	15.9	14.6	16.7	14.2
Under 138% FPL	15.5	18.8	20.7	19.4	20.1	21.5

Notes:
Percentages calculated among childless adults in CPS March Supplements, 18-64.

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