Three Essays in the Economics of Unemployment and Aging

by

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For my parents

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CHAPTER I

Social Security Offsets and the Labor Force Attachment of the Late-Career Unemployed

1.1 Introduction

A long literature on unemployment insurance (UI) and search effort suggests that the availability of unemployment benefits lowers search effort and raises reservation wages for UI claimants. While the insurance may be welfare-improving if it corrects for market failures preventing workers from smoothing consumption across shocks, the moral hazard effects are assumed to lower the reemployment hazard among claimants. In this paper, I examine previously-unexploited variation in state policies affecting UI benefits available to Social Security recipients. The large changes in benefits have meaningful impacts on the share of Social Security recipients who claim UI. Survey measures of search effort are also increased among those workers who are induced to claim. I interpret this finding as in line with prior research showing that higher UI benefits raise the value of unemployment relative to nonparticipation. Employment outcomes appear to be unaffected on average, with no evidence the marginal participants find work or that inframarginal participants return to work more slowly.

The analysis of these policies is important in its own right. They remain unexamined since Hamermesh (1980) reviewed their potential impacts when they first became widespread.

Since that time, workers over 62 have become an increasingly important part of the labor force. These workers are uniquely eligible for both unemployment insurance and Socal Security retirement benefits. The former program is designed to insure income in the case of job loss, while the latter provides income assurance in later life. Until recently, these "offset" policies in most states restricted the benefits that could be drawn from the two programs at the same time, limiting the role of social insurance in smoothing employment shocks in late career and retirement. These offset policies have operated by reducing the unemployment compensation available to Social Security recipients by some fraction of their retirement benefits. The elimination of these rules has increased the incentive for workers to collect UI.

There has been a dramatic increase in the number of workers potentially affected by this policy since 2000. Estimates from the Current Population Survey indicate that the size of the male labor force over 62 increased by almost 75 percent in the ten years between the end of 2002 and the end of 2012. This is a product of both an aging population and increased labor force participation rates for older workers. Coupled with the economic downtown beginning in 2008, the number of unemployed men in this age group increased more than threefold between mid-2006 and mid-2012. Contemporaneous extensions to unemployment benefit durations further increased the importance of understanding the effects of UI for this group.

This increase in the number of older labor force participants coincides with the steady elimination of most state offset rules. As of the late 1980s, more than 40 state UI systems offset unemployment benefits by some fraction of Social Security retirement benefits, a number that fell to 20 just after the turn of the century. Only Illinois continued to offset UI for all Social Security recipients as of the beginning of 2015. As I will elaborate below, the policy changes increase the unemployment benefits available to workers, changing the value of unemployment and the returns to job search. The magnitude of the policies' effects are also large relative to those studied in the existing unemployment literature. While much of the existing work on UI in the United States exploits cross-state variation in benefit levels or within-state changes to benefit formulas, offset eliminations induce large swings in available unemployment benefits, often determining whether workers can receive any benefits at all.

I examine the effects of offset policies using administrative records of UI claims in the Department of Labor's (DOL) Benefit Accuracy Measurement (BAM) program and twelve panels of the Survey of Income and Program Participation (SIPP), a longitudinal survey of American households. I estimate the policy effects using fixed effect models in state and time, controlling for time-invariant state-specific characteristics and national trends. In my analysis of the SIPP microdata, I additionally control for a number of demographic variables that are predictive of UI claiming, search intensity, and employment.

I additionally use the policy changes as instruments for UI benefit levels and generate estimates of the effects of marginal UI dollars for this population. These estimates succinctly summarize the impacts of the policy changes on the whole, showing that the marginal UI benefits increase takeup and reported job search. They show that the benefits ultimately have no measurable effect on employment, either positively or negatively.

The paper proceeds as follows. Section 1.2 discusses the relevant institutional rules and briefly outlines the related literature. Section 1.3 lays out the theoretical motivation for the effects of unemployment benefits in an environment with labor force participation. Section 1.4 outlines the empirical strategy, Section 1.5 describes the data that will be used for the estimation, Section 1.6 presents the results, Section 1.7 discusses them in the context of other estimates from the literature, and Section 1.8 concludes.

1.2 Background

This section first outlines the basic institutional features of the two social insurance systems most relevant to this paper—Social Security retirement insurance and UI. It then describes the policy changes of interest and briefly discusses the closely-related existing research.

1.2.1 Institutional Setting

Social Security retirement insurance provides a monthly benefit to older workers who meet a threshold for number of quarters paying into the system over their careers. The system is funded through payroll taxes remitted by employers and employees. Details of the Old-Age and Survivors' Insurance (OASI) system appear in a number of papers in the literature (see, e.g., Diamond and Gruber 1999), but I summarize the relevant features here. Workers can first claim their retirement benefit at age 62, but benefit levels increase with each year of delayed claiming up to age 70. Benefits are determined by earnings histories and are paid according to a progressive schedule. Recipients can continue to work while receiving Social Security benefits, but are subject to the "earnings test" if they are below the full retirement age (FRA).[1] The earnings test reduces benefits for each dollar of additional earnings above a certain level. Benefits are increased after workers reach the FRA to account for these reductions. Prior to 2000, all workers up to age 69 were subject to the earnings test, although those who were over 65 faced less-stringent reductions. The earnings test has important implications for who chooses to claim retirement benefits while intending to continue work, making claiming most attractive to those who expect low earnings. Unemployment benefits, however, which are described below, do not apply towards the Social Security earnings test.

Unemployment insurance, while governed by a set of federal guidelines under the Federal Unemployment Tax Act (FUTA), differs across states. In general, it provides compensation to full-time, permanent workers who lose their employment through no fault of their own. They must also demonstrate that they are searching for employment and that they are ready and able to start work. Initial eligibility is determined by meeting thresholds for employed quarters and earnings in a set period before the job separation. As with Social Security, the benefit level is determined by earnings over the covered period. In general, benefits are

[1] The full retirement age (FRA) was 65 for those born through 1937. It increased by two months for each subsequent year of birth until reaching 66 for those born in 1943. The full retirement age is 66 for workers born 1943 to 1954, inclusive, and will again increase in two month increments for each subsequent year until 1960.

paid for 26 weeks, but the benefit duration is increased through both automatic and ad hoc extensions during periods of high unemployment.

Since at least the early 1970s, some states have reduced unemployment benefits for pension recipients. The provisions were intended to protect employers, who paid into both systems, from effectively funding multiple benefits for a worker at the same time (National Employment Law Project, 2003). Additionally, because active labor market attachment is a requirement for UI recipiency, some states saw pension receipt as disqualifying income because it indicated retirement and withdrawal from the labor force. Offset rules were federalized by Congress in the Unemployment Compensation Amendments of 1976, which required unemployment benefits to be reduced by 100 percent of income from some pensions, including Social Security. This provision took effect in April of 1980 (Hamermesh, 1980). Congress quickly reversed the requirement in the Multiemployer Pension Plan Amendments Act of 1980 and offsets were no longer mandated by September of that year. The provisions of this bill allowed states, at their discretion, to reduce or eliminate the offset because workers, too, had contributed to their pensions. This aspect of the Federal Unemployment Tax Act remains in effect today.

From 1980 to 1983, the DOL issued five Unemployment Insurance Program Letters to state employment security agencies interpreting the recent changes in Federal law. These were ultimately clarified in Letter No. 22-87, issued in April of 1988, which unequivocally stated that states had "very broad latitude" in reducing offsets given any amount of employee contributions to pensions. This program letter was updated in 2003, further establishing that states had discretion in setting offsets for pension plans to which workers had contributed.

State legislative histories from the early 1980s reflect some of the confusion over the changes in the Federal laws. Many states passed legislation adopting the language of the 1976 or 1980 amendments to FUTA. Some states included both sets of language and left it up to the directors of their state employment agencies to implement the provisions that would keep their system in compliance with the DOL interpretation of FUTA. By the middle of the decade, however, most states had codified specific rules regarding offsets from Social Security and pensions in general. Table 1.1 summarizes the major changes to offset provisions since 1986. My research indicates that 46 UI systems had some Social Security offset in place at the beginning of that year. Of these, only two states, Illinois and Louisiana, still had offsets at the end of 2012. Sixteen UI agencies initially offset benefits dollar-for-dollar by Social Security benefits, with the rest offsetting somewhere between 45 and 63 cents on each dollar.[2] While most states completely eliminated their offsets in a single act, others reduced the amount of the offset before eliminating it. South Dakota and Virginia both initially passed laws that tied their offset provisions to the funding levels in their UI trust funds and subsequently eliminated offsets completely. Figure 1.1 summarizes the national trend in offset policies by showing the number of UI systems with any offset policy for each month since 1986. The general trend is fairly smooth, although there are a few periods of inaction in the 1990s. The mid-2000s saw 12 states eliminate their offsets, perhaps reflecting the additional clarification of the laws provided by the DOL, increased lobbying by AARP, and flush UI trust funds.

1.2.2 Existing Literature

There are extensive theoretical and empirical literatures studying the design and effects of unemployment insurance. Several decades of work comments on the optimal level and path of unemployment benefits (e.g. Baily 1978; Hopenhayn and Nicolini 1997; Chetty 2006). The empirical literature shows modest effects of increases in benefit levels and durations on the length of nonemployment spells (see, recently, Card et al. 2007; Chetty 2008; Rothstein 2011; Schmieder et al. 2012). The search models employed in many of these papers suggest that the reemployment hazard is lowered because of decreased search intensity and increased reservation wages associated with more generous UI benefits. Measured unemployment will also increase in UI generosity because unemployment becomes more attractive relative to

[2] The vast majority of these states offset benefits at 50 cents on the dollar. Accordingly, hereinafter I refer to all offsets greater than zero and less than 100 as 50 percent offsets.

exiting the labor force. Rothstein (2011), in particular, focuses on the increased labor force participation associated with longer benefit durations. He finds that delayed labor force exit accounted for at least half of the increase in measured unemployment caused by benefit extensions during the Great Recession. The effects of UI on labor force participation may be especially important for older workers, as the participation decision is particularly salient for them. A somewhat shorter literature examines the decision to claim unemployment insurance, but does not specifically examine the effects on labor force participation. Both Anderson and Meyer (1997) and Kroft (2008) find that increased unemployment benefits are associated with higher takeup rates.

An important, related, and growing literature examines the interaction of Social Security Disability Insurance (DI) and UI, primarily studying the relationship between job loss and application to both systems. While DI is certainly important for labor supply in late career, I forgo a discussion of it here because of both its unique eligibility requirements and my focus on retirement-eligible workers over 62. Instead, I briefly consider the research on job loss for older workers. Chan and Stevens (2001) show that the effects of job displacement are large and persistent for those in late career, even when compared to the experience of displaced younger workers. In a later paper, they specifically examine the labor force exit rates among older displaced workers (Chan and Stevens, 2004). They find high exit rates even when considering the decreased wages and other disincentives to work associated with job loss. They suggest that high search costs and other obstacles may depress reemployment rates for older workers. Consistent with that suggestion, Lahey (2008a) finds markedly lower callback rates for older women in an audit study of female job search. This literature indicates a unique experience of job loss and unemployment for older workers. While much of the existing literature indicates that older workers interact with the labor market in a way that is different from the average worker, there is little good evidence on the impacts of UI benefits on this group.

1.3 Theoretical Motivation

1.3.1 Environment

I present a single-period model of labor force participation and search for nonemployed workers. As in Card et al. (2007) and Chetty (2008), workers are differentiated by their asset holdings, given by A. While other work has shown assets to be intrinsically important for smoothing, they are largely included here to introduce heterogeneity in labor force participation within the model. Agents choose whether to participate in the labor force and how intensively to search for reemployment. I abstract from wage offers, reservation wages, and, initially, variation in unemployment benefits. Net wages received by workers are given by w, while unemployment benefits are given by b. Utility is a function of consumption and leisure and is given by $u(c, l)$. I assume that utility is increasing and concave in consumption, the first argument. For simplicity I consider only two potential values for leisure, with $l = 0$ when agents are employed and $l = 1$ otherwise. Further assume that

$$u(c, 1) > u(c, 0) \;\; \forall \;\; c, \tag{1.1}$$

$$\frac{\partial u(c, 1)}{\partial c} > \frac{\partial u(c', 0)}{\partial c} \;\; \forall c' > c. \tag{1.2}$$

That is, there is some disutility to employment and marginal utility in nonemployment is higher than marginal utility in employment for any higher level of consumption. This formulation is consistent with a number of functional forms, most simply a fixed disutility of work $u(c, 0) = u(c, 1) - d$, but only the general restriction that appears in inequality (1.2) is necessary.

Nonemployed workers can choose to withdraw from the labor force, look for a new job without claiming UI, or claim UI. Utility in these three states is given, respectively, by

$$U_W(A) = u(A, 1) \tag{1.3}$$

$$U_L(A) = \max_{s>0} \; su(A + w, 0) + (1 - s)u(A, 1) - \phi(s) \tag{1.4}$$

$$U_C(A, b, \underline{s}, \psi) = \max_{s \geq \underline{s}} \; su(A + w, 0) + (1 - s)u(A + b, 1) - \phi(s) - \psi. \tag{1.5}$$

Workers who withdraw from the labor force consume their endowment while not working. Workers who search without claiming choose a search effort s, which is normalized to the probability of becoming reemployed.[3] Workers face some disutility of search, given by $\phi(s)$, which is assumed to be increasing and convex in s. It is assumed that a search effort of zero is equivalent to labor force withdrawal. UI claimants receive a benefit b in unemployment, but must also pay a fixed utility cost of claiming ψ, as in Anderson and Meyer (1997), the basic model of Kroft (2008), and others. The unemployment agency is assumed to be able to enforce some minimum search effort, $\underline{s}$, through its forms of search monitoring. As this minimum level could be zero, this assumption is without loss of generality.[4]

1.3.2 Participation and Search in the Absence of UI

I first consider workers' decisions in the absence of the unemployment insurance system. In this case, workers will choose to withdraw from the labor force if

$$u(A, 1) \geq \max_{s>0} \; su(A + w, 0) + (1 - s)u(A, 1) - \phi(s). \tag{1.6}$$

[3] Alternatively, in the style of Anderson and Meyer (1997), s can be considered the share of the period spent in reemployment. For the purposes of this exposition, it only matters that it is the expected share of time spent in reemployment.

[4] In the event that $\underline{s} = 0$, search at this level while claiming is considered to be participating in the labor force.

For the purposes of characterizing the decision to search or exit the labor force, I make three additional assumptions about the form of search costs, which are given by

$$\phi(0) = 0, \tag{1.7}$$

$$\lim_{s \uparrow 1} \phi'(s) = \infty, \tag{1.8}$$

and

$$\lim_{s \downarrow 0} \phi'(s) = 0. \tag{1.9}$$

While the first two assumptions are generally consistent with the literature, the final assumption is not. In particular, it differs from assumptions made in Hopenhayn and Nicolini (1997) and Hairault et al. (2012). In essence, I assume that workers with arbitrarily small returns to working will search at some level. If one assumes instead that the limit in equation (1.9) approaches a positive value, a wedge exists in which some workers who would prefer to be employed will not search. I make this assumption so as to ensure an interior solution for search intensity as long as $u(A+w,0) > u(A,1)$.

Thus, for workers participating and not claiming UI, we have the familiar first order condition for optimal search intensity,

$$\phi'(s^*) = u(A+w,0) - u(A,1). \tag{1.10}$$

By the assumption in inequality (1.2), the right side of equation (1.10) is falling in A for a fixed wage. As search costs ϕ are increasing and convex, optimal search effort $s^*(A)$ is falling in A as well.

Suppose there exists an asset level $\bar{A}$ such that $u(\bar{A}+w,0) = u(\bar{A},1)$, then workers with assets below $\bar{A}$ will search, while those with assets above $\bar{A}$ will withdraw. Above $\bar{A}$ there is no search effort that can make the right side of inequality (1.6) larger than the left side. In short, as assets increase, workers search less because employment is relatively less attractive to the point that nonemployment is preferred. A stylized version of this relationship is displayed in Figure 1.2. At the point where employment is less attractive than nonemployment, search intensity reaches zero and workers withdraw from the labor force.

1.3.3 Optimal UI Claiming and Search

Preferred search by UI claimants, $\hat{s}(A, b)$, would be given by

$$\phi'(\hat{s}) = u(A + w, 0) - u(A + b, 1). \tag{1.11}$$

Denote the minimum asset level associated with zero preferred search intensity $\bar{\bar{A}}$. That is, define $\bar{\bar{A}}$ such that

$$u(\bar{\bar{A}} + w, 0) = u(\bar{\bar{A}} + b, 1). \tag{1.12}$$

Note also that $\bar{\bar{A}} < \bar{A} - b$ because of the distortion to search incentives. The optimal stopping point for search while claiming lies to the left of $\bar{A} - b$ because the benefit is only paid in the unemployed state, depressing search intensity.[5]

While claimants would prefer to search at the intensity given by $\hat{s}$, they are compelled to search above the threshold $\underline{s}$, so actual search effort is given by

$$s^{**}(A, b, \underline{s}) = \max\{\hat{s}(A, b), \underline{s}\}. \tag{1.13}$$

The general relationship between search intensity for claimants and nonclaimants is depicted in Figure 1.3. For a given asset level, claimants search less than nonclaimants because they have higher consumption in the unemployed state, unless they are compelled to more intensive search by some $\underline{s} > 0$. However, this does not indicate when claiming is optimal

[5]By contradiction: Suppose that $\bar{A} - b \leq \bar{\bar{A}}$. Then $u(\bar{A}, 1) \leq u(\bar{\bar{A}} + b, 1)$. By the definitions of $\bar{A}$ and $\bar{\bar{A}}$, we know $u(\bar{A}, 1) = u(\bar{A} + w, 0)$ and $u(\bar{\bar{A}} + b, 1) = u(\bar{\bar{A}} + w, 0)$. Thus, we have $u(\bar{A} + w, 0) \leq u(\bar{\bar{A}} + w, 0)$. Plugging in $\bar{\bar{A}} + b$ for $\bar{A}$ in the first term, we have $u(\bar{\bar{A}} + b + w, 0) \leq u(\bar{\bar{A}} + w, 0)$, which is a contradiction because u is strictly increasing in its first argument.

relative to nonclaimant search or labor force withdrawal.

1.3.4 Choice of Labor Force Status

Consider first if $\underline{s} = 0$ and $\psi = 0$. As there are essentially no costs of claiming, the utility from claiming is given by:

$$U_C(A, b, 0, 0) = U_W(A + b) \qquad if A \geq \bar{\bar{A}} \tag{1.14}$$

$$U_W(A + b) < U_C(A, b, 0, 0) < U_L(A + b) \qquad if A < \bar{\bar{A}} \tag{1.15}$$

Above asset level $\bar{\bar{A}}$, U_C is a direct leftward shift of U_W by the amount of b. Workers in either case expend no search effort and have no probability of becoming employed, but claimants can achieve the same utility with $A - b$ assets as nonclaimants A assets. Below $\bar{\bar{A}}$, $U_C(A, b, 0, 0)$ is bounded above by $U_L(A + b)$ and below by $U_W(A + b)$. To see the former, note that workers who are searching would be willing to trade their unemployment benefit for a smaller increase in A because A is consumable in either state and does not distort search incentives. Similarly, workers would require more than the guarantee of b to be induced to give up their ability to search and enter U_W. As before, recall that $\bar{\bar{A}} < \bar{A} - b$. A representation of the utility in each state is given in Figure 1.4. In this case, claiming is always optimal. Workers expend some search effort up to $\bar{\bar{A}}$, after which they simply collect the UI benefit without searching. Clearly, this is not realistic, as indicated by the well-documented non-universality of unemployment insurance claiming.

More realistic implications for claiming can be recovered by manipulating ψ and $\underline{s}$. Increases in ψ shift the U_C curve directly downward through the additive effect on the utility of claiming. An increase in $\underline{s}$ creates a kink point to the left of $\bar{\bar{A}}$ at the point where $\hat{s}(A, b) = \underline{s}$. To the right of this point, U_C and its slope are lowered because claimants are compelled to search at a higher intensity than is optimal. Figure 1.5 suggests the effects of positive ϕ and $\underline{s}$ on search effort and claiming behavior.

For the particular level of ψ depicted in Panel A, claiming remains optimal over much of the range of A shown. The downward shift in the U_C curve is enough to make claiming not worthwhile at high levels of assets. As this point is above $\bar{A}$, looking without claiming is never optimal. The increase in claiming costs precludes some high-asset workers from claiming, but otherwise has no impact on search behavior. Further downward shifts in U_C would lower the threshold of withdrawal while also potentially creating ranges in which U_L exceeds U_C. This latter effect would increase search effort by leading to more nonclaimants who look for jobs.

Panel B indicates the optimal behavior when the unemployment agency enforces a minimum search effort $\underline{s}$. As noted before, search effort for claimants reaches this level at some point before $\bar{\bar{A}}$. Workers then search at level $\underline{s}$ until either nonclaiming or withdrawal becomes optimal. In this depiction, there is a brief range of A over which agents return to the nonclaiming search curve before $\bar{A}$, above which they withdraw from the labor force.

1.3.5 Effects of Benefit Increases

The impacts of changes to unemployment benefits will vary with the nature of costs faced by the workers. As a general rule, increases in b shift the U_C curve and the $\bar{\bar{A}}$ point to the left, while also changing the slope on the part of the curve to the left of $\bar{\bar{A}}$ through changed search incentives. In the case of no claiming costs or restrictions, as in Figure 1.4, an increase in b has no impact on the claiming, participation, or search behavior of the workers. The unemployment benefit free lunch simply becomes bigger. In the example with $\psi > 0$ as in Panel A of Figure 1.5, the increase in benefits will induce more claimants and decrease search effort among those already claiming. If the newly-induced workers have assets higher than $\bar{A}$, their search effort will remain zero. If they were instead in a range where U_L was previously optimal, their search effort will be reduced, potentially to zero. Finally, if $\underline{s} > 0$, the higher benefits will again induce more claiming while changing effort among prior claimants. Workers who were previously claiming and searching at levels above $\underline{s}$ will have an incentive to reduce their search effort. The maximum asset level for claiming will also increase. If the marginal claimants were previously looking for work without claiming, the effect on their search effort is ambiguous. As depicted in Panel B of Figure 1.5, a further increase in b would induce greater search effort among the marginal claimants. However, for a lower claiming threshold, it could easily be the case that the marginal searchers would have previously been looking with greater intensity. Any previous withdrawers who chose to claim would have an unambiguous increase in search effort.

In short, a model with no enforcement of search effort but fixed costs of claiming would predict that increasing benefits would decrease search effort and decrease claiming. Claiming would be increased mostly at middle and high wealth levels, while search effort would be depressed mostly at low and middle wealth levels. A model with enforcement of minimum search effort will again show benefits decreasing search intensity and reemployment for low wealth workers, but ambiguous or positive search and employment effects for those of middle and high wealth.

While I forgo any formal welfare analysis, it is important to note that many of the changes to labor force status induced by benefits are likely welfare-reducing. As is addressed in Gruber (1997) and Chetty (2008), UI benefits have the potential to increase welfare by smoothing consumption losses, particularly for workers with limited liquidity. This analysis indicates that unemployment benefits could distort not only search effort, but labor force participation as well. Indeed, workers with assets above $\bar{A}$ may be induced to expend search effort even though they would prefer to not be employed even in the absence of a UI benefit. This result, however, is sensitive to the assumption that search effort and participation in the absence of insurance are chosen optimally. Analysis of an alternative formulation is provided by DellaVigna and Paserman (2005), who describe a model of hyperbolic discounting in which impatience leads to otherwise-suboptimal choice of job search effort. They find that measures of impatience are associated with lower unemployment exit rates, suggesting that this sort of impatience measurably reduces search intensity.[6] Socially suboptimal search effort can also appear in a richer model where benefits are funded through taxation of wages. In this case, additional effort leads to additional employment and a lower necessary tax rate, which is not internalized by the job-seekers.

1.4 Methodology

The primary analysis in this paper involves the estimation of state and time fixed effects models at the level of individual separations. In particular, I consider models of the form

$$y_{ist} = \alpha_0 + \alpha_1 \mathbf{D}_{st} + \alpha_2 \mathbf{x}_{ist} + \gamma_s + \delta_t + \epsilon_{ist} \qquad \forall i \in \{i | S_{ist} = 1\} \tag{1.16}$$

and

$$y_{ist} = \beta_0 + \beta_1 \mathbf{D}_{st} + \beta_2 S_{ist} + \beta_3 \mathbf{D}_{st} S_{ist} + \beta_4 \mathbf{x}_{ist} + \gamma_s + \delta_t + \epsilon_{ist}, \tag{1.17}$$

where y_{ist} is an outcome for separation i in state s and month t, $\mathbf{D}_{st}$ is a vector of indicators for offset policies, and S_{ist} is a measure of (or proxy for) Social Security receipt at the time of separation. Because Social Security receipt is endogenous to many labor force outcomes, I consider specifications that use age as a proxy for exposure to the policy. Controls for separation characteristics are included in the vector $\mathbf{x}_{ist}$. State and month fixed effects are given by γ_s and δ_t, respectively, and ϵ_{ist} is an idiosyncratic error term.

Estimates of the coefficient vectors of interest, α_1 and β_3, indicate the effects of offset policies on y under different assumptions. The estimates of α_1 in equation (1.16) show the average difference in outcomes between Social Security recipients in state-months with offset policies and Social Security recipients in state-months without them. This is generated from within-state variation in policies over time. Consistent estimation of this effect relies on the assumption that there is no correlation between offset policies and outcomes for Social Secu-

[6] The lower exit rates further imply that the reduced search effort is large enough to swamp decreases in reservation wages, which arguably would also be associated with higher levels of impatience.

rity recipients, conditional on the fixed effects and covariates. Because the provision of offset policies may be correlated with time-varying local labor market conditions, I additionally estimate models as in equation (1.17). Here, β_3 shows how the estimated policy effects differ between Social Security recipients and nonrecipients. The identifying assumption is that the counterfactual difference in outcomes between recipients and nonrecipients does not vary across state-months with and without the offset policies. Threats to identification for this specification include uncontrolled variables that are specifically correlated with the offset policies and the outcomes for the recipient separators. In both cases, the general concern is that there are unobserved factors driving the outcomes for workers of Social Security age in the periods with active policies.

Throughout, I treat variation in offset policies as exogenous variation in UI benefits available to older workers. I ignore the complexities of the interactions between the Social Security and UI programs. Under an offset regime, Social Security claiming eliminates the option of some future unemployment benefits. On the margin of choosing whether to claim or delay, workers could be induced to delay so as to continue to have their full unemployment benefit available. The importance of this mechanism is likely to be small if workers are unaware of offset policies before job loss, they view the probability of job loss as small, or few workers are on the particular margin of Social Security claim delays. In recent years, mainstream media reporting on offsets, which may be prone to hyperbole, has suggested that workers are surprised to learn about offset policies when they go to claim their unemployment benefits.[7]

[7] Granted, the existence of mainstream media reporting on offsets suggests the opposite effect.

1.5 Data

1.5.1 Characteristics of the Insured Unemployed

I first examine the impact of offsets on aggregate UI claims using administratively-supplied "Characteristics of the Insured Unemployed" data published by the DOL. Since 2001, the DOL has provided monthly averages of weekly total UI claims by state along with some demographic characteristics of claimants. In particular, the data show the share of weeks claimed by workers in certain age groups, including 60 to 64 and 65 & older. The data are provided to the DOL by state unemployment agencies, which construct them from either the universe or samples of state unemployment claimants. They reflect the average number of weekly claims over the month and the composition of continuing claimants for the week containing the 19th.

1.5.2 Benefit Accuracy Measurement

The BAM program,[8] which has run since 1988, is designed to study the incidence and source of mispayments in state unemployment systems. Following DOL guidelines, state auditors investigate samples of UI claimants, checking their satisfaction of eligibility requirements and the calculation of their benefit amounts. While the data provide important information about the accuracy of UI payments, they also serve as a source of randomly-sampled administrative records of UI recipients.[9] For each claim appearing in the sample, the BAM data include information on demographics, initial claim date, base period wages, job characteristics, appropriate deductions or offsets, and UI benefit amounts, both before and after the audit. I use the before-audit measures to study the characteristics of random UI claims under various offset policies.

[8]Formerly Benefit Quality Control (BQC).

[9]The sample that is drawn in each state in each week is effectively a random sample that is stratified by claimant benefit level. The sampling design is described in full in the *Benefit Accuracy Measurement State Operations Handbook* (ET Handbook No. 395).

1.5.3 Survey of Income and Program Participation

I perform the main micro analysis using the twelve panels of the SIPP between 1986 and 2008. The SIPP is a nationally-representative longitudinal survey of households, with panels that have generally lasted for three or four years.[10] Respondents are interviewed once every four months and answer questions about income, employment, and participation in various benefit programs during each of the intervening months. Labor force status is reported at the weekly level, but earnings, retirement benefits, and unemployment compensation are reported at monthly frequency. For each week between SIPP interviews, respondents categorize themselves as employed, employed and absent from work, on layoff, unemployed and looking for a job, or other. In the analysis of labor force participation, I consider participation to be any of the first four categories and job search to be a self-report of "unemployed and looking for a job."

Much of the forthcoming micro analysis is focused on UI-eligible job separators who are near traditional retirement ages, as it is these workers who are most directly affected by offset policies. The primary analysis restricts to workers who are aged 51 or older when they are observed separating from a job in the data. Those who are above 62 are potentially eligible for Social Security and are the group primarily treated by the policies, while those under 62 serve as near-aged controls in some specifications.[11] I drop any respondents who ever report receiving DI, both because offset policies vary in their treatment of the program and because of the complex interplay demonstrated in the literature between UI and DI receipt.

At each SIPP wave, respondents are asked if they are still working for their two main employers from the previous interview. If they are not, they are asked to give a reason for the separation and a date when they last worked for the employer. I define job separators as those

[10] The panels were fielded beginning annually from 1986 to 1993, as well as in 1996, 2001, 2004, and 2008.

[11] I do not restrict based on vestedness in Social Security, even though workers who are not vested will not be affected by offsets. This is partially because vesting is not entirely observable in the public versions of these datasets, but also because workers who are employed in their late 50s and beyond are very likely to have the necessary 40 quarters of coverage over their lifetimes.

workers who report such a date and a reason.[12] For each separation, I check whether base period earnings are high enough to satisfy state earnings thresholds for UI eligibility using their self-reports of monthly earnings and the state UI rules collected and reported every six months by the DOL. Although many job separators in the SIPP have not been in the survey for their full base period before a separation, I follow Chetty (2008) in using whatever earnings are reported and assuming they are representative of the unobserved period. I drop separations that do not meet the UI monetary eligibility thresholds. I also calculate a UI weekly benefit amount for each individual using their reported earnings and their state's UI rules for the relevant time period. I similarly assign each respondent the offset regime prevailing in their state of residence at the time of separation.[13] Maine, North Dakota, South Dakota, Vermont, and Wyoming cannot be individually distinguished in panels before 2004, so I omit them from the analysis. I further restrict to respondents whose states of residence are constant over their panel in an effort to avoid confusion over the applicable UI policies.

For the purposes of the forthcoming analysis, I define Social Security receipt as any individual Social Security income in the first six months after separation. I use this definition because this is the time period in which workers are likely to be claiming UI in the wake of a separation. While there is a distinct uptick in Social Security receipt in the months post-separation for job-leavers on average, this uptick does not appear in the post-separation period for the likely-UI-eligible sample described in the following section.

[12] This definition has a number of advantages. First, it gives reasonable assurance that a job separation actually happened, as it is unlikely that a respondent would provide both pieces of information erroneously. It also reduces the impact of well-known "seam bias" in the SIPP, in which changes in responses tend to spike at the end (seam) of a four-month wave. While separations are likely to happen at the end of a month, there does not seem to be a particular effect of seam months, perhaps because respondents are asked for specific dates.

[13] While some states' legislation explicitly indicate whether ongoing UI claims are affected by policy changes many states do not. In practice, only a small share of the SIPP separators in this sample experienced policy changes during their benefit years. Excluding them from the analysis has no appreciable impact on the results.

1.5.3.1 UI Eligibility

Although some of my analysis is performed on this entire sample of job separators, it would be ideal to further restrict to workers whose separations make them eligible to collect unemployment benefits. While UI claiming is observed in the SIPP, eligibility among nonclaimants is not observed. Therefore, I use other information provided about the job separation to infer information about eligibility. The simplest strategy is to restrict to respondents who provide reasons for a job separation that are consistent with UI eligibility (e.g. "laid off" or "business closed"). A version of this is implicitly employed in prior papers that use the Displaced Worker Survey, such as McCall (1995) and Kroft (2008). However, there is some subjectivity inherent in these survey responses, and it is possible that they could be influenced by the policy in question. UI benefit levels and recipiency may impact the likelihood of a respondent describing a separation as a layoff or plant closing versus a retirement.

Lacking other measures of UI eligibility, I explicitly test for endogeneity of these responses after controlling for observed features of separations. I identify a likely-UI-eligible sample as those separations with given reasons that are associated with UI receipt at younger ages.[14] I test for the endogeneity of this sample definition in the SIPP using linear probability models by regressing an indicator for inclusion in the sample on state offset policy and all controls that will be used in the final analysis. The estimates from these regressions are reported in Table 1.2 for specifications of the form of equations (1.16) and (1.17). The first column restricts to separations for workers who were 62 or older at the time and the second column restricts to separations with Social Security receipt as defined above. The third and fourth columns include separations at ages down to 51, but report the coefficient on the interaction term between offset policy and being older than 62 or receiving Social Security. The top panel of results examines only the first observed separation for each individual and the bottom

[14] These reasons include "layoff," "discharged," and "temporary job" in the 1986 to 1993 SIPP panels and "layoff," "discharged," "employer bankrupt," "employer sold business," "temporary job," and "slack work conditions" in the last four SIPP panels.

panel uses all separations. There is modest but limited evidence that workers are less likely to label a separation as one of the UI-eligible categories when in an offset state. While the coefficients are generally not statistically different from zero, they are largely negative. Because the outcome variable is 1 for about 30 percent of separations at ages 62 or over, estmates on the order of -0.04 suggest that more than 10 percent of otherwise-eligible job separators are changing their reported separation reason due to the policies. However, most of the estimates indicate smaller effects that are not distinguishable from zero.

1.5.3.2 Final Sample

All of the survey data analysis that follows restricts to the first-observed likely-UI-eligible job separation for each individual. The characteristics for this sample are reported by age and prevailing offset policy in Table 1.3. Offsets are more likely to be associated with separations in earlier years because the policies have been eliminated over time. The key difference across the age groups is the difference in Social Security receipt between younger and older job separators. Although Social Security receipt as measured in this table is own retirement benefits only (for which eligibility begins at age 62), I count a separation as being associated with receipt if claiming began within 6 months, allowing some under-62 job separators to have Social Security. Receipt of other pensions is also higher in the older age group, though the differences are much less stark.

The two listed "pension policies" in Table 1.3 refer to state rules for offsetting UI benefits for pensions other than Social Security. In both cases, having the policy indicates lower offsets. States with pension policy A only offset due to private pensions if pension eligibility or benefits were affected by the worker's base period employment. Pension policy B indicates states that "consider" employee contributions to pensions by not offsetting some part of private pensions. As demonstrated by the low values in the third and sixth columns, states with 100 percent Social Security offsets are particularly unlikely to be lenient in their policies of offsetting other pensions. For this reason, I explicitly control for pension receipt and these policies in the forthcoming analysis.

1.6 Results

1.6.1 Direct Effects on UI benefits

Data from the BAM program suggest meaningful impacts of offset policies on both the number of claimants and the benefits received by those who claim. For various offset policies, Panel A of Figure 1.6 displays the average weekly benefits received by claimants of each age. Received UI benefits fall with age even in the absence of Social Security offsets, likely reflecting lower base period wages, differential selection into claiming, and other pension offsets. However, the pattern of declining benefits is starker in states with Social Security offsets, particularly above age 65. The reason for this difference clearly comes from Social Security (and potentially other pension) deductions. Panel B shows the share of claimants with any offsets or deductions to their benefits. The rate of deductions rises in the late 50s and dramatically increases for workers over age 62 in offset states. Figure 1.7 conveys similar information more starkly through replacement rates. Replacement rates based on UI monetary formulas alone (shown in Panel A) are effectively identical across the states. This suggests that the basic UI benefit rules are not meaningfully different across states with the different policies. Under all policies, basic replacement rates drift upward at older ages, again due to UI progressivity and lower earning at older ages. After accounting for all adjustments and deductions, replacement rates as measured by *actual payments* to UI claimants deviate sharply in the 60s and 70s in offset states (Panel B). While average replacement rates in states without offsets are above 0.40 at these ages, they fall below 0.30 in offset states. Because the BAM program only samples claimants, these graphs likely dramatically understate the difference in potential benefits, because workers whose benefits are most reduced by the offsets are the least likely to be observed.

1.6.2 Effects on UI Recipiency

I next test for evidence of offsets' effects on late-career workers in the DOL "Characteristics of the Insured Unemployed" data described in Section 1.5.1. Figure 1.8 shows the share of weeks claimed by workers 65 and older for six UI systems that changed their 100 percent offset policies between 2001 and 2011.[15] Presenting the outcomes as a share of weekly claims (as opposed to absolute number of claims for older workers) effectively controls for state movements in aggregate claims. It does not, however, control for seasonality[16] or demographic trends, which would tend to increase the share of 65+ claimants over this period due to population aging. The vertical lines indicate the month in which the 100 percent offset was eliminated. In five of the states, no offset prevailed after the policy change, while Utah maintained an offset of 50 percent until it switched to no offset at the end of 2010. All of the initial policy changes appear to be associated with a level jump and, in some cases, also a trend break.

Figure 1.9 plots the series for 11 states that eliminated 50 percent offsets during this decade. On the whole, the graphs show much less stark effects of the policy changes, although small effects are seen in some states. I test for effects of either type of policy change by running fixed effects regressions at the state-month level. In particular, I regress the log number of weekly claims for workers ages 65 and over on indicators for offset policy, month fixed effects, state fixed effects, and the log number of claims by workers ages 55 to 59. This last term is included to control for labor market conditions faced by relatively similar workers who are arguably not directly affected by offset policies. The first panel of Table 1.4 reports the results for this basic specification along with other regressions that control for linear and quadratic state-specific trends. The second panel of Table 1.4 repeats the exercise for claims

[15]Virginia is the only other state to have a 100 percent offset policy during this time range. I exclude it here and elsewhere because of the use of a trigger policy in the sample period.

[16]While the total number of UI weeks claimed by workers 65 and over follows a seasonal pattern similar to that seen in the total population, the share of claims by this group tends to peak in late summer when claims among prime-age workers are lowest. I forgo deseasonalizing the data for these figures so as to more sharply reveal the effects of policy changes.

by workers ages 60 to 64.[17]

The first panel, which reports results for workers over 65, suggests strong effects of offsets on UI recipiency for the 65 and over group. 50 percent offsets are estimated to reduce claims by 13 and 11 log points in the models with fixed effects only and linear trends, respectively. 100 percent offsets reduce claims by a third to a half, a finding that is robust to all the trends included here. The results in the second panel show only limited effects on the log number of claims by workers 60 to 64. The estimates for the 50 percent offset are not statistically different from zero in any of the specifications. The 100 percent offset policies are estimated to reduce claims by 10 percent in the model without any state trends, but this result is not robust to the inclusion of such trends.

On the whole, these results suggest that aggregate claims by workers over 65 are strongly affected by 100 percent offsets and somewhat less affected by 50 percent offsets. The aggregate numbers for workers ages 60 to 64 show little effect of any offset policy. This latter result is arguably a function of the small share of this group that has claimed their own Social Security retirement benefits. Workers under 62 are not yet eligible for their benefits and anyone under 65 would be subject to the earnings test if they claimed their retirement benefits. Thus, it is likely that many of the workers in this group would not have claimed Social Security at the time they became eligible for UI and so would not have their unemployment benefits offset. Most of the workers over age 65, on the other hand, would be eligible to collect their Social Security benefits. Those who are above their full retirement age would additionally not be subject to the earnings test. While this data cannot speak to the exact share of this group that would already be receiving Social Security benefits, the larger effect of offsets is consistent with a higher Social Security recipiency rate.

This analysis also suggests a much stronger effect of 100 percent offsets than 50 percent offsets. While this may simply be a result of differences between states with offsets of 50 and

[17] From this analysis, I omit Virginia and South Dakota, whose policy changes depended explicitly on the performance of their trust funds, and Maine and Pennsylvania, which showed irregularities and high missing rates in this data.

100, it could also indicate important nonlinearities in the effects of UI benefits. In particular, if offsets of 100 tend to reduce UI benefits to zero then much of the effect of offsets is generated by the availability of any UI benefits and UI participation on the extensive margin. This data, however, does not indicate whether the UI effects of offsets tend to operate on the extensive or intensive margin of benefit receipt.

1.6.3 Graphical Trends

Figure 1.10 displays UI recipiency in the SIPP for the likely-UI-eligible sample by age and offset policy. In this figure and those that follow, I simply show general trends, leaving formal statistical tests for the forthcoming regression analysis. In Figures 1.10 to 1.13 the left panels show outcomes for SIPP job separators in the likely-UI-eligible sample who are under 62 and the right panels show outcomes for those over 62.[18] In all of these figures, I restrict to separations before 2008 because of the large labor market effects of the Great Recession at a time when almost all states had eliminated their offset policies.[19]

Figure 1.10 demonstrates that UI recipiency rises at all ages in the sample in the wake of job separations. As one might expect, the claiming rates are lower across the board for workers over 62. While claim rates are lower in 100 percent offset states even at younger ages, they are cut in half for workers over 62. This is consistent with the general findings of the aggregate data in the previous section. States with 50 percent offsets do not exhibit much difference in claim rates as compared to states with no offsets. Again, as with the aggregate data, the UI participation effects of offsets are highly concentrated in the 100 percent policies. Figure 1.11 shows weekly rates of unemployed search in the SIPP. While the differences are somewhat less stark, the evidence suggests that in addition to reducing UI recipiency for workers over 62, offsets (and 100 percent offsets in particular) reduce reported search effort in the weeks following a separation. However, as the complement of searchers includes workers

[18]The analysis of the previous section suggests that it would be ideal to distinguish job separators who are 65 and over, but this group proves too small to provide meaningful estimates on its own, even with all SIPP samples pooled together.

[19]I do not make this restriction in the regressions, where national trends in years are plausibly controlled.

who are both employed and out of the labor force, it is important to verify that this difference is indeed because workers are choosing between search and nonemployment and not because of differences in immediate reemployment rates. Figure 1.12 demonstrates that labor force participation (search versus nonemployment) is the affected margin. Participation rates are lower in 100 percent states for workers over 62 and the magnitude of the difference is similar to the change in search.

However, it is unclear whether these differences reflect actual effects on search behavior or simply differences in survey reports. If UI claimants know they are supposed to be searching for work, they may be more likely to report search to a SIPP surveyor, even if their behavior is unchanged. Differences in actual reemployment rates would suggest that UI participation has actual meaningful effects on job finding. Panel B of Figure 1.13 shows no difference in reemployment rates for workers over 62 across different policies. It is perhaps notable that this is despite the fact that reemployment rates are higher in offset states for younger ages, but the regression analysis will demonstrate that these differences are not generally statistically significant.

1.6.4 Regression Results

Using SIPP data, I estimate versions of equations (1.16) and (1.17) for outcomes including UI benefit levels, UI claiming, reported search effort, labor force participation, and employment at various horizons. Tables 1.5 to 1.7 display the same general structure as Table 1.2, though each section of the tables now presents results for a different outcome. Each pair of estimates (50 percent offset and 100 percent offset) are generated by a different linear probability model. Columns differentiate estimation samples with the estimated relevant treatment effect always reported. Throughout, the regressions include controls for sex, race, age, education, month and year, state, and private pension receipt interacted with state pension policy. Model (1) restricts to workers who are 62 and older at separation and reports the estimated coefficients on indicators for each policy. Model (2) does the same but restricts to Social Security recipients over age 62. Models (3) and (4) include the sample down to age 51 and report the estimated interactions between the policies and indicators for either being over 62 or a Social Security recipient. In both (3) and (4), the main effects of each interaction term are also included but not reported.

1.6.4.1 Effects on Available UI Benefits

Table 1.5 presents the estimated effects of the offset policies on UI benefits available to job separators. The outcomes in this table are generated using UI benefits calculated from survey reports of earnings and from Social Security benefits reported to be received in the first six months after job separation. Because the outcomes are calculated from survey responses, they are likely to be subject to more error than the benefits displayed in Figure 1.6, however, they are calculated for all job separators, not just those who claim UI benefits.

The first part of Table 1.5 shows estimates from linear probability models for whether the individual is still eligible for a UI benefit greater than zero after any relevant offset has been applied. Across all of the models, 50 percent offsets are estimated to reduce the share of separators who can receive benefits by some 25 to 40 percentage points. 100 percent offset effects are estimated to be roughly twice as large. The control group means are universally 1.00 in these regressions because the sample is restricted to job separators who are calculated to be UI eligible. Thus, the estimated percentage point effects would also be interpreted here as percent estimates as well. A meaningful percentage of otherwise-eligible UI claimants are precluded from receiving unemployment benefits because of the offset policies.

The second part of Table 1.5 displays the estimated effects on weekly UI benefit levels, including zeros for those whose benefits have been entirely eliminated. Clearly there is a mass point at zero for this outcome, but the estimates still indicate the average decrease in benefits in practice. Average weekly benefits for the control groups are $236 for the over 62 sample and $215 for the Social Security recipient sample. Among the treated over the age of 62 (models (1) and (3)) benefits are reduced by some $100 on average under 50 percent offsets and $130 under 100 percent offsets. These amount to average reductions in UI benefits of approximately 40 to 55 percent. The estimated decreases are larger in models (2) and (4) because all treated individuals are Social Security recipients and have their UI benefits reduced by the offsets. As these groups also have lower earnings, the estimated effects amount to even larger percentages of UI benefits. In short, average benefits are dramatically reduced by offset policies.

1.6.4.2 Effects on Labor Force Outcomes

Estimated effects of the offsets on labor force outcomes in the first six months after the job loss are presented in Table 1.6. The offsets are generally estimated to have statistically significant effects on UI receipt in the first six months after separation, as expected. The magnitudes for the 100 percent effects vary between 9 and 18 percentage points. These estimates are roughly in line with the differences seen in the graphical analysis of the previous section. Unlike the graphs, however, these results do suggest an effect of 50 percent offsets in some of the models. Because they are on a base of about 30 percent of workers over 62 receiving UI, the magnitudes for the 100 percent estimates imply that claiming is reduced by an amount that is on the order of one half, which is comparable to the estimates from the aggregate data in Table 1.4.[20] Similarly, the estimates of 50 percent offset effects in models (1) and (2) are somewhat higher than the aggregate estimates. The same is not true in models (3) and (4). The differences in all of the UI receipt estimates between models (1) and (2) and models (3) and (4) suggest that claiming rates are relatively lower in offset states, even among those who do not appear to be affected by the offsets themselves. It may be important to include these additional untreated individuals to control for other characteristics of states and times that were subject to offsets.

The estimated effects on work search indicate that the decrease in UI benefits leads to

[20] It should be noted that the implied decrease in claiming is sometimes smaller than the estimated decrease in individuals with available benefits shown in Table 1.5. In principle, the latter should always be smaller, which means there is some error introduced by either the models or the calculated benefits and measures in use here.

job-separators being less likely to be unemployed and looking for work. The estimates are relatively small and are statistically indistinguishable from zero for 50 percent offsets, but are all negative. The 100 percent estimates, on the other hand, indicate meaningful reductions in the share of job separators who are looking for a job at any time in the six months following a separation. However, these results do not indicate whether the affected workers are being moved along the margin of search and nonparticipation or search and employment.

The labor force participation estimates suggest that, for the 100 percent offsets at least, many of the workers who are not searching are instead exiting the labor force. The 50 percent offset estimates for labor force participation at any point in the six months after separation do not show meaningful effects. 100 percent offsets are associated with some 12 to 18 percent of otherwise-participants not appearing in the labor force at any point in the relevant period.

These participation effects do not seem to translate strongly into employment effects, as indicated by the last set of results in Table 1.6. None of the estimates are statistically different from zero at reasonable levels though a few of the point estimates are positive and of economically meaningful magnitudes. These results indicate that the higher search and participation induced by offset elimination did not result in additional employment in the short term.

A similar story is told by the longer-term results in Table 1.7, which reports the effects on labor force participation and employment at seven to twelve months after separation. The participation estimates remain negative across the board, but are no longer statistically significant or as large as they were in the first six months. The employment estimates are now all negative and the larger positive point estimates have disappeared.

1.6.4.3 IV Estimated Effects of Marginal UI Benefits

I next estimate the implied effects of the marginal UI benefit on the labor force outcomes. For each model, I use the coefficients I have reported throughout (main effects in models (1) and (2), interacted effects in models (3) and (4)) as the only excluded instruments in a two-stage least squares estimation of the effects of UI benefits on these outcomes. Essentially, I use the weekly UI benefits estimates from Table 1.5 as a first stage for outcomes in Tables 1.6 and 1.7. This exercise is attractive in that it gives more parsimonious estimates for each outcome, because it combines both the 50 percent and 100 percent policies, and because it is more easily compared to prior work in this area. Clearly, the estimated effects are quite local to those who are affected by these policies, but I argue that is desirable in this situation because this group of retirement-age workers is potentially different from the average worker in an interesting way.

The estimated effects of a \$100 increase in benefits are shown in Table 1.8. The first row of results shows that there are 10 percentage point increases in takeup associated with a \$100 increase in benefits in models (1) and (2). The takeup effects in models (3) and (4) are closer to five percentage points. In principle, these estimates are comparable to takeup estimates in the literature, particularly those reported by Anderson and Meyer (1997), who find a 10 percent increase in benefits to be associated with a two percent increase in takeup. The \$100 increase estimated in model (1) here is approximately a 40 percent increase for the control group's base of \$236, and the estimated effect of 10 percentage points amounts to a roughly 30 percent increase on the base of 0.315. The implied elasticity of approximately 0.75 is thus markedly larger than the existing estimate in the literature. A similar elasticity is suggested by model (2), while models (3) and (4) suggest elasticities that are smaller, but still different from Anderson and Meyer (1997). This difference suggests that workers in the age range being studied here are more sensitive to UI benefits, but it is important to acknowledge that large swings in benefits are generated by offset policies, especially as they reduce benefits to zero for a nontrivial part of the treatment group.

As one might expect given the estimates in Table 1.6, the work search effects are of comparable magnitudes to the takeup effects. Again, the estimates are more muted in models (3) and (4) than in models (1) and (2), though they still show that a \$100 increase in benefits can move the probability of searching for work by a few percentage points.

Labor force participation in the first six months appears to be increased by some two to five percentage points for the \$100 increase in benefits, though most of the coefficients are at least marginally insignificant. A similar story prevails for labor force participation at months seven to twelve. Employment effects are never statistically different from zero, but are largely positive, with the exception of the model (2) estimate for the first six months. It appears that while the results do not show increased employment from the additional benefits, most of them reject large negative effects.

1.6.4.4 IV Estimates Over Time

These results, particularly for employment, may mask delays in job-finding because they aggregate too many months. If the increased benefits lead workers to delay claiming until the end of their benefit period at six months, that would not be indicated by these results. Therefore, I perform the analysis at individual months. This has the advantage of testing for such effects, but results in rather less precise estimates owing to the disaggregation.

I present the model (3) estimated effects at each month in Figure 1.14. Each point is generated by a separate regression where the outcome is for that month alone. I privilege the estimates from model (3) because they are more precise than the model (1) and (2) estimates and they may control better for labor market conditions in offset states. They also are less subject to concerns of endogenous Social Security claiming associated with model (4). However, I display the results from the other models in Figure 1.15.

Figure 1.14 shows patterns of UI receipt and work search effects that one might expect: the most positive estimates are in the first few months and the effects generally fade over time. The labor force estimates remain positive over the entire period and are occasionally different from zero at the five percent level. Perhaps most interestingly, the employment point estimates are also positive throughout and would seem to rule out meaningful decreases in employment even at short horizons. It is important to point out that employment in the first few months is where the model (1) and (2) estimates in Figure 1.15 appear to deviate most strongly from Figure 1.14. The point estimates from models (1) and (2) hug zero quite closely, but because they are imprecise, I cannot rule out decreases of five or even ten percentage points.

1.7 Discussion

Ignoring some of the ambiguity, a simple characterization of the results is that eliminating offsets leads to additional participation (job search) activity by Social Security recipients, but does not lead to additional employment. These findings appear to invite two interpretations: 1) additional participation in the UI system has no effect on reemployment in general, or 2) the additional participation does not result in reemployment for this population. In the first interpretation, these marginal claimants (and other claimants) are able to say they are participating in the labor force and claim UI benefits without doing anything that reasonably leads to reemployment. In the second interpretation, other factors reduce the rate at which this population converts participation and labor force activity into employment. I have no direct test to distinguish between these two interpretations, but I argue that existing research points toward the latter interpretation and not the former. While the particular effects of UI policies on the Social Security-age population are of interest, the group's unique labor force experience makes it relatively less appropriate for examining the efficacy of UI participation in general.

Job-losers at older ages have been shown to be more likely to exit the labor force (e.g., Chan and Stevens 2001) and more sensitive to labor market conditions in their exit decisions (von Wachter, 2007; Coile and Levine, 2007). However, this particular evidence is far from a smoking gun: older workers may exit the labor force because their outside options are relatively better and not because the productivity of their job search is relatively lower. I focus here on identifying research that points toward limited labor market opportunities for unemployed workers near retirement ages.

The most direct evidence on this topic comes from the audit study of Lahey (2008a). This type of resume-based audit study is very convenient for considering the differential effects of search requirements because it estimates the productivity of an additional employer contact. Lahey submitted pairs of resumes carrying female names to job openings in two cities during the early 2000s. High school graduation years on the resumes were randomly assigned one of five values that implied applicant ages ranging from 35 to 62. The primary finding is that, splitting the resumes into ages 35 to 45 and 50 to 62, the number of applications it took to generate one interview offer was 40 percent higher for the older group than the younger group (24.8 versus 17.5). As with all audit studies, it is not clear that this result is representative of all aspects of the actual hiring process. It should also be noted that search requirements generally demand more aggressive employer contacts than simple resume submission, and it is not clear if the in-person contacts would tend to exacerbate or reduce the age differential. Still, the study provides some evidence that employer contacts may be less likely to generate employment for older workers.

At least three other studies suggest that unemployed older workers may face fewer labor market opportunities than their younger counterparts. Hutchens (1988) compares newly-hired older workers to both newly-hired young workers and to all older workers and finds that they are concentrated in a narrower range of occupations and industries than both comparison groups. While there could be other causes behind this pattern, it is consistent with a market in which older workers who are looking to find employment face a limited set of job opportunities. Using employer-level data through the 1980s and early 1990s, Scott et al. (1995) find that new hires were less likely to be over age 55 at firms with health care plans. The implication is that through variation in wages, hiring processes, or other amenities, it is possible for firms to avoid hiring employees who are expected to have high benefit costs when the firms are required to provide employee benefits universally. Other work by Lahey (2008b) examines historical state variation in the ease with which workers could file age discrimination claims. Her results indicate that states where filing was easier had lower employment rates for older workers. Her interpretation is that easier discrimination claims incentivize firms to avoid hiring older workers and thus avoid claims from their own employees. If this mechanism indeed is driving the result, then it indicates that older workers' job opportunities may be limited in the presence of anti-discrimination laws.

Most of this evidence alone is not enough to show that UI claiming and participation is less productive for older workers. Lahey (2008a) compares responses from a fixed set of firms, while Hutchens (1988) and Scott et al. (1995) can only make claims about how workers sort into firms. It is possible that Lahey's older applicants could generate relatively higher callback rates if they applied to different firms that are outside of her sample. Similarly, the sorting implied by Hutchens (1988) and Scott et al. (1995) could just mean that older workers' search productivities are as high as younger workers' if they apply to the appropriate jobs. However, if those jobs are limited or if workers apply without regard to them, then we should expect that marginal job-seeking efforts should be less productive for older workers.

1.8 Conclusion

This paper analyzes the effects of state-level Social Security offset policies to unemployment insurance on labor force behavior of workers over 62. Using UI claimant counts from the Department of Labor, data from the Benefit Accuracy Measurement program, and survey responses from the Survey of Income and Program Participation, I exploit numerous state policy changes over a period extending from the early 1980s to 2012. I estimate considerable effects of offset policies on UI benefits and claiming at the individual level. I use aggregate state data to show that the offsets reduce UI claims among workers over 60, with particularly large effects for 100 percent offset policies for workers over 65. I find evidence of reduced UI takeup and job search activity associated with the offset policies in micro-level data. Using the policy changes as instruments for UI benefits, I find that a \$100 increase in benefits leads to a five to ten percentage point decrease in UI claiming (on a base of 30 percent) and a similarly-sized effect on the likelihood of ever looking for a job. Ultimately, there are no effects on reemployment. The evidence broadly suggests that UI receipt can positively impact search behavior on the extensive margin, at least as it is reported in surveys. The availability of UI benefits to near-retirement-age workers increases their labor force participation but not their employment. However, most of the estimates indicate that UI benefits do not have a depressive effect on reemployment for this population.

Table 1.1: State changes in Social Security offset policies since 1986

State	1986 Policy	Statute	Amending Law	Month	Effect
Alabama	50	25-4-78	1995 PA 311	Oct1995	50 to 0
Alaska	0				
Arizona	45	23-791	2004 Ch 251	May 2004	45 to 0
Arkansas	100	11-10-517	1991 No 48	Jul 1991	100 to 0
California	0				
Colorado	100	8-73-110	2000 Ch 288	Oct 2000	100 to 50
			2009 Ch 408	Jun 2009	50 to 0
Connecticut	50	31-227	PA 04-214	Oct 2004	50 to 0
Delaware	50	3313	71 Del. Laws 146	Jul 1997	50 to 25
			71 Del. Laws 404	Jan 1999	25 to 0
DC	100	51-107	DC Law 15-282	Apr 2005	100 to 0
Florida	50	443.101	1992 Ch 38	Jul 1992	50 to 0
Georgia	50	34-8-193	1991 No 15	Jan 1992	50 to 0
Hawaii	50	383-23.5	2005 Act 106	Jul 2005	50 to 0
Idaho	50	72-1312	1998 Ch 1	Jul 1998	50 to 0
Illinois	50	405/611			
Indiana	100	22-4-15-4	1998 PL 3	Jul 1998	100 to 0
Iowa	50	96.5	2001 Ch 111	May 2001	50 to 0
Kansas	50	44-706	2003 Ch 158	Jul 2003	50 to 0
Kentucky	100	341.390	1998 Ch 167	Jul 1998	100 to 0
Louisiana	50	23:1601			
Maine	50	26 MRS 1193	2007 Ch 352	Sep 2007	50 to 0
Maryland	0	Labor 8-1008			
Massachusetts	50	151A Sec. 29	2006 Ch 123	Jun 2006	50 to 0
Michigan	0	421.27			
Mississippi	100	71-5-513	2001 Ch 405	Jul 2001	100 to 0
Missouri	100	288.040	1991 HB 422	Jun 1991	100 to 0
Montana	50	39-51-2203	1989 Ch 618	Jul 1989	50 to 0
Nebraska	100	48-628	2005 LB 484	Jan 2006	100 to 0
Nevada	50	612.375	1989 Ch 583	Jun 1989	50 to 0
New Hampshire	100	282-A:28	1987 Ch 243	Jul 1987	100 to 0
New Jersey	0	43:21-5a			
New Mexico	50	51-1-4	1993 Ch 209	Apr 1993	50 to 0
New York	0	CLS Lab. 600			
North Carolina	100	96-14	1993 Ch 122	Jun 1993	100 to 0
North Dakota	62.5	52-06-02	1985 Ch 545	Jul 1986	62.5 to 50
			2005 Ch 460	Mar 2005	50 to 0
Ohio	100	4141.312	152 v SB 116	Nov 2007	100 to 0
Oklahoma	100	40 OK 2-411	1995 HB 1462	Jul 1995	100 to 0
Oregon	50	657.205	1987 Ch 270	Jun 1987	50 to 0
Pennsylvania	50	Ch 14 UC Law	Act 2005-80	Dec 2005	50 to 0
Rhode Island	50	28-44-19.1	2007 Ch 89	Jun 2007	50 to 0
South Carolina	50	41-27-370	2000 No 349	Jun 2000	50 to 0
South Dakota	50	61-6-35	2006 Ch 269	Mar 2006	50 to tr
			2012 Ch 252	Feb 2012	tr to 0
Tennessee	50	50-7-303	1987 Ch 424	May 1987	50 to 0
Texas	100	207.050	199 Ch 906	Jun 1995	100 to 0
Utah	100	35A-4-401	2004 Ch 246	Jul 2004	100 to 50
			2010 Ch 293	Dec 2010	50 to 0
Vermont	50	21 VSA 1344	1987 No 179	May 1988	50 to 0
Virginia	100	60.2-604	2003 Ch 555	Jul 2003	100 to 50
			2005 Ch 1	Jan 2006	50 to tr
			2011 Ch 748	Jul 2011	tr to 0
Washington	50	50.04.323	1993 Ch 483	Jul 1993	50 to 0
West Virginia	100	21A-6-3	2005 Ch 242	Jul 2005	100 to 0
Wisconsin	50	108.05	2001 AB 553	Jan 2003	50 to 0
Wyoming	100	27-3-13	1990 Ch 49	Jul 1990	100 to 50
			2003 Ch 73	Jul 2003	50 to 0

Notes: "Month" indicates the first month in which the new policy was effective. South Dakota instituted a trigger policy in 2006 and Virginia passed a trigger law in 2005, both of which tied their offsets to the funding levels of their UI trust funds. Utah's original 2004 reduction was temporary, but was extended in 2006 and ultimately superceded by the 2010 law. Minnesota is omitted due to ambiguity concerning the effects of 2007 Ch 128.

Table 1.2: Effects of offsets on probability of inclusion in likely-UI-eligible sample, conditional on controls

	(1)	(2)	(3)	(4)
Sample:	62+	SS	51+	51+
Reported coeff.:	Policy	Policy	Policy x 62+	Policy x SS
First observed separation:				
50% offset	-0.01	-0.03	-0.03	-0.05
	(0.03)	(0.03)	(0.02)	(0.02)
100% offset	0.01	0.01	-0.04	-0.03
	(0.03)	(0.03)	(0.02)	(0.02)
N	*4,698*	*4,406*	*14,287*	*14,287*
All separations:				
50% offset	-0.00	-0.02	-0.01	-0.03
	(0.03)	(0.02)	(0.02)	(0.02)
100% offset	0.03	0.02	-0.03	-0.02
	(0.03)	(0.03)	(0.02)	(0.02)
N	*5,388*	*5,043*	*17,601*	*17,601*

Notes: Huber/White/sandwich standard errors clustered at the state level are reported below each estimate. Coefficients are estimated from linear probability models using the sample of job separators who are 62+, receiving Social Security within six months of separation, or 51+, as indicated. Dependent variable is one if the separation reason is given as layoff, discharge, employer bankruptcy, sale of employer, temporary job ending, or slack work and zero otherwise. Models (1) and (2) report the coefficient estimates on indicators for each offset policy. Models (3) and (4) report the indicated interaction terms, though main effects are also included in the specifications. All regressions include controls for sex, race, education, calendar year, calendar month, age at separation, reported reason for separation, state, and pension receipt interacted with state offset policy for private pensions.

Table 1.3: Characteristics for survey layoff sample

	Age < 62			Age ≥ 62		
Offset policy:	0%	50%	100%	0%	50%	100%
SIPP:						
Year of separation	2002	1996	1994	2002	1996	1993
Age at separation	55.2	55.5	55.1	66.9	66.3	66.7
Black	0.097	0.090	0.084	0.078	0.086	0.089
Male	0.522	0.533	0.577	0.508	0.513	0.554
High school only	0.269	0.330	0.379	0.278	0.393	0.315
Some college	0.323	0.246	0.236	0.294	0.206	0.250
College grad	0.259	0.230	0.191	0.258	0.206	0.173
Social Security recipient	0.021	0.021	0.029	0.758	0.739	0.838
Pension recipient	0.123	0.138	0.161	0.371	0.345	0.455
Pension policy A	0.576	0.420	0.295	0.559	0.408	0.262
Pension policy B	0.723	0.825	0.187	0.700	0.846	0.202
N	*2,777*	*874*	*593*	*908*	*267*	*168*

Notes: Table displays SIPP sample characteristics for job separators ages 51 and over at their first observed likely-eligible job separation. This includes separations due to layoff, discharge, employer bankruptcy, sale of employer, temporary job ending, or slack work. Social Security and pension receipt are defined as each type of income within six months of separation. Pension policy A indicates a state policy of only offsetting private pensions if eligibility or benefits were affected by base period work. Pension policy B indicates a state policy of considering employee contributions to pensions.

Table 1.4: Effects of offsets on aggregate monthly total weeks of unemployment insurance claimed by workers over 60

	Log claims 65+		
50% offset	-0.13	-0.11	-0.10
	(0.04)	(0.03)	(0.04)
100% offset	-0.50	-0.35	-0.47
	(0.17)	(0.14)	(0.13)
State FE	X	X	X
Lin. state trends		X	X
Quad. state trends			X
	Log claims 60-64		
50% offset	-0.03	-0.02	-0.01
	(0.02)	(0.01)	(0.01)
100% offset	-0.10	-0.04	-0.03
	(0.03)	(0.03)	(0.03)
State FE	X	X	X
Lin. state trends		X	X
Quad. state trends			X

Notes: Estimates are generated using 6,285 state-month observations from Department of Labor data on characteristics of the insured unemployed. Huber-White-sandwich standard errors are clustered at the state level. All regressions additionally include controls for individual months and the monthly log claims in the 55 to 59 age range. The sample period extends from January 2001 to October 2012. 45 states and the District of Columbia are included. Maine, Pennsylvania, and South Dakota are dropped because of irregularities in the data. Virginia is dropped after the implementation of its trigger law. Minnesota and Massachusetts are dropped during the periods of hybrid policy in which offsets did not apply to those who claimed before the base period. A further 247 state-months are dropped because of missing age data for more than 5% of the population. Three quarters of these dropped months are from Georgia, New Mexico, and South Carolina. Fully dropping these additional three states has no impact on the estimates.

Table 1.5: Offset Effects on Calculated UI Benefits

	(1)	(2)	(3)	(4)
Sample:	62+	SS	51+	51+
Reported coeff.:	Policy	Policy	Policy x 62+	Policy x SS
	Any UI Benefit Available			
50 % Offset	-0.260	-0.360	-0.305	-0.393
	(0.039)	(0.047)	(0.029)	(0.039)
100 % Offset	-0.576	-0.729	-0.581	-0.683
	(0.075)	(0.075)	(0.052)	(0.055)
Control group mean:	1.000	1.000	1.000	1.000
	Weekly UI Benefit			
50 % Offset	-96.5	-126.2	-105.1	-141.0
	(14.6)	(14.7)	(11.5)	(11.3)
100 % Offset	-134.8	-154.4	-126.7	-148.2
	(18.7)	(13.9)	(10.1)	(10.6)
Control group mean:	235.7	214.5	235.7	214.5
N:	*1,252*	*1,017*	*5,744*	*5,744*

Notes: Huber/White/sandwich standard errors clustered at the state level are reported below each estimate. Coefficients are estimated from linear models using the sample of job separators who are 62+, receiving Social Security within six months of separation, or 51+, as indicated. The dependent variables are calculated using reported earnings and Social Security benefits and published state UI rules. Models (1) and (2) report the coefficient estimates on indicators for each offset policy. Models (3) and (4) report the indicated interaction terms, though main effects are also included in the specifications. All regressions include controls for sex, race, education, calendar year, calendar month, age at separation, reported reason for separation, state, and pension receipt interacted with state offset policy for private pensions. The reported control group means are for the Social Security proxy group only. This is the entire control group in models (1) and (2) but only those 62 and older in model (3) and only Social Security recipients in model (4). Weekly benefits are reported in 2012 dollars.

Table 1.6: Labor Force Outcomes: First Six Months after Job Loss

	(1)	(2)	(3)	(4)
Sample:	62+	SS	51+	51+
Reported coeff.:	Policy	Policy	Policy x 62+	Policy x SS
		Any UI Receipt		
50 % Offset	-0.097	-0.139	0.007	-0.023
	(0.050)	(0.063)	(0.032)	(0.036)
100 % Offset	-0.136	-0.179	-0.088	-0.121
	(0.054)	(0.061)	(0.034)	(0.037)
Control group mean:	0.315	0.300	0.315	0.300
		Any Work Search		
50 % Offset	-0.041	-0.042	-0.007	-0.022
	(0.056)	(0.064)	(0.043)	(0.038)
100 % Offset	-0.178	-0.182	-0.114	-0.112
	(0.071)	(0.061)	(0.052)	(0.043)
Control group mean:	0.405	0.372	0.405	0.372
		Any Labor Force Participation		
50 % Offset	-0.005	0.013	0.016	0.004
	(0.055)	(0.074)	(0.033)	(0.035)
100 % Offset	-0.116	-0.101	-0.098	-0.123
	(0.052)	(0.066)	(0.021)	(0.031)
Control group mean:	0.701	0.663	0.701	0.663
		Any Employment		
50 % Offset	0.025	0.077	-0.015	0.008
	(0.070)	(0.081)	(0.046)	(0.042)
100 % Offset	-0.014	0.047	-0.047	-0.062
	(0.069)	(0.082)	(0.030)	(0.043)
Control group mean:	0.460	0.438	0.460	0.438
N:	*1,252*	*1,017*	*5,744*	*5,744*

Notes: Huber/White/sandwich standard errors clustered at the state level are reported below each estimate. Coefficients are estimated from linear models using the sample of job separators who are 62+, receiving Social Security within six months of separation, or 51+, as indicated. Models (1) and (2) report the coefficient estimates on indicators for each offset policy. Models (3) and (4) report the indicated interaction terms, though main effects are also included in the specifications. All regressions include controls for sex, race, education, calendar year, calendar month, age at separation, reported reason for separation, state, and pension receipt interacted with state offset policy for private pensions. The reported control group means are for the Social Security proxy group only. This is the entire control group in models (1) and (2) but only those 62 and older in model (3) and only Social Security recipients in model (4).

Table 1.7: Labor Force Outcomes: Seven to Twelve Months after Job Loss

	(1)	(2)	(3)	(4)
Sample:	62+	SS	51+	51+
Reported coeff.:	Policy	Policy	Policy x 62+	Policy x SS
	Any Labor Force Participation			
50 % Offset	-0.046	-0.011	-0.005	-0.015
	(0.068)	(0.088)	(0.045)	(0.050)
100 % Offset	-0.071	-0.009	-0.038	-0.048
	(0.075)	(0.096)	(0.045)	(0.048)
Control group mean:	0.656	0.614	0.656	0.614
	Any Employment			
50 % Offset	-0.044	-0.014	-0.007	-0.015
	(0.062)	(0.078)	(0.038)	(0.043)
100 % Offset	-0.039	0.015	-0.021	-0.017
	(0.077)	(0.102)	(0.047)	(0.051)
Control group mean:	0.530	0.495	0.530	0.495
N:	*1,003*	*831*	*4,464*	*4,464*

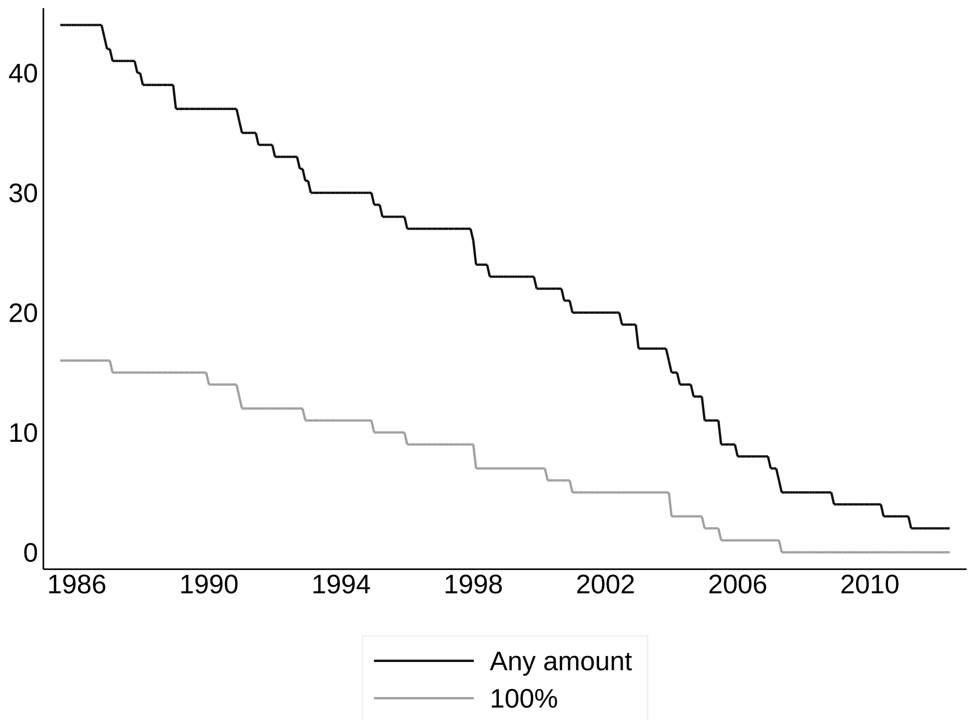
Notes: Huber/White/sandwich standard errors clustered at the state level are reported below each estimate. Coefficients are estimated from linear models using the sample of job separators who are 62+, receiving Social Security within six months of separation, or 51+, as indicated. Models (1) and (2) report the coefficient estimates on indicators for each offset policy. Models (3) and (4) report the indicated interaction terms, though main effects are also included in the specifications. All regressions include controls for sex, race, education, calendar year, calendar month, age at separation, reported reason for separation, state, and pension receipt interacted with state offset policy for private pensions. The reported control group means are for the Social Security proxy group only. This is the entire control group in models (1) and (2) but only those 62 and older in model (3) and only Social Security recipients in model (4).

Table 1.8: 2SLS Estimated Local Effects of $100 Increase in Weekly UI Benefits

	(1)	(2)	(3)	(4)
Sample:	62+	SS	51+	51+
Any UI receipt	0.097	0.106	0.057	0.044
	(0.035)	(0.034)	(0.020)	(0.024)
Any Work Search	0.092	0.073	0.067	0.042
	(0.046)	(0.036)	(0.030)	(0.024)
Any LF Participation 1-6	0.049	0.023	0.046	0.035
	(0.037)	(0.043)	(0.020)	(0.019)
Any LF Participation 7-12	0.061	0.013	0.056	0.026
	(0.042)	(0.047)	(0.024)	(0.027)
Any Employment 1-6	0.002	-0.045	0.040	0.019
	(0.045)	(0.045)	(0.026)	(0.021)
Any Employment 7-12	0.048	0.006	0.033	0.021
	(0.046)	(0.050)	(0.023)	(0.025)
N:	*1,003*	*831*	*4,464*	*4,464*

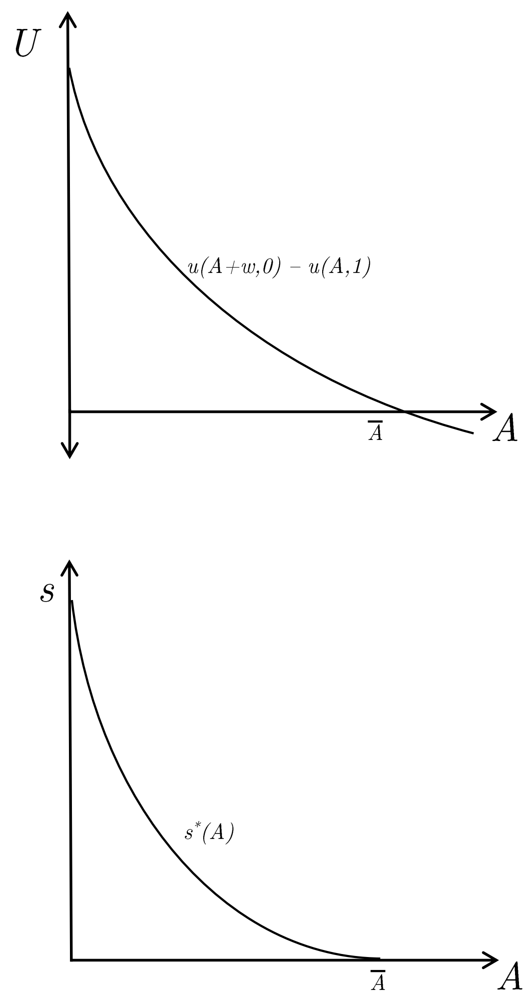
Notes: Huber/White/sandwich standard errors clustered at the state level are reported below each estimate. Coefficients are estimated from linear two-stage least squares (2SLS) models using the sample of job separators who are 62+, receiving Social Security within six months of separation, or 51+, as indicated. The endogenous variable, weekly UI benefits in 100s of dollars, is calculated from state UI rules and survey reports of Social Security benefits and base period earnings. Models (1) and (2) use dummies for 50% and 100% offset policies as excluded instruments. The instruments in model (3) are policy indicators interacted with an indicator for being over 62. The instruments in model (4) are policy indicators interacted with an indicator for being a Social Security recipient. The main effects of the two parts of these interaction terms are included in the second stage in both models (3) and (4). All regressions include controls for sex, race, education, calendar year, calendar month, age at separation, reported reason for separation, state, and pension receipt interacted with state offset policy for private pensions.

Figure 1.1: Number of states offsetting unemployment benefits by Social Security



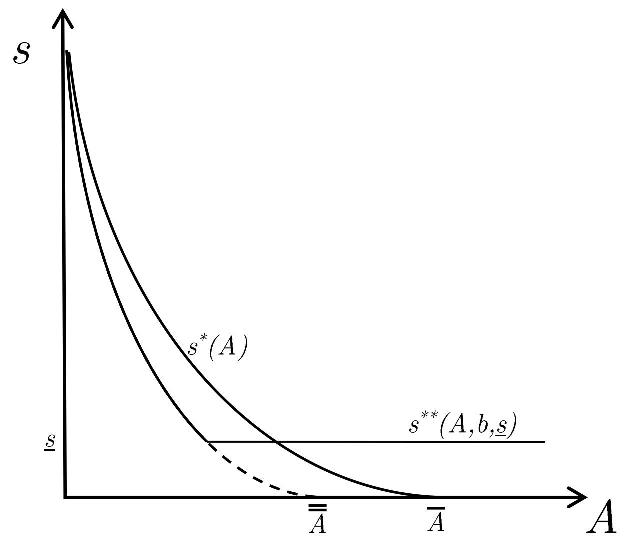
Notes: Figure indicates the number of states offsetting unemployment benefits by some fraction of Social Security retirement income each month. 49 states and the District of Columbia are included, with Minnesota being the dropped state because of policy ambiguity.

Figure 1.2: Returns to employment and optimal search effort by assets



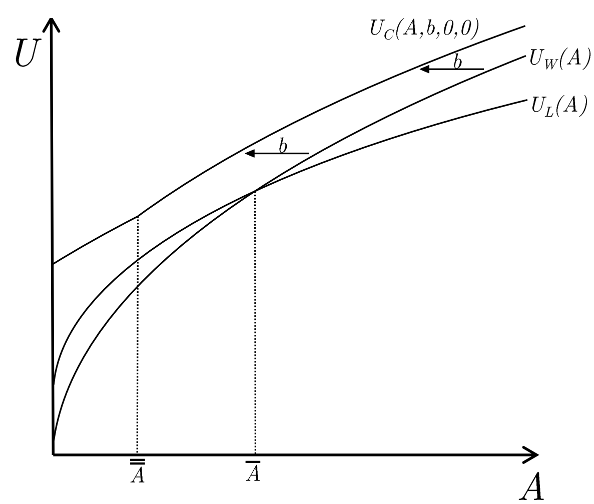
Notes: Figure presents a stylized representation of the varation in the value of employment and optimal search effort for the model described in Section 1.3. $\bar{A}$ marks the asset level at which workers are indifferent between unemployment without benefits and employment at wage w.

Figure 1.3: Search effort with and without receipt of unemployment benefits, with search effort floor for claimants



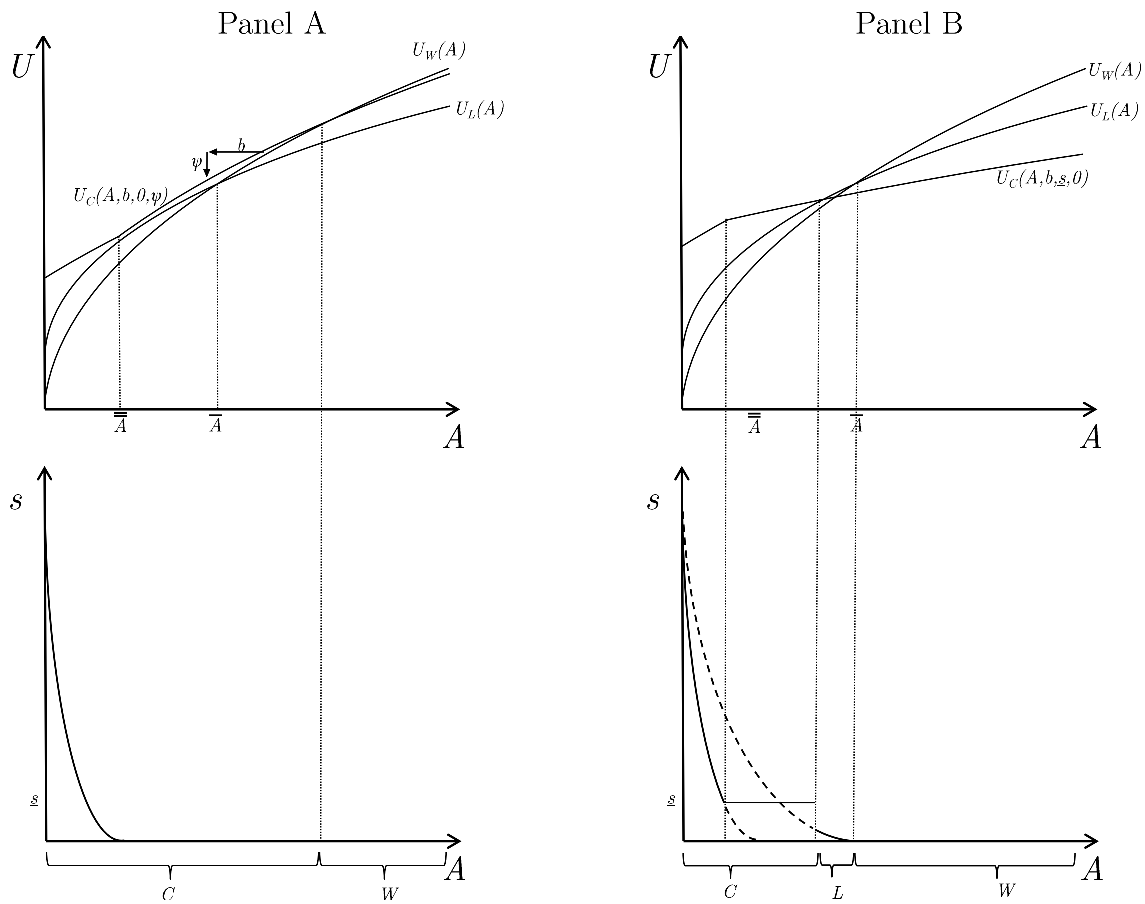
Notes: Figure presents a stylized representation of the effects of unemployment benefits and a search effort floor on search intensity for job searchers.

Figure 1.4: Value of labor force states by assets



Notes: Figure shows the values of each labor force state at all asset levels. The U_C curve assumes no search effort floor enforced by the unemployment agency and no fixed costs of claiming.

Figure 1.5: Value of labor force states and search effort

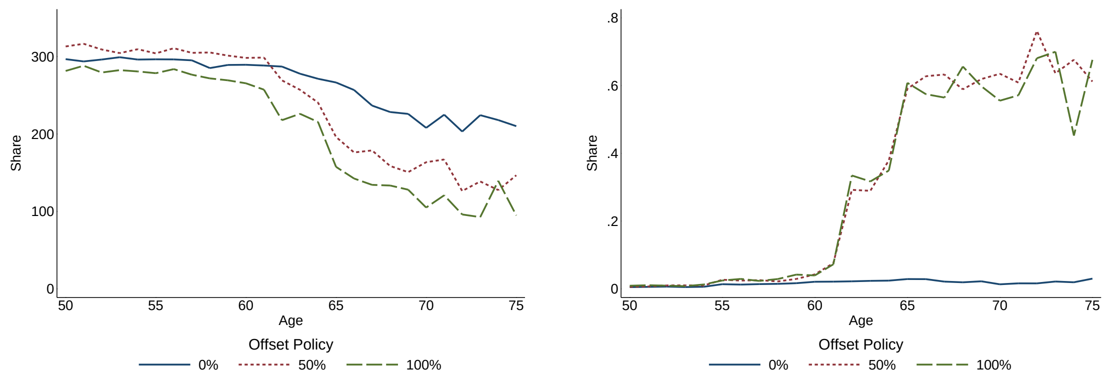


Notes: Panel A depicts values of each state with a fixed cost of unemployment claiming ψ. The dashed vertical line dropping between the panels shows the point at which labor force withdrawal becomes optimal relative to UI claiming. The regions over which those two states are optimal is shown on the horizontal axis by "W" and "C," respectively. Panel B depicts the values of each state when the unemployment agency enforces a particular search intensity floor for claimants. "C," "L," and "W" along the horizontal axis show the region over which claimaing, search without claiming, and withdrawal are optimal, respectively.

Figure 1.6: BAM measures of effects of offsets on UI benefits

Panel A: Average benefits paid

Panel B: Claimants with income deductions

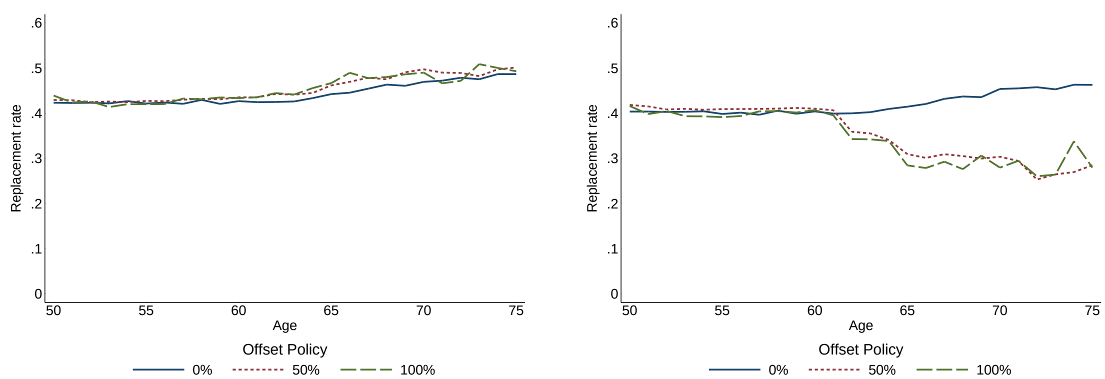


Notes: Averages are generated from BAM data on UI claimants audited between 1988 and 2012.

Figure 1.7: BAM measures of effects of offsets on replacement rates

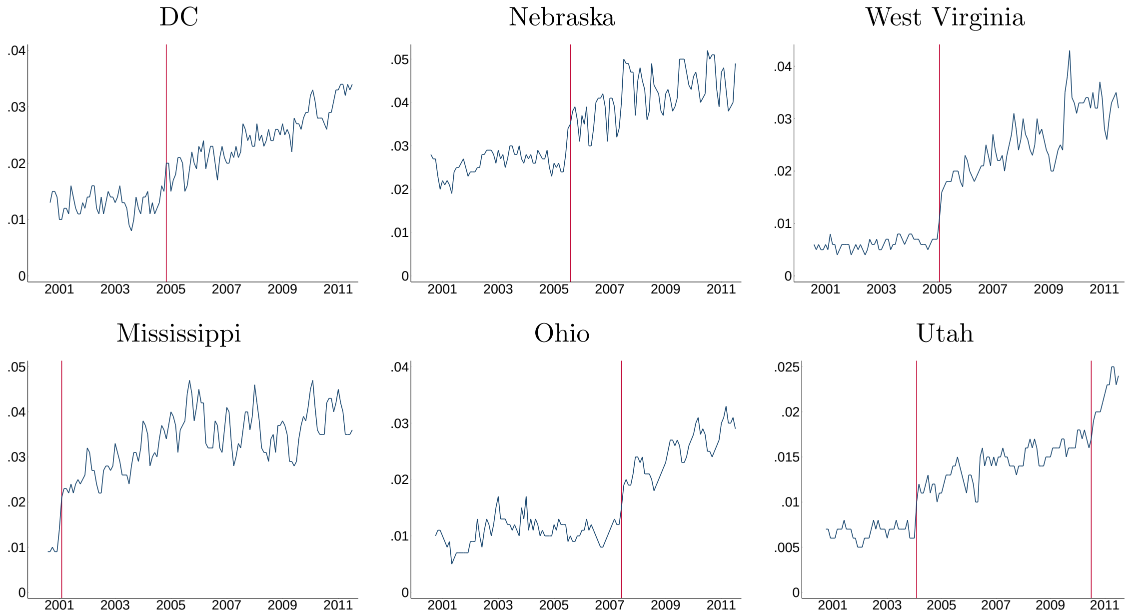
Panel A: Before deductions

Panel B: Actual payments



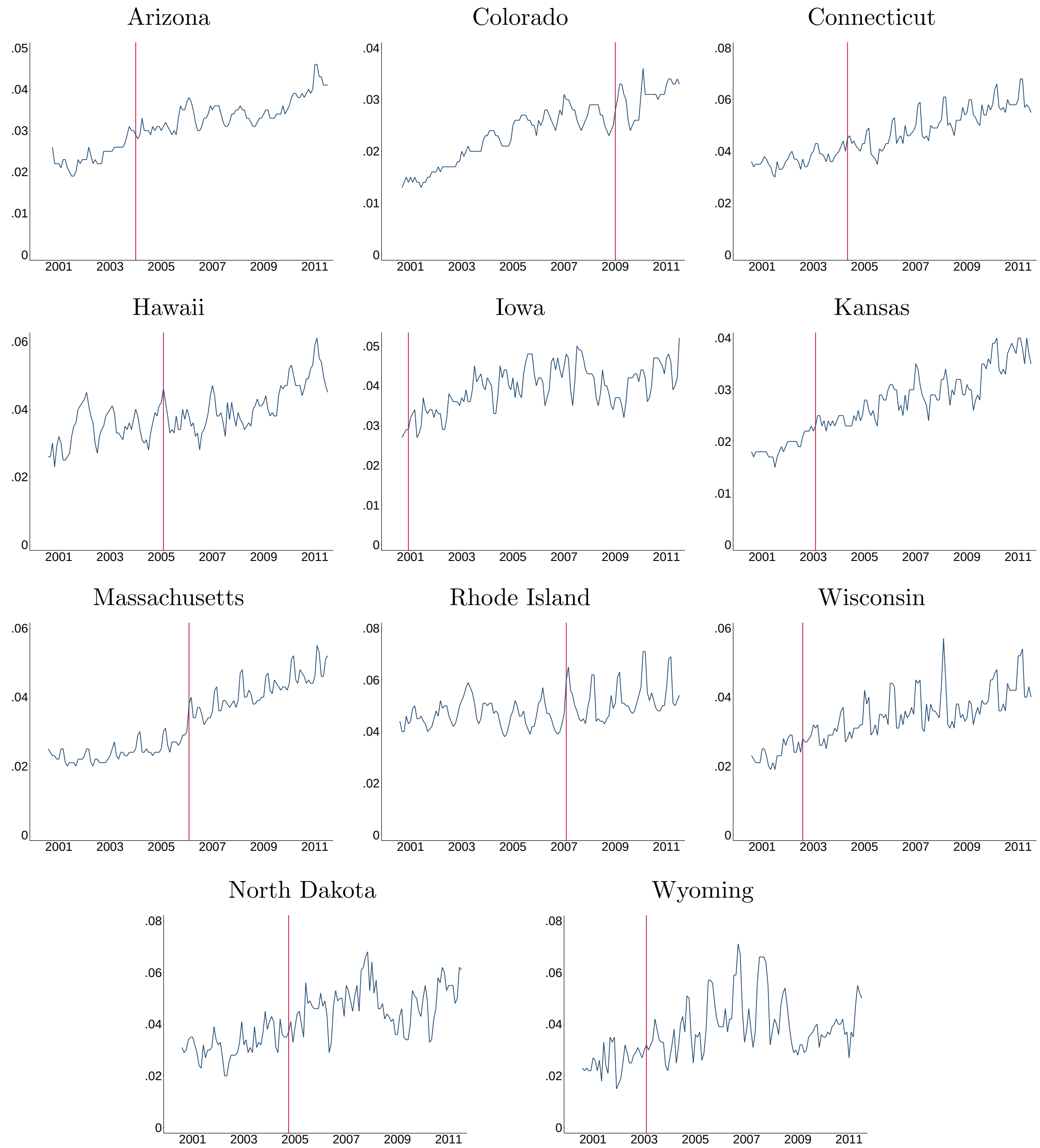
Notes: Averages are generated from BAM data on UI claimants audited between 1988 and 2012.

Figure 1.8: Share of total claim weeks received by workers over 65, 100% offset states



Notes: Each panel shows the share of total week unemployment claims received by workers over 65 in states that had 100% Social Security offset policies in 2001. Figures are generated using "Characteristics of the Insured Unemployed" data posted by the Department of Labor. Vertical lines indicate the timing of a policy change. With the exception of Utah, this policy change represents the elimination of the Social Security offset. Utah's initial policy change replaced the 100% offset with a 50% offset before eliminating it at the end of 2010.

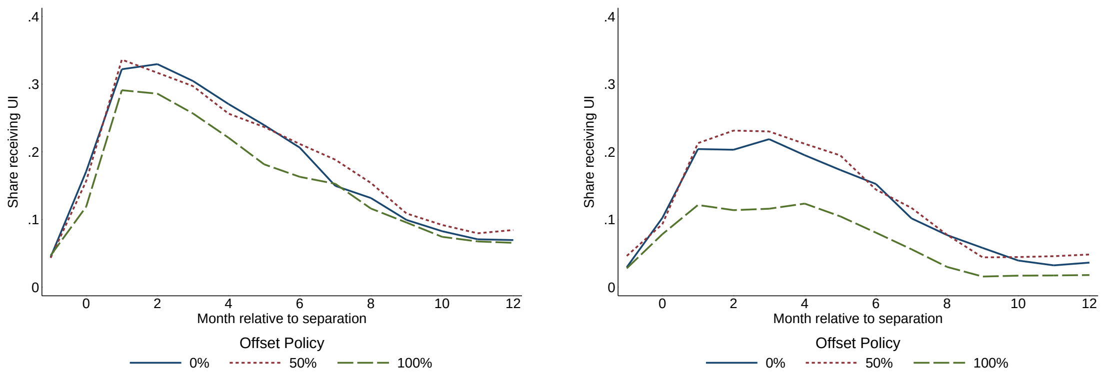
Figure 1.9: Share of total claim weeks received by workers over 65, 50% offset states



Notes: Each panel shows the share of total week unemployment claims received by workers over 65 in selected states that had 50% Social Security offset policies in 2001. Arizona's offset was actually only 45%. Figures are generated using "Characteristics of the Insured Unemployed" data posted by the Department of Labor. Vertical lines indicate the month in which the offset was eliminated.

Figure 1.10: UI recipiency among job separators in SIPP, by age and offset policy

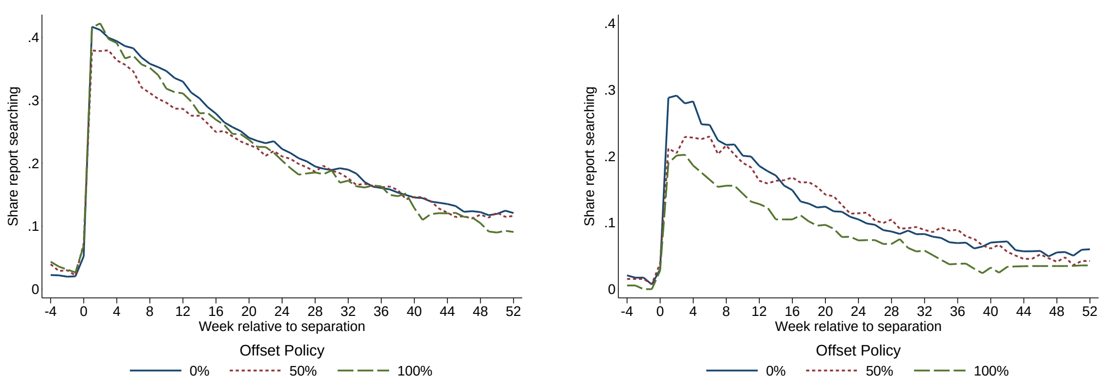
Panel A: Ages < 62 Panel B: Ages ≥ 62



Notes: Averages are generated from an unbalanced panel of likely-UI-eligible job separators in the SIPP as described in Table 1.3 and the text. Separations are from 1986 to 2007.

Figure 1.11: Job search among job separators in SIPP, by age and offset policy

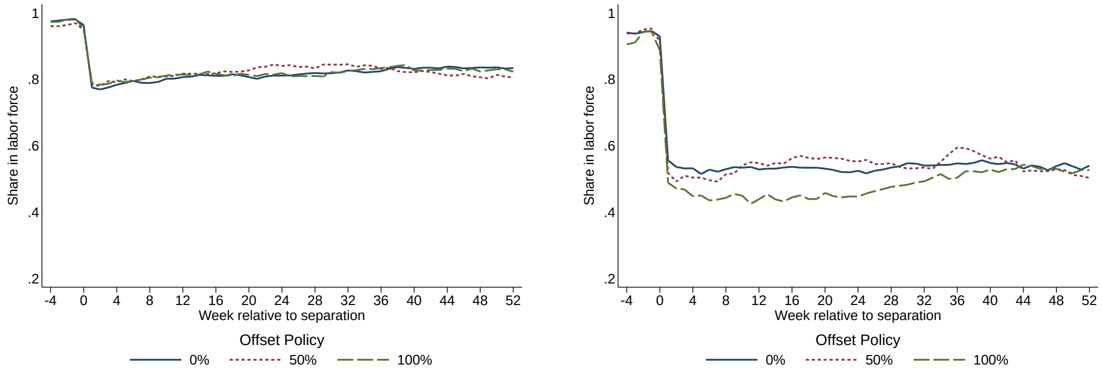
Panel A: Ages < 62 Panel B: Ages ≥ 62



Notes: Averages are generated from an unbalanced panel of likely-UI-eligible job separators in the SIPP as described in Table 1.3 and the text. Separations are from 1986 to 2007.

Figure 1.12: Labor force participation among job separators in SIPP, by age and offset policy

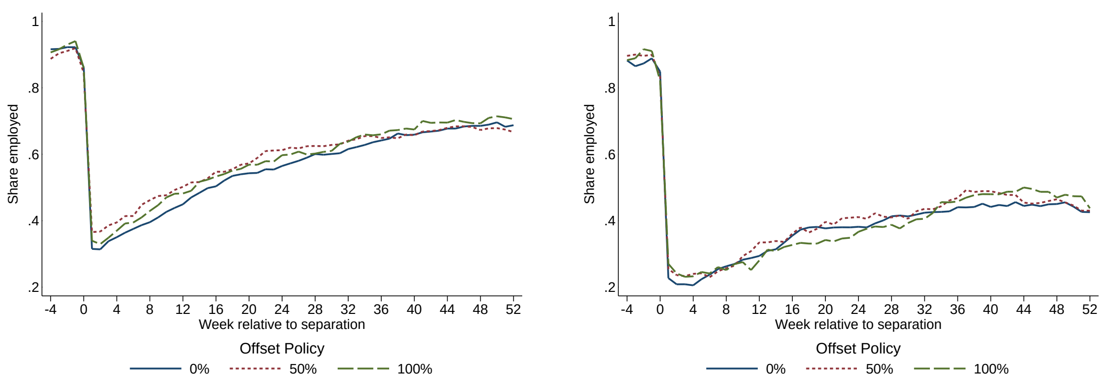
Panel A: Ages < 62 Panel B: Ages ≥ 62



Notes: Averages are generated from an unbalanced panel of likely-UI-eligible job separators in the SIPP. Separations are from 1986 to 2007.

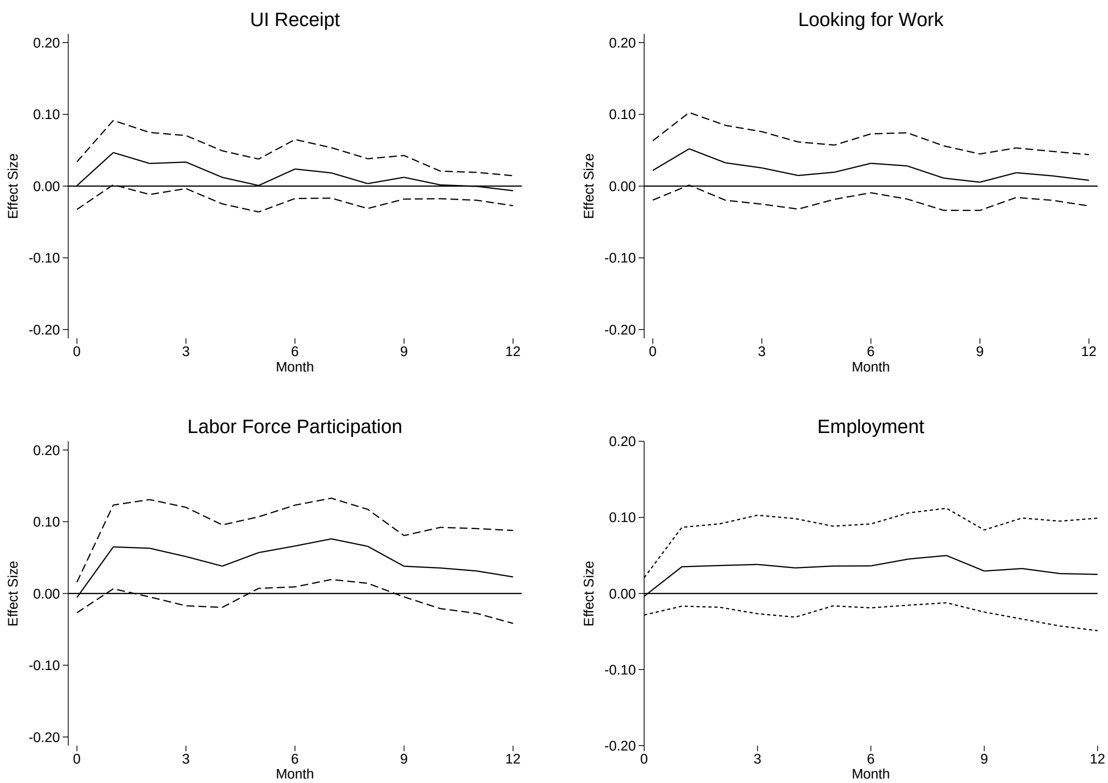
Figure 1.13: Employment among job separators in SIPP, by age and offset policy

Panel A: Ages < 62 Panel B: Ages ≥ 62



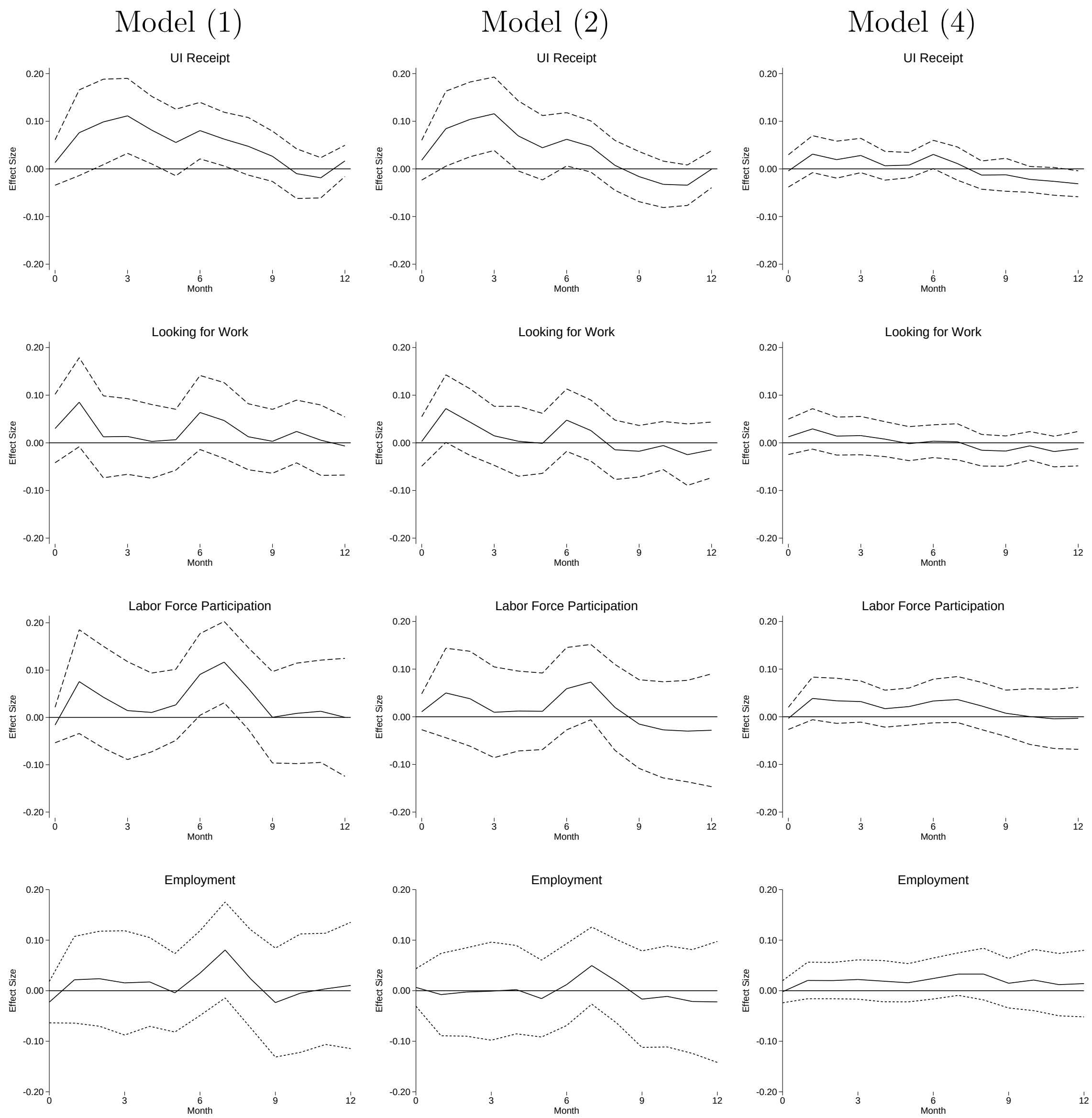
Notes: Averages are generated from an unbalanced panel of likely-UI-eligible job separators in the SIPP. Separations are from 1986 to 2007.

Figure 1.14: 2SLS Monthly Estimated Local Effects of $100 Increase in Weekly UI Benefits, Model (3)



Notes: Huber/White/sandwich standard errors clustered at the state level produce the 95% confidence intervals around each point. Each point is a coefficients estimate from a different linear two-stage least squares (2SLS) model using the sample of job separators who are or 51 or older. The excluded instruments in model are policy indicators interacted with an indicator for being over 62. The main effects of the the policies and being over 62 are included in the second stage. Regressions include controls for sex, race, education, calendar year, calendar month, age at separation, reported reason for separation, state, and pension receipt interacted with state offset policy for private pensions. Sample and methods match those described for model (3) in the text and Table 1.8.

Figure 1.15: 2SLS Monthly Estimated Local Effects of $100 Increase in Weekly UI Benefits, Models (1), (2), and (4)



Notes: Huber/White/sandwich standard errors clustered at the state level produce the 95% confidence intervals around each point. Each point is a coefficients estimate from a different linear two-stage least squares (2SLS) model using the sample of job separators who are 62+, receiving Social Security within six months of separation, or 51+, as indicated for the same models in Table 1.8. The endogenous variable, weekly UI benefits in 100s of dollars, is calculated from state UI rules and survey reports of Social Security benefits and base period earnings. Models (1) and (2) use dummies for 50% and 100% offset policies as excluded instruments. The instruments in model (4) are policy indicators interacted with an indicator for being a Social Security recipient. The main effects of the policies and of being a Social Security recipient are included in the second stage in (4). All regressions include controls for sex, race, education, calendar year, calendar month, age at separation, reported reason for separation, state, and pension receipt interacted with state offset policy for private pensions. Sample and methods match those described for models (1), (2), and (3) in the text and Table 1.8.

CHAPTER II

The Effectiveness of Unemployment Insurance Work-Search Requirements and the Implications for Job Rationing

2.1 Introduction

In recent years, policymakers in many states have dramatically increased the requirements for unemployment insurance (UI) claimants to show that they are actively searching for work. Requiring evidence of work search is an attractive option for policymakers facing UI budgets that have remained weak and unemployment rolls that have remained long in the wake of the Great Recession. There is initially good reason to believe that the changes should be effective: randomized experiments examining the effects of UI job search requirements have shown that they lead to faster reemployment (Johnson and Klepinger, 1994; Klepinger et al., 2002). However, a growing literature indicates that negative externalities may limit the benefits of reemployment policies in general equilibrium (Davidson and Woodbury, 1993; Lise et al., 2004; Crépon et al., 2013). Further, recent job-rationing models of the labor market suggest that a substantial fraction of unemployment in recessions is driven not by matching frictions but by wage rigidities (Michaillat, 2012; Landais et al., 2010). Because search requirements are largely aimed at increasing the rate of matches, their efficacy will be limited if other rigidities are the primary drivers of unemployment. In weak labor markets, search requirements may lead UI claimants to simply compete harder for the same number of job openings. It initially remains unclear whether search requirements can improve the functioning of the labor market and lead to faster reemployment when implemented at the state level.

In this paper, I show that while there is some evidence that work search requirements lead to faster reemployment for UI claimants, the equilibrium effects of work search policies depend on the strength of the labor market. I exploit increases in search requirements in a number of states over the last decade using fixed effects models in state and time. I test whether the requirements measurably affect search intensity and whether any additional search efforts pay off in the form of faster reemployment. Although stronger requirements are associated with measurable increases in a number of search proxies, I find that the increased search requirements have only small impacts on the rate at which UI claimants find employment. Using two search-and-matching models of the labor market, I show that the efficacy of search requirements is likely to depend on ability of the market to create additional jobs. If jobs are limited ("rationed") in downturns, then search requirements may be ineffective at alleviating unemployment in recessions. Evidence on the effects of search requirements across labor market conditions shows that they do seem to be less effective when the labor market is weak.

Although job search requirements have returned to near-ubiquity in UI systems, they are only minimally documented and studied in the existing literature. The prototypical job search requirement in the US requires claimants to contact a minimum number of prospective employers each week. The details of the nature and number of contacts required each week vary across states and over time. I catalog the changes in state-level search requirements since the turn of the century using UI handbooks and forms provided to claimants over time. On the whole, the documented policy changes suggest a strong reversal of the slackening in search rules during the 1980s and 1990s, with a number of states implementing more stringent requirements over the last decade.

I compare these policy codings to the actual number of contacts reported by claimants when audited by their states' Benefit Accuracy Measurement (BAM) programs. First, this analysis shows that claimants respond directly to rising search requirements by reporting more contacts to UI authorities. Second, it shows that many of these contacts are genuine: the number of contacts found by auditors to be acceptable also rises. This latter finding suggests that the additional contacts are reflective of actual search and are not bogus contacts invented by claimants.

I use within-state changes in search requirements to identify the effects of the policies. Using fixed effects models in state and time, I control for time-invariant state characteristics and national trends over time. As it is possible that the policy changes are correlated with time-varying state and claimant characteristics, I control for a number of variables that are likely to be drivers of individual search effort and labor market outcomes.

The results show that likely UI claimants increase some measures of observed search effort when search requirements go up. I show this using measures of search methods as reported in the Current Population Survey (CPS) as in Shimer (2004). Despite this increase in measured effort, there is only slight and statistically undetectable evidence of more rapid reemployment. Reemployment hazard models using monthly CPS data reveal positive but small and statistically insignificant impacts of search requirements on the rate of reemployment. Published statistics on weeks claimed during claimants' benefit years suggest mixed and statistically insignificant impacts.

I interpret the effects of an increase in search requirements in the context of two search-and-matching models. I show that under standard assumptions, search requirements are likely to bind when the labor market is weak. If the market faces no rigidities beyond matching frictions, search requirements will lead workers to find faster reemployment, while firms also open additional vacancies to take advantage of the additional search effort. General equilibrium effects should compound the effectiveness of search requirements. However, if wages are rigid as in recent job-rationing models, a limit on the number of available jobs in downturns may stunt the effectiveness of search requirements. I test for these business cycle differentials and find that search requirements are relatively less effective in weak labor markets when using both lagged unemployment and an industry shift-share measure to proxy for labor market strength.

This paper builds on the experimental results of the search policy literature exemplified by Klepinger et al. (2002) and Ashenfelter et al. (2005) by examining effects in general equilibrium. It also extends search policy analysis in other contexts as in Borland and Tseng (2007), McVicar (2008), and McVicar (2010) to the United States environment by examining a variety of different policies in the US unemployment system. A number of these studies (Ashenfelter et al., 2005; McVicar, 2008, 2010) vary the incidence of job search monitoring, which may have different effects from changing job search requirements under a stable monitoring regime.

More generally, this paper directly examines the general equilibrium effects of labor market policies in the spirit of Davidson and Woodbury (1993) and Lise et al. (2004). Those papers, however, primarily consider search externalities exerted by a treated group on untreated groups, while this paper largely focuses on the mechanisms for crowd-out even if everyone is treated.[1] To my knowledge, it is also the first paper to interpret unemployment policy changes in the context of a job-rationing model as in Michaillat (2012). While Landais et al. (2010) suggest that some labor market policies may be less effective during downturns, this paper directly tests the implication.

The paper proceeds as follows. Section 2.2 describes the relevant institutional features of UI and details the way in which the policies studied in this paper are measured. Section 2.3 discusses the general empirical strategy employed in much of the paper. Section 2.4 presents the estimated effects of search policies on observable effort and reemployment. Section 2.5 examines the predicted effects of a search requirement in a standard search-and-matching model and a job-rationing model. Section 2.6 presents evidence on the efficacy of search

[1]The effects of these policies on nonclaimants or search-exempt workers is an important topic for future work.

requirements across labor market conditions. Section 2.7 discusses and concludes.

2.2 Institutional Setting

Search requirements are an important feature of UI systems even though they receive limited attention in the existing literature. Many states have implemented dramatic changes in these requirements over the past decade. I document the recent changes in search requirements using the documentation available to UI claimants at the time of their claims. I compare my coding of the policies to data on the number of job contacts actually reported by claimants who are audited and find a strong relationship between the published rules and the reported contacts.

2.2.1 Search Requirement History

Unemployment insurance, while governed by a set of federal guidelines under the Federal Unemployment Tax Act (FUTA), differs across states. In general, it provides compensation to full-time, permanent workers who lose their employment through no fault of their own. To be initially eligible, workers must meet thresholds for quarters of employment and earnings in a set period before a job separation. Benefit levels are determined by earnings over the covered period. In general, benefits can be paid for approximately 26 weeks, but the benefit duration is increased through both automatic and ad hoc extensions during periods of high unemployment via the Extended Benefits (EB) and Extended Unemployment Compensation (EUC) programs.

Unemployment insurance is affected by a well-known moral hazard problem: higher unemployment benefits raise the relative value of remaining unemployed, lowering the incentive to search for and accept new employment. Conversely, UI allows workers to smooth consumption over employment shocks (Gruber, 1997). While a second-best result can be reached by setting benefits to balance moral hazard against consumption-smoothing benefits (Chetty, 2006, 2008), policymakers have historically attempted to mitigate the problem by requiring demonstrable search effort and job offer acceptance from claimants.

Search requirements of one form or another have been an important part of UI systems since at least the 1980s. While the original UI system established in the Social Security Act of 1935 did not specifically require search from claimants, later additions to federal law called for claimants to demonstrate active search. In particular, federal law first demanded these requirements for claimants receiving benefits under the EB program (Anderson, 2001). Over the last few decades of the twentieth century, states variously implemented and eliminated search requirements for claimants of regular UI benefits (Klepinger et al., 2002). The 2000s and 2010s have been characterized by a steady increase in the strength of search requirements across many states. While most states already had wording in their statutes requiring that claimants be "able, available, and actively searching" for work, this language was added to the United States Code as a condition for state UI funding as part of the Middle Class Tax Relief and Job Creation Act of 2012.

The prototypical search requirement calls for claimants to contact some minimum number of potential employers each week. In some cases, these contacts are required to be in person, while some more general requirements simply disallow phone calls. The most general requirements simply ask claimants to contact employers in the way that is customary for their professions. In general, the same employer cannot be contacted again for a minimum number of weeks or unless there is reason to believe that another position has become available. Currently, virtually all states require claimants to at least track their contacts in a diary or work search log. Blank search logs are often provided to claimants with the rest of their UI documentation. However, there is considerable variation in how often these logs are checked by state workforce agencies. A steeper and increasingly common requirement is that claimants must report the details of their employer contacts at the time of making their weekly or biweekly claims. Claimants who are found to have not fulfilled their work search requirements will be deemed ineligible for the week and, potentially, disqualified from receiving future benefits.

While search requirements have become common across states, they do not apply to all workers. In particular, most states exempt claimants who find work through a union hiring hall. They are not required to make regular contacts with other employers, though a weekly minimum may be set on the number of times a claimant must contact the hiring hall. Workers who are on layoff and awaiting recall can also be exempt, though they must often have a definite recall date within a set number of weeks. Claimants who are participating in agency-approved training programs may also be exempt.

2.2.2 Existing Studies

Characteristics of UI systems are studied extensively in the existing literature, but search requirements themselves receive somewhat less attention. Borland and Tseng (2007) is the only other study of which I am aware that examines broad changes in work-search requirements outside the specific context of an experiment. The authors examine a job-search diary program in Australia shortly after implementation in the late 1990s. Due to a labor dispute involving unemployment caseworkers, some benefit offices did not enforce the requirement that claimants keep a job-search diary satisfying a particular number of employer contacts.[2] Those who kept the job-seeker diaries experienced shorter unemployment durations. However, part of the identification is generated from variation in the level of implementation between geographic regions and the study does not specifically try to estimate the implied effect of the policy under universal application. Further, workers with relatively poorer labor market options, as inferred from their recent unemployment histories, did not exhibit faster reemployment under the job-search diary regime. This finding in the Australian context is consistent with the idea that work search requirements are not effective in generating reemployment when workers are already constrained in their job market opportunities.

The two studies most relevant to the examination of search requirements in the US are Johnson and Klepinger (1994) and Klepinger et al. (2002). Johnson and Klepinger (1994)

[2] In general, this requirement was for eight contacts every two weeks.

describes the Washington Alternative Work Search Experiment, in which the 9,634 eligible UI claimants who applied for benefits in Tacoma between July 1986 and July 1987 were randomly assigned to different work search treatments. The first group was exempted from search requirements and was not even required to file biweekly continuing claims forms. This resulted in a 3.34 week increase in UI durations over the reference group's 14.48 average weeks. A second treatment group was assigned individualized work search requirements based on their circumstances. A third treatment group participated in an intensive job-search training workshop early in their employment spells. The individualized requirement group saw no change in benefits drawn while the workshop group drew, on average, half a week fewer benefits. The dramatic increase in UI durations for the first treatment group suggests that, in this context, work search requirements decrease UI claim durations. However, because the sweeping treatment effectively removed all costs of continuing to claim UI, the results likely overstate the effects of the search requirement alone.

The Maryland Unemployment Insurance Work Demonstration, as detailed in Klepinger et al. (2002), provides a sharper test of the direct experimental effects of work search requirements. During 1994, all new claimants at six randomly-selected Maryland UI offices were enrolled in the study. The experiment included treatment groups that faced increased search requirements, decreased search requirements, required participation in a job search workshop, and monitored work search. Both informed and uninformed control groups were included to test for Hawthorne effects. The simple changes in work search requirements are of greatest interest for the purposes of this paper. The decreased search treatment required no contacts as compared to Maryland's standard of two, while the increased treatment required four weekly contacts. The zero-contact group saw an increase of 0.36 weeks claimed over the control mean of 11.94, and the four-contact group saw weeks claimed fall by 0.72. While the results are less striking than those seen with the sweeping treatment in Washington, they suggest a distinct slope in required contacts. In the case of both experiments, though the effects are well-estimated in partial equilibrium, there is little scope for considering the general equilibrium effects of search policies. Even if the entire labor market were randomized into the experiments, which arguably happened in Tacoma or at each site in Maryland, only some of the claimants saw their work requirements change, and they were always counteracted by a treatment group changing in the opposite direction.

As these studies do not address general equilibrium issues, it may be important to consider them as in Davidson and Woodbury (1993), Lise et al. (2004), and Crépon et al. (2013). Both Davidson and Woodbury (1993) and Lise et al. (2004) consider the general effects of some manner of reemployment bonus. In both cases, a model that allows for general equilibrium effects is calibrated using the partial equilibrium results of an experiment. Davidson and Woodbury (1993) then discuss the implied equilibrium effects of the policy, while Lise et al. (2004) compare the model's predictions to an out-of-sample group before using the results to identify feedback in the policy. In both cases, partial equilibrium experimental results are at least partially reversed when implemented in general. Crépon et al. (2013) endeavors to explicitly measure spillover effects of a French job search assistance program through a two-level randomization process. Municipalities were first randomized into groups that would vary the share of unemployed that would be treated with the program and then the appropriate percentage of individuals within each municipality were randomly treated. They find spillover effects in markets where the treated individuals compete for jobs mostly with other similar individuals and when labor market conditions are poor. Overall, this literature indicates that there is scope for analysis of policies in which general effects should be considered.

Another strand of the literature examines the effects of differential monitoring under constant search requirements. In two papers, McVicar (2008, 2010) uses the refurbishment of unemployment benefit offices in Northern Ireland as a source of variation in the monitoring of claimant search requirements. On the whole, his findings show that eliminating monitoring reduces the unemployment exit hazard and job entry hazard and increases the stock of claimants. If we interpret these findings as being equivalent to moving between a no-search requirement (when claimants are not monitored) and a standard search requirement (under standard monitoring), we would expect changes in search requirements to induce changes in unemployment exit and job entry in other contexts. However, experimental evidence from four US states in Ashenfelter et al. (2005) suggests that UI claimants complied with work-search requirement even under very limited monitoring regimes. Performing additional verification of reported work-search did not induce meaningful changes in the duration of unemployment claims.

2.2.3 Policy Measurement

Although state-specific search requirements are quite common, documentation of the policies is not readily available at most points in time. The DOL reported state search requirements in its annual *Comparison of State Unemployment Insurance Laws* through 1999, but it was determined that there was not enough cross-state variation at the time to justify continuing to include search requirements. Around the same time, Anderson (2001) performed a cross-sectional review of the standing search requirements, notably finding that some workforce agencies' publications suggested different rules from those listed in the DOL report. Until search requirements were added back into the *Comparison of State Unemployment Insurance Laws* in 2012, information on policies is largely limited to O'Leary (2004), who provided a cross-section of rules as reported in a survey of state workforce agencies. A handful of other papers mention the search requirements for individual states at various points in time over this period.

I overcome the lack of existing information on search policies by constructing new data from state workforce agency publications. All states make some documentation available to claimants, whether through stand-alone UI claimant handbooks or "frequently asked questions" brochures and web pages. Many of these instruct claimants on the exact search requirements they are expected to fulfill. Agency-provided work search logs, which have become more common in recent years, also often detail the rules that claimants are supposed to follow. Although most of these are not directly available online, many are archived through Internet crawler caches and others are available through university and state government libraries. Through these sources, I have collected all relevant and available documents published by workforce agencies between 2001 and 2014. I examine these documents for information on work search requirements and track within-state changes via document publication dates.

The rules implied by the agency publications generally correspond with the other available sources, but there are some discrepancies with DOL's *Comparison of State Unemployment Insurance Laws* and O'Leary (2004). For the sake of consistency, I defer to the agency publications throughout. I also argue that the rules as they are described to claimants in this information are the most relevant for measuring the policies. However, I cannot rule out that workforce agency staff provide different information in person. In Sections 2.2.4 and 2.4.1, I compare the rules recorded from agency publications to actual numbers of employer contacts reported by claimants in BAM data.

2.2.4 Changes in Policies

I group search policies by the number required weekly employer contacts. During the 2000s, various states have specifically required claimants to make between zero and five contacts. States that do not make these specific requirements fall into two groups. First, many states do not indicate an exact number of employer contacts. In general, these are the states listed as "no specific number" in *Comparison of State Unemployment Insurance Laws* and recorded in O'Leary (2004) as states in which claimants are instructed to follow the customs of hiring in their profession. Throughout this paper, I refer to these policies as "nonspecific" search requirements. A second group of six states provides search requirements that are in some way individualized for claimants. My research suggests that Arkansas, Colorado, Idaho, Missouri, Texas, and West Virginia have all had such "directed" search policies at some point in the 2000s.[3] I exclude these states from my analysis both because the determinants of the individual requirements are not always clear and because they generate cross-sectional within-state heterogeneity that makes analysis at the state level difficult.

Figure 2.1 displays the number of states with each policy from zero to five required employer contacts over the ten years from 2004 through 2013.[4] While there are a few state movements over the first three quarters of the displayed period, most of the increases in requirements take place in 2011 or later. The increases are also concentrated at the high end of the requirement distribution. While six states required at least three weekly employer contacts in 2004, 16 required three or more contacts by the end of 2013. There are 14 instances of increasing search requirements in the sample examined in this paper, 12 of which took place under the administrations of Republican governors. Common narratives surrounding the policy changes indicate that search requirements have been raised to get the unemployed back to work by increasing labor force attachment and more or less explicitly raising the burden of drawing UI benefits.

Figure 2.2 displays the number of employer contacts reported to the BAM program by audited UI claimants. The BAM program is designed to identify the sources of overpayments in the UI system. It accomplishes this task by randomly selecting UI claimants and thoroughly checking their monetary and nonmonetary eligibility for a given week. As part of the audit, claimants are asked to record their requisite employer contacts for the week, even if they are not generally required to explicitly report contacts. While the question of whether the recorded contacts satisfy the actual requirements is of interest, my first goal with this data is to see how many contacts claimants think they should be reporting. The seven different search policies examined in this paper are listed along the horizontal axis of Figure 2.2,

[3]Much of the documentation for Ohio appears to indicate that it should also fall into this group because workers' requirements have been individually communicated to claimants at the time of claiming. However, various unemployment information websites, newspaper articles, and the BAM data suggest that there is no cross-sectional variation in Ohio's requirements for those who are instructed to search. It appears that the individualized information is just whether claimants are subject to work search.

[4]Analogous graphs of the total number of claimants under each policy and the share of claimants under each policy appear in Appendix A.

with the number of state-months for each listed below. The bars above each policy number show the discrete empirical PDF of contacts reported by the $159,613$ claimants who were audited by BAM between 2001 and 2013 and were not union- or attached to an employer with a definite recall date. In general, the patterns suggest a strong relationship between the policies I have recorded and the BAM reports. For each of the specific policies, the required number makes up a plurality of the reports. It is easier to explain apparent overreports than underreports because claimants have an incentive to list extra contacts if they are concerned that some of their contacts will be deemed ineligible. Thus, the most striking deviations are for states with three-contact policies, which have over 40 percent of claimants reporting zero or one contacts.[5] On the whole, however, the BAM data are supportive of the policies as they are coded.

The relationship between BAM contact reports and state changes is displayed in Figure 2.3. For each of the 12 states with a policy change during the period available in the BAM data, the graphs indicate the policy (as indicated by the thick, dashed line) and the monthly average of reported contacts (the thin, solid line).[6] No dashed line is displayed for periods during which a nonspecific policy prevailed. A number of the changes appear only at the very end of the available BAM data, with Mississippi, Tennessee, and Rhode Island having only brief available data after the implementation of search policies. Florida, Hawaii, and South Carolina's graphs all suggest reasonably close adherence to their new policies after these states switched away from nonspecific rules. Likewise, North Dakota, Ohio, Pennsylvania, and Utah all generally follow changes in their explicit policies. While the data do seem to reflect the more recent change in Louisiana's requirements, they follow the earlier increased

[5]Further examination of this pattern indicates that almost half of these observations are from Massachusetts. Bay Staters almost exclusively reported zero contacts over the vast majority of the sample period even though Massachusetts' UI claimant handbooks very clearly require three contacts throughout. As several other sources also suggest a three-contact coding in Massachusetts, it appears that there is some idiosyncrasy with the state's BAM codings. Efforts to learn more about this from Massachusetts workforce authorities have been unsuccessful. Another 35 percent of these underreporters comes from Connecticut, Indiana, and Washington, each of which shows a small but noticeable minority of claimants reporting zero or one.

[6]Analogous graphs for other states are displayed in Appendix B. Graphs displaying the distribution of reported contacts for all states appear in Appendix C.

period somewhat less closely. The single policy change in Maine does not initially seem to show up in the data, but this is because deviations away from three contacts were fairly balanced between increases and decreases, having little effect on the average.

2.3 Primary Empirical Strategy

The primary analysis in this paper involves the estimation of regressions on aggregate and individual outcomes with state fixed effects and time fixed effects. In particular, I consider individual-level models of the form

$$y_{ist} = \alpha_0 + \boldsymbol{\alpha}_1 \mathbf{D}_{st} + \boldsymbol{\alpha}_2 \mathbf{x}_{ist} + \boldsymbol{\alpha}_3 \mathbf{X}_{st} + \gamma_s + \delta_t + \epsilon_{ist} \tag{2.1}$$

where y_{ist} is an outcome for individual i in state s and month t, and $\mathbf{D}_{st}$ is a vector measuring the prevailing search policy. Controls for individual characteristics are included in the vector $\mathbf{x}_{ist}$ and aggregate state-level controls are included in the vector $\mathbf{X}_{st}$. State and month fixed effects are given by γ_s and δ_t, respectively, and ϵ_{ist} is an idiosyncratic error term.

The coefficient vectors of interest are given by $\boldsymbol{\alpha}_1$. These coefficients estimate the difference in outcomes between state-months with differing search requirements. The estimates are identified by within-state variation in policies over time. In general, I parameterize the policies in two different ways. First, I estimate the effect of moving through the zero- to five-contact policies with a continuous linear measure of the number of required contacts. While this specification is simple in that it provides a single estimate of the effects of an additional contact, it constrains that effect to be constant as the number of contacts increases. In these regressions, I include nonspecific policies as a separate indicator and assign them a zero in the continuous measure. The coefficient estimate on the indicator indicates the estimated difference between zero-contact and nonspecific policies. Second, I estimate the effects of each policy individually by including a full set of indicators, one for each policy with zero-contacts omitted.[7] This specification is clearly more flexible, but suffers from relatively few observations for the zero and four-or-more contact policies.[8]

No matter the exact policy coding, consistent estimation relies on the assumption that there is no correlation between search policies and outcomes for the sampled respondents, conditional on the fixed effects and covariates. A primary source of concern is the possibility of policy endogeneity: states with deteriorating labor markets may implement policy increases in response. To test for this, I regress monthly state unemployment rates on indicators for months until a search requirement increase and state and time fixed effects. The indicators show no pretreatment rise in unemployment immediately before policy changes. Estimated leads a year in advance of policy changes do show statistically significantly higher unemployment rates, but these are followed by a downward trend to the policy change.

2.4 Evidence on Policy Effects

In this section, I examine the estimated effects of search requirements on a number of different outcomes using models broadly of the form described in Section 2.3. I first examine measures of search effort using data from the BAM program and the CPS. I then test for effects of search requirements on the reemployment hazard as measured through Cox proportional hazard models using CPS data. Finally, I examine the estimated effects on average UI claim durations from administrative claims data.

2.4.1 Evidence on Search Requirements Affecting Search Effort

Table 2.1 displays the estimates from regressions of the form of equation (2.1). The first two columns regress the number of contacts reported in the BAM data on measures of search

[7] Florida is the only state to implement a five-contact requirement, so I group it with the four-contact policies for the purposes of these indicators.

[8] I choose the zero-contact policies as the omitted group for these specifications because it seems most straightforward to compare more stringent policies to a simple zero-contact requirement. Using other omitted groups would generally reduce the statistical significance implied by the stars in the paper's tables. In the absence of another natural omitted group, I forgo providing tests of all pairwise comparisons and generally rely on the linear specification as a summary of the average differences.

policies, quantifying the effects shown in Figure 2.3. Columns (3) through (6) use different measures of genuine, acceptable employer contacts as the dependent variables. Columns (7) through (10) extend the analysis to more general measures of search methods and effort in the CPS.

2.4.1.1 Employer Contacts Reported to BAM

The number of employer contacts reported is regressed initially on a linear measure of required contacts with an indicator for nonspecific policies. Claimants living under non-specific policies are assigned a zero in the linear term. Thus, the estimate in the first row indicates the number of additional reported contacts associated with a one-contact increase in the requirement. The estimate in the last row indicates the conditional expectation of the difference between reports under a nonspecific policy and a zero-search policy.

A single additional required contact is estimated to increase reported contacts by 0.564 and claimants under nonspecific policies are estimated to report 0.848 more contacts than claimants under zero-search policies. The estimates reflect a number of patterns apparent in Figures 2.2 and 2.3. First, nonspecific policies are associated with some variation in number of reported contacts, but the average level is greater than that of the lowest specified search policies. Second, the estimated effect of increasing a search policy by one is statistically significantly less than one, though much greater than zero. This likely reflects both measurement differences between the BAM data and the policies[9] and less-than-full adjustment to policies by claimants. Note that this latter effect does not imply anything about noncompliance. Because the share of claimants exceeding the requirement decreases as the policy increases, the estimated effect of the policy should be attenuated below one.

The second column of Table 2.1 makes some of the source of this difference clear. When estimated with individual indicators, the number of reported contacts generally increases in search requirement, though nonmonotonically. Here, the relatively low levels of reported

[9] See discussion of discrepancies in Section 2.2.4.

contacts associated with the three-contact policy are apparent.

The estimates in columns (1) and (2) indicate that higher requirements are generally associated with more reported contacts. However, it is initially unclear whether these reports reflect actual contact with employers or fake contacts by claimants trying to appear as though they have satisfied the requirement. To separate these two possibilities, I use data on the results of BAM audits themselves. As part of determining claimant eligibility, BAM auditors attempt to verify that the employer contacts were made and were legitimate under state rules. Ultimately, employer contacts are identified as acceptable, unacceptable, or not verified. A large portion of the contacts (42%) fall into this last category, indicating that there was not enough information to rule the contact as acceptable or unacceptable.[10] Of those that can be verified, approximately 88% of the contacts in the sample are found to be acceptable.

Columns (3) and (4) of Table 2.1 repeat the same specifications as the first two columns but use the number of contacts that are *not* found to be *un*acceptable as the dependent variable. This measure is an upper bound on the number of contacts that would be found to be acceptable if all contacts could be verified one way or the other. Columns (5) and (6) use a lower bound: the dependent variable is the number of reported contacts that are found to be acceptable. In either case, the coefficient on the linear measure remains positive and significant. While only half of contacts are unverified, the linear estimate for the verified, acceptable contacts in column (5) is attenuated rather more than half as compared to column (1). This suggests that, on average, the additional contacts provided under higher search requirements are found to be unacceptable at a higher rate, but the total measured search effort is increasing regardless.

2.4.1.2 CPS Search Effort Proxies

A limitation of the analysis based on reported contacts is that, even if all contacts are genuine, the differences across policies may simply represent differences in reporting alone.

[10]For the purposes of determining eligibility for UI benefits, the audits generally treat these contacts as acceptable. Payments are not ruled as improper because of unverified contacts.

That is, the above results could be generated by all claimants actually contacting well more than the requirement and only reporting the required number. Therefore, I turn to the CPS for additional measures of search effort that are not driven by directly by reporting requirements.

Columns (7) and (8) of Table 2.1 demonstrate that search requirements broadly increase the number of job-search methods used by basic monthly CPS respondents between 2003 and 2013. Unemployed CPS respondents are asked to indicate what job-search methods they have used in the past four weeks, with twelve possible methods. These regressions estimate the policy effects on the total number of indicated methods among those respondents who indicate they were laid off from or otherwise lost their most recent job. This is intended to capture the population of the likely-UI-eligible unemployed. It is important to note that the population differs from that sampled by the BAM data, which is UI claimants. Measuring the usage of the 12 search methods described in the CPS provides a reasonable proxy for search effort. Because job-seekers are likely to diversify the ways in which they look for jobs as they put more effort into search, these measures provide readily-available proxies for effort. Table 2.2 reports the prevalence of each job search method in the sample. While contacting employers and sending out resumes are each used by approximately 40 percent of respondents, no individual method is used by more than half of the sample, and there is considerable variation in the prevalence of each method.

On a sample average of approximately 1.8 methods, the linear estimate suggests that an additional required contact increases the number of search methods used by approximately 0.04. The individual policy indicators show that this effect is largely concentrated in a jump between a zero-search policy and a one-contact policy. The estimated effects increase monotonically up to four required contacts, though the estimates are not statistically different from each other.

Columns (9) and (10) of Table 2.1 display the effects on whether respondents made use of one particular search method: directly contacting or interviewing with employers. This method was chosen because it appears to correspond most directly to the definitions of employer contacts in search requirements. The linear estimate is again positive, indicating that an additional contact requirement raises the probability of contacting employers directly by 1.3 percentage points. The specification with indicators shows that the effect is largely driven by difference between zero-search policies and other policies. All of the estimates, including the one for nonspecific policies, put the effect between 0.06 and 0.12. In general, this result should not be surprising. If respondents in states with one-contact policies are following the requirements and making at least one employer contact, there should not be much effect on this probability from adding additional contacts.

2.4.2 Evidence on Search Requirements Affecting Reemployment

I next show that search requirements have no impact on the duration of unemployment spells, an outcome of more fundamental interest to economists and policymakers. I first show that search requirements have small effects on the reemployment hazard, which is one of the most important outcomes for the overall welfare of UI claimants. I estimate Cox proportional hazard models with the same general form of controls as the linear models of the previous Section. I estimate the models on a subset of monthly CPS respondents who can be linked across months. For the unemployed each month, failure is defined as reemployment the following month. I allow the hazard to vary proportionally in month, year, state, education, two-digit occupation occupation codes, two-digit industry codes, race, ethnicity, and sex. Estimates from these models are reported in Table 2.3. The estimates are all greater than one, indicating faster reemployment, and are largely increasing across the increasing search requirements, but they are never statistically significant. The linear specification suggests that the hazard of leaving unemployment each month is about one percent higher for each additional required contact. While the estimates for some of the individual policies are larger, it is difficult to distinguish them from no effect.

2.4.3 Evidence on Search Requirements Affecting UI Claim Durations

I next turn to measures of the duration of UI claims as reported in administrative data. The data are generated from DOL quarterly reports of the total weeks paid over the previous year divided by the number of first payments over that time period. Though this definition makes the outcome a moving average, the measure is advantageous because it is more directly comparable to the estimated effects of search requirements from the experimental literature. Given the aggregate nature of the outcome, I simply regress these average durations on the policies and fixed effects in state and time. I lag the measured policy changes by two quarters in an effort capture the point at which approximately half of the new claimants and weeks drawn for each observation are under the prior policy and the new policy.

The estimated effect from the linear specification is the first estimate reported in Table 2.4. It suggests that an increase of one required contact lowers average number of weeks drawn by one eighth of a week. The estimates from the indicators in the second column exhibit large standard errors, though all the point estimates are negative. The point estimates themselves are, in some cases, of quite meaningful magnitude. The large standard errors, emphasized by the rejection of joint F-tests, highlight the limitations of this data for this estimation.

In principle, the number of weeks claimed can be estimated from the BAM data as well. Unfortunately, because BAM only samples ongoing spells and not completed spells, the average length of completed spells must be inferred from the distribution of randomly-sampled ongoing spells. This can be solved through a nonlinear least squares problem, but despite the many observations in BAM, the resulting estimates are also extremely imprecise. In ongoing work, I am attempting to make this procedure more efficient and seeking microdata better suited to estimating claim durations. Ultimately, it is difficult to say much with certainty about the exact effects on unemployment claim durations. Regardless, the amount of time people actually spend unemployed, as shown in the CPS proportional hazard models, is estimated relatively precisely and shown to be affected very little on average.

2.5 A Model of Search Requirements

While there are some basic empirical results apparent from testing the effects of search requirements on search effort and unemployment duration, I turn to a general search-and-matching model to interpret how we might expect search requirements to operate in equilibrium more generally. The model includes a measure of technology, which in general could be used to drive business cycles. However, for the purposes of this exposition, I assume technology is fixed and I examine comparative statics across different equilibria. Thus, from the perspective of the model's agents, there is no uncertainty over technology, wages, firm size, or aggregate measures of the labor market.

I begin by presenting the general features of the model. I then find the equilibrium conditions under two alternative wage-setting mechanisms. The first allows wages to be flexibly bargained. The second imposes wage rigidity that prevents the market from clearing under some circumstances. In particular, it allows the market to ration jobs when productivity is low and to exhibit unemployment beyond that caused by search frictions (Michaillat, 2012).

2.5.1 Environment

The model takes place in discrete time. At the beginning of each period t, unmatched workers search for jobs and hires are made. Production then takes place and wages are paid. Matches are then exogenously destroyed and the period ends. Firms and workers both have discount factor β. At the end of each period, a share λ of existing employment matches are exogenously destroyed. Matches occur between the measure of unemployed workers, u_t, who exert average search effort s_t, and posted vacancies, v_t, according to a constant returns to scale (CRS) matching function

$$m_t = m(s_t u_t, v_t) \tag{2.2}$$

which is increasing in both of its arguments. Given the CRS assumption, matches per vacancy can be expressed as a function of average search effort, s_t, and labor market tightness,

$\theta_t = \frac{v_t}{u_t}$:

$$\frac{m_t}{v_t} = m(s_t \frac{u_t}{v_t}, 1) = q(s_t, \theta_t). \tag{2.3}$$

An individual worker exerting s_{it} efficiency units of search is providing $\frac{s_{it}}{s_t u_t}$ of total search effort and receives that fraction of the matches:

$$\frac{s_{it}}{s_t u_t} m_t = \frac{s_{it}}{s_t} m(s_t, \frac{v_t}{u_t}) = \frac{s_{it}}{s_t} f(s_t, \theta_t). \tag{2.4}$$

As in Pissarides (2000), I consider only symmetric Nash equilibria in which all workers exert average search effort ($s_{it} = s_t$). Thus, the transition probability for the representative worker is given by $f(s_t, \theta_t)$. Workers take the variables s_t and θ_t as given, so the derivative with respect to an individual's search effort is $\frac{1}{s_t} f(s_t, \theta_t)$. The vacancy and worker transition probabilities are related through $f(s_t, \theta_t) = \theta_t q(s_t, \theta_t)$. In equilibrium, flows into and out of unemployment are balanced, defining the standard Beveridge curve:

$$u_t = \frac{\lambda}{\lambda + \theta_t q(s_t, \theta_t)}. \tag{2.5}$$

2.5.1.1 Workers

The value of being employed at the time of production is given by

$$W_t = w(N_t, a_t) + \beta[(1 - \lambda) W_{t+1} + \lambda S_{t+1}], \tag{2.6}$$

where $w(N_t, a_t)$ is the wage, which is potentially a function of technology (a_t) and the number of workers employed by the firm (N_t), and S_t is the value of being an unemployed job-seeker at the beginning of a period. The value of being unemployed during the time of production is given by

$$U_t = b + \beta S_{t+1}, \tag{2.7}$$

where b is the flow utility received by the unemployed. The value of searching for a job at the beginning of a period is given by

$$S_t = -\psi(s_{it}) + \tfrac{s_{it}}{s_t} f(s_t, \theta_t) W_t + (1 - \tfrac{s_{it}}{s_t} f(s_t, \theta_t)) U_t, \tag{2.8}$$

where ψ is the disutility of search effort, which is assumed to be increasing and convex in its argument. If workers are free to choose search intensity, then their choice will satisfy the first order condition given by

$$\psi'(s_t) = \frac{f(s_t, \theta_t)}{s_t} [W_t - U_t]. \tag{2.9}$$

Combining equations (2.6) and (2.7), we have

$$W_t - U_t = w(N_t, a_t) - b + \beta[(1 - \lambda)(W_{t+1} - S_{t+1})]. \tag{2.10}$$

Using the value of S_t as indicated by equation (2.8) evaluated at symmetric equilibria where $s_{it} = s_t$, this worker surplus becomes

$$W_t - U_t = w(N_t, a_t) - b + \beta(1 - \lambda) \left[\psi(s_{t+1}) + (1 - f(s_{t+1}, \theta_{t+1}))[W_{t+1} - U_{t+1}]\right]. \tag{2.11}$$

2.5.1.2 Firms

Firms use inputs of labor, N_t, and technology, a_t, to produce output via the production function $F(N_t, a_t)$. The production function is initially quite general, though I later parameterize it to have diminishing marginal product of labor, which is a necessary condition for the model to exhibit job rationing (Michaillat, 2012). The value of a firm entering period t

with $(1-\lambda)N_{t-1}$ employees remaining from the previous period is given by

$$\Pi((1-\lambda)N_{t-1}) = \\ \max_{N_t} F(N_t, a_t) - w(N_t, a_t)N_t - \frac{ca_t}{q(s_t, \theta_t)}[N_t - (1-\lambda)N_{t-1}] + \beta\Pi((1-\lambda)N_t), \quad (2.12)$$

where $N_t - (1-\lambda)N_{t-1}$ is hires made during the matching period. Firms make hires by posting vacancies at cost ca_t. Each of these vacancies yields $q(s_t, \theta_t)$ hires. Thus, the firm posts $1/q(s_t, \theta_t)$ vacancies to make one additional hire, and the cost of that hire is given by $\frac{ca_t}{q(s_t,\theta_t)}$.

The first order condition on N_t is

$$F_N(N_t, a_t) - w(N_t, a_t) - w_N(N_t, a_t)N_t - \frac{ca_t}{q(s_t, \theta_t)} + \\ \beta(1-\lambda)\Pi_N((1-\lambda)N_{t-1}) = 0 \quad (2.13)$$

Where N subscripts on functions indicate the derivative with respect to the argument N. The marginal value of an additional worker carried into the next period is given by

$$\Pi_N((1-\lambda)N_t) = \frac{ca_{t+1}}{q(s_{t+1}, \theta_{t+1})}, \quad (2.14)$$

so the first order condition in equation (2.13) can be written as

$$w(N_t, a_t) + w_N(N_t, a_t)N_t + \frac{ca_t}{q(s_t, \theta_t)} = F_N(N_t, a_t) + \beta(1-\lambda)\frac{ca_{t+1}}{q(s_{t+1}, \theta_{t+1})}. \quad (2.15)$$

That is, firms hire until the marginal costs equal the marginal benefits. The costs include the marginal worker's wage, the change in wages for all other workers, and the cost of posting the marginal vacancies. The benefits include the marginal production of the worker and savings on the following period's hiring costs.

2.5.2 Equilibrium under Bargained Wages

I first consider flexible wages determined by Stole and Zwiebel (1996) bargaining as in Elsby and Michaels (2013) and Michaillat (2012). In this bargaining environment, after firms and workers have matched, they bargain over the marginal surplus produced by the match. I denote the firm's marginal surplus after hiring costs are sunk by $J(N_t)$, where

$$J(N_t) = F_N(N_t, a_t) - w(N_t, a_t) - w_N(N_t, a_t)N_t + \beta(1-\lambda)\frac{ca_{t+1}}{q(s_{t+1}, \theta_{t+1})}. \tag{2.16}$$

If the firm sets its choice of employment optimally, as in equation (2.15), then this marginal surplus is also equal to the cost of making a hire:

$$J(N_t) = \frac{ca_t}{q(s_t, \theta_t)}. \tag{2.17}$$

The Stole and Zwiebel (1996) game implies that, for a worker's bargaining weight η, the wage satisfies

$$(1-\eta)[W_t - U_t] = \eta J(N_t). \tag{2.18}$$

If wages are assumed to be bargained the same way in all periods, then by equations (2.17) and (2.18),

$$W_{t+1} - U_{t+1} = \frac{\eta}{1-\eta}\frac{ca_{t+1}}{q(s_{t+1}, \theta_{t+1})}, \tag{2.19}$$

which can be plugged into equation (2.11) to obtain

$$\begin{aligned} &W_t - U_t = \\ &\qquad w(N_t, a_t) - b + \beta(1-\lambda)\left[\psi(s_{t+1}) + (1 - f(s_{t+1}, \theta_{t+1}))\tfrac{\eta}{1-\eta}\frac{ca_{t+1}}{q(s_{t+1}, \theta_{t+1})}\right]. \end{aligned} \tag{2.20}$$

Using the surpluses as in equations (2.16) and (2.20) in the wage condition of equation (2.18) gives the wage in the form of a differential equation:

$$w(N_t, a_t) = \\ \eta \left[F_N(N_t, a_t) - w_N(N_t, a_t)N_t + \beta(1-\lambda)ca_{t+1}\theta_{t+1}\right] + \\ (1-\eta)\left[b - \beta(1-\lambda)\psi(s_{t+1})\right]. \quad (2.21)$$

Assuming the production function is given by $F(N_t, a_t) = a_t N_t^{\alpha}$, the wage equation becomes

$$w(N_t, a_t) = \\ \eta \left[\frac{a_t \alpha N_t^{\alpha-1}}{1-\eta(1-\alpha)} + \beta(1-\lambda)ca_{t+1}\theta_{t+1}\right] + (1-\eta)\left[b - \beta(1-\lambda)\psi(s_{t+1})\right]. \quad (2.22)$$

This can be plugged into equation (2.15) to remove wages from the firm's employment decision condition:

$$(1-\eta)\left[\frac{\alpha a_t N_t^{\alpha-1}}{1-\eta(1-\alpha)} - b + \beta(1-\lambda)\psi(s_{t+1})\right] - \\ \beta(1-\lambda)ca_{t+1}\left[\eta\theta_{t+1} - \frac{1}{q(s_{t+1}, \theta_{t+1})}\right] - \frac{ca_t}{q(s_t, \theta_t)} = 0. \quad (2.23)$$

In equilibrium, the wage condition of equation (2.18) and the firm's marginal surplus in equation (2.17) can be used to rewrite the condition on optimal search effort from equation (2.9):

$$s_t \psi'(s_t) = \frac{\eta}{1-\eta}\theta_t c a_t. \quad (2.24)$$

Firms open vacancies, increasing N_t and θ_t, until equation (2.23) is satisfied at an s_t-θ_t pair that also satisfies equation (2.24). The steady state equilibrium is defined by the point where this job creation curve intersects the Beveridge curve given by equation (2.5). Such an equilibrium is illustrated by the solid lines in Figure 2.4. For lower levels of technology, the value of vacancies is lower for any given level of unemployment and worker search effort because filled jobs are less productive. Thus, equation (2.23) is satisfied at a lower level of vacancies and the job creation curve rotates down, as in the dashed line of Figure 2.4.[11]

2.5.3 Equilibrium under Rigid Wages

I compare the flexible wage-setting environment of the previous section to one in which rigid wages are set set via a reduced form schedule as in Michaillat (2012) and Blanchard and Galí (2010). That is, I assume that wages follow a schedule given by

$$w(N_t, a_t) = \omega a_t^{\gamma} \tag{2.25}$$

where $\omega \in [0, 1]$ is a measure of wage flexibility. Under this assumption and Cobb-Douglas production, the firm's employment condition simplifies to

$$\omega a_t^{\gamma-1} + \frac{c}{q(s_t, \theta_t)} = \alpha N_t^{\alpha-1} + \beta(1-\lambda)\frac{c}{q(s_{t+1}, \theta_{t+1})}\frac{a_{t+1}}{a_t}. \tag{2.26}$$

Workers' chosen search intensity can be determined using equations (2.9) and (2.11). While next period's search intensity and job finding rate can used to create a sufficient statistic for next period's worker surplus ($W_{t+1} - U_{t+1}$), it is more convenient to consider steady state equilibria. That is, in steady state with $a_t = a_{t+1}$, the change in technology disappears from the end of equation (2.26). Further, $W_t - U_t = W_{t+1} - U_{t+1}$ and $s_t = s_{t+1}$, so optimal search intensity satisfies

$$s_t \psi'(s_t) = f(s_t, \theta_t)\frac{\omega a_t^{\gamma} - b + \beta(1-\lambda)\psi(s_t)}{1 - \beta(1-\lambda)(1 - f(s_t, \theta_t))}. \tag{2.27}$$

If technology is high enough, the rigidity of the wage schedule does not affect employment. If marginal productivity at $N = 1$ exceeds the wage, then the job creation curve will intersect the origin. This is again as in the solid upward-sloping line of Figure 2.4. For some lower

[11] I assume throughout that even under low technology levels, marginal productivity never falls below $b - \beta(1-\lambda)\psi(s')$. That is, there is always surplus to be gained from employment.

levels of technology, workers beyond some employment level will be less productive than the wage. Thus, the wage rigidity moves the job creation curve away from the origin: employment would not increase beyond some level even if the cost of vacancy-posting were eliminated. This scenario is illustrated by the dotted job creation curve at the right of Figure 2.4.

2.5.4 Partial Restrictions on Search Effort

I first discuss the effects of increasing the search effort of an atomistic worker or group of workers. That is, what is the effect of exogenous increases in search intensity that do not move the economy to a new equilibrium? In particular, suppose an unemployed worker is constrained to set an individual search effort higher than the economy-wide equilibrium, $s_{it} > s_t$. The effect on unemployment duration is straightforward, as it will decrease by $\frac{s_t}{s_{it}}$:

$$\frac{s_t}{s_{it} f(s_t, \theta_t)} < \frac{1}{f(s_t, \theta_t)}. \tag{2.28}$$

This operationalization is meant to approximate the effects of raising the search requirement for a small group of workers in a randomized controlled trial. Such searchers can find faster reemployment at the (possibly imperceptible) expense of other searchers in the market.

2.5.5 Equilibrium Restrictions on Search Effort

In considering the effects of a search effort restriction on the equilibrium, it is first important to note when such a restriction will bind. Under either wage-setting regime, optimal search effort, given by equations (2.24) and (2.27) can be shown to be an increasing function of labor market tightness.[12] Therefore, as shown in Figure 2.5, in u-v space, search is constant along rays from the origin and is increasing as the rays rotate to the northwest. If an economy-wide floor is put on search effort at $\underline{s}$, it will affect behavior below some ray $\theta(\underline{s})$.

[12] Shimer (2004) presents a model in which search effort is instead countercyclical and presents supporting evidence using a measure of CPS search methods. Gomme and Lkhagvasuren (2013) review the research on this topic and argue both that controlling for spell duration reverses this result and that, more generally, the weight of the evidence is behind procyclical search effort.

At points below this ray, search is no longer defined by equation (2.24) or equation (2.27) and is set at $\underline{s}$. Because this is an increase in search effort, the Beveridge curve shifts in as displayed in Figure 2.6. Any job creation curves also shift up under either wage-setting regime. This is apparent from equations (2.23) and (2.26), as $q(s_t, \theta_t)$ rises for any given θ_t. The additional search effort on the part of workers lowers the cost of filling a vacancy, and more vacancies are posted.[13]

While the effects on equilibrium depend on a number of factors, some potential outcomes are illustrated in Figure 2.7. In all cases, the firm-side response to increased search effort should serve to further lower unemployment beyond what would be expected if firms did not respond endogenously. However, the impact on unemployment depends on the relative positions of the Beveridge and job creation curves and on the ability of the economy to absorb new jobs. If a search requirement is implemented in a market where the Beveridge and job creation curves are flat, then small horizontal movements can bring about large changes in unemployment, as illustrated in the "Weak Economy, Flexible Wages" curves in Figure 2.7. If however, there is little scope to increase employment because of wage rigidities, then a search requirement may have little impact on equilibrium unemployment, as in rigid wage curves of Figure 2.7. On the whole, the predictions for how search requirements should affect the functioning of a slack labor market are initially unclear.

2.6 Differential Effects by Market Conditions

The theory in Section 2.5 suggests that the efficacy of search requirement policies may vary considerably across labor market conditions. If unemployment is always driven by matching frictions, then search requirements may be very effective in reducing unemployment during recessions due to all the slack in the labor market. If, on the other hand, jobs are rationed in recessions because of wage rigidities, then search requirements will have little

[13] It is also the case that the additional disutility of search lowers a worker's threat point under the flexible wage regime and decreases the bargained wage the firm must pay.

ability to reduce unemployment. Therefore, at the level of the labor market, I test whether search requirements are effective at reducing unemployment, allowing the effects to vary according to labor market strength.[14]

One strategy for examining differential effects across market conditions would be to simply interact the policy measures with the unemployment rate, a commonly-chosen measure of labor market strength. However, in the current analysis, it is also the outcome of interest. Therefore, I use two plausibly-exogenous measures of market strength. First, I separate labor markets by lagged unemployment, testing to see whether the implementation of a search requirement differentially impacts the equilibrium in labor markets that were weaker before the policy change. Second, I use a shift-share measure of employment-by-industry to proxy local labor market demand with national employment trends.

2.6.1 Equilibrium Effects by Lagged Unemployment

Within states that implement changes to their search requirements, I explicitly examine differences across labor markets by their conditions before implementation. If search requirements are more effective when the labor market is weak, then unemployment should fall relatively more for markets with initially high unemployment. If search requirements are not as effective in weak markets, then unemployment rates should fall relatively more in the initially low unemployment locations. I implement the test via a specification of the form

$$u_{mt} = \alpha L_m \mathbb{1}(t > t^*) + \gamma_m + \delta_t + \varepsilon_{mt}, \tag{2.29}$$

where u_{mt} is the unemployment rate in market m at time t, L_m is an indicator for being a low-unemployment market prior to implementation, which is multiplied by an indicator function for time t being post-implementation. Market and time fixed effects are given by γ_m and δ_t. Estimates of the coefficient α indicate the average change in unemployment differ-

[14] Effects of the same sign are generated by performing similar tests at the individual level using the CPS duration data. However, the results are relatively imprecise and the theory suggests that an analysis of the labor market as a whole is reasonable.

ential between high- and low-unemployment markets following implementation of a search requirement increase. Negative estimates of α show that low-unemployment markets had relatively even lower unemployment after implementation. Positive estimates of α show that low-unemployment markets had relatively higher unemployment after implementation. The former suggests that search requirements are relatively more effective in low unemployment markets, while the latter suggest that they are relatively more effective in high unemployment markets.

I estimate regressions of the form of (2.29) using data from the Local Area Unemployment Statistics (LAUS) program. LAUS data are useful in that they provide monthly estimates of unemployment and labor force participation at relatively disaggregated levels. A disadvantage of the data are that they are partially constructed by Bureau of Labor Statistics' (BLS) models that combine data from a number of different sources. Unemployment estimates at the level of the labor market are constructed from data on current and past UI claims and estimates of new entrants and reentrants into the labor force.[15] Entrants and reentrants are estimated using state-level estimates of these groups modeled from current and past CPS data, which are then divided into market areas based on the relative age distributions of the markets. Each market is assigned new entrants in proportion to its share of the state's age 16–19 population. Reentrants are assigned based on the market's share of the population ages 20 and over.

In essence, the process combines high-quality data on local unemployment claims, which are not always otherwise available, with an averaged apportioning of entrants. The estimates may be biased if state policy changes are correlated with changes in the number of state entrants and reentrants and these groups are differently assigned to markets defined as high- or low-unemployment before implementation. It is difficult to explicitly control for these concerns because the BLS does not reveal the exact models used in creating LAUS data. I therefore simply proceed using the LAUS data as they are published.

[15] This description draws heavily on the LAUS estimation methodology details found at www.bls.gov/lau/laumthd.htm.

Table 2.5 reports the regression results using data from 2001 to 2014 and four states that increased their search requirements: Florida, Ohio, Pennsylvania, and Utah. Within each state, I divide the micropolitan and metropolitan statistical areas by their unemployment rates pre-implementation using two measures. The first row of results are generated defining low-unemployment areas as those that had average unemployment rates below the state median between 2001 and the implementation of the policy change. The second row defines low-unemployment areas as those that had average unemployment below the state median in the year before the policy change. The former identifies areas that are consistently low in unemployment, while the latter identifies those that may have been transitively so. In both rows of Table 2.5, the estimates are universally negative, indicating that low-unemployment markets do relatively better after a search requirement increase under either definition. It is striking that the estimates are more negative when using the transitory definition (second row), as one might expect areas that are briefly low-unemployment to regress to the mean over time.

2.6.2 Equilibrium Effects by Industry Shift-Share

I next proxy for labor market strength using a shift-share measure as in Bartik (1991). This method uses changes in the national distribution of employment-by-industry to proxy for local labor demand in individual markets with different industry mixes. Intuitively, if employment in manufacturing declines nationally, one would expect markets that have larger shares of workers in manufacturing to see larger employment declines. If the national movements in industry employment are exogenous to an individual market's local labor conditions, then the measure is a plausibly exogenous measure of local labor demand.

In practice, I calculate the proxy using predicted employment in market m at time t as

$$\hat{E}_{mt} = \sum_{k} \frac{N_{kt}}{N_{kb}} E_{mkb}, \tag{2.30}$$

where b is a chosen baseline date and k indexes industries. N_{kt} is national employment in industry k at time t, N_{kb} is national employment in the industry at baseline, and E_{mkb} is employment in market m in industry k at baseline. Thus, $\hat{E}_{mt}$ is predicted using baseline employment in each industry (E_{mkb}) multiplied by national growth in that industry since the baseline ($\frac{N_{kt}}{N_{kb}}$) and summed across industries. For the purposes of the regressions that follow, I use predicted employment growth,

$$\hat{G}_{mt} = \frac{\hat{E}_{mt} - E_{mb}}{E_{mb}}, \tag{2.31}$$

which has the advantage of being the same scale for all markets.

While this measure can be used as an instrument, I interpret it directly and estimate reduced-form regressions given by

$$u_{mt} = \alpha_0 + \alpha_1 \hat{G}_{mt} + \boldsymbol{\alpha}_3 \mathbf{D}_{st} + \boldsymbol{\alpha}_4 \mathbf{D}_{st} \hat{G}_{mt} + \gamma_m + \delta_t + \epsilon_{mt}. \tag{2.32}$$

The estimates of α_4 indicate the differential effect of a search policy when predicted employment growth is 100 percent higher. Negative estimates for these coefficients suggest that search policies do more to lower unemployment when labor demand is relatively stronger.

I calculate the shift-share proxy using the Quarterly Workforce Indicators (QWI) for all available micropolitan and metropolitan statistical areas. The QWI provide quarterly estimates of various employment stocks and flows using Longitudinal Employer-Household Dynamics (LEHD) microdata. The QWI are sourced with high-quality administrative data from a number of sources, but have some noise infused to protect individual confidentiality. For the purposes of constructing the shift-share measure, I use employment counts in two-digit North American Industry Classification System (NAICS) sectors at the level of the statistical areas. I use a baseline of the first quarter of 2004 because it is before most of the policy changes of interest, but is at the point at which almost all states appear in the

QWI.[16] The following estimates are robust to other choices of baseline quarters, including using each quarter's lag as its baseline.[17]

The first row of Table 2.6 indicates a strong relationship between the shift-share measure and the local unemployment rate, again taken from LAUS data. In these results, both the unemployment rate and $\hat{G}_{mt}$ are scaled as shares of one (e.g., the unemployment rate is 0.05 and $\hat{G}_{mt}$ is 1.01). While there is some variability in the interaction estimates, the trend is summarized by the coefficient on the linear interaction term in the second row of column (1). A one percentage-point increase in predicted growth strengthens the decrease in the unemployment rate from each additional required contact by 0.0003. Alternatively, increasing from one contact to five should lower unemployment by 0.15 percentage points for each additional percentage point of expected growth. While the pattern in column (2) is not monotonic, the general trend across the first three policies tells the same story. The relative outlier estimate on the interaction with four-plus policies (0.016) turns out to be driven by the five-contact policy in Florida. Dropping Florida from this analysis results in a negative estimate for the four-plus policies of the same magnitude as that for the three-contact policies. As one would expect, dropping this nonmonotonic outlier from the regression in column (1) increases both the magnitude and the significance of the linear estimate. The reason for Florida appearing as an outlier is not clear. In the absence for a compelling reason to treat it differentially, I leave it included in the main results presented here.

2.7 Conclusion

The wave of increases in search requirements for UI claimants does not appear to have dramatically increased the speed at which claimants return to work. Although some measures of search intensity suggest that stronger requirements have effects on search effort, there is no

[16]Not all geographies are available in the QWI data. Massachusetts is missing entirely and no estimates are available for New England City and Town Areas (NECTAs) in other states. Washington, DC is not available until 2005 and is also excluded from the analysis.

[17]I do not present results using individual lags because of concerns that industry mixes and employment could be affected by the policies of interest when they change.

evidence that this is translated to meaningfully faster reemployment in microdata. I motivate additional analysis of the labor market with a general search-and-matching model. In some cases, such a model predicts that the general equilibrium effects of search requirements will compound the search effort effects alone: firms will open more vacancies as searchers exert more effort. However, if the market is not fully flexible, as in rigid-wage, job-rationing models, search requirements may have little scope to improve labor market conditions. In particular, short-term partial equilibrium experiments may not generalize to fully-implemented and permanent policies in general equilibrium.

The limitations of some active labor market policies in recessions are strongly suggested in the job-rationing analyses of Michaillat (2012) and Landais et al. (2010), particularly those that aim to counter search-and-matching frictions. The evidence in this paper provides some empirical support for those limitations. Although UI search requirements may be unambiguously effective for small groups of workers in partial equilibrium, their effectiveness appears muted in general equilibrium in weak labor markets. While policymakers may wish to continue raising search requirements as a way of increasing the burden of UI claiming, these changes are unlikely to improve outcomes for UI systems through faster reemployment in recessions.

Table 2.1: Evidence on Efficacy of Search Requirements

Data:	BAM		BAM		BAM		CPS		CPS	
Outcome:	Number of Reported Contacts		Acceptable Contacts (upper bound)		Acceptable Contacts (lower bound)		Number of Search Methods		Contacted Employers?	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
# contacts	0.564		0.559		0.218		0.037		0.013	
	(0.121)		(0.129)		(0.071)		(0.008)		(0.005)	
1 contact		0.651		0.544		1.332		0.108		0.079
		(0.182)		(0.194)		(0.135)		(0.037)		(0.012)
2 contacts		1.610		1.438		1.680		0.117		0.072
		(0.324)		(0.359)		(0.217)		(0.051)		(0.016)
3 contacts		1.108		0.988		1.534		0.138		0.063
		(0.100)		(0.106)		(0.042)		(0.018)		(0.008)
4+ contacts		3.086		2.947		2.171		0.204		0.104
		(0.371)		(0.407)		(0.224)		(0.069)		(0.021)
Nonspecific	0.848	0.698	0.903	0.637	0.394	1.401	0.122	0.151	0.059	0.097
	(0.420)	(0.165)	(0.443)	(0.168)	(0.270)	(0.145)	(0.045)	(0.065)	(0.022)	(0.018)
N:	126,857	126,857	126,844	126,844	104,922	104,922	175,689	175,689	175,689	175,689
Sample Mean:	1.819	1.819	1.686	1.686	0.930	0.930	1.797	1.797	0.422	0.422
F-stat, all=0:	14.8	47.6	12.3	46.5	8.8	311.2	10.8	19.9	3.9	40.6
p-value:	0.000	0.000	0.000	0.000	0.001	0.000	0.000	0.000	0.029	0.000

Notes: Huber/White/sandwich standard errors clustered at the state level appear below estimates in parentheses. Columns (1) and (2) display the estimated effects of search policies on the number of contacts reported by UI claimants audited by the BAM program in the 45 sample states described in Section 2.4. The dependent variable in columns (3) and (4) is the number of contacts that are not explicitly found to be unacceptable. The dependent variable in columns (5) and (6) is the number of contacts that are explicitly found to be acceptable. Regressions include state fixed effects, year-by-month fixed effects, and controls for industry, occupation, education, UI claim duration, race, ethnicity, and sex. Columns (7) and (8) regress the number of search methods reportedly used in the prior 4 weeks by unemployed job-losers in the monthly CPS, again for the 45 states in question. Columns (9) and (10) report estimates from linear probability models for contacting employers directly using the same CPS sample. Both sets of CPS models include state fixed effects, year-by-month fixed effects, and controls for occupation, industry, race, education, unemployment duration, unemployment reason, and sex.

Table 2.2: Share of CPS Respondents Reporting Each Search Method

Contacted employer directly/interview	0.422
Contacted public employment agency	0.175
Contacted private employment agency	0.075
Contacted friends or relatives	0.203
Contacted school/university employment center	0.022
Sent out resumes/ filled out applications	0.388
Checked union/professional registers	0.037
Placed or answered ads	0.141
Other active	0.060
Looked at Ads	0.255
Attended job training programs/courses	0.011
Other passive	0.006
N:	175,643

Notes: Table reports the share of unemployed basic monthly CPS respondents reporting each type of job search method over the prior four weeks between 2003 and 2013. Sample includes all CPS respondents ages 25 to 65 who are unemployed and looking for work and who reported losing or being laid off from their previous jobs.

Table 2.3: Cox Proportional Hazard Models of Policy Effects on Reemployment

	Failure= Reemployment	
# contacts	1.009	
	(0.009)	
1 contact		0.992
		(0.044)
2 contacts		1.032
		(0.042)
3 contacts		1.005
		(0.021)
4+ contacts		1.052
		(0.043)
Nonspecific	1.027	1.027
	(0.034)	(0.034)
Observations:	142,825	142,825
χ^2*-stat, all=0:*	1.1	7.1
p-value:	0.574	0.215

Notes: Table reports hazard ratios for measures of search requirement policies using longitudinally-linked CPS data. Included controls allow the hazard to vary proportionally in month, year, state, education, occupation, industry, race, ethnicity, and sex.

Table 2.4: Policy Effects on Average UI Weeks Claimed

	Outcome=Running Avg Weeks Claimed	
# required contacts	−0.124	
	(0.397)	
1 contact		−0.393
		(1.327)
2 contacts		−1.004
		(1.394)
3 contacts		−0.320
		(0.251)
4+ contacts		−1.803
		(1.434)
Nonspecific	−0.622	−1.042
	(1.602)	(0.644)
Observations:	2,205	2,205
F-stat, all=0:	0.08	0.84
p-value:	0.927	0.528

Notes: Huber/White/sandwich standard errors clustered at the state level are displayed below estimates in parentheses. Table presents estimates of the effects of search policies on quarterly aggregate average UI claim duration, which is given by (total weeks claimed)/(initial claims), each over the past calendar year. Regressions include state fixed effects and year-by-quarter fixed effects. Estimation sample is 2,205 state-quarter observations from 2001 to 2013.

Table 2.5: Relative Policy Effects by Pre-period Labor Market Strength

			Outcome=Unemployment Rate			
		All	FL	OH	PA	UT
L_m defined 2001 to t^*		−0.002	−0.004	−0.001	−0.0001	−0.009
		(0.001)	(0.002)	(0.002)	(0.001)	(0.004)
Pretreatment avg:	High u:	*0.066*	*0.070*	*0.063*	*0.068*	*0.053*
	Low u:	*0.053*	*0.055*	*0.050*	*0.054*	*0.048*
L_m defined $t^* - 1$ to t^*		−0.004	−0.006	−0.001	−0.004	−0.009
		(0.001)	(0.002)	(0.002)	(0.002)	(0.004)
Pretreatment avg:	High u:	*0.069*	*0.063*	*0.063*	*0.066*	*0.053*
	Low u:	*0.054*	*0.056*	*0.051*	*0.056*	*0.048*
Number of statistical areas:		*110*	*30*	*38*	*33*	*9*
Policy Change:			*NS to 5*	*1 to 2*	*0 to 2*	*2 to 4*

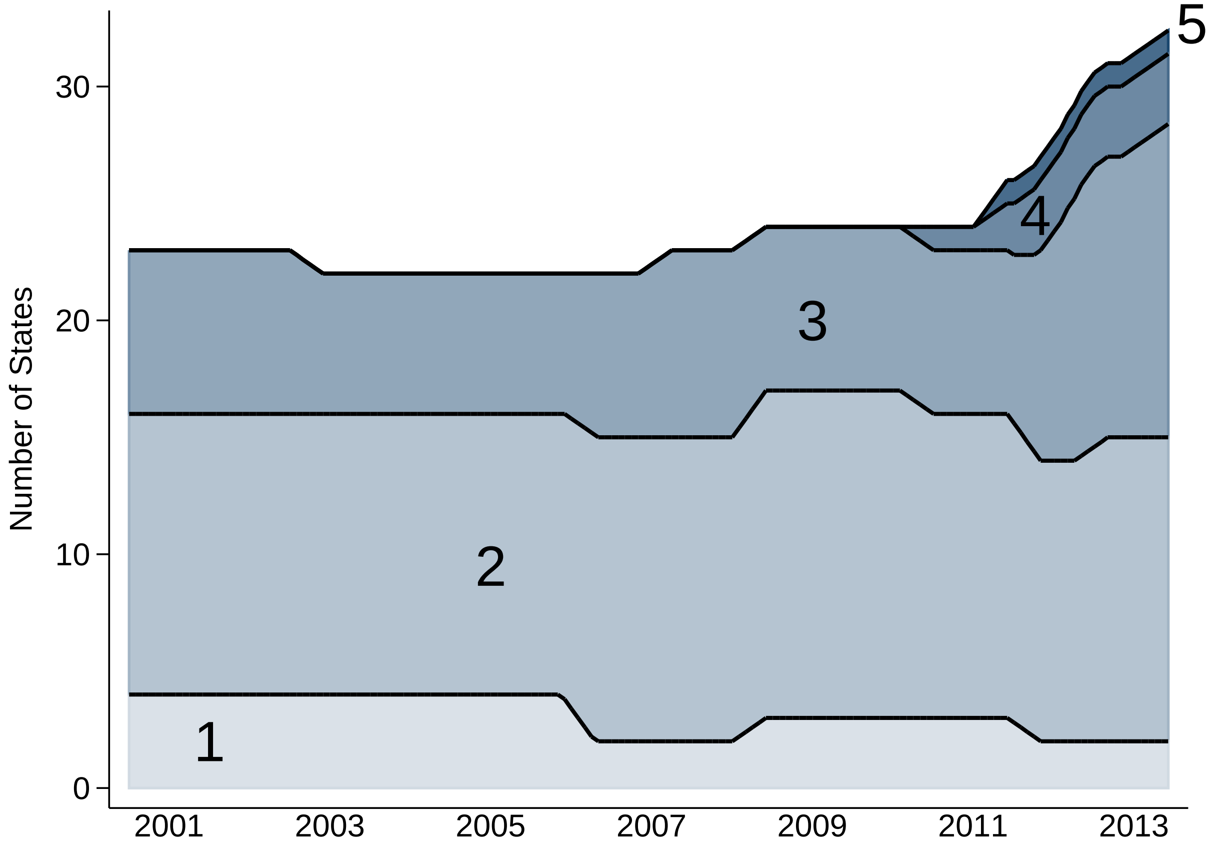
Notes: Table reports regression estimates from a specification as described by equation (2.29). Estimates indicate the differential change in unemployment rates for initially-low-unemployment statistical areas when a search requirement increase is implemented at time t^*. Low unemployment statistical areas are defined as those that have average unemployment below the state median in the indicated period (2001 to t^* or $t^* - 1$ to t^*). Monthly data on statistical area unemployment rates come from the LAUS program.

Table 2.6: Policy and Shift-Share Interaction Effects

	Outcome= Unemployment Rate	
$\hat{G}_{mt}$	-0.115	-0.115
	(0.052)	(0.053)
$\hat{G}_{mt}$ x # contacts	-0.030	
	(0.018)	
$\hat{G}_{mt}$ x 1 contact		0.027
		(0.053)
$\hat{G}_{mt}$ x 2 contacts		-0.060
		(0.052)
$\hat{G}_{mt}$ x 3 contacts		-0.146
		(0.026)
$\hat{G}_{mt}$ x 4+ contacts		0.016
		(0.059)
$\hat{G}_{mt}$ x Nonspecific	-0.140	-0.142
	(0.057)	(0.035)

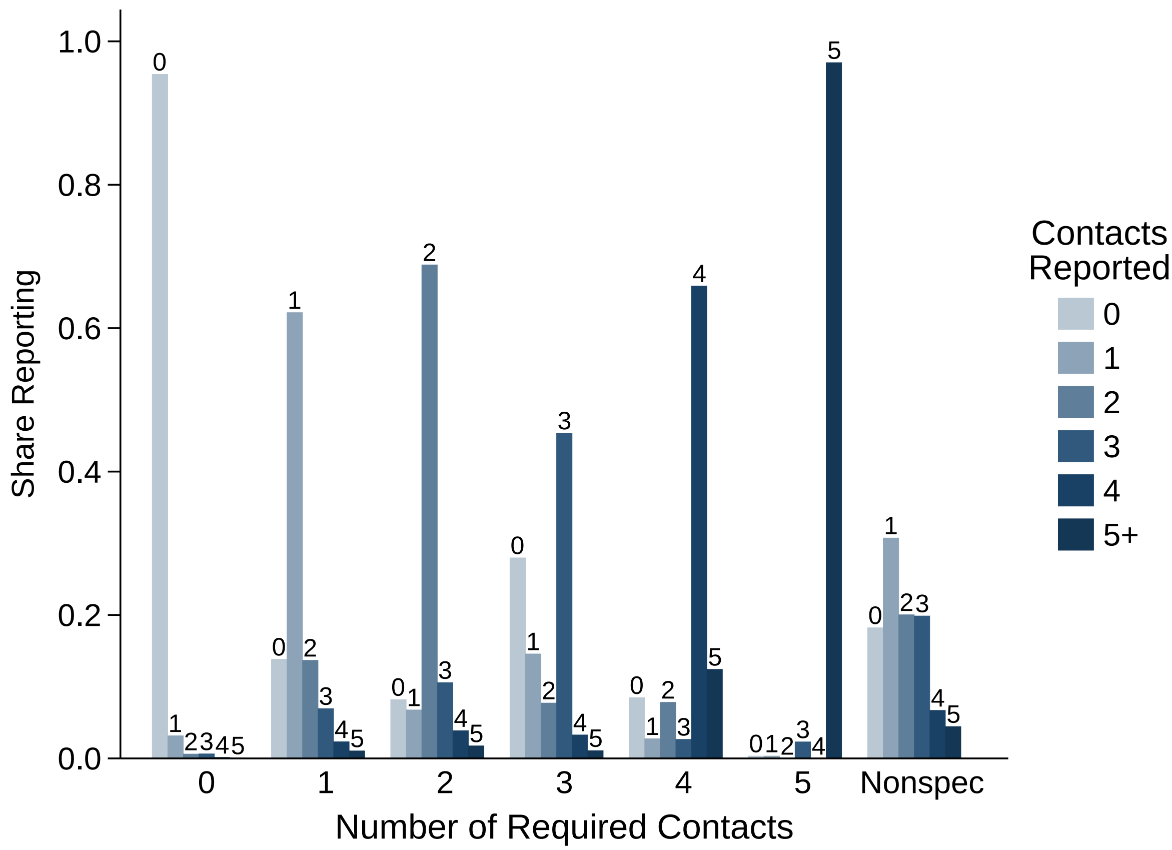
Notes: Standard errors are calculated with 1000 repetitions of a clustered bootstrap at the state level. Reported coefficients are estimated analogs of α_1 and $\boldsymbol{\alpha}_4$ from equation (2.32). $\hat{G}_{mt}$ is calculated at quarterly frequency using QWI data as described in Section 2.6.2. In addition to the reported coefficients, main effects of the policies are included along with statistical area fixed effects and month fixed effects. The outcome measure is the unemployment rate from the LAUS program. Estimation is performed on $70,584$ area-month observations from 692 individual statistical areas in 37 states.

Figure 2.1: Number of States with Each Required Number of Employer Contacts



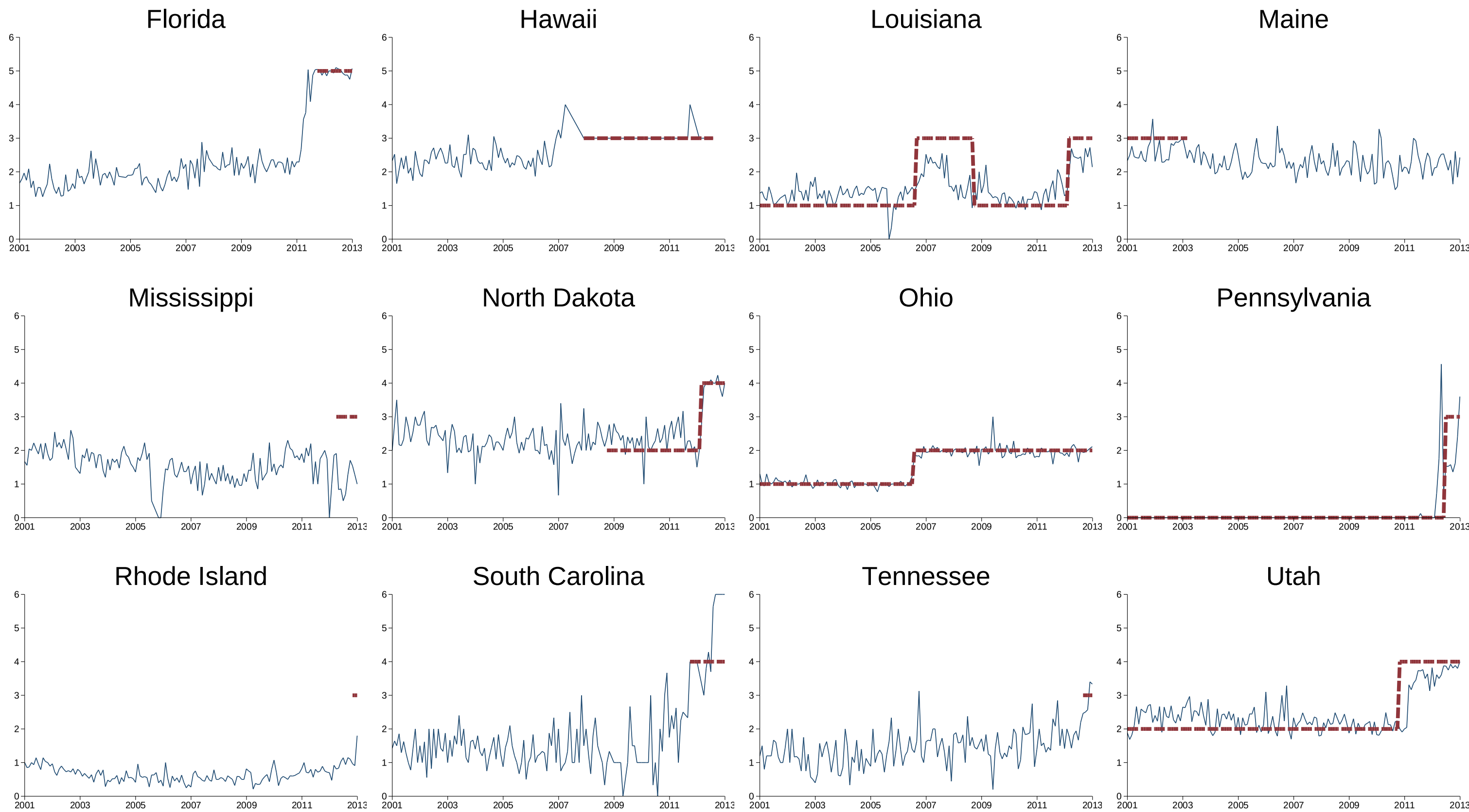
Notes: Figure indicates the number of states with each specific search requirement from one employer contact per week to five employer contacts per week. Policies are identified using state workforce agency publications.

Figure 2.2: BAM Distribution of Reported Contacts by Policy



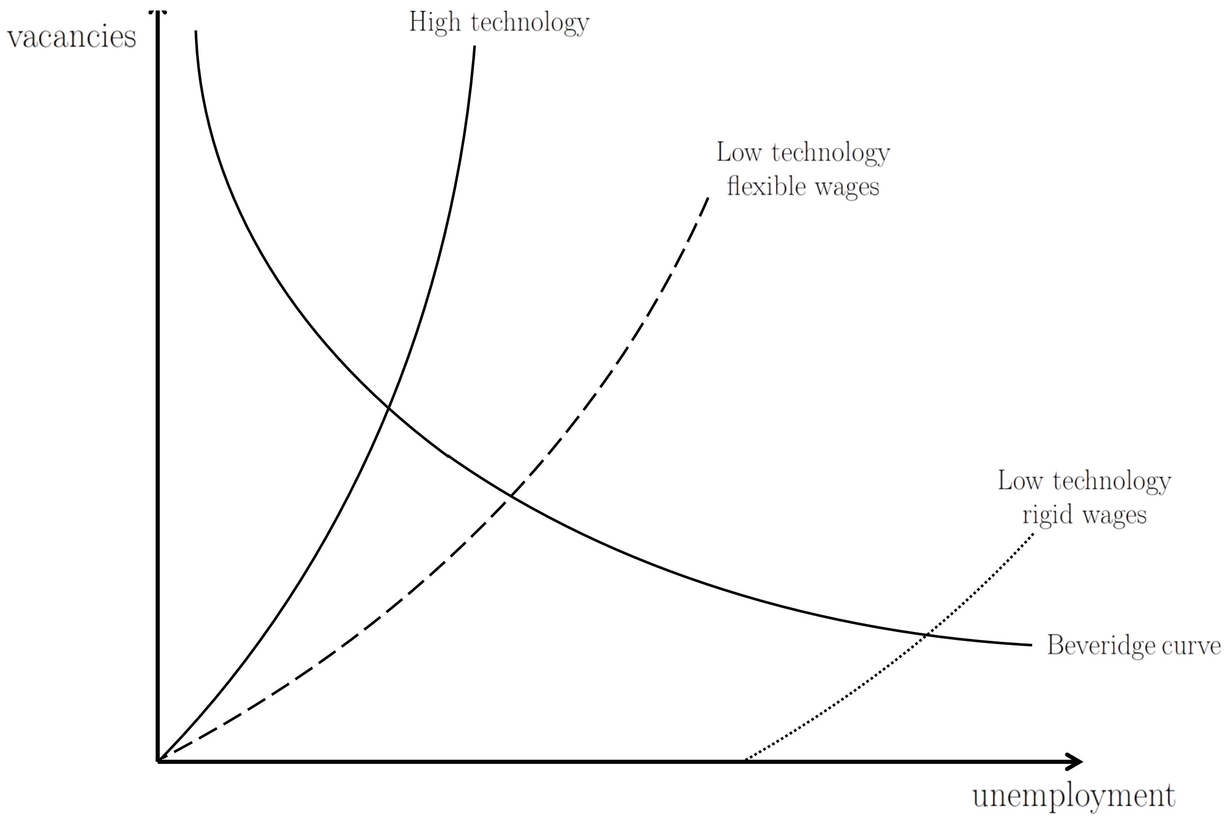
Notes: Figure indicates share of BAM-audited UI claimants reporting each number of employer conducts by search requirement policy.

Figure 2.3: Search Policy and Average Contacts Reported by State, states with policy changes



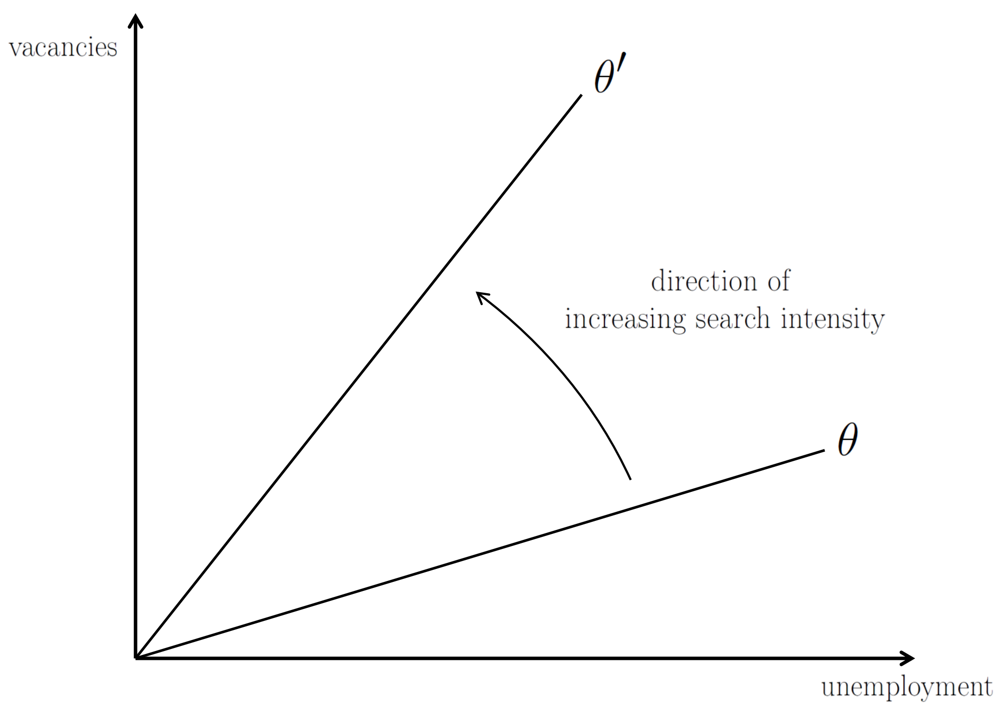
Notes: Thick dashed lines indicate search requirements as found in workforce agency publications. Any time without a dashed line indicates a nonspecific search policy. Thin solid lines show the monthly average number of employer contacts reported by audited claimants in the BAM data.

Figure 2.4: Initial Economy Equilibria



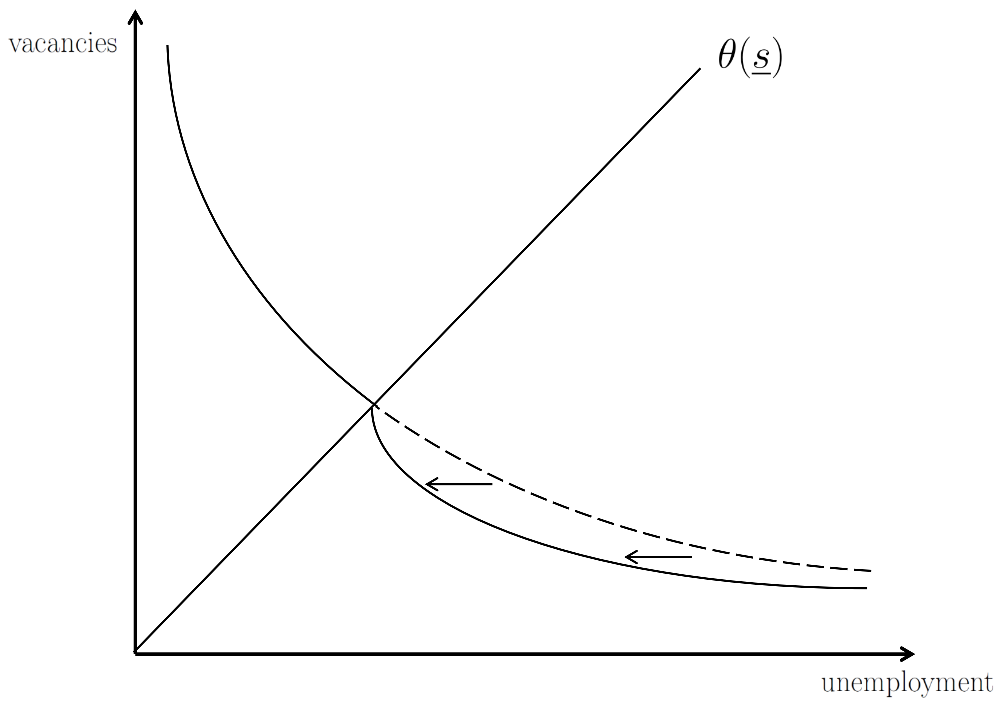
Notes: Unrestricted equilibria for the model described in Section 2.5.

Figure 2.5: Optimal Search is an Increasing Function of θ



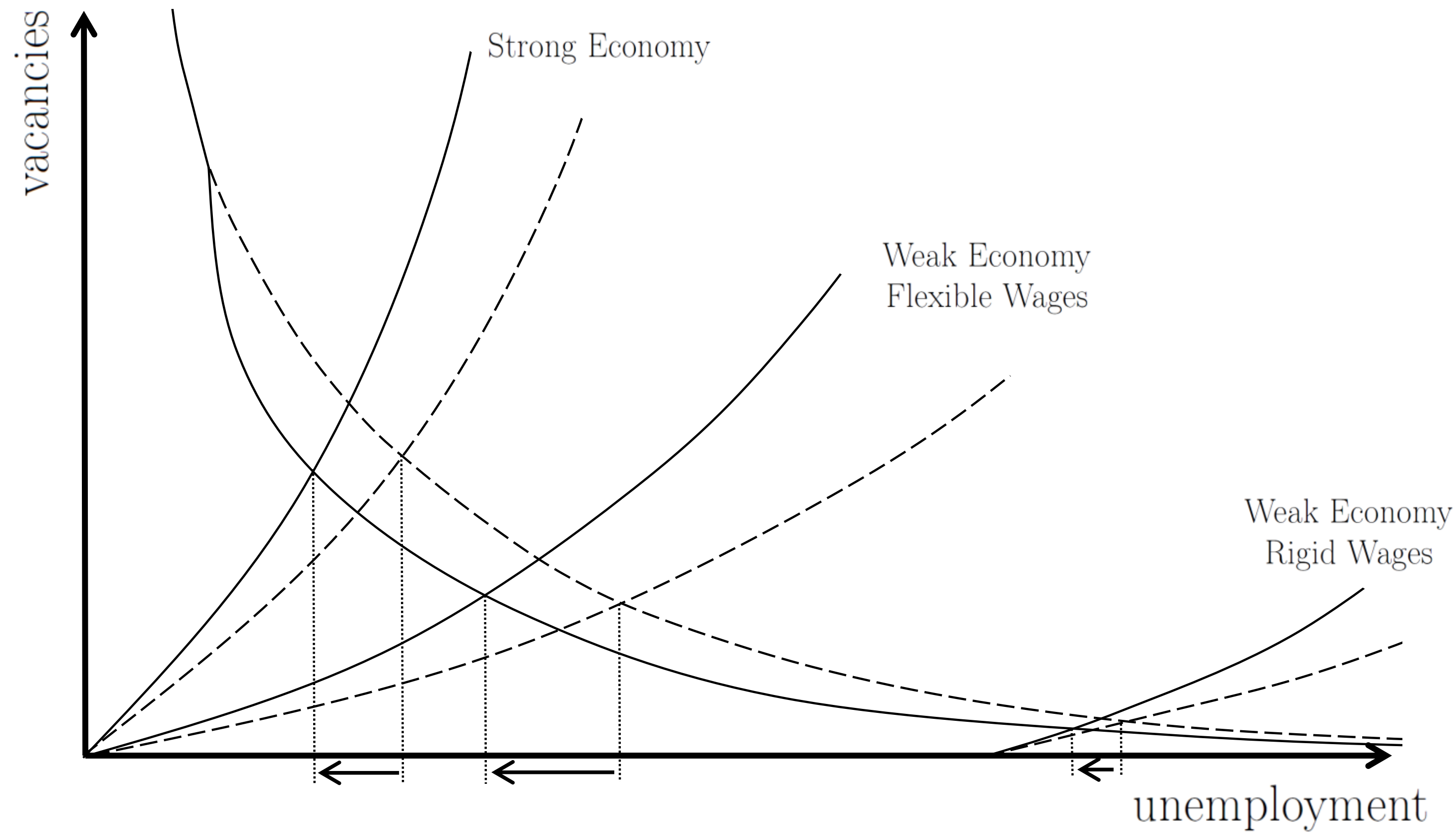
Notes: Variation in symmetric equilibrium optimal search effort for various levels of label market tightness for the model described in Section 2.5.

Figure 2.6: Beveridge Curve Under Search Restriction



Notes: Effects of an search requirement implemented at $\underline{s}$ for the model described in Section 2.5.

Figure 2.7: New Equilibria Under Search Restriction



Notes: New equilibria under the search requirement for the models described in Section 2.5 at different levels of labor productivity.

CHAPTER III

The Added Worker Effect in Late Career

3.1 Introduction

Involuntary job loss has dramatic and persistent negative impacts on a variety of household outcomes. A large literature on layoffs and plant closings shows their long-lasting effects on household finances and family stability. When such displacements[1] strike shortly before retirement, households have less working time to recover from the negative effects, may be less able to self-insure against the income shock, and may decide to exit the labor force entirely.

In theory, households with two potential workers can insure against some of the negative effects of displacement by increasing the labor supply of the nondisplaced worker. If households are unwilling or unable to formally insure against idiosyncratic income risk, they may adjust their behavior in response to income shocks. Following displacements, which the literature has shown to be associated with persistent decreases in average income, households may optimally increase their labor supply. If they can do so, they may be able to reduce the extent to which they draw down asset holdings or reduce consumption.

The propensity of one spouse to increase labor supply in response to the other's job loss

[1] I define displacement here as involuntary job loss due to layoff or plant closing. Some of the literature examines any such job losses while other parts study only the effects for high-tenure workers. I follow Jacobson et al. (1993) in using displacement to describe any job loss due to layoff or plant closing and specifying high-tenure displacement when necessary.

(the "added worker effect") has been studied since at least the 1970s and documented by Stephens (2002). However, older workers are a unique and important group. With fewer working years in front of them, older couples that suffer a negative income shock have less time to make up lost income. Additionally, as previous research has shown that the effects of displacement are strongest for those with long job tenure (Hamermesh, 1989), long-tenured older workers may be particularly negatively affected. If they also face discrimination in the job market, it may be more difficult for them to find new employment. The study of the labor supply responses of older workers to their spouses' job losses also allows for the potential documentation of coordination of leisure in retirement following individual income shocks.

This paper examines the labor supply responses of the nondisplaced spouse in late-career households facing displacement. I present a simple model that suggests that the added worker effect may be smaller or even negative for older workers. I then use the Health and Retirement Study (HRS) to identify displacements for a sample of older workers between 1992 and 2008. I construct monthly employment histories for HRS respondents over this period and examine the effects of a spouse's displacement on employment in each month. I also estimate the effects of displacement on labor force participation and earnings. I find little evidence of an added worker effect for women, but I do find that men increase their probability of employment as much as 10 percent in response to their wives' displacements. Further, I find that the workers who report the strongest enjoyment of time spent with their spouses exhibit added worker effects, while those reporting less enjoyment do not. I perform an additional analysis of the response of women to their spouse's job displacements in the Survey of Income and Program Participation, comparing across ages and using specifications that have appeared more recently in the literature.[2]

This paper lies at the confluence of a long literature on household responses to involuntary job loss (e.g. Chan and Stevens 2001; Stephens 2002; Charles and Stephens 2004)

[2]This work yields inconclusive results and appears in Appendix D.

and a literature on joint labor supply at older ages (e.g. Blau 1998; Gustman and Steinmeier 2000; Casanova 2010). The displacement literature analyzes labor force outcomes and responses for households facing displacement, but generally abstracts from the complementarity or substitutability of leisure and focuses on the income effects of job loss. I study older households because they have relatively less time remaining in their working lives to insure against displacement and because the labor-leisure choice on the extensive margin is particularly salient for them.

The paper proceeds as follows. Section 3.2 describes the background and existing literature. Section 3.3 discusses a simple two-period model of household labor supply. Section 3.4 explains the empirical methodology, identification strategy, and their limitations. Section 3.5 describes the data. Section 3.6 details the results of estimation. Section 3.7 discusses and concludes.

3.2 Background

Job displacements have broad implications for household welfare and have enjoyed considerable study in the economics literature. They are often viewed as plausibly exogenous shocks to income, which makes them attractive for research on household behavior. Hamermesh (1989) reviews the available research on the nature of displacements a few years before the beginning of the sample period studied in this paper. He finds that older workers in general are not especially at risk for experiencing displacement, but older minority workers are. He also finds that displacement is associated with large losses in individual earnings, which are most noticeable for workers with long tenure prior to the displacement.

Jacobson et al. (1993) examine the dynamics of post-displacement employment and earnings. Using a sample of Pennsylvania workers who were separated from their jobs in the 1980s, they identify leavers who were plausibly victims of "mass-layoff" because they worked for firms that experienced large decreases in employment. The years following displacement are associated with lower average incomes for these workers. Ruhm (1991) also documented this effect. The literature has further examined displacement's impacts on spouse labor supply (Stephens, 2002), divorce (Charles and Stephens, 2004), and future employment (Stevens, 1997; Chan and Stevens, 2001).

Previous research on the added worker effect, as in Stephens (2002), is most relevant to this paper. Although the sample used by Stephens includes workers up to age 65, he does not specifically examine the response of older workers. Using the Panel Study of Income Dynamics (PSID) Stephens (2002) examines wives' labor supply responses to their husbands' displacements and finds that wives increase both their hours worked and rates of employment. He documents modest increases in these margins prior to displacement and larger increases after displacement. In doing so, Stephens estimates that the households recoup more than 25 percent of the husbands' lost earnings. Stephens also finds some differences in behavior according to whether the displacement results from a plant closing or a layoff. This paper does not distinguish between the two because of sample size issues. Additionally, Stephens outlines the assumptions necessary to interpret his findings as estimates of structural parameters. This paper, in contrast, will take an entirely reduced form approach.

In a resume-based audit study, Lahey (2008) finds that employers are more than 40 percent more likely to offer interviews to young workers than to old workers. If older workers face this kind of discrimination, whether taste-based or statistical, the viability of the added worker effect as an insurance mechanism is reduced. If the discrimination also extends to spouses who are already working, they may be less able to increase their hours or work more years in response to displacement. Thus, the experience of displacement for older households may be quite different from that of younger households.

The literature on joint retirement indicates that husbands and wives have a preference for joint leisure even when controlling for financial incentives. Blau (1998) uses the Retirement History Study to show that joint retirement is more common than would be expected without this complementarity and that nonemployment of one spouse will increase the labor force exit rate or decrease the labor force entry rate of the other. Gustman and Steinmeier (2000) and Casanova (2010) both estimate structural models of joint labor supply decisions for older households. They similarly find incidence of joint retirement beyond that which is explained by financial incentives for retirement timing. This suggests complementarities in leisure that might also be relevant when a spouse is displaced. I would also like to consider the relationship of this paper to the "unretirement" literature, which studies the reentry of retired individuals into the workforce. Unfortunately, my sample contains relatively few respondents who are retired at the time when their spouses are displaced, so analysis of them is currently infeasible.

3.3 Theoretical Background

Stephens (2002) describes the dynamics of the added worker effect in a life cycle model of consumption and labor-leisure choice, demonstrating that the added worker effect may appear predisplacement, and may persist permanently. This paper's contribution lies in considering the different incentives facing households at older ages instead of considering response dynamics, so I present a simple two-period model to motivate why late-career households may behave differently from average households in responding to displacement. While any number of stories could justify differences in the added worker effect at older ages, the crucial assumption I make in this simple model is that work effort in the early period affects expected wages in the later period. While the model presented in this draft abstracts from uncertainty, continuing work on this paper will incorporate full dynamics and uncertainty over future wages.

A unitary household seeks to maximize utility from consumption and leisure over two periods, early career and late career. There are two workers in the household, here called the husband (h) and wife (w) for simplicity, who each have an endowment of one unit of time in each period. I abstract from discounting and interest rates, as they add little to the intuition suggested by the model. I assume that utility is intertemporally additively separable and intratemporally additively separable in consumption and leisure. Thus, the household seeks to maximize lifetime utility

$$max\,\mathcal{U} = \sum_{t=1,2} u(c_t) + v(l_t^w, l_t^h) \tag{3.1}$$

subject to

$$c_1 + c_2 = \sum_{i=h,w} (1 - l_1^i)w_1^i + (1 - l_2^i)(w_2^i - \alpha l_1^i). \tag{3.2}$$

That is, leisure in the first period has a negative effect on the return to work in the second period. This assumption is effectively based in a basic human capital story, in which work in the first period develops or maintains skills valued by the market that increase productivity in the second period. Assuming an interior solution in both workers' labor supply, we have the familiar first order conditions

$$u'(c_t) = \lambda \qquad \forall t = 1, 2 \tag{3.3}$$

$$\frac{\partial v(l_1^h, l_1^w)}{\partial l_1^h} = \lambda[w_1^h + (1 - l_2^h)\alpha] \tag{3.4}$$

$$\frac{\partial v(l_1^h, l_1^w)}{\partial l_1^w} = \lambda[w_1^w + (1 - l_2^w)\alpha] \tag{3.5}$$

$$\frac{\partial v(l_2^h, l_2^w)}{\partial l_2^h} = \lambda w_2^h \tag{3.6}$$

$$\frac{\partial v(l_2^h, l_2^w)}{\partial l_2^w} = \lambda w_2^w \tag{3.7}$$

where λ is the multiplier on the budget constraint, the marginal value of wealth. Given a vector of wages, these conditions along with (3.2) pin down the interior solution.

To analyze the effects of displacement, I follow Stephens (2002) in suggesting that displacement is equivalent to low wage draws. However, as I am less interested in describing dynamics and my two periods are meant to represent large portions of a worker's career, I consider the effect of a low wage offer in one period alone and analyze comparative statics within that period. If we assume wage offers are in a region where labor supply is not backward-bending and consider only the first-order effect of each workers' own wages on his or her own labor supply (that is, ignoring feedback to one's own leisure through spouse leisure), then $\frac{\partial l_t^i}{\partial w_t^i} < 0$. If we assume complementarity of leisure, as suggested by Gustman and Steinmeier (2000) and Casanova (2010), then decreases in a spouses' wage have the dual effects of increasing the marginal value of wealth and increasing the marginal utility of leisure.

While the net effect of these income and substitution effects is ambiguous, the change in marginal value of wealth is exacerbated in period 1 by the effect of first-period labor on second-period wages. That is, if λ and l^w both increase, a more positive adjustment in l^h is necessary to bring equation (3.6) back to equality as compared to (3.4). The intuition suggested by this model is that, assuming the leisure complementarities are similar in both periods, workers will be less likely to reduce their labor supply in response to spouse displacement at younger ages because of the negative effects on their future labor market opportunities. Thus we may be more likely to see the substitution effect induced by complementary leisure dominate the income effect of job loss for workers who are more advanced in their careers.

3.4 Empirical Methods

3.4.1 Specification

I analyze the response of labor force outcomes to spouse displacement in an event history framework. I estimate linear fixed effects models of the form

$$y_{it} = \sum_{\tau=-k}^{l} (\beta_\tau D_{i,t-\tau}) + \alpha X_{it} + \delta_i + \gamma_t + \epsilon_{it}, \tag{3.8}$$

where y_{it} is some outcome of person i in time t (e.g. employment, log earnings), $D_{i,t-\tau}$ is a flag for spouse displacement in period $t-\tau$. $D_{i,t-l}$ is also equal to one if there was a spouse displacement measured in periods before $t-l$. X_{it} is a vector of time-varying controls, δ_i is an individual fixed effect, γ_t is a period control, and ϵ_{it} is the error.

3.4.2 Controls

The estimates of β_τ are estimated effects of displacement on the outcome variable at each period τ around the displacement. These estimates are generated by the average within-worker difference between y near the displacement and y more than k periods before the displacement, after accounting for controls. The fixed effect, δ_i, the period controls, γ_t, and the time-varying covariates, $X_{i,t}$, effectively construct a counterfactual for each worker who has a displaced spouse. The estimates of β_τ indicate the average difference between the counterfactual and the observed outcomes for these workers. Thus, interpretation of β_τ as estimates of the effects of spouse displacement requires that the counterfactual account for other things that could impact labor market outcomes of treated workers.

In this reduced-form analysis, it is infeasible to control for everything that varies with time and affects labor market outcomes for these workers. I therefore lean heavily on those things I can observe, including age, health, and age relative to baseline retirement expectations.[3] Other time-varying work-related incentives, such as pension and social security benefits, parental health, and labor market discrimination are not explicitly controlled. However, if these unobserved effects are only correlated with spouse displacement through the included covariates, their effects will be absorbed by the controls I do use.

Thus, the age controls in particular play a large role in proxying for unobserved changes in incentives to work. They indicate the age-profile of the average worker, including the age-correlated impacts of all uncontrolled effects. As long as those effects that are not explicitly controlled are either uncorrelated with spouse displacement or are absorbed on average into the age controls, they will not bias the estimates of β_τ away from the true causal effects of spouse displacement. That is, provided the age-profile of the average worker is, in expectation, a good counterfactual for workers with displaced spouses, the estimates

[3] I further discuss these controls in Section 3.5 after describing the nature of the data.

of interest will be unbiased.

3.4.3 Propensity Score Weighting

It is reasonable to believe that the assumption outlined at the end of the last section—that the average worker provides a decent counterfactual for displaced workers—does not hold. In particular, it fails to hold if the spouses of displaced workers exhibit particular trends in employment even in the absence of displacement. Although fixed effects models control some of this heterogeneity and are common in the displaced worker literature, it is important to consider their limitations in the context of constructing a counterfactual for older workers. Fixed effects control time-invariant characteristics, allowing the estimates of interest to show the dynamics of movements around within-worker averages in response to treatment. However, if outcomes are driven by heterogeneous secular trends (e.g., transitions to retirement, health shocks) that are temporally correlated with treatment, fixed effects may control little of the relevant heterogeneity. It is important to note that time-varying heterogeneity in this context includes changes in incentives and conditions that workers anticipate but do not affect their behavior in advance and are unobserved by the econometrician.

In theory, this is a shortcoming of using fixed effects in levels and could be overcome by allowing for some manner of individual trends. However, it seems unlikely that one could estimate a sufficiently flexible individual trend without confounding estimates of the treatment effect, particularly if early-period outcomes do not generally portend anything about late-period retirement behavior. Although fixed effects cannot control for all of the relevant heterogeneity, they are certainly robust to time-invariant heterogeneity in ways that a more restricted model is not. Therefore, I use them in conjunction with the reweighting strategy described below.

Given that fixed effects cannot control for all the relevant dynamics faced by workers, it falls to the time-varying covariates described above to construct the relevant counterfactual. In essence, these controls can provide the flexible trends that the fixed effects cannot. However, they are only as relevant as the untreated observations off of which they are estimated. If there are large differences between these trends, say, across industry, and treated workers are concentrated in particular industries, they would provide an inaccurate counterfactual. Ideally, this problem could be solved with a sufficiently unrestricted model that would allow the effects of covariates to differ across groups of workers. However, both because of sample size restrictions and because it is not a priori clear how to define such groups, it is difficult to employ such an unrestricted methodology in this case.

Instead, I follow the inverse propensity weighting literature in an effort to make the sample of untreated workers observationally similar to the average displaced worker. That is, I estimate a probability of treatment in the sample period for each individual, p_i and reweight the untreated individuals by

$$\frac{\hat{p}_i}{1-\hat{p}_i}\frac{1-\bar{\hat{p}}}{\bar{\hat{p}}}. \tag{3.9}$$

This procedure generates a sample of untreated workers that is similar to those that are treated. Accordingly, the coefficients on the age controls (and all other controls) are estimated in such a way as to provide the counterfactual for the hypothetical worker who is observationally similar to the average displaced worker. That is, if the time-varying heterogeneity is uncorrelated with the treatment or is only correlated with the treatment through baseline variables that are effectively controlled by the reweighting, it will not bias the estimates away from the true causal effect.

In all models, I estimate confidence intervals and standard errors using a stratified block bootstrap with 1000 replications. The bootstrap randomizes over individuals within HRS strata and accounts for the Study's sampling probabilities.

3.5 Data

I perform the empirical analysis using nine waves of the HRS, a nationally-representative longitudinal survey of older Americans. In particular, I study the original HRS cohort, made up of individuals born 1931 to 1941, who have been interviewed biennially since 1992.[4] Each interview year, respondents answer a battery of questions about demographics, health, employment, assets, and expectations.

3.5.1 Sample Restrictions

I restrict my sample to individuals who are married or partnered at the first interview and remain in their unions at least through the second wave. I use data from the 1992 baseline and any interviews until the partnership dissolves or the respondents miss a wave for any reason. Because the goal is to estimate the dynamics associated with displacement and the estimation strategy employs fixed effects models, respondents must stay in the same union at least through wave 2 for me to observe them over time.[5]

I further restrict my sample to individuals whose spouses were not displaced in the three years before and two years after the first interview. This restriction is intended to allow me to measure the effects of a "first" displacement, in some sense. The literature has demonstrated that displacements tend to come in groups. My goal is to estimate the total effect of an initial displacement, including the effects of subsequent displacements. This restriction also allows me to measure baseline characteristics and consider them to be plausibly exogenous to the future displacements. On the other hand, restricting the sample in this way eliminates frequently-displaced workers from my sample, who may also be an interesting group, and it limits the analysis to those individuals whose spouses survived the recession of the early

[4]Although other birth cohorts have been added to the study over its lifetime, these cohorts yielded relatively few useable observations because of their smaller size and shorter tenure in the study.

[5]Given that Charles and Stephens (2004) find an increase in the divorce hazard following displacement, one effect of displacement is increased likelihood of exiting my sample. However, it is unclear how to interpret the added worker effect for partners who dissolve their union, so dropping these post-dissolution observations seems appropriate. The divorce hazard could be modeled explicitly, but that is currently beyond the scope of this paper.

1990s without being displaced. It also biases the raw sample to over-represent respondents whose spouses do not work, as these individuals would not be at risk for displacement around the first interview, but this effect is mitigated by the propensity score reweighting I employ.

3.5.2 Measures of Interest

3.5.2.1 Treatment

In the event of a job change between interviews, respondents are asked why they left their previous employer and why they left any intervening employment arrangements. If any of the responses given include "layoff," "plant closing" or "business closing," I consider the job separation to be a displacement. At first interview, respondents are also variously asked about their last employer and any previous employment lasting five years or more. I use answers to these question to exclude individuals displaced in the three years before the first interview, as described in Section 3.5.1.

3.5.2.2 Outcomes

As this paper is primarily interested in labor force outcomes, I collect data from the HRS on employment, labor force participation, and earnings. While I can measure employment at monthly frequency, as I will describe shortly, the latter two measures are only observed at each biennial interview. Respondents are considered labor force participants if they report any work or if they report they are not employed but looking for a job. Earnings for the previous calendar year are also measured at each interview. My primary analysis, however, concerns employment at the extensive margin, which I analyze using monthly employment histories. I use questions about the starting and ending months of jobs to determine a workers' employment status at each month. Respondents can also give exceptions to these periods–indicating months of work during periods that were otherwise indicated to have non-work, and vice versa. HRS employment histories constructed in this way have also been used by Chan and Stevens (2001) in their study of the effects of workers' job losses on their own subsequent employment.

3.5.2.3 Controls

I use baseline reports of planned retirement year and the year in which respondents expect to significantly reduce their work to help control for pre-existing retirement plans. These variables are measured as a calendar year, so in my month-level analysis, I include indicator variables for whether the month in question is in the planned retirement year or is after the planned retirement year. At each interview, respondents are also asked if they have a work-limiting health condition. If they report that they have such a condition and that it is permanent (lasting more than three months), they are asked when it first bothered them, when it began to interfere with work, and when it began to prevent work entirely, if applicable. I use these to construct dummy variables that indicate whether the current period is after the first month reported by the respondent for each of these three levels of limitation.

Possible other controls that are not used could include parental health and presence of a dependent in the household. The former is not directly observed, but a probabilistic measure of parental health could be constructed using information from other questions, including parents' ages and dates of death. Information on who else lives in the household, along with data on the school enrollment status of children, could be used to construct measures of other dependents.

I do not include controls for the worker's own job losses, even though there are arguments to be made both ways as to their inclusion. In the most extreme example, two spouses working for the same firm could both be displaced by the same set of layoffs or plant closings. In this case, lacking controls for own displacement, the spouse displacement would appear to be generating decreased employment for the non-displaced worker. On the other hand, if a spouse's displacement leads a worker to change jobs or begin working and the worker is subsequently displaced, we would not want to control away the effects of this displacement. In practice, these controls have negligible impacts on the estimates of interest and are excluded from the results reported in this paper.

3.5.3 Sample Description

The final analysis is performed on a sample of 2,802 women and 3,139 men, which is described in Table 3.1. Between 10 and 11 percent of the respondents experience spousal displacement while they are in the sample. This treated group tends to have spouses who are younger at first interview, reflecting the higher labor force participation and associated greater risk for displacement at younger ages. Spouses who are eventually displaced also report higher probabilities for their likelihood of losing their job within one year. It is interesting to note that, given the sample restrictions described above, none of these individuals are observed being displaced at any time up to two years after reporting this probability. Still, higher reported probabilities are correlated with displacement at longer horizons.

3.5.4 Propensity Score Estimation

I estimate logit models for spouse displacement in the sample period for men and women individually. I include a large set of baseline covariates including characteristics of respondents' current jobs and careers and answers to questions asked of working individuals about their expectations, including their reported probabilities of losing their jobs in the next year, being able to find new work if they lose their jobs, working after age 62, and working after age 65. The logit model estimates appear in Appendix E. Only a few of the variables are statistically significant in these models, but this is at least partially due to considerable collinearity among them. The distributions of propensity scores are displayed in Figure 3.1. While the treated and control distributions are clearly distinct (more so for women than for men), there is also considerable overlap between them for both sexes.

Figures 3.2 and 3.3 show quantile-quantile (QQ) plots of the distributions of particular variables for the treated group, the control group, and the reweighted control group. Each point on a QQ plot indicates a percentile pair for the two distributions in question. That is, the upper rightmost point indicates the location of the 99th percentile of the two distributions. Its location on the horizontal axis shows the position of the 99th percentile of the treated group and its position on the vertical axis indicates the 99th percentile for the control group. If the distributions for the two groups are identical at all percentiles, all points in the QQ plot will fall on the 45-degree line. Points below the line indicate higher values for those percentiles in the treated group, while those above the line indicate higher values for the control group. To the extent that the reweighted QQ plots lie closer to the 45-degree line, the distributions of these variables are more similar after the reweighting takes place.

The distributions of propensity scores are, by construction, somewhat more similar after the control group is reweighted. The largest other differences appear to be in those variables related to spouse labor force participation, and thus, displacement. After reweighting, the distributions of spouse earnings, spouse tenure, spouse experience, and spouse birth month are all right-shifted. The considerable heaping in the expectation questions appearing at the bottom of the figures reduces the usefulness of QQ plots in comparing their distributions. However, the reweighted distributions generally appear to have fewer points that are far off the 45-degree line.

3.5.5 Observed Displacements

Figure 3.4 shows the frequency of sample displacements at each age for the respondent and displaced spouse. The distributions in Panel A appear quite similar for men and women, with the generally higher levels for men reflecting the greater number of men in the sample who have a spouse displaced. Panel B shows the age distribution of displaced men to be right-shifted relative to the distribution of women. This is because the sample is defined by the age of the nondisplaced spouse and husbands are generally older than their wives in the sample. Because the age distributions of the non-displaced spouses are similar for the two sexes, the displaced wives will generally be younger than the displaced husbands.

Figures 3.5 and 3.6 present the effects of displacement on the displaced spouse. The estimates are generated by a specification like (3.8) with propensity score reweighting. The solid lines in Panels A and B show the predicted probabilities of employment for workers displaced at month 0. The dashed lines show what those predictions would be if those same workers had the effects of their displacements zeroed out. One way of interpreting these estimates is to note that while employment rates for displaced spouses are stable in the range of 0.7 to 0.8 in the months immediately before displacement, at the peak of recovery 24 months after displacement they are about 0.2 lower. However, we would expect some of these workers to be drifting out of the labor force in the absence of displacement anyway.

Panels C and D divide the solid lines by the dashed lines, normalizing to the model's predicted employment rates for the treated group in the absence of treatment. Probability of employment ticks up in advance of displacement, reflecting the fact that workers must be employed in the month of displacement to have been displaced, while overall employment rates are falling for workers in this age group. Displaced men and women both have employment rates at only 50 percent of expected levels in the 6 months following a displacement. Figure 3.6 demonstrates decreases in log earnings at the time of displacement and in its aftermath. Earnings recover somewhat, but the effects are understated in this regression as calendar years with no earnings are dropped from the log analysis. However, this also means that earnings are below expected levels even for those who manage to have some earnings in post-displacement years. The effects on earnings are rather imprecise as earnings are measured at only yearly frequency and likely with considerable error.[6]

3.6 Results

Figure 3.7 displays the results for women in graphical form. Upon initial inspection, there is little evidence of an added worker effect, as the confidence bounds include zero for

[6] Future work will use the restricted administrative earnings data available with the HRS, potentially improving the precision of earnings measures.

all estimates in Panel A. At a rather considerable lag of more than three years, the estimated effects do increase to six percentage points, but they remain statistically indistinguishable from zero. Panel B shows predicted employment rates for treated women (solid line) along with their predicted employment rates in the absence of spouse displacement (dashed line), the model's counterfactual. Panel C divides the former by the latter and demonstrates that the increased probability of work at long horizons, though statistically not different from zero, represents an increase in expected employment of 15 to 20 percent.

One theory for the lack of an added worker effect at the time of the job loss would be that women would like to work but cannot find employment because of discrimination or other reasons. The estimates in Figure 3.8 are designed to partially answer this question by showing the effects of spouse displacement on labor force participation. Unfortunately, because labor force participation is only measured at interview, there are considerably fewer observations that can be used to measure this outcome. Again, the results show the same general pattern as the employment estimates and are again statistically indistinguishable from zero, suggesting that an inability to find work is not driving the results. That being said, if workers know that they will face discrimination and thus do not enter the labor force at all, we would see the same effect here.

The same estimates for men, which are presented in Figures 3.9 and 3.10, tell a somewhat different story. The coefficient estimates indicate an increase in employment probability contemporaneous with their wives' job losses that is, at least briefly, statistically different from 0. As for women in Stephens (2002), the increase in expected labor supply leads spousal job loss, presumably because the household has information about the impending displacement. While the effect fades out in the several years following the displacement, the effect at horizons of more than four years suggests a long-run effect of six percentage points, although this too is not statistically different from zero. Panel B in Figure 3.9 puts these estimates in context, showing what could be interpreted as delayed exit from employment at the time of spousal displacement. This amounts to a 10 percent increase in employment at the time of the displacement, and a 15 percent increase in the long run, as seen in Panel C. The comparatively-large and somewhat-discontinuous long-run effect could suggest that a more flexible model is needed to less-restrictively account for effects at long horizons. The labor force participation estimates in Figure 3.10 tell roughly the same story, with the potential exception of the effect two years after displacement. However, this difference could be attributed to the lack of precision for these estimates.

Imprecise estimates also plague the estimated effects on earnings, as presented for both sexes in Figure 3.11. In general, there appears to be no impact of spouse displacement on earnings. Since these are estimates are conditional on earnings observations being positive, one interpretation could be that all of the effect is happening at the level of the participation decision. Apart from increasing labor supply on the extensive margin, older workers neither increase hours nor find higher-paying jobs in response to spouse displacement.[7]

I further divide the sample by respondents' reported enjoyment of their time with their spouses. I divide the sample into those who report at baseline that they find such time to be "extremely enjoyable" (EE) and those who give all other responses.[8] Sample statistics for the divided groups appear in Table 3.2. As a general rule the EE group is more white and better educated, and has higher earnings and more assets. It is also notable that both members of the household are more likely to be in the labor force for this group.

Figure 3.12 presents findings from propensity-weighted fixed effects models estimated for each group. Women in the EE group whose husbands are displaced exhibit an added worker effect lagging the displacement by six to twelve months. The effect is considerably stronger than both the non-EE group and women on average, as seen in Figure 3.7. They demonstrate a considerable increase in long-run propensity to work even after their husbands' employment rates have returned to expected levels. The short-run effect for men that is seen in Figure

[7]Estimates of the effects on hours, which are not presented here, were similarly noisy.

[8]The three other response categories are "very enjoyable," "somewhat enjoyable," and "not too enjoyable." Over half of the sample responded "very enjoyable," while "somewhat enjoyable" and "not too enjoyable" make up less than 19 percent. I would have preferred to compare these lowest two groups to "extremely" and "very" but there were two few responses in the low groups to make a meaningful comparison.

3.9 seems to be almost entirely driven by the EE group, despite the fact that their wives employment rates return to expected levels more quickly. The long-run effect, on the other hand is driven by the non-EE group, as spouse displacement has effectively no impact on the employment of EE men at long horizons.

The stronger added worker effect for the EE group is somewhat surprising, as those are the individuals who theoretically have the most to gain from coordinated leisure. However, the short-run increases in labor supply could reflect a desire to more fully insure against the negative income shock and coordinate leisure further in the future. It could also suggest stronger intrahousehold cooperation, as those couples who most enjoy their time together may be more likely to share the burden of extra work. Unfortunately, given that assets, education, and any number of other variables are endogenous to enjoyment of time with spouse, it is impossible to determine from these findings whether one of these effects is driving the different result.

3.7 Conclusion

I study the effects of spouse displacement on a workers' own labor force outcomes for a sample of workers who were over age 51 at the time of the displacement. The analysis initially shows a temporary increase in the probability of work for men whose wives are displaced, perhaps reflecting delayed retirement. Women, on average, show no increase in employment or labor force participation at short horizons, but may increase their market labor effort three or more years beyond their spouses' displacements. An analysis of the subsample of workers who report finding time with their spouses "extremely enjoyable" reveals that the short-term effect for men is concentrated in this group and that women in this group exhibit a contemporaneous and permanent increase in employment in response to their husbands' displacements.

Table 3.1: HRS Sample Means

	Women		Men	
Spouse Displaced?:	No	Yes	No	Yes
Birth Year	$1,936.14$	$1,936.45$	$1,936.09$	$1,936.70$
	(3.17)	(3.14)	(3.16)	(3.09)
Sp Birth Year	$1,932.97$	$1,934.70$	$1,939.59$	$1,941.11$
	(5.98)	(5.44)	(5.97)	(6.12)
Race:				
Black	0.06	0.04	0.07	0.05
Hispanic	0.05	0.05	0.06	0.04
Education:				
HS Grad	0.46	0.42	0.35	0.38
Some College	0.20	0.24	0.20	0.18
College Grad	0.14	0.15	0.25	0.22
Spouse Education:				
HS Grad	0.37	0.36	0.43	0.50
Some Coll	0.17	0.20	0.21	0.22
Coll Grad	0.22	0.29	0.17	0.14
Earnings (000s)	11.47	11.40	34.47	30.36
	(15.69)	(13.58)	(39.55)	(33.26)
Sp Earnings (000s)	27.27	31.29	12.96	15.60
	(37.00)	(23.42)	(15.97)	(16.22)
Assets (000s)	71.84	61.48	58.12	58.89
	(185.97)	(211.10)	(170.41)	(202.10)
In LF	0.59	0.63	0.85	0.87
Sp in LF	0.69	0.91	0.64	0.81
Planned Rtrmnt Yr	$1,999.26$	$1,999.13$	$1,999.91$	$2,000.09$
	(4.21)	(4.46)	(4.64)	(4.38)
Sp Planned Rtrmnt Yr	$1,998.88$	$1,998.86$	$2,002.27$	$2,003.30$
	(4.93)	(4.83)	(6.31)	(6.35)
Pr(Lose Job)	0.08	0.07	0.11	0.09
	(0.19)	(0.19)	(0.22)	(1.94)
Sp(Pr Lose Job)	0.08	0.13	0.08	0.14
	(0.20)	(0.23)	(1.92)	(2.41)
N:	*2,513*	*289*	*2,797*	*342*

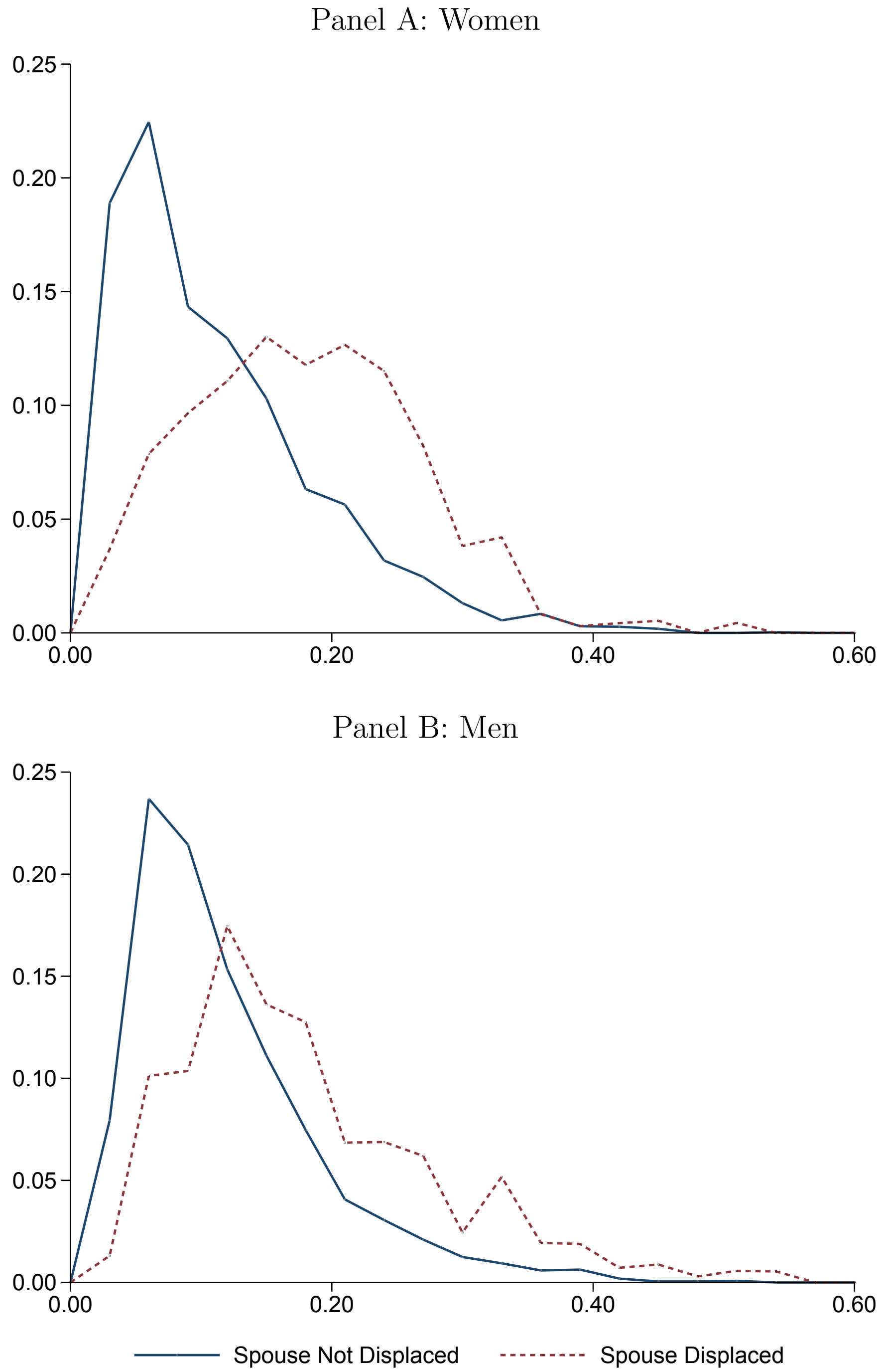
Notes: Sample standard deviations for nonbinary variables in parentheses. Means are weighted by inverse sampling probability. All variables measured at first interview. Earnings refer to previous calendar year. Planned Retirement year only includes positive responses. All other variables include zeros.

Table 3.2: Sample Means by Enjoyment of Leisure Time with Spouse

	Women		Men	
Extremely Enjoy?:	No	Yes	No	Yes
Birth Year	1,936.12	1,936.33	1,936.14	1,936.20
	(3.14)	(3.23)	(3.16)	(3.16)
Sp Birth Year	1,932.97	1,933.68	1,939.74	1,939.78
	(5.78)	(6.40)	(6.02)	(5.97)
Race				
Black	0.07	0.03	0.08	0.04
Hispanic	0.06	0.04	0.06	0.05
Education				
HS Grad	0.44	0.49	0.35	0.36
Some College	0.20	0.23	0.19	0.22
College Grad	0.14	0.15	0.23	0.28
Spouse Education				
HS Grad	0.36	0.38	0.44	0.43
Some Coll	0.18	0.17	0.21	0.23
Coll Grad	0.21	0.27	0.16	0.19
Earnings (000s)	11.15	12.37	32.66	37.19
	(15.50)	(15.41)	(35.94)	(45.01)
Sp Earnings (000s)	25.81	33.24	12.90	14.08
	(32.27)	(44.27)	(16.22)	(15.51)
Assets (000s)	66.89	82.15	55.96	63.48
	(182.23)	(206.46)	(161.46)	(200.78)
In LF	0.59	0.61	0.85	0.86
Sp in LF	0.70	0.76	0.65	0.69
Planned Rtrmnt Yr	1,999.15	1,999.53	1,999.93	1,999.94
	(4.14)	(4.47)	(4.61)	(4.63)
Sp Planned Rtrmnt Yr	1,998.74	1,999.20	2,002.42	2,002.39
	(4.97)	(4.77)	(6.12)	(6.75)
Pr(Lose Job)	0.07	0.08	0.12	0.10
	(0.19)	(0.19)	(0.23)	(2.10)
Sp(Pr Lose Job)	0.09	0.09	0.09	0.09
	(0.20)	(0.20)	(1.97)	(2.04)
Spouse Displaced	0.10	0.11	0.11	0.10
N:	*2,122*	*680*	*2,227*	*912*

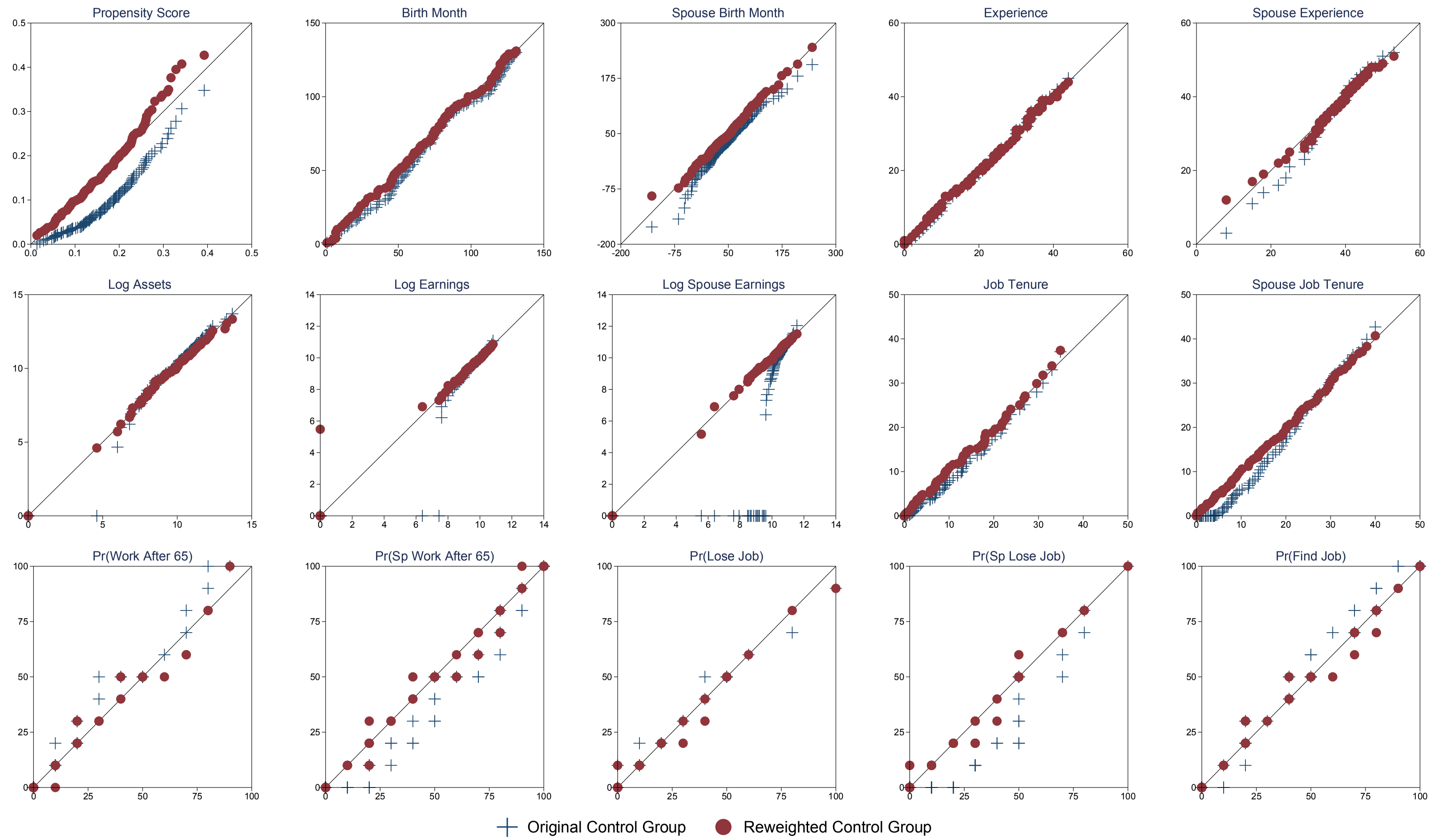
Notes: Sample standard deviations for nonbinary variables in parentheses. Means are weighted by inverse sampling probability. All variables measured at first interview. Earnings refer to previous calendar year. Planned Retirement year only includes positive responses. All other variables include zeros.

Figure 3.1: Distribution of Propensity Scores for Spouse Displacement



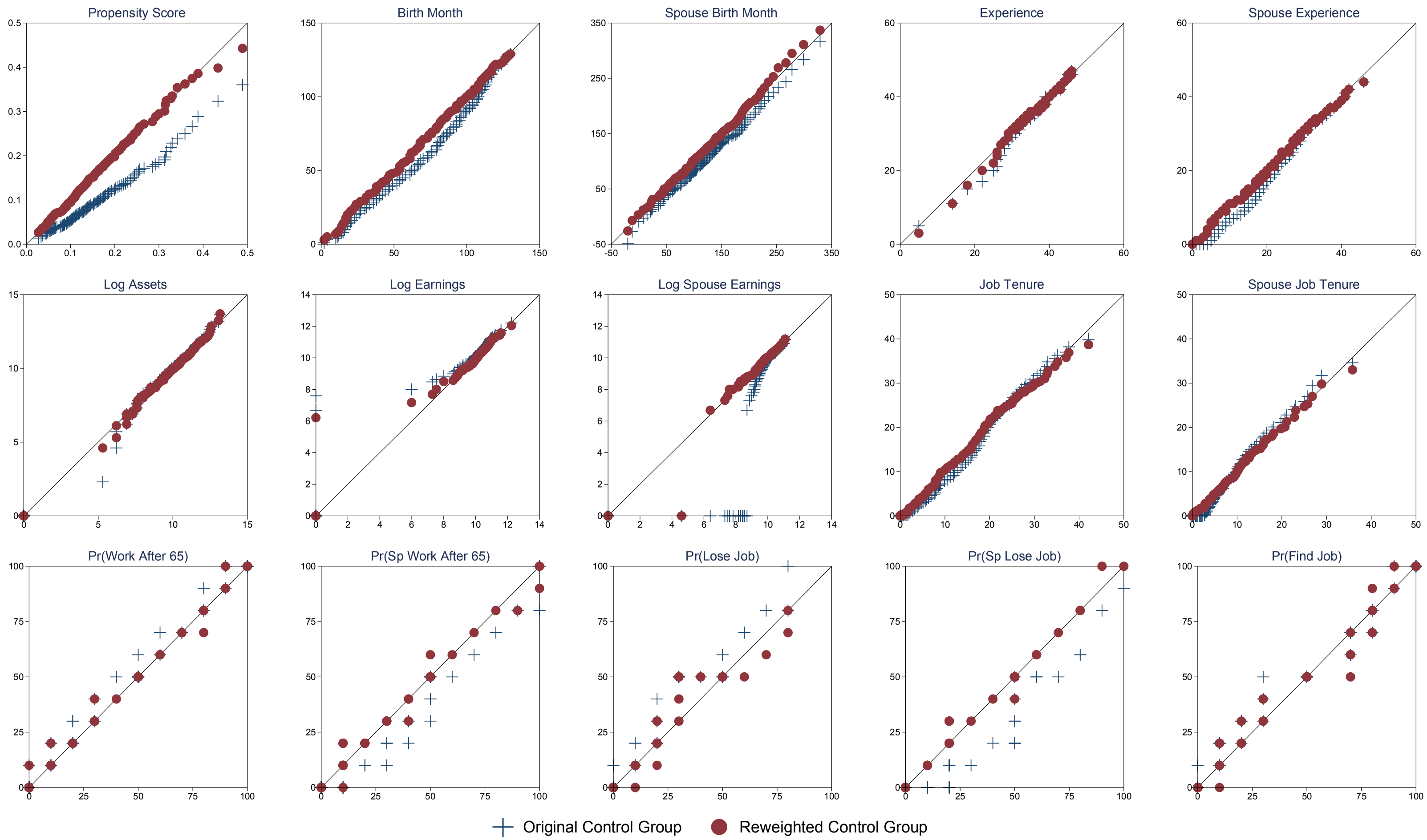
Notes: Figures display the estimated propensity scores for spouse displacement using HRS data, as described in Section 3.5.4. The logit estimates for the propensity score model are reported in Appendix E.

Figure 3.2: QQ Plots of Baseline Variables for Treated Group (Horizontal Axes) and Control Groups (Vertical Axes), Women



Notes: All values are measured at first interview. Distributions are weighted by inverse sampling probability. Birth month 0 is January 1931. The horizontal position of each QQ plot point shows the value of a particular percentile for the treated group, while the vertical position shows the value of that percentile for one of the control groups.

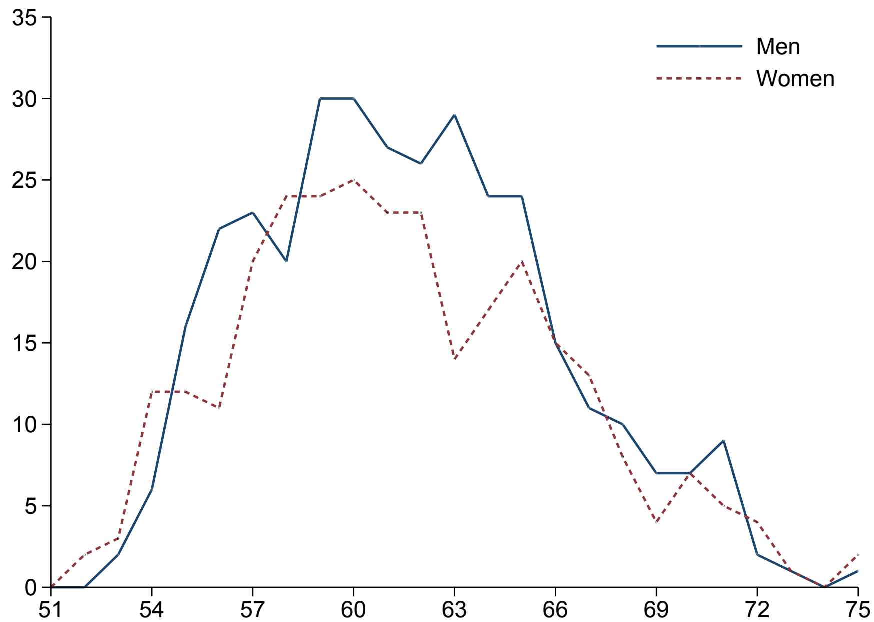
Figure 3.3: QQ Plots of Baseline Variables for Treated Group (Horizontal Axes) and Control Groups (Vertical Axes), Men



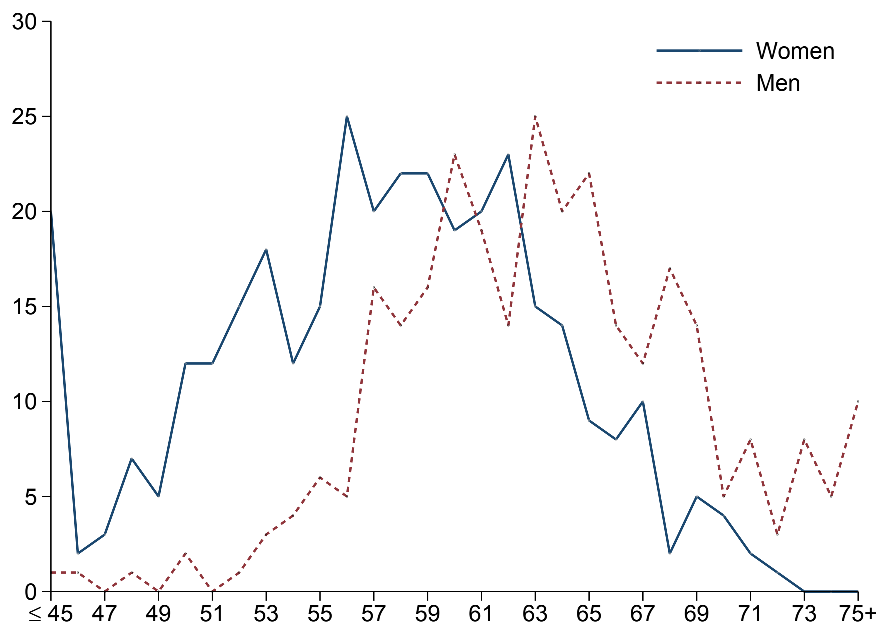
Notes: All values are measured at first interview. Distributions are weighted by inverse sampling probability. Birth month 0 is January 1931. The horizontal position of each QQ plot point shows the value of a particular percentile for the treated group, while the vertical position shows the value of that percentile for one of the control groups.

Figure 3.4: Age at Time of Displacement

Panel A: Age of Nondisplaced Spouse, by Sex of Nondisplaced Spouse



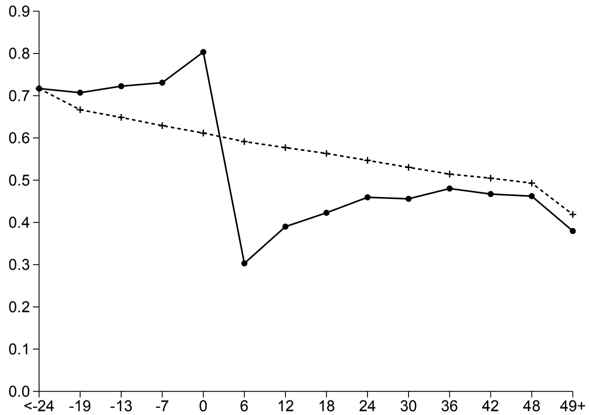
Panel B: Age of Displaced Spouse, by Sex of Displaced Spouse



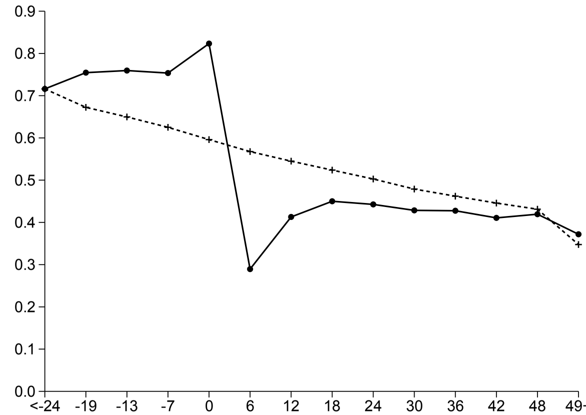
Notes: Figures display the distribution of ages of members of married couples experiencing displacement in the HRS. The sample is described in the text and Table 3.1

Figure 3.5: Predicted Probability of Employment for Displaced Workers

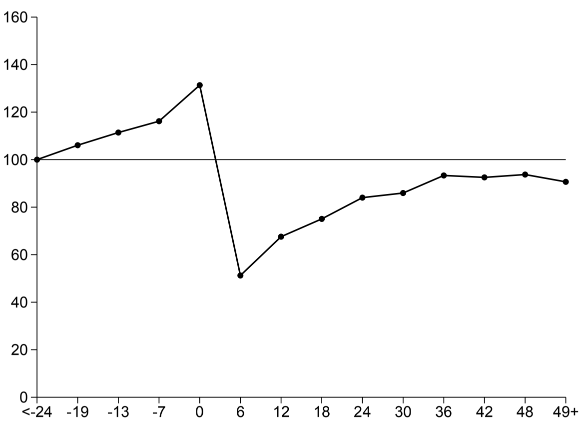
Panel A: Displaced Women



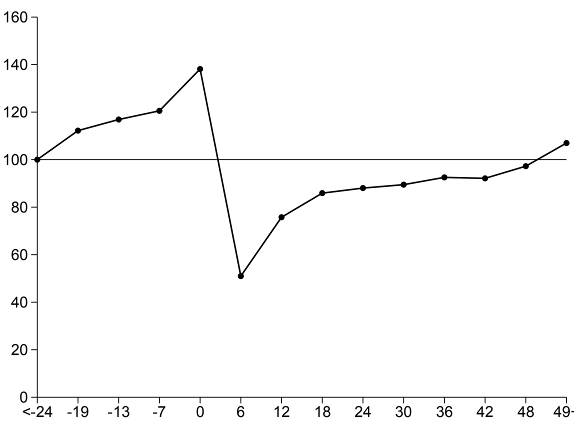
Panel B: Displaced Men



Panel C: Women, As a Percentage of Expected Employment



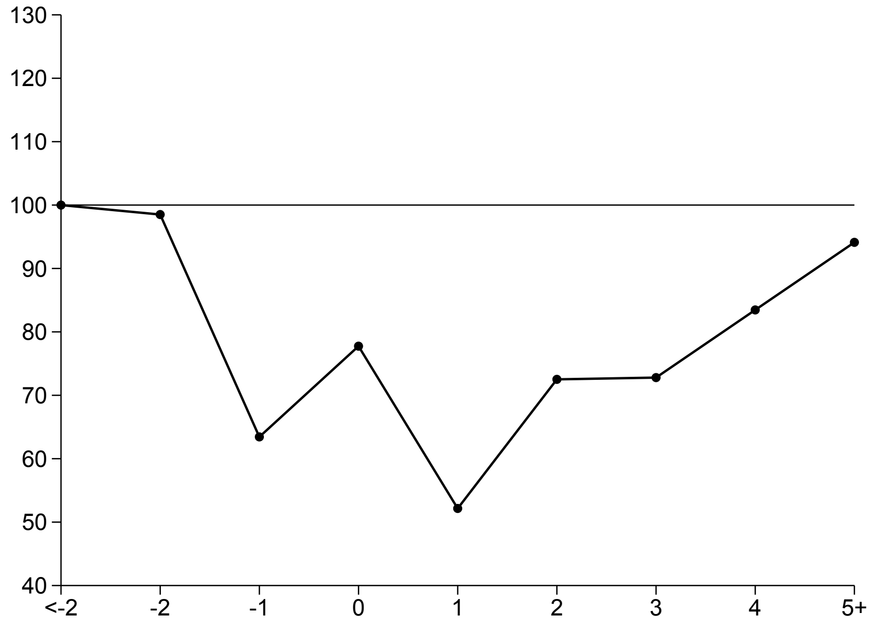
Panel D: Men, As a Percentage of Expected Employment



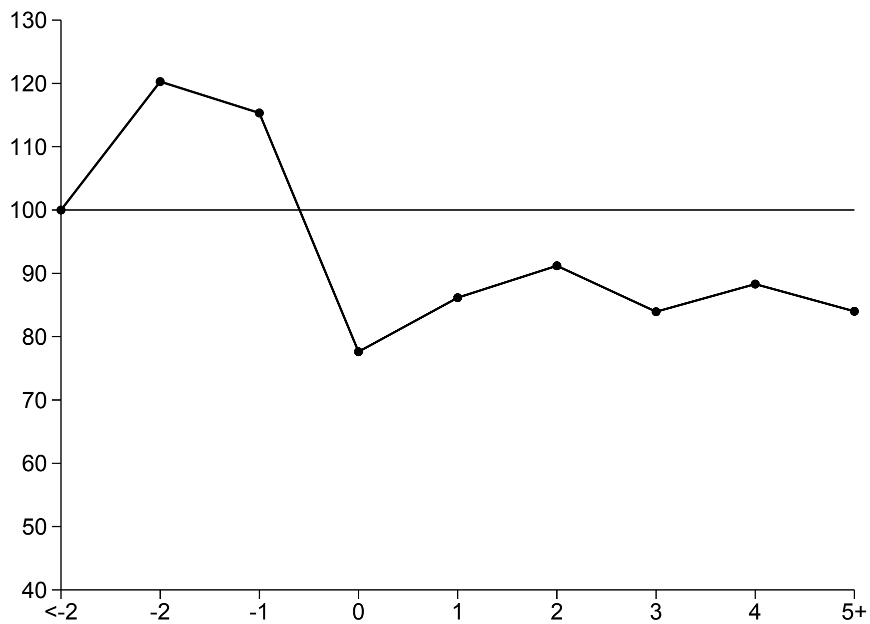
Notes: Dashed lines in Panels A and B indicate predicted probability of employment in the absence of treatment. Predicted values are calculated from inverse propensity weighted linear fixed effects models with controls for age, calendar year, calendar month, work-limiting health conditions, and time relative to expected retirement year. Months relative to displacement are group in six-month bins, with the last month indicated on the horizontal axis.

Figure 3.6: Earnings as a Percent of Expected, Displaced Workers

Panel A: Women



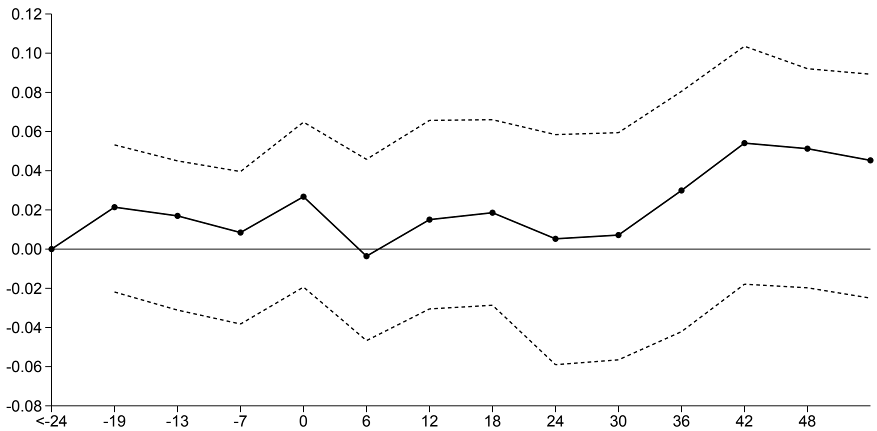
Panel B: Men



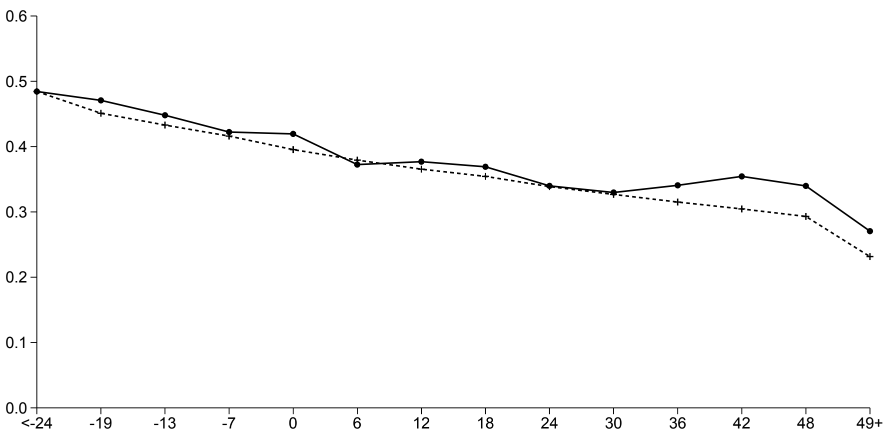
Notes: Estimates are generated by inverse propensity-weighted linear fixed effects models of log earnings with controls for age, calendar year, calendar month, work-limiting health conditions, and time relative to expected retirement year. Percent effects are calculated as $100e^{\beta}$.

Figure 3.7: Effect of Spouse Displacement on Probability of Employment, Women

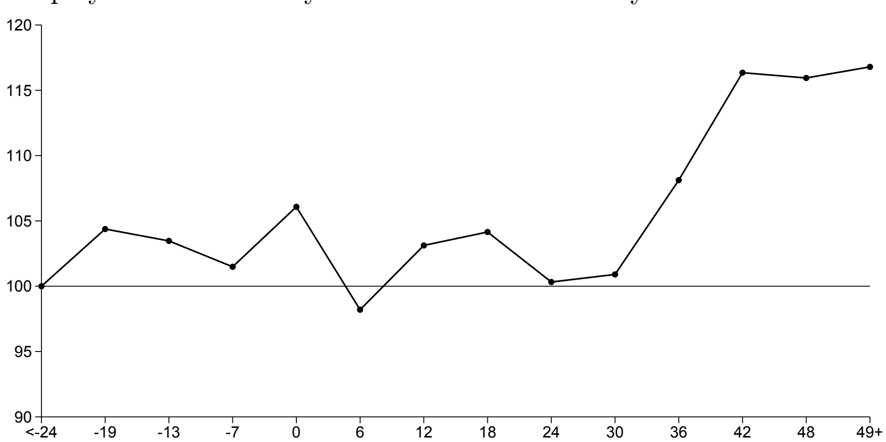
Panel A: Coefficient Estimates:



Panel B: Predicted Employment Probability:



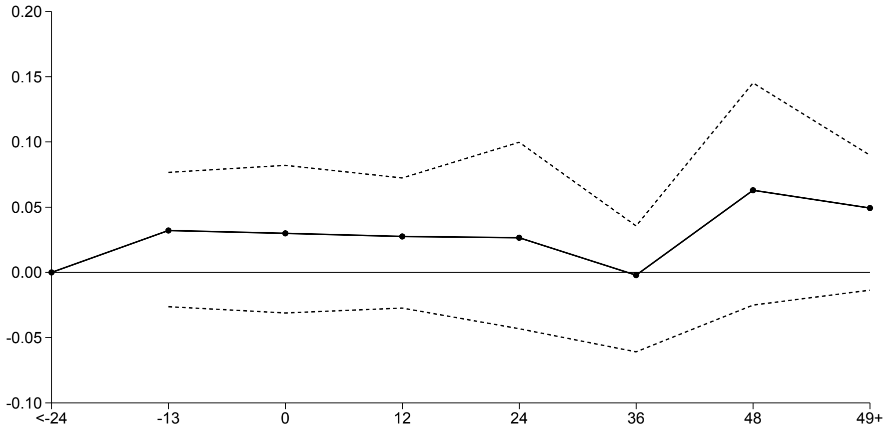
Panel C: Employment Probability as Percent of Probability in Absence of Treatment:



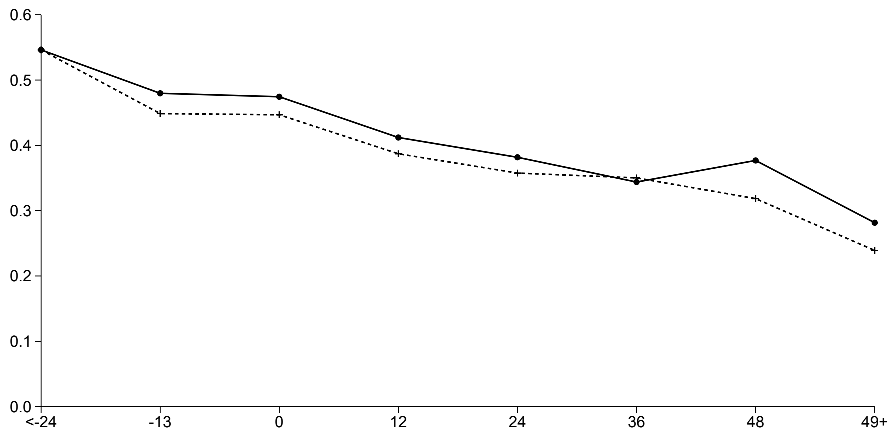
Notes: Dashed lines in Panel A indicate bootstrapped 95% confidence intervals. Dashed line in Panel B indicates predicted probability of employment in the absence of treatment. Estimates are generated by an inverse propensity-weighted linear fixed effects model with controls for age, calendar year, calendar month, work-limiting health conditions, and time relative to expected retirement year.

Figure 3.8: Effect of Spouse Displacement on Probability of LF Participation, Women

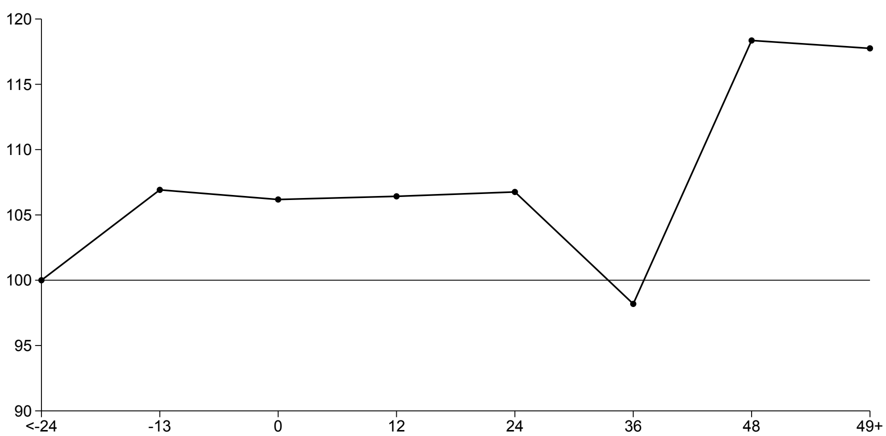
Panel A: Coefficient Estimates:



Panel B: Predicted LF Participation Probability:



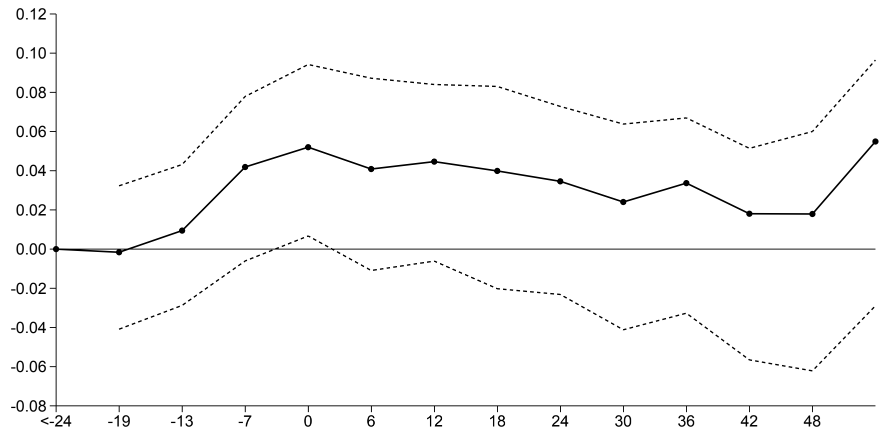
Panel C: LF Participation Probability as Percent of Probability in Absence of Treatment:



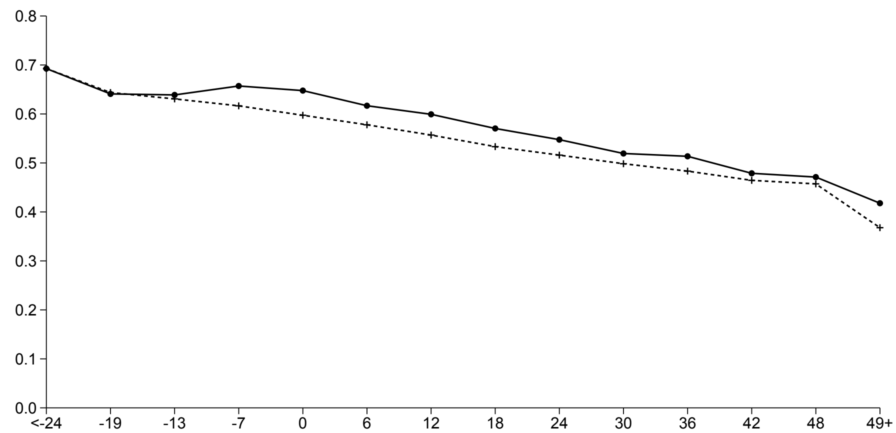
Notes: Dashed lines in Panel A indicate bootstrapped 95% confidence intervals. Dashed line in Panel B indicates predicted probability of labor force participation in the absence of treatment. Estimates are generated by an inverse propensity-weighted linear fixed effects model with controls for age, calendar year, calendar month, work-limiting health conditions, and time relative to expected retirement year.

Figure 3.9: Effect of Spouse Displacement on Probability of Employment, Men

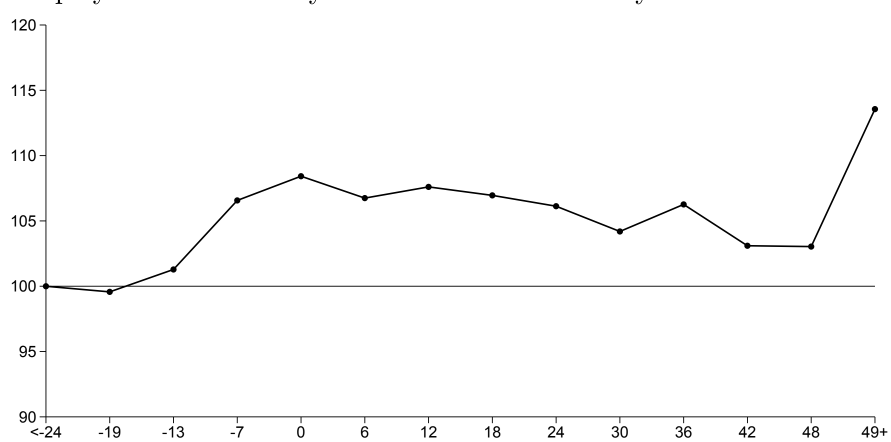
Panel A: Coefficient Estimates:



Panel B: Predicted Employment Probability:



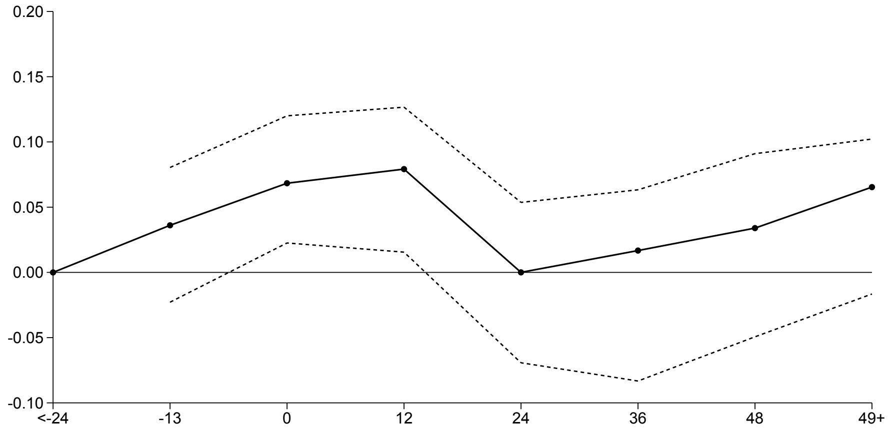
Panel C: Employment Probability as Percent of Probability in Absence of Treatment:



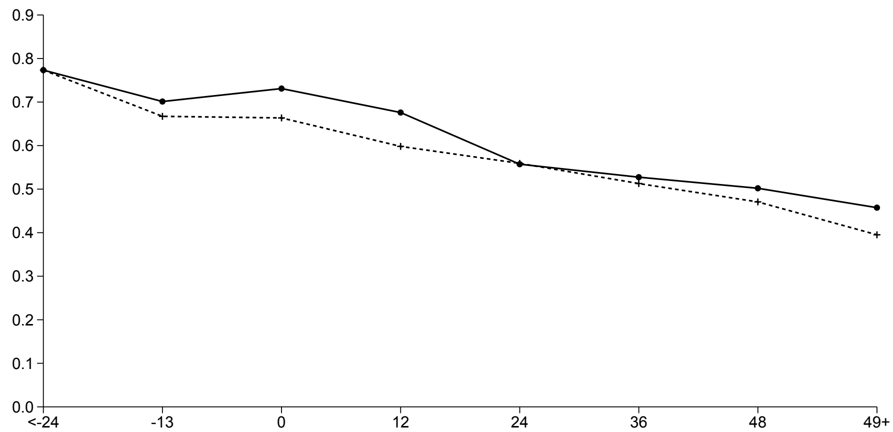
Notes: Dashed lines in Panel A indicate bootstrapped 95% confidence intervals. Dashed line in Panel B indicates predicted probability of employment in the absence of treatment. Estimates are generated by an inverse propensity-weighted linear fixed effects model with controls for age, calendar year, calendar month, work-limiting health conditions, and time relative to expected retirement year.

Figure 3.10: Effect of Spouse Displacement on Probability of LF Participation, Men

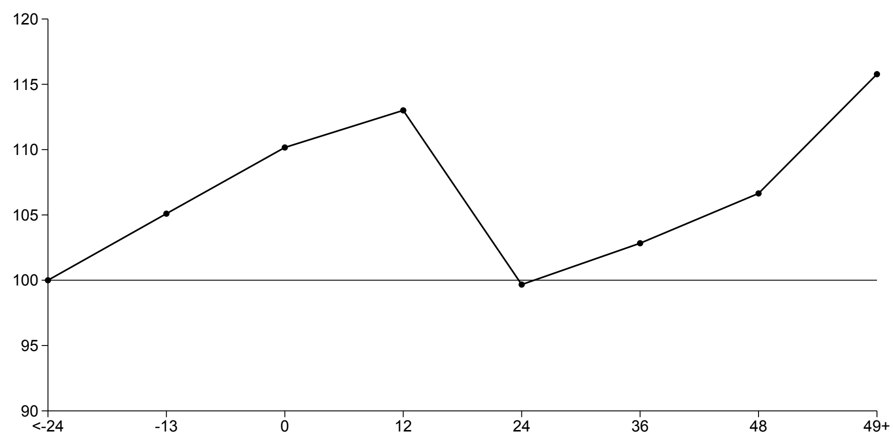
Panel A: Coefficient Estimates:



Panel B: Predicted LF Participation Probability:

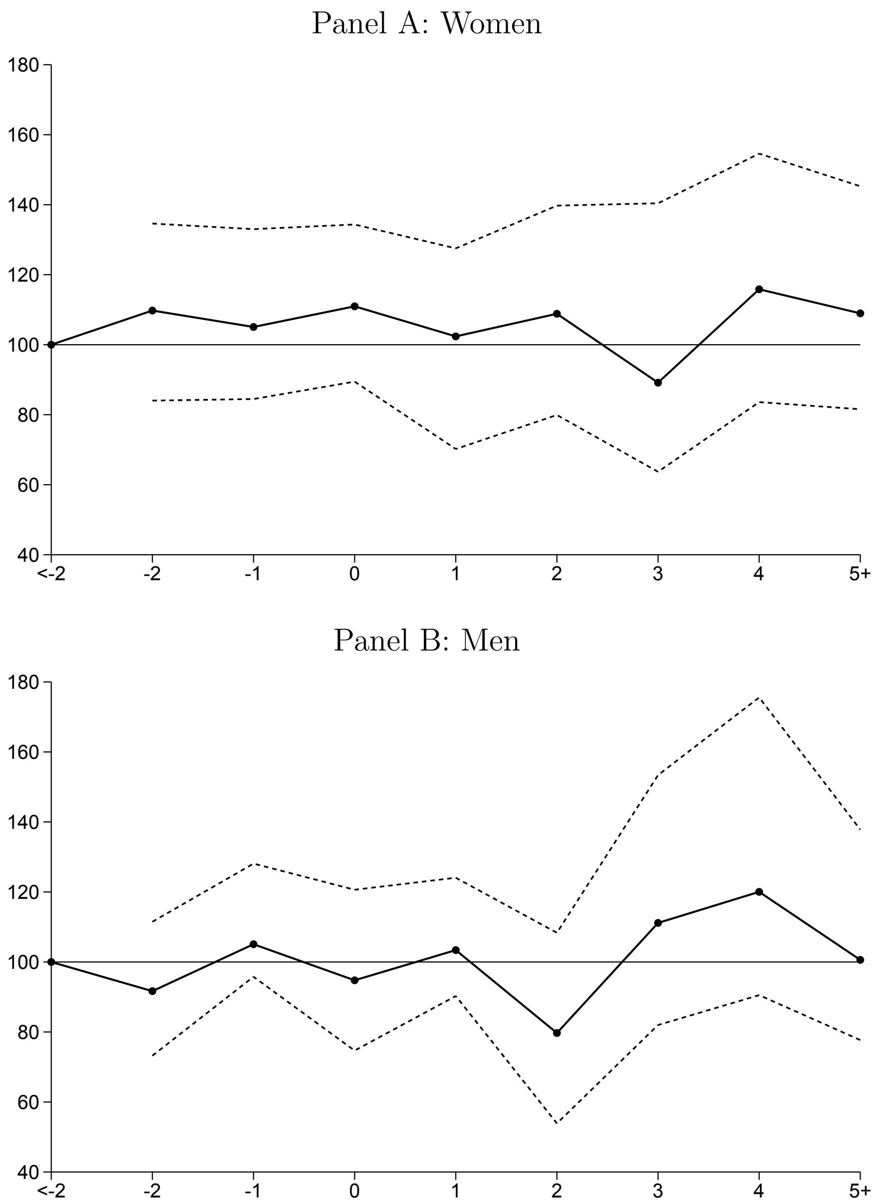


Panel C: LF Participation Probability as Percent of Probability in Absence of Treatment:



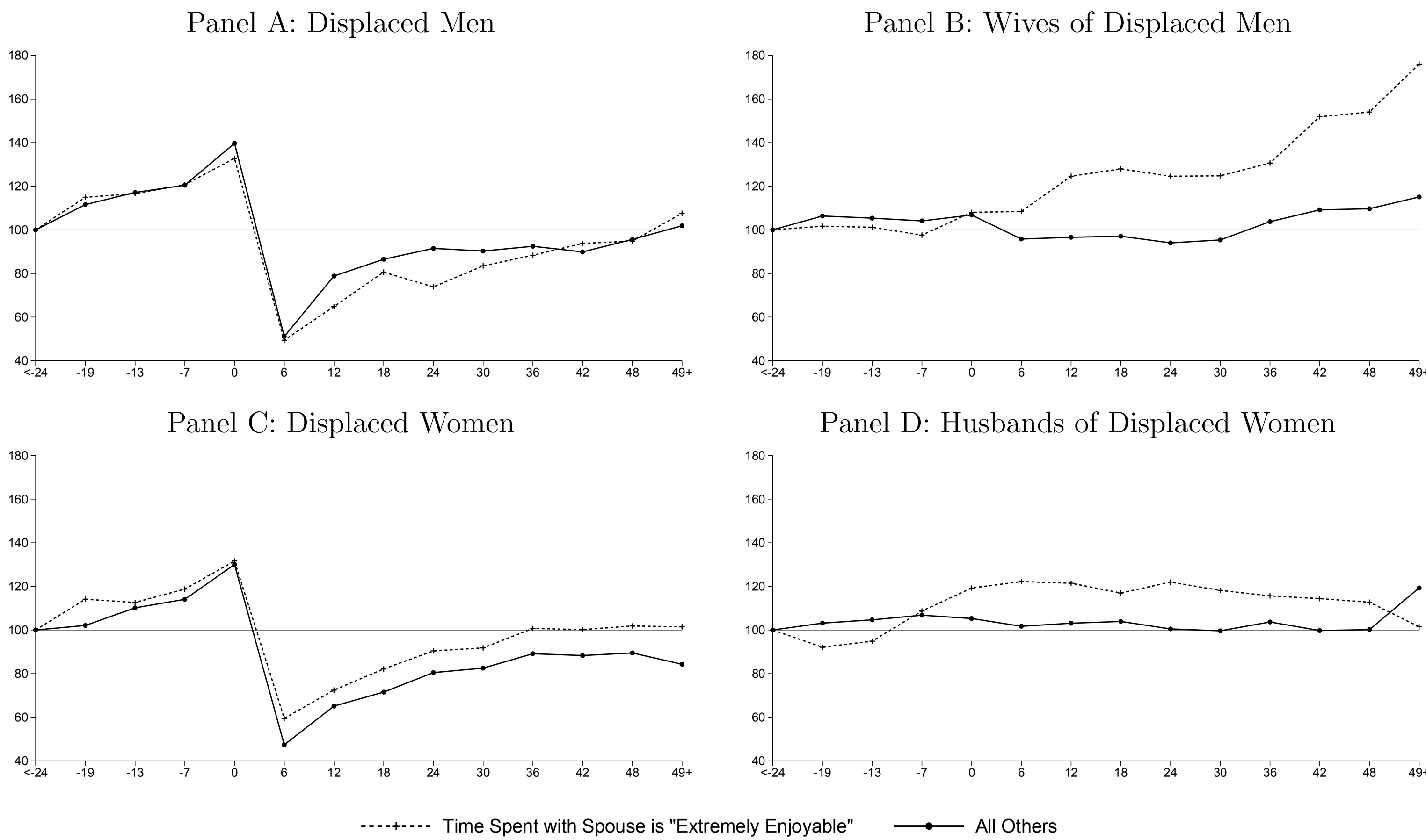
Notes: Dashed lines in Panel A indicate bootstrapped 95% confidence intervals. Dashed line in Panel B indicates predicted probability of labor force participation in the absence of treatment. Estimates are generated by an inverse propensity-weighted linear fixed effects model with controls for age, calendar year, calendar month, work-limiting health conditions, and time relative to expected retirement year.

Figure 3.11: Effect of Spouse Displacement on Earnings



Notes: Dashed lines indicate bootstrapped 95% confidence intervals. Estimates are generated by inverse propensity-weighted linear fixed effects models of log earnings with controls for age, calendar year, calendar month, work-limiting health conditions, and time relative to expected retirement year. Percent effects are calculated as $100e^{\beta}$.

Figure 3.12: Employment as a Percent of Probability in Absence of Treatment, by Enjoyment of Time with Spouse



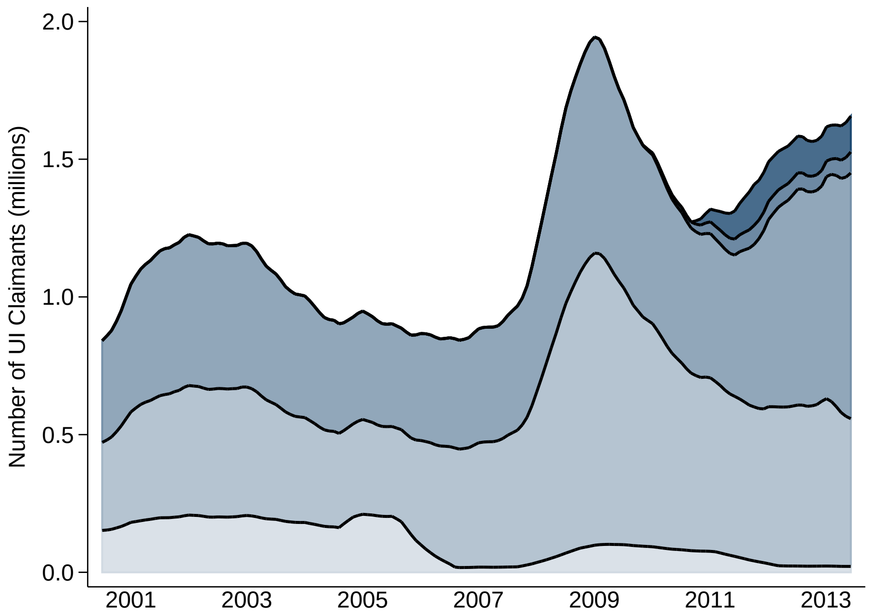
Notes: Estimates for each series are generated by an inverse propensity-weighted linear fixed effects model with controls for age, calendar year, calendar month, work-limiting health conditions, and time relative to expected retirement year.

APPENDICES

APPENDIX A

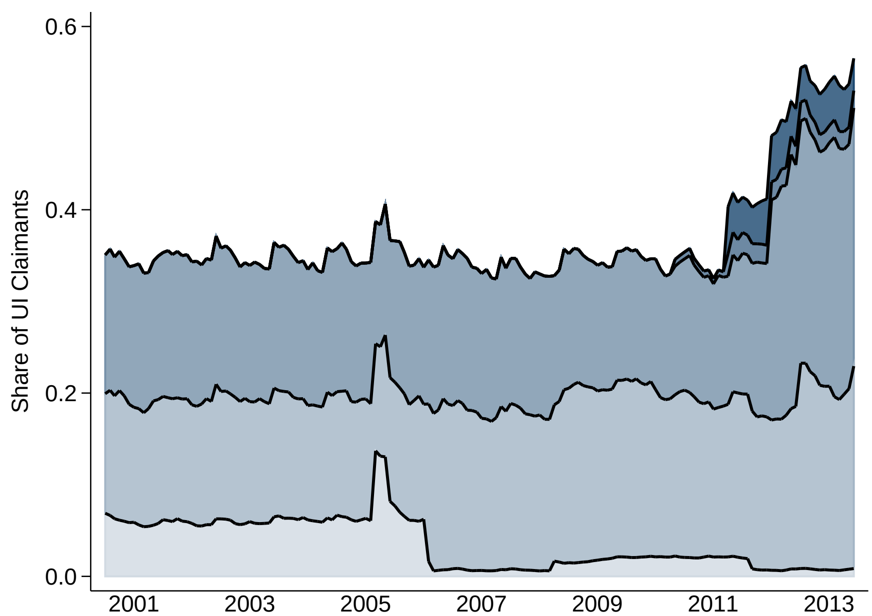
Alternative Measures of Search Requirement Policy Prevalence

Figure A.1: Number of UI Claimants Living Under Each Required Number of Employer Contacts Policy



Notes: Figure indicates the number of UI claimants living under each specific search requirement from one employer contact per week to five employer contacts per week. The thick dashed line indicates movements in national UI claims at one-third scale. Policies are identified using state workforce agency publications. Total claims under each policy are determined by summing continuing claims each month across states. The lightest color closest to the horizontal axis represents one required employer contact per week. Each darker shade represents one additional required contact, up to five, which is visible at the upper right. The omitted group includes nonspecific policies, individualized policies, and zero-contact policies.

Figure A.2: Share of UI Claimants Living Under Each Required Number of Employer Contacts Policy

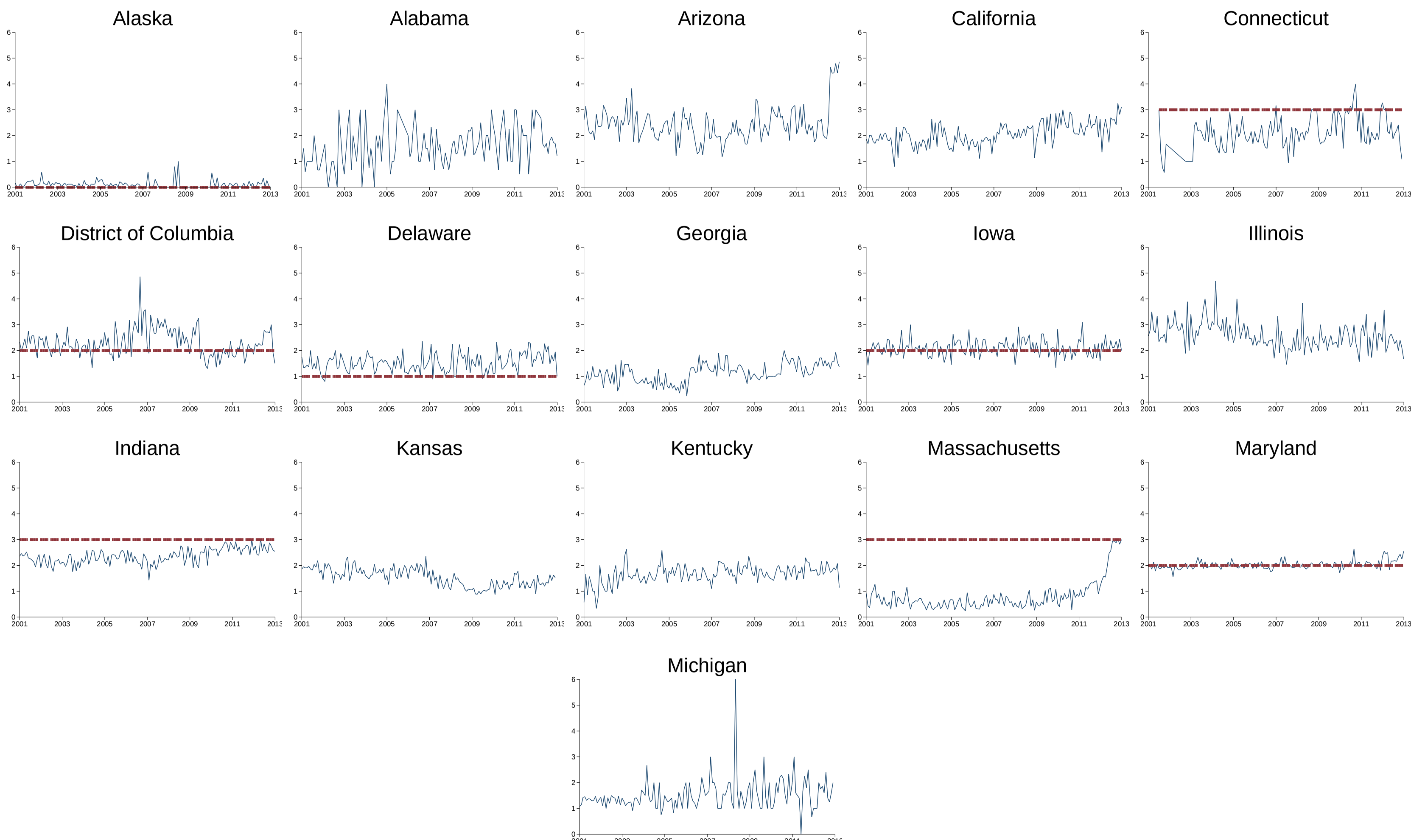


Notes: Figure indicates the share of UI claimants living under each specific search requirement from one employer contact per week to five employer contacts per week. Policies are identified using state workforce agency publications. Total claims under each policy are determined by summing continuing claims each month across states. Shares are then recovered by dividing by national claims. The lightest color closest to the horizontal axis represents one required employer contact per week. Each darker shade represents one additional required contact, up to five, which is visible at the upper right. The omitted group includes nonspecific policies, individualized policies, and zero-contact policies.

APPENDIX B

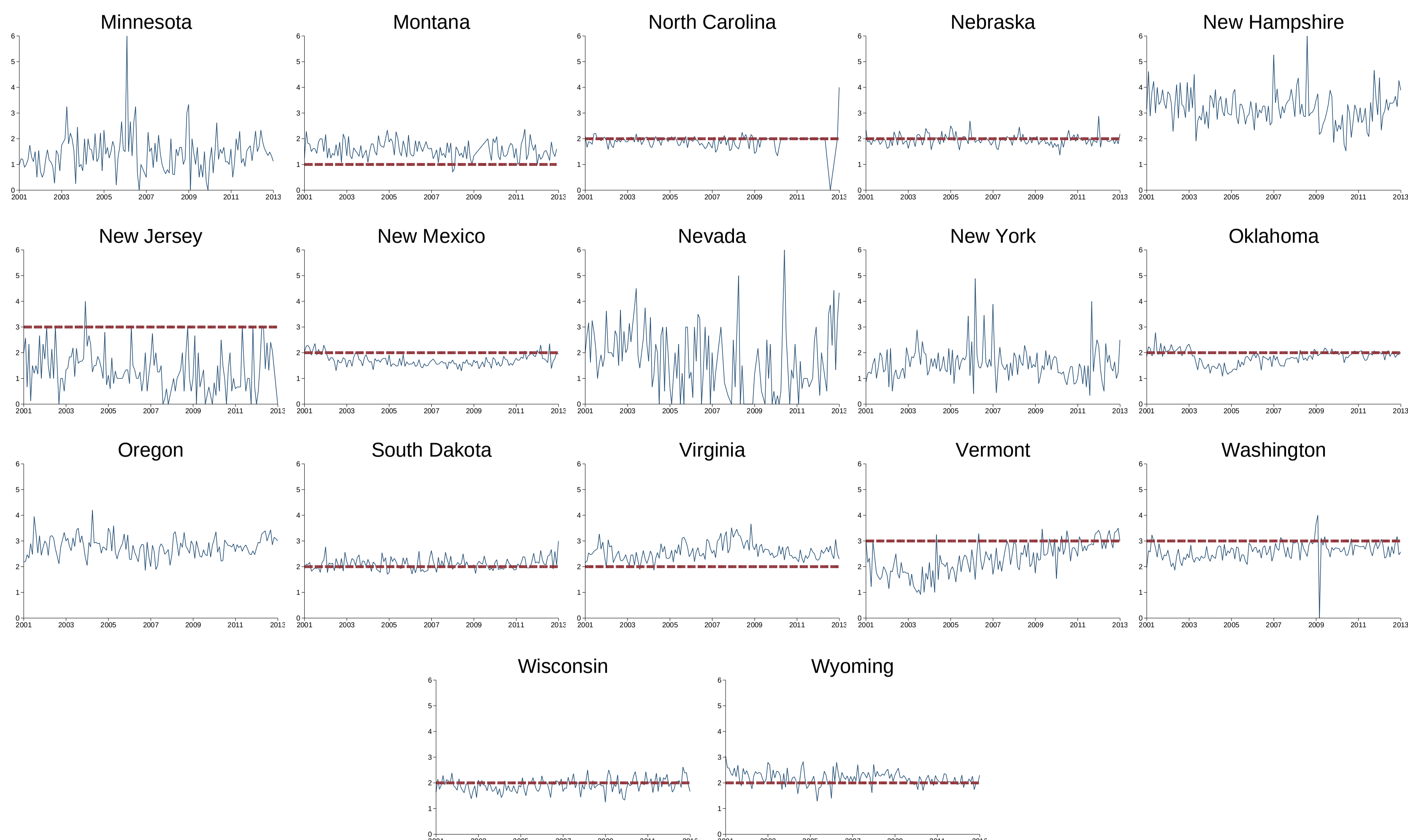
Average Reported Employer Contacts and Search Requirement Policies, States without Policy Changes

Figure B.1: Search Policy and Average Contacts Reported by State, states without policy changes in period of available BAM data, Alaska to Michigan



Notes: Thick dashed lines indicate search requirements as found in workforce agency publications. Any time without a dashed line indicates a nonspecific search policy. Thin solid lines show the monthly average number of employer contacts reported by audited claimants in the BAM data.

Figure B.2: Search Policy and Average Contacts Reported by State, states without policy changes in period of available BAM data, Minnesota to Wyoming

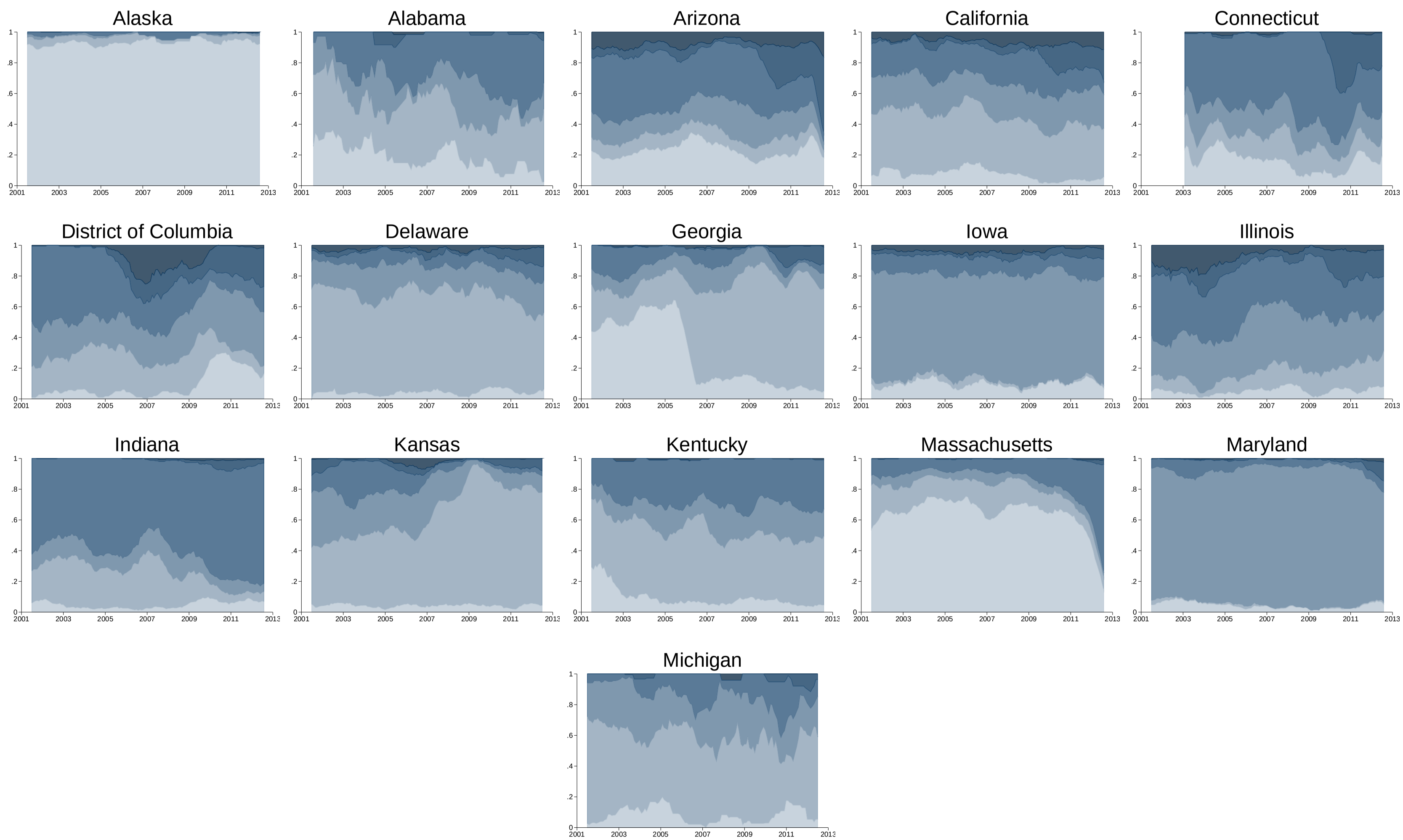


Notes: Thick dashed lines indicate search requirements as found in workforce agency publications. Any time without a dashed line indicates a nonspecific search policy. Thin solid lines show the monthly average number of employer contacts reported by audited claimants in the BAM data.

APPENDIX C

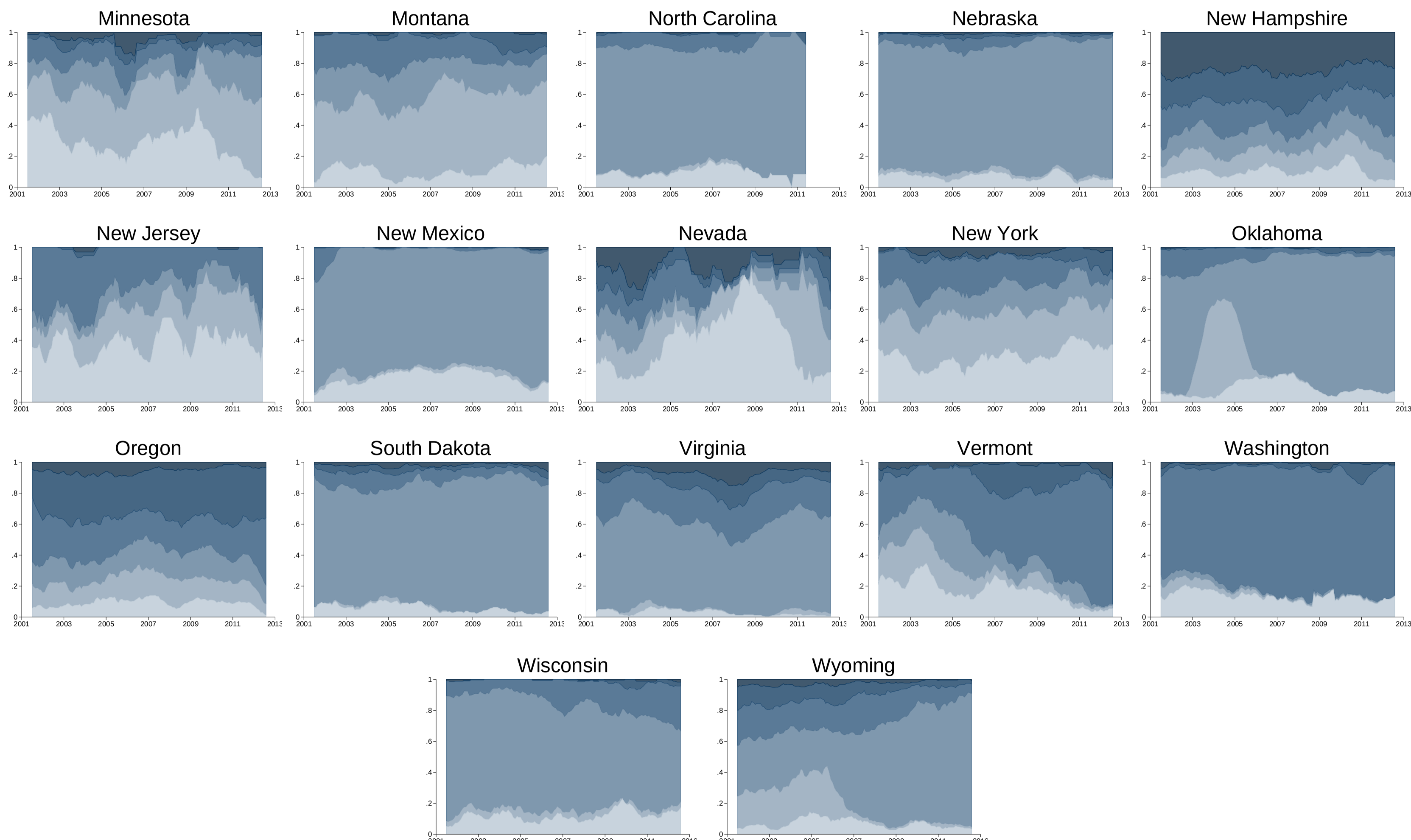
Distributions of Reported Employer Contacts

Figure C.1: Distribution of Contacts Reported by State, states without recorded policy changes in period of available BAM data, Alaska to Michigan



Notes: Graphs show the share of BAM claimants each month reporting each number of employer contacts. Employer contacts increase from zero to six-plus in increasing darkness.

Figure C.2: Distribution of Contacts Reported by State, states without recorded policy changes in period of available BAM data, Minnesota to Wyoming



Notes: Graphs show the share of BAM claimants each month reporting each number of employer contacts. Employer contacts increase from zero to six-plus in increasing darkness.

APPENDIX D

Survey of Income and Program Participation Analysis of the Added Worker Effect

I use the Survey of Income and Program Participation (SIPP) to compare estimates for the same models from the same data sources across relatively younger and relatively older workers. I employ specifications that are in line with recent work on displacements as in Davis and von Wachter (2011) and Flaaen et al. (2013). That is, I estimate models that are initially described by

$$y_{it}^m = \sum_{\tau=-k}^{l} (\beta_\tau^m D_{i,t-\tau}^m) + \alpha^m X_{it} + \delta_i^m + \gamma_t^m + \epsilon_{it}^m, \tag{D.1}$$

where m indexes the specific month in which a displacement takes place. This has the same general distributed-lag form as the main estimates in this paper, but constructs a different counterfactual. The treatment indicators $D_{i,t-\tau}^m$ equal to one only for the workers whose spouses were displaced in that calendar month. The counterfactual outcomes are constructed from all workers who were not displaced in that particular month. The estimates are therefore the effect of displacement in that month relative to not being displaced in that month. This is in contrast to the main estimates in the paper, which show the effects of displacement relative to never being displaced. The composition of the control group is more easily defined in this formulation: it does not require any restrictions on the displacement patterns of the control group workers outside of the month in question. As it is presented in equation (D.1), the coefficients could be estimated individually for each month of displacement. In practice, only a small number of spouses is displaced in any particular month, so I assume that the coefficients of interest are the same across values of m and pool the samples for every potential month of displacement. While there are surely differences in the actual effects of displacement at different time periods (as indicated by Davis and von Wachter 2011), pooling in this way simply recovers an average of the treatment effects.

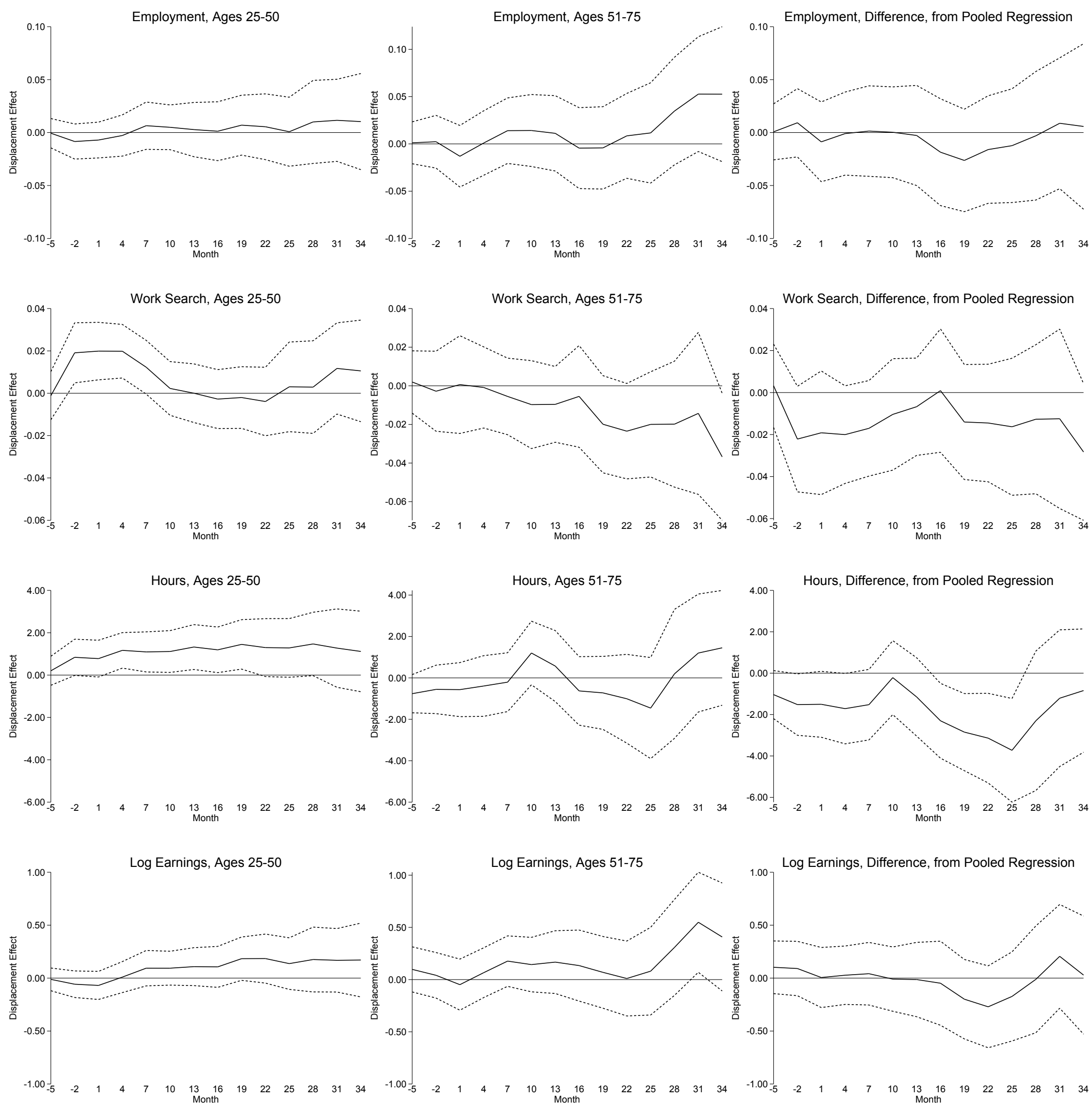
It is important to note that because different months of treatment are pooled, the same individual can appear many times in the estimation sample. A worker whose spouse is displaced in one month will be part of the treatment group for that particular m, but likely part of the control group for $m + 1$. In terms of statistical inference, this is accounted for via clustering at the level of the individual across all treatment months. Ultimately, I perform two-way clustering of standard errors, as in Cameron et al. (2011), also accounting for arbitrary correlation within actual calendar months of outcome, t.

I estimate these pooled models on a sample of SIPP respondents from the 1990, 1991, 1992, 1993, 1996, 2001, 2004, and 2008 SIPP panels. I focus on the response of women to their husband's displacements in an initial effort to draw comparisons to the existing literature. The SIPP is a relatively high-frequency survey that interviews respondents once every four months and collects information about their behavior and income during each of the intervening months. I identify displacements using responses to survey questions about whether the respondent is still working for an employer identified at the previous wave. Respondents who report no longer working for a previous-wave employer are asked for the reason. I mark the separation as a displacement if the respondent reported being "laid off" in the first four SIPP panels in use, or if they say the separation was due to "layoff," "employer bankruptcy," "employer sale of business," or "slack work or business conditions." I include in the sample of displacements only those who had at least one year of tenure at separation. This same one-year restriction applies to the control gorup as well. That is, the sample is made up of those who would have been identified as displaced and included in the treatment group if they had separated from an employer during that month for one of the given reasons.

Spouse separations are then linked to labor force outcomes for women in the panels. I estimate the effects of the spouse displacements on a woman's own probability of being employed at any point in a month, probability of reporting looking for work at any point in a month, total weekly hours worked, and log earnings. In addition to the various fixed effects and treatment indicators in the regressions, I control for a quadratic in age for the month in question, t. The estimates are reported in Figure D.1. The first two columns of graphs show the estimates for women ages 25-54 and 55-75, respectively. I then pool the samples and present the difference for the older group in the third column. All other coefficients are constrained to be the same across the age groups in the pooled sample.

The top left panel of Figure D.1 shows little employment response on the extensive margin for women under 55. The older sample similarly shows no effects that can be differentiated from zero, and the restricted pooled model suggests no difference between the two age groups. The second row appears to show that women in the young age group are more likely to be looking for work in the first six months immediately after a spouse displacement. This effect is positive and different from zero at the five percent level for this group. The pooled estimates also reject that the two groups' effects are the same at this horizon. Hours worked rise for the younger age group following a spouse displacement and remain consistently one to two hours higher over the entire horizon examined. This steady response is distinctly not observed for older women, with negative point estimates at a number of the horizons. The differences between the age groups is also marked and statistically significant, with the older group not showing the earnings increase at all. Earnings point estimates are positive over much of the period for both groups, but are not statistically significant at the five percent level and do not appear to differ from each other.

Figure D.1: SIPP Estimates of Added Worker Effects



Notes: Figures present the estimated effects of spouse displacement on labor force outcomes for women in the Survey of Income and Program Participation (SIPP). The sample and methods are described in Appendix D. Coefficients are pooled for three month intervals. The first month in each interval is indicated on the horizontal axis.

APPENDIX E

Logit Estimates for Propensity to Experience Spouse Displacement

Table E.1: Logit Estimates for Spouse Displacement in Sample Period

	Women			Men		
	Coeff	Bootstrap SE	Odds Ratio	Coeff	Bootstrap SE	Odds Ratio
Birth Month:						
Linear	−0.040	0.038	0.961	0.071	0.037	1.073
Quad.*10^{-2}	0.186	0.113	1.204	−0.209	0.113	0.811
Cubic*10^{-4}	−0.261	0.127	0.770	0.250	0.129	1.284
Sp Birth Month:						
Linear	0.001	0.003	1.001	0.000	0.004	1.000
Quad.*10^{-2}	−0.001	0.002	0.999	−0.001	0.004	0.999
Cubic*10^{-4}	0.002	0.002	1.002	0.002	0.002	1.002
Spouse Race:						
Black	−0.351	0.327	0.704	−0.564	0.312	0.569
Hispanic	0.279	0.263	1.322	−0.524	0.350	0.592
Sp Education:						
Less than HS	−0.938	0.271	0.391	−0.044	0.306	0.957
HS Grad	−0.530	0.222	0.588	0.213	0.224	1.237
Some College	−0.333	0.220	0.716	0.048	0.227	1.049
Education:						
Less than HS	0.388	0.306	1.474	0.171	0.228	1.187
HS Grad	0.154	0.250	1.166	−0.069	0.203	0.933
Some College	0.218	0.260	1.244	−0.314	0.219	0.730
Sp Occupation:						
Farm, Forest	−0.419	0.448	0.658	0.247	0.762	1.280
Sales, Adm.	−0.163	0.222	0.850	0.456	0.199	1.577
Mech., Prec.	−0.052	0.201	0.949	0.717	0.527	2.049
Services	−0.961	0.400	0.383	0.413	0.251	1.512
Operators	0.203	0.221	1.225	0.818	0.319	2.266
Census Region:						
Midwest	0.197	0.207	1.217	−0.035	0.204	0.966
South	0.079	0.214	1.082	−0.088	0.185	0.916
West	−0.022	0.239	0.978	0.256	0.208	1.292
In LF	0.209	0.258	1.232	−0.236	0.291	0.790
Spouse in LF	1.203	0.340	3.331	0.356	0.311	1.428
Sp Pr(Lose Job)						
Linear	0.090	0.103	1.094	0.023	0.078	1.023
Quad.	−0.010	0.013	0.990	0.002	0.010	1.002
Sp Pr(Find Job)						
Linear	0.136	0.069	1.146	0.187	0.070	1.205
Quad.	−0.006	0.007	0.994	−0.019	0.007	0.981
Pr(Lose Job)						
Linear	−0.110	0.122	0.896	0.028	0.099	1.028
Quad.	0.014	0.015	1.014	−0.012	0.015	0.988
Pr(Find Job)						
Linear	0.054	0.093	1.055	−0.082	0.066	0.921
Quad.	−0.009	0.010	0.991	0.007	0.007	1.008

continued...

Table E.1 continued

	Women			Men		
	Coeff	Bootstrap SE	Odds Ratio	Coeff	Bootstrap SE	Odds Ratio
Experience:						
Linear	0.005	0.021	1.005	−0.059	0.038	0.942
Quad.*10^{-2}	−0.016	0.048	0.984	0.146	0.068	1.157
Sp Experience:						
Linear	0.020	0.050	1.020	0.023	0.022	1.023
Quad.*10^{-2}	0.001	0.077	1.001	−0.029	0.051	0.971
Earnings:						
Linear*10^{-2}	−0.003	0.003	0.997	−0.002	0.001	0.998
Quad.*10^{-7}	0.011	0.012	1.011	0.001	0.001	1.001
Cubic*10^{-13}	−0.132	0.144	0.876	−0.002	0.002	0.998
Sp Earnings:						
Linear*10^{-3}	0.003	0.013	1.003	−0.001	0.022	0.999
Quad.*10^{-8}	0.002	0.026	1.002	0.029	0.080	1.030
Cubic*10^{-14}	−0.078	0.155	0.925	−0.154	0.792	0.857
Assets:						
Linear*10^{-4}	−0.012	0.013	0.988	0.010	0.016	1.010
Quad.*10^{-11}	0.051	0.371	1.052	−0.122	0.401	0.885
Cubic*10^{-18}	−0.006	2.506	0.994	0.336	5.150	1.399
Sp Pr(Work 62+):						
Linear	0.004	0.009	1.004	0.007	0.009	1.007
Quad.*10^{-2}	−0.003	0.008	0.997	−0.006	0.009	0.994
Sp Pr(Work 65+):						
Linear	−0.006	0.009	0.994	−0.006	0.009	0.994
Quad.*10^{-2}	0.004	0.009	1.004	0.006	0.010	1.006
Pr(Work 62+):						
Linear	−0.019	0.010	0.981	0.002	0.007	1.002
Quad.*10^{-2}	0.012	0.010	1.012	0.001	0.007	1.001
Pr(Work 65+):						
Linear	0.020	0.013	1.021	−0.005	0.007	0.995
Quad.*10^{-2}	−0.022	0.014	0.978	0.001	0.008	1.001
Yrs to Sp Rtrmnt:						
Linear	−0.010	0.038	0.990	0.037	0.030	1.037
Quad.*10^{-2}	−0.048	0.239	0.953	−0.155	0.128	0.857
Years to Rtrmnt:						
Linear	−0.013	0.048	0.987	−0.041	0.033	0.959
Quad.*10^{-2}	0.135	0.297	1.145	0.080	0.176	1.083
Sp Job Tenure:						
Linear	−0.001	0.021	0.999	−0.077	0.027	0.926
Quad.*10^{-2}	−0.037	0.056	0.964	0.142	0.091	1.152
Job Tenure:						
Linear	0.033	0.030	1.034	0.086	0.023	1.090
Quad.*10^{-2}	−0.046	0.085	0.955	−0.212	0.064	0.809
Constant	−3.638	1.006	0.026	−3.599	0.729	0.027

Notes: Table reports logit estimates for spouse displacement in the HRS as described in Section 3.5.4. The estimation sample is described in Table 3.1.

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