On the Efficiency of Progressive Social Insurance and a Novel Test for Posted Wages

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Abstract

We develop a novel test for and document evidence of posted wages in the market for low-income workers in the US. In the US, weekly unemployment insurance (UI) benefits are set as a percentage of past wages up to a cap. We show that, in a model with wage posting and random search, an increase in the progressivity of UI—due to either a ceteris paribus increase in the replacement rate or a decrease in the benefit cap—has a positive spillover effect on the job-finding rate of untreated workers in this policy environment. The spillover arises from a shift in firms' posted wages which generates a different opportunity set for workers. The impacted workers include those whose own benefits are unaffected and for whom the model implies equilibrium changes in reservation wages are infra-marginal. Using variation in state-level UI parameters over time and data from the Benefits Accuracy Measurement (BAM) program and the Survey of Income and Program Participation (SIPP), we identify such workers and find evidence of spillovers. Taken together, our results provide evidence of monopsony power in the market for the lowest paid and most cyclically sensitive labor. In addition, the model that we have empirically validated implies that progressive social insurance alleviates an inefficiency caused by monopsony wage-setting.

JEL Classifications:

J65:	Unemployment Insurance
J64:	Unemployment Models • Unemployment Duration
D62, H23:	Externality • Spillover
J42, D42:	Monopsony
D83:	Imperfect Information • Search Market Equilibrium

Keywords: Spillover, Externality, Information Friction, Monopsony, Wage Posting, Random Search, Unemployment Insurance

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1 Introduction

Economics offers little clarity on the interpretation of inequality, with different models offering radically different conclusions. For example, wage inequality may be a benign reflection of underlying differences in productivity—as in a Roy (1951) Model—or a reflection of search frictions as some workers are more or less lucky in the productive opportunities they find—as in a McCall (1970) Model.

This paper studies a particularly striking case: in a model with wage posting and random search (WPRS), ex-ante inequality can *cause* inefficiency. The intuition follows from noting that a firm's optimal wage choice depends not only on a single prospective employee's reservation wage and matching probability, but on the reservation wage and matching probability of all of her peers. If some peers have lower reservation wages, due to ex-ante heterogeneity, then some firms find it optimal to commit to lower wages than the prospective employee in question would accept, even if the match would have been productive.

This theoretical possibility has been documented at least as far back as Albrecht and Axell (1984), but, to date, limited empirical evidence of wage posting with random search has been documented.[1] The difficulty lies in eliciting information about search and wage setting strategies. Due to this complication, the evidence to-date has been derived from surveys, for example (Hall and Krueger, 2012). This paper offers a novel test for WPRS using *observational data*. Applying this test, we document evidence of WPRS in the market for low wage earners in the United States. The result implies that greater progressivity of social insurance would improve market efficiency by reducing dead weight loss associated with information frictions.

Two features of the design of unemployment insurance (UI) in the United States facilitate

[1] The contribution of Albrecht and Axell (1984) was multifaceted and the subsequent literature has focused on its seminal contribution to breaking the Diamond (1971) Paradox. As such, the subsequent literature has focused on attempts and failures at structural estimation of the contrition of search frictions to wage dispersion, beginning with the unsuccessful empirical application of Albrecht and Axell (1984) by Eckstein and Wolpin (1990). Effort to reconcile short search durations with large wage dispersion has been a decades long and ongoing saga (Hornstein, Krusell, and Violante, 2011). Meanwhile, the efficiency of progressively result fell to the background.

our novel test. First, UI benefits are set as a replacement rate of past earnings up to a maximum benefit cap. We show that a model of WPRS the presence of cap generates testable predictions for the job finding rate of workers who are *not directly* treated by policy changes affecting the cap or replacement rate. In particular, the model suggests that 1) raising the replacement rate while holding the cap fixed should increase the job finding rate of recipients of the cap and 2) increasing the cap should decrease the job finding rate workers with benefits "just shy" of the cap.[2]

Importantly, these predictions stand in contrast to those of main stream models of the labor market. Specifically, the Neoclassical model and directed search models predict positive effects on the job finding rate of the indirectly treated group in both experiments. The intuition is that an increase in the price of one type of labor leads to substitution toward the other. Meanwhile the Diamond-Mortensen-Pissaredes model predicts negative effects in both experiments, the intuition being that the increase in benefits makes vacancies, on average, less lucrative.[3] Landais, Michaillat, and Saez (2018) offers a model in which both effects may be present, but one must dominate. WE WILL SEE IN OUR EMPIRICAL RESULTS THAT THE DATA ARE NOT EASILY RECONCILED WITH Landais et al. (2018) EITHER. THIS DISCUSSION WILL FLOW MORE NATURALLY WHEN WE REORGANIZE THE PAPER TO LEAD WITH THE EMPIRICAL RESULTS ON SPILLOVERS.

Second, the parameters governing the replacement rate and cap are set at the state level and have varied over time. We document the state legislative records in detail from 1986 TO 2014 to identify the parameters of UI policy at the state level and 737 event studies—198

[2]We provide a rigorous definition of "just shy" that derives from observing that the benefit cap results in a distribution of reservation wages that has a mass point at its maximum.

[3]Our work is preceded by a long theoretical literature studying the efficiency of UI provision. Key contributions study the market distortions due to the effects of the *level* of UI benefits on job search incentives and on firm entry (Baily, 1978; Chetty, 2008; Acemoglu and Pischke, 1999; Acemoglu and Shimer, 1999a,b, 2000). In contrast, our work examines the efficiency implications of the *distribution* of UI benefits, as described by Albrecht and Axell (1984). Our empirical results confirm that unemployment is elevated by the *dispersion* of benefits induced by the assignment of UI benefits based on wage histories. While this dependence may increase efficiency by mitigating moral hazard—as in Baily (1978) and Chetty (2008)—and encouraging the entry of firms into low productivity labor markets—as in Acemoglu and Pischke (1999); Acemoglu and Shimer (1999a,b, 2000)—we show that the resulting dispersion is not without cost.

replacement rate changes and 639 cap changes. For each we identify a sample of only indirectly treated individuals—at the cap or "just shy" of the cap respectively—in the Survey of Income and Program Participation. This requires implementing a detailed unemployment insurance calculator and, in the case of the "just shy" sample, turning to administrative data from the Benefits Accuracy Measurement (BAM) program to learn about the distribution of peer's benefits. Analyzing these event studies using these data and both a linear probability and cox proportional hazard model of the job finding rate confirms both predictions of the model with WPRS and rules out the more mainstream competitors.

REWRITE THIS PARAGRAPH AND MOVE IT FORWARD IN THE INTRODUCTION In documenting equilibrium spillovers of unemployment insurance benefits, our paper also relates to an extensive literature aimed at estimating the sensitivity of a workers reemployment hazard to their own replacement rate. In particular, our analysis suggests that control group claimants who are not directly affected by UI policy changes are still affected through general equilibrium channels. These spillover effects are related to the search externalities considered by Davidson and Woodbury (1993), Lise, Seitz, and Smith (2004), Crépon, Duflo, Gurgand, Rathelot, and Zamora (2013), and Lalive, Landais, and Zweimüller (2015) but differ in that they operate specifically through changes in employer behaviors as opposed to changes in the relative competitiveness of unemployed agents.[4]

Confirming the presence of WPRS in the market for the labor of UI recipients in the US speaks to a long and ongoing debate. The WPRS market structure violates the Barro (1977) Critique, since productive matches remain unformed. Yet, despite this influential view, research into the impact of monopolistic wage posting on market functioning as flourished and acknowledged the harm that information frictions may impose (Manning, 2003). We have used particulars of the US UI system for identification; however, additional considera-

[4]In addition, our results suggest that identification strategies that exploit policy changes by using workers whose benefits are unaffected as a control group, such as Meyer and Mok (2014) and others documented in Krueger and Meyer (2002) are biased since the policy change induces a shift in the wage offer distribution, which effects the job finding rate of both treated and control groups. However, in contrast to the typical finding with respect to spillovers, the spillovers documented in this paper suggest that the results of studies like Meyer and Mok (2014) are attenuated rather than inflated.

tions—particularly the likely presence of moral hazard—muddies the efficiency implications of manipulating the dispersion induced through the wage-history-dependence of benefits in that system. That said the presence of WPRS has implications for other, equally important, sources of ex-ante inequality. Poverty provides a particularly clear cut example. Theoretical and empirical work has documented workers with assets have higher reservation wages (Card, Chetty, and Weber, 2007; Shimer and Werning, 2008). A particularly striking consequence of WPRS in this context is that the presence of poverty harms the slightly better off since some firms target their wage offers exclusively at the poor. In this clear cut example, the implication clear: progressive social insurance—transfers to the poor—could make *everyone* better off (Albrecht and Axell, 1984). Our results underscore that that these considerations apply to the real world and are not just a theoretical possibility.

Section **??** briefly describes the US UI system and the potential to identify spillovers that lies therein. Section 2 briefly describes the BAM and SIPP data and our measurement of the policy parameters. Section 3 describes how we measure an individuals likely UI benefits and how we assign individuals to samples of indirectly treated workers. Section 4 lays out the empirical model. Section 5 documents the positive spillover effects of progressivity and discusses how these can not be reconciled with main-stream models containing spillover effects. Section 6 lays out a parsimonious model of wage posting and random search in the context of the US UI system. Section 7 derives the model's testable implications. Section 8 maps objects in the theoretical model to objects we can measure in the data and rehashes our identification strategy with the samples refined explicitly correspond to the model and reconfirms our main empirical findings. Section 9 concludes.

2 Data

To test the predictions of the model we appeal to the SIPP—a workhorse data in this research area—and augment it with novel metrics derived from our detailed reading of State legislative records in conjunction with administrative data on UI claimants from the BAM.

2.1 Survey of Income and Program Participation

WE SHOULD SAY IT IS RUN BY DOL AND CITE THE DATASET/WHERE IT IS DOWNLOAD FROM.

Primarily, we examine job-finding rates using a sample of unemployment spells observed in the Survey of Income and Program Participation (SIPP), a series of nationally-representative short-panel surveys.[5] Following the guidance of Corollary 1, we test whether changes in the benefit cap or the replacement rate have measurable spillovers on the job-finding rates of the specified workers in this data who are not directly impacted. We perform this analysis using a sample of SIPP respondents who report separating from a job due to lay-off or business closing. We model job-finding rates for this sample using weekly employment histories in the SIPP as in Chetty (2008) while controlling for a rich set of demographics.

SIPP records income from employment, government transfers, and other sources on a WHAT frequency. However, non-random non-response is a known defect of the non-labor income data in the SIPP (**?**). Meanwhile, the labor-income data is widely considered to be of high quality (CITATION). To address this, we follow the literature and augment the SIPP with a calculation of UI benefits for each unemployed individual based on past labor earnings and the details of that respondents state UI policy at the time as described in this and the following section.

[5] Our main analysis uses data from the 1986 through 2008 panels of the SIPP.

2.2 Legislative Records

WE SHOULD SAY HOW WE OBTAINED THESE. AN APPENDIX TABLE WITH THE CITATION FOR EACH STATE-YEAR WOULD BE SUPER (BUT HOPEFULLY YOU HAVE AN RA WHO COULD PUT IT TOGETHER). I MIGHT BE ABLE TO FIND ONE.

Our approach requires us to identify the relevant workers—those who are at the cap or just shy of it—as well as chronicle the policy changes we use for identification. To those ends, we additionally document all state-level legislation concerning UI benefit formulas across all 50 states and the District of Columbia from 1986 through 2015. In addition to providing the state-level variation that underlies our identification strategy, this detailed reading of state policy allows us to calculate weekly benefits for individuals in the SIPP: in particular, allowing us to observe when an individual's benefits are at or just shy of the cap.

2.3 Benefits Accuracy Measurement Program

WE SHOULD SAY IT IS RUN BY DOL AND CITE THE DATASET/WHERE IT IS DOWNLOAD FROM/WHAT NEEDS TO BE DONE TO OBTAIN IT.

Finally, we supplement the SIPP and the legislative record with administrative data from the BAM program, which provides a weekly random sample of ongoing claims, in order to address two policy complications. First, although the benefit cap is a straightforward state-level variable, the $\tilde{b}$ threshold for identifying workers "just shy of the cap," as in Corollary 1, depends on the distribution of benefits in the claimant population. The BAM sample allows us to observe this distribution. Second, states differ in how they implement the prototypical benefit formula given by $b = \min\{c, \alpha\chi\}$, particularly in how they measure prior earnings (χ). Because these differences complicate direct comparisons of α, we generate a consistent measure of the replacement rate across all states and policy regimes. Our use of the BAM data to address these issues is described below.

3 Measurement

3.1 Weekly Unemployment Benefits for SIPP Respondents

As noted above, labor earnings data in the SIPP are of higher quality than data on respondent's income from other sources. To address this we take a strategy in line with the literature and calculate each job-losers unemployment benefits based on their prior earnings history and the details of their states UI laws at the time CITATION. At each interview, SIPP respondents report monthly earnings for each of the prior four months. We use these to calculate earnings by calendar quarter in the quarters prior to job loss, as calendar quarter earnings are the actual measures used in UI formulas. We then calculate benefits using the exact rules described in each states legislation at the time of the unemployment spell. In the most straightforward cases, this simply applies a fixed replacement rate to some function of the four quarters of earnings.

To validate our read of the legislative record and approach to UI income receipt, figure **??** Panel A illustrates the relationship between actual UI income and UI income calculated using earnings histories, both as observed in the BAM. The coefficient of correlation is X and with statistical significance Y. The tight fit indicates that our read of the state legislation and implementation as a UI calculator have high fidelity.

Ideally, workers would be observed for five full quarters prior to the quarter of job loss, allowing us to observe the same interval of earnings used by actual UI agencies. However, because these full base periods are not observed for most survey respondents prior to job separation, we restrict to observations where at least three months of earnings are observed and follow Chetty (2008) in assuming that these are representative of unobserved earnings.

3.2 Policy Parameters

This subsection describes details of the measurement of the major policy parameters of the prototypical benefit formula underlying our identification: the replacement rate (α) and the benefit cap (c).

3.2.1 Replacement Rate

As noted in Section 2, the interpretation of α in the benefit formula $b = \min\{c, \alpha\chi\}$, is complicated by differences in the measurement of prior earnings (χ). Policymakers have multiple levers affecting the actual benefits received by claimants below the cap. In addition to changes to the statutory replacement rate (e.g., changing a 0.47 replacement rate to 0.50), which directly impact the share of earnings replaced by UI, policymakers may also adjust the time period over which claimants' prior earnings levels are defined. For example, one benefit formula may replace 50 percent of weekly earnings in the highest-earning quarter of the base period while another benefit formula may replace 50 percent of earnings across all four quarters of the base period. For workers with stable employment and earnings, these two formulae will produce the same weekly benefit, but for workers with variable earnings histories, benefits will be lower under the latter policy.

In order to summarize the effective replacement rates produced by all of these policy levers, we calculate the effective empirical replacement rate ($\hat{\alpha}$) under each state policy regime. For each BAM observation with benefits below the cap, we calculate the individual's replacement rate as their weekly benefit amount divided by 1/52 of total base period earnings.[6] We average this measure across claimants both before and after each policy change, generating an effective empirical replacement rate for each policy regime. This approach also allows us to assign a summary replacement rate even in the presence of nonlinearities or other formula complications below the cap.

[6] All states report full base period earnings in the BAM data, whether this is the actual input into the benefit formula or not.

Figure 3 displays our calculated replacement rates for various states over time, highlighting the impacts of these different policy changes. For example, Kentucky used a consistent measure of earnings (full base period earnings) throughout the entire sample period: changes in $\hat{\alpha}$ are due entirely to changes in its statutory replacement rate. In contrast, North Carolina kept a consistent 0.50 replacement rate throughout, while changing the earnings definition. In particular, the state cut *effective* replacement rates in 2013 by changing the earnings definition from the highest quarter to the two most recent quarters.

3.2.2 Cap

While the measurement of the benefit cap is straightforward, state legislatures have taken multiple approaches to adjusting it over time. We categorize these approaches into legislated changes, in which the legislature writes the maximum amount directly into the state code, and automatic changes, wherein the legislature has defined a rule for determining the maximum (e.g., 50 percent of the state average weekly wage) and the time at which the unemployment agency should recalculate and implement the new maximum. In principle, if legislated policy changes are enacted regularly, they can create the exact same policy as any given automatic policy. In practice, automatic changes are small and regular, while legislated changes tend to be larger and more infrequent.

For the sake of comparison, we display example states' weekly benefit caps as shares of their quarterly average weekly wage in Figure 2.[7] The overlaid vertical lines identify changes in the nominal value of the cap, with solid lines indicating actively legislated changes and dotted lines indicating automated changes defined by existing legislation. California, in the upper left panel of the figure, has updated its cap relatively infrequently and always through direct legislation. The cap in Iowa, on the other hand, has been changed annually through small automatic updates.

[7] In constructing these measures, we use average weekly wages from the Quarterly Census of Employment and Wages (QCEW) so that we have a consistent measure of average wages across states and over time.

3.2.3 Environment in which Policies are Changed

Policy changes do not appear to occur at exceptionally outlying moments of the business cycle or in exceptionally outlying states: Table 5

3.3 Defining Samples of Indirectly Treated Workers

We agnostically define two samples of indirectly treated workers. Those "just below" the cap and those who are "capped". (NOTE, later the model will suggest a more precicely defined set of samples.)

NOTES

Below the cap = calculated benefits $\geq$ 75% of the cap and less than the cap.

- Robustness check: we test robustness to trimming workers whose benefits are calculated to be within 2.5% of the cap. These workers, if miss categorized could bias our coefficient down (further away from zero).

At the cap = calculated benefits greater than the cap but less than twice the cap.

END NOTES

Table 3 plausible that directly and indirectly treated workers compete for opportunities in the same labor market. NOTE! We don't need these to be as similar as Table 4 since the directly treated are NOT the control group.

Table 4 pre-treatment workers appear to be a good control group for post-treatment workers.

4 Empirical Tests

We employ two approaches in examining the spillover effects of changes to UI benefit formulas in survey data. First, we model the weekly reemployment hazard directly using linear probability models. This approach admits the inclusion of fixed effects for each policy change, allowing a straightforward comparison of weekly reemployment probabilities before and after. Second, we study the same effects using proportional hazard models, which make more efficient use of the spell data but, due to the model's nonlinearity, do not admit the same (large) number of fixed effects. In both cases, we restrict to unemployment spells beginning within one year (before or after) a state's policy change so as to identify effects explicitly off of these changes. Throughout, we examine a sample of spells for which the policy change has no direct effect on the workers' UI benefits, so the estimated effects are due entirely to changes in the market equilibrium.

4.1 Linear Reemployment Probability Model

We first model reemployment as a binary outcome for each claimant in each week. We assume that the probability of reemployment can be modeled linearly as

$$P(E_{int} = 1) = \beta_0 + \beta_1 \ln D_{in} + \mathbf{X}_{int}\boldsymbol{\beta}_2 + \gamma_t + \delta_n + \varepsilon_{int} \tag{4.1}$$

where E_{int} is an indicator for reemployment for individual i near policy change n at unemployment duration t and D_{in} is a policy parameter of interest prevailing for that individual: either the log maximum weekly benefit or the log replacement rate. For a given policy change, n, spells observed in the year before the change are assigned the pre-change value of D_{in} and spells observed after are assigned the new value.

Individual and state level controls are included in the vector $\mathbf{X}_{int}$, γ_t is a set of fixed effects for number of weeks of unemployment duration, and δ_n is a set of fixed effects for policy changes. The set of controls includes the individual's own replacement rate, the predicted job-finding rate in the state, and the set of demographic and prior-job characteristics described in the previous section. The γ_t effects allow the unemployment exit hazard to vary with unemployment duration and the δ_n effects allow for different exit probabilities across states and time.[8] Due to the δ_n effects, the β_1 parameter is identified off of differences in reemployment probabilities before and after each policy change, n, while controlling for an underlying hazard (γ_t) and individual characteristics. As this is a linear-log specification, we interpret β_1 as the effect of a 100 log point increase in D_{in} on the probability of reemployment in a week.

4.2 Proportional Hazard Model

We also model the reemployment hazard for individual i near policy change n at unemployment duration t using proportional hazard models of the form

$$\lambda_{in}(t) = \lambda_0(t) \exp\{\alpha_1 \ln D_{in} + \mathbf{X}_{int}\boldsymbol{\alpha}_2 + \mu_n\} \tag{4.2}$$

where $\lambda_0(t)$ is an unspecified baseline hazard.[9] As in the previous subsection, D_{in} is a policy parameter of interest and $\mathbf{X}_{int}$ is a set of controls. In principle, like δ_n in equation (4.1), μ_n can represent a full set of effects for each policy change, but due to the model's nonlinearity and the incidental parameters problem, they may not admit consistent estimation of α_1. For the sake of completeness, we report results using this full set of effects, but we also report results using simple state and year effects.[10] Taking logs of both sides of equation (4.2) yields a log-log relationship between the reemployment hazard and D_{in}, implying that α_1 represents the elasticity of reemployment with respect to the policy variables.

[8] In addition to a full set of fixed effects for every policy change, we also report specifications which use fixed effects for each state and year.

[9] We use Efron's method to handle tied failure times.

[10] In our specifications, this approach reduces the number of parameters by more than 200. While the state and year effects may also be subject to the incidental parameters problem, there are many more observations associated with each parameter than when individual policy change effects are included.

5 Results

Tables 6 and 7 report estimates of the effect of the benefit cap on job-finding rates for claimants with benefits below the cap. Table 6 reports estimates of β_1 in equation (4.1) for different samples and specifications. In each case, the outcome is a binary variable for reemployment in a given week and the reported coefficient is on the log benefit cap. Every observation is associated with a specific change in the cap and has a calculated UI benefit below the lesser of the two caps and above the greater of the $\tilde{b}$ values for the two cap regimes. As such, an estimate of -0.10 indicates that a 1 log point increase in the benefit cap decreases the probability of weekly reemployment by 0.001. On a sample average of 0.067, this corresponds to a decrease of 1.5 percent.

While the estimated magnitudes in Table 6 vary, the estimates are universally negative and most often statistically significant. With the inclusion of the full set policy fixed effects and individual-level controls, the estimated effect is still -0.21 at the average. While this is only statistically different from zero at the 5 percent level, the estimates suggest that the effect is driven by legislated policy changes, which show a larger, statistically significant effect. Column 3 suggests that a one percent increase in the cap decreases job-finding by 3.1 percent (0.0021 on an average base of 0.067) and column 4 indicates that this is largely driven by a 3.2 percent effect for the legislated change subsample (0.0024 on a base of 0.074 for this subsample). These marginal effects may appear shockingly large, but they are, in fact, in line with the small literature estimating the elasticity of labor supply to individual employers (Azar, Berry, and Marinescu, 2019; Bassier, Dube, and Naidu, 2020).

NOTE: THE QUOTED FIGURES ARE BALLPARK RIGHT BUT WE NEED TO DOUBLE CHECK THEM AGAINST THE FINAL TABLES.

COMMENT THIS OUT LATER!!!!!

Cox model estimates of the cap effects are directly interpretable as elasticities. Comparable estimates for the linear cap models are recovered with the following calculation:

1 log point change in cap effect is .01* coefficient divide this by initial job-finding rate

to get effect of 1pct change expressed as probability multiply by 100 to get approximate elasticity

Effect of 1 pct change in reprate in cox model is coefficient divided by pre-sample mean replacement rate .033/.75 This gives the implied elasticity at approx 4.4

Effect of 1 percent change in rep. rate in linear model is coefficient divided by pre-sample mean replacement rate .005/.75 divide this by initial job-finding rate (.081) to get effect expressed as probability multiply by 100 to get approximate elasticity 8.2

END THE COMMENTING! STOP COMMENTING!

The same negative effects are also observed in the proportional hazard estimates presented in Table 7. As noted in Section 4.2, these estimates represent the elasticity of the job-finding rate with respect to the benefit cap for those whose benefits are just below it. In the left half of the table, the estimated elasticities are all approximately -1.35, roughly consistent with the magnitudes of the linear probability models estimated on the same data. The estimates are universally larger with the inclusion of individual policy change effects in columns (3) and (4), but, as noted in Section 4.2, these estimates in particular may be suspect due to the incidental parameters problem. In any case, the pattern of larger and more statistically significant effects from legislated changes is preserved.

To alleviate concern that the effect may be driven by individuals who are measured to be near the cap but are actually at the cap (and therefore directly treated) we trim the sample to remove individuals with measured benefits within 2.5% of the cap. These are present in columns 5 and 6. In the trimmed sample we obtain similar point estimates in the linear model but with a loss of precision. Meanwhile, in the hazard model the coefficients remain the same and retain significance at the 1% level.

The estimated effects of changes to the replacement rate on job-finding for individuals at the benefit cap are reported in Tables 8 and 9. Here, the listed coefficients are on the average replacement rate for individuals below the cap, calculated as described in Section **??**. Columns 1 and 3 in each table report estimates using all changes to the replacement rate. However, large coincident cap changes are liable to confound the estimates since in this case individuals in the indirectly treated sample are also treated by a large increase in benefits via the change in cap and therefore are likely to have a decreased job finding rate. Columns 2 and 4 restrict to changes that do not coincide with a legislated cap change. While the full sample of replacement rate changes suggests a negative or zero effect on the indirectly treated sample, removing coicident and confounding changes in the benefit cap reveals a positive effect that is statistically significant in three out of four of our specifications. Column 2 in Table 8suggests that a one percent increase in the replacement rate raises the weekly job-finding rate by 0.002, or XX percent of the sample mean. When policy change fixed effects are included as in column 4, the estimated effect is a slightly larger 0.005 and becomes statistically significant at the 1% level. A similar pattern is recovered from the proportional hazard estimates in Table 9.

5.1 Discussion

The following table summarizes the predicted effects of canonical models and of the cutting-edge synthesis of these due to Landais et al. (2018) as well as our empirical results. Plainly, or results do not fit neatly into any existing set of predictions. Further, one might think to shoehorn results into this framework by positing that the average change in benefits is larger for a change in the cap than the replacement rate such that the wage effect dominates in the change in cap experiment but not in the change in replacement rate experiment. However, examining Table 5 reveals that only approximately one-third of UI recipients have capped benefits. This suggests the opposite balance of "rat-race effect" to "wage effect". Thus, we conclude that a new (or revitalized old) model is required to rationalize our findings.

	Δ Cap on "Just Below"	Δ Rep. Rate on "Capped"	Intuition
"Rat-race Effect" or Directed Search / Neoclassical Models	+	+	Increasing other's benefits reduces competition / firms shift toward employing the relatively cheaper labor
"Wage Effect" Diamond-Mortensen-Pissarides Model	-	-	On average workers are better insured, so on average posting a vacancy is less worthwhile.
Our Results	**-**	+	??

6 Model

Albrecht and Axell (1984) introduced a novel result regarding general equilibrium effects in wage posting models: a change in the value for nonemployment for one sub-population of the unemployed may change the job finding rate of another sub-population—whose flow value of nonemployment is constant—through a general equilibrium effect on the wage offer distribution. Here we extend the intuition of the result from their stylized setting with discrete valuations of non-employment to one in which valuations are continuous with a mass point at the maximum. Our setting approximates key features of the U.S. unemployment insurance system, which we will exploit to test for the presence of wage posting. In our setting we establish several results which, together, yield the empirically testable predictions: 1) an increase in the cap on unemployment insurance benefits decreases the job-finding rate of individuals below the cap. 2) a proportional increase in the generosity of benefits below the cap increases the job-finding rate of individuals at the cap. In each case, the change in the job-finding rate is for a sub-population whose benefits are unchanged.

6.1 Environment

Let $b = \min\{c, \alpha\chi\}$ be the potential unemployment benefit for a worker with earnings given by χ. We will investigate the implications of variation in α and c, policy parameters that we will call the replacement rate and the benefit cap. We assume that the cumulative distribution of χ in the population of low wage earners, $\Phi(\cdot)$, is twice differentiable and concave. For ease of exposition we take χ to be a permanent characteristic of the worker.[11]

Unemployed workers search sequentially and randomly for wage offers, w, from distribution $F(w)$, which is pinned down in equilibrium, and accept any job yielding higher value than continued search.[12] Let f be the Poisson arrival rate of job offers [assumed, for now, to be invariant to policy changes], δ be the Poisson arrival rate of exogenous job destruction, and r be the rate at which workers discount the future.

Infinitely patient firms, which produce using a constant returns to labor technology summarized by labor productivity p, post wages in order to maximize ex-ante expected profits. Productivity is heterogeneous across firms and exogenously and permanently assigned according to a twice differentiable and concave distribution $\Gamma(p)$.

Distributions $\Phi(\cdot)$ and $\Gamma(\cdot)$ and policy parameters c and α are common knowledge while the individual realizations of χ and p are private.

[11] In the U.S. UI systems χ is a function past wages. The assumption of constant χ can be interpreted as workers fully discounting the effect of future wages on future benefits. In addition, typically the base period—the interval of time over which a measure of past wages is calculated—is long relative to expected tenure on the first job out of unemployment and in addition is lagged at least a quarter. Typically, earnings are measured using one to four quarters of work history ending at least two quarters prior to job separation. Meanwhile, reemployment tenures are liable to be short for workers with wages low enough to quality for non-capped UI benefits. Thus, the effect on base period wages of any particular job is small for the population studied.

[12] For ease of exposition we assume that workers search for jobs that offer constant flow wages until exogenous separation. This is an innocuous assumptions. If later we want to add on-the-job search and generate a more dispersed wage distribution it is easy to have employers offer jobs with value equivalent to the job with a flat wage schedule that we are writing down here if we posit that wages are set on the job via Bertrand competition as in Postel-Vinay and Robin (2002).

6.2 Workers

As stated, workers draw wage offers from wage offer distribution $H(w)$—to be pinned down in equilibrium—and accept any which yield greater value than continued unemployment. We can write the discounted value of unemployment and employment:

$$rU(b) = b + f \int_{w_R(b)}^{\infty} [E(x) - U(b)]h(x)dx, \text{ and}$$
$$rE(w) = w + \delta[U(b) - E(w)],$$

where $h(\cdot)$ is the density of $H(\cdot)$. The first term of $rU(b)$ is the flow value enjoyed from benefit b. The second is the option value of receiving a wage offer in excess of reservation wage $w_R(b)$, which is pinned down in equilibrium. Meanwhile, the first term of $rE(w)$ is the flow value of wage w and the second is the option value of exogenous separation to unemployment. The simplicity of the second term depends on the assumption that χ is a permanent characteristic of the worker. Throughout our analysis we make this simplifying assumption.[13]

Manipulating the value of employment we have:

$$E(w) = \frac{w + \delta U(b)}{r + \delta}$$

And substituting into $rU(b)$ and manipulating:

$$rU(b) = \frac{b(r+\delta) + f \int_{w_R(b)}^{\infty} xh(x)dx}{r + \delta + f(1 - H(w_R(b)))}.$$

Finally, the reservation wage—for which $rU(b) = rE(p(b))$—is therefore implicitly defined

[13] An alternative assumption, which is more akin to these empirical evidence, is that workers' innovations in b due to employment in χ arrive with a Poisson hazard but that individuals fully discount any such innovations. This alternative introduces tedium but does not alter our results.

as:

$$w_R(b) = \frac{b(r+\delta) + f\int_{w_R(b)}^{\infty} xh(x)dx}{r+\delta+f(1-H(w_R(b)))}. \tag{6.1}$$

Note that $w_R(b)$ is a one-to-one mapping.

Finally let $G(b)$ and $J(w_R)$ be the distributions of benefits and reservation wages, respectively, among the unemployed. Noting that, in steady state, $\frac{r+\delta}{r+\delta+f[1-H(w_r(b(\chi)))]}$ and $\phi(\chi)$ are the unemployment rate and mass of of χ-type individuals, respectively, and making a change of variables we can write

$$G(b|H) = \begin{cases} \int_{\underline{b}}^{b} \frac{r+\delta}{r+\delta+f[1-H(w_R(x))]}\phi\left(\frac{x}{\alpha}\right)\frac{1}{\alpha}dx & \text{if } b < c \\ 1 & \text{if } b \geq c, \end{cases} \tag{6.2}$$

where the first case follows from recognizing that, in steady state, the flow into and out of unemployment with benefit b or less must balance. Meanwhile, since $J(w) = G(w_R^{-1}(w))$ and $\frac{dw_R}{db} = \frac{r+\delta}{r+\delta+f[1-H(w)]}$

$$J(w|H) = \begin{cases} \int_{\underline{w_R}}^{w} \phi\left(\frac{w_R^{-1}(z)}{\alpha}\right)\frac{1}{\alpha}dz & \text{if } w < w_R(c) \\ 1 & \text{if } w_R(c), \end{cases} \tag{6.3}$$

where $w_R^{-1}(\cdot)$ denotes the inverse of $w_R(\cdot)$. Given the concavity assumptions on $\Phi(\cdot)$ it is straightforward to show that under policy regime $\{c, \alpha\}$ the distribution $J(\cdot)$ is differentiable and concave on its interior.

COMMENT THESE OUT EVENTUALLY

$$j(w) = \frac{dJ}{dw} = \frac{\phi\left(\frac{w_R^{-1}(w)}{\alpha}\right)}{\alpha}$$

and

$$\frac{dj}{dw} = \frac{d^2J}{dw^2} = \frac{\frac{d\phi}{d\chi}\left(\frac{w_R^{-1}(w)}{\alpha}\right)}{\alpha^2}\frac{r+\delta+f[1-H(w)]}{r+\delta} < 0, \text{ since } \Phi \text{ is concave.}$$

END EVENTUAL COMMENT OUT

6.3 Firms

Turning to the firms. Each posts wages ex-ante to maximize expected profits:

$$w_P(p) = argmax_w\{(p-w)J(w)\},$$

where $(p-w)$ is the rent earned from hiring a worker at wage w and $J(w)$ is the probability that a randomly encountered unemployed worker has reservation wage less than or equal to w. Understanding $J(w)$ as the labor supply curve faced by the firm, the problem is simply that of a monopsonist whose control variable is the wage.

Since $J(\cdot)$ has a convexity at its maximum the profit maximizing solution is defined (impliciltly) peicewise:

$$w_P(p) \equiv \begin{cases} p - \frac{J(w_P(p)))}{j(w_P(p))} & \text{if } p - w_R(c) \leq (p-w)J(w)\,\forall\, w \quad (interior) \\ w_R(c) & \text{if } p - w_R(c) > (p-w)J(w)\,\forall\, w \quad (corner). \end{cases} \tag{6.4}$$

A few things need to be noted. First, it is straightforward to show that $w_P(p)$ is increasing in p if the solution is interior. Denoting $\tilde{p}$ as the least productive firm for which $(p-w)J(w) \leq (p-w_R(c))\,\forall\, w$ monotonicity of the interior solution implies $(\tilde{p}-\tilde{w}_P)J(\tilde{w}_P) = (\tilde{p}-w_R(c))$ where $\tilde{w}_P$ is defined as the $\tilde{p}$ firm's profit maximizing interior solution. Second, for all $p > \tilde{p}$

Diamond (1971) implies that the optimal posted wage of firms with $p \geq \tilde{p}$ and the reservation wage of workers with $b = c$ are both $w_R(c) = c$.

Now we can write $H(w)$, the equilibrium distribution of wage offers, as

$$H(w) = \begin{cases} \Gamma(w_P^{-1}(w)) & \text{if } w < \tilde{w}_P \\ \Gamma(\tilde{p}) & \text{if } \tilde{w}_P \leq w < c \\ 1 & \text{if } w = c \end{cases} \tag{6.5}$$

6.4 Equilibrium

Definition 1. *Equilibrium:*

- *Workers follow reservation wage rule 6.1.*
- *Firms follow wage setting rule 6.4.*
- *6.3 is distribution of reservation wages.*
- *6.5 is the distribution of posted wages.*

Sections 6.2 and 6.3, which prove existence and the monotonicity of $w_R(\cdot)$ and $w_P(\cdot)$, guarantee uniqueness. Given the monotonicity of $w_P(p)$ we can rewrite Equation 6.1 as

$$w_R(b) = \frac{b(r+\delta) + f\left[\int_{p_R}^{\tilde{p}} w_P(x)\gamma(x)dx + (1-\Gamma(\tilde{p}))c\right]}{r+\delta+f(1-\Gamma(p_R))}, \tag{6.6}$$

where $p_R \equiv w_P^{-1}(w_R(b))$ is the least productive firm posting wages that are acceptable to a worker with benefits equal to b. The option value now has two parts. The first is the option value of meeting a firm with $p \in [p_R, \tilde{p})$ and the second is the option value of meeting a firm with $p \geq \tilde{p}$. The option value of meeting any firm in the second category is the same since they all offer wages equal to c.[14]

[14] Definition 1 applies in and out of steady-state. In steady-state we can derive additional equilibrium

Equation 6.4 implies that the minimum productivity that posts wage offers acceptable to all the unemployed is

$$\tilde{p} = c + \frac{J(w_R(\tilde{b}))^2}{j(w_R(\tilde{b}))}. \tag{6.8}$$

Discussion

As stated above, Diamond (1971) guarantees that for $b = c$ all workers holding the same reservation wage, also equal to c. However, convexity of $J(w_R)$ at the cap implies that the interior solution associated with $\tilde{p}$ is strictly less than the cap: $\tilde{w}_R < c$. Define $\tilde{b}$ as the b for which $w_R(\tilde{b}) = \tilde{w}_P$. From this it is straightforward to see that while $w_R(b)$ continues to be increasing in b beyond $\tilde{b}$ these reservations are inframarginal: the only firms making wage offers acceptable to these workers are those posting $w = c$ and hiring everyone. This implies that the job finding rate is decreasing and the unemployment rate rising in b (in the cross-section) up until $\tilde{b}$ after which they are constant.

7 Comparative Statics

We consider two changes in the parameters of the UI system and show that each implies an equilibrium effect on an only indirectly treated sub-population.

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characteristics. Balance of flows into and out of unemployment for a worker of type b imply

$$\delta(1 - u(b))m(b) = u(b)m(b)f(1 - \Gamma(p_R(b))),$$

where $m(b)$ is the mass of workers in the economy with benefit b (under policy regime $\{c, \alpha\}$), $u(b)$ is the unemployment rate of such workers, and $f(1 - \Gamma(p_R(b)))$ is Poisson job finding rate of such workers. This implies that the steady-state unemployment rate of a worker with benefit b is

$$u(b) = \frac{\delta}{\delta + f(1 - \Gamma(p_R(b)))} \tag{6.7}$$

and the aggregate unemployment rate, u, is

$$u = \int_{\underline{b}}^{c} u(b)m(b)db.$$

Proposition 1. *All else equal, an increase in α (resp. c) increases (resp. decreases) the fraction of firms offering wages at or above the cap.*

Proof of Proposition 1 is provided in Appendix C.1. Here we provide intuition. For this, it is instructive to first understand the partial equilibrium effect that would occur if the functions $w_R(\cdot)$ and $w_P(\cdot)$ were independent of c and α. Figure 1 Panels A and B illustrate the partial equilibrium effect of a progressive change in the cap and the replacement rate, respectively. Solid black lines trace out the cumulative distribution $J(w)$ prior to the policy change, while black dashed lines trace $J(w)$ after the policy change. Firm's iso-profit curves appear in red and blue. Higher profits lie toward the northeast. Note that the firm's iso-profit curve is a hyperbola with an asymptote at p. In matching long-dash we plot the asymptotes for each firm.

In Panel A, the increase in the cap shifts the mass at the maximum of $J(w)$ to the right. After the increase in the cap the initially marginal firm (red) now strictly prefers to offer wages below the new cap. Meanwhile, we can trace out the isoprofit curve of a new marginal firm (blue) and observe that this firm is of greater productivity.

In Panel B, the increase in the replacement rate stretches $J(w)$ to the right on the support $[0, c]$ while the cap remains in place. After the increase in the replacement rate the initially marginal firm (red) now strictly prefers to offer wages at the cap. Meanwhile, we can trace out the isoprofit curve of a new marginal firm (blue) and observe, as in the scenario involving the reduction in the cap, that this firm is of lesser productivity.

These partial equilibrium examples are instructive for intuition; however, the change in $\tilde{p}$ that they suggest clearly belies the invariance of the functions $w_R(\cdot)$ and $w_P(\cdot)$. In Appendix C.1, we prove that under the concavity assumptions of $\Phi(\cdot)$ and $\Gamma(\cdot)$ the equilibrium shifts in $w_R(\cdot)$, $w_P(\cdot)$, $J(\cdot)$, and $H(\cdot)$ do not reverse the intuition gleaned from Figure 1.

Proposition 1 has the following corollary:

Corollary 1. *Policy changes have spillovers on the job finding rate of workers whose own benefits are not changed:*

1. *An increase in α increases the job finding rate of workers with benefits at the cap.*

2. *An increase in c decreases the job finding rate of worekrs with benefits "just shy of the cap", $b \in (\tilde{b}, c)$.*

Case 1 is straightforward. The shift in α increases the share of firms offering wages at the cap, as proved in Proposition 1. This increases the job-finding rate of workers with capped benefits. Obviously, these workers benefits are not changed by the change in the replacement rate, since they are capped.

Case 2 follows from noting that, while the change in c reservation wage of workers with benefits below the cap, the change is infra-marginal for workers just shy of the cap.[15] For these workers, the only acceptable wages from the wage offer distribution are $w = c$.

In addition to being empirically testable, 1 establishes implications that, together, contradict the Neoclassical model as well as search models with directed search and search models with random search and wages set ex-post. In particular, the Neoclassical and directed search models both imply that a cut in the benefit cap should *decrease* the job finding rate of workers just shy of the cap, since these workers have become comparatively more expensive. Meanwhile, random search models with wages set ex-post suggest that an increase in the replacement rate should *decrease* the job finding of workers at the cap since, on average, reservation wages have gone up and, therefore, vacancies are less productive and fewer are created.

8 Model Goggles

We test corollary 1 by estimating the specifications described in Section 4 a refined the sample of unemployment spells observed in the SIPP.

Corollary 1 establishes two testable implications of wage posting that we would like to

[15]Indeed, as an intermediate step in the proof of Proposition 1, we found that the sign of $\frac{dw_R(b)}{dc}$ is ambiguous, even for $\tilde{b}$.

submit to the data:

$$\left.\frac{df(b)}{d\alpha}\right|_{b=c} > 0 \qquad \text{and} \qquad \left.\frac{df(b)}{dc}\right|_{b\in(\tilde{b},c)} < 0.$$

Each of these requires us to examine outcomes for workers with either $b = c$ or $b \in (\tilde{b}, c)$. The first test remains identical to that present in Section 5. However, the second suggests refining our sample of "just below the cap" to match the models notion of "just shy of the cap".

8.0.1 Just Shy of the Cap

Defining the "just shy of the cap" sample requires us to find $\tilde{b}$ under each policy regime.

Equations 6.4 implies

$$w_R(\tilde{b}) = c - \frac{J(1-J)}{j}.$$

While we don't observe w_R or J, by noting that $\frac{dw_R(b)}{db} = \frac{r+\delta}{r+\delta+f(1-\Gamma(p(b)))}$ and changing variables we can restate this condition in terms of observables b and the distribution of b in the population of unemployed, $G(b)$:

$$\tilde{b} = c - \frac{G(1-G)}{g}. \tag{8.1}$$

Using BAM's random sample from the population of UI payments made in each state each week, we construct the empirical analogue of G on both sides of each policy change. To accomplish this, we focus on weeks within one year of each policy change and, in an effort to focus on steady states, ignore payments sampled in the first six months after a change. Thus, for example, for a state that changes its policy annually on July 1, we sample the pre period as January 1 to June 30 of the same year and the post period as January 1 to June 30 of the following year.

Within each pre and post period around a policy change, we take the observed share of claimants with the maximum weekly benefits as the size of the mass point at the cap. We then kernel-smooth the observed PMF of weekly benefit amounts for all benefits below the cap in order to generate g at all lower values. We combine the kernel-smoothed g and the mass point at the cap to construct the corresponding CDF, G. We find $\tilde{b}$ as the b value that minimizes the difference between the two sides of equation 8.1. Figure 4 displays the empirical CDF and corresponding isoprofit curve at $\tilde{b}$ for Washington, DC in 1991 when the cap was \$293. The dashed vertical line displays the asymptote of the given isoprofit curve at the value $\tilde{p} = 389$. Using this approach, we find the implied $\tilde{b}$ values before and after each policy change.

8.1 Results with Model Goggles On

Tables 10 and 11 reveal results that are broadly similar to Tables 6 and 7 and slightly more robust.

9 Conclusion

We develop in this paper a test for wage posting in the market for unemployed workers in the US. The test leverages the feature of the UI system that creates a mass point at the maximum weekly benefit. We show that a wage posting model is unique in predicting that an increase in the progressivity of UI benefits, either through an increase in the replacement rate below the cap or a decrease in the cap, will increase the job-finding rate for some workers whose benefits are not directly impacted by the change. The prediction is validated using reduced form models of job-finding in survey data.

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A Tables

Table 1: Percentiles of Log Cap Change Distributions

	All Changes	Legislated	Automatic
1st	−0.08	−0.15	−0.01
5th	0.01	−0.03	0.01
10th	0.02	0.01	0.02
25th	0.03	0.04	0.03
50th	0.04	0.06	0.04
75th	0.06	0.10	0.05
90th	0.10	0.15	0.08
95th	0.12	0.19	0.09
99th	0.20	0.32	0.11
N	1,228	347	881

Notes: Sample of documented state-level UI policy changes between 1986 and 2015. "Legislated" indicates actively legislated changes to the maximum weekly benefit amount, while "automatic" indicate changes triggered automatically by existing legislation.
Source: Authors' legislative research, Benefit Accuracy Measurement (BAM) program data, and US Department of Labor's "Significant Provisions of State UI Laws."

Table 2: Coincidence of Policy Changes

	Replacement Rate Change:	
	No	Yes
Cap Change:		
None		74
Automatic	684	197
Legislated	195	152

Notes: Sample of documented state-level UI policy changes between 1986 and 2015. "Legislated" indicates actively legislated changes to the maximum weekly benefit amount, while "automatic" indicate changes triggered automatically by existing legislation.
Source: Authors' legislative research, Benefit Accuracy Measurement (BAM) program data, and US Department of Labor's "Significant Provisions of State UI Laws."

Table 3: Sample Means: Individuals Observed in Unemployment in the SIPP

Panel A: Policy Changes to the Benefit Cap						
Sample:	Just Shy of the Cap (Indirectly Treated)			At the Cap (Directly Treated)		
	All	Legislated	Automatic	All	Legislated	Automatic
Male	0.57	0.57	0.57	0.64	0.62	0.64
Age	38.9	38.3	39.1	39.8	38.7	40.3
Years of education	13.2	12.9	13.3	13.7	13.4	13.9
Weekly job-finding rate	0.068	0.082	0.064	0.086	0.088	0.086
Individuals	4,497	1,213	3,284	6,465	2,094	4,371
Policy changes	639	152	487	664	162	502

Panel B: Policy Changes to the Replacement Rate				
Sample:	Just Shy of the Cap (Directly Treated)		At the Cap (Indirectly Treated)	
	All	Excluding Coincident Legislated Cap Changes	All	Excluding Coincident Legislated Cap Changes
Male	0.58	0.57	0.65	0.68
Age	38.7	38.6	39.7	40.2
Years of education	13.1	13.1	13.6	13.6
Weekly job-finding rate	0.074	0.071	0.082	0.081
Individuals	1,763	1,264	2,480	1,603
Policy changes	186	128	198	136

Notes: "Cap Change" sample comprises claimants just shy of the cap: individuals with calculated UI benefits between $\tilde{b}$ and c. "Rep. Rate Change" sample comprises claimants at the cap: individuals with calculated UI benefits at c. In both cases, c is defined as the lesser of the two caps when there is a cap change. The sample sizes for the legislated and automatic cap change samples sum up to more than the full sample because a small number of individuals appear at least once in each of those samples.

Source: Census Bureau's Survey of Income and Program Participation 1986 to 2008 panels, authors' legislative research, and authors' calculations.

Table 4: Sample Means: Indirectly-Treated Individuals Observed in Unemployment in the SIPP

Panel A: Policy Changes to the Benefit Cap

Sample:	Pre-Change			Post-Change		
	All	Legislated	Automatic	All	Legislated	Automatic
Male	0.58	0.57	0.58	0.57	0.57	0.57
Age	38.9	38.5	39.0	38.7	38.1	39.0
Years of education	13.2	12.9	13.3	13.2	12.9	13.3

Panel B: Policy Changes to the Replacement Rate

Sample:	Pre-Change		Post-Change	
	All	Excluding Coincident Legislated Cap Changes	All	Excluding Coincident Legislated Cap Changes
Male	0.66	0.67	0.64	0.68
Age	39.6	40.2	39.7	40.2
Years of education	13.7	13.7	13.6	13.6

Notes: "Cap Change" sample comprises claimants just shy of the cap: individuals with calculated UI benefits between $\tilde{b}$ and c. "Rep. Rate Change" sample comprises claimants at the cap: individuals with calculated UI benefits at c. In both cases, c is defined as the lesser of the two caps when there is a cap change. The sample sizes for the legislated and automatic cap change samples sum up to more than the full sample because a small number of individuals appear at least once in each of those samples.

Source: Census Bureau's Survey of Income and Program Participation 1986 to 2008 panels, authors' legislative research, and authors' calculations.

Table 5: Sample Means: Policy Changes

Panel A: Policy Changes to the Benefit Cap			
	All	Legislated	Automatic
Share of claimants at cap	0.35	0.43	0.32
Initial Cap over Average Weekly Wages	0.51	0.42	0.53
Δ Cap (percent)	4.44	7.10	3.61
Initial Monthly Job-finding Rate (CPS)	0.488	0.530	0.475
Deviation from State Trend	-0.007	0.0003	-0.009
Deviation from Contemp. National Average	0.019	0.023	0.018
Policy Changes	639	152	487

Panel B: Policy Changes to the Replacement Rate		
	All	Excluding Coincident Legislated Cap Changes
Share of claimants at cap	0.34	0.30
Initial Replacement Rate	0.745	0.744
Δ Rep. Rate	-0.003	-0.005
Initial Monthly Job-finding Rate (CPS)	0.487	0.479
Deviation from State Trend	-0.015	-0.023
Deviation from Contemp. National Average	0.025	0.026
Policy Changes	198	136

Notes: "Cap Change" sample comprises claimants just shy of the cap: individuals with calculated UI benefits between $\tilde{b}$ and c. "Rep. Rate Change" sample comprises claimants at the cap: individuals with calculated UI benefits at c. In both cases, c is defined as the lesser of the two caps when there is a cap change. The sample sizes for the legislated and automatic cap change samples sum up to more than the full sample because a small number of individuals appear at least once in each of those samples.

Source: Census Bureau's Survey of Income and Program Participation 1986 to 2008 panels, authors' legislative research, and authors' calculations.

Table 6: Effect of an Increase in the Log Benefit Cap on Weekly Reemployment Probability of Workers with Benefits Just Below the Cap

	(1)	(2)	(3)	(4)	(5)	(6)
Log Benefit Cap						
All Changes	-0.05		-0.21^{**}		-0.19	
	(0.03)		(0.09)		(0.12)	
Legislated		-0.04		-0.24^{***}		-0.20^{*}
		(0.03)		(0.08)		(0.11)
Automatic		-0.06^{*}		-0.17		-0.17
		(0.03)		(0.19)		(0.23)
Observations						
Individuals	4,497	4,497	4,497	4,497	4,013	4,013
Policy Changes (Leg; Auto)	639	152; 487	639	152; 487	623	151; 472
R-squared	0.13	0.13	0.16	0.16	0.16	0.16
Controls	X	X	X	X	X	X
State & Year FE	X	X	X	X	X	X
Policy change FE			X	X	X	X

Notes: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$. Standard errors clustered at the state level appear below each estimate in parentheses. Workers are "just shy of the cap" if their calculated weekly benefit is at least as large as $\tilde{b}$ and less than the cap.

Controls: All models include a set of controls comprising the predicted job-finding rate in the state-month, a quadratic in age, and indicators for, male, Black, four education groups, reported reason for separation, six broad occupation categories and 14 broad industry categories.

Source: Census Bureau's Survey of Income and Program Participation 1986 to 2008 panels, authors' legislative research, and authors' calculations.

Table 7: Effect of an Increase in the Log Benefit Cap on Reemployment Hazard of Workers with Benefits Just Below the Cap

	(1)	(2)	(3)	(4)	(5)	(6)
Log Benefit Cap						
All Changes	-0.65^{*}		-3.79^{***}		-3.89^{***}	
	(0.37)		(1.03)		(1.31)	
Legislated		-0.54		-3.86^{***}		-4.03^{***}
		(0.40)		(0.84)		(1.13)
Automatic		-0.86^{**}		-3.67		-3.69
		(0.40)		(2.25)		(2.57)
Observations						
Individuals	4,497	4,497	4,497	4,497	4,013	4,013
Policy Changes (Leg; Auto)	639	152; 487	639	152; 487	623	151; 472
Controls	X	X	X	X	X	X
State & Year FE	X	X	X	X	X	X
Policy change FE			X	X	X	X

Notes: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$. Reported coefficients are estimated via Cox proportional hazard models for reemployment observed at weekly frequency. Standard errors clustered at the state level appear below each estimate in parentheses. Workers are "just shy of the cap" if their calculated weekly benefit is at least as large as $\tilde{b}$ and less than the cap.

Controls: All models include a set of controls comprising the predicted job-finding rate in the state-month, a quadratic in age, and indicators for, male, Black, four education groups, reported reason for separation, six broad occupation categories and 14 broad industry categories.

Source: Census Bureau's Survey of Income and Program Participation 1986 to 2008 panels, authors' legislative research, and authors' calculations.

Table 8: Effect of an Increase in the Replacement Rate on Weekly Reemployment Probability of Workers with Capped Benefits

	(1) All Changes	(2) No Cap Leg.	(3) All Changes	(4) No Cap Leg.
Replacement Rate	−0.001	0.002	0.001	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
Observations				
Individuals	2,480	1,603	2,480	1,603
Policy Changes	198	136	198	136
R-squared	0.17	0.17	0.19	0.20
Controls	X	X	X	X
State & Year FE	X	X	X	X
Policy change FE			X	X

Notes: $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$. Standard errors clustered at the state level appear below each estimate in parentheses.

Controls: All models include a set of controls comprising the predicted job-finding rate in the state-month, a quadratic in age, and indicators for, male, Black, four education groups, reported reason for separation, six broad occupation categories and 14 broad industry categories.

Source: Census Bureau's Survey of Income and Program Participation 1986 to 2008 panels, authors' legislative research, and authors' calculations.

Table 9: Effect of an Increase in the Replacement Rate on Reemployment Hazard of Workers with Capped Benefits

	(1) All Changes	(2) No Cap Leg.	(3) All Changes	(4) No Cap Leg.
Replacement Rate	-0.010^{*}	0.016^{*}	0.006	0.033^{**}
	(0.006)	(0.009)	(0.015)	(0.016)
Observations				
Individuals	2,480	1,603	2,480	1,603
Policy Changes	198	136	198	136
Controls	X	X	X	X
State & Year FE	X	X	X	X
Policy change FE			X	X

Notes: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$. Reported coefficients are estimated via Cox proportional hazard models for reemployment observed at weekly frequency. Standard errors clustered at the state level appear below each estimate in parentheses.

Controls: All models include a set of controls comprising the predicted job-finding rate in the state-month, a quadratic in age, and indicators for, male, Black, four education groups, reported reason for separation, six broad occupation categories and 14 broad industry categories.

Source: Census Bureau's Survey of Income and Program Participation 1986 to 2008 panels, authors' legislative research, and authors' calculations.

Table 10: Effect of an Increase in the Log Benefit Cap on Weekly Reemployment Probability of Workers with Benefits Just Shy of the Cap

	(1)	(2)	(3)	(4)	(5)	(6)
Log Benefit Cap						
All Changes	-0.08^{**}		-0.19^{**}		-0.20^{*}	
	(0.04)		(0.09)		(0.10)	
Legislated		-0.08^{*}		-0.22^{**}		-0.22^{**}
		(0.04)		(0.09)		(0.10)
Automatic		-0.08^{**}		-0.16		-0.15
		(0.04)		(0.20)		(0.24)
Observations						
Individuals	4,212	4,212	4,212	4,212	3,729	3,729
Policy Changes (Leg; Auto)	623	150; 473	623	150; 473	603	147; 456
R-squared	0.14	0.14	0.17	0.17	0.17	0.17
Controls	X	X	X	X	X	X
State & Year FE	X	X	X	X	X	X
Policy change FE			X	X	X	X

Notes: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$. Standard errors clustered at the state level appear below each estimate in parentheses. Workers are "just shy of the cap" if their calculated weekly benefit is at least as large as $\tilde{b}$ and less than the cap.

Controls: All models include a set of controls comprising the predicted job-finding rate in the state-month, a quadratic in age, and indicators for, male, Black, four education groups, reported reason for separation, six broad occupation categories and 14 broad industry categories.

Source: Census Bureau's Survey of Income and Program Participation 1986 to 2008 panels, authors' legislative research, and authors' calculations.

Table 11: Effect of an Increase in the Log Benefit Cap on Reemployment Hazard of Workers with Benefits Just Shy of the Cap

	(1)	(2)	(3)	(4)	(5)	(6)
Log Benefit Cap						
All Changes	-1.05^{**}		-3.69^{***}		-3.94^{***}	
	(0.41)		(1.00)		(1.20)	
Legislated		-1.08^{**}		-3.57^{***}		-4.00^{***}
		(0.49)		(1.06)		(1.27)
Automatic		-1.02^{**}		-3.87^{*}		-3.85
		(0.44)		(2.18)		(2.61)
Observations						
Individuals	4,212	4,212	4,212	4,212	3,729	3,729
Policy Changes (Leg; Auto)	623	150; 473	623	150; 473	603	147; 456
Controls	X	X	X	X	X	X
State & Year FE	X	X	X	X	X	X
Policy change FE			X	X	X	X

Notes: $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$. Reported coefficients are estimated via Cox proportional hazard models for reemployment observed at weekly frequency. Standard errors clustered at the state level appear below each estimate in parentheses. Workers are "just shy of the cap" if their calculated weekly benefit is at least as large as $\tilde{b}$ and less than the cap.

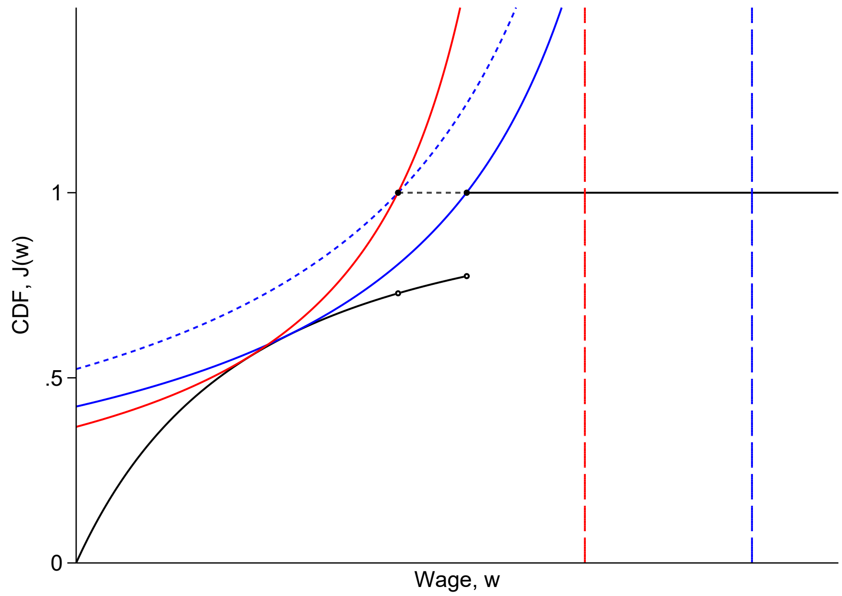
Controls: All models include a set of controls comprising the predicted job-finding rate in the state-month, a quadratic in age, and indicators for, male, Black, four education groups, reported reason for separation, six broad occupation categories and 14 broad industry categories.

Source: Census Bureau's Survey of Income and Program Participation 1986 to 2008 panels, authors' legislative research, and authors' calculations.

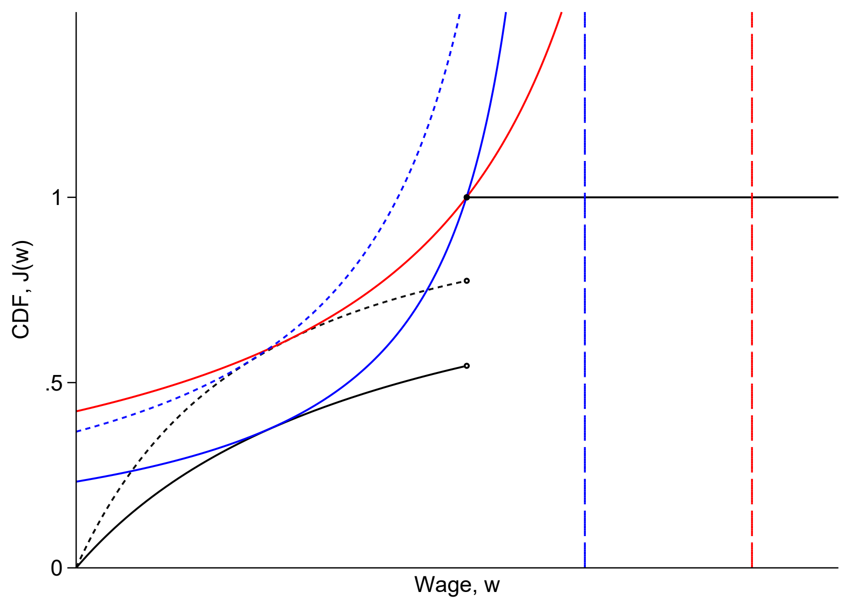
B Figures

Figure 1: Partial Equilibrium Effects of Policy Changes

Panel A: An Increase in the Benefit Cap

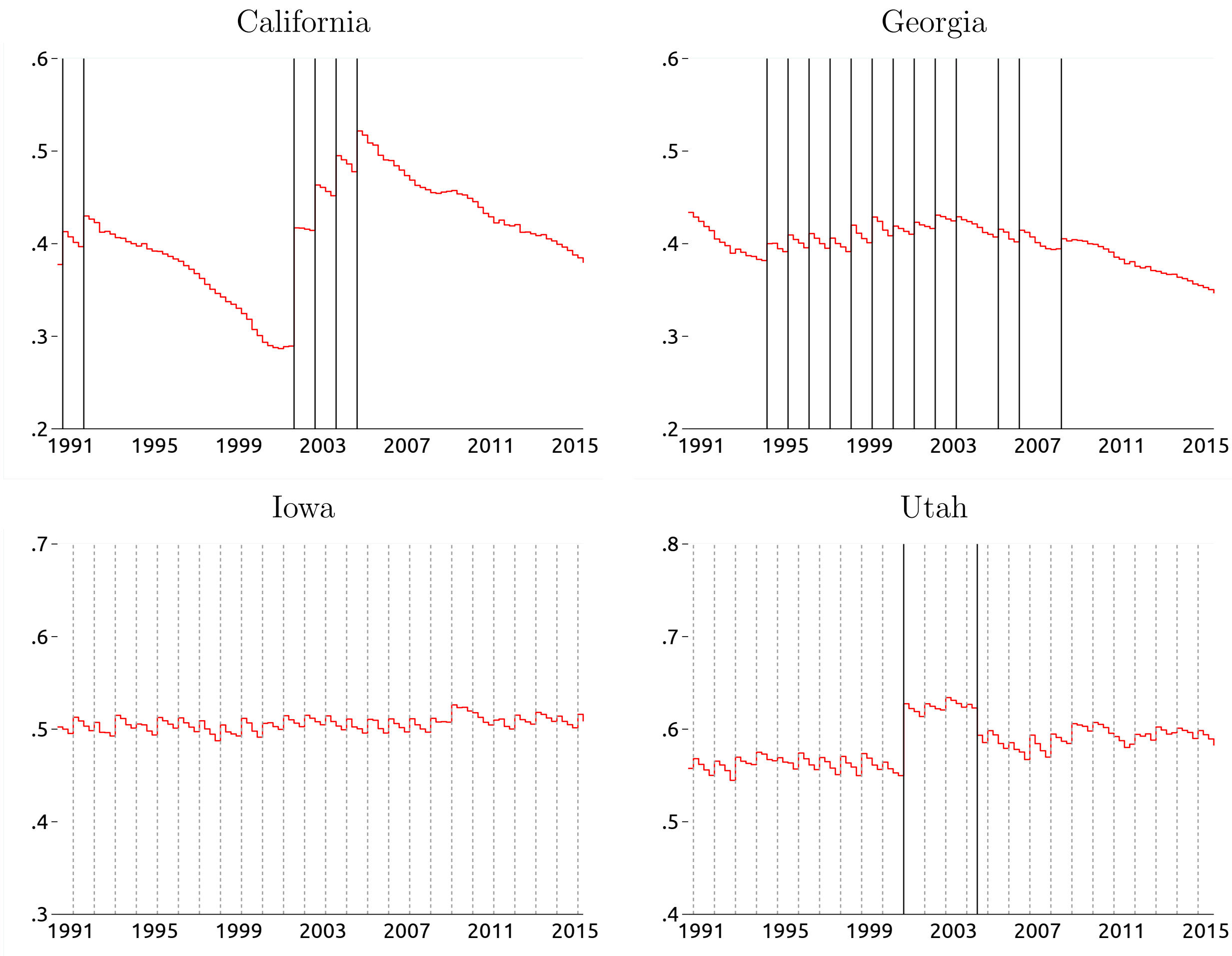


Panel B: An Increase in the Replacement Rate



Notes:

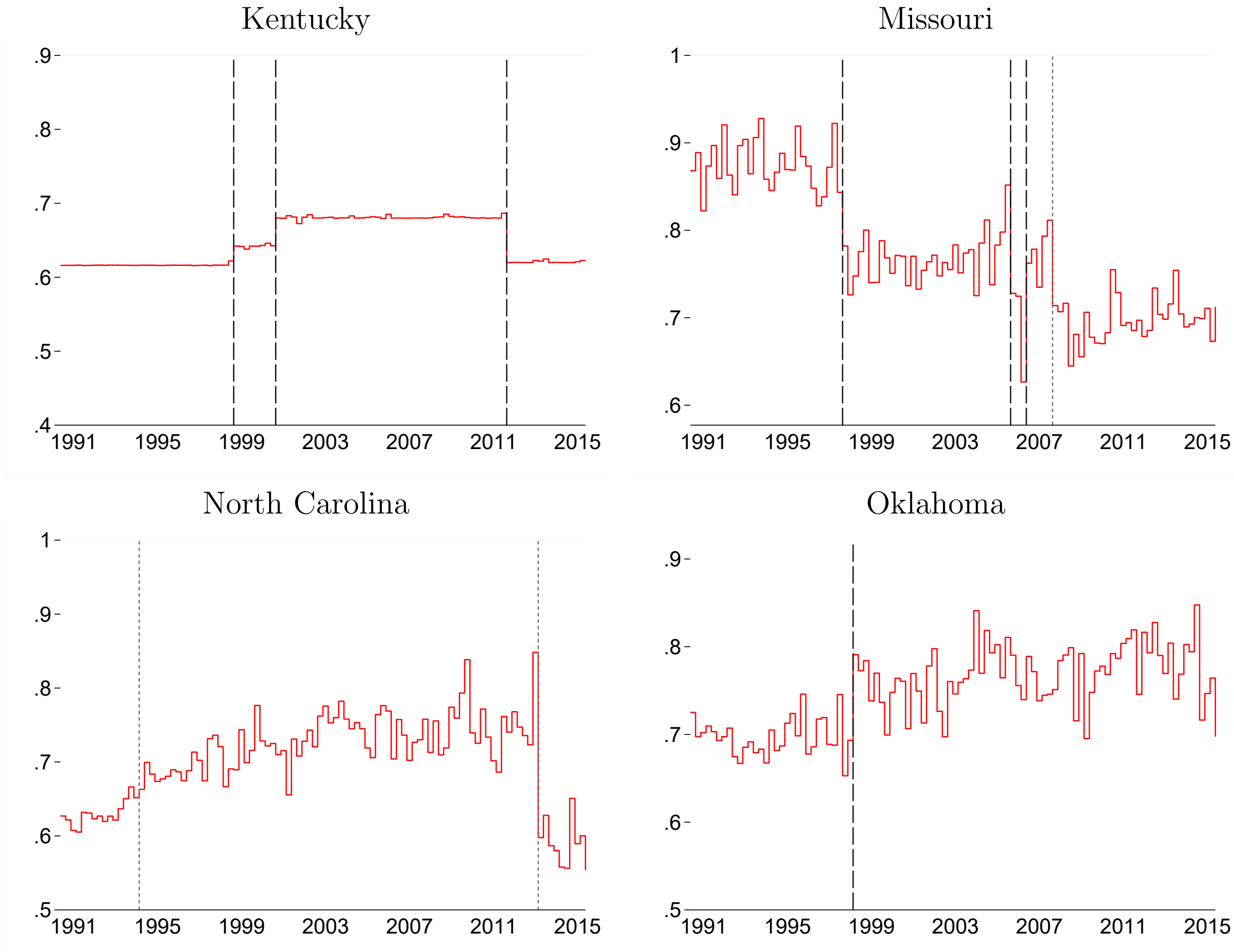
Figure 2: State Maximum Weekly Benefits as a Share of Average Weekly Wages



Notes: Solid bars indicate actively legislated changes to the maximum weekly benefit amount. Dotted bars indicate changes triggered automatically by existing legislation.

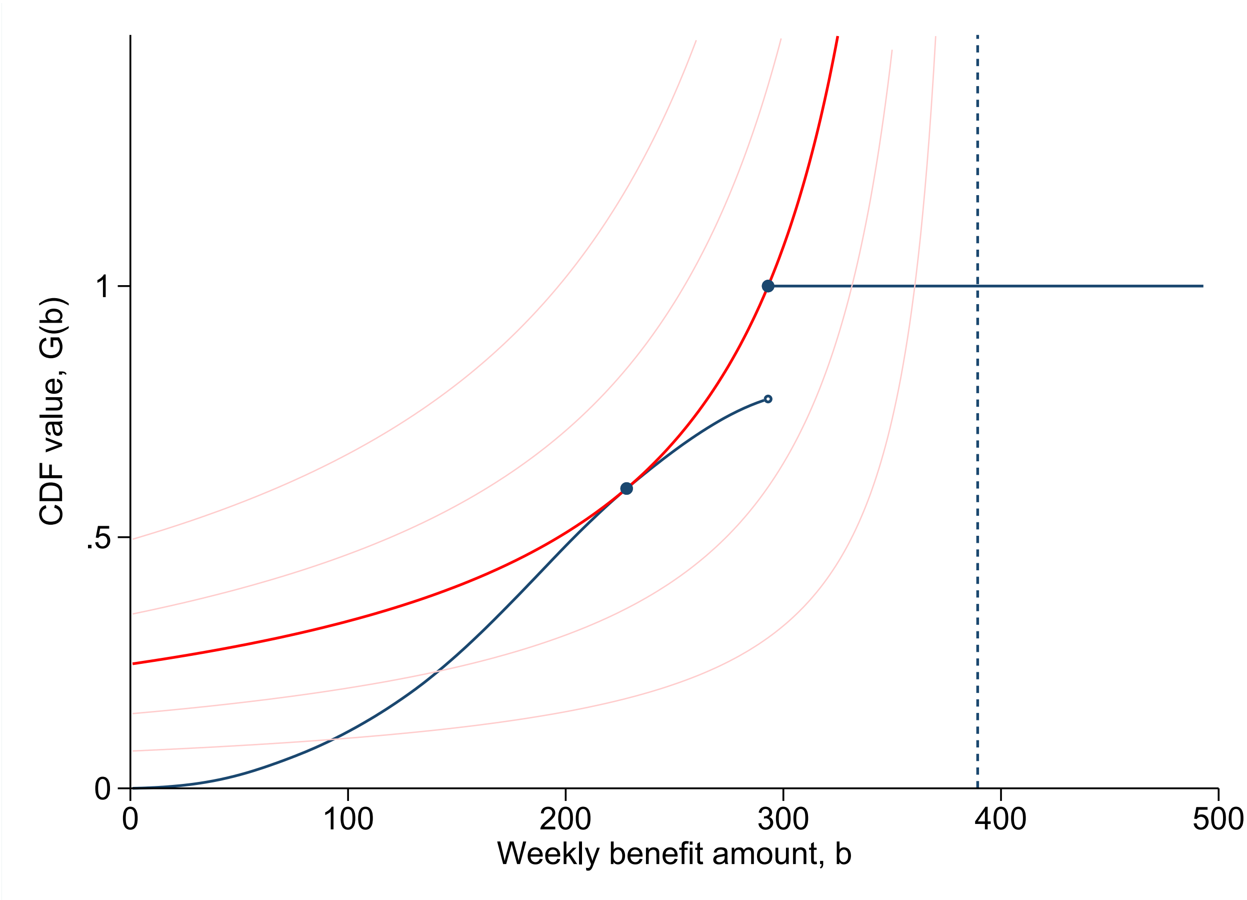
Source: Authors' legislative research, Quarterly Census of Employment and Wages (QCEW), and authors' calculations.

Figure 3: State Average Replacement Rates below Maximum Weekly Benefits



Notes: Long-dash bars indicate legislated changes to the replacement rate. Short-dash bars indicate changes to the definition of the base period, which trigger changes to the *de facto* replacement rate.
Source: Authors' legislative research, Quarterly Census of Employment and Wages (QCEW), and authors' calculations.

Figure 4: Empirical CDF of Weekly Benefit Amounts and Corresponding Isoprofit curve at $\tilde{b}$, Washington, DC in 1991



Source: Benefit Accuracy Measurement (BAM) program data and authors' calculations.

C Model Appendix

C.1 Proof of Proposition 1

1(i): Differentiating $\tilde{p}$ (equation 6.8) with respect to c and applying the envelope theorem, we have

$$\frac{d\tilde{p}}{dc} = 1 + \frac{dw_R}{dc}\left[2J - \frac{dj}{dw_R}\left(\frac{J}{j}\right)^2\right]\Big|_{\tilde{b}}$$

The term in brackets is guaranteed to be positive by by our assumptions on $\Phi(\cdot)$. The change in $\tilde{p}$ depends on the change in the $\tilde{b}$ worker's reservation wage: $\frac{dw_R}{dc}$. Differentiating $w_R(b)$ (equation 6.6) with respect to c and evaluating at $\tilde{b}$ we have

$$\frac{dw_R(b)}{dc}\Big|_{\tilde{b}} = \frac{f[(1-\Gamma(\tilde{p})) - \frac{d\tilde{p}}{dc}\gamma(\tilde{p})(c - w_R(\tilde{b}))]}{r + \delta + f(1-\Gamma(\tilde{p}))}.$$

The sign is ambiguous. The first term in the numerator captures the upward pressure on reservation wages due to the $(1-\Gamma(\tilde{p}))$ firms that increase wage offers from c^0 to c^1. Meanwhile, the second term in the numerator captures the downward pressure on reservation wages due to the $\frac{d\tilde{p}}{dc}\gamma(\tilde{p})$ firms which cut their wages cutting wages from c to $w_R(\tilde{b})$.

Despite this ambiguity we can easily prove the proposition by constructing a contradiction. Suppose $\frac{dp}{dc} \leq 0$. This implies $\frac{dw_R(b)}{dc} \geq 0$ which in turn implies $\frac{d\tilde{p}}{dc} > 0$ whenever J is concave and produces the desired contraction. Thus, we must have that $\frac{d\tilde{p}}{dc} > 0$.

1(ii): Differentiating $\tilde{p}$ (equation 6.8) with respect to α and applying the envelope theorem, we have

$$\frac{d\tilde{p}}{d\alpha} = \frac{dw_R}{d\alpha}\left[2J - \frac{dj}{dw_R}\left(\frac{J}{j}\right)^2\right]\Big|_{\tilde{b}} + \frac{J}{j}\left(2\frac{dJ}{d\alpha} - \frac{dj}{d\alpha}\frac{J}{j}\right)\Big|_{\tilde{b}}$$

As before, the term in square brackets is positive due to our concavity assumptions on Φ. The final term is guaranteed to be negative since J is a cdf, Φ is concave, and $\alpha \in [0,1]$. Meanwhile,

$$\frac{dw_R}{d\alpha}\Big|_{\tilde{b}} = -\frac{d\tilde{p}}{d\alpha}\frac{f\gamma(\tilde{p})[c - w_P(\tilde{p})]}{r + \delta + f(1-\Gamma(\tilde{p}))} < 0$$

Thus, $\frac{d\tilde{p}}{d\alpha} < 0$ as was to be shown. □