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These Caps Spilleth Over: Equilibrium Effects of Unemployment Insurance

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Abstract

The design of US unemployment insurance (UI) policy—which features benefits assigned as a percentage of past wages *up to a cap*—engenders tests for spillovers from policy variation to workers who are not directly treated. We test for and find a pattern of spillovers from state-level UI policy changes that cannot be neatly reconciled with workhorse or cutting-edge models of UI spillovers. Instead, we show that the documented pattern conforms with the predictions of a canonical model of information frictions: wage posting with random search. Taken together, our results provide novel evidence of quantitatively- and policy-relevant information frictions in this market. Moreover, our estimates suggest that aggregate unemployment of insured individuals would *decrease* if the replacement rate were increased while holding the cap constant.

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

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

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1 Introduction

Does policy variation change the job-search outcomes of individuals whose treatments are unchanged? The answer to this question is of clear importance for both econometric measurement and for policy design. Spillovers bias measurements of policy effects if not appropriately accounted for in the econometric design. In addition, spillovers reveal features of the market's structure that influence the optimal design of policy.

In this paper, we exploit two features of the design of unemployment insurance (UI) in the United States to document spillovers in the market for low wage earners. First, UI benefits are set as a replacement rate of past earnings up to a maximum benefit cap. Thus, changes in the replacement rate do not change the benefits assigned to individuals at the cap and changes in the cap do not change the benefits assigned to those below the cap, generating clearly-defined categories of job-seekers for whom effects of policy changes could only arise from changes in the equilibrium. Second, the parameters governing the replacement rate and cap are set at the state level and have varied over time. State-level changes over time, thus, provide a panel of policy experiments.

To facilitate our analysis, we document, quantify, and validate the quantification of the legislative records in detail in order to identify the parameters of UI policy at the state level.[1] For each change in a policy parameter, we identify separate samples of individuals who are directly and only indirectly treated given the parameter of the UI benefit schedule that determines their benefits in the Survey of Income and Program Participation (SIPP). Using both linear probability and Cox proportional hazard models of the job-finding rate, we test for spillover effects of UI on indirectly-treated individuals. This analysis reveals a positive, statistically significant, and economically substantial effect of increases in the replacement rate on job-finding for those with capped benefits. Meanwhile, for those with benefits just

[1] In administrative data, imputed benefits based on our quantification of state laws reproduce realized benefits for 99.3 percent of individuals, see figure B1. Further, in section 9 and appendix B.3, we use our measures of the geographic and intertemporal variation in state UI laws to identify effects of UI on the directly treated that are in line with estimates of the same from the literature.

below the cap, we find a negative effect of increases in the cap on job-finding that is equally statistically significant and economically substantial.[2]

These results stand in contrast to the predictions of both mainstream and cutting-edge models of job search that accommodate UI spillovers, substitution effects, or both. Specifically, 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.[3] Meanwhile the Diamond-Mortensen-Pissarides model predicts negative effects on the indirectly treated group in both experiments, the intuition being that the increase in benefits makes vacancies, on average, less lucrative. Landais, Michaillat and Saez (2018) offer a model in which both positive and negative spillovers may be present, with the negative spillover being more likely to dominate as the fraction of the population that is treated nears one.[4] However, the observed share of workers receiving capped benefits—near 30 percent—coupled with the synthesis of Landais et al. (2018) suggests spillovers with the *opposite* pattern of signs from what we document.

To understand our facts we turn to another model from the canon: wage posting with random search (WPRS). Modeling the benefit cap in this framework generates predictions for the job-finding rate of indirectly-treated workers that match our empirical results. 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 of workers with benefits "just shy" of the cap.[5] The intuition follows from noting that, in the WPRS framework, a firm's optimal wage choice depends not on a single prospective employee's reservation wage and matching probability, but on

[2]These results are robust to plausibly-sized error in the measurement of benefits, see Section 5.1.

[3]While not containing a notion of job search, the Neoclassical model makes corresponding predictions for the effects on employment unless the cross-price elasticities are nil.

[4]The negative spillover effect flows from the same logic as the Diamond-Mortensen-Pissaredes model while the positive spillover follows from a change in the relative search effort of treated and untreated individuals but results in spillovers of the same sign as the Neoclassical and directed search models. As treatment nears unity relative search effort effects evaporate.

[5]We provide a model-based definition of "just-shy," map it to the data, and reconfirm the empirical results in Section 9.

the distribution of reservation wages and matching probabilities with all of her peers. If policy variation increases the reservation wage of individuals *below the cap*, it makes offering high wages that are acceptable to those at the cap relatively more attractive, increasing the share of firms making wage offers acceptable to capped individuals—indeed, to all the unemployed. On the other hand, if policy variation increases the benefits of workers *at the cap*, it makes these workers relatively less attractive and decreases the share of firms making offers acceptable to them. Moreover, the cap induces a convexity in the labor supply curve in the neighborhood of the cap and, under the assumption of random search, firms cannot differentially attract workers with different reservation wages by manipulating their wage offers, so firms that decrease their wage offers do so discretely. This generates a pool of workers just shy of the cap who have reservation wages that exceed these discretely-lower wage offers and who, therefore, experience the documented spillovers.

To these authors' knowledge, the collection of our results provide the first evidence of WPRS based upon observational data on job seekers.[6] Evidence of this market structure yields important policy implications. In the WPRS environment, both the *level* and *distribution* of workers' reservation wages have implications for efficiency. In particular, policymakers might be able to increase both average employment and the average income of the unemployed *at the same time* (Albrecht and Axell, 1984). The results presented herein move this possibility from the realm of theoretical plausibility to documented empirical reality. Indeed, a back-of-the-envelope calculation based on the magnitude of our estimated effects suggests that, in the typical policy environment we study, an increase in the replacement rate while holding the cap fixed would *decrease* the average unemployment rate. This conclusion follows

[6]The evidence to-date for job seekers has been derived from self-reports elicited in surveys, for example Hall and Krueger (2012). Meanwhile, evidence of monopsony derived from variation in minimum wages—Lee (1999); Autor, Manning and Smith (2016); Engbom and Moser (2021)—and from responses to wages of granular competitors—Staiger, Spetz and Phibbs (2010); Hjort, Li and Sarsons (2020); Derenoncourt, Noelke, Weil and Taska (2021)—do not (try to) pin down the search or wage setting mechanisms. Most closely related to our study, Chetty, Friedman, Olsen and Pistaferri (2011) exploit kinks in the Danish tax system to document bunching that spills over to indirectly-affected workers and argue that such spillovers arise in an environment that features information frictions quite similar to ours.

from the large magnitudes of our estimated spillover effects relative to canonical estimates of the direct effects of UI on job-finding, as well as our own.

Regarding the design of optimal policy, positive spillovers in the WPRS model derive from eliminating a dead-weight loss due to information frictions. As such, positive spillovers from raising replacement rates would continue to accrue until the distribution of UI benefits collapses to a single point at the benefit cap. This implication stands in sharp contrast to the existing studies of spillovers in which positive spillovers derive from the relative characteristics of competing workers and thus are expected to fade to zero when treatment becomes universal. That said, such an extreme change in policy would be well outside the neighborhood in which our marginal effects are estimated.[7]

We view our evidence regarding market structure, in particular the presence of economically significant information frictions in matching and wage setting, as a key contribution of this paper. However, we stop short of providing a fully specified structural model and optimal policy in its lens. This choice reflects our read of the current state of the art surrounding a well documented puzzle in WPRS models: it is not possible to simultaneously fit data on labor market flows and wage dispersion without implying extremely low (negative) flow values of non-employment (Eckstein and Wolpin, 1990; Hornstein, Krusell and Violante, 2011). Instead, we argue for a sufficient statistics approach and highlight the moments required to adjust a Baily (1978)-Chetty (2006) type formula.[8]

[7] In particular, the negative effect of increasing benefits on the job finding rate of directly treated workers may be increasingly large, outweighing the positive spillover long before benefits become uniform. For concreteness, observe that in some states the benefit cap is larger than the insured earnings of workers on the low end of the distribution.

[8] When studying one or serial steady states, as in Engbom and Moser (2021), a large negative flow value of leisure is not inconsistent with the facts and indeed could be micro-founded, for example, based on non-wage amenities such as anticipated returns to tenure, as in Burdett, Carrillo-Tudela and Coles (2020). However, this solution is impractical for analysis of policy or, in general, comparative statics because it implies implausibly elastic responses of reservation wages to policy variation. Recent contributions by Bradley and Gottfries (2021); Faberman, Mueller, Şahin and Topa (2022) and Doniger (2014) make strides in resolving this tension, but calibration of these models require novel moments that are themselves yet to be extensively studied and are not the focus of the present paper. In addition, the aim of a structural model of this type in the context the present paper would be to infer the option value of search, particularly for those with capped UI benefits, from the empirical distribution of wages. Since structural modeling is fraught for the aforementioned reason and as discussed at greater length in Section 10, we argue for a different approach: empirical evaluation of the option value of search while unemployed. This is the approach taken

Regarding econometric design, spillovers from UI benefits mean that estimates of the sensitivity of a worker's reemployment hazard to their own replacement rate require nuance. First, in the presence of spillovers, 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 affects the job-finding rates of both treated and control groups. Second, spillovers imply that the micro effect—the effect of policy changes on directly-affected individuals' outcomes—may differ from the macro effect—inclusive of spillovers (Davidson and Woodbury, 1993; Lise, Seitz and Smith, 2004; Crépon, Duflo, Gurgand, Rathelot and Zamora, 2013; Lalive, Landais and Zweimüller, 2015). Our paper differs from the literature on spillovers in that the channel for which we find evidence depends on the *distribution* of benefits. This allows our model to provide a unified framework through which to understand our empirical findings. In addition, in light of our evidence, we make a correction to the difference-in-differences research design of Meyer and Mok (2014) and others and, with this correction obtain estimates closer to the modal range of the broader literature on UI's direct effects.

Our paper proceeds as follows. Section 2 briefly describes the UI system in the US and the potential to identify spillovers therein. Section 3 lays out the empirical model. Section 4 describes the state legislative histories, BAM, and SIPP. Additionally, section 4 gives overviews of our measurement of the policy parameters, how we measure an individual's likely UI benefits, and how we assign individuals to samples of indirectly-treated workers. Further details of these procedures can be found in Appendix B. Section 5 documents the spillover effects and discusses why they are difficult to reconcile with mainstream and cutting-edge models of UI spillovers. Section 6 lays out a parsimonious model of wage posting and random search in the context of the US UI system and Section 7 derives the model's testable

in the sufficient statistics literature and the pioneering work of Gruber (1997). As we discuss in Section 10, estimates of the consumption change at job loss, while an imperfect measure of the option value of search, are instructive and give us confidence that our results imply that, in the typical configuration of policy, welfare could be improved by increasing the replacement rate while hold the cap fixed.

implications, which coincide with our empirical findings. Sections 8 and 9 present additional empirical tests and discuss results in the literature that corroborate the model and shed new light on difference-in-differences strategies for identifying the direct effects of UI benefits. Section 10 discusses the policy implications of our results. Section 11 concludes.

2 Unemployment Insurance in the United States

Unemployment insurance in the US is governed by a set of federal guidelines, but weekly benefit formulas and other policy specifics are set at the state level. The prototypical weekly benefit formula can be summarized as

$$b = \min\{\rho\chi, c\}$$

where ρ is the fraction of earnings, χ, that are replaced up to a dollar-valued cap, c, on the weekly benefit amount.

Changes to the two parameters of this benefit structure, ρ and c, affect claimants at different parts of the benefit distribution. The benefits of claimants receiving the cap are unaffected when states adjust ρ. Similarly, claimants' benefits that are unconstrained by the cap are not affected when the cap is raised. While spillovers may arise even when all claimants are directly affected by policy changes, this policy environment specifically admits tests for spillovers through analyses of unaffected claimants.

Our approach to testing for spillovers requires us to identify the relevant indirectly-treated workers—those who are at the cap or just shy of it—as well as chronicle the policy changes we use for identification. We accomplish both of these by documenting all state benefit formulas prevailing in all 50 states and the District of Columbia from 1986 through 2015. As benefit formulas are set via state legislation, we reference the original laws, which are found in session laws compilations published at the end of each state's legislative sessions. We use annotated state codes to ensure we have referenced all relevant legislation and thus changes to UI benefits.[9]

3 Empirical Strategy

We test for spillover effects from changes to UI benefit formulae using a sample of job separators in survey data and two econometric approaches.[10] In both approaches, we identify effects by comparing outcomes before and after state-level policy changes. First, we model the weekly reemployment hazard directly using linear probability models. Second, we study the same effects using proportional hazard models.

3.1 Linear Reemployment Probability Model

Our main analysis models reemployment as a binary outcome for each claimant in each week of their unemployment spell. We assume that the probability of reemployment can be modeled linearly as

$$\mathbb{P}(F_{n,t,s,d} = 1) = \beta_0 + \beta_1 P_{n,t} \mathbb{1}^{\rho}_{n,t,s} + \beta_2 P_{n,t} \mathbb{1}^{c}_{n,t,s} + \mathbf{X}_{n,t,s,d}\mathbf{B} + FE_n + \varepsilon_{n,t,s,d} \tag{3.1}$$

where $F_{n,t,s,d}$ is an indicator for reemployment at unemployment duration d for spell s in progress either before ($t = 0$) or after ($t = 1$) policy change n. $P_{n,t}$ is the value of a policy parameter—either the log maximum weekly benefit or the replacement rate—prevailing before or after policy change n.[11] $\mathbb{1}^{\rho}_{n,t,s}$ and $\mathbb{1}^{c}_{n,t,s}$ are binary variables equal to one if the earnings and work history associated with spell s qualify it for benefits in the ρ or c part of the benefit

[9] Specifics of the data on state law are documented in section 4 and appendix B.2.

[10] The data on job seekers are described in section 4.

[11] For cap changes, because $P_{n,t}$ is the log cap prevailing for spell s near cap change n, the estimate of β_1 indicates the spillover effect of a 100 log point increase in the cap on the probability of reemployment in a week for those below the cap. For replacement rate changes, $P_{n,t}$ is ρ, the fraction of pre-displacement weekly earnings replaced by UI. As replacement rates are already a percentage, which we measure on a 0 to 1 scale, we model them linearly. The spillover onto those at the cap is captured by β_2, which is directly interpretable as the effect of changing the replacement rate from 0 to full replacement.

schedule under both permutations of policy around n. Specifically, we compute realized and counterfactual benefits for each spell s as functions of observed earnings and work history under both the $t = 0$ and $t = 1$ policies associated with policy change n. $\mathbb{1}^{\rho}$ is equal to one only for spells for which benefits would be determined by the ρ portion of the policy schedule under both $t = 0$ and $t = 1$ regimes. $\mathbb{1}^{c}$ is defined analogously.[12] The vector $\mathbf{X}_{n,t,s,d}$ comprises spell and state-level controls, including a set of fixed effects for number of weeks of unemployment duration, while FE_n is a set of fixed effects for policy changes. The set of controls includes both individual characteristics and labor market conditions at the time of job loss and week of unemployment d.[13] We interact all spell-level controls—all those other than the labor market controls and the policy change fixed effects—with the subsample indicators $\mathbb{1}^{\rho}_{n,t,s}$ and $\mathbb{1}^{c}_{n,t,s}$, allowing their effects to vary for the directly- and indirectly-treated subsamples. Accordingly, the contribution of the directly-treated subsamples to estimating the effects of interest is through improved estimates of aggregate labor market conditions. These aggregate controls, especially the job-separation and job-finding rates, are essential to avoiding spurious results stemming from the possibility of correlation between policy changes and macroeconomic conditions.

Due to the FE_n effects and controls, the β_1 and β_2 parameters are identified by differences in reemployment probabilities before and after each policy change, n. As such, if policy changes are exogenous after controlling for $\mathbf{X}_{n,t,s,d}$ and FE_n, then β_1 and β_2 measure the direct and indirect effects of a policy change to ρ, respectively. Conversely, for a policy change to c, β_1 and β_2 measure the *indirect* and *direct* effects. Since we are primarily interested in the indirect effects, our analysis focuses on β_1 in the cases of changes to the policy cap and β_2 in the cases of changes to the effective replacement rate.

[12] Note, the exclusion of spells qualifying for benefits between the two caps, when they differ, serves to create two well-defined, homogeneous groups: one that is directly treated identically throughout and one that is only indirectly treated.

[13] The individual spell characteristics are log base period wages, 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. The labor market conditions include state average weekly wages, and predicted state-level job-separation and job-finding rates, as described in appendix B.1.

3.2 Proportional Hazard Model

We also perform analysis in which the reemployment hazard is assumed to vary proportionally in the covariates. That is, we estimate Cox models of the form

$$\lambda_{n,t,s}(d) = \lambda_0(d) \exp\{\alpha_0 + \alpha_1 P_{n,t} \mathbb{1}^{\rho}_{n,t,s} + \alpha_2 P_{n,t} \mathbb{1}^{c}_{n,t,s} + \mathbf{X}_{n,t,s,d}\mathbf{A} + FE_n\} \tag{3.2}$$

where $\lambda_{n,t,s}(d)$ is the reemploymed hazard at unemployment duration d for spell s in progress either before ($t = 0$) or after ($t = 1$) policy change n, $\lambda_0(d)$ is an unspecified baseline hazard, and the exponential term contains effectively the same specification as the right side of equation (3.1).[14] The coefficients can be interpreted as the effects of the variables on the log hazard, meaning α_1 and α_2 are elasticities in our cap change analysis, where $P_{n,t}$ is the log cap, and they are semi-elasticities in our replacement rate change analysis. The same variation contributes to identifying the parameters as in equation (3.1), though the nonlinearity precludes interpreting the parameters of interest as simple, linear within-policy-change differences.

4 Data

Our project requires accurate and consistent measurement of (1) local UI policies and the timing of their changes and (2) individual job seekers' benefits and job-search outcomes. We achieve the first through a detailed reading and coding of UI policies at the state level as reported in state session laws. We achieve the second by exploiting a well-studied panel data set in the context of UI: the Survey of Income and Program Participation. We also make use of administrative records of actual UI receipt collected by the Benefit Accuracy Measurement program in order to harmonize policy measures across time and space and to identify additional parameters needed for model validation. This section provides an overview of the key features of each of these data sources. Appendix B provides additional details about

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

specific computations. We supplement these data with information on local economic conditions from the Current Population Survey and Quarterly Census of Employment and Wages, which are described in the same appendix.

4.1 Benefits Accuracy Measurement (BAM) Program

The BAM program is administered by the US Department of Labor and randomly samples UI payments weekly in every state to test for patterns of improper payments (U.S. Department of Labor, Employment and Training Administration, 1988–2015).[15] For each observation in this weekly random sample of ongoing claims, BAM records weekly benefit amount, date of job loss, date of initial claiming, claimant demographics and measures of pre-job-loss earnings: total base period wages and high quarter wages. Base period wages refer to employer-paid earnings during the base period, which is the four quarters ending two quarters before the worker starts their UI benefit claim (e.g., the prior calendar year for someone making a claim in quarter two). High quarter wages refer to total earnings during the quarter of the base period in which earnings are highest. These are the two measures of earnings most commonly used to calculate benefits. In addition, the BAM sample is stratified by UI payment amounts and dense. This enables us to reliably recover the empirical distribution of benefits, which is crucial for the model verification tests in Section 9.

4.2 State Sessions Laws

As described in Section 2, UI benefit formulas are defined at the state level; we document all details of these formulas and their changes over time from the original legislation published in session laws compilations (HeinOnline, 2022). From the legislative records we develop a program that computes weekly benefit amounts based on pre-displacement earnings data

[15] BAM microdata may be obtained by contacting the Employment and Training Administration (ETA) at DOL.

and the exact rules described in each state's legislation applicable at the time of job loss.[16] In the vast majority of states, weekly benefits are calculated once at the beginning of a spell and remain unchanged even if state policy changes while the spell is ongoing. To avoid any ambiguity regarding this issue, we treat ongoing spells as censored when state policy changes, as described in the following subsection.

HeinOnline (2022) provides a compilation of the full text of state laws. Relative to the more commonly used Significant Provisions of State UI Laws (Employment and Administration, 2022)—which are compiled by the Department of Labor Employment and Training Administration every January and July and reflect policy at the compilation date—the full text of the actual state laws allows us to date policy changes precisely.[17] Precise dating is important given our empirical design in which we examine one-year windows of time around policy changes.[18]

In addition, and in contrast to the simple presentation in Section 2, the actual benefit formulas for individual states can be highly complex. For example, over the entire sample period, California's benefit schedules are nonlinear even below the cap. The schedules appear explicitly in the legislation in the form of tables that map ranges of prior earnings to weekly benefit amounts. In other cases, the legislation implements a form of $b = \min\{\rho\chi, c\}$. For example, chapter 96-378 of the Laws of Florida, effective July 1, 1996, stipulates that

> [a]n individual's 'weekly benefit amount' shall be an amount equal to one-twenty-sixth of the total wages for insured work paid during that quarter of the base

[16]We validate the fidelity of this calculator by testing its ability to accurately predict realized benefit amounts recorded in the BAM administrative records (see figure B1). When the variables recorded in the BAM research data set coincide exactly with those referred to in state law our calculator computes weekly benefits that exactly match those recorded in the administrative data 99.3 percent of the time. When the BAM administrative data are missing the exact objects referred to in state law the errors remain small and have the predicted sign.

[17]For canonical and prototypical studies exploiting the Significant Provisions see Gruber (1997); Chetty (2008) and Ganong, Greig, Liebeskind, Noel, Sullivan and Vavra (2021).

[18]In addition to issues caused by the biannual frequency, which leads to many cases of an early-July or October change not appearing until the following January, there are cases in which the appearance of policy change in Significant Provisions lags the actual change by more than a half year or is missing entirely. For example, although Tennessee increased its weekly benefit cap from \$220 to \$240 in July of 1997 (Public Acts 1997, Chapter No. 95), a change that is confirmed in the BAM data, Significant Provisions continues to show the cap as \$220 until a subsequent increase to \$255 in July of 1998.

> period in which such total wages paid were the highest, but not less than \$32 or more than \$250.

However, such formulas differ across state and time in their definition of insured earnings (e.g. which weeks of earnings in the base period are used to calculate insured weekly earnings).

With the goal of analyzing the panel of event studies, we create simplified measures of the underlying and often complex UI policies that are harmonized across time and space and make note of whether policy changes were triggered by existing or newly-enacted legislation. Appendix B.2 provides details of our calculation of ρ, the fraction of earnings replaced below the cap, under different policies. In appendix B.3 and section 9 we validate the econometric power of these metrics by estimating direct effect of UI benefits on recipients using the panel of policy change event studies and the SIPP data detailed in the following subsection. These validation exercises produce estimated elasticities for the direct effect of UI in $(-0.50, -0.35)$ which corroborates and is corroborated by the range of estimates in the literature.[19]

Table 1 summarizes the prevailing policy environments for our samples of policy changes. We examine 694 and 144 policy changes to the cap and replacement rate that can be linked to the subsequently-described worker-level data from 1988 to 2013. Of the cap changes, 145 result from newly-enacted legislated and the remaining are triggered by existing legislation. We will see that our results are powered by the new legislation, a result that is compelling since those triggered by existing legislation are explicit functions of lagged economic conditions and simply keep pace with average wage growth by design. The legislated changes are also larger on average and affect a greater share of claimants. Because cap changes are almost exclusively modest increases, we drop the small number of cap reductions and increases of more than 15 log points: these are most likely to be enacted under extraordinary circumstances, when confounding conditions are most likely to prevail, and are less likely to be representative of typical policy changes.

[19] Chetty (2008, p. 174) summarizes the existing evidence: "For example, Moffitt (1985), Meyer (1990), and others have shown that a 10 percent increase in unemployment benefits raises average unemployment durations by 4–8 percent in the United States." More recent work, is also consistent with these magnitudes (Landais, 2015; Card, Johnston, Leung, Mas and Pei, 2015).

We define a replacement rate change as any change to benefit calculations for those receiving benefits below the cap. After restricting to changes that are not contaminated by coincident legislated changes to the cap, we analyze 144 changes to replacement rates.[20] Because many of these changes are (automatic) increases to the statutory minimum benefit amount, we also focus on the 14 changes to other aspects of formulas—these are changes to statutory replacement rates, the definition of insured earnings, or nonlinear formulas for benefits below the cap.

We note that deviations in job finding rates at the time of policy changes from both the state trend and from the contemporaneous national average are an order of magnitude smaller than the baseline job-finding rate. Taken together, these suggest that the changes are not systematically in atypically strong or weak labor markets.[21]

4.3 Survey of Income and Program Participation (SIPP)

While the SIPP is a workhorse data source in this research area, we review its key features here and describe the selection of our analysis sample. The SIPP is a series of short-panel surveys of households (U.S. Census Bureau, 1986–2008).[22] We examine job-finding rates using a sample of unemployment spells. Specifically, we use a sample of SIPP respondents who report separating from a job due to layoff or business closing and are therefore likely eligible to receive UI benefits. We measure 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. Specifically, we code a spell as ending with reemployment if the respondent reports two consecutive weeks employed, though our results are robust to coding reemployment as a single week of employment or four consecutive weeks.

[20] We make this restriction to avoid the large direct effects and equilibrium effects induced by legislated cap changes. Legislated changes tend to be larger and more infrequent (see panel A). As a result, they also tend to occur when the cap is lower relative to average weekly wages and when more claimants are constrained by the cap.

[21] State and national unemployment rates are from the Current Population Survey.

[22] Our main analysis uses data from the 1988 through 2008 panels of the SIPP, ultimately covering job separations from 1988 to 2013.

The SIPP records income from employment, government transfers, and other sources at a monthly frequency. However, it is well known that non-random non-response is a defect of the non-labor income data in the SIPP (Meyer and Mok, 2014). Meanwhile, the labor-income data is widely considered to be of high quality (Abowd and Stinson, 2013). In light of 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 respondent's state UI policy at the time. 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.[23]

To conduct our main analysis, we need to identify similar workers directly and indirectly affected by a policy change. Specifically, we identify two groups for each policy experiment: those "just below" the higher of the two benefit caps and those at or above it.[24] We generate our headline set of facts based on an agnostic definition of "just below:" job seekers with benefits within seventy five percent of the higher of the two caps. In addition, we restrict the capped sample to those whose past earnings would yield benefits no more than twice the actual value of the benefit cap were the cap to be removed.

We then restrict our sample to unemployment spells starting within a window extending one year before and after each policy change.[25] If the pre-change policy has been in effect for less than a year or the policy changes again less than one year after the policy change,

[23]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 some survey respondents prior to job separation, we take two approaches to calculating benefits for them. First, if we only observe one or zero quarters of the standard base period, we treat the quarter prior to job loss as part of the base period as well. Second, 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. In practice, we observe the full standard base period for approximately 55 percent of observations.

[24]Benefits may be above the cap in some states due to dependent allowances, which increase the weekly benefits received by some claimants with dependents. While these allowances induce deviations from the simple $b = \min\{\rho\chi, c\}$ definition, the statutory cap for those without dependents remains the largest and most salient mass point in the benefit distribution.

[25]Unemployment spells may appear in our analysis up to twice, in the post period for one policy change and in the pre period for a subsequent change.

we truncate the window symmetrically on both sides. We treat ongoing pre-change spells as censored at the time the policy changes and ongoing post-change spells as censored at the end of the one-year (or shorter) window.[26] In other specifications, we have included ongoing spells at the time of the policy changes and assigned them the values of new policy parameters extant in each week of unemployment. As expected, inclusion of these portions of spells attenuates our results since they increase the fraction of the sample that is observed immediately after the policy change, when few of the unemployed have benefits set under the new policy and, therefore, general equilibrium effects are weakest. This is consistent with the notion that, for the kink in the benefit formula to induce distortions in firms' wage-setting incentives, a non-trivial population must be treated, as in Chetty et al. (2011).[27]

Table 2 reports sample means for these two indirectly-treated subsamples along with the corresponding directly-treated subsamples.[28] Panel A describes the subsamples associated with cap changes, for which the just below subsample is indirectly treated and the capped sample is directly treated. In table B1, we include the "well below" sample for completeness: those whose benefits are less than 75 percent of the cap. Panel B of table 2 reports means for observations associated with the replacement rate sample, for which the directly-treated samples are well below and just below the cap, while the indirectly-treated sample is at the cap. In both cases, the capped sample is more male, older, more educated, and has a higher average weekly job-finding rate. All of these differences are consistent with the capped sample being higher-earning by definition. However, we note that we do not identify effects through comparisons of these two groups, so table 2 is not intended to show balance. Instead, our analysis merely requires that directly- and indirectly-treated workers compete

[26] We also treat spells as censored if they are ongoing at the end of their SIPP panel or they have consecutive weeks of missing labor force status during the spell, which would preclude us from measuring reemployment under our definition.

[27] Results are available upon request.

[28] Because of the size of the SIPP and its rate of sampling in some states, we do not always observe in every subsample for every policy change. As a result, the numbers of policy changes differ from those reported in table 1.

for opportunities in the same labor market so that spillovers from one to the other are plausible.

Table 3 reports means for the samples that do identify the effects we examine: the indirectly-treated samples in the pre- and post-periods for each policy change type. The empirical strategies described in section 3 effectively use pre-change observations as a control group for post-change observations, so we note that the pre- and post-change means are substantively similar. Analogous tables for the directly-treated samples appear in tables B2 and B3.

5 Results

Here we document an economically and statistically significant negative spillover on job seekers just below the benefit cap when the cap is raised and an equally economically and statistically significant positive spillover for workers at the cap when the replacement rate is raised. Further, we also show that these results are robust to measurement error and document an absence of confounding pre-trends. Finally, we discuss the puzzle posed by our empirical results for canonical and cutting edge models of spillovers in job search.

5.1 Effect of the Cap on those Just Below the Cap

Columns (I) and (II) of table 4 report linear estimates of the effect of the benefit cap on job-finding rates for claimants with benefits just below the cap. In particular, these are estimates of β_1 in equation (3.1) for two different specifications. Each 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 75 percent of the greater of the two caps (i.e. the caps pertaining before and after each policy change n).

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. As such, the estimated effect of -0.07 in column (I) indicates that a 1 log point increase in the benefit cap decreases the probability of weekly reemployment by 0.0007. On a sample average weekly job-finding rate of 0.062, this corresponds to a decrease of 1.1 percent. Column (II) allows for different effects of legislated and automatic cap changes. As we have previously discussed, automatic cap changes tend to induce less variation because they tend to be small, resulting in a less precise estimated effect even though we observe far more of them. The coefficient for these changes is also smaller, though the difference is not statistically significant. The results suggest a spillover elasticity of about −1.1 on the "just below" subsample, primarily driven by legislated changes.[29] While average job-finding rates are higher in the legislated-change sample (0.070 as compared to 0.060 for automatic changes), the estimated effects both suggest elasticities at the mean near negative one.

The same negative effects are also observed in the proportional hazard estimates presented in table 5 columns (I) and (II). As noted in section 3, these estimates represent the elasticity of the job-finding rate with respect to the benefit cap for those whose benefits are just below it. The estimated elasticities are all between −1.01 and −1.13. This consistency with the linear probability models estimated on the same data suggests that our results are robust to misspecification of the underlying job-finding hazard.

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 calculated benefits near the cap. As these observations are those most likely to be miscategorized, we repeat the analysis with them removed from the just-below subsample. Figure 1 shows the estimated linear coefficients from this exercise, which are analagous to columns columns (I) and (II) in table 4. The leftmost estimates in the figures repeat the coefficients reported in that table, including everyone above 75 percent of the larger cap and below the smaller cap. Moving to the right in the figures, progressively more claimants with calculated benefits near the smaller of the two caps are excluded. For

[29] We report the full set of estimates from these two regressions—including the effects on those well below the cap and at the cap —in table 7.

example, the estimates in the center ($.90c$) exclude those above 90 percent of the smaller cap, only including those between 75 percent of the larger and 90 percent of the smaller in the subsample. We note that the estimated effects are quite robust to this trimming, suggesting that the negative estimates are not due to miscategorized, directly-affected claimants who are actually at the cap.

To alleviate the different concern that the effect may be driven by spurious auto-correlation between local macroeconomic environments and policy parameters, we check for pre-trends. Specifically, we estimate an analogue to equation 3.1 on the sample of job seekers one year prior to any of our identified policy changes. That is, the sample contains workers from periods $t = -1$ and $t = 0$, rather than $t = 0$ and $t = 1$ as in our main regression, relative to each policy change n. We also augment these regressions with controls for the policies extant contemporaneously with this sample. Specifically, we estimate:

$$\begin{aligned}\mathbb{P}(F_{n,t-1,s,d} = 1) = \beta_0 &+ \beta_1 ln(c)_{n,t} \mathbb{1}^{\rho}_{n,t-1,s} + \beta_2 ln(c)_{n,t} \mathbb{1}^{c}_{n,t-1,s} \\ &+ \beta_3 \rho_{n,d} \mathbb{1}^{\rho}_{n,t-1,s} + \beta_4 \rho_{n,d} \mathbb{1}^{c}_{n,t-1,s} \\ &+ \beta_5 ln(c)_{n,d} \mathbb{1}^{\rho}_{n,t-1,s} + \beta_6 ln(c)_{n,d} \mathbb{1}^{c}_{n,t-1,s} + \mathbf{X}_{n,t-1,s,d}\mathbf{B} + FE_n + \varepsilon_{n,t-1,s,d}\end{aligned} \tag{5.1}$$

where $ln(c)_{n,t}\mathbb{1}^{\rho}_{n,t-1,s}$ and $ln(c)_{n,t}\mathbb{1}^{c}_{n,t-1,s}$ are the log caps studied in the main regression interacted with the indicators for the part of the UI schedule that the observed individual's benefits would be set under if their spell occurred during the main sample window a year later.[30] Meanwhile, $P_{n,d}\mathbb{1}^{\rho}_{n,t-1,s}$ and $P_{n,d}\mathbb{1}^{c}_{n,t-1,s}$ are measures of the policies extant at the week that the job seeker is observed interacted with the same indicators. As such, β_3 and β_4 are the direct and indirect effect of the contemporaneous replacement rate on individuals in the lagged sampling frame and β_5 and β_6 are the indirect and direct effect of the contemporaneous log benefit cap on those same individuals. Meanwhile, β_1 and β_2 are the indirect and direct

[30] We inflate the earnings measures for this sample by the growth in state-level average weekly wages during the one-year period after their actual job-loss date. Using these inflated earnings, we categorize the directly- and indirectly-treated subsamples for each policy change as we do in our main analyses: we calculate benefits under the pre and post change policies and define just-below and capped samples as described in section 4.3.

effect of a change in the log benefit cap one year in the future, if any. If our main results in table 7 were driven by ongoing trends in job-finding rates for workers at particular parts of the benefit distribution, the effects could be observed using this specification. Absence of pre-trends is validated by null estimates for β_1 and β_2, reported columns (I) and (II) of table 6.

5.2 Effect of the Replacement Rate on those at the Cap

The estimated effects of changes to the replacement rate on job-finding for individuals at the benefit cap are reported in columns (III) of tables 4 and 5. Here, the listed coefficients are on the average replacement rate for individuals below the cap, calculated as described in section B.2. Changes to effective replacement rates are relatively rarer than changes to the cap. Restricting to instances when the replacement rate changed but the cap remained the same leaves us with too few policy changes to make meaningful inference. However, large coincident cap changes are liable to confound the estimates since, in this case, individuals in the indirectly-treated sub-sample are also treated by a large increase in benefits due to the change in cap and therefore are likely to have a decreased job finding rate. As we have previously argued that automatic cap changes approximately keep pace with average wage growth while legislated cap changes are both larger and more salient, we restrict our estimates of interest to policy changes that do not correspond to any legislated change in the cap. Column (III) in table 4 suggests that a one percentage point increase in the replacement rate for those below the cap raises the weekly job-finding rate of those at the cap by 0.20. As the average value of ρ is approximately 0.73, this implies that a one percent increase in ρ raises weekly job-finding by 2.0 percent of the sample mean (0.076).

The proportional hazard model estimates in table 5 are again consistent with these results, suggesting that our findings are robust to misspecification of the underlying job-finding hazard. Meanwhile, as in the case of the cap change experiments, table 6 reports null coefficients from the analogue of regression 5.1 for the case of a change in the replacement rate, documenting the absence of pretrends.[31]

Finally, many of the policy changes below the benefit cap are to the statutory benefit minimum, as we discussed in section 4.2. While these changes yield variation in measured ρ, they only affect individuals at the minimum, who are least likely to induce spillovers to capped claimants. To address this concern, in column (IV) of table 4, we repeat the estimation using only identifying variation stemming from the 14 changes to a statutory replacement rate, the definition of insured earnings, or a nonlinear benefit formula governing the interior of the benefit distribution.[32] These 14 policy changes identify the coefficient of interest, while the remainder of the sample aids in identifying the effects of the controls, in particular those accounting for local macroeconomic fluctuations. This exercise produces an essentially identical estimate.[33] We interpret this as evidence that we have not identified the effect of ρ using spurious variation in ρ due to macroeoconomic fluctuations affecting the BAM sample used to calculate it.[34]

5.3 Discussion

Neoclassical and directed search models suggest that, when the price of one type of labor increases, firms will substitute toward hiring more of the other: a relative wage effect. This implies that the spillover in both experiments should have been positive.[35] On the other hand, Diamond-Mortensen-Pissarides models suggest that, if benefits rise on average and firms must post vacancies before learning their individual realization of productivity, then

[31] Note, in the case of changes to the replacement rate, miscategorization of individuals near the cap would only attenuate results.

[32] That is, we assign the same value of ρ to both pre and post observations whenever the only change occurring in that n is to the benefit minimum; this variation is then absorbed by the policy change fixed effects, FE_n.

[33] Standard intuition regarding measurement error on in the right hand side variable suggests that the estimate in column (III) is attenuated; however, we note that the variation in these 130 cases is dramatically smaller than in the 14 with well-identified (both narrative and statistically) signal.

[34] Further, if we simply run this regression using only the 14 changes to policies other than the minimum, we again obtain a positive estimate: 0.13 (SE=0.11). However, without the larger sample aiding in estimating the effects of the controls, the estimate is imprecise.

[35] Although, the neoclassical model lacks a proper notion of unemployment or job-finding.

increasing benefits on average will depress vacancy posting. This implies that the spillover in both experiments should have been negative: an average wage effect. Plainly, our results do not fit these predictions. Augmenting the standard Diamond-Mortensen-Pissarides framework with a capacity constraint or decreasing returns to scale provides a synthesis in which both relative and average wage effects are present and, depending on the policy experiment, one or the other dominates (Michaillat, 2012; Lalive et al., 2015; Landais et al., 2018). In such a framework, the average wage effect is increasing in the share of the population that is treated and the relative wage effect is nil when the full population is treated.

One might think to shoehorn these results into the DMP-Landais et al. (2018) framework by positing that the average change in benefits is larger for a change in the cap than the replacement rate, causing the average wage effect to dominate in the cap change experiment but not in the replacement rate change experiment. However, examining table 1 reveals that only approximately one-third of UI recipients have capped benefits. This suggests the opposite balance of relative and average wage effects in the two types of experiments. One might also think to shoehorn these results into a directed search or Neoclassical framework by positing that workers with lower benefits are complements to workers with higher benefits but workers with higher benefits are substitutes for workers with lower benefits. However, we note that the observable characteristics of workers with ρ-schedule benefits are similar to that of workers with c-schedule benefits (see table 2) and that the border between these regions varies in an arbitrary way across states and times. Thus, we conclude that a new (or revitalized old) model is required to rationalize our findings.

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 on the interior and have a mass point at the maximum. Our setting approximates key features of the US unemployment insurance system, in particular the benefit cap. In our setting, we establish several results which, together, yield predictions consistent with the empirical tests: 1) an increase in the cap on unemployment insurance benefits decreases the job-finding rate of individuals below the cap and 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 $b = \min\{c, \rho\chi\}$ be the potential unemployment benefit for a worker with earnings given by χ. We will investigate the implications of variation in policy parameters ρ and c. 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.[36]

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.[37] Let f be the Poisson arrival rate of job offers, δ be the Poisson

[36]In the UI systems of the US, 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.

[37]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 we wanted to add on-the-job search and generate a more dispersed wage distribution, it is straightforward to have employers offer jobs with value equivalent to these job with flat wage schedules if we posit that wages are set on the job via Bertrand competition as in Postel-Vinay and Robin (2002a).

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.[38] Productivity is heterogeneous across firms and exogenously and permanently assigned according to a twice differentiable distribution $\Gamma(p)$.

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

6.2 Workers

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 $H(\cdot)$.[39] 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

[38]We can obtain similar results by positing impatient firms that post vacancies and then learn their productivity sequentially. A multi-stage decision for the firm has precedent in Lalive et al. (2015). Such a multi-stage decision would allow us to endogenize the job-finding rate; however, since we have already noted that policy-induced variation in the job-finding rate is insufficient to capture our empirical results we forgo developing this here in favor of a simpler version of the model that parsimoniously captures the novel insights.

[39]Given the discontinuity in the distribution of benefits, it is not obvious that the distribution of wage offers is differentiable. Indeed, it is not everywhere differentiable. We assume, and later verify, that where $h(\cdot)$ is required for equilibrium definition it is well defined.

unemployment.[40] 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.[41]

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 $rU(b) = rE(w_R(b))$—is therefore implicitly defined 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

[40] For simplicity of exposition we write the value of unemployment as a function of a flow wage with is constant over time and an option value of separation. However, the model easily admits on the job search and wage growth modeled as Bertrand competition in the on-the-job search block—as in Postel-Vinay and Robin (2002b,a)—without any alteration to the Bellman equation of the unemployed. The assumptions supporting such a model—unemployment benefits are not observable to prospective employers but the productivity of a current incumbent employer is—are not unreasonable. Modeling wage-posting or Bertrand competition with bargaining—as in Burdett and Mortensen (1998) or Cahuc, Postel-Vinay and Robin (2006), respectively—yield a significantly more complex reservation wage equation for the unemployed. However, under plausible calibrations—in particular, those consistent with observed transition rates—do not alter the main results, which derive from the shape of the benefit distribution.

[41] 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 such innovations. This alternative introduces tedium but does not alter our results.

of variables and imposing $b = min\{c, \rho\chi\}$ we can write

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

Meanwhile, since $J(w) = G(w_R^{-1}(w))$ and $\frac{dw_R}{db} = \frac{r+\delta}{r+\delta+f[1-H(w_R(b))]}$

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

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 $\{c, \rho\}$, the distribution $J(\cdot)$ is differentiable and concave on its interior.

6.3 Firms

We next turn 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 a reservation wage less than or equal to w.[42] 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

[42] As noted earlier, we can obtain similar results by positing impatient firms that post vacancies and then learn their productivity sequentially. What follows would then describe the second stage of the firm's decision, after the volume of vacancies is set. A multi-stage decision for the firm has precedent in Lalive et al. (2015). Such a multi-stage decision would allow us to endogenize the job-finding rate; however, since we have already noted that policy-induced variation in the job-finding rate is insufficient to capture our empirical results we forgo developing this here in favor of a simpler version of the model that parsimoniously captures the novel insights.

(implicitly) 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

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

Note that $h(w)$ is well defined for $w < \tilde{w}_P$, affirming the assumption made earlier.

The firms' optimality conditions are illustrated in figure 2. At interior solutions, a firm's optimality conditions can be illustrated in a standard monopsony diagram, with the small adjustment that we plot wages on the x-axis because w is the choice variable. As we have shown in Section 6.2, the reservation wage distribution $J(w)$ is a concave function with a mass at its maximum, c. In each panel, $J(W)$ is illustrated by the solid black lines. Marginal cost can be derived in the usual way and is illustrated by the black dashed lines. Since the production technology is linear in labor, the marginal revenue curve is a vertical line at p, illustrated by the pink dashed lines. As in the standard monopsony problem, the profit-maximizing interior solution equates marginal revenue and marginal cost. The optimal wages can be observed as the x-coordinate of the intersection between $J(w)$ and the horizontal drawn from this intersection. To check for a corner solution, one must compare profits at the interior solution to profits at the corner solution. To this end, we illustrate firms' profit-maximizing iso-profit curves as the solid pink lines. Given firms' objective function, these are hyperbolas with horizontal and vertical asympototes at 0 and p, respectively. In Panel A, the interior solution is profit maximizing. In Panel B, the corner solution maximizes profits. In Panel C, the corner and interior solutions yield equal profits. Observe that, since the iso-profit curve is a hyperbola with asymptotes 0 and p its slope is everywhere flatter for higher p. Thus, for the same $J(w)$, firms with optimality described by Panel A (interior solutions) are less productive than firms described by Panel B (corner solutions) and the indifferent firm (Panel C) has productivity at the boundary, illustrating the monotonicity of the mapping $w_P(p)$.

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.

Discussion

As stated above, Diamond (1971) guarantees that, for $b = c$, all workers hold the same reservation wage, which is equal to c.[43] 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 successfully hiring every worker they meet. This implies that, across workers in the cross-section, the job finding rate is decreasing and the unemployment rate rising in b 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.

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

Proof of Proposition 1 is provided in appendix A.1. Here we provide intuition. In the case of the cap there are two, offsetting, effects. First, some firms now offer higher wages commensurate with the new higher cap. Second, some firms switch between offering wages at the cap to wages strictly in the interior. In principle, the sign of the second effect is ambiguous; indeed, our goal is to show that these firms switch from cap to interior and not the other way around. For intuition it is useful to consider a counterfactual in which the

[43] We interrogate the realism and implications of this result in Section 10.

set of firms offering at the cap is unchanged. In this case the shift in the reservation wage distribution is unambiguously positive. But, we prove in appendix A.1 that the initially $\tilde{p}$ firm strictly prefers the interior solution in this counterfactual. In other words, the rightward shift in the reservation wage distribution cannot be large enough to induce a firm which previously set wages in the interior to set wages at the cap. Rather, in the new equilibrium, some firms that previously offered the old cap now offer wages in the interior and the new $\tilde{p}$ firm is more productive than the old. The case of the effective replacement rate is simpler. The change in the replacement rate shifts the reservation wage distribution to the right on the interior but has no effect on the location of the max. Thus, some firms which previously offered wages in the interior now strictly prefer to increase wages to the cap.

Figure 3 panels A and B illustrate the equilibrium effect of an increase in the replacement and in the cap, respectively. Dotted black lines trace out the cumulative distribution $J(w)$ prior to the policy change, while solid black lines trace $J(w)$ after the policy change. Firm iso-profit curves appear in pink and blue. Higher profits lie toward the northwest. Recall that the firm's iso-profit curve is a hyperbola with horizontal and vertical asymptotes at 0 and p, respectively. As such, the slope of the iso-profit curve of a more productive firm is less than that of a less productive firm, given the same argument. The vertical asymptotes for each firm, which are plotted at their productivities (p) on the horizontal axis, appear in long dash, matching the colors of the iso-profit curves.

In panel A, 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 (pink) now strictly prefers to offer wages at the cap. Meanwhile, we can trace out the iso-profit curve of a new marginal firm (blue) and observe that this firm is of lesser productivity.

In panel B, 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 (pink) 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.

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 ρ **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 ρ 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.[44] 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.

[44] 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 $\tilde{b}$.

8 Model Validation

The model offers additional testable implications with respect to wages and to the job finding rates of directly-treated individuals "just shy of the cap".

8.1 Wage Distribution and Wage Responses to Policy Variation

Our model implies stark predictions regarding the distribution of wages. Specifically, there should be bunching in the wage schedule at the reservation wage of workers with benefits at the cap. Evidence of such bunching has been studied in the public finance literature with respect to kinks in the tax schedule. In a closely related paper, Chetty et al. (2011) document evidence of bunching at kinks even for individuals who are themselves not subject to the tax rules that yield the kink. However, in our context, evidence of bunching is muddied by the less direct mapping between flow values of unemployment benefits and reservation wages than the mapping between before and after tax wages studied in Chetty et al. (2011).[45] For example, there could be heterogeneity in the flow values of leisure, workers' labor market productivities, and the types of labor contracts. The first of these would directly smooth the wage distribution while the second two would distort the relationship between the observed wage distribution and the value of employment at those wages. Ultimately, while the BAM data allow us to observe distributions of wages, they are composed exclusively of job separators and are too far removed from the wage distribution of job finders to provide meaningful evidence of bunching in light of these effects.

Our model also makes predictions regarding the interaction between policy and reemployment wages. Specifically, an increase in c should have a positive effect on the reemployment wages of workers with benefits "just shy of the cap" while an increase in ρ should have *no effect* on the reemployment wages of workers with benefits at the cap. Using our SIPP

[45] In the interest of parsimony we exclude these from our baseline model. In Section 6 we hinted at how these features could be included in the model and not contradict the key predictions regarding job finding rates. In section 10 we discuss our expositional choice in greater detail.

data we test for these predictions but do not find evidence for or against them.[46] In large part this is due to the data demands of our empirical design, which requires significant pre-displacement earnings to be recorded. Further requiring enough observed reemployment earnings to generate a reliable measure of wages leaves us with a small and selected sample, given the panel lengths of the SIPP. In addition, we note that—as in the case of bunching—the model implies effects of policy changes on the value of employment, which may not necessarily translate to the new hires' observed remitted wages in the presence of long-term labor contracts (Kudlyak, 2014; Basu and House, 2016; Doniger, 2021).[47] Further, absence of an effect of UI on reemployment wages is not an uncommon finding even in longer panels, see, for example, Jäger, Schoefer, Young and Zweimüller (2020).

8.2 Job Finding Rates of Directly Treated Individuals

Specifically, the model implies that the job finding rate of job seekers "just shy of the cap" is identical to that of capped seekers, where "just shy of the cap" is defined as those with reservation wages between the interior solution of the $\tilde{p}$ firm and the cap. The relative similarity between coefficients on the sub-sample "just below the cap" and at the cap, reported in table 7 columns (I) and (II), as compared to those well below the cap conform with this model prediction. While none of the respective just-below and at-cap effects are statistically different from each other in columns (I) and (II), each is always statistically different from the well-below estimates.[48]

To investigate further we redefine the definitions of the sub-samples to match the model exactly. Defining the "just shy of the cap" as the sub-sample threshold requires us to find $\tilde{b}$ under each policy regime. Equations 6.4 implies $w_R(\tilde{b}) = c - \frac{J(1-J)}{j}$ and, while we don't

[46]Results available upon request.

[47]This is not to say that we believe the SIPP earnings data to be unreliable. Rather, the mapping from remitted earnings to UI benefits is more direct than between remitted wages and the value of employment, especially in the presence of long-term contracting. Further, accounting for the divergence in remitted wages and the value of employment require extensive post-reemployment wage histories.

[48]In column (I), testing that well-below equals just-below produces a p-value of 0.029, while testing that well-below equals at-cap produces a p-value of 0.083. In column (II), the same comparisons produce p-values of 0.030 and 0.049 for the automatic effects and 0.032 and 0.049 for the legislated effects.

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.[49]

Column (III) of table 7 reports coefficients based on the model-consistent definitions of subsamples. Notably, the estimates are little altered and the coefficient on the just below cap subsample remains close to that on the capped sub-sample. Additionally, the implied elasticities from legislated changes for these two subsamples are very close to each other at -1.3 and -1.2, respectively. As was the case in columns (I) and (II), none of the respective just-

[49]Note, in states that define benefit allowances for claimants' dependants, UI recipients may receive benefits above the cap. We note that in such states the mass point generated by the cap is still large and salient. In calculating $\tilde{b}$ we find the interior solution that yields equal profits as setting a wage at the statutory cap in all states.

below and at-cap effects are statistically different from each other, and each is statistically different from the well-below estimates.[50]

9 Implications for Estimating Direct Effects

What do our results, particularly those of the preceding section, mean for difference-in-differences estimates of the direct effects? In an exemplary study, Meyer and Mok (2014) exploit the kink in the UI benefit schedule to identify nominally treated and untreated group when the cap on benefits rises: those with benefits set above and below the cap in the state's benefit schedule. In that study, they then use the untreated group as a control for the treated group to derive a difference-in-differences estimate of the effect of UI benefits on the treated. Their estimated effect is negative but much smaller in absolute value than the modal range of the literature. In light of the evidence we present here, the small size of the Meyer and Mok (2014) estimate likely derives in part from spillovers that attenuate differences between the directly- and indirectly-treated groups when the cap changes. Most explicitly, Meyer and Mok (2014) effectively pool the well below and "just shy" samples as the "untreated" control group while our estimates and model suggest that the behavior of the "just shy" should be and is the same as that of the directly treated group. This biases the difference-in-differences estimate toward zero if the WPRS model even partially describes the market, as we believe we have shown it does. In light of the market structure, a more properly posed difference-in-differences estimate should be the difference between the "well below" coefficient and the coefficient on the pooled "just shy" and capped sub-samples. Using our sample of cap changes, this yields an estimate of the elasticity of job-finding to one's own benefit of approximately -0.35, considerably larger than Meyer and Mok (2014) and closer to the modal range of literature.[51]

[50] Testing that well-below equals just-below produces a p-value of 0.023 and 0.050 for automatic effects and legislated effects, respectively. Testing that well-below equals at-cap produces a p-value of 0.024 and 0.051.

[51] One could, in principle, conduct a similar confirmatory test and difference-in-differences analysis based on replacement rate changes. However, in our data the "just below" and "just shy" sub-samples corresponding to replacement rate change policy experiments are too small to yield precise results.

10 Is the Typical Policy Optimal?

A novel feature of wage posting with random search is the possibility of positive spillovers onto the less-poor stemming from policies that offset poverty. We have shown that, empirically, spillovers are large relative to the direct effects of UI on job seekers. The question remains: is the size of the population experiencing these positive spillovers large enough to offset the effect on the directly-treated population?

It is straightforward to make a back-of-the-envelope calculation addressing this question. Specifically, the effect of a change in the replacement rate on the average job-finding rate of insured job seekers will be the simple weighted average of the effects on the directly- and indirectly-treated groups:

$$\varepsilon_{f,\rho}^{direct} f^{direct} G(\tilde{b}) + \varepsilon_{f,\rho}^{indirect} f^{indirect} (1 - G(\tilde{b})) \tag{10.1}$$

where $\varepsilon_{f,\rho}^{direct}$ and $\varepsilon_{f,\rho}^{indirect}$ are the elasticities of the job-finding rates with respect to a change in the replacement rate for individuals who are directly and indirectly treated, respectively.

The literature finds $\varepsilon_{f,\rho}^{direct}$ between -0.4 to -0.8 (Chetty, 2008). Meanwhile, we find $\varepsilon_{f,\rho}^{indirect}$ of 2.0, more than twice as large in absolute value! Meanwhile, an upper bound on $G(\tilde{b})$ is $G(c) \approx 0.66$ in the data. Thus, we conclude that in the modal configuration of policy in the US, the replacement rate could be increased without reducing the average job-finding rate if the cap were held constant. Indeed, our estimates suggest that such a change would lead to a *reduction* in the unemployment rate.

What does this mean for welfare? Could a planner improve welfare by increasing ρ and financing the increase with a tax on the c-scheduled seekers? The parsimonious model we use to illustrate the predictions of WPRS lacks key features necessary for analysis of optimal policy. In particular, it lacks a payoff from search for searchers with capped benefits: in the model, these individuals are equally well off employed as unemployed. Indeed, as we noted earlier, these workers and the firms that hire them compose a version of the Diamond (1971)

Paradox.[52] Without a payoff from search for these seekers, adjusting policy to increase their job-finding rate, as our back-of-the-envelope suggests is possible, does not improve their welfare. As a result, a Baily (1978)-Chetty (2006) formula for optimal ρ still applies.[53]

This stark and simplistic result is almost surely unrealistic. If, instead, the value of employment exceeds the value of unemployment for c-schedule workers, then ρ higher than suggested by Baily (1978)-Chetty (2006) is optimal because costs induced by excess moral hazard relative to the consumption smoothing benefits for the ρ-schedule individuals are offset by the increased job-finding rate of the c-schedule workers. Available evidence, while imperfect, suggests that value of employment does, indeed, exceed the value of unemployment for these c-schedule workers. To these authors' knowledge, the closest available estimates in the literature follow the tradition of Gruber (1997) and study the consumption response to unemployment shocks and its covariation with UI (Browning and Crossley, 2001; Bloemen and Stancanelli, 2005; East and Kuka, 2015; Ganong and Noel, 2019; Ganong et al., 2021). Given that these studies document a decline in consumption upon unemployment, we may infer that workers are better off when employed than unemployed. In addition, the relation

[52]One could supply a payoff from search to c-schedule individuals by positing on-the-job search either constrained by wage posting (à la Burdett and Mortensen (1998); Bontemps, Robin and van den Berg (1999, 2000)) or during which workers obtain some bargaining power in addition to the improving value of their outside option (a la Cahuc et al. (2006)). Such an approach appears appealing since it also supplies a micro-founded wage distribution and, thereby, χ distribution, which the parsimonious model, unrealistically, takes as exogenous. However, when such models are asked to generate realistic wage dispersion while respecting observed labor market flows, they contradict plausible flow values of non-employment (Eckstein and Wolpin, 1990; Hornstein et al., 2011; Doniger, 2014). While solutions have been proposed, none is parsimonious or universally appealing. Very low values of leisure may not be implausible if there are non-pecuniary benefits to employment, for example Burdett et al. (2020). However, in such a model, reservation wages will be highly sensitive to shifts in the wage offer distribution. A solution to implausibly high sensitivity is to assume that on-the-job search is as efficient or nearly as efficient as search while not employed, for example as in van Vuuren, van den Berg and Ridder (2000). However, such an assumption contradicts observed job finding and job-to-job flows data. An alternative solution is to posit that unemployed and employed workers face fundamentally different opportunities. Faberman et al. (2022) have documented evidence of such differences and models, for example Postel-Vinay and Robin (2002b,a) or Bradley and Gottfries (2021), exist that provide plausible micro-foundations. Yet another alternative is to model a mixture of wage-contracting mechanisms competing in the same labor market. Such a models can generate both realistic wage dispersion plausible option value of search, for example Doniger (2014). However, the UI policies studied here reveal relatively little about the opportunities and constraints faced by employed workers.

[53]Note, the model suggests that since capped seekers are equally well off in employment as unemployment their change in consumption at job-loss should be zero, given our and the literature's estimate of a negative effect of increasing c on capped seekers on job-finding rate this implies that they are over insured in a Baily (1978)-Chetty (2006) framework.

between consumption declines upon unemployment and pre-displacement earnings is non-negative.[54] That is, based on this evidence, c-schedule workers gain at least as much from reemployment as ρ-schedule workers.

What about firms? Under our estimates, equation 10.1 implies that employment *rises* when ρ rises. How is this consistent with the average increase in UI benefits? In other words, why do we not see a wage effect? The model's answer: while fewer vacancies are posted, the yield on these is greater since they are at higher wages. Thus, the proposed increase in ρ would increase labor share but not at the expense of total output. To corroborate this, we appeal to studies documenting the "trickle-up" effects of minimum wages noting that shifting reservation wages via UI policy is likely to have a similar effect on the wage distribution. Lee (1999) and Autor et al. (2016) find effects of minimum wages on percentiles of the distribution strictly above where they bind. However, Autor et al. (2016) fails to reject that observed effects are not due to measurement error. Thus, we infer that UI also likely has modest effects of the overall wage distribution, and therefore modest "wage-effects" on the both the labor share and the vacancy posting margins.

To summarize, based on our read of the literature regarding structural labor market models and the empirical literature regarding the utility returns to reemployment, precise statements regarding optimal policy are elusive. That said, we are confident that our method and results offer clear evidence that in the modal configuration of policy and in a standard Baily (1978)-Chetty (2008) formula, ρ is too low.

[54]This result is substantive considering that one's prior might be that c-schedule workers, by virtue of their higher past earnings, might be better able to self-insure and would therefore have lower rather than higher sensitivity of consumption to job loss. Still, in the presence of a job-ladder or tenure-contingent pay Gruber (1997) style estimates overstate the difference between the value of reemployment and unemployment. As with self-insurance, one could worry that c-schedule workers are positively selected on tenure or wrung of the job-ladder, making the change in their consumption upon job loss an over estimate of the change in their consumption upon job-finding. One would prefer estimates of the change in consumption upon job finding for workers at and workers below the cap. The simple (but likely unrealistic) model present in this paper would be confirmed if consumption rises for those with benefits below the cap but remains constant for following job finding for those with benefits at the cap. To our knowledge, no such study exists but this result seems unlikely in the context of the existing literature.

11 Conclusion

In the cutting-edge theory of spillovers from active labor market policies, spillovers onto indirectly treated job-seekers arise from changes in the average level of reservation wages and from the relative search behavior of job-seekers (Lalive et al., 2015; Landais et al., 2018). The two effects have opposing sign and the aggregate effect reflects the balance of the two. In this paper we leverage an understudied feature of the US unemployment insurance (UI) system—the dollar cap on benefits—to document spillovers from policy variation. The pattern that we document, however, is neither easily reconcilable with the cutting-edge theory nor the canonical models whose intuition it nests.[55] Instead, we show that the pattern of spillovers that we document is reconciled by a wage redistribution effect captured by a model of wage posting with random search (WPRS). Such a model suggests that progressive changes in the policy environment—e.g. changes that shift the dispersion of UI benefits toward the cap—might reduce unemployment. Our estimates imply that for the typical policy configuration in the US, the replacement rates could indeed be increased without decreasing the average job-finding rate. In addition, our results shed light on existing difference-in-differences estimates of the direct effect of UI benefits on job finding, suggesting they are attenuated. Adjusting for the spillovers we identify brings these estimates closer to the modal estimate of direct effects in the literature.

The results documented herein, particularly pertaining to spillovers, contribute novel facts to the literature and provide the first empirical evidence based on observational data of policy-relevant spillovers arising from information asymmetries in wage setting in the United States.[56] This complements evidence of similar market structures in other parts of

[55] Both Neoclassical and Directed Search models provide for a positive effect via substitution. Meanwhile, the Diamond-Mortensen-Pissarides framework provides for a negative effect via the average reservation wage of the unemployed.

[56] Additionally, we have shown that our data and analysis offer a fresh look at the literature regarding the direct effects of UI on recipients. Using a variety of identification strategies, we obtain estimates of these effects broadly in line with the literature. Further, we offer a refinement of a recent difference-in-differences approach that, in light of the model we have validated, removes a bias. Our refined difference-in-differences estimate is in the central tendency of the literature whereas Meyer and Mok (2014) is outlying and small in absolute value.

the labor market, in other contexts, and in other countries. For example, Lee (1999); Autor et al. (2016) and Engbom and Moser (2021) document that minimum wages "trickle-up" the wage distribution, shifting percentiles much higher in the distribution than where the minimum wage binds. As another example, Staiger et al. (2010); Hjort et al. (2020); Willén (2021) and Derenoncourt et al. (2021) document wage and employment responses in similar and plausibly competing employers when VA hospitals and large employers (e.g. Amazon), respectively, raise employer-level minimum wages. Each provides evidence that an individual employer's wage offers are sensitive to the distribution of wage offers and outside options faced by current and potential employees. Most closely related to our results, Chetty et al. (2011) document spillovers arising from kinks in the Danish tax schedule. In that context, the wages of workers for whom the particular kink is irrelevant nonetheless exhibit bunching around the kink. This phenomenon is consistent with information frictions in which firms, unable to filter according to worker's exposure to the kinks, pre-commit to wages that are optimal on average and workers are unable to perfectly sort: in other words, though not explicitly stated as such, WPRS.

With respect to optimal policy, our estimates suggest that, given the typical cap, the typical effective replacement rate is too low and that unemployment among the insured could, on average, be *reduced* by *increasing* benefits. The empirical result offers the first confirmation, to these authors' knowledge, that this theoretical possibility—first studied by Albrecht and Axell (1984)—in fact pertains in reality. We have argued that welfare, in addition to employment, would be increased by raising replacement rates.[57]

Finally, we make a policy-relevant comparison. Our estimates, as with all estimates, should be taken with a grain of salt when extrapolating beyond small marginal adjustments. However, the model of wage posting with random search, which we validate here, differs considerably from that of Landais et al. (2018) in its implications for the magnitude of spillovers as treatment becomes larger or near universal. In the Landais et al. (2018) model,

[57]In section 10, we have discussed the requisite but absent features, the intuition regarding these features that can be gleaned from the empirical literature, and the challenges to including these in the model.

positive spillovers arise from marginal differences in who gets to the scarce job offer first. Thus, if treatment is applied to everyone, positive spillovers are annulled.[58] In contrast, in the WPRS model, positive spillovers arise from eliminating information frictions that impose dead weight loss. As a result, positive spillovers should continue to accrue until the distribution of UI benefits collapses to a single point.

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[58] Similarly, in a Neoclasical model raising both workers' prices proportionately will not alter relative employment.

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Tables

Table 1: 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)	4.38	6.85	3.73
Initial Monthly Job-finding Rate (CPS)	0.491	0.532	0.481
Deviation from State Trend	-0.005	0.009	-0.009
Deviation from Contemp. National Average	0.022	0.019	0.023
Policy Changes	694	145	549

Panel B: Policy Changes to the Replacement Rate		
	All	Excl. Changes to Benefit Min
Share of claimants at cap	0.30	0.32
Initial Replacement Rate	0.744	0.756
Δ Rep. Rate	-0.005	-0.028
Initial Monthly Job-finding Rate (CPS)	0.477	0.453
Deviation from State Trend	-0.025	0.027
Deviation from Contemp. National Average	0.026	0.027
Policy Changes	144	14

Notes: Automatic cap changes are those in which the cap is updated by a mechanical formula set by previous legislation. In legislated changes, the legislation sets the cap to a specific number or changes the mechanical formula.

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

Table 2: Sample Means: Individuals Observed in Unemployment in the SIPP

Panel A: Policy Changes to the Benefit Cap						
Subsample:	Just Below the Cap (Indirectly Treated)			At the Cap (Directly Treated)		
	All	Leg	Auto	All	Leg	Auto
Male	0.56	0.52	0.57	0.63	0.57	0.65
Age	39.8	39.8	39.8	40.9	39.9	41.2
Years of education	13.1	12.7	13.2	13.6	13.4	13.7
Weekly job-finding rate	0.062	0.070	0.060	0.078	0.076	0.079
Individuals	4,079	1,040	3,039	4,625	1,310	3,315
Policy changes	591	134	457	615	137	478

Panel B: Policy Changes to the Replacement Rate				
Subsample:	Just Below the Cap (Directly Treated)		At the Cap (Indirectly Treated)	
	All	Excl. Changes to Benefit Min	All	Excl. Changes to Benefit Min
Male	0.56	0.55	0.68	0.72
Age	39.1	40.1	40.9	38.4
Years of education	12.9	12.6	13.5	13.2
Weekly job-finding rate	0.068	0.079	0.076	0.068
Individuals	1,149	93	1,329	104
Policy changes	119	12	130	11

Notes: "Just Below the Cap" subsamples comprise claimants with calculated UI benefits between 75 percent of the larger of the two caps and the smaller of the two caps. "At the Cap" subsamples comprise claimants with calculated benefits at least as large as the smaller of the two caps. UI benefits are calculated using reported earnings and a UI calculator generated from a detailed reading of state legislation. Automatic cap changes are those in which the cap is updated using a mechanical formula set by previous legislation. Legislated cap changes are those in which the cap is set to a specific number in the legislation or the mechanical formula is changed by legislation.

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

Table 3: Sample Means: Indirectly-Treated Individuals Observed in Unemployment in the SIPP

Panel A: Policy Changes to the Benefit Cap – Just Below Subsample						
Subsample:	Pre-Change			Post-Change		
	All	Legislated	Automatic	All	Legislated	Automatic
Male	0.57	0.53	0.58	0.55	0.52	0.55
Age	39.8	39.4	39.9	39.8	40.1	39.8
Years of education	13.1	12.7	13.2	13.1	12.8	13.2

Panel B: Policy Changes to the Replacement Rate – At Cap Subsample				
Subsample:	Pre-Change		Post-Change	
	All	Excl. Changes to Benefit Min	All	Excl. Changes to Benefit Min
Male	0.68	0.74	0.68	0.71
Age	40.8	37.1	41.0	39.7
Years of education	13.6	13.3	13.4	13.0

Notes: "Just Below the Cap" subsamples comprise claimants with calculated UI benefits between 75 percent of the larger of the two caps and the smaller of the two caps. "At the Cap" subsamples comprise claimants with calculated benefits at least as large as the smaller of the two caps. UI benefits are calculated using reported earnings and a UI calculator generated from a detailed reading of state legislation. Automatic cap changes are those in which the cap is updated using a mechanical formula set by previous legislation. Legislated cap changes are those in which the cap is set to a specific number in the legislation or the mechanical formula is changed by legislation.

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

Table 4: Effects of Policy Changes on Job-Finding for Indirectly-Treated Subsamples, Linear Probability Models

LHS=Job-Finding Rate	Subsample:			
	Just Below		Capped	
	I	II	III[a]	IV[b]
Log Benefit Cap	-0.07^{**}			
	(0.03)			
× Legislated		-0.08^{***}		
		(0.03)		
× Automatic		-0.06		
		(0.05)		
Replacement Rate			0.20^{**}	0.20^{**}
			(0.09)	(0.10)
Implied Elasticity at Mean (Leg; Auto)	-1.2	-1.2; -1.0	2.0	1.9
Observations				
Individuals	29,625	29,625	8,169	8,169
Policy Changes (Leg; Auto)	694	145; 549	144	144
R-Squared	0.12	0.12	0.11	0.11
Controls	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.

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.

[a] Excluding coincident legislated cap changes

[b] Identified using only variation derived from changes to the statutory replacement rate, definition of insured earnings, or non-linear benefit schedule governing the interior of the distribution. There are 14 such changes.

Table 5: Effects of Policy Changes on Job-Finding for Indirectly-Treated Subsamples, Proportional Hazard Models

LHS=Job-Finding Rate	Subsample:			
	Just Below		Capped	
	I	II	III[a]	IV[b]
Log Benefit Cap	-1.10^{***}			
	(0.42)			
× Legislated		-1.13^{***}		
		(0.37)		
× Automatic		-1.01		
		(0.73)		
Replacement Rate			1.92^{***}	2.13^{***}
			(0.65)	(0.66)
Observations				
Individuals	29,625	29,625	8,169	8,169
Policy Changes (Leg; Auto)	694	145; 549	144	144
Controls	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.
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.
[a] Excluding coincident legislated cap changes
[b] Identified using only variation derived from changes to the statutory replacement rate, definition of insured earnings, or non-linear benefit schedule governing the interior of the distribution. There are 14 such changes.

Table 6: Pre-Trend Placebo Tests, One-Year Lagged Samples

LHS=Job-Finding Rate	Subsample:			
	Just Below		Capped	
	I	II	III[a]	IV[b]
Log Benefit Cap	0.01			
	(0.09)			
× Legislated		0.12		
		(0.11)		
× Automatic		−0.03		
		(0.09)		
Replacement Rate			0.05	0.02
			(0.12)	(0.13)
Observations				
Individuals	28,644	28,644	8,506	8,506
Policy Changes (Leg; Auto)	671	135; 536	140	140
R-Squared	0.12	0.12	0.11	0.11
Controls	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.

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.

[a] Excluding coincident legislated cap changes

[b] Identified using only variation derived from changes to the statutory replacement rate, definition of insured earnings, or non-linear benefit schedule governing the interior of the distribution. There are 14 such changes.

Table 7: Effects of Cap Changes on Job-Finding for All Subsamples

LHS=Job-Finding Rate	Subsample definitions:		
	Simple[a]		Model Based[b]
	I	II	III
Log benefit cap			
× Well below cap	−0.047		
	(0.032)		
× Just below cap	-0.074^{**}		
	(0.030)		
× At cap	-0.072^{**}		
	(0.033)		
Log benefit cap × automatic changes			
× Well below cap		−0.033	−0.033
		(0.049)	(0.047)
× Just below cap		−0.060	−0.062
		(0.047)	(0.045)
× At cap		−0.063	−0.063
		(0.049)	(0.047)
Log benefit cap × legislated changes			
× Well below cap		−0.057	-0.059^{*}
		(0.035)	(0.034)
× Just below cap		-0.084^{**}	-0.088^{***}
		(0.034)	(0.032)
× At cap		-0.088^{**}	-0.090^{***}
		(0.034)	(0.033)
Observations			
Individuals	29,625	29,625	29,625
Policy Changes (Leg; Auto)	694	145; 549	145; 549
Controls	X	X	X
Policy change FE	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.

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.

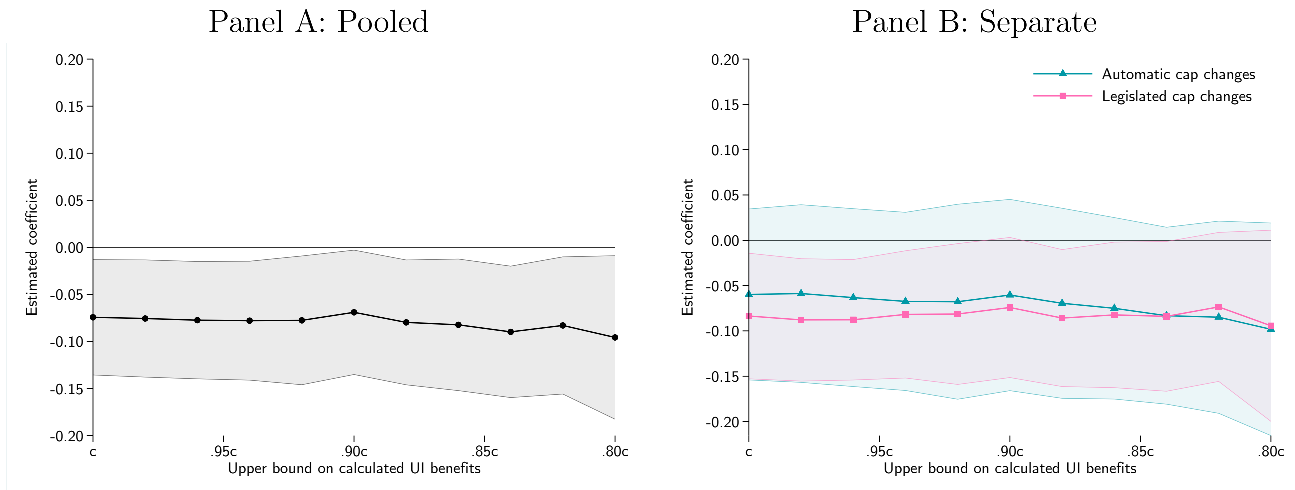
Source: Census Bureau's Survey of Income and Program Participation 1986 to 2008 panels, authors' legislative research, and authors' calculations.

[a] Simple definition: border of "Well below" and "Just below" subsamples is set at 75 percent of the larger of the two caps.

[b] Model Based definition: border of "Well below" and "Just below" is set at the larger of the two calculated $\tilde{b}$ values for the policies as described in Section 9.

Figures

Figure 1: Robustness of Linearly-Estimated Cap Change Effects to Sample Trimming



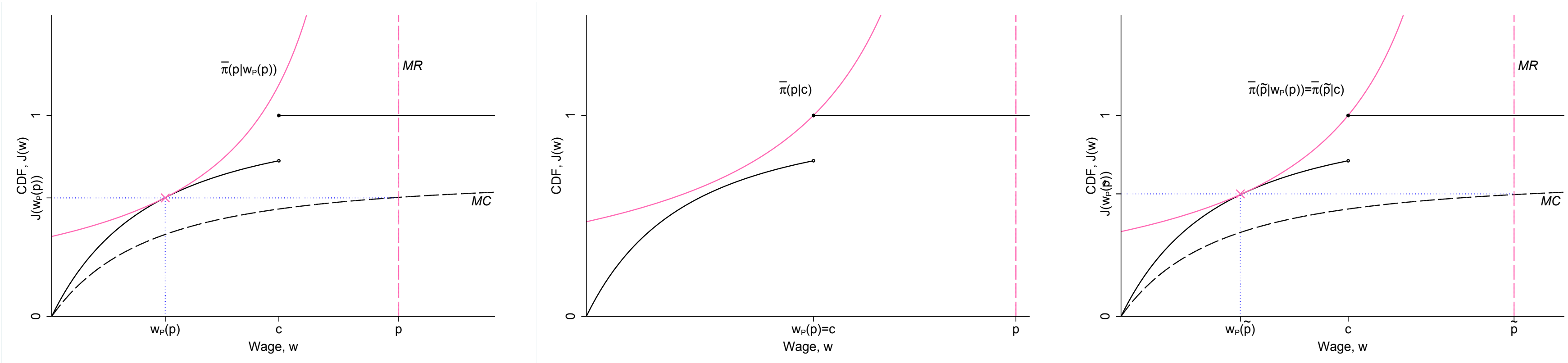
Notes: Each point displays a point estimate of the effect of the log cap on the indirectly-treated "just below" subsample, analogous to columns 1 and 2 in Table 4. Shaded areas indicate 95 percent confidence intervals, with errors clustered at the state level. Moving from left to right within each panel, more of the "just below" subsample that is close to the cap is trimmed from the estimation sample, as described in Section 5.1.

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.

Figure 2: Illustration of Individual Firms' Optimality Conditions.

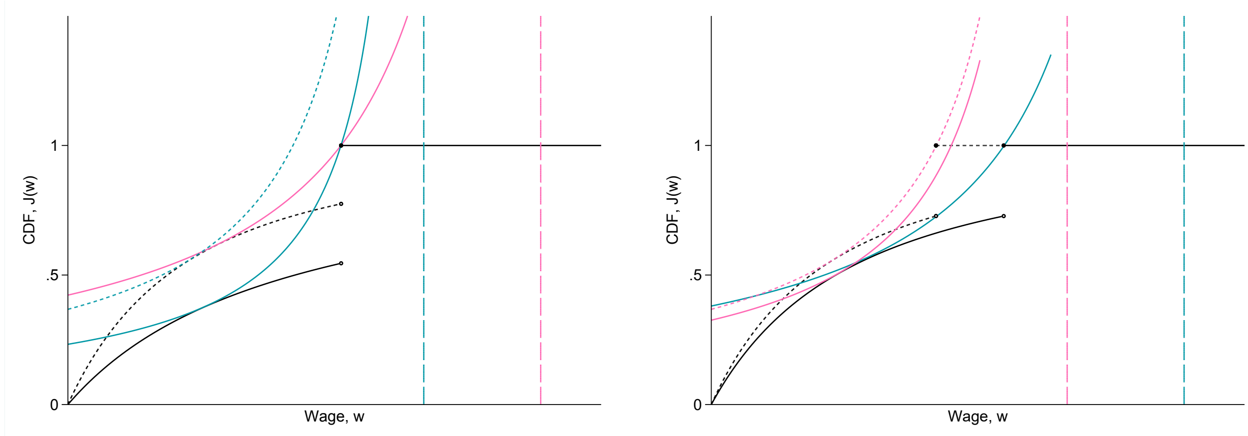
Panel A: Interior Solution ($p < \tilde{p}$)	Panel B: Corner Solution ($p > \tilde{p}$)	Panel C: Indifference ($p = \tilde{p}$)



Notes: At interior solutions a firm's optimality conditions can be illustrated in a standard monopsony diagram, with the small adjustment that we plot wages on the x-axis sine these are the choice variable. As we have shown in Section 6.2 the reservation wage distribution $J(w)$ is a concave function with a mass at its maximum, c. In each panel $J(W)$ is illustrated in the solid black lines. Marginal cost can be derived in the usual way and is illustrated by the black dashed lines. Since the production technology is linear in labor the marginal revenue curve is a vertical line at p, illustrated by the pink dashed lines. As in the standard monopsony problem, the profit maximizing interior solution equates marginal revenue and marginal cost and the optimal wages can be observed as x-coordinate of the intersection between $J(w)$ and the horizontal drawn from this intersection. To check corner solution one must compare profits at the interior solution to profits the corner solution. To this end we illustrate firms' iso-profit curves as the solid pink lines. Given firms' objective function, these are hyperbola with asympototes at 0 and p. In Panel A the interior solution is profit maximizing. In Panel B, the corner solution. In Panel C corner and interior solutions yield equal profits. Observe that, since the iso-profit curve is a hyperbola with asymptotes 0 and p its slope is everywhere flatter for higher p. Thus, for the same $J(w)$ firms with optimality described by Panel A (interior solutions) are less productive than firms described by Panel B (corner solutions) and the indifferent firm (Panel C) has productivity at the boundary, illustrating the monotonicity of the mapping $w_P(p)$.

Figure 3: Model Implied Equilibrium Effects of Policy Changes

Panel A: An Increase in the Replacement Rate	Panel B: An Increase in the Benefit Cap



Notes: Panel A considers the effect of an increase in the effective replacement rate. Since workers below the cap receive more UI compensation, the distribution of reservation wages on this interval shifts right. Meanwhile, those at the cap are unaffected and their reservation wages remain constant. Prior to the shift, the firm with productivity indicated by the pink vertical line was indifferent between offering wages at the cap and wages strictly below, as illustrated. Meanwhile the firm with lower productivity, indicated by the blue vertical line strictly preferred to offer wages below the cap. After the shift, the more productive (pink) firm strictly prefers to offer wages at the cap while the less productive (blue) firm finds that it is indifferent between interior and corner solutions. In this scenario, all firms between these two productivity switch from interior to corner solutions. Panel B considers the effect of an increase in the benefit cap. The policy change has two effects on the reservation wage distribution. First, firms offering the cap now offer higher wages. Second, some firms previously offering at the cap may switch to offering below the cap or visa versa. The first effect shifts the reservation wage distribution to the right on the interval below the cap. The sign of the second effect and depends on the equilibrium wage posting response of firms. In the appendix, we show that the change in the cap must trigger some firms previously at corner solutions to switch to interior solutions, as illustrated.

A Model Appendix

A.1 Proof of Proposition 1

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

$$\frac{d\tilde{p}}{dc}[1 - J(w_P(\tilde{p}))] = 1 + (\tilde{p} - w_P(\tilde{p}))\frac{dJ(w)}{dc}\Big|_{w_P(\tilde{p})} + \frac{dw_P(\tilde{p})}{dc}[\underbrace{(\tilde{p} - w_P(\tilde{p}))j(w_P(\tilde{p})) - J(w_P(\tilde{p}))}_{\text{Envelope Theorem } \implies =0}]$$

$$\frac{d\tilde{p}}{dc} = \frac{1 + \overbrace{(\tilde{p} - w_P(\tilde{p}))\frac{\partial J(w)}{\partial c}}^{>-1}}{1 - J(w_P(\tilde{p})) - \underbrace{(\tilde{p} - w_P(\tilde{p}))\frac{\partial J(w)}{\partial \tilde{p}}}_{<0}} > 0. \quad \square$$

Alternatively, consider the $\tilde{p}$ firm's profit if c rises. If the firm sets wages at the new cap the change in profits are:

$$\frac{d\pi}{dc}\Big|_{corner} = -1.$$

If the firm sets wages at its best interior solution given the shift in the distribution of reservation wages that would occur c shifted but this firm remained threshold then the change in its profits are:

$$\begin{aligned}
\frac{d\pi}{dc}\Big|_{interior} &= \underbrace{-\frac{dw_p(\tilde{p})}{dc}J(w_P(\tilde{p}) + (\tilde{p} - w_P(\tilde{p}))\frac{dJ(w_P(\tilde{p}))}{dw_P(\tilde{p})}\frac{dw_p(\tilde{p})}{dc}}_{=0, \text{ Envelope Theorem}} + (\tilde{p} - w_P(\tilde{p}))\frac{\partial J(w_P(\tilde{p}))}{\partial c} \\
&= (\tilde{p} - w_P(\tilde{p}))\frac{\partial J(w_P(\tilde{p}))}{\partial c} \\
&= \frac{J(w_P(\tilde{p}))}{j(w_P(\tilde{p}))}\frac{\partial J(w_P(\tilde{p}))}{\partial c} \\
&= -\frac{J(w_P(\tilde{p}))}{j(w_P(\tilde{p}))}\left[\frac{\partial w_R(\underline{b})}{\partial c}j(w_R(\underline{b})) + \int_{\underline{w_R}}^{w}\frac{d^2 J}{dw^2}\frac{\partial w_R(x)}{\partial c}dx\right] \\
&> -\frac{J(w_P(\tilde{p}))}{j(w_P(\tilde{p}))}\left[j(w_R(\tilde{p}))\right] \\
&= -J(w_P(\tilde{p})) \\
&> -1.
\end{aligned}$$

Since, the shift in J due to the change in c if $\tilde{p}$ remains fixed is:

$$\frac{\partial J(w)}{\partial c} = -\frac{\partial w_R(\underline{b})}{\partial c}j(w_R(\underline{b})) - \int_{\underline{w_R}}^{w}\frac{d^2 J}{dx^2}\frac{\partial w_R(x)}{\partial c}dx$$

And the the shift in w_R due to the change in c if $\tilde{p}$ remains fixed is:

$$\frac{\partial w_R(x)}{\partial c} = \frac{f(1-\Gamma(\tilde{p}))}{r+\delta+f(1-\Gamma(p_R(x)))} < 1$$

Also, since $J(\cdot)$ is concave $j(w_R(b)) > j(w_R(\tilde{p}))$.

So, profits are strictly higher at the interior solution after the shift in c and, since $w_p(\cdot)$ is monotonically increasing, the new $\tilde{p}$ is greater than the old. $\square$

1(ii): Differentiating the indifference condition that defines $\tilde{p}$ with respect to ρ and applying the envelope theorem, we have

$$\frac{d\tilde{p}}{d\rho}[1-J(w_P(\tilde{p}))] = (\tilde{p}-w_P(\tilde{p}))\frac{dJ(w)}{d\rho}\Big|_{w_P(\tilde{p})} + \frac{dw_P(\tilde{p})}{d\rho}\underbrace{[(\tilde{p}-w_P(\tilde{p}))j(w_P(\tilde{p})) - J(w_P(\tilde{p}))]}_{\text{Envelope Theorem } \implies =0}$$

$$\frac{d\tilde{p}}{d\rho} = \frac{\overbrace{(\tilde{p}-w_P(\tilde{p}))\frac{\partial J(w)}{\partial\rho}}^{<0}}{1-J(w_P(\tilde{p})) - \underbrace{(\tilde{p}-w_P(\tilde{p}))\frac{\partial J(w)}{\partial\tilde{p}}}_{<0}} < 0. \quad \square$$

Alternatively, consider the change in the $\tilde{p}$ firm's profit if ρ rises. If this firm sets wages at the cap the change in profits are:

$$\frac{d\pi}{d\rho}\Big|_{corner} = 0.$$

If this firm instead sets wages at its best interior solution given the (the distribution of) reservation wages that occur if it remains the threshold firm then the change in its profits are:

$$\begin{aligned}\frac{d\pi}{d\rho}\Big|_{interior} &= \underbrace{-\frac{dw_p(\tilde{p})}{d\rho}J(w_P(\tilde{p}) + (\tilde{p}-w_P(\tilde{p}))\frac{dJ(w_P(\tilde{p}))}{dw_P(\tilde{p})}\frac{dw_p(\tilde{p})}{d\rho}}_{=0,\text{ Envelope Theorem}} + (\tilde{p}-w_P(\tilde{p}))\frac{\partial J(w_P(\tilde{p}))}{\partial\rho} \\ &= (\tilde{p}-w_P(\tilde{p}))\frac{\partial J(w_P(\tilde{p}))}{\partial\rho} \\ &< 0\end{aligned}$$

So, profits are strictly higher at the corner solution after the shift in ρ and, again, since $w_p(\cdot)$ is monotone increasing, the new $\tilde{p}$ is smaller than the old. $\square$

B Measurement

B.1 Local Macroeconomic Conditions

An obvious threat to identification stems from spurious covariation between local macroeconomic conditions and UI policy. To address this, we augment the SIPP data with local macroeconomic aggregates: state-level measures of separation rates and job-finding rates. We measure these labor market flows using the Bureau of Labor Statistic's Current Population Survey and the methods proposed by Shimer (2012) applied at the state level. In applying Shimer's methods we utilize CPS data extracted from the IPUMS-CPS project and the individual identifiers provided therein (Flood, King, Rodgers, Ruggles and Warren, 2020). While we could simply use these flow measures as controls in each state-month, we recognize that they may be endogenous to our policy parameters of interest. Therefore, we predict the job-finding and separation rates for each state-month using all other states in the same month. That is, calling f_{sm} the CPS-implied job-finding rate in state s at year-by-month m, we estimate the coefficients in $\ln f_{sm} = \kappa_0 + \sum_{r \neq s} \kappa_r^s \ln f_{rm} + \varepsilon_{sm}$. In short, for each state, we estimate a set of coefficients that allow us to predict that state's job-finding rate using all other states' contemporaneous job-finding rates. We execute this in logs to ensure positive predicted values in levels. When converting the predicted log value to levels, we assume the error terms are normally distributed. We use the same approach for state-by-month separation rates. In an effort to remove long-term trends in these predicted measures, we again follow Shimer (2012) by aggregating to the quarterly level and using the Hodrick-Prescott filter with smoothing parameter 10^5. As noted in section 4.2 policy changes do not occur during systematically outlying macroeconomic climates.

B.2 Consistent Policy Metrics over Time and Space

Our goal is to measure the elasticity of job finding rates for indirectly-treated individuals to the generosity of UI directed at workers who are similar but in an adjacent part of the UI schedule. In the ideal, simplified, policy space in which policies are defined by two parameters, ρ and c, as we suggested in section 2, inference can be conducted in a straightforward way. However, as we discussed in section 4, actual UI laws are sometimes described by a much less parsimonious set of parameters. To move forward, we use the BAM data to define measures of c and in particular ρ that are consistent across time and space using administrative measures.

Replacement Rate

Our econometric approach requires a succinct summary measure state policies that govern the below-cap segment of the UI schedule: the "effective replacement rate", which we have called ρ. However, actual state laws specify a variety of related parameters and the constellation of these parameters differs from state to state and within state over time and none of which map directly to ρ. Here, we harmonize the underlying data to generate a synchronous metric.

For clarity, we require some definitions. We define average weekly base period wages, χ^{52}, as $\frac{1}{52}$ of a worker's earnings accumulated in their base period, the four quarters ending two quarters before the claim UI. Insured earnings, $\breve{\chi}$, are the average weekly earnings accrued during a weak subset of the base period, as specified by a state at a particular time. The statutory replacement rate, $\breve{\rho}$, is the fraction of insured earnings the UI policy replaces. While the relationship between a percent change in $\breve{\rho}$ and ρ appears clear, the relationship between the definitions of insured earnings is less clear. However, we document that these sources of variation are no less important.

To fix ideas it is helpful to consider some examples. Over the horizon we study, Kentucky has insured earnings accrued during the full base period but altered its statutory replacement rate thrice. The average absolute value of these changes was four percentage points. At the other end of the spectrum, North Carolina changed from insuring the highest quarter to latest two quarters of a workers based period wages in 2013. Since the typical UI recipient does not have particularly stable employment this change lead to a substantial decrease in UI generosity: the fraction of base period wages insured by North Carolina's UI policy fell by 12 percentage points.

As suggested by this example, we summarize state laws regulating the below cap segment of the schedule as:

$$\rho_n = \mathbb{E}\left\{\frac{b_{in}}{\chi_{in}^{52}}\right\} \tag{B.1}$$

where n indexes a policy in a state-time and i indexes an individual who has received benefits under that policy. This metric has many advantages. For a state like North Carolina $b_{in} = \breve{\chi}_{in}\breve{\rho}_n$; however, a minority of states—for example, California—have more complex policies that do not lend themselves to parsimonious formulas. Yet, the simple calculation of the numerator as the average realization of weekly benefits under the policy yields a tractable summary. In addition, both b_{in} and χ_{in}^{52} are consistently recorded in BAM while the specifics necessary to calculate benefits under more complex policies are not always observed. Finally, the measure allows us to compare the magnitude of the difference between and changes within state.

Figure B2 plots ρ_n quarterly for states in this example, Kentucky and North Carolina, against a backdrop of other state policies. In any given quarter, ρ_n varies roughly from 0.5 to 1.0 across states, with a mean near 0.75 and standard deviation of approximately 0.1. Compared to general notions of UI replacement rates in the US, which would suggest that UI replaces 50 percent or less of prior earnings, our calculated measure ρ_n is high for two reasons. First, we explicitly exclude claimants with capped benefits, whose replacement rates tend to drive down the average. Second, we compare benefits to average weekly earnings across the entire base period, while many UI formulae replace 50 percent (or more) of *high quarter* earnings. Again, to the extent that earnings are not stable across the base period, the share of base period earnings replaced will be higher than the share of high quarter earnings replaced.

Figure B2 also shows that, in states that do not calculate benefits exclusively using total base period wages (e.g., North Carolina), there is considerable variation in ρ_n even outside of the legislated changes we identify. This variation reflects variation in claimants and their earnings over time. Both the composition of claimants and their insured earnings histories are likely cyclically and seasonally sensitive, so states that have more favorable definitions of insured earnings, such as high quarter wages only, will be more sensitive to this. In our analysis, we focus on plausibly exogenous variation due to legislated adjustments while including the macroeconomic controls described in the previous section. The average policy change in our sample adjusts ρ_n by 0.03 in absolute value.[59]

Cap

Measurement of the benefit cap is comparatively straightforward: it can be read directly from the legislative record or observed directly in the BAM. However, state legislatures have taken two broad approaches to adjusting it over time: legislated changes—in which the legislature writes the maximum amount directly into the state code—and automatic changes—in which 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, but, in practice, automatic changes are small and regular, while legislated changes tend to be larger and more infrequent. In addition to their generally small size, automatic changes pose two problems for inference. First, they are likely to be anticipated, which would attenuate effects. Second, since they depend on

[59]While Figure B2 displays ρ_n calculated at quarterly frequency, in our analysis we calculate it across the entire pre- and post- policy change windows of up to one year in length.

lagged local macroeconomic aggregates, inference drawn from automatic changes may be contaminated by spurious co-variation. In comparison, legislated changes are comparatively free from these concerns. Conscious of these issues, we distinguish between legislated and automatic changes in our analysis. Ultimately, our results are driven by the legislated cap changes, which are more plausibly exogenous.

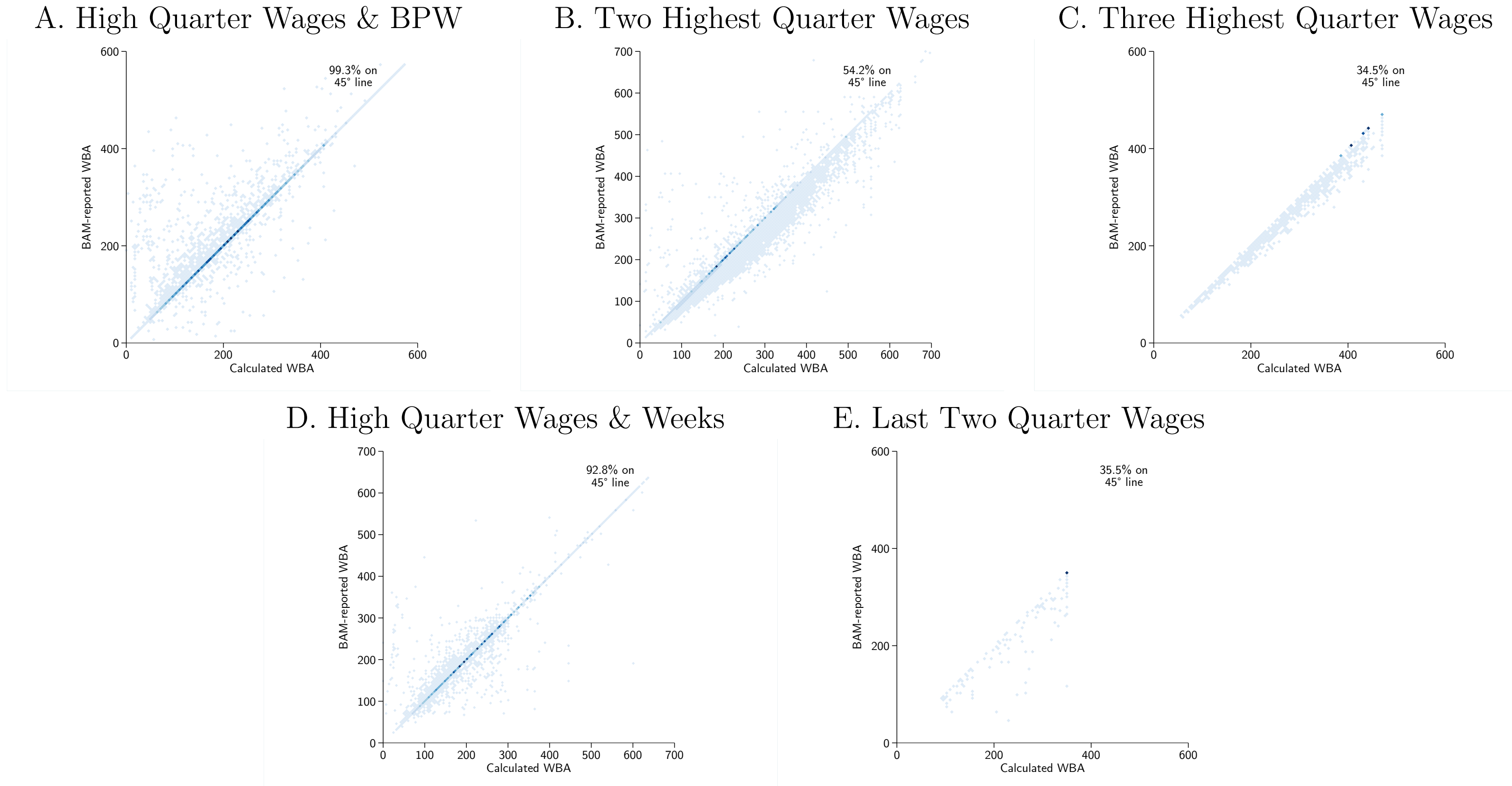
For the sake of comparison, we display example states' weekly benefit caps as shares of their quarterly average weekly wage in figure B3.[60] The overlaid vertical lines identify changes in the nominal value of the cap, with solid lines indicating actively legislated changes and dotted lines indicating 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 and has, as a result, remained virtually constant.

B.3 Validating the Benefit Calculator and Policy Metrics

We validate our policy metrics by estimating the direct effect of UI replacement on the job finding rate. Details of these validation tests are found in section 9 and table B4. Using various identification strategies we estimate elasticities of a job seeker's job0finding rate to her own benefits. While not every specification yields statistical significance, we obtain estimated elasticities in the range of -0.35 to -0.5, which coincides with the range of estimates documented in the literature (Moffitt, 1985; Meyer, 1990; Chetty, 2006, 2008; Landais, 2015).

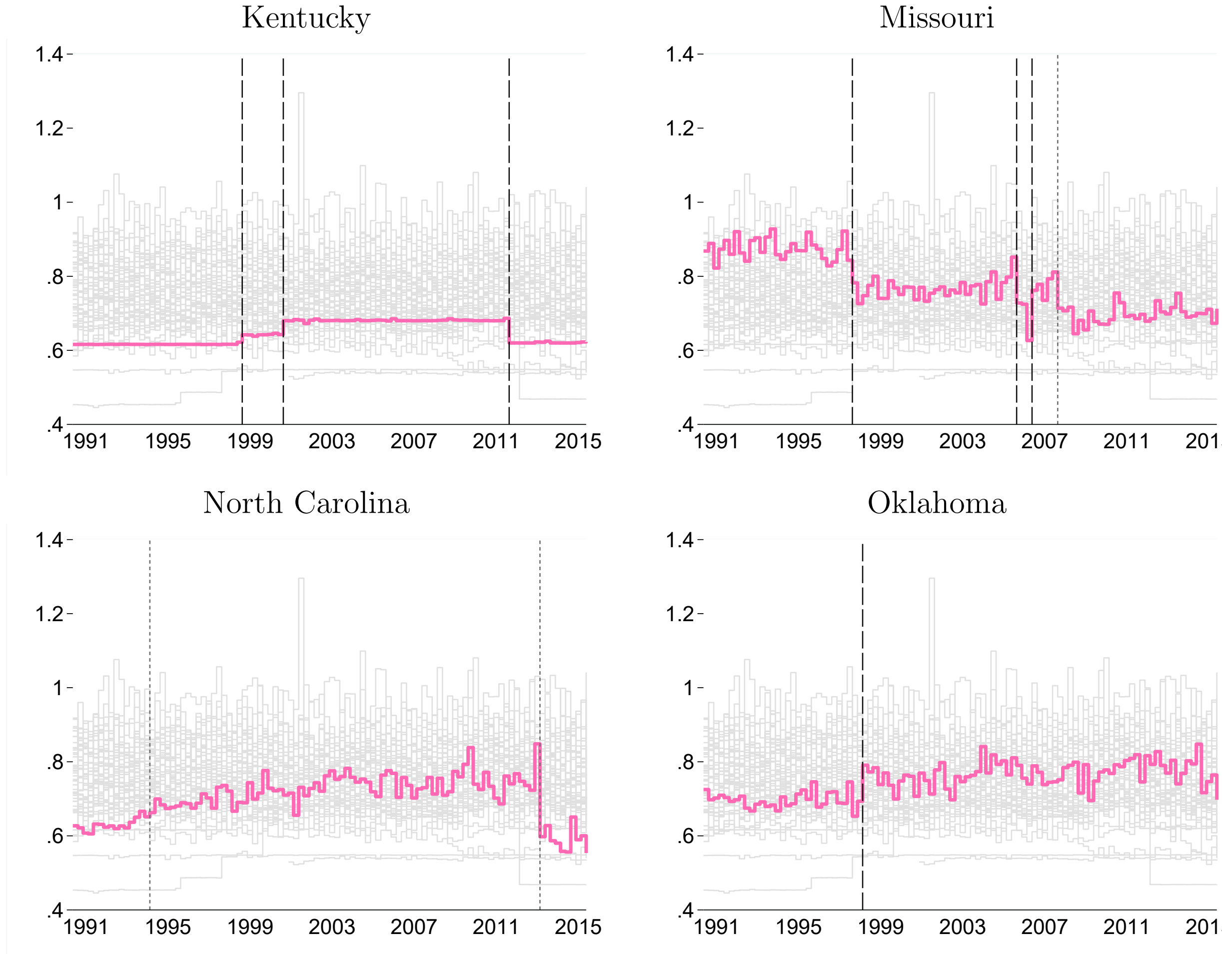
[60]In constructing these measures, we use average weekly wages from the Quarterly Census of Employment and Wages (QCEW) so that we have a consistent measure of average wages across states and over time.

Figure B1: UI Calculator Accuracy in BAM Data by Insured Earnings Measures



Notes: Panel A shows the strong relationship between reported and calculated benefits for claimants under policy regimes in which benefits depend only on total base period and high-quarter wages—the earnings measures appearing in BAM. This panel, which represents a majority of claimants, shows that our calculated benefits match administratively-recorded benefits in the overwhelming majority of cases when BAM records the necessary earnings measures for calculating benefits. We attribute the small number of deviations to recording errors in the data as we can identify no particular patterns across states, time, or earnings in explaining them. The remaining panels show the same results for other policy regimes. While the BAM data limitations preclude us from exactly validating the calculator for these policy regimes, as in Panel A, the differences between calculated and reported benefits are consistent with the data limitations as opposed to suggesting that there are errors in the calculator. As indicated by the panel titles, the earnings measures required by other policy regimes are generally measures of quarterly earnings outside the highest quarter. For this analysis, we take the simple approach of assuming that claimants' earnings are the same (and equivalent to high-quarter earnings) across all four quarters of their base periods. Unless earnings are stable across all four base period quarters, we will have assigned earnings and calculated benefits that are too high in most cases, which is why most of the off-diagonal mass in these figures falls below the 45-degree line. Additionally, for the overwhelming majority of observations, calculated benefits fall in the range consistent with a correct calculation using an incorrect assumption on earnings stability.
Source: Authors' legislative research, BAM data 1988–2015, and authors' calculations.

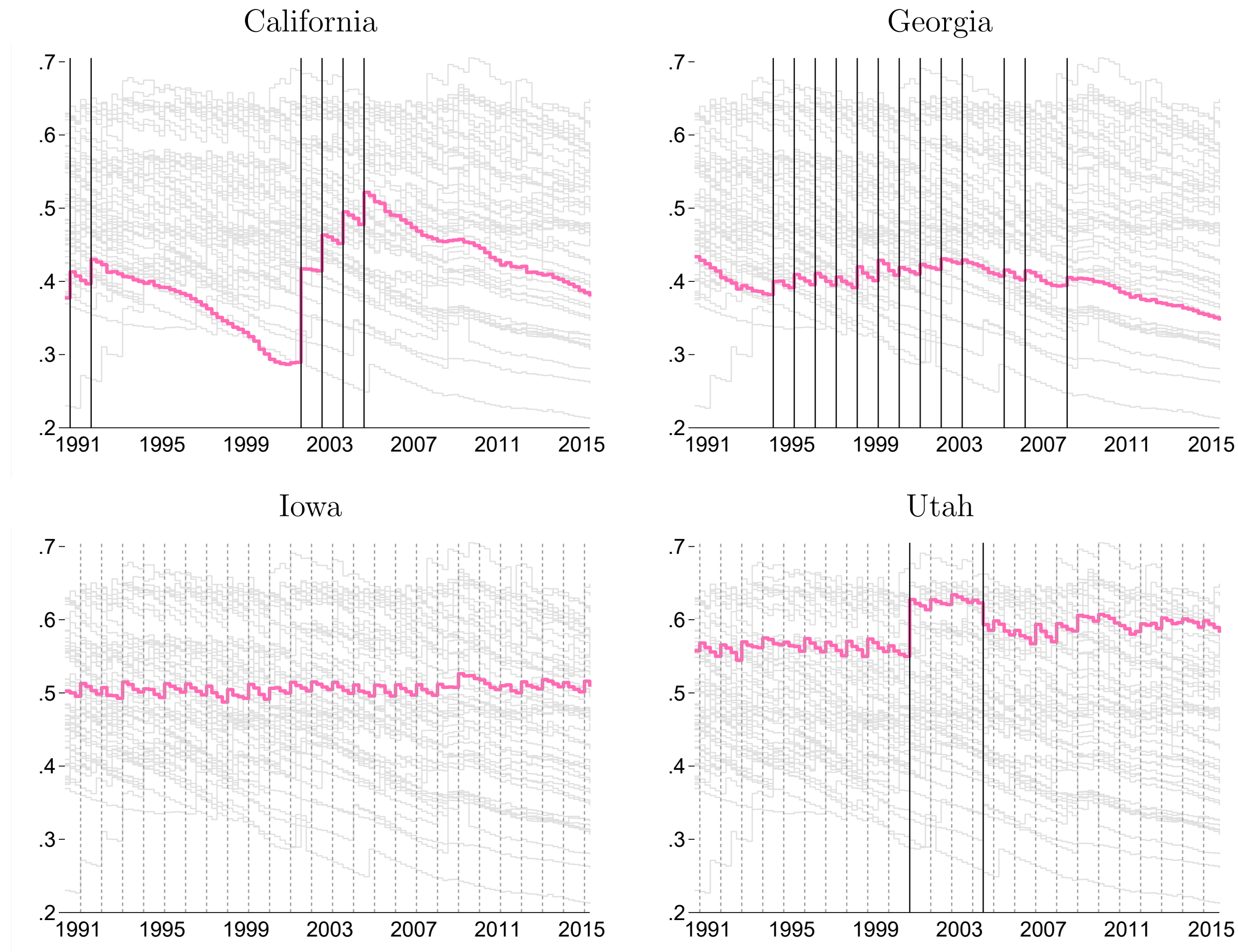
Figure B2: State Average Replacement Rates below Maximum Weekly Benefits



Notes: Figure displays quarterly calculated ρ_n, as described in Section B.2, for selected states. 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. All other states' series appear in light gray.

Source: Authors' legislative research, BAM data 1988–2015, and authors' calculations.

Figure B3: State Maximum Weekly Benefits as a Share of Average Weekly Wages



Notes: Figure displays quarterly weekly benefit caps as a share of state average weekly wages reported in the QCEW for selected states. Solid bars indicate actively legislated changes to the maximum weekly benefit amount. Dotted bars indicate changes triggered automatically by existing legislation. All other states' series appear in light gray.

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

Table B1: Sample Means: Individuals Observed in Unemployment in the SIPP

Panel A: Policy Changes to the Benefit Cap

Subsample:	Well Below the Cap			Just Below the Cap			At the Cap		
	All	Leg	Auto	All	Leg	Auto	All	Leg	Auto
Male	0.43	0.40	0.44	0.56	0.52	0.57	0.63	0.57	0.65
Age	39.0	38.7	39.0	39.8	39.8	39.8	40.9	39.9	41.2
Years of education	12.3	12.0	12.4	13.1	12.7	13.2	13.6	13.4	13.7
Weekly job-finding rate	0.055	0.058	0.054	0.062	0.070	0.060	0.078	0.076	0.079
Individuals	20,921	5,028	15,893	4,079	1,040	3,039	4,625	1,310	3,315
Policy changes	685	145	540	591	134	457	615	137	478

Panel B: Policy Changes to the Replacement Rate

Subsample:	Well Below the Cap		Just Below the Cap		At the Cap	
	All	Excl. Changes to Benefit Min	All	Excl. Changes to Benefit Min	All	Excl. Changes to Benefit Min
Male	0.46	0.44	0.56	0.55	0.68	0.72
Age	38.9	40.2	39.1	40.1	40.9	38.4
Years of education	11.9	12.3	12.9	12.6	13.5	13.2
Weekly job-finding rate	0.055	0.055	0.068	0.079	0.076	0.068
Individuals	5,691	443	1,149	93	1,329	104
Policy changes	144	14	119	12	130	11

Notes: "Well Below the Cap" subsamples comprise claimants with calculated benefits less than 75 percent of the larger of the two caps in a policy change. "Just Below the Cap" subsamples comprise those with benefits above this range but below the smaller of the two caps. "At the Cap" subsamples comprise those with benefits at least as large as the smaller of the two caps. UI benefits are calculated using reported earnings and a UI calculator generated from a detailed reading of state legislation. Automatic cap changes are those in which the cap is updated by a mechanical formula set by previous legislation. In legislated changes, the legislation sets the cap to a specific number or changes the mechanical formula.

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

Table B2: Cap Change Sample Means: All Individuals Observed in Unemployment in the SIPP

Panel A: Well Below the Cap						
Subsample:	Pre-Change			Post-Change		
	All	Legislated	Automatic	All	Legislated	Automatic
Male	0.43	0.39	0.44	0.43	0.40	0.43
Age	39.0	38.8	39.0	38.9	38.7	39.0
Years of education	12.3	12.0	12.4	12.3	12.0	12.4
Panel B: Just Below the Cap						
Subsample:	Pre-Change			Post-Change		
	All	Legislated	Automatic	All	Legislated	Automatic
Male	0.57	0.53	0.58	0.55	0.52	0.55
Age	39.8	39.4	39.9	39.8	40.1	39.8
Years of education	13.1	12.7	13.2	13.1	12.8	13.2
Panel C: At the Cap						
Subsample:	Pre-Change			Post-Change		
	All	Legislated	Automatic	All	Legislated	Automatic
Male	0.63	0.55	0.65	0.63	0.59	0.65
Age	40.7	39.8	41.0	41.0	40.0	41.4
Years of education	13.7	13.5	13.7	13.5	13.2	13.6

Notes: "Well Below the Cap" subsamples comprise claimants with calculated benefits less than 75 percent of the larger of the two caps in a policy change. "Just Below the Cap" subsamples comprise those with benefits above this range but below the smaller of the two caps. "At the Cap" subsamples comprise those with benefits at least as large as the smaller of the two caps. UI benefits are calculated using reported earnings and a UI calculator generated from a detailed reading of state legislation. Automatic cap changes are those in which the cap is updated by a mechanical formula set by previous legislation. In legislated changes, the legislation sets the cap to a specific number or changes the mechanical formula.

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

Table B3: Replacement Rate Change Sample Means: All Individuals Observed in Unemployment in the SIPP

Panel A: Well Below the Cap				
Subsample:	Pre-Change		Post-Change	
	All	Excl. Changes to Benefit Min	All	Excl. Changes to Benefit Min
Male	0.47	0.48	0.46	0.39
Age	38.9	39.8	38.8	40.6
Years of education	11.9	12.4	11.9	12.2
Panel B: Just Below the Cap				
Subsample:	Pre-Change		Post-Change	
	All	Excl. Changes to Benefit Min	All	Excl. Changes to Benefit Min
Male	0.56	0.55	0.57	0.55
Age	39.4	40.6	38.8	39.6
Years of education	12.9	12.0	12.8	13.1
Panel C: At the Cap				
Subsample:	Pre-Change		Post-Change	
	All	Excl. Changes to Benefit Min	All	Excl. Changes to Benefit Min
Male	0.68	0.74	0.68	0.71
Age	40.8	37.1	41.0	39.7
Years of education	13.6	13.3	13.4	13.0

Notes: "Well Below the Cap" subsamples comprise claimants with calculated benefits less than 75 percent of the larger of the two caps in a policy change. "Just Below the Cap" subsamples comprise those with benefits above this range but below the smaller of the two caps. "At the Cap" subsamples comprise those with benefits at least as large as the smaller of the two caps. UI benefits are calculated using reported earnings and a UI calculator generated from a detailed reading of state legislation. Automatic cap changes are those in which the cap is updated by a mechanical formula set by previous legislation. In legislated changes, the legislation sets the cap to a specific number or changes the mechanical formula.

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

Table B4: Estimated Direct Effects of Changes to ρ

LHS=Job Finding Rate		
ρ coefficient	-0.042^{*}	-0.033^{*}
	(0.022)	(0.016)
Implied elasticity at mean	-0.53	-0.41
Excluding Coincident Cap Changes		X
Observations		
Individuals	9,082	6,478
Policy Changes	206	143
R-Squared	0.10	0.11

Notes: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$. Standard errors clustered at the state level appear below each estimate in parentheses. The reported coefficients are estimated from specifications analagous to equation (3.1), including only the directly-treated sub-sample: those with calculated benefits below the smaller of the two caps associated with any policy change.

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.