Does Inconvenience Explain Low Take-up? Evidence from Unemployment Insurance*

Avraham Ebenstein† Kevin Stange‡

December 8, 2008

Comments Welcome

Abstract

Inconvenience is one popular explanation for why many individuals do not receive the social benefits for which they are eligible. Applications take time and some individuals may decide that the financial benefits do not outweigh these time costs. This paper investigates this explanation using cross-state variation in administrative changes that made applying for Unemployment Insurance (UI) benefits substantially more convenient over the past decade. We find that the introduction of phone- and internet-based claiming did not have an appreciable impact on overall UI take-up, nor did it lead to a shift towards recipients that are higher income or likely to be receiving the maximum benefit amount. These findings are inconsistent with a time- and transaction-cost explanation for low take-up if remote UI claiming is indeed less time-intensive, as claimant surveys suggest.

*We are particularly thankful to Andy Spizak from the U.S. Department of Labor Employment Training Administration for providing us with the BAM data and to Brian McCall for sharing his UI eligibility programs. Helpful comments on an earlier version were received from Alan Auerbach, Raymundo Campos, Raj Chetty, Marit Rehavi, Emmanuel Saez and seminar participants at U.C. Berkeley and the RWJ Health Policy Scholars Scholars Annual Meeting. All errors are of course our own.

†Robert Wood Johnson Scholar in Health Policy Research, Harvard University; aebenste@rwj.harvard.edu.

‡Robert Wood Johnson Scholar in Health Policy Research, University of Michigan; kstange@umich.edu.

1 Introduction

Participation varies considerably across most social programs in the United States and is generally less than complete. In her extensive survey of the empirical evidence, Currie (2006) finds that take-up rates for means tested programs range from very low (8-14% for State Children's Health Insurance Program) to relatively high (82-87% for the Earned Income Tax Credit and 60-90% for Temporary Assistance for Needy Families), with most other major social programs falling somewhere in between. Take-up of non means-tested programs is generally higher (nearly 100% for Medicare), but often far from complete (72-83% for unemployment insurance). The latter is particularly puzzling if one expects the stigma associated with participation in non means-tested programs to be lower than that associated with means-tested ones. She concludes that concrete transaction costs - including inconvenience - must also be a major determinant of participation in social programs among those eligible.

Most research on take-up has focused on monetary incentives.[1] However, there is a small but growing body of recent work suggesting that non-monetary program features such as application complexity, default or automatic enrollment, or mandated in-person interviews may also be an important factor in participation.[2] In this paper, we examine the take-up consequences of the recent introduction of phone- and internet-based claiming for unemployment insurance (UI), which greatly reduced the time required to file for UI benefits.[3] While almost all unemployed workers were historically required to initially apply for UI benefits in person, now only thirteen percent do. We exploit cross-state variation in the timing of these changes to identify the effect of inconvenience on the number and characteristics of UI recipients. The present study is most closely related to that of Kopczuk and Pop-Eleches (2007), who find that the introduction of electronic state tax

[1] For example, McCall (1995) and Anderson and Meyer (1997) find strong effects of benefit levels on take-up of unemployment insurance. Daponte, Sanders, and Taylor (1999) find that poor information partially explains Food Stamps non-participation, but that poor information is most common among those whose potential benefits are low.

[2] In their survey, Remler, Rachline, and Glied (2001) highlight the importance of non-monetary program features to take-up. Currie and Grogger (2002) find that Medicaid administrative reforms did not increase use of prenatal services but Bansak and Raphael (2006) identify several non-monetary design features of SCHIP that have significant effects on take-up including simplifying the application and renewal processes. Currie and Grogger (2001) find that longer recertification intervals increase Food Stamps participation among single parents. In the area of retirement savings, see Madrian and Shea (2001) on the importance of automatic/default enrollment and Saez (2007) on the importance of program framing.

[3] Needels et al. (2000) report results from claimant surveys that suggest considerable time savings. Marcus and Frees (1998) report results from a nationwide survey of 2,773 claimants, where respondents estimated that it took 11 minutes to file an initial claim by telephone and 61 minutes to file in-person.

filing had a large effect on participation in the EITC.

We find that the aggregate unemployment insurance take-up rate has increased slightly over the past decade and a half, in contrast to the declining take-up observed in the 1980's.[4] This trend coincides with dramatic changes in the ease of applying for benefits and, more recently, increases in benefit levels and a shift towards more educated unemployed workers. However, state-level estimates suggest that increased ease of filing is not behind this aggregate take-up trend. The fraction of unemployed who are on UI has no relationship to the introduction of phone- or internet-based claiming or to the closing of UI offices. This result is robust to controls for state and year effects, characteristics of the unemployed, and the maximum benefit amount. Regressions using estimated take-up as a dependent variable are generally consistent with this finding, but are much less precise due to measurement error in our estimates of the fraction of unemployed who are eligible for UI.

We also find little evidence of a shift towards higher-wage claimants resulting from reduced UI application time, as a time-cost explanation for low take-up would predict. The introduction of phone claiming had no effect on the fraction of claimants receiving the maximum benefit, the average and distribution of pre-unemployment wage, or claimant education. The only exception to this pattern for internet claiming is an increase in the proportion receiving the maximum benefit when internet claiming is introduced. The primary effect of the introduction of remote claiming technology was to reduce states' payroll costs, but with no effect on the number or pool of UI participants. This finding leaves incomplete UI take-up an unresolved puzzle and points to incomplete information about eligibility or application processes, rather than inconvenience, as possible explanations.

This paper is organized as follows. The next section presents recent trends in UI participation and provides background on UI claiming procedures in the US. Section 3 describes our data and empirical approach. Results are reported in Section 4. Section 5 concludes.

[4] See Blank and Card (1991) for UI take-up trends in this earlier period.

2 Background

2.1 Aggregate Trends in Unemployment Insurance

The Federal-state UI program provides income support during spells of unemployment to workers in almost all sectors of the economy. Most of the research on UI participation is motivated by three features: (1) low levels of participation among those eligible; (2) large cross-state differences in participation; and (3) long term declines in participation, particularly in the early 1980's. Two supplements to the Current Population Survey (CPS) in 1989-90 and 1993 asked unemployed workers whether they had applied for UI benefits and, if not, why. As summarized in Wandner and Stettner (2000), the most common reason for non-application was perceived ineligibility (mostly due to insufficient work hours and/or earnings and quits) and optimistic job expectations.[5] Responses suggestive of lack of information ("Didn't know about UI or how to apply"), inconvenience ("Too much work or hassle"), or stigma ("Too much like charity or welfare") were relatively rare.

There is also considerable variation in UI participation across states and regions. Vroman (2002) investigates the sources of this variation and concludes that low participation is linked to high rates of misconduct determination and frequent eligibility determination by states. Unionization and the prevalence of employer-initiated claims tends to increase UI participation.

There has also been a fair amount of attention paid to the decline in aggregate UI participation over the past half century, particularly in the early 1980's. Blank and Card (1991) conclude that none of this recent decline is due to changes in eligibility. Instead, changes in the regional distribution of unemployment from high to low take-up states and declines in unionization explain most of the recent decline. Anderson and Meyer (1997) conclude that changes in the tax treatment of UI benefits accounts for most of the decline not explained by Blank and Card.

In contrast to declines observed in the 1980's, we estimated that aggregate UI take-up rate has increased slightly over the past decade and a half. Figure I plots our estimates of the fraction of unemployed workers that are eligible for UI, the fraction that are receiving UI benefits, and the implied take-up rate from 1989 to 2006 (the construction of these measures is described in Section 5). Fraction eligible and fraction on UI generally trend together - increasing during recessions

[5] The surveys rely on self-reported perceived eligibility because an attempt to match survey respondants to administrative data that would enable actual eligibility determination was not successful.

and falling during recovery. The gap between these series narrowed during the 1990's, particularly during the 2001 recession, increasing the take-up rate.

Coincident with this take-up increase, states made it much less burdensome to apply for UI benefits. Figure II plots the fraction of all initial UI claims that were filed using various methods from 1989 to 2006. Though nearly all initial claims for benefits were made in person in 1990, by 2006 only thirteen percent were. More than half of all initial claims are now made over the phone and more than thirty percent are filed on-line. While suggestive of a relationship between claiming ease and participation, this interpretation is obscured by changes in program generosity and the characteristics of unemployed workers during this period. Figure III plots the average maximum weekly benefit amount UI recipients are eligible for and the fraction of unemployed workers that have a Bachelor's degree over time. UI generosity grew gradually during the 1990s and jumped considerably after 2000. The composition of unemployed workers also shifted, with the 2001 recession bringing in more educated workers into the ranks of the unemployed. These aggregate changes in program generosity and the characteristics of the unemployed (and potential UI eligible), both of which may affect UI participation, motivate the cross-state analysis that follows.

2.2 Changes to UI Claiming Procedures

Since the program's inception in 1935, most workers who lost their job visited state-run local UI offices in order to file their initial claim, certify that they were available for work, and confirm that they were actively seeking employment.[6] In the mid-1990's, however, states began to implement procedures to receive initial UI claims over the telephone and many began to close UI offices. The receipt of claims over the internet soon followed. Figure II masks considerable cross-state variation in timing of these policy changes which are important for our empirical approach. We use cross-state variation in the timing of these policy changes to identify the effect of inconvenience on take-up. Figure IV plots the cumulative number of states that have introduced telephone and internet claiming or have closed UI offices by year. Colorado was the only state to offer phone initial claiming for the first half of the nineties, followed by Wisconsin in 1995 and Massachusetts and California in 1996. Phone claiming quickly took off, and by 2005 40 states accepted UI claims via

[6] Due to the remote nature of many of its communities, Alaska has long permitted individuals to file UI benefits through postal mail.

phone. Phone claiming was usually followed - often with a one to two-year lag - by the elimination of initial claims takers at UI offices. Internet claiming, introduced widely in 2000, was nearly as common as phone-claiming by 2005.

Cost reduction and improved customer service were the primary motivations for states' implementation of telephone and internet claiming. In their survey of seven early telephone claims adopters, Needels et al. (2000), found that state UI administrators switched from in-person filing at local UI offices to telephone claiming to reduce administrative costs and improve customer service. Kenyon et al. (2003) found that cost savings, improved efficiency, and convenience (extended hours and reduced wait time) also motivated the adoption of internet claiming. The adoption of both forms of remote claiming technology were partially fueled by the availability of grants from the U.S. Department of Labor to implement these technologies and almost all states took advantage of this funding source.

Since cost-savings was a primary motivation, most states closed local UI offices (eliminating in-person filing) after the introduction of telephone claiming and instead now provide dedicated on-site telephones and/or internet access at local One-Stop Centers to use for claims filing.[7] Most states make use of Interactive Voice Response technologies to automate part of the initial claims process and four of the seven profiled states offer toll-free numbers. Our interviews with many state UI offices suggest that phone claiming may have improved linguistic accessibility because most states utilized phone translation services. How these changes have affected access to job information and re-employment services is not clear, though interviews in a recent GAO report [U.S. Government Accountability Office, 2005] suggest it has not been diminished.

Important for the interpretation of our findings is whether phone and internet claiming is more convenient than in-person claiming since most states eliminated the in-person filing option following the introduction of telephone claiming.[8] As reported in Needels et al. (2000), customer satisfaction surveys in Maine, Massachusetts, Missouri, and Wisconsin suggest that claimants overwhelmingly prefer telephone to in-person claiming. Eighty-six to ninety-six percent of respondents in these states, half of whom are former in-person filers, report that telephone claiming is easier, more convenient, or faster.[9] The case of Massachusetts is also illustrative. Despite being given the option

[7] One-Stop Centers provide other UI-related and job-search services.

[8] At the time of this paper, there are no states that currently receive UI initial claims via the Internet exclusively.

[9] In a nationwide survey of 2,773 claimants, Marcus and Frees (1998) report that respondents estimated it took

to file in-person, only 11% of individuals chose to do so in 2003. Kenyon et al. (2003) reports that claimants seem to be even more satisfied with internet-based claiming, due to its convenience and speed.[10] Though far from conclusive, these anecdotes suggest that remote claiming methods are overwhelmingly preferred to in-person methods.

Figures V and VI examine which baseline factors predict when states implemented remote claiming. Figure V plots year of policy change by four different demographic characteristics from the 1990 Census. Phone claiming was implemented earlier by states with higher household income, more educated populations, and fewer minorities. Surprisingly, rural states who presumably have the greatest to gain from remote claiming were late adopters. There is much less variation in the timing of internet adoption, but the adoption pattern is similar: higher income, education, and urbanization all predict earlier adoption.

Figure VI repeats this analysis for four baseline labor market and UI program characteristics. Early adoption has only weak correlation with low unemployment, but is positively correlated with UI benefit generosity. The bottom panels show the correlation between adoption and measures of UI program coverage. States in which more unemployed workers are eligible and/or receiving UI in 1990 were more likely to introduce phone and internet claiming earlier. Since timing of adoption was clearly not random among the states, our difference-in-difference approach controls for fixed differences in take-up between states.

3 Data and Empirical Approach

We construct a panel dataset of states (plus the District of Columbia) for the years 1989 to 2006 from several different sources. Information on UI claimants is from the Benefit Accuracy Measurement (BAM) program, administered by the U.S. Department of Labor. BAM is designed to measure the accuracy of paid and denied UI claims and determine the source of any inaccuracies, in order to improve UI administrative processes. BAM samples approximately 400 UI claimants per year

11 minutes to file an initial claim by telephone and 61 minutes to file in-person. A similar picture of considerable time savings from telephone claiming was also found in surveys of claimants in San Diego (2.33 hours in-person to 14 minutes via phone) and Colorado (3.4 hours to 1.7 hours).

[10] The time savings (in minutes) were 19 for Colorado, 9 for Utah, and 7 for Washington relative to filing a claim over the phone.

in each state.[11] Important for our purposes is that claiming method (in person, phone, mail, Internet, through employer) is recorded for each person in the sample. From this data, we estimate the fraction of UI claimants using each method in each state for every year and also measures of UI claimant characteristics (fraction at maximum benefit amount, pre-unemployment wages, education).

Administrative data on the number of initial claimants, average duration on UI, number of weeks compensated and claimed, average weekly benefit amount, and several other measures of UI utilization were obtained from the Department of Labor Employment and Training Administration, quarterly by state. Program rules and regulations (such as the maximum benefit amount), the unemployment rate and the number of individuals unemployed, employed, and in the labor force was obtained from the same source. We construct two measures of UI program participation: weeks compensated per unemployed person and weeks compensated per UI-eligible unemployed person. Our method for estimating eligibility is described below. Finally, from the March CPS we obtain characteristics of the unemployed population in each year which we use as control variables in our regression analysis. All nominal values for earnings, wages, and benefit amounts are converted to 2003 Dollars using the CPI-U. Table I provides summary statistics of our dataset.

3.1 Dating Policy Changes

We identified the year in which states first offered phone claiming, first offered internet claiming, and first closed UI offices (eliminating in-person claiming) using three complementary approaches. Identifying the conceptually appropriate date of implementation is complicated by the fact that some states piloted the initiatives in a few locations and/or phased in implementation across the state gradually. Our primary analysis relies on policy dates inferred from sharp changes in trends in the methods used to file UI claims. For each state, we identified the year that the share of claims filed via phone (or internet) accelerated by the greatest amount and designated this the event year.[12] This approach allows us to account for both slow phase-in and the presence of special-case

[11] Since the sample is drawn from all claims in a given week, BAM overrepresents claimants with long durations. That is, they are weighted by weeks claimed. We also construct weights for the BAM sample using basic demographic information on the universe of all UI claimants also collected by the U.S. D.O.L. to ensure that our BAM sample matches the race-gender composition of claimants in each state-year.

[12] We restricted it to years where this maximum acceleration exceeded five percentage points and where the share was increasing to prevent falsely recording minor blips as policy changes.

individuals filing by phone prior to its widespread implementation.[13] Putting an exact date on office closure is also complicated by the gradual geographically-based phase-in of many of the closures. We used the first year that in-person claims dropped below 20% as our office closure date.

To verify our assignment of policy timing, we also contacted and interviewed 23 state UI offices directly, obtaining the dates of these three changes (if at all) and some qualitative information about the process (i.e. motivation and roll-out). We supplemented this information for six states using the dates reported in Needels (2000) and from press releases. Our third approach involves identifying the year that phone or internet claims exceed certain threshold of all claims. In this approach we designate the phone and internet claiming year to be the first year that each method exceeded 5% of all claims. Office closing year was the first year that in-person claims dropped below 10%.

The dates for each of these events for each state using the first two methods (imputed and interviews) are reported in Appendix Table AI. Overall the imputation seems to do a good job of capturing policy event dates: the correlation between imputed and interview-derived event dates are 0.95, 0.64, and 0.95 for phone claiming, internet claiming, and office closure, respectively. We do not view the interview-derived dates as necessarily more preferable for quantifying the importance of time costs. If states implemented internet claiming without advertising it or working out system kinks, then low initial utilization of remote claiming implies negligible reduction in inconvenience. Our preferred analysis relies on policy events that had large and sudden impacts on the methods workers used to file a UI benefit claim.

3.2 Estimating Eligibility

In order to examine take-up among those eligible (rather than total participation), we follow Blank and Card (1991) and estimate the fraction of unemployed persons that are eligible for unemployment insurance in each state and year using data from the March CPS.[14] Unemployed persons are deemed ineligible for five reasons: (1) current duration less than the required waiting period; (2) current duration exceeds the maximum; (3) quit last job; (4) not in a covered sector; and (5) insufficient base period earnings. We use earnings in the previous calender year as our approximation of base

[13] Most states accepted some applications over the phone in the case of very special circumstances prior to the widespread implementation of phone claiming for all individuals.

[14] We are greatful to Brian McCall for generously sharing his code on UI eligibility regulations.

period earnings.

Take-up is calculated as the ratio between the fraction of unemployed persons receiving UI (from the DOL) to the estimated fraction that are eligible. It should be noted that since eligibility is measured with considerable error, our take-up rates sometime exceed one (in fact the maximum in our dataset is two). This partly reflects systematic labor market differences between states. Alaska, for instance, has many migrant oil field workers and always has a very high estimated take-up rate because many people live outside the state (in the CPS) but claim UI benefits from Alaska (where they work). State fixed effects will net out these fixed differences across states. Appendix Table AII presents the correlation between the fraction of unemployed who are on UI with our estimate of the fraction of unemployed who are UI eligible, by state and year. Overall this correlation is 0.26, which should be much closer to one if eligibility were accurately measured.

3.3 Empirical Approach

We utilize cross-state variation in the timing of changes to UI claiming procedures to assess the importance of convenience to UI program participation. States implemented phone- and internet-based UI claiming at different times or sometimes not at all. This permits us to identify treatment effects separately from aggregate year effects and unobserved state characteristics, both of which may also influence take-up and claimant composition. We estimate the following simple reduced form model using ordinary least squares.

$$\begin{aligned} y_{i,t} &= \alpha_0 + \alpha_p PostPhone_{i,t} + \alpha_n PostNet_{i,t} + \alpha_c PostClose_{i,t} \\ &\quad + State_i + Year_t + \alpha_b B_{i,t} + \alpha_x X_{i,t} + \varepsilon_{i,t} \end{aligned} \tag{1}$$

where $PostPhone_{i,t}$, $PostNet_{i,t}$, and $PostClose_{i,t}$ are indicators for whether the observation is after the policy change. Program characteristics such as the maximum weekly benefit amount are captured by $B_{i,t}$ and the characteristics of the unemployed population are captured by $X_{i,t}$. The error term $\varepsilon_{i,t}$ represents unmodeled determinants of take-up. The parameters of interest are α_p, α_n, and α_c, which can be interpreted as the effect of having adopted remote UI claiming sometime in the past on UI take-up in the current period. We assume these effects to be constant across states and over calendar time. When a full set of state and year effects is included, these parameters

are estimated on policy changes within states over time, net of any aggregate yearly changes in UI take-up common to all states. Fixed differences in the levels of UI take-up across states will be absorbed into state fixed effects. Any aggregate correlation between policy changes and take-up, such as increased take-up during the 2001 recession which coincided with the introduction of internet-claiming, will be absorbed into aggregate year effects. In Equation (1), the unobserved counterfactual is implicitly estimated from individual state-level effects (identified by pre-event observations) and aggregate time trends (identified by control states with no policy changes).

There has been recent attention paid to the consistency of standard errors in regression frameworks similar to ours due to the possible within-state serial correlation of outcome and policy variables. Following Bertrand et al. (2004), we allow for an arbitrary variance-covariance structure by estimating a clustered robust covariance matrix (e.g. clustered by state). We also utilize a non-parametric permutation test which assigns placebo patterns of treatment status to states and uses the empirical distribution of estimated coefficients from many of these placebo treatments for inference.

We can foresee at least two challenges to our identification strategy. First, states may have implemented other reforms that may also affect take-up (e.g. expanded outreach) concurrent with changes to claiming methods. Depending on the nature of the reform, omitted variable bias may over- or under-state the true causal effect. Our review of the literature and discussion with state UI administrators did not reveal any concurrent policy changes, but this cannot be entirely ruled out.[15]

A second problem is policy endogeneity. State administrators may adopt more automated claiming methods in response to higher anticipated demand placed on program resources due to higher anticipated take-up. If so, our estimates will be biased upwards. We address this concern by estimating "event study" models with leading and lagging treatment indicators, so that we can observe pre-event trends in the outcome variables. To implement this, we estimate the following

[15] See Needels et al. (2000), Kenyon et al. (2003), and U.S. GAO (2006) for discussions of the implementation of remote claiming technologies.

model using ordinary least squares:

$$\begin{aligned} y_{i,t} &= \alpha_0 + \sum_{s=-3}^{3} \alpha_p^s PhoneEvent_{i,t}^s + \sum_{s=-3}^{3} \alpha_n^s NetEvent_{i,t}^s + \sum_{s=-3}^{3} \alpha_c^s CloseEvent_{i,t}^s \\ &\quad + State_i + Year_t + \alpha_b B_{i,t} + \alpha_x X_{i,t} + \varepsilon_{i,t} \end{aligned} \tag{2}$$

where $PhoneEvent_{i,t}^s$ is an indicator for time relative to the introduction of phone claiming. $PhoneEvent_{i,t}^s$ equals one if state i implemented phone claiming in period $t - s$ (where s can be positive or negative), and zero otherwise. $NetEvent_{i,t}^s$ and $CloseEvent_{i,t}^s$ are defined similarly. Coefficients on the policy event time indicators, α_p^s, measure the differences between actual and predicted outcomes in the current period, having implemented the policy s years earlier (if $s > 0$). For instance, α_p^0 is the take-up increase during the first year phone claiming was introduced relative to it not being implemented. These are the parameters of interest.

4 Results

4.1 Effects on Claiming Method and Administrative Costs

Though some states phased in phone claiming over time, on average adoption was pretty rapid. Figures AI to AIII in the Appendix plot the fraction of claims filed in person, by phone, and over the internet separately for each state. To characterize the average pattern of adoption across all states, we first estimate Equation (2) using the fraction using each filing method as dependent variables and plot the coefficients in Figure VII. The regressions include a full set of state and year dummy variables but no other covariates. Conceptually, this procedure realigns the graphs in Figures AI-AIII around a common vertical line at the time of each policy event to create a common "event time" relative to the event. The plotted coefficients α_p^s and α_n^s are the average deviation of each plot from the zero horizontal axis for each "event time" period. The top panel does not include indicators for office closure.

The estimates suggest that phone claiming grew to account for half of claims within two years of implementation, drawing almost exclusively from in-person claims (rather than employer-initiated, mail, and other methods). Internet claims also grew rapidly, mostly substituting for telephone and, to a lesser extent, in-person claims. Including indicators for time relative to office closure (bottom

panel) does not change the general pattern. Even when people retain the option of filing in person, the growth of phone and internet claiming is still very rapid.

Cost reduction was one central motivation for states' move towards remote claiming. According to Needels et al. (2000), states believed that centralized call centers would allow them to realize economies in staff, office space, and training while making claiming easier for clients. They found that some states experienced a reduction in costs, while others did not. In some states, reductions in personnel and office space rental costs were offset by increases in communication and in equipment-related costs. Kenyon et al. (2003) conclude that cost savings, improved efficiency, and convenience (extended hours and reduced wait time) also motivated the adoption of internet claiming and that these improvements were achieved in the six states they studied.

To assess whether the policies had the intended effects on administrative costs, we collected data on employment and payroll from the Annual Survey of Government Employment and Payroll collected by the U.S. Census. Administration of the Federal-state unemployment compensation system is the primary component of expenditure function code 22, "Social Insurance Administration."[16] The data exclude benefits paid through the UI program and activities funded by Federal job training programs. Table II presents estimates of Equation (1) (excluding covariates) using employment and payroll costs as the dependent variables. The introduction of remote claiming in conjunction with office closure substantially reduced employment and payroll. Phone and internet claiming on their own (without office closure) has no independent effect on employment costs. Also, these changes were not brought about by a shift towards more part-time employment. Figure VIII depicts the coefficients (and 95% confidence intervals) from event study estimates of log payroll, without indicators for office closure. Payroll fell approximately two years after the introduction of phone claiming, but the introduction of internet claiming did not have an appreciable impact on administrative costs.

[16] The total employment and payroll of the California Employment Development Department, whose primary task is administration of the UI program, accounts for about half of the employment and payroll reported for "Social Insurance Administration" in California (California Department of Finance, Salaries and Wages Supplement, various years).

4.2 Effects on Aggregate Take-up

Table III presents our main results for aggregate take-up, using fraction of unemployed workers on UI as the dependent variable. UI participation does not increase with the introduction of either phone or internet claiming once state and year effects are accounted for, as shown in column (1). The point estimates are negative and very close to zero and their precision allows us to rule out positive effects of 1.8 percentage points for both policies at a 95% level of confidence. If some UI eligibles preferred making claims in person at UI offices, then phone claiming could actually be associated with reduced participation since phones often replaced the in-person option. Column (2) includes an indicator for whether UI offices were closed. The coefficient on phone claiming increases slightly and is now positive, but is still close to zero. Column (3) includes the real maximum benefit amount (in $100) to account for any changes in the generosity of the UI system that happen to coincide with the adoption of remote claiming. The estimated coefficients are unaffected by this inclusion. Column (4) includes controls for various characteristics of the unemployed population from the CPS, including pre-unemployment average hourly wage, education, demographics, and recent labor market experience (weeks worked last year and weeks looking for work). These controls are generally insignificant, though the coefficient on weeks spent looking for work is negative, likely reflecting the exhaustion of (and ineligibility for) UI benefits during extended periods of unemployment. Our estimates in this preferred specification allows us to rule out positive effects of 0.024 and 0.016 percentage points for phone- and internet-claiming respectively.

4.2.1 Accounting for eligibility

While the controls included in specification (4) of Table III may partially account for changes in eligibility, Table IV offers another approach. In these specifications, we use the estimated take-up rate as a dependent variable. This is calculated as the ratio of the fraction of unemployed people on UI (from administrative sources) to the fraction of unemployed people eligible for UI (estimated from the March CPS). We believe that the denominator is estimated with considerable error, introducing quite a bit of noise into our outcome measure.[17] This will decrease our precision

[17] Table AII in the Appendix presents the correlation between fraction of unemployed on UI and the estimated fraction eligible for UI by year and by state. Overall the correlation is 0.26, which should be closer to one if eligibility were accurately measured.

considerably, but should not introduce any systematic bias into our estimates if this measurement error is uncorrelated with our explanatory variables.

Accounting for eligibility has little effect on our conclusions, though the point estimates are now negative for both phone- and internet-based claiming and greater in magnitude. Since the mean of take-up is a little more than twice as large as the fraction of unemployed on UI, the coefficients should be divided by 2.2 to make them comparable to Figure III. Even with this correction, the coefficients are larger in magnitude when take-up is used as the dependent variable. As expected, standard errors are much larger due to eligibility measurement error. Together with the evidence in Table III, we conclude that neither phone- nor internet-based claiming has an appreciable impact on participation in UI. We find weak evidence that increasing the real maximum benefit amount results in higher participation, though our estimates are not statistically significant.

4.2.2 Permutation test for inference

To test the robustness of our inference, for our main specification we implement a non-parametric permutation test which assigns placebo patterns of treatment status to states and uses the empirical distribution of estimated effects from many of these placebo treatments for inference. This method is discussed by Johnston and DiNardo (1997, Chapter 11.2) and Bertrand, Duflo, and Mullainathan (2002) but has not found widespread use in the difference-in-difference literature.[18] The benefit of this approach is that we do not have to specify anything about the structure of the error term, instead relying on the treatment randomization assumption (conditional on covariates).

To implement the test, we randomly assign each state one of the full treatment patterns of another state: $\{PostPhone_{i,t}, PostNet_{i,t}, PostClose_{i,t}\}$, drawing without replacement. Since six of the patterns are shared by two states each, there are $\frac{51!}{2^6}$ possible permutations of states with treatments. We then estimate Equation (1) using the placebo treatment patterns with OLS, storing the coefficient estimates $\widehat{\alpha}_{p_j}$, $\widehat{\alpha}_{n_j}$, and $\widehat{\alpha}_{c_j}$, where j denotes the jth randomization. This process is repeated a large number of times. Define F(.) to be the empirical distribution of these placebo treatment effect estimates. To test the hypothesis that our estimates using actual policy dates are statistically different from 0, we observe where they fall on the F(.) distribution. The 95%

[18] This is a variant of Fisher's permutation or randomization test (Fisher 1935). Also see Chetty, Looney, and Kroft (2008) for a recent application.

confidence interval, for instance, is given by the $\widehat{\alpha}_{.j}$ that fall between the 0.025 and 0.975 percentiles of F(.).

Figure IX plots the empirical distribution of $\widehat{\alpha}_{p_j}$ and $\widehat{\alpha}_{n_j}$ using 5,000 randomizations of treatment assignment. The dashed vertical line is the point estimate from the base preferred model (Table 3 column (4)). For both phone and internet claiming, the point estimate is well within the distribution of point estimates from the placebo assignments. The 95% confidence interval is denoted by the solid vertical lines. The width of this confidence interval is very similar to that derived using our state-clustered standard errors.

4.2.3 Event study estimates

In order to identify any pre-existing trends in take-up that may bias our difference-in-difference estimates, we estimated Equation (2) both with and without indicators for year relative to office closure. Figure X plots the coefficients from these regressions along with 95% confidence intervals. For phone claiming, there are no pre-event trends in participation regardless of whether office closure is controlled for and the coefficients lie on the zero line. This suggests that phone claiming was implemented during a time that was "typical" for states. There appears to be a moderate short-term increase in participation following the introduction of phone-claiming, but the estimate of this increase is very imprecise and not different from zero. Internet claiming, however, may have been implemented during a period of atypically low UI participation, as suggested by the negative and downward-trending pre-event coefficients. Though the confidence intervals are wide, our results on internet claiming should be interpreted with caution. There also appears to be a short-term increase in participation coinciding with internet claiming, but again this increase is not significant at conventional levels.

4.2.4 Other robustness checks

Table V presents several different checks on the robustness of our main findings using the fraction of unemployed on UI as our dependent variable. Column (1) just repeats our preferred base model estimates from Table 3, column (4). In specification (2), we include state-specific linear time trends to account for trends at the state level that are not picked up by aggregate year effects and the time-varying covariates we've included. This increases the magnitude of the phone and internet claiming

effects, but both are still insignificant and fall within the original confidence intervals. In (3) we instead include a linear time trend interacted with baseline demographic and labor market variables from the 1990 Census: median household income, fraction rural, fraction nonwhite, fraction with BA, unemployment rate, and not-in-labor force rate. This inclusion changes our results very little from the base case.

Specifications (4) and (5) use alternative methods for identifying the years that states implemented phone and internet claiming and closed UI offices. Specification (4) restricts analysis to only those states we were able to interview about the timing of their remote claims. The coefficient for phone claiming is unchanged but the coefficient on internet claiming is now large and negative, but imprecise and we cannot reject that it is equal to zero. Identifying policy events as the first year that phone/internet claims pass a 5% threshold (column 5) produces similar results as the base case.

The final specifications restrict the sample in different ways. One advantage of the staggered timing of policy adoption is that treatment effects can be estimated exclusively on the sample of states that adopted the policy, excluding control states that never adopt. Specifications (6) and (7) use only states that eventually implemented phone claiming and internet claiming, respectively. In these specifications, later adopters serve as controls for earlier adopters. The estimated effects are larger when the sample is restricted to adopters, particularly for the policy (phone or internet) whose enactment the sample is conditioned on, but they are still insignificant. Specification (8) restricts the sample to post-1994 observations for states that had not yet implemented phone claiming by 1997. The intent is to estimate the state fixed effects using pre-treatment data that is closer to the date the policy changes actually happened. The estimates using this restricted sample are nearly identical to the base case.

4.3 Effects on UI Recipient Characteristics

If the policy changes affected potential UI recipients differentially - say because lower income claimants preferred speaking directly to UI workers or had limited internet access - then we may see changes in recipient characteristics even in the absence of aggregate take-up changes. Table VI presents regression results where various characteristics of UI recipients are used as the dependent variables. We include a full set of controls for characteristics of unemployed people in each

regression. Column (1) examines the fraction of UI recipients that are receiving the maximum benefit amount. Raising the maximum benefit level mechanically reduces this fraction, while weeks worked in the previous year (which influences potential benefit levels) increases it. Both phone and internet claiming are positively related to the fraction at the maximum and the coefficient on internet claiming is different from zero at the 95% level of confidence.

Column (2) examines the fraction of UI recipients that have a college degree. If college graduates have greater facility with the internet or easier eligibility determination over the phone, we may see a shift towards more college graduates following the adoption of these claiming methods. We find no evidence for such a shift. The point estimates are close to zero and statistically insignificant.

In column (3), we use average real pre-unemployment hourly wage as the dependent variable. This provides a direct test of the time cost explanation for incomplete take-up if time spent applying for benefits is more costly to workers with higher wages. While the positive coefficients on the policy variables are consistent with this explanation, the estimates are small ($0.03 to $0.22 on a base of $13.83) and insignificant. If increased convenience primarily affects those whose time is most valuable, then we may expect to see greater effects higher on the wage distribution. Columns (4) to (7) address this issue, by using different log wage percentiles of UI recipients as the dependent variable. The estimated effects of introducing phone- and internet-based claiming methods is consistently small and insignificant across the wage distribution.

Figure XI provides graphical evidence on whether the wage distribution of UI recipients shifted in conjunction with the introduction of remote forms of UI claiming. We first estimate the density of log pre-unemployment real hourly wages for all UI recipients in all states in 1995, 2000, and 2005 using kernel density estimation on the BAM micro sample of UI recipients. The left panel plots the change in this density from 1995 to 2000 for all states and separately by whether states had or had not yet adopted phone claiming technology by 2000. This was the time period when more than half of all states did so, but internet technology was not yet present. Though the wage distribution of all UI recipients shifted upwards, there does not appear to be differential trends in the earnings distribution by phone claiming adoption. The right panel plots the change in log wage density from 2000 to 2005 for all states and separately by whether states had adopted internet claiming technology by 2005. Again, we see no differential change in the earnings distribution of UI recipients by states' claiming technology.

In the context of finding only one significant change in the UI recipient population (fraction at maximum benefit amount) with the introduction of remote claiming, we conclude that increased filing ease had minimal impact on the characteristics of UI recipients.

5 Conclusion and Discussion

Our analysis suggests that states' adoption of remote forms of UI claiming (phone and internet) did not have an appreciable impact on UI participation and take-up. At face value, this finding is inconsistent with a time- and transaction-cost explanation for low take-up if remote UI claiming is indeed less time-intensive, as claimant surveys suggest. Sigma is also potentially less important in the UI context since the program is not means-tested. However, our estimates are sufficiently imprecise such that we cannot rule out moderate effects of a few percentage point increase in take-up rates. Large to moderate effects for a small population of marginal claimants may not show up in state aggregate take-up rates. This possibility underscores the importance of testing for heterogeneous effects. The evidence on whether claimant characteristics changed with the advent of remote claiming is also weak. We conclude that neither the adoption of phone nor internet claiming shifted the characteristics of UI claimants. States appear to have made considerable changes in administrative procedures and achieved substantial payroll cost reductions without a measurable impact on UI participation.

This finding differs from that of Kopczuk and Pop-Eleches (2007), who find that the introduction of electronic tax filing had a large effect on EITC participation. We speculate that differences in the role of intermediaries (e-file occurs primarily through private tax preparers) and information (e-filing provides information about EITC eligibility) may be important contextual differences between their study and ours. Movement to a system of employer-initiated automatic enrollment in unemployment insurance following job loss may be closer to the changes they examine than the ones we do and may be expected to have much larger effects. Future research about the role of program features and information on participation is needed. Recent randomized field experiments funded by H&R Block to test for presentation and inconvenience effects on participation in Food Stamps, federal financial student aid, and several other social programs holds particular promise.

References

[1] Anderson, Patricia M. and Bruce D. Meyer, 1997. “Unemployment Insurance Take-up Rates and the After-Tax Value of Benefits.” *The Quarterly Journal of Economics*, August 1997.

[2] Bansak, Cynthia and Steven Rapheal, 2006. "The effects of state policy design features on take-up and crowd-out rates for the state children’s health insurance program," *Journal of Policy Analysis and Management*, Volume 26, Issue 1, Winter 2007, Pages: 149-175.

[3] Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan, 2004. "How Much Should We Trust Differences-in-Differences Estimates?" *Quarterly Journal of Economics*, February 2004.

[4] Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan, 2002. "How Much Should We Trust Differences-in-Differences Estimates?" NBER Working Paper 8841.

[5] Blank, Rebecca M. and David E. Card, 1991. “Recent Trends in Insured and Uninsured Unemployment: Is There an Explanation?” *The Quarterly Journal of Economics*, November 1991.

[6] Chetty, Raj, Adam Looney, and Kory Kroft, 2008. "Salience and Taxation: Theory and Evidence," forthcoming *American Economic Review*.

[7] Currie, Janet, 2006. “The Take-Up of Social Benefits.” in Alan Auerbach, David Card, and John Quigley (eds). in *Poverty, The Distribution of Income, and Public Policy*, New York: Russell Sage, 2006.

[8] Currie, Janet and Jeffrey Grogger, 2001. "Medicaid Expansions and Welfare Contractions: Offsetting Effects on Prenatal Care and Infant Health?" *Journal of Health Economics*, Vol. 21 (2002): 313-335.

[9] Currie, Janet and Jeffrey Grogger, 2002. "Explaining Recent Declines in Food Stamp Program Participation." *Brookings-Warton Papers in Urban Economics 2*, 2001.

[10] Daponte, Beth Osborne, Seth Sanders, and Lowell Taylor, 1999. "Why Do Low-Income Families Not Use Food Stamps? Evidence from an Experiment." *The Journal of Human Resources*, Vol. 34, No. 3. (Summer, 1999), pp. 612-628.

[11] Fisher, R.A., 1935. *The Design of Experiments*. Edinburgh: Oliver and Boyd.

[12] Kenyon, Robert, Karen Needels, Todd Anderson, James Gerding, Michelle VanNoy, 2003. "Internet Initial Claims Evaluation." Report prepared for US Department of Labor. ETA Occasional Paper 2004-01.

[13] Kopczuk, Wojciech and Cristian Pop-Eleches, 2007. "Electronic filing, tax preparers and participation in the Earned Income Tax Credit," *Journal of Public Economics*, Vol 91 (2007): 1351-1367.

[14] Krueger, Alan and Bruce D. Meyer, 2002. "Labor Supply Effects of Social Insurance," in *Handbook of Public Economics*, vol. 4, Alan Auerbach and Martin Feldstein (eds.), (Amsterdam: North Holland, 2002).

[15] Johnston, Jack and John DiNardo, 1997. *Econometric Methods* (4th ed). New York: McGraw-Hill.

[16] Madrian, Brigitte C. and Dennis F. Shea, 2001. "The Power of Suggestion: Inertia in 401(k) Participation and Savings Behavior," *The Quarterly Journal of Economics*, Vol. 116, No. 4. (Nov., 2001), pp. 1149-1187.

[17] Marcus, Steven S., and Joseph W. Frees, 1998. "US Department of Labor Unemployment Insurance Claimant Satisfaction Study." Prepared for the US Department of Labor, Unemployment Insurance Service. Portland, OR: Bardsley and Neidhart, Inc. September 1998.

[18] McCall, Brian P. "The Impact of Unemployment Insurance Benefit Levels on Recipiency," *Journal of Business & Economic Statistics*, Vol. 13, No. 2, JBES Symposium on Program and Policy Evaluation. (April, 1995), pp. 189-198.

[19] Needels, Karen, Walter Corson, Tim Meier, Ira Harley, and Karen Blass, 2000. "Evaluation of the Impact of Telephone Initial Claims Filing." Report submitted to the US Department of Labor and the National UI Information Technology Support Center. Princeton, NJ: Mathematica Policy Research, Inc. March 2000.

[20] Remler, Dahlia K., Jason E. Rachlin, and Sherry A. Glied, "What Can The Take-up Of Other Programs Teach Us About How to Improve Take-up of Health Insurance Programs?" Working Paper 8185, NBER 2001.

[21] Saez, Emmanuel, "Details Matter: The Impact of Presentation and Information on the Take-up of Financial Incentives for Retirement Saving," CEPR Discussion Paper No. 6386, July 2007.

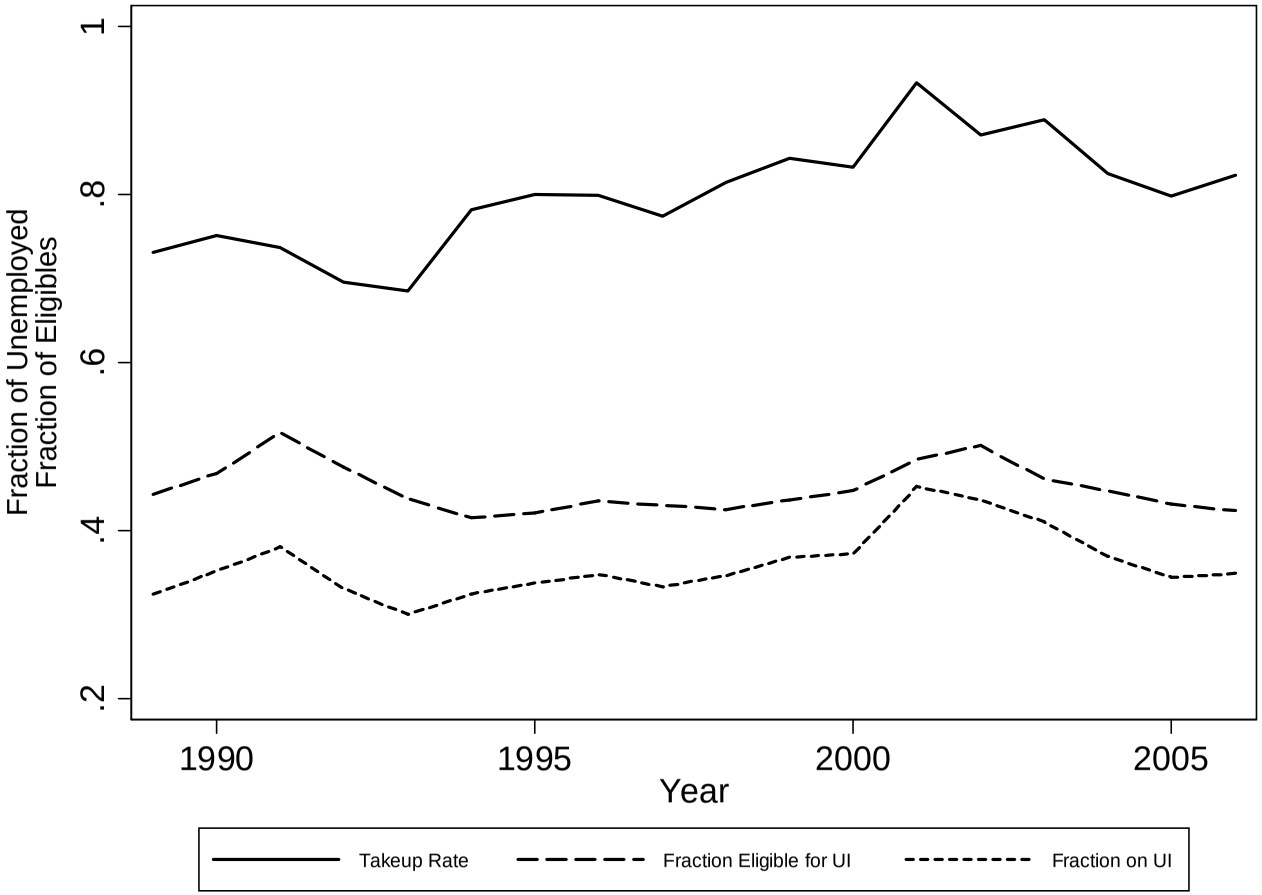
[22] Storer, Paul and Marc A. Van Audenrode, 1995. "Unemployment insurance take-up rates in Canada: facts, determinants, and implications." *Canadian Journal of Economics*, November 1995.

[23] U.S. Government Accountability Office, 2005. "Unemployment Insurance: Better Data Needed to Assess Reemployment Services to Claimants." GAO-05-413. Washington, D.C.: June 2005.

[24] Vroman, Wayne, 2002. "Low Benefit Recipiency in State Unemployment Insurance Programs." ETA Occasional Paper 2002-2 Prepared for the U.S. Department of Labor. Washington, DC: Urban Institute. June 2002.

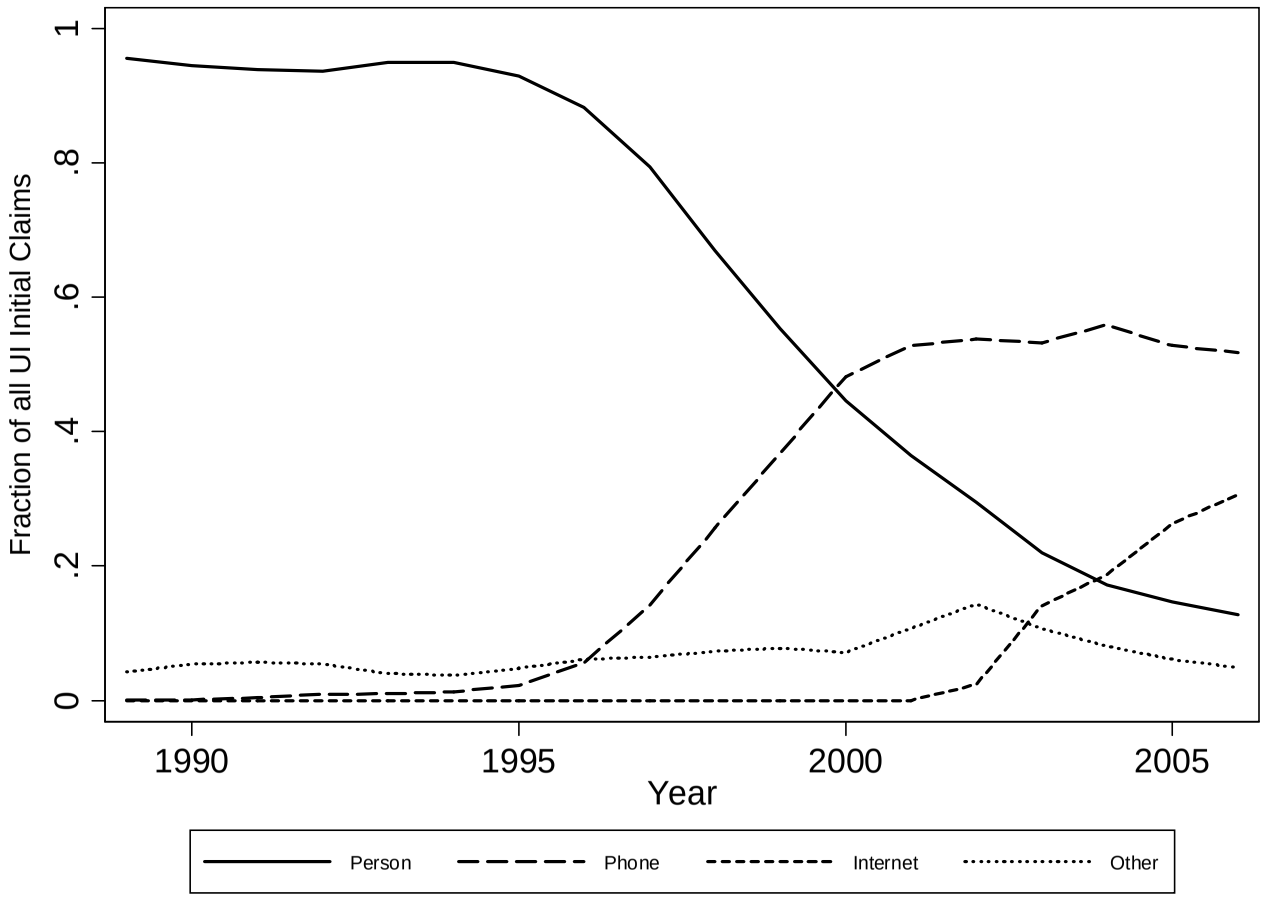
[25] Wandner, Stephen and Andrew Stettner, 2000. "Why are many jobless workers not applying for benefits?" *Monthly Labor Review*, June 2000.

Figure I: Aggregate Participation in Unemployment Insurance Program



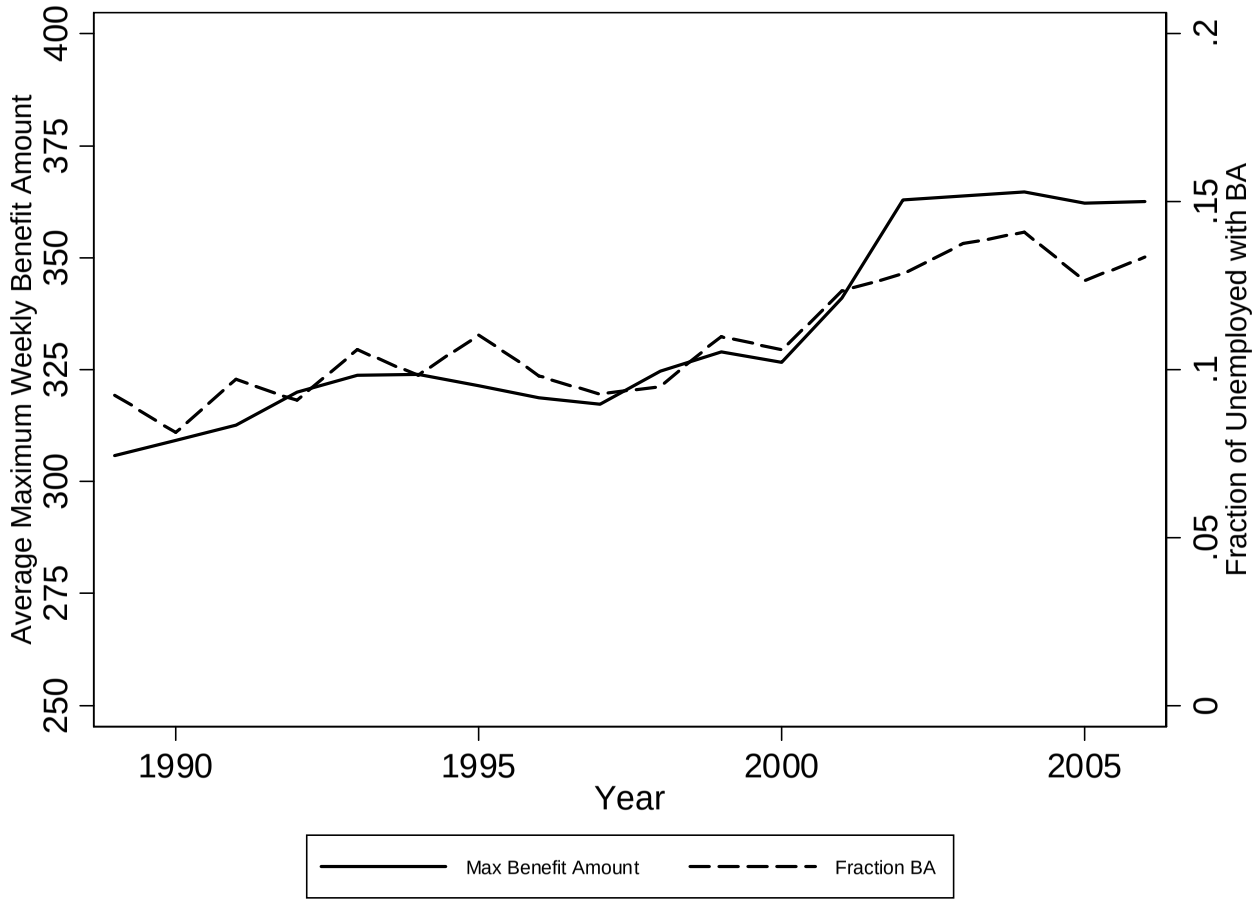
Sources: Fraction of unemployed on UI is from DOL administrative records and fraction of unemployed eligible for UI is estimated by the authors using the March CPS. See text for details on eligibility imputation. Take-up is the ratio between these two variables. These measures were constructed at the state-year level and aggregated to the U.S. using the number of unemployed workers as weights.

Figure II: Unemployment Insurance Claiming Method



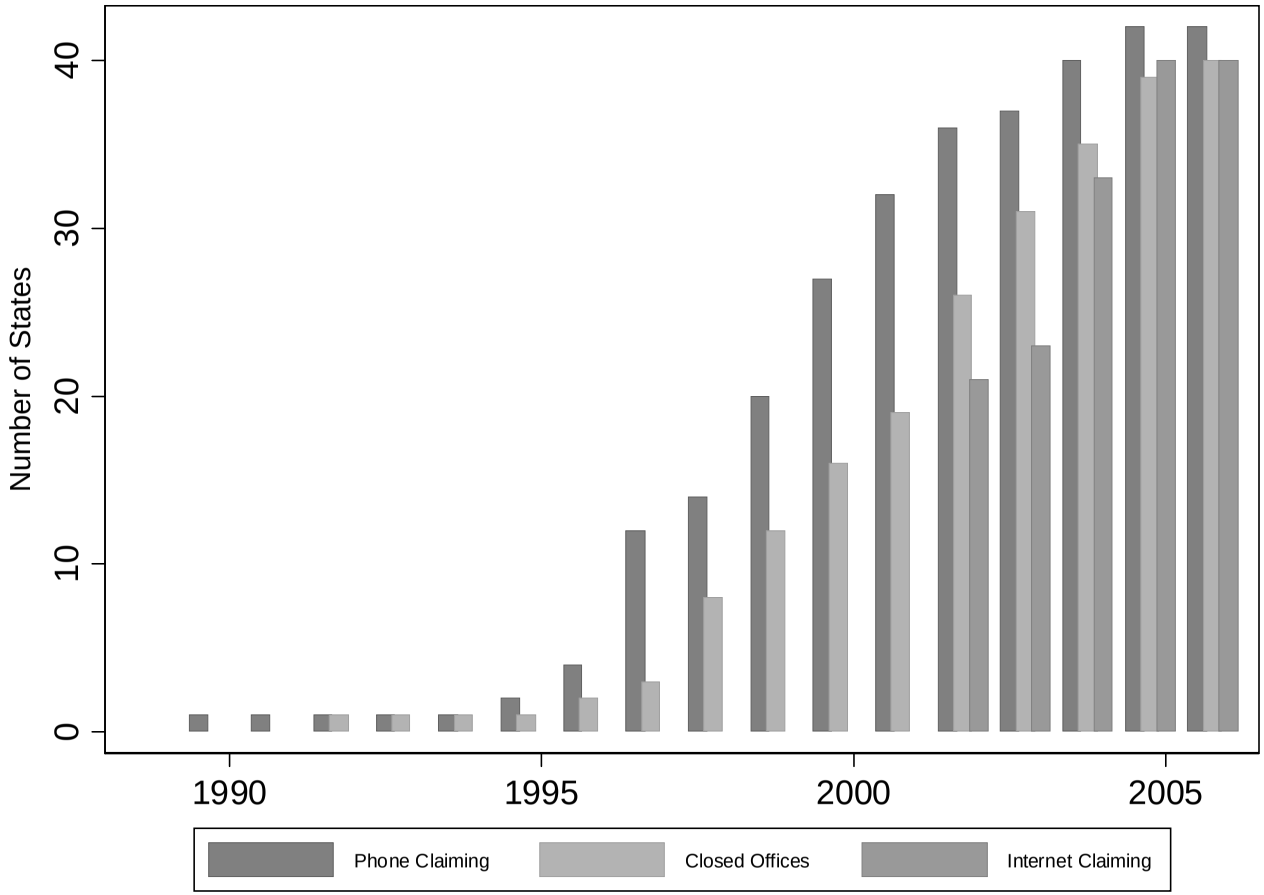
Sources: Authors calculations using BAM data. See text for details. Fractions were constructed at the state-year level and aggregated to the U.S. using number of UI claimants as weights.

Figure III: Changes in UI Generosity and Characteristics of the Unemployed



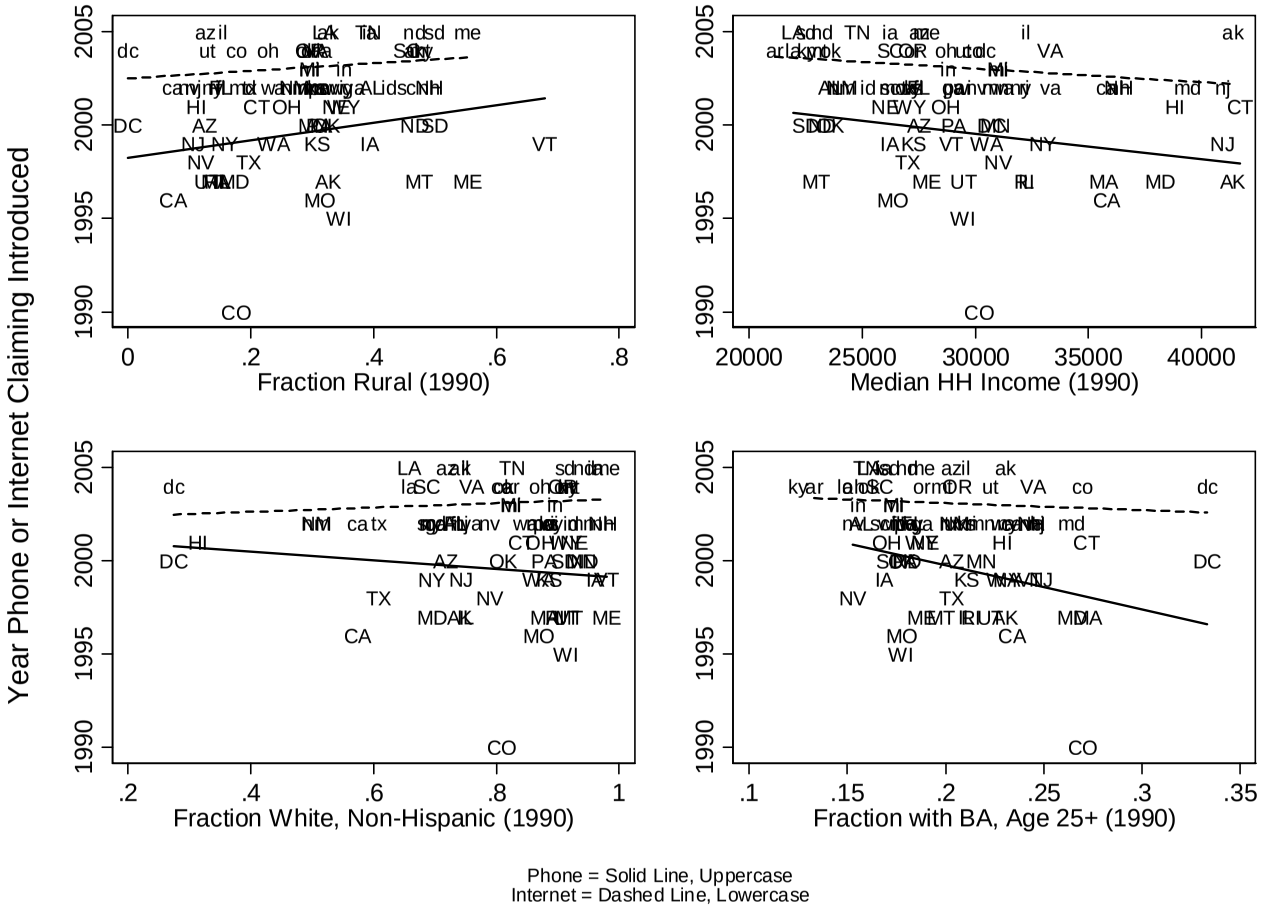
Sources: Maxmimum benefit amount is from the DOL ETA Significant Provisions of State Unemployment Insurance Laws. Fraction of unemployed with a BA is from authors calculations from the March CPS. Both were estimated at the state-year level and aggregated to the U.S. using the number of unemployed workers as weights.

Figure IV: Cumulative Number of States with Policy Change



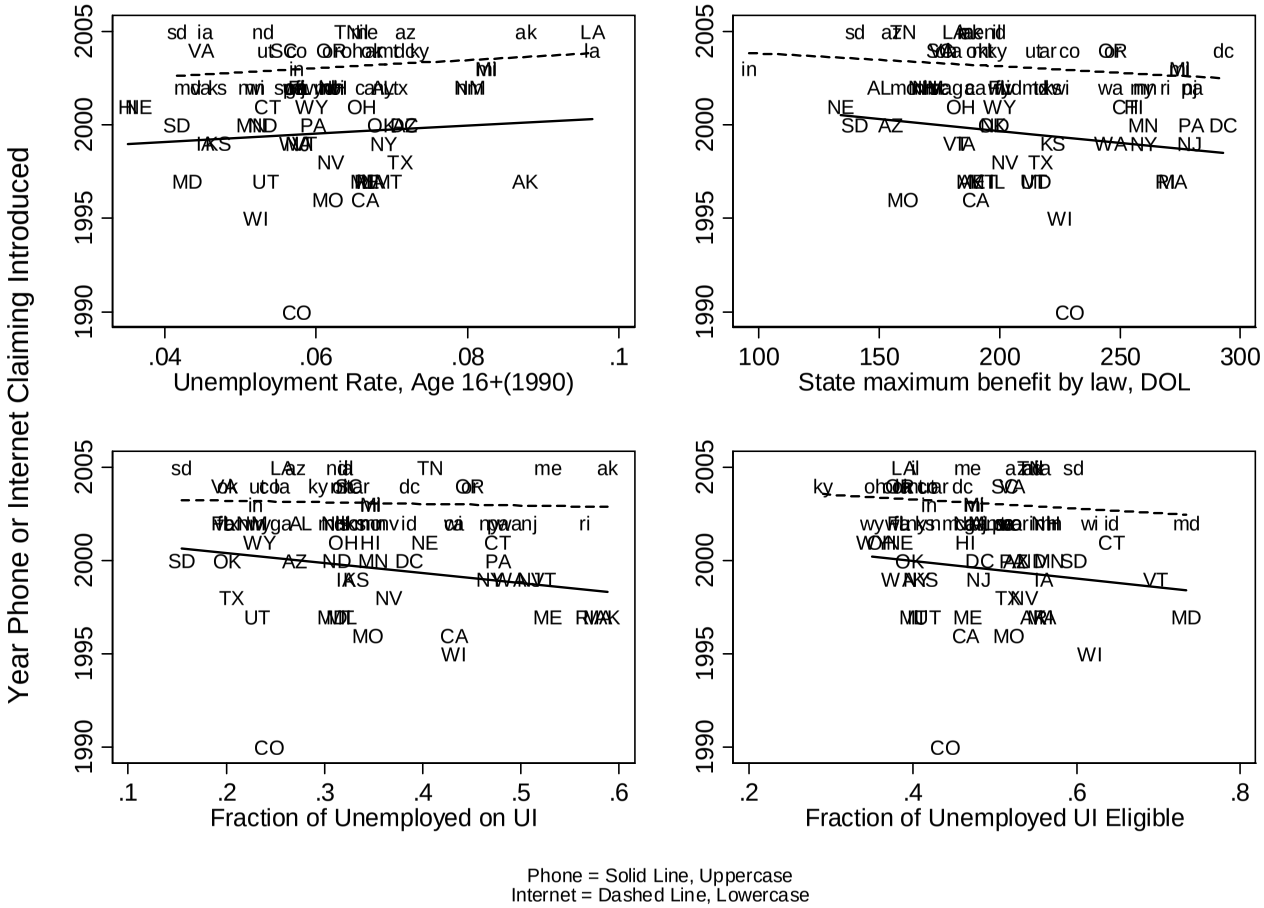
Sources: Constructed by authors from various sources. See Section 3 of text for details.

Figure V: Baseline Demographic Predictors of Policy Timing



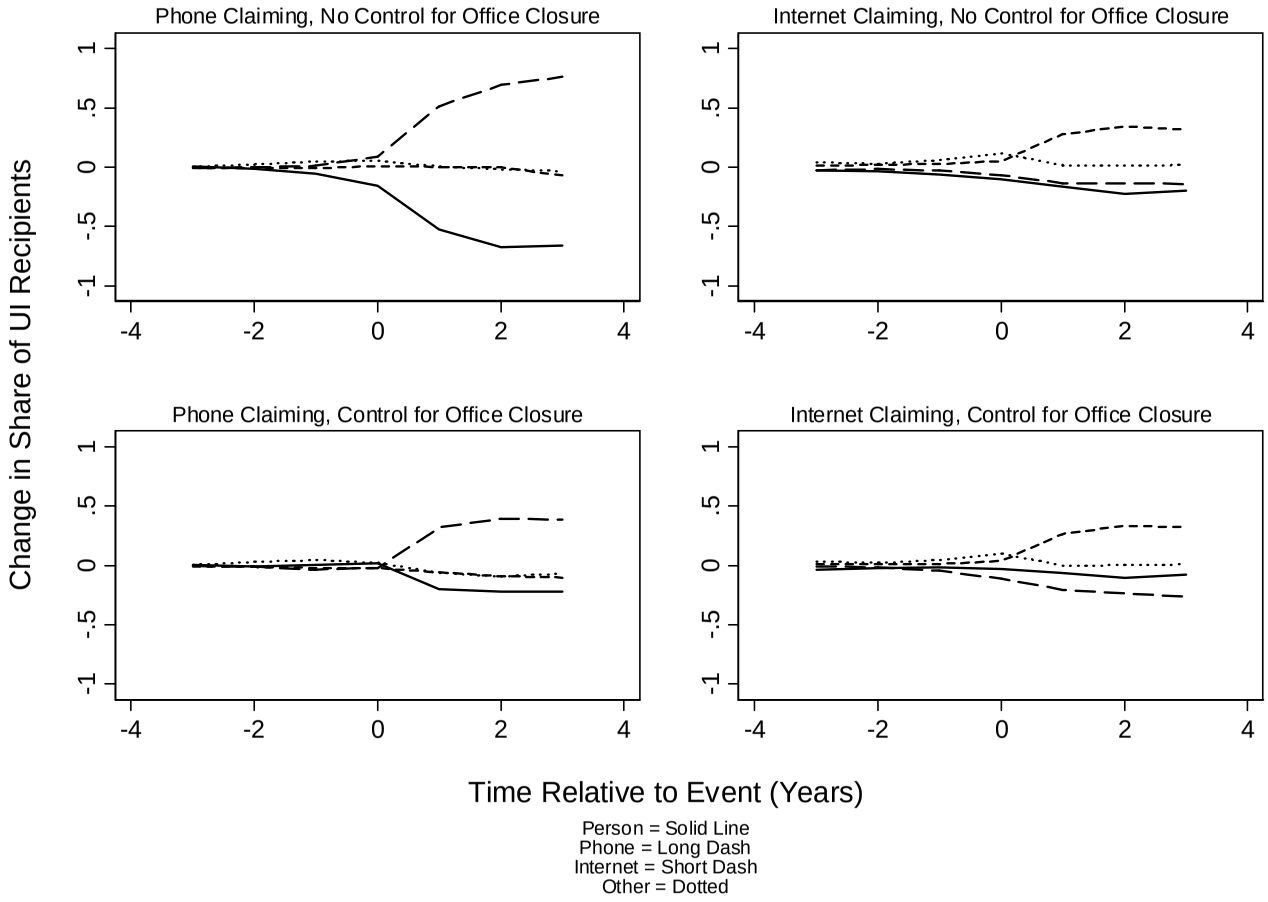
Sources: State demographic information is from the 1990 U.S. Census. Year of phone and internet claiming introduction was constructed by the authors from various sources. See text for details.

Figure VI: Baseline Labor Market Predictors of Policy Timing



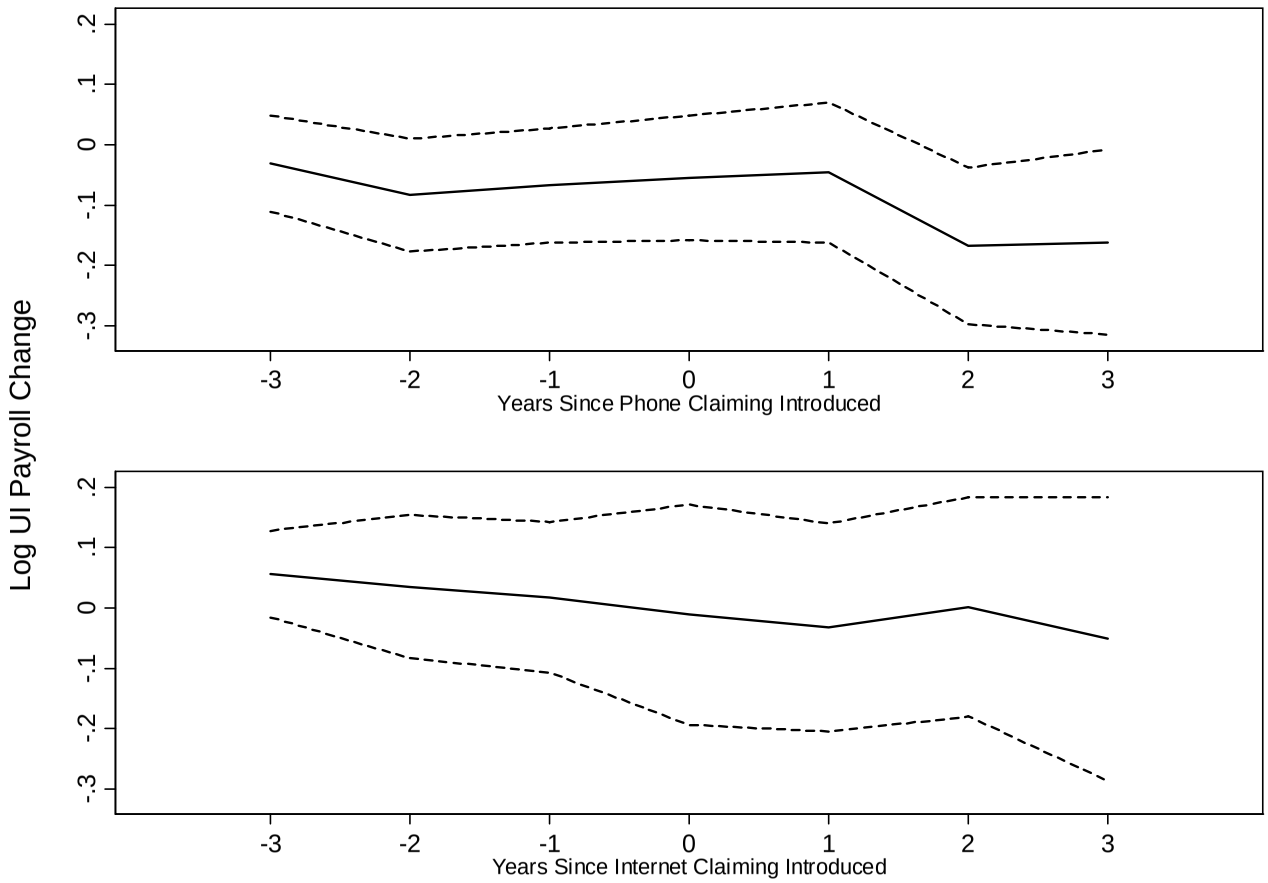
Sources: Unemployment rate is from the 1990 U.S. Census. State maximum benefit amount is from the DOL ETA. Fraction of unemployed on UI was calculated by the authors from administrative sources. Fraction of unemployed workers eligible for UI was estimated by the authors from the March CPS. Year of phone and internet claiming introduction was constructed by the authors from various sources. See text for details.

Figure VII: Speed of Adoption After Policy Change



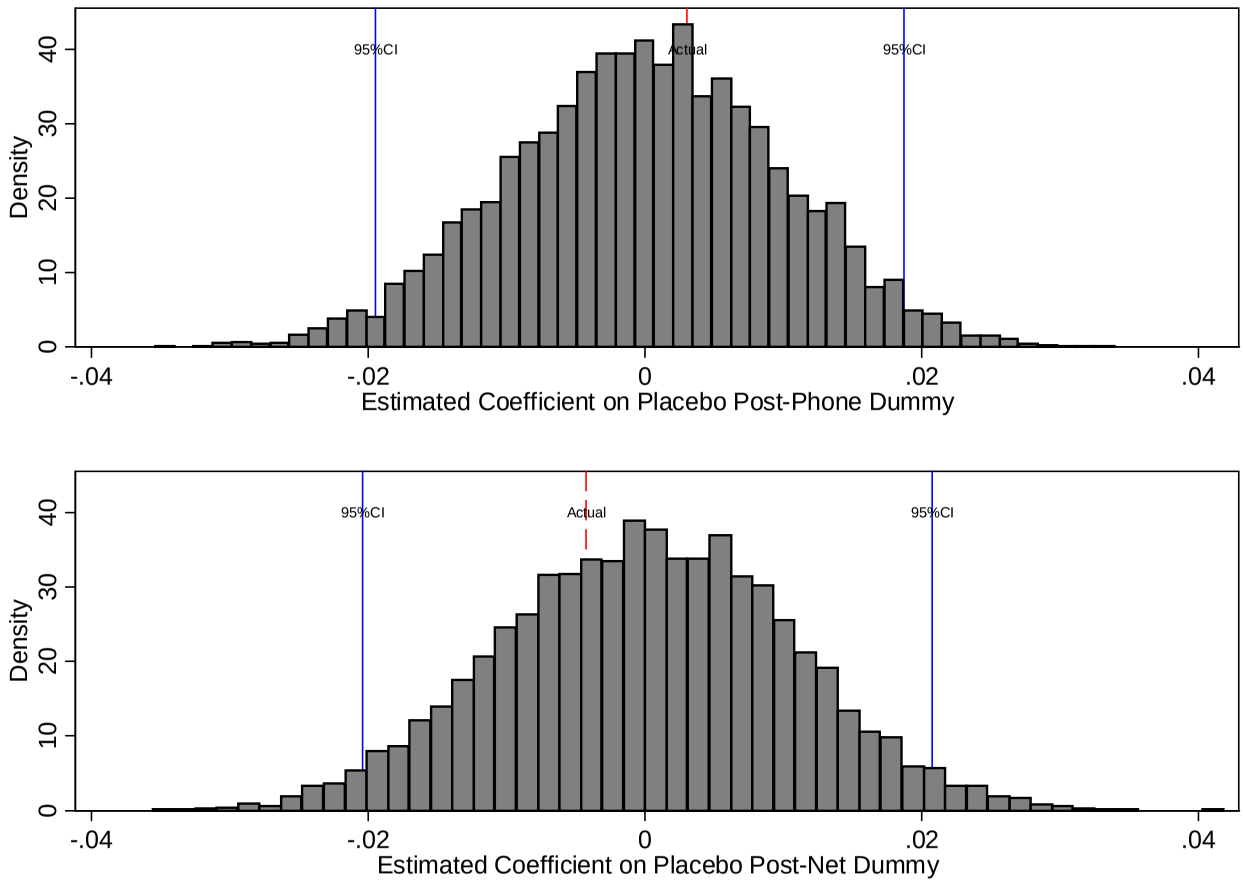
Notes: Graphs plot coefficients from OLS estimates of Equation (2) without controls for covariates. See text.

Figure VIII: Effect of Claiming Procedures on State Government UI Payroll Costs



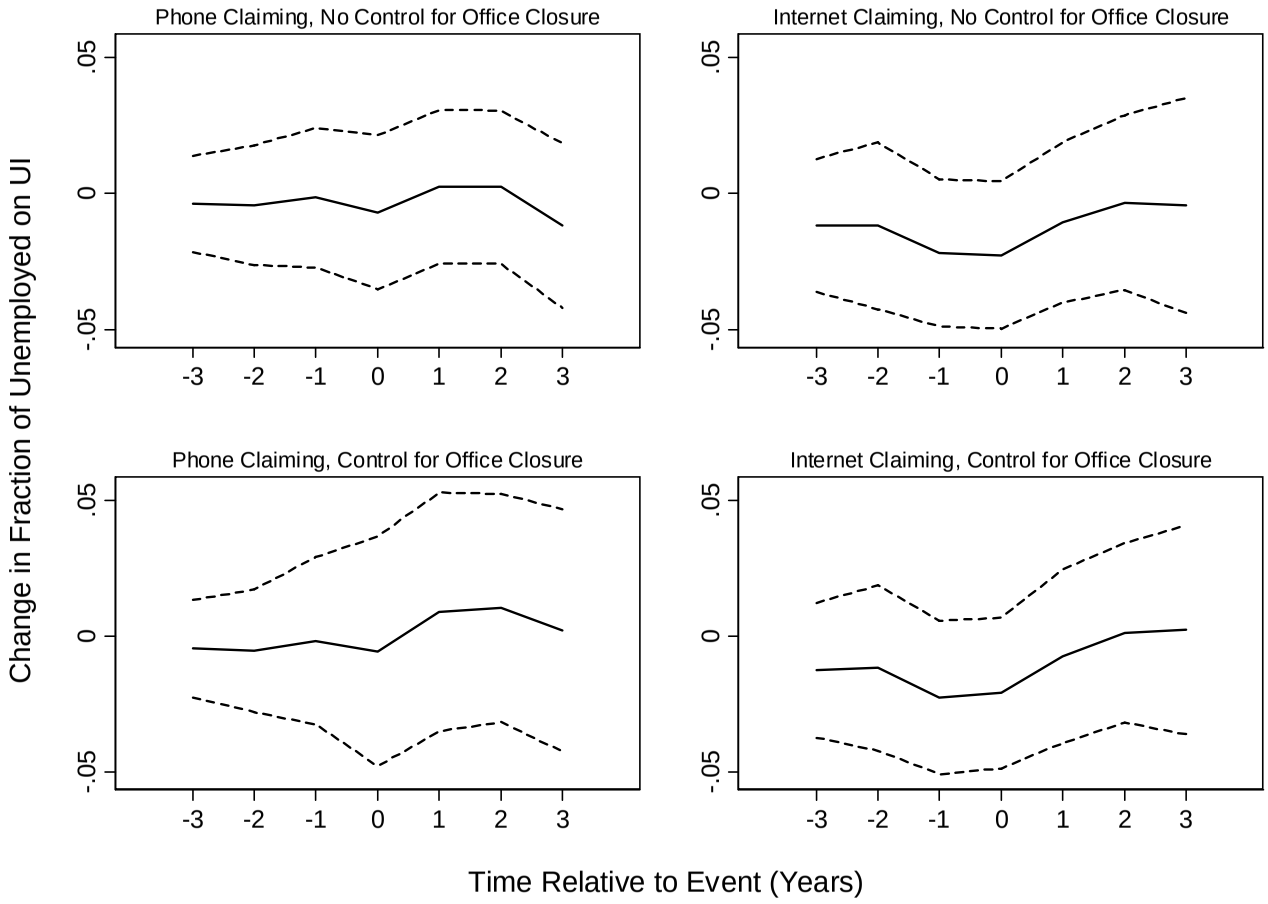
Notes: Graphs plot coefficients and 95% confidence intervals from OLS estimates of Equation (2) without controls for covariates or indicators for office closure. See text. Payroll data are from the Annual Survey of Government Employment and Payroll from the U.S. Census and refer to expenditure function code 22, "Social Insurance Administration."

Figure IX: Permutation Test for Inference of Main Result



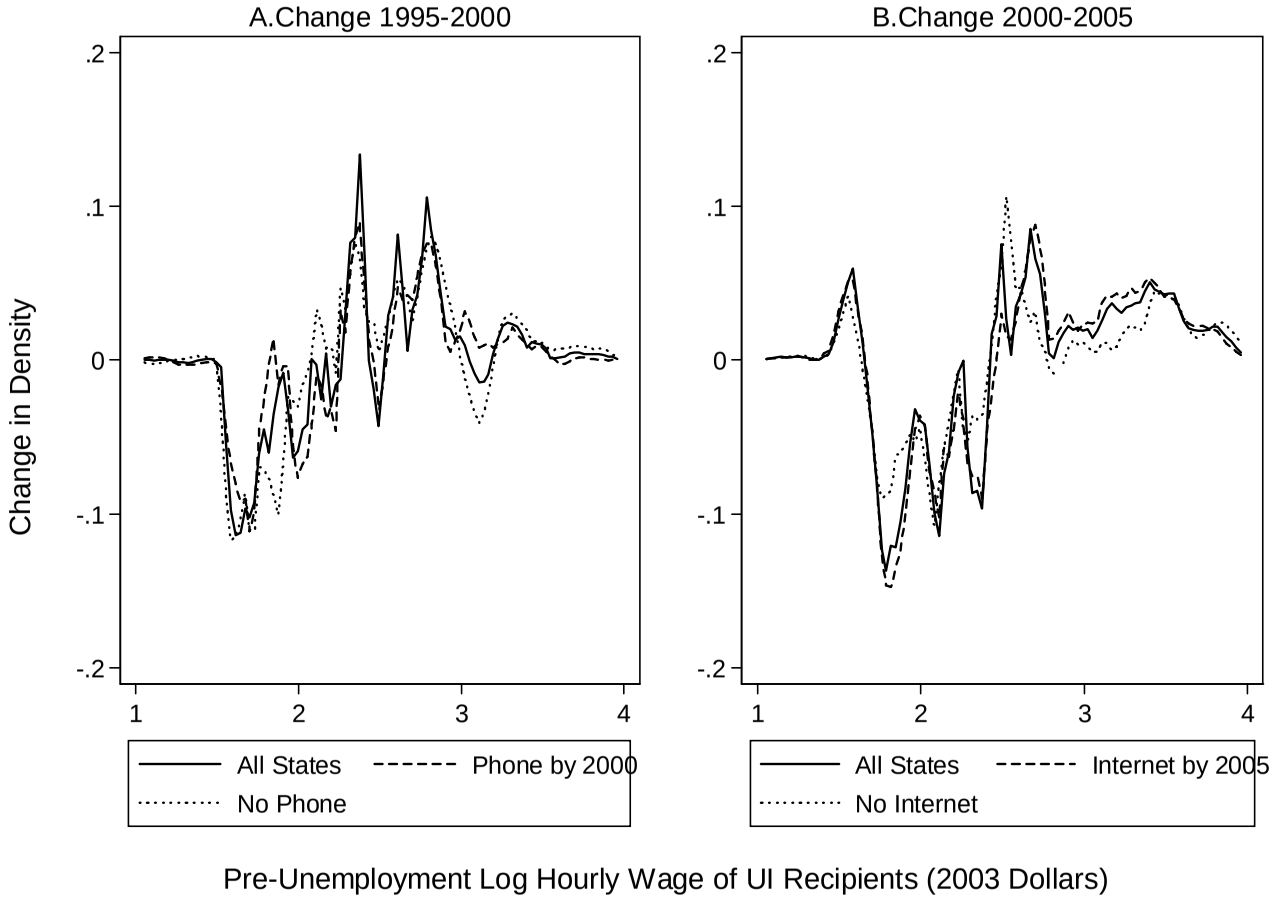
Notes: Figures plot the empirical distribution of estimated placebo treatment effects from 5,000 randomizations. Solid lines are the empirical 95% confidence intervals. Dashed lines are the actual treatment effect point estimates from Table 3, column (4). See text.

Figure X: Event Study Analysis of Fraction of Unemployed on UI



Notes: Graphs plot coefficients and 95% confidence intervals from OLS estimates of Equation (2) with full controls for covariates, with and without indicators for office closure. See text.

Figure XI: Change in Pre-Unemployment Wage Density by Policy Adoption



Notes: Graphs plot changes in wage density estimated from the BAM sample of UI claimants. Kernel density estimates use the Epanechnikov kernel with optimal bandwidth and were caclulated at 100 points. Weights used in the estimation were calculated using demographic characteristics of all UI claimants. Nominal wages are deflated by the CPI-U. Observations with real hourly wages greater than \$50/hr or less than \$3/hr were set to missing and not used in the estimates.

Table I: Summary Statistics

Variable	mean	st dev	min	max
Measures of Participation				
Fraction of unemployed on UI	0.36	0.11	0.15	0.76
Fraction of unemployed eligible for UI	0.47	0.10	0.19	0.79
Estimated UI take-up rate	0.79	0.27	0.26	2.04
Characteristics of UI Recipients				
% College degree	0.11	0.05	0.03	0.49
% At max benefit amount	0.24	0.16	0.00	0.91
Average pre-unemployment hourly wage	13.83	2.19	9.17	21.82
UI Policies and Claiming Method				
Max benefit amount ($100)	3.14	0.67	1.35	5.24
Post phone	0.34	0.47	0.00	1.00
Post internet	0.17	0.38	0.00	1.00
Post close UI offices	0.26	0.44	0.00	1.00
% Claims filed in person	0.65	0.40	0.00	1.00
% Claims filed over phone	0.23	0.35	0.00	1.00
% Claims filed over internet	0.05	0.14	0.00	0.99
% Claims filed postal mail	0.03	0.10	0.00	0.86
% Claims filed by employer	0.02	0.05	0.00	0.37
% Claims filed other method	0.01	0.05	0.00	0.53
Characteristics of the Unemployed				
Average age (x10)	3.10	0.36	1.90	4.50
% Female	0.42	0.08	0.18	0.72
% Nonwhite	0.25	0.19	0.00	0.91
% College degree	0.10	0.06	0.00	0.40
Avg weeks worked last year (x10)	2.55	0.37	1.31	3.76
Avg weeks looking for work (x10)	1.54	0.45	0.52	3.13
Avg earnings in previous year ($1000)	10.98	4.10	3.33	35.53

There are 918 state x year observations, corresponding to 51 states (including DC) and 18 years (1989-2006). Statistics are unweighted.

Table II: Effect of Remote Claiming on UI Administrative Costs

	Log of FTE Employment		Log of Payroll		Fraction of Employment PT	
	(1)	(2)	(3)	(4)	(5)	(6)
Post phone	-0.048	0.024	-0.052	0.018	-0.003	0.003
	(0.037)	(0.042)	(0.038)	(0.038)	(0.011)	(0.010)
Post internet	-0.032	-0.014	-0.041	-0.023	-0.007	-0.006
	(0.056)	(0.049)	(0.058)	(0.052)	(0.010)	(0.010)
Post close		-0.149**		-0.144**		-0.011
		(0.059)		(0.055)		(0.011)
Observations	700	700	700	700	700	700
R-squared	0.955	0.956	0.957	0.958	0.763	0.765

All regressions include state and year fixed effects. Robust standard errors clustered by state in parentheses. There are 700 state x year observations, corresponding to 50 states and 14 years (1992-2006 excluding 1996). Outcome data are from the Annual Survey of Government Employment and Payroll collected by the U.S. Census, expenditure fuction code 22: "Social Insurance Administration." * significant at 90%; ** significant at 95%; *** significant at 99%.

Table III: Effect of Remote Claiming on Fraction of Unemployed on UI

	Fraction of Unemployed on UI			
	(1)	(2)	(3)	(4)
Post phone	-0.002	0.002	0.003	0.003
	(0.010)	(0.011)	(0.011)	(0.010)
Post internet	-0.003	-0.001	-0.003	-0.004
	(0.010)	(0.010)	(0.010)	(0.010)
Post close		-0.008	-0.009	-0.010
		(0.013)	(0.012)	(0.012)
Max bens ($100)			0.016	0.014
			(0.016)	(0.015)
Avg earnings of unemployed ($1000)				0.001
				(0.001)
% Unemployed female				0.026
				(0.033)
% Unemployed nonwhite				-0.043
				(0.027)
% Unemployed college				0.049
				(0.054)
Average age of unemployed (x10)				0.002
				(0.008)
Avg weeks worked last year (x10)				0.008
				(0.008)
Avg weeks looking for work (x10)				-0.019***
				(0.007)
Observations	918	918	918	918
R-squared	0.835	0.836	0.837	0.845

All regressions include state and year fixed effects. Robust standard errors clustered by state in parentheses.There are 918 state x year observations, corresponding to 51 states (including DC) and 18 years (1989-2006). Fraction on UI is from administrative records. * significant at 90%; ** significant at 95%; *** significant at 99%.

Table IV: Effect of Remote Claiming on Takeup Among UI Eligible

	(Fraction of Unemployed on UI)/ (Fraction of Unemployed Eligible for UI)			
	(1)	(2)	(3)	(4)
Post phone	-0.043*	-0.026	-0.025	-0.017
	(0.024)	(0.028)	(0.027)	(0.026)
Post internet	-0.025	-0.020	-0.021	-0.026
	(0.030)	(0.029)	(0.030)	(0.032)
Post close		-0.035	-0.036	-0.038
		(0.031)	(0.030)	(0.032)
Max bens ($100)			0.018	0.021
			(0.040)	(0.041)
Avg earnings of unemployed ($1000)				0.002
				(0.003)
% Unemployed female				0.139*
				(0.078)
% Unemployed nonwhite				-0.129
				(0.135)
% Unemployed college				0.175
				(0.125)
Average age of unemployed (x10)				-0.034
				(0.026)
Avg weeks worked last year (x10)				-0.148***
				(0.030)
Avg weeks looking for work (x10)				0.0072
				(0.022)
Observations	918	918	918	918
R-squared	0.656	0.656	0.657	0.685

All regressions include state and year fixed effects. Robust standard errors clustered by state in parentheses.There are 918 state x year observations, corresponding to 51 states (including DC) and 18 years (1989-2006). Fraction on UI are from administrative records. Fraction eligible are estimated from March CPS; see text for details. * significant at 90%; ** significant at 95%; *** significant at 99%.

Table V: Robustness of Main Results on Fraction of Unemployed on UI

		Linear trends		Policy dates		Restricted sample		
	Base model	State-specific trends	Linear trends X baseline chrx	Interviews only	Threshold procedure	Phone states only	Internet states only	Post-1994
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post phone	0.003	0.016	0.007	0.005	0.003	0.018	0.008	0.003
	(0.010)	(0.012)	(0.011)	(0.018)	(0.009)	(0.011)	(0.011)	(0.011)
Post internet	-0.004	0.003	-0.002	-0.030	0.001	0.002	0.012	-0.001
	(0.010)	(0.011)	(0.009)	(0.019)	(0.012)	(0.009)	(0.011)	(0.011)
Post close	-0.010	0.005	-0.009	0.001	-0.009	-0.004	-0.011	-0.011
	(0.012)	(0.013)	(0.012)	(0.018)	(0.012)	(0.013)	(0.013)	(0.014)
Max bens ($100)	0.014	0.013	0.008	0.018	0.014	0.014	0.013	0.006
	(0.015)	(0.016)	(0.012)	(0.027)	(0.015)	(0.016)	(0.018)	(0.016)
Avg earnings of unemployed ($1000)	0.001	0.000	0.001	0.002	0.001	0.001	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
% Unemployed female	0.026	0.009	0.021	0.034	0.024	0.053	-0.015	0.010
	(0.033)	(0.027)	(0.030)	(0.047)	(0.033)	(0.032)	(0.035)	(0.033)
% Unemployed nonwhite	-0.043	-0.025	-0.043	-0.049	-0.041	-0.012	-0.021	-0.056*
	(0.027)	(0.026)	(0.027)	(0.040)	(0.027)	(0.036)	(0.035)	(0.032)
% Unemployed college	0.049	0.035	0.055	0.178**	0.049	0.060	0.009	0.090
	(0.054)	(0.059)	(0.053)	(0.073)	(0.054)	(0.059)	(0.044)	(0.079)
Average age of unemployed (x10)	0.002	0.002	-0.001	0.019	0.002	0.001	-0.008	0.002
	(0.008)	(0.008)	(0.008)	(0.013)	(0.008)	(0.009)	(0.007)	(0.009)
Avg weeks worked last year (x10)	0.008	0.0129**	0.009	-0.006	0.008	0.013	0.021***	0.005
	(0.008)	(0.006)	(0.007)	(0.013)	(0.008)	(0.009)	(0.007)	(0.008)
Avg weeks looking for work (x10)	-0.019**	-0.014**	-0.015**	-0.017**	-0.019***	-0.017**	-0.009	-0.012
	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)	(0.008)	(0.007)	(0.008)
Observations	918	918	918	396	918	756	720	611
R-squared	0.845	0.882	0.854	0.834	0.844	0.852	0.856	0.867

Dependent variable is fraction of unemployed on UI from administrative records. All regressions also include state and year fixed effects. Robust standard errors clustered by state in parentheses. (2) includes state-specific linear time trends. (3) includes linear time trends interacted with median household income, fraction rural, fraction nonwhite, fraction with BA, unemployment rate and not-in-labor force rate from 1990 Census. (4) uses only interviewed states. (5) uses passing a 5% threshold for indicating phone and internet introduction. (6) uses only states that implemented phone claiming. (7) uses only states that implemented internet claiming. (8) uses post-1994 observations that had not implemented phone claiming by 1997. * significant at 90%; ** significant at 95%; *** significant at 99%.

Table VI: Effect of Remote Claiming on Characteristics of UI Claimants

	Characteristics of UI Recipients			Percentile of Log Hourly Pre-Unemployment Wage			
	% Receive Max Ben	% College	Avg Hourly Wage	25th	50th	75th	90th
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Max bens ($100)	-0.192***	0.013	0.442	0.032*	0.037*	0.045*	0.030
	(0.033)	(0.010)	(0.411)	(0.017)	(0.020)	(0.023)	(0.030)
Post phone	0.005	-0.001	0.026	-0.007	0.000	-0.009	-0.006
	(0.022)	(0.004)	(0.181)	(0.009)	(0.010)	(0.013)	(0.014)
Post internet	0.045**	-0.002	0.218	0.004	0.012	0.021	0.022
	(0.022)	(0.007)	(0.218)	(0.011)	(0.011)	(0.014)	(0.017)
Post close	-0.037*	0.003	-0.054	0.001	-0.003	-0.002	-0.003
	(0.020)	(0.004)	(0.200)	(0.011)	(0.011)	(0.013)	(0.016)
Avg earnings of unemployed ($1000)	-0.003	0.001	0.023*	0.001	0.002**	0.002**	0.003***
	(0.002)	(0.001)	(0.012)	(0.001)	(0.001)	(0.001)	(0.001)
% Unemployed female	0.012	-0.002	-0.049	-0.001	-0.021	-0.005	0.020
	(0.059)	(0.009)	(0.411)	(0.024)	(0.025)	(0.028)	(0.036)
% Unemployed nonwhite	0.002	0.000	-0.556	-0.043	-0.024	-0.050	-0.005
	(0.053)	(0.015)	(0.425)	(0.028)	(0.030)	(0.031)	(0.031)
% Unemployed college	-0.006	0.033*	0.951	0.019	0.016	0.028	0.025
	(0.066)	(0.017)	(0.608)	(0.037)	(0.038)	(0.046)	(0.058)
Average age of unemployed (x10)	-0.011	0.002	-0.170*	-0.019***	-0.020***	-0.017***	-0.006
	(0.017)	(0.002)	(0.093)	(0.006)	(0.005)	(0.006)	(0.006)
Avg weeks worked last year (x10)	0.029**	-0.001	0.123	0.024***	0.0107*	0.009	0.001
	(0.014)	(0.002)	(0.086)	(0.007)	(0.006)	(0.007)	(0.008)
Avg weeks looking for work (x10)	0.020	-0.002	0.085	0.009	0.011*	0.015**	0.015*
	(0.014)	(0.002)	(0.088)	(0.006)	(0.006)	(0.006)	(0.008)
Observations	918	918	918	918	918	918	918
R-squared	0.593	0.762	0.894	0.891	0.908	0.916	0.903

All regressions include state and year fixed effects. Robust standard errors clustered by state in parentheses. There are 918 state x year observations, corresponding to 51 states (including DC) and 18 years (1989-2006). All outcome variables are estimated from the DOL BAM sample of UI claimants. * significant at 90%; ** significant at 95%; *** significant at 99%.

Appendix: Additional Tables and Figures

Figure A I: UI Claiming Method by State

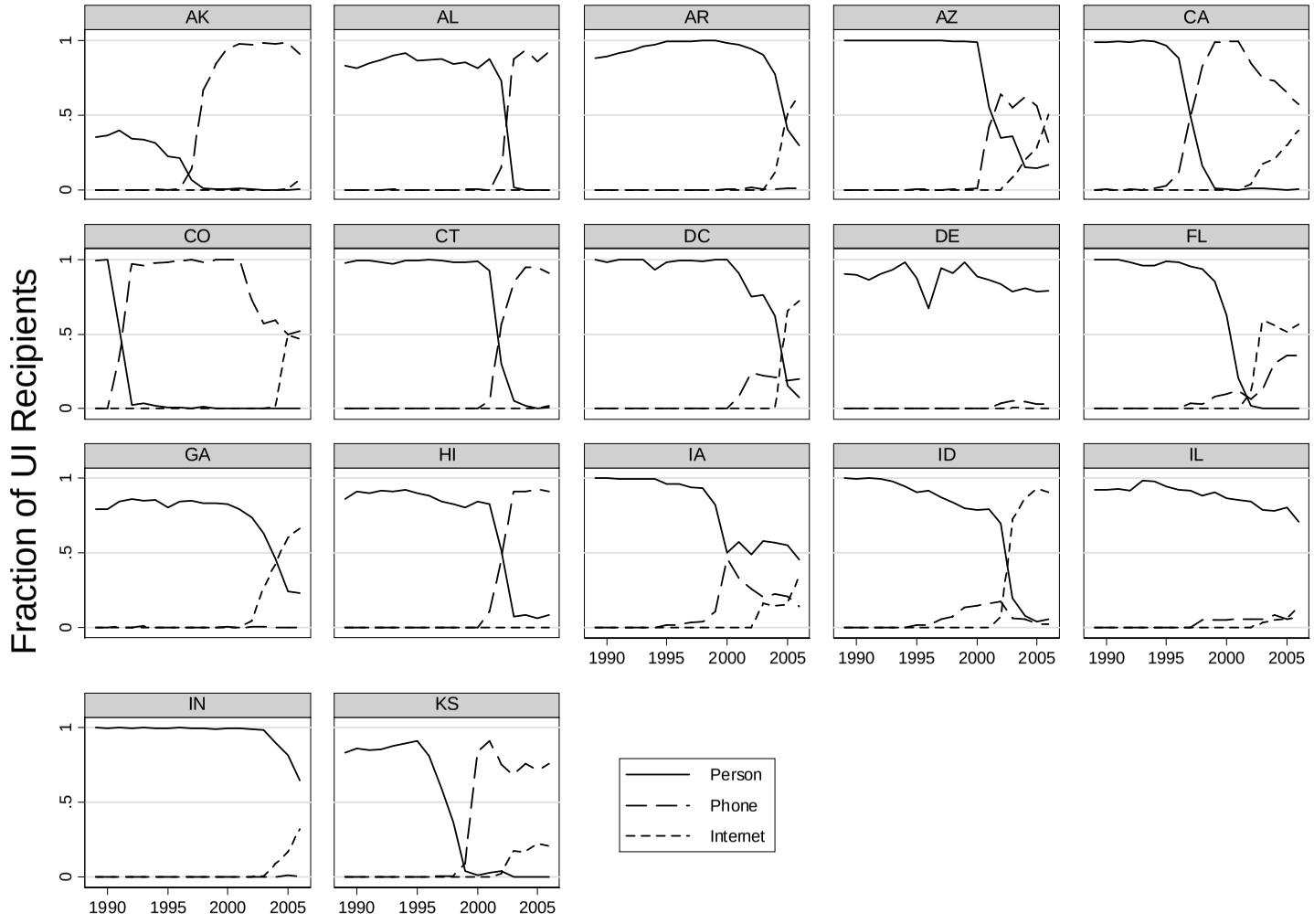


Figure A II: UI Claiming Method by State (Continued)

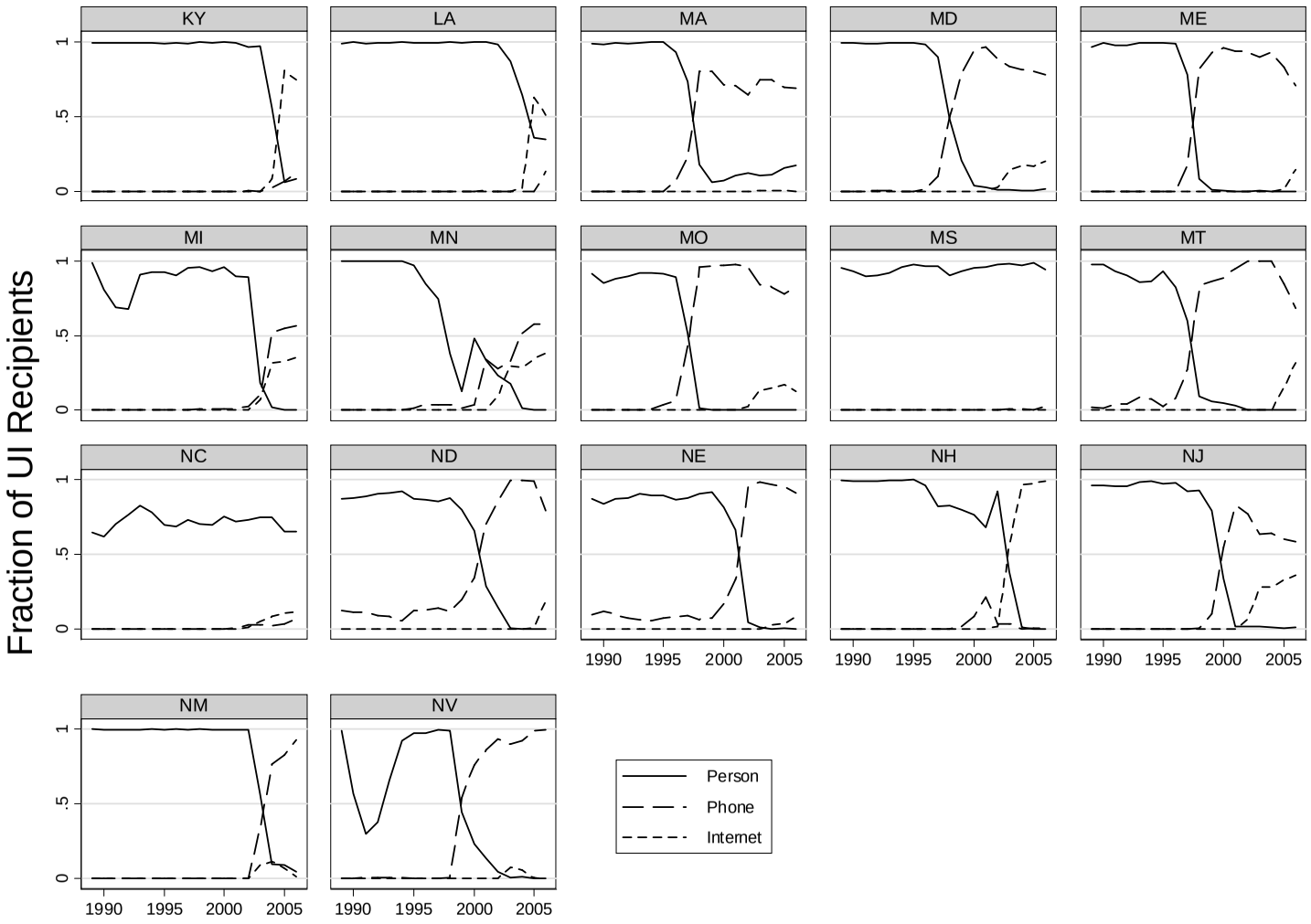


Figure A III: UI Claiming Method by State (Continued)

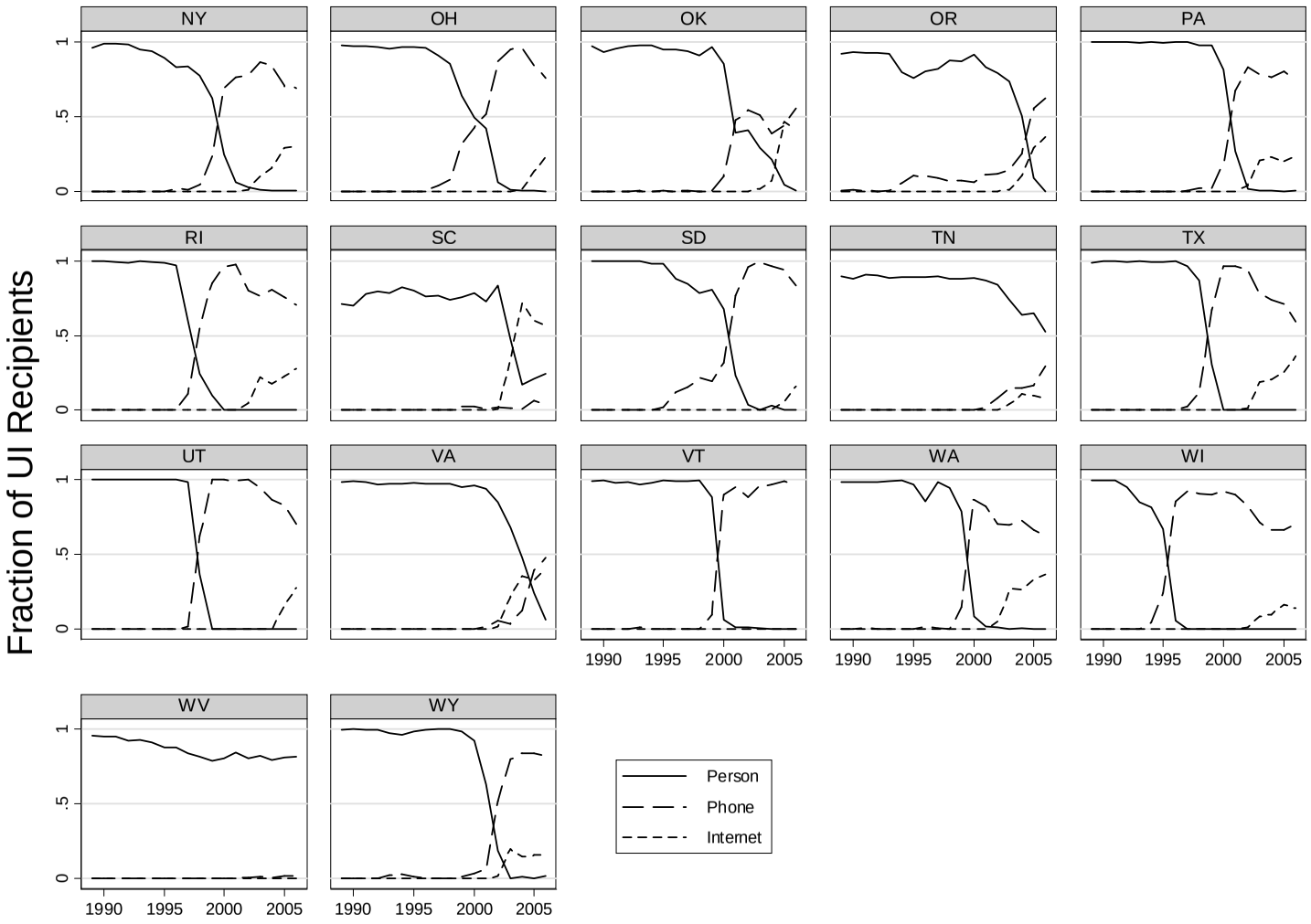


Table A I: Dates of Changes in Claiming Method

	Phone Claims		Internet Claims		Office Closure	
State	Imputed	Interview	Imputed	Interview	Imputed	Interview
AK	1997	.	2005	.	1997	.
AL	2002	2002	never	never	2003	2002
AR	never	.	2004	.	never	.
AZ	2000	.	2005	.	2004	.
CA	1996	.	2002	.	1998	.
CO	1990	1991	2004	2005	1992	1991
CT	2001	2001	never	2005	2003	2001
DC	2000	never	2004	2005	2005	never
DE	never	never	never	never	never	never
FL	2002	.	2002	.	2002	.
GA	never	never	2002	never	never	never
HI	2001	.	never	.	2003	.
IA	1999	.	2005	.	never	.
ID	never	.	2002	.	2003	.
IL	1997	.	2005	.	never	.
IN	never	.	2003	.	never	.
KS	1999	.	2002	.	1999	.
KY	never	.	2004	.	2005	.
LA	2005	.	2004	.	never	.
MA	1997	1996	never	never	1998	never
MD	1997	1997	2002	2002	2000	1997
ME	1997	1997	2005	2006	1998	1997
MI	2003	.	2003	.	2003	.
MN	2000	.	2002	.	1999	.
MO	1996	1997	2002	2003	1998	1997
MS	never	.	never	.	never	.
MT	1997	1997	2004	2005	1998	1997
NC	never	2005	never	2003	never	never
ND	2000	2001	2005	2004	2002	2001
NE	2001	2001	never	2004	2002	never
NH	2002	never	2002	2002	2004	never
NJ	1999	.	2002	.	2001	.
NM	2002	.	2002	.	2004	.
NV	1998	.	2002	.	2001	.
NY	1999	1998	2002	2001	2001	2002
OH	2001	2002	2004	2004	2002	2003
OK	2000	2000	2004	2004	2005	2003
OR	2004	2004	2004	2005	2005	2005
PA	2000	.	2002	.	2002	.
RI	1997	1998	2002	2002	1999	1999
SC	2004	never	2002	2002	2004	never
SD	2000	.	2005	.	2002	.
TN	2005	2001	never	2003	never	never
TX	1998	.	2002	.	2000	.
UT	1997	1997	2004	2000	1999	1997
VA	2004	2003	2002	2002	2006	never
VT	1999	.	never	.	2000	.
WA	1999	1999	2002	2000	2000	1999
WI	1995	1995	2002	2003	1996	1995
WV	never	never	never	never	never	never
WY	2001	2002	2002	2004	2002	2002

Notes: Policy dates were imputed using the share of claims by filing method. The year in which the share of claims filed via phone (internet) accelerated by the greatest amount was designated the event year. Office closure date was imputed as the first year that in-person claims fell below 20%. Missing values indicate that an interview was not conducted with that state nor was policy information found in published sources or news releases.

Table A II: Correlation between Fraction Eligible for UI and Fraction on UI

Correlation between fraction on UI and fraction eligible for UI							
Within year		Within state					
1989	0.06	AL	-0.04	KY	0.30	ND	0.27
1990	0.19	AK	-0.10	LA	-0.18	OH	0.45
1991	0.02	AZ	0.53	ME	0.08	OK	0.50
1992	0.21	AR	0.23	MD	0.12	OR	0.02
1993	0.19	CA	0.65	MA	0.48	PA	0.60
1994	0.20	CO	0.16	MI	0.35	RI	0.20
1995	0.15	CT	0.20	MN	0.36	SC	0.50
1996	0.27	DE	-0.28	MS	-0.06	SD	0.19
1997	0.29	DC	0.51	MO	0.17	TN	0.69
1998	0.29	FL	0.34	MT	0.27	TX	0.19
1999	0.14	GA	0.22	NE	-0.12	UT	0.49
2000	0.34	HI	0.30	NV	0.45	VT	0.00
2001	0.18	ID	0.22	NH	0.54	VA	-0.17
2002	0.24	IL	0.74	NJ	0.41	WA	-0.02
2003	0.41	IN	0.10	NM	0.39	WV	0.29
2004	0.35	IA	0.52	NY	0.47	WI	-0.06
2005	0.36	KS	0.00	NC	-0.21	WY	0.09
2006	0.31						
		All states, all years: 0.26					

Sources: Fraction of unemployed on UI is from DOL administrative records and fraction of unemployed eligible for UI is estimated by the authors using the March CPS. See text for details on eligibility imputation. These measures were constructed at the state-year level and aggregated to the U.S. using the number of unemployed workers as weights.