UNEMPLOYMENT INSURANCE AS A WORKER INDISCIPLINE DEVICE? EVIDENCE FROM SCANNER DATA*

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Abstract

We provide causal evidence of an ex ante moral hazard effect of Unemployment Insurance (UI) by matching plausibly exogenous changes in UI benefit duration across state-weeks during the Great Recession to high-frequency productivity measures from individual supermarket cashiers. Estimating models with day and cashier-register fixed effects, we identify a modest but statistically significant negative relationship between UI benefits and worker productivity. This effect is strongest for more experienced and less productive cashiers, for whom UI expansions are especially relevant. Additional analyses from the American Time Use Survey reveal a similar increase in shirking during periods with increased UI benefit durations.

Keywords: Unemployment Insurance; Shirking; Scanner Data

JEL codes: I38; J24; J38; J65; L81

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1 Introduction

The Great Recession saw a dramatic increase in the duration of benefits available through the Unemployment Insurance (UI) program in the United States. Previously limited to between 26 and 30 weeks, by late 2009 eligible unemployed individuals in some states were able to receive benefits for up to 99 weeks. These dramatic expansions together with the decline in job availability led to a near 500% rise in the program's per-capita expenditures, making it the largest safety net program by per capita spending at that time (Bitler and Hoynes, 2016). Several studies have added to a large body of work on the relationship between UI generosity and unemployment duration—i.e., the ex post moral hazard effect—by exploiting these plausibly exogenous changes in UI potential benefit duration (PBD) (Rothstein, 2011; Farber, Rothstein and Valletta, 2015; Farber and Valletta, 2015; Marinescu, 2017; Johnston and Mas, 2018).[1] Since UI benefit changes alter the expected cost of job loss, theory also predicts an *ex ante* moral hazard response to UI benefit changes among the employed. In a simple model where effort is costly but protective against job loss, workers will respond to an increase in UI generosity by exerting less effort (shirking).[2] Because it is difficult for an employer to differentiate shirking from poor performance, this ex ante moral hazard effect should exist even if shirking would disqualify a worker from receiving UI benefits.[3]

To date, only a handful of studies have tested this theoretical prediction. Burda, Genadek and Hamermesh (2020) use the American Time Use Survey (ATUS) to study the effect of unemployment rates on shirking, which they measure as time spent not working while at work. They also demonstrate that maximum and average UI benefit levels are correlated with the intensive margin of shirking, but they do not attempt to identify a causal effect. The only other empirical[4] paper on this question focuses on self-employed workers in Denmark, which has a relatively unique, voluntary UI system (Ejrnæs and Hochguertel, 2013).[5] They

[1]Prior studies on the ex post moral hazard effect of UI include Katz and Meyer (1990); Meyer (2002); Card et al. (2015). In a recent review of this large literature, Schmieder and Von Wachter (2016) report a median elasticity of unemployment duration with respect to potential benefit duration of 0.13.

[2]Since we do not model optimal worker effort from the perspective of either the firm or a social planner, we do not explicitly define shirking and we use the terms "a decrease in effort" and "an increase in shirking" interchangeably.

[3]We provide a theoretical basis for the predicted effects of UI on worker effort in Online Appendix 1. Goerke (2000) shows that this result relies on the assumption that a worker fired for shirking is relatively unlikely to have their benefits reduced. Later in the paper, we provide empirical evidence for this assumption through a series of supplemental analyses with administrative UI claims data. In brief, we show that a meaningful proportion of UI claimants come from workers who were fired, and that such claims are likely to be accepted. We also discuss how specific UI policies make it unlikely that a worker fired for shirking would be denied benefits.

[4]Theoretical work is more common. For example, ex ante moral hazard has been shown to be a source of market failure in (hypothetical) private UI markets (Chiu and Karni, 1998).

[5]Related empirical work shows that inflows into unemployment spike when UI eligibility is obtained (Christofides and McKenna, 1995, 1996; Green and Sargent, 1998; Rebollo-Sanz, 2012) and when benefit levels increase (Winter-Ebmer, 2003; Jäger, Schoefer and Zweimüller, 2018). Since shirking does not necessarily result in job loss, the importance of a shirking ex ante moral hazard effect includes, but is not limited to, an explanation of these results. Since employer responses to these benefit changes could potentially explain these spikes in inflows, these results do not necessarily imply an ex ante moral hazard effect.

exploit a policy change which differentially incentivized certain cohorts to enroll in UI and find that affected self-employed individuals were moderately more likely to become unemployed. No paper has yet identified the causal effects of UI generosity on worker effort in the context of a mandatory UI system such as exists in the US.[6]

This paper aims to fill this gap in the literature by matching plausibly exogenous changes in the PBD of UI benefits in the United States during the Great Recession with task-level productivity measures from a large sample of individual supermarket cashiers. The productivity measures are derived from high-frequency scanner data covering over 500,000 transactions conducted by nearly 2,000 cashiers spanning 39 grocery stores that are part of a national supermarket chain. The stores in our sample are located within a roughly 25 mile radius in the Washington D.C. metropolitan area, including eight stores in the District of Columbia, 17 stores in Maryland, and 14 stores in Virginia. During the sample time period (December 2008 to February 2011), changes in the parameters of the Extended Benefits (EB) and Emergency Unemployment Compensation (EUC) programs led to a series of large discrete increases in UI PBD. These extensions were designed as a response to the economic downturn. They were available to all UI eligible individuals and they differentially[7] affected the jurisdictions in our sample. Following recent studies on the effect of these extensions on job search (e.g. Farber and Valletta, 2015; Marinescu, 2017), we utilize this quasi-experimental cross-state variation in the size and timing of these expansions for identification.[8] Following earlier papers using nearly identical data (Mas and Moretti, 2009; Taylor, 2020), we measure productivity as the time-length of checkout transactions processed by cashiers.[9] These data and variation grant us the ability to estimate models with both cashier-register and day fixed effects while including transaction-level controls (e.g., number of items scanned by product category).

We provide several pieces of evidence that these PBD extensions were salient to workers similar to those in our sample. First, using Google Trends search data and nationally representative polls, we demonstrate that individuals in the US were likely to be aware of these extensions. Notably, we show that Google search frequencies for terms related to UI spiked dramatically on key PBD extension dates. Second, we demonstrate that the vast majority of a different sample of the retailer's cashiers had earnings histories that

[6] According to Schmieder and Von Wachter (2016), among OECD countries only Denmark and Finland have voluntary UI systems.

[7] Extensions varied across states in timing, magnitude, or both, depending on the specific extension.

[8] An important point detailed further in section 3.1 is that while the extensions were at times directly related to changes in state unemployment rates, the parameters of these programs also changed on several occasions during the Great Recession. We primarily rely on PBD changes that occur as a result of these federal and state policy changes which altered program parameters, as opposed to PBD changes that occur as a result of changes in unemployment rates.

[9] Investigating the impacts of discrimination in the workplace, Glover, Pallais and Pariente (2017) also observe worker productivity as measured by length of supermarket cashier transactions.

would make them UI eligible in our state-years.[10] Third, we point out that the relevant eligibility criteria for UI are unlikely to disqualify a claimant fired for low productivity. We also demonstrate that it is common for fired workers to receive UI benefits in the US and that such workers do not typically see their claims rejected. We therefore argue that the typical cashier in our sample is very likely to have been UI eligible in the event of a job loss resulting from shirking.

In our main results, we demonstrate a modest but statistically significant negative relationship between the PBD extensions and worker productivity. Specifically, we show that cashiers who experience increased PBD levels take longer to complete customer transactions. The effect is stronger for cashiers who work more shifts during the sample period, i.e. cashiers who are more likely to be UI eligible. The effect is also more prevalent for less productive cashiers, who are likely closer to the margin of being terminated for poor performance. In our preferred specification, we predict an average increase of 2.4 seconds in transaction length for cashiers who experienced an 18-week increase in PBD.[11] With a mean transaction length of just under two minutes, this is roughly equivalent to a two percent decrease in worker productivity.[12] Over time these effect sizes can accumulate into rather large losses. Back-of-the-envelope calculations similar to those in Mas and Moretti (2009) and Taylor (2020) suggest that stores would need to staff 144 additional hours per year to offset the loss in productivity associated with an 18 week increase in PBD levels.[13] Assuming a $10 median hourly wage for cashiers in the US, this would cost each store in our sample $1,440 per year in additional wages, for an estimated total cost of $4 million per year across our sample states.

We are able to rule out several potential confounds. First, by estimating models with cashier fixed effects, we rely strictly on cashiers who experienced varying levels of PBD; this addresses concerns regarding changes in cashier composition in response to increases in PBD levels.[14] Similarly, register fixed effects account for potential shifts in the use of different registers (e.g., express registers). We also examine whether changes in consumer behavior related to PBD expansions drive the results. We find little evidence that consumers' purchases respond to PBD levels, with no statistically significant changes in expenditures and items per transaction or in the use of price discounts and coupons. We bring in additional data to examine

[10] These data are from Mas and Moretti (2009) and include cashiers working in a different state several years prior to our sample. Since these data include all transactions occurring at several stores, we can estimate hours worked and UI eligibility at the cashier-shift level. This is not possible in our data.

[11] The average change in PBD across states per change in our time frame was 18.6 weeks, with each state experiencing two separate extensions. From the beginning to the end of our sample, Washington D.C. and Virginia experienced a total increase of 40 weeks in PBD, while Maryland experienced a 27 week increase.

[12] With a sample standard deviation just over 100 seconds, this effect is also roughly equivalent to a 2.3 percent standard deviation decrease in worker productivity.

[13] Additional details on these calculations are provided in the results section of the paper (section 4.2).

[14] We also find cashier experience is uncorrelated with PBD levels, suggesting managers did not significantly change the employment of their cashiers through our period.

aggregate store-level sales and find that sales significantly decrease with the unemployment rate but remain the same, or slightly increase, with PBD expansions. Finally, we find no statistically significant relationship between PBD levels and local unemployment rates, which highlights the discrete nature of the PBD changes that occurred during our period. Importantly, we estimate specifications of our main model controlling for the aforementioned potential confounds and find that they do not change our results.

To investigate whether our results may be generalizable to other sectors and regions of the US, we combine our identification strategy with a shirking measure used by Burda, Genadek and Hamermesh (2020). Specifically, we use the ATUS to test for an effect of increased PBD levels on the self-reported percentage of time spent on non-work activities while at work. This is made possible by the unique detail of the ATUS—a repeated cross-sectional survey which measures time spent doing precise activities (e.g., eating, childcare, socializing) and the location where those activities are performed. Utilizing PBD changes across the US from 2003 to 2014, and estimating models with state and month-year fixed effects, we estimate a positive effect of the PBD of UI on shirking in the ATUS sample. For our fully specified model, off a mean of 6.68%, we estimate a 0.35 percentage point increase in time spent at work not working in response to an 18-week increase in PBD. This analysis suggests that shirking responses to these benefit changes were not limited to our sample of cashiers.

Our results offer several important contributions to the limited empirical literature on UI's ex ante moral hazard effect. They constitute, to our knowledge, the first quasi-experimental evidence of such an effect *either* among the non-self-employed or within a mandatory UI system such as those utilized by nearly all developed countries. These results have several important implications. First, they quantify an understudied margin through which changes to UI benefit generosity affect social welfare (Baily, 1978; Chetty, 2006). Second, they contribute to the relatively small base of empirical evidence on ex ante moral hazard effects in *any* type of insurance.[15] Third, we contribute to the wider literature on the determinants of worker effort (e.g., the efficiency wage literature) by providing new estimates for two important theoretical predictions (the effects of the unemployment rate and unemployment benefits on effort) (Shapiro and Stiglitz, 1984; Lazear, Shaw and Stanton, 2016). Our results provide evidence that worker effort varies over both the business cycle and corresponding policy response, rising with the unemployment rate[16] and falling with UI generosity.

[15] Clear empirical evidence exists for automobile and workers compensation insurance (Cohen and Dehejia, 2004; Fortin and Lanoie, 2000). Mixed evidence exists in the case of health insurance (Newhouse and Group, 1993; Decker, 2005; Dave and Kaestner, 2009). Hansen, Nguyen and Waddell (2017) document increases in injury length and subsequent take-up of workers compensation in response to increased benefits.

[16] We jointly estimate the impact of both local (county or ward) and state unemployment rates and find that cashier effort rises with both of these.

The remaining sections of this paper are as follows. Section 2 presents the data. Section 3 describes the quasi-experimental variation in UI benefits that we exploit and demonstrates the relevance of these benefit extensions for the workers in our samples. Section 4 presents our empirical specification and main results. Section 5 discusses potential threats to our identification strategy and conducts several robustness checks and placebo tests. Finally, section 6 replicates the analysis with the ATUS data and section 7 concludes.

2 Data Sources and Summary Statistics

Quantifying a change in cashier productivity requires a detailed dataset on the speed of checkout transactions linked to cashiers. To this end, we obtained access to proprietary scanner data from a large supermarket chain for 39 stores in the District of Columbia (DC) metropolitan area (a roughly 25 mile radius around DC), including 8 stores in DC, 17 stores in Maryland, and 14 stores in Virginia.[17,18] These data—which span from December 2008 until February 2011—were originally obtained by Taylor (2020) to study how the 2010 disposable carryout bag tax in DC affected the transaction time of supermarket checkout.[19] These data are ideal for our research question for two reasons. First, during the time period of these data, a series of discrete changes to the PBD of UI benefits occurred which varied across DC, Maryland, and Virginia. Second, the richness of the data allows us to construct measures of cashier productivity, following other studies using the same data source for different purposes (e.g., Mas and Moretti, 2009; Taylor, 2020). Specifically, for each checkout transaction in our sample, we have information on when and where a checkout transaction occurred (e.g., register 4 in store G and state S on Saturday, June 19, 2010 at 5:37pm), what was purchased (e.g., a gallon of milk costing $3.06), which cashier processed the transaction, and importantly, how much time the transaction took to complete. Using these identifiers, we are able to track stores and cashiers over time, before and after the changes in the PBD of UI benefits.

Our main outcome variable is *transaction time*—the duration of each checkout transaction measured in seconds, from the start of a transaction until the start of the next transaction in line. We are able to construct this variable using the transaction time-stamp, which includes the day, hour, and minute each transaction was completed. The sample includes all transactions at cashier-operated registers in the 39 stores between 5:00pm and 6:00pm for every Saturday during the roughly two year period. This weekend hour was chosen because the retailer cited it as a peak shopping time in their stores.[20] Since there is only one time-stamp per

[17]We do not include stores that were remodeled to add self-checkout registers during the sample, as self-checkout adoption could confound our productivity results.

[18]Online Appendix Figure A1 presents a stylized map of the Washington DC Metropolitan area.

[19]The SIEPR-Giannini Data Center archives and documents existing datasets from this supermarket chain.

[20]We drop transactions occurring at self-checkout registers because only seven stores have self-checkout during the sample

transaction, having peak hours enables us to calculate transaction time under the assumption that transactions in the scanner data occur back-to-back, with little or no downtime in between. Taylor (2020) verifies this assumption using observational data collected in-store during peak hours, where transaction length is timed with a stopwatch by enumerators stationed near checkout.[21] The advantage of using one time-stamp—and thus the full duration from one transaction to the next—is that all actions a cashier takes before swiping the first item (e.g., starting the conveyor belt) and all the actions after finalizing the purchase (e.g., printing the receipt and handing it to the customer) are included in our productivity measure. Downtime, on the other hand, refers to when there are no customers at the registers or in line, which is unlikely to occur during peak hours.[22]

2.1 Summary statistics for scanner data

Table 1 presents the average transaction, cashier, and store characteristics, by state (columns 1–3) and for the full sample (column 4). Starting in panel A, there are 515,636 transactions in the Saturday 5:00-6:00pm sample. The average transaction has an approximate duration of 120 seconds, is comprised of 12 items, and costs \$35.[23] Average transactions in DC stores take slightly longer to complete yet have roughly the same size and cost as stores in Maryland and Virginia. The average transaction saves roughly \$8 in price discounts. Price discounts occur when stores put items on sale, or when customers use their rewards card or coupons.[24]

Panel B of Table 1 presents average characteristics at the cashier-level. There are 1,984 unique cashiers in our sample. The average cashier works 13.5 of the 113 Saturdays in our sample. The average span of days from when we first see a cashier until we last see them is 230 days, suggesting cashier is a position with high turnover. The average cashier works 40 minutes per hour and the median cashier works 48 minutes per hour—this being less than 60 minutes reflects cashiers entering/exiting their shift within the hour. Panel C presents average store-level characteristics. Stores in DC have more registers and more cashiers than stores in Maryland and Virginia. In the full sample, the average store has 7 registers and 51 cashiers.

Figure 1 plots the relationship between the number of items in a transaction and average transaction time.

period, and these registers are not manned by an individual cashier.

[21]To additionally ensure that transactions occur back-to-back, we also drop all transactions that are more than three standard deviations longer than the average transaction of its size (in terms of number of items scanned) and all transactions that are longer than 20 minutes.

[22]Saturday from 5:00–6:00pm is not the only peak foot-traffic hour in a week; however, due to size constraints in obtaining data from the retailer at the transaction level, the original data request was limited to one hour per week for the sample of stores.

[23]Transaction expenditure is created by summing up the individual amounts paid per item in a transaction. This variable does not include sales tax.

[24]Price discounts are measured as the difference between the shelf price of an item and the price customers pay, summed across all items in a transaction.

This figure reveals that the fixed time cost of processing a transaction is a minute, with the average one-item transaction taking 60 seconds to complete. Furthermore, the variable time cost is linear in transaction size, with each additional item adding roughly 3 seconds to the baseline transaction time.

2.2 Additional data - American Time Use Survey (ATUS)

In additional analyses, we utilize the American Time Use Survey (ATUS). The primary benefit of the ATUS is that we can measure on-the-job effort for workers across different sectors and for the entire US. The ATUS is a repeated cross sectional survey of former Current Population Survey (CPS) respondents which elicits time diaries of individuals. The time diaries collect detailed information on the nature of activities, the duration of activities (in minutes), and the location of the activities (e.g., at the workplace). A total of 159,937 surveys were conducted between 2003 and 2014.

Our measure of effort in the ATUS closely follows that of Burda, Genadek and Hamermesh (2020), who investigate the relationship between shirking and state unemployment rates in the ATUS. Among activities conducted "at the workplace" by a sample of adult workers in the ATUS,[25] we identify work-related activities using ATUS activity codes 50000 to 50299. We then reclassify "socializing, relaxing, leisure, eating, drinking, sports, exercise as part of the job" as non-work (codes 50201-50203), "travel related to work" as work (codes 180501, 180502, 180599), and "work and work-related activities not elsewhere classified (n.e.c.)" (code 59999) as work. For our primary dependent variable, we calculate each individual's percentage of time at the workplace that they engaged in non-work activities.

Online Appendix Table A1 presents summary statistics for our ATUS sample of 30,094 workers.[26] The average worker in our sample is 40 years old. Approximately 46% of workers are female, 83% are white, 92% are born in the US, 83% work in the private sector, 12% work part time, and 45% are paid hourly. The three most popular occupational sectors are management (11%), sales (10%), and office and administrative support (15%). Respondents report working an average of 42 hours per week, with weekly earnings of $900. For the day that the worker was surveyed, respondents spent an average of 479 minutes (approximately eight hours) working, and over 31 minutes not working while at the workplace.

[25] Specifically, we limit the sample to US citizens with a single job, who reported "usually" working at least 20 hours per week, are 18-65 years old, and are not self-employed (self-employed workers are ineligible for UI).

[26] These statistics use the probability weights provided.

3 UI Benefit Extensions, Awareness, and Eligibility

In this section, we first detail the expansions in Unemployment Insurance (UI) potential benefit duration (PBD) during the period of our study. We then provide evidence for two key prerequisites for the effect of interest. First, we show that awareness of the PBD extensions was widespread. Second, we show that workers similar to those in our data were likely to be eligible for UI benefits.

3.1 Unemployment Insurance benefit extensions

In the US, the PBD of UI benefits is set by individual states.[27] Typically limited to 26 weeks,[28] PBDs are regularly extended during economic downturns. During the period of our study these extensions were driven by three separate programs—the Extended Benefits (EB) program (1970-current), the Emergency Unemployment Compensation (EUC) program (7/2008-12/2013), and the Temporary Extension of Unemployment Compensation (TEUC) program (3/2002-12/2003).[29] The exact number of additional weeks available to UI claimants in each state by the EB and EUC programs are made available online at the weekly level by the US Department of Labor (DOL), Office of Unemployment Insurance.

Table 2 presents the changes in PBD due to these programs for the state-weeks in our scanner data sample and Figure 2 depicts similar information for the state-months in our ATUS analysis.[30] The PBDs shown are those available to new claims filed on a given date. Since the timing and magnitude of these extensions varied by state, PBD variation resulting from the EB, EUC, and TEUC programs has been utilized as identifying variation in a series of recent studies on the effects of UI benefit generosity (Farber, Rothstein and Valletta, 2015; Rothstein, 2011; Marinescu, 2017; Farber and Valletta, 2015; Boone et al., 2016; Chodorow-Reich, Coglianese and Karabarbounis, 2019). More detailed descriptions of these programs and related legislation can be found in these studies and in Online Appendix 2.

Here we emphasize the key point that the extensions which we exploit for identifying variation occur for one of three reasons: (1) a state's unemployment rate (specifically its average Insured Unemployment Rate (IUR) over the past 13-weeks or Total Unemployment Rate (TUR) over the past 3-months) crosses a threshold or "trigger" value currently in place, (2) the relevant authority (state government for EB, federal for EUC or TEUC) changes the trigger value to a level below the state's current 13-week IUR or 3-month

[27] PBD is determined by the state of employment, not residence.

[28] With the exception of Montana and Massachusetts, which provide 28 and 30 weeks, respectively.

[29] The EB and EUC programs are relevant for both the scanner data and ATUS analyses, whereas the TEUC program is relevant only for the ATUS analysis.

[30] Data for the ATUS sample extensions were obtained from a replication file for Farber, Rothstein and Valletta (2015). Note that, although the analyses in Farber, Rothstein and Valletta (2015) are limited to the years 2008-2014, the replication file includes extension on/off dates starting in January 2000.

TUR, or (3) the federal government alters the (EUC or TEUC) program by changing the number of weeks available or allowing the program to expire (either temporarily or permanently). Notably, only 2 of 7 separate PBD changes exploited in our scanner data sample (shown in Table 2) occurred as a direct result of changes to the relevant state's unemployment rate.[31] The remaining extensions occurred due to policy changes implemented at the state or federal level. This is important to note because extensions which are a direct result of changes in state unemployment rates may be problematic for our design, as further discussed in section 5.1.2.

Between 4/2011 and 8/2014 the states of Arkansas, Florida, Georgia, Illinois, Michigan, Missouri, North Carolina, and South Carolina each passed legislation reducing their regular PBDs below 26 weeks (Isaacs, 2019). This variation is not relevant for our scanner data sample but is utilized in our ATUS analyses. The specific dates and magnitudes of these reductions are also explained in more detail in Online Appendix 2.

3.2 Awareness and Relevance of UI benefit extensions

To investigate the possibility that workers were unaware of the extensions that we study (which would preclude our effect of interest), we turn to Google Trends to look at search frequency of the terms "Unemployment benefits" and "Emergency Unemployment Compensation" across the US on Google's search bar. Google Trends reports the relative search frequency of particular items on Google Search within a queried geography (e.g., the US) and time period (e.g., January 2008 - December 2009), indexed to a range of 0 and 100.

Figure 3 plots these trends. For the search item "Unemployment benefits," there are three jumps in search frequency corresponding to enactment (June 30, 2008) and subsequent adjustment (November 21, 2008 and November 6, 2009) of the Emergency Unemployment Compensation (EUC) program; note, however, the search frequency for "Unemployment benefits" was highest, within this period, during the time when the American Recovery and Reinvestment Act (ARRA) was implemented. In the second panel of Figure 3, we report the trends for the search item "Emergency Unemployment Compensation." Though noisier, we find that across the sample of 104 weeks, search frequency was at its highest during the weeks after the EUC program enactment and two subsequent alterations. In fact, the two weeks of the EUC alterations produced the two highest search volumes for EUC within our sample, while the week after the enactment of the EUC program carried the fourth largest search volume. Though these results do not reflect *absolute* search volumes, the relative spikes in search volume reflect, among Google Search users, an awareness of the EUC program enactment and expansions.

[31] This ignores three EUC program lapses shown in Table 2 which also do not result from changes in unemployment rates.

The online appendices provide further evidence of both UI benefit awareness and the relevance of PBD extensions to workers during the Great Recession. In Online Appendix 3, we use Google Trends to show that "Unemployment benefits" was also a popular search relative to several other common search terms during this time period—such as "Social security," "Disneyland," and "Wall-E." This appendix additionally provides evidence on UI awareness from national polls conducted during the Great Recession. In Online Appendix 4, we use the CPS to provide evidence that unemployment spells often extended into and beyond periods covered by the PBD expansions and thus are sufficiently long to be relevant to the workers in our data.

3.3 UI eligibility

UI eligibility is determined by two sets of criteria. Monetary eligibility requires a minimum amount of earnings in a certain time period pre-claim, and non-monetary eligibility conditions UI benefits on the reason for the job loss. In this section we provide supporting evidence that our workers are likely to meet these criteria in the event of a shirking-induced job loss.

Mas and Moretti (2009) utilize a different sample of our retailer's cashiers, covering a two-year period for six stores between 2003 and 2006.[19] Since their data includes all transactions occurring at these stores, we can use it to estimate monetary eligibility. To do so, we assume that the retailer is each cashier's only employer and that each cashier was paid the relevant minimum wage. We apply the UI eligibility rules for each state-year in our sample to each cashier-shift in theirs and find an average eligibility rate of 74%. Online Appendix Table A2 presents the results from this analysis.

The key non-monetary criteria in all states is that the worker lost their job through no fault of their own. A concern in our setting is that employees who are fired for shirking may be "at fault." However, in practice a discharged claimant is only at fault if they have committed misconduct. Legal definitions of misconduct vary by state, but according to the DOL's Employment and Training Administration (2019), many states rely on the definition established in the 1941 Wisconsin Supreme Court Case, Boynton Cab Co. v. Neubeck: *"Misconduct...is limited to conduct evincing such willful or wanton disregard of standards of behavior which the employer has the right to expect of his employee, or in carelessness or negligence of such degree as to manifest an equal culpability, wrongful intent or evil design, or to show an intentional and substantial disregard of the employer's interest or of the employee's duties and obligations to his employer."*

Importantly, the burden of proof for an allegation of misconduct is on the employer. As Hagedorn et al. (2013) discuss in detail, attempting to prove misconduct is costly for employers and the probability of success is low. Especially relevant for our setting, Hagedorn et al. (2013) point out that an employer must prove "willfulness" on the part of the claimant in order to demonstrate misconduct. So long as worker effort is imperfectly observable by employers,[32] this standard seems unlikely to be met in the case of a worker laid off for decreasing their on the job effort in response to an improvement in their outside option.[33] Various public resources provided by employment law specialists reach similar conclusions. For example, Nolo (a leading provider of do-it-yourself legal advice) states that "An employee who is fired for being a poor fit for the job, lacking the necessary skills for the position, or failing to perform up to expected standards will likely be able to collect unemployment."[34]

We support these arguments empirically in three ways. First, using administrative claim-level data from the State of California's Employment Development Department (EDD), we find that approximately 10% of claimants in California were fired from their previous job. This share rises to 20-25% among cashiers and/or food store workers. Second, using the DOL's Benefit Accuracy Measurement (BAM) data we show that the proportion of UI claimants who were fired during the time period of our ATUS analysis was around 20% nationally. Third, we utilize the DOL Employment and Training Administration's quarterly report number 207 to document variation across states in the fraction of misconduct determinations resulting in a denial of benefits. Consistently from 2003 to 2014, in the median state, roughly 40% of such determinations result in denials; many of these denials only partially reduce benefits and many of these misconduct determinations are unrelated to shirking. Together these three results strongly suggest that a monetarily eligible worker who is fired for shirking is likely to receive UI benefits.[35]

Finally, performance need not be the direct reason for a worker's termination to still impact their probability of termination. Most obviously, a downsizing firm may first decide to terminate their lowest productivity workers. In our setting, it is feasible that the supermarkets first terminated their slowest cashiers in the event that they had to downside during the Great Recession. Later results will investigate differential shirking responses by cashier productivity to find that lower productivity cashiers (i.e. those most likely on the margin for termination) responded more strongly to changes in UI PBD.

[32]This is a common assumption in efficiency wage models (e.g., Shapiro and Stiglitz, 1984).

[33]We direct the interested reader to Appendix VI in Hagedorn et al. (2013) for detailed descriptions of specific related cases in the context of California's UI program.

[34]Similarly, in their definition of misconduct, the National Employment Law Project states that "Neither workers fired simply for poor performance nor capriciously fired workers should be denied UI benefits" (National Employment Law Project, 2015).

[35]Plotted figures from these three datasets can be found in Online Appendix Figure A3. EDD is the state agency which administers UI in CA, and data were acquired through a partnership between the EDD and the California Policy Lab. The BAM program, run by the DOL's Employment and Training Administration, audits a random sample of UI benefit recipients in every state-week, and their data are publicly available.

4 Main results

4.1 Econometric specification

Our primary specification estimates the following equation:

$$\text{TransactionLength}_{tdcrs} = \beta \text{UIPBD}_{ds} + \lambda_d + \lambda_{crs} + \gamma X_{tdcrs} + u_{tdcrs} \tag{1}$$

where $\text{TransactionLength}_{tdcrs}$ is the length of transaction t performed on day d (e.g., February 12, 2010) by cashier-register cr (e.g., Cashier ID #456 working checkout line #5) in state s (e.g., Virginia), UIPBD_{ds} is the maximum PBD of UI benefits available in state s on day d, λ_d and λ_{crs} are day and cashier-register fixed effects, and X_{tdcrs} is a vector of transaction-level controls. The coefficient β can be interpreted as the predicted increase in transaction length (in seconds) in response to a one-week increase in the UI benefit duration. A positive β would imply that cashiers shirk in response to more generous benefits.

Cashier-register fixed effects denoted by λ_{crs} control for all unobserved factors that vary at the cashier-register level and affect transaction time. Importantly, since cashier-register fixed effects strictly rely on variation within cashiers, our identification strategy accounts for the possibility that the composition of employed cashiers changed with PBD. For example, without cashier fixed effects, our estimates would be biased away from a shirking effect if workers employed during higher levels of PBD were more productive. Register fixed effects account for any differences in processing speeds across registers (e.g., express lane vs. regular). By estimating cashier-register fixed effects, we account for any potential cashier-specific differences across registers; for instance, some cashiers may work more quickly on express lanes compared to other cashiers. Transaction-level controls X_{tdcrs} include: the total expenditures paid in dollars, the total number of items purchased, price discounts and coupons in dollars, indicators for whether the transaction included items from particular departments (e.g., alcohol), an indicator for whether a plastic bag tax was in place at the store, number of households participating in SNAP per state-month, the cashier's experience as measured by total number of transactions completed up to that point in the sample, the cashier's "fatigue" as measured by the number of transactions the cashier had previously completed on that shift, the cashier's length of shift measured in both number of transactions and in minutes, prior month local unemployment rates—at the county level for MD and VA (from the BLS) and at the ward level for DC (from the DC Department of Employment Services)—and state unemployment rates (TURs, from the BLS).

4.2 Results

Table 3 presents results from four different versions of our baseline model (equation 1), all estimated via OLS with cluster-robust standard errors at the state-by-week level. Across all specifications, we estimate a significant positive effect of PBD on transaction duration. Notably, the inclusion of the transaction-level control variables X_{tdcrs} does not greatly alter our coefficient of interest. In our preferred specification (column 4), we estimate a 0.130 second increase in transaction length for a one-week increase in PBD.

In column 5, we replicate column 4 with the inclusion of customer-card fixed effects. At this supermarket chain, customers may opt to use a customer rewards card. If a customer uses their rewards card we can follow them across multiple transactions. Approximately 70% of transaction are carried out by rewards card members. Since this approach relies on rewards card members who shopped on at least two separate Saturdays from 5-6pm with differing PBDs, we do not prefer it to column 4. Still, it is reassuring that our estimate for β in this model (0.136) is very close to that from our fully-specified model (0.130).

The average PBD extension in our sample is just over 18 weeks. With an 18-week extension, these estimates correspond to an increase in transaction duration between 2.4 and 2.7 seconds, or roughly 2% of the overall sample mean. Using a back-of-the-envelope calculation, stores would need to staff 144 additional hours of work to maintain the same level of store productivity as before the PBD extension.[36] With a $10 median hourly wage for cashiers in the US (BLS, 2017*b*), this would cost the stores in our sample $1,440 higher wage bills per year. Aggregating further, if the 39 supermarkets in our sample are representative of the 2,891 supermarkets and other grocery stores in DC, Maryland, and Virginia,[37] the PBD extension would cost supermarkets in these states $4 million per year collectively. The estimated coefficients on the size of the transaction provide additional context. A typical PBD extension in our sample increases the transaction durations of affected cashiers by a magnitude roughly equivalent to increasing the size of the transaction by 0.8 items (7% of the sample mean transaction size).

Coefficients on the state and local unemployment rate are negative and are only statistically significant, at the 10% level, in our customer fixed effects model. This result suggests that a cashier's direct response to the increasing unemployment rates is to boost effort and productivity (i.e. it works against our estimated effect of PBD). Coefficients on the local unemployment rate (county-level in Maryland and Virginia, ward-

[36]On average, stores in our sample process 130 transactions per Saturday 5:00pm hour before the PBD extension. To maintain this level of production when the average transaction is 2.4 seconds slower, stores would need 312 seconds more work per hour (i.e., open up an additional register for 312 seconds). This is only for one hour per week. To aggregate this to the annual level, we use an industry white paper which finds that half of grocery shopping transactions in the US occur during 32 peak hours in a week (Goodman, 2008), where a peak hour is defined as a time wherein more than 3 million people shop during that hour of the week. This translates to stores needing to staff 144 additional hours per year than before the PBD extension (i.e, 312 seconds $\times$ 32 peak hours per week $\times$ 52 weeks in a year $\div$ 3600 seconds per hour).

[37]Store counts come from the US Census Bureau, 2017 Retail Trade Summary Statistics.

level in D.C.) are much smaller than those for the state.

4.3 Subsamples

To understand heterogeneity in the effect of PBD on transaction duration, in Table 4 we estimate equation (1) for several subgroups. These subgroups are defined based on cashier or register characteristics which are expected to alter the effects of PBD on worker effort.

The first split is defined by a measure of cashier experience: the number of shifts the cashier worked in our sample before the first PBD change (i.e., before April 5, 2009). As described in section 3.3, more experienced cashiers are more likely to be UI eligible. Results in Table 4 suggest a slightly stronger treatment effect for such cashiers. In a subsample of cashiers who worked more than the top quartile of shifts in our sample, the predicted effect of PBD on transaction duration is 25% larger relative to the full sample while the treatment effect for the lower quartiles is in line with the full sample. Estimates remain statistically significant for both of these subsamples.[38]

Next, we consider heterogeneity by cashier productivity. Since productivity is imperfectly observable by managers and unproductive workers are likely closer to the margin of being terminated, the effort decisions of less productive workers are expected to be more responsive to changes in PBD levels. Conversely, it is relatively unlikely that highly productive workers would be terminated for a drop in performance, and thus, these workers would be less responsive. To test this, we first estimate each individual cashier's fixed effect in the pre-policy period (pre-April 5, 2009), conditioning on transaction-level controls and day fixed effects.[39] We then estimate our preferred specification separately among productive ($\geq$75% percentile of productivity) and less productive cashiers. Our primary cashier productivity split is presented across the fourth and fifth columns of Table 4. We find virtually no treatment effect for high-productivity cashiers, while less productive cashiers display a statistically significant increase in transaction length during periods with higher PBD levels.[40]

Lastly, we consider a set of subgroups defined by the type of register that was used to conduct the

[38]Online Appendix Figure A5a demonstrates this pattern of results visually. As the minimum number of shifts worked grows beyond 8 shifts (i.e., the top quartile of shifts worked before the first PBD change), point estimates increase. However, even though most point estimates are statistically significantly different from zero, we cannot conclude that the difference in treatment effects between high and low experience cashiers is statistically significant.

[39]Transaction level controls include the number and types of items scanned and the register worked.

[40]Note that the number of observations across these two subgroups do not sum to the full sample since new cashiers enter the sample after the first PBD change on April 5, 2009. Approximately 38% of the full sample of transactions were conducted by cashiers that did not work before the first PBD change. Online Appendix Figure A5b tests the sensitivity of this split by plotting estimated treatment effects across an array of subsamples by cashiers' ranked productivity, starting with the full sample on the left and the most productive cashiers on the right (culminating with a subsample limited to cashiers ranked 600 and above). Once again, we observe no treatment effects when we focus strictly on the most productive workers. More generally, point estimates decreases as we move from the full sample on the left to the most productive workers on the right.

transaction—express vs. regular. With smaller transactions and more time sensitive customers, one may expect cashiers working at express registers to have little opportunity to shirk. Conversely, larger transactions conducted on regular registers plausibly present more of an opportunity for shirking. The results from the last two columns of Table 4 are consistent with this hypothesis. For transactions conducted on express registers, the estimated effect is nearly zero. At regular registers, the treatment effect is larger than in the full sample, with an 18-week extension translating to a statistically significant 4.6 second increase in transaction length.[41]

5 Threats to identification, robustness checks, and placebo tests

5.1 Threats to identification

In our two-way fixed effect specification, potential sources of endogeneity bias in estimates of β are from omitted time-varying factors correlated with both PBD and transaction length. Below, we address three key examples: Changes in consumer purchases/composition, time-varying changes to local labor market or cashier characteristics, and other policy changes that may influence transaction length.

5.1.1 Changes in consumer purchases

Since higher levels of PBD are (partly) triggered by higher unemployment rates, they may be associated with changes to consumer purchasing behavior. This is a concern in our setting if such changes also affect transaction length. During downturns, consumers might make fewer trips to the grocery store while purchasing more goods during each visit. Consumers may also increase their "price-consciousness" during higher PBD periods, seeking out coupons and price discounts.[42] More generally, consumers might buy more of certain types of goods during high PBD periods which take longer to scan. Such responses are not driven by cashier behavior but could lead to increases in transaction length. In light of these concerns, it is important to note that our preferred specification controls for the number, price, and types of items purchased by consumers. While we find that these controls do not greatly change our coefficient of interest (as shown in Table 3), we provide supplementary evidence against this potential bias in this section.

We begin by analyzing how total supermarket sales vary with UI benefit generosity. While studies have shown that food expenditures fell during the Great Recession (Kumcu and Kaufman, 2011; Griffith,

[41] Ignoring register type, Online Appendix Figure A6 demonstrates a consistent pattern of increasing treatment effects across larger transactions. For the full sample, we estimate a treatment effect of 0.13, and as we reduce the sample to include only larger transactions, this estimate slowly increases, culminating in a treatment effect over 0.4 seconds for transactions with at least 20 items.

[42] Nevo and Wong (2019) find that households purchased more on sale and used more coupons during the Great Recession.

O'Connell and Smith, 2016) and that food consumption falls at UI benefit exhaustion (Ganong and Noel, 2019) (suggesting that PBD increases would increase food consumption among benefit exhaustees), no study that we are aware of has examined how total supermarket sales vary with PBD extensions. To address this open question, we use retail scanner data collected by Nielsen© and made available through the Kilts Center at The University of Chicago Booth School of Business. The retail scanner data consist of weekly price and quantity information generated by point-of-sale systems for nearly 40,000 participating grocery, drug, and mass merchandiser stores across the US. Nielsen's sample of stores cover more than half the total sales volume of US grocery and drug stores and more than 30 percent of all US mass merchandiser sales volume. We use the data spanning from January 2009 through December 2014.

We aggregate the raw micro data—i.e., store j sold x units of product z in week w, where a product is represented by a universal product code (UPC)—to the store-month level for three variables: (1) total sales in dollars, (2) total sales in the number of items sold, and (3) average expenditures per item.[43] The average store in 2009 had monthly sales equal to 183,808 items and $541,605 and average expenditures per item equal to $3.21. In Table 5, we test how store sales correlate with state PBD levels and lagged unemployment rates. In the simplest specifications with only PBD level, store fixed effects, and month-year fixed effects, we find no statistically significant relationship between PBD and total store sales (columns 1, 3, and 5). When we add lagged state UE rates, we find a positive and significant relationship between PBD level and sales measured in dollars (column 2) and a positive but not significant relationship with sales measure in items (column 4). These effect sizes are quite small, equal to 0.03% and 0.01%, respectively. Moreover, similar to the prior literature, we find a negative relationship between the unemployment rate and store sales. This effect size is much larger, with a 1 percentage point increase in the prior month's unemployment rate corresponding to a 1% decrease in sales (both in dollars and in items sold). Lastly, we find a tiny but statistically significant relationship between the amount spent per item and PBD, with an 18-week increase in PBD duration corresponding to 0.03% increase in the amount spent per item (column 6). Overall, these results suggest store sales decrease in economic downturns and remain the same, or slightly increase, when UI benefit generosity increases.

Returning to our primary transaction-level scanner data, in Table 6, we test for each of the considerations involving changes in consumer behavior by collapsing our data to the store-date level and regressing a series of characteristics on PBD, conditional on date and store fixed effects. From the first two cells, we immediately see that consumers are not buying more per visit during periods with higher PBD levels, both in terms of total dollars and number of items. The coefficients on expenditures per transaction and items scanned per

[43] The average expenditures per item is calculated as total sales in dollars divided by total sales in the number of items sold.

transaction are both statistically insignificant. Moreover, we find no statistically significant changes in the usage of price discounts during higher PBD periods. This is reassuring because we might expect transactions using price discounts, such as coupons and reward cards, to take longer to process. Therefore, while customer coupon- and sale-use may have been higher during the Great Recession (Nevo and Wong, 2019), we do not find that these behaviors responded to changes in PBD during the Great Recession.

We also do not find statistically significant changes in the number of registers opened during higher PBD periods. This is reassuring because, with only one time-stamp per transaction, we assume that the time between customer transactions is unchanging. If PBD extensions led to fewer customers, stores might reduce the number of lanes open to avoid register idle time. However, we do not find a reduction in the number of registers open, which we would expect if stores were experiencing fewer customers. Unreported in this manuscript, we also estimate a hazard model to test whether higher PBD periods decrease the probability that a customer returns to a store in subsequent weeks, given they have yet to have returned to the store. We find no statistically significant relationship between PBD extensions and the likelihood customers return, suggesting once again that PBD extensions do not lead to lower customer volume.

Finally, when we investigate by product category (Alcohol/Tobacco, Bakery/Deli, Dairy, Floral, Frozen Items, Meat/Seafood, Produce), we do not find statistically significant changes in the purchases of a particular type of products, with the exception of alcohol and tobacco which is positively correlated with higher PBD periods. Given alcohol and tobacco purchases are associated with slower checkout speeds—as cashiers must check the identification cards of the purchasers—absent the controls we include for purchases of alcohol and tobacco, our estimates for β would be biased in the direction of a shirking effect.

There are also important consumer behaviors that we cannot measure in our data and may be a concern for our identification strategy. First, we are unable to measure payment method, which could be correlated with both PBD and transaction length. Hurd and Rohwedder (2010) find that the ownership of credit cards declined by 2.8% of households during the Great Recession. Polasik et al. (2012) show that cash is a significantly faster payment method than traditional payment card, but that contactless cards and mobile payments have similar time efficiency to cash. Thus, while we cannot measure changes in payment methods directly, the literature suggests slower forms of payment were used less often during the Great Recession.

5.1.2 Changes in time-varying labor market conditions and cashier characteristics

Since our identification strategy utilizes variation within cashiers (and across days) with cashier-register fixed effects, our estimates will only be biased in response to cashiers if there are any time-varying cashier characteristics that are associated with PBD and transaction length. A key concern is that cashier effort responds directly to state unemployment rates. We first note that theory clearly suggests that this response would move in the opposite direction of our effect of interest. A weaker labor market implies that the costs of job loss are higher and workers are expected to respond by increasing their effort on-the-job.

We show in Table 6 that lagged state unemployment rates do not display a statistically significant relationship with PBD levels. This is perhaps not surprising given that (as discussed in section 3.1) most PBD extensions occurring in our scanner data sample did not result from changes in state unemployment rates. Further, as described in Online Appendix 2, when state unemployment rates are relevant, the specific rates that matter are both measured over longer time frames (e.g., 13 weeks) and in more complicated ways (e.g., benchmarking relative to similar rates in prior calendar years via "lookback" provisions). Finally, we further addressed concerns related to the correlation between PBD levels and unemployment rates by controlling for both state and local unemployment rates[44] in our preferred specification in Table 3.

A separate possibility is that cashier shift length changes with PBD levels. This would bias our estimates away from zero if cashiers work longer shifts during higher PBD levels and longer shifts correspond to reduced productivity. (e.g., if less productive cashiers were laid off, while more productive cashiers were given longer shifts.) Cashier fixed effects control for the change in the composition of cashiers, but they do not account for the possibility of increased shift length within cashiers. Since our sample contains all transactions during a single hour on Saturdays, we cannot directly test for changes in shift length. However, we proxy for this using the average number of open registers, the average experience of employed cashiers, and the number of employed cashiers. From Table 6, we find no statistically significant relationship between PBD levels and these three covariates, suggesting there was little response from labor supply to differing PBD levels.[45]

5.1.3 Other policy changes

A final class of relevant omitted factors are other policy changes that are correlated with PBD extensions and that may have influenced transaction length. For instance, more generous food stamp (officially known as Supplemental Nutrition Assistance Program or SNAP) policies may have been adopted during periods with higher PBDs. Returning to Table 6, in the last row, we regress the number of state-month SNAP participating households on PBD levels and find no statistically significant relationship.[46] Further, if SNAP

[44] It is reasonable to assume that it is the strength of the local economy, and not the economy of the entire state, that is affecting worker effort decisions. Throughout our sample we observe substantial within-state variation in unemployment rates across these different local areas.

[45] This finding is further supported by Mas and Moretti (2009), who suggest managers have relatively little say in cashier shift timings and length.

[46] SNAP participation data come from the US Department of Agriculture, Food and Nutrition Service, SNAP Data Tables.

had been correlated with PBD levels, we find it unlikely that food stamp usage would influence transaction length since SNAP benefits are paid in the form of Electronic Benefit Transfer (EBT) cards that are swiped at checkout in the same manner as debit cards with a PIN (Bartfeld et al., 2015). EBT cards are specifically designed to look and act like debit cards in order to reduce the potential stigma of participating in SNAP.

Another policy of relevance is the adoption of plastic bag taxes. During the period of our study, the only jurisdiction to adopt a bag tax was DC, but this adoption still leads to a statistically significant correlation (at the 1% level) between the bag tax policy and PBD levels (see Table 6). This generates an obvious concern for a bias in the same direction as a shirking channel, since plastic bag taxes have been shown to have a significant negative impact on worker productivity (Taylor, 2020). To account for this in our preferred specification (in Table 3), we simply control for the adoption of the bag tax policy.

5.2 Robustness checks

In the online appendices, we present various robustness checks to our main estimates. First, we consider an analysis where we collapse our data to the cashier-register-day level and calculate the (log of the) average of the cashier's transaction length for that day. Results from these additional specifications are in Online Appendix Table A3 and are consistent with our main results. Next, in Table A4, we show that our main results remain unchanged after dropping all cashier(-shift)-level controls. One motivation for dropping these controls is their potential endogeneity with PBD levels. In columns 3–6 of Table A4, we replicate our preferred specifications after (a) ignoring periods where EUC benefits temporarily dropped to zero (and coding these periods with the pre-change PBD level) and (b) dropping all weeks where EUC benefits temporarily dropped to zero. Results remain largely the same across these specifications. We also consider the sensitivity of our standard errors to various clusters in Table A5. Results for our two primary specifications remain statistically significant after clustering by (a) state-week, (b) state-month, (c) cashier, (d) cashier-register, and (e) store.

5.3 Placebo tests

5.3.1 Permutation test

In order to further test the robustness of our main results, we estimate our fully specified model across a variety of placebo treatments. We adopt the permutation test outlined by Bertrand, Duflo and Mullainathan (2004) and utilized in several studies including Ebenstein and Stange (2010) and Chetty, Looney and Kroft (2009). To perform the test, we estimate the preferred model after reassigning treatment status, and use the distribution of these "placebo" estimates for inference. A benefit of such an approach is that no assumption is made on plausible serial correlation of the error term; instead, the "true" estimate is compared against many placebo estimates generated from reassignment of treatment. Since treatment patterns are assigned across three states, we simply consider reassignment of state-treatment statuses across our three states, and juxtapose the true estimate against the remaining five combinations of placebo estimates. These results are in Table 7. Of the six plausible combinations of state-treatment assignments to states, the true estimate of 0.130 is the largest.

5.3.2 Lack of PBD effect at self-checkout registers

In this section, we replicate the main analysis for a separate sample of registers. Before, we excluded transactions completed at self-checkout registers because cashiers do not process these transactions, and thus, these transactions do not provide a measure of cashier productivity. However, the data from self-checkout registers can be used as another type of placebo test. Specifically, we should expect no effect of PBD extensions on transaction length at self-checkout registers under the hypothesis that PBD strictly affects the cashiers (conditional on transaction-level controls).

In Table 8, we estimate a variant of equation (1) without the cashier c index and using scanner data from self-checkout registers. Of the 39 stores in the sample, only 7 have self-checkout registers—2 in DC, 2 in MD, and 3 in VA. The specification in column 1 includes date and register-store fixed effects. Column 2 adds the set of controls excluding those related to cashiers (such as cashier experience). Though this analysis suffers from considerably reduced statistical power, we find no evidence of a statistically significant relationship between PBD extensions and transaction length. Thus, reassuringly, we do not find effects where there are no cashiers, adding internal validity to our results above.

6 Shirking by workers in the American Time Use Survey (ATUS)

In order to determine whether our results extend to other state-years, industries, and occupations, we use the American Time Use Survey (ATUS) to test for shirking responses to PBD extensions that occurred between 2003 and 2014. Exploiting the same variation described in section 3.1, we estimate models with various combinations of state fixed effects, month-year fixed effects, and a vector of controls. These controls include state unemployment rate, the maximum weekly benefit amount available to UI recipients in the state[47], the worker's age, "usual" amount of hours worked per week, weekly earnings, and dummies for

[47]Retrieved from the replication package for Hsu, Matsa and Melzer (2018), available at the AEA website for the article.

family income, gender, race, type of US citizenship, class of worker (e.g., federal government vs. state government vs. private for profit), and general occupational category (e.g., "sales and related occupations" vs. "healthcare support occupation").

In Table 9, similar to Table 6, we investigate whether average worker characteristics are associated with state-month-year PBD levels. We find no statistically significant relationship across eight worker characteristics considered, including earnings and number of hours worked in the week prior. This helps alleviate concerns that PBD changes may lead to changes in worker composition, which are especially important in this analysis given that the ATUS is a cross-section.

Our main results with the ATUS sample are in Table 10. In three of the four specifications we find statistically significant (at the 10% level) increases in the percentage of time at work spent not working in response to more generous PBD levels. In our fully-specified model, we estimate a 0.35 percentage point increase (off a sample mean of 6.68%) in time spent not working in response to an 18-week increase in PBD level.[48]

We perform several additional analyses to test the sensitivity of these results. First, in Figure 4, we consider the permutation test outlined in Bertrand, Duflo and Mullainathan (2004), plotting the empirical distribution of estimated placebo treatment effects from 3,000 randomizations. For each randomization, workers are randomly assigned a state treatment pattern at the state level (without replacement). Results from this test suggest statistical significance at the 10% level for our fully specified model. Second, in Online Appendix Table A6, we find no statistically significant response in minutes spent at the workplace in response to higher PBD levels, suggesting that increased shift length cannot be driving these findings. Overall, these results suggest that the ex ante moral hazard effect observed in our cashier data is potentially pertinent across the US and in other sectors as well. Third, in Figure 5, we test for impacts of future and past PBD levels on current worker behavior in the ATUS data. Lagged effects suggest a type of delayed response by workers, while future PBD levels serve as a falsification test for our results. In panel (a), we show that same week PBD and seven of the eight PBD lags correspond to an increase in transaction length that is statistically significant at the 10% significance level. This suggests that PBD in current and past periods affects current productivity, which we would expect if responses to PBD changes are delayed for some individuals. On the other hand, nearly all of the lead coefficients are statistically insignificant. Only the closest of the eight leads reports positive effects.[49,50]

[48]Since the outcome variable is bounded between zero and one, and the mean is relatively small (0.0668), we rescaled our PBD measure to be counts of 18-weeks (instead of counts of single weeks).

[49]We cannot be certain why this lead is significant; however, we think the size and significance of it could reflect current productivity responding to news about eminent changes in PBD levels.

[50]In panel (b), we run a similar specification in the scanner data which produces analogous results.

To get a sense of the magnitude of the ATUS effects, we perform the following back-of-the-envelope calculations. Given the average worker in our sample spends 510.45 minutes at their workplace on the days they work, of which 31.83 minutes are spent not working,[51] a 0.35 percentage point increase in time spent not working at work translates to a 1.60 minutes increase in shirking per workday. For workers that work five days a week, this aggregates to an additional 6.9 hours of shirking per year. Further aggregating across all full-time workers in the US, an 18-week increase in the PBD level would lead to 773 million additional hours of shirking per year in the US—equivalent to $14 billion at the median hourly wage.[52]

We can compare the ATUS estimates with the cashier estimates in several ways. First, given the units between the two outcomes are different, we can compare our coefficients to the standard deviations of their respective outcomes and roughly interpret in standard deviation units. For an 18-week increase in PBD in the scanner data, our full model estimates a 2.34 percent of a standard deviation increase in transaction length. For ATUS, an 18-week increase in PBD is associated with a 2.84 percent of a standard deviation increase in percentage of time spent at work doing non-work activities. These estimates are markedly similar to each other.

We can also compare the estimates in terms of time lost. From the scanner data, the average transaction takes approximately two minutes to complete, and a cashier takes 2.4 seconds longer to complete a transaction in response to an 18-week increase in PBD. Assuming cashiers work a full 8-hour shift, the cashier would "lose" approximately 576 seconds (i.e., 240 possible transactions $\times$ 2.4 seconds per transaction), or 9.6 minutes, in response to an 18-week PBD increase. ATUS workers on the other hand are spending 1.6 minutes more doing non-work activities while at work. The "true" gap between these time estimates is likely to be smaller, however. Given the scanner data only look at peak processing times, it is likely that cashiers are not spending all of their time processing transactions during non-peak hours. Furthermore, our measure for ATUS workers involves non-work activities—it may be that these workers are additionally experiencing decreases in productivity for actual-work activities and thus "costing" additional time.

7 Conclusions

Numerous studies have investigated the ex post moral hazard effect of more generous UI benefits on unemployment duration. Despite strong theoretical evidence for its importance, empirical evidence of an *ex ante* moral hazard effect of UI (in which workers reduce on-the-job effort in response to increases in UI

[51] This estimate is similar to what the BLS (2015) reports for full-time workers in the US using the 2014 ATUS data.

[52] There were 111,487,000 full-time workers 18 years or older in the US working 35 hours or more per week in 2017, and the US median hourly wage across all occupations was $18 in 2017 (BLS, 2017*a*; 2017*b*).

benefit generosity) remains scant.

In this paper, we exploit state variation in the timing and size of potential benefit duration (PBD) of the UI program in the United States, occurring during the Great Recession, to provide estimates of the ex ante moral hazard effect of UI on worker productivity. Our scanner data consist of roughly 500,000 transactions which occurred at 39 locations of a large national supermarket chain in Maryland, Virginia, and Washington D.C. between November 2008 and February 2011. We estimate statistically significant negative effects of UI benefit duration on worker effort, where effort is measured by observing the length of time (in seconds) a cashier takes to complete a transaction. Our primary specifications utilize cashier-register and day fixed effects, as well as a series of transaction-level controls, to account for an array of potential confounding factors.

Preferred specifications suggest the average 18-week increase in PBD observed in our sample increases transaction time by roughly 2% of the sample mean. Though point estimates are modest, back-of-the-envelope calculations suggest non-trivial losses in time. In order to make up these productivity losses, each affected store would need to acquire over 144 additional hours of cashier labor per year. Our results are driven by the cashiers who are more likely to be terminated due to shirking (lower productivity cashiers) and by the cashiers more likely to be eligible for UI benefits (cashiers who worked more days during the sample period). Shirking is significantly attenuated by transactions on express registers, or those transactions of which there is likely less opportunity for cashiers to shirk. Results using a national cross-sectional survey of workers from the American Time Use Survey (ATUS) further corroborate this ex ante moral hazard effect.

Given the size and ubiquity of unemployment insurance programs, the potential policy implications for these results are substantial. Unemployment insurance programs exist in all OECD countries and are very large—in the United States, per capita expenditures on the UI program have exceeded those for all other safety net programs during each of the last four recessions (Bitler and Hoynes, 2016; Schmieder and Von Wachter, 2016). Our results suggest that, when evaluating the merits of benefit extensions, policymakers should consider the behavioral costs that are likely to occur not only among unemployed recipients of UI, but also among the employed who are potential future recipients.

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Table 1: Summary statistics from scanner data

	DC	Maryland	Virginia	Full Sample
Panel A. Sample characteristics, transaction level				
Transaction Time (in seconds)	128.06	122.11	111.38	119.62
	(107.68)	(102.77)	(90.79)	(100.03)
Total # of Items Scanned per Transaction	12.06	11.25	12.13	11.77
	(13.32)	(12.68)	(13.26)	(13.06)
Price Discounts/Coupons per Transaction	6.99	7.52	8.40	7.71
	(9.89)	(10.73)	(11.60)	(10.88)
Total Expenditure per Transaction	33.98	31.97	38.75	34.97
	(39.62)	(37.39)	(42.80)	(40.12)
# Items Returned per Transaction	0.00	0.00	0.00	0.00
	(0.07)	(0.08)	(0.07)	(0.07)
N	127,445	197,949	190,242	515,636
Panel B. Sample characteristics, cashier level				
Cashier Experience (in # of transactions in sample)	223.98	253.78	299.59	259.90
	(294.91)	(334.45)	(465.77)	(372.96)
Cashier Experience (span of days in sample)	210.28	245.39	227.03	229.44
	(241.15)	(257.03)	(258.31)	(253.28)
Cashier Experience (# of Saturday shifts in sample)	12.13	13.50	14.69	13.49
	(14.41)	(15.90)	(19.49)	(16.77)
Minutes Worked per Hour Shift	40.85	39.80	40.10	40.18
	(17.63)	(18.57)	(18.65)	(18.36)
N	569	780	635	1,984
Panel C. Sample characteristics, store level				
# of Registers per Store	8.13	6.47	7.00	7.00
	(2.64)	(1.28)	(1.96)	(1.92)
# of Unique Cashiers per Store	71.13	45.88	45.36	50.87
	(23.06)	(12.35)	(16.91)	(19.20)
N	8	17	14	39

Source: Authors' calculations from scanner data. Notes: Cashier experience is measured as the total number of transactions observed in the sample for the given cashier prior to the current transaction. Cashier fatigue is measured as the total number of transactions during the current shift for the given cashier prior to the current transaction.

Table 2: Potential benefit duration (PBD) changes during sample period

	Washington D.C.			Maryland			Virginia		
	EB	EUC	Total	EB	EUC	Total	EB	EUC	Total
12/1/2008	0	33	59	0	20	46	0	20	46
4/5/2009	0	33	59	0	20	46	0	20	46
4/12/2009	20	33	79	0	33	59	0	20	46
5/3/2009	20	33	79	0	33	59	13	33	72
11/8/2009	20	53	99	0	47	73	13	47	86
4/5/2010	20	0	46	0	0	26	13	0	39
4/15/2010	20	53	99	0	47	73	13	47	86
6/2/2010	20	0	46	0	0	26	13	0	39
7/22/2010	20	53	99	0	47	73	13	47	86
11/30/2010	20	0	46	0	0	26	13	0	39
12/17/2010	20	53	99	0	47	73	13	47	86

Notes: EB = extended benefits. EUC = emergency unemployment compensation. Numbers represent maximum duration (in weeks) of UI benefits available during the time period beginning on the date in the first column. Total weeks are calculated as the sum of any EB extensions, EUC extensions, and the standard pre-extension PBD for all states (26 weeks).

Table 3: Main results from scanner data

	Transaction Length (in seconds)				
	(1)	(2)	(3)	(4)	(5)
Potential benefit duration	0.139	0.150	0.133	0.130	0.136
	(0.061)	(0.065)	(0.047)	(0.056)	(0.079)
Total expenditures			0.134	0.134	0.230
			(0.013)	(0.013)	(0.018)
Total items scanned			2.821	2.825	2.591
			(0.052)	(0.052)	(0.067)
Price discounts/coupons			0.720	0.726	0.506
			(0.024)	(0.024)	(0.031)
Local UE rate (prior month)			-0.678	-0.661	-0.977
			(0.429)	(0.463)	(0.576)
State UE rate (prior month)			-1.537	-1.354	-3.245
			(1.196)	(1.268)	(1.860)
Observations	515636	515618	515618	515433	354281
Controls			X	X	X
Date FE	X	X	X	X	X
Register X Store FE	X	X	X		
Cashier X Store FE		X	X		
Cashier X Register X Store FE				X	X
Customer FE					X

Notes: Potential benefit duration is measured in weeks. Transaction expenditure is measured in dollars. Controls for each regression include indicators for whether the transaction included items from particular departments (e.g., alcohol), an indicator for whether a plastic bag tax was in place at the store, number of households participating in SNAP per state-month, the total number of registers open during the transaction, the cashier's experience as measured by total number of career transactions completed, the cashier's "fatigue" as measured by the number of transactions the cashier had previously completed on that shift, and the cashier's length of shift measured in both number of transactions and in minutes. "Date" refers to exact date (e.g., August 3, 2017). Standard errors, shown in parentheses, are clustered at state by date level.

Table 4: Main results from scanner data by subsample

		# of shifts		Productivity		Register type	
	Full	High	Low	High	Low	Express	Regular
Outcome: Transaction Length							
Potential benefit duration	0.130	0.163	0.132	-0.084	0.173	-0.003	0.251
	(0.056)	(0.065)	(0.078)	(0.099)	(0.067)	(0.058)	(0.082)
Total expenditures	0.134	0.157	0.130	0.144	0.141	0.234	0.108
	(0.013)	(0.020)	(0.023)	(0.030)	(0.018)	(0.020)	(0.015)
Total items scanned	2.825	2.719	2.756	2.498	2.813	2.668	2.901
	(0.052)	(0.087)	(0.088)	(0.121)	(0.071)	(0.076)	(0.063)
Price discounts/coupons	0.726	0.707	0.778	0.567	0.789	0.644	0.760
	(0.024)	(0.034)	(0.043)	(0.050)	(0.033)	(0.033)	(0.029)
Local UE rate (prior month)	-0.661	0.110	-0.562	-0.584	-0.285	-0.839	-0.532
	(0.463)	(0.669)	(0.596)	(0.615)	(0.627)	(0.466)	(0.729)
State UE rate (prior month)	-1.354	1.450	-4.222	-1.387	-1.766	0.021	-2.704
	(1.268)	(1.941)	(2.010)	(2.831)	(1.722)	(1.349)	(1.944)
Observations	515433	163806	156865	72093	248114	281291	234098
Controls	X	X	X	X	X	X	X
Date FE	X	X	X	X	X	X	X
Cashier X Register X Store FE	X	X	X	X	X	X	X

Notes: Potential benefit duration is measured in weeks. Transaction expenditure is measured in dollars. Controls for each regression include indicators for whether the transaction included items from particular departments (e.g., alcohol), an indicator for whether a plastic bag tax was in place at the store, number of households participating in SNAP per state-month, the total number of registers open during the transaction, the cashier's experience as measured by total number of career transactions completed, the cashier's "fatigue" as measured by the number of transactions the cashier had previously completed on that shift, and the cashier's length of shift measured in both number of transactions and in minutes. In columns 2 and 3, shift subsamples are defined by cashiers working above the 75th percentile of shifts worked (high shifts) or below the 75th percentile (low shifts) before the first PBD change. In columns 4 and 5, productivity subsamples are defined by estimating cashier fixed effects from a regression of transaction length for the pre-policy period and separating by whether the cashier was above or below the 75th percentile fixed effect. "Date" refers to exact date (e.g., August 3, 2017). Standard errors, shown in parentheses, are clustered at state by date level.

Table 5: Is UI potential benefit duration correlated with total supermarket sales?

	Change in Store-Month Sales					
	Sales ($)	Sales ($)	Sales (# Items)	Sales (# Items)	$ Spent Per Item	$ Spent Per Item
18-week PBD increase	-14.734	184.010	-39.442	18.571	0.000	0.001
	(82.492)	(71.742)	(30.233)	(30.698)	(0.000)	(0.000)
State UE rate (prior month)		-7011.488		-2046.647		-0.011
		(1548.421)		(539.266)		(0.004)
Observations	2381922	2381922	2381922	2381922	2381922	2381922
Mean of Y	563245	563245	184385	184385	3.33	3.33
Store FE	X	X	X	X	X	X
Month-Year FE	X	X	X	X	X	X

Source: Nielsen Scanner data aggregated to the store-month level from 2009–2014 for 39,622 grocery, drug, and mass merchandiser stores in the US. Notes: Outcome variables are at the store-month level: total sales in dollars (columns 1–2), total sales in items sold (columns 3–4), and average expenditures per item (columns 5–6). Models include store and month-year fixed effects. Standard errors, shown in parentheses, clustered at the state level.

Table 6: Is UI potential benefit duration correlated with other covariates?

	Expend per Txn.	Items Scanned per Txn	Price Discount/ Coupons	# Open Registers	# Items Returned	Alcohol/ Tobacco
Potential benefit duration	-0.005	0.003	0.014	-0.006	-0.000	0.001
	(0.025)	(0.008)	(0.010)	(0.008)	(0.000)	(0.000)
Observations	4407	4407	4407	4407	4407	4407
Y mean	33.904	11.488	7.466	5.228	0.002	0.133
Date FE	X	X	X	X	X	X
Store FE	X	X	X	X	X	X

	Bakery/ Deli	Dairy	Floral	Frozen	Meat/ Seafood	Produce
Potential benefit duration	-0.000	0.000	-0.000	-0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.002)
Observations	4407	4407	4407	4407	4407	4407
Y mean	0.371	1.150	0.048	0.612	0.712	1.638
Date FE	X	X	X	X	X	X
Store FE	X	X	X	X	X	X

	Lag Local UE Rate	Lag State UE Rate	# Employed Cashiers	Cashier Experience	Food Stamps	Bag Tax
Potential benefit duration	-0.003	-0.001	-0.007	-0.561	0.624	0.017
	(0.012)	(0.001)	(0.010)	(1.131)	(0.400)	(0.005)
Observations	4407	4407	4407	4407	4407	4407
Y mean	6.074	7.442	6.073	492.275	249.411	0.105
Date FE	X	X	X	X	X	X
Store FE	X	X	X	X	X	X

Notes: Each cell reports a coefficient from a single regression of potential benefit duration on an outcome, collapsed to the store-date level. Potential benefit duration is measured in weeks. "Expend per Txn." is the average amount spent per transaction, measured in dollars. "Items Scanned per Txn." is the average number of items scanned per transaction. "Price Discount/Coupons" is the average dollar amount of price discounts per transaction (from sale items, coupons, and reward cards). "# Open Registers" is the average number of open registers per store-date. "# Items Returned" is the number of items scanned and then un-scanned (i.e., returned) per transaction. The seven department categories are the average number of products bought per transaction by category. The "Lag Local and State UE Rates" are the one month lagged county-month and state-month level unemployment rates. "# Employed Cashiers" is the number of cashiers working per store-date. "Cashier experience" is the average length of time the cashier appears in the sample. "Food Stamps" is the number of households participating in SNAP per state-month, measured in thousands. "Bag Tax" is an indicator for whether a bag tax was in place per store-date. Standard errors clustered at store level are shown in parentheses.

Table 7: Placebo tests - Reassignment of treatments across states

		Placebos				
	Actual	(1)	(2)	(3)	(4)	(5)
Outcome: Transaction Length						
Potential benefit duration	0.130	-0.009	0.075	-0.052	-0.012	-0.153
	(0.056)	(0.066)	(0.063)	(0.057)	(0.065)	(0.068)
Total expenditures	0.134	0.134	0.134	0.134	0.134	0.134
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Total items scanned	2.825	2.825	2.825	2.825	2.825	2.825
	(0.052)	(0.052)	(0.052)	(0.052)	(0.052)	(0.052)
Price discounts/coupons	0.726	0.726	0.727	0.726	0.726	0.726
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)
Local UE rate (prior month)	-0.661	-0.756	-0.741	-0.726	-0.749	-0.703
	(0.463)	(0.463)	(0.462)	(0.465)	(0.462)	(0.463)
State UE rate (prior month)	-1.354	-0.744	-0.770	-1.006	-0.862	-0.270
	(1.268)	(1.211)	(1.222)	(1.233)	(1.287)	(1.195)
Observations	515433	515433	515433	515433	515433	515433
# of Treatment Swaps	0	1	1	1	2	2
Controls	X	X	X	X	X	X
Date FE	X	X	X	X	X	X
Cashier X Register X Store FE	X	X	X	X	X	X

Notes: Potential benefit duration is measured in weeks. Columns (1) through (5) consider all five remaining permutations of swaps of PBD levels by state. In (1), Washington D.C. and Virginia are swapped. In (2), Washington D.C. and Maryland are swapped. In (3), Virginia and Maryland are swapped. In (4), Virginia is assigned Maryland PBD levels, Maryland to D.C. levels, and D.C. to Virginia levels. In (5), Virginia is assigned D.C. PBD levels, Maryland to Virginia levels, and D.C. to Maryland levels. Transaction expenditure is measured in dollars. Controls for each regression include indicators for whether the transaction included items from particular departments (e.g., alcohol), an indicator for whether a plastic bag tax was in place at the store, number of households participating in SNAP per state-month, the total number of registers open during the transaction, the cashier's experience as measured by total number of career transactions completed, the cashier's "fatigue" as measured by the number of transactions the cashier had previously completed on that shift, and the cashier's length of shift measured in both number of transactions and in minutes. "Date" refers to exact date (e.g., August 3, 2017). Standard errors, shown in parentheses, are clustered at state by date level.

Table 8: Placebo test - Results from self-checkout scanner data

	Transaction Length (in seconds)	
	(1)	(2)
Potential benefit duration	0.386	-0.001
	(0.492)	(0.491)
Total expenditures		-0.143
		(0.085)
Total items scanned		6.985
		(0.389)
Price discounts/coupons		1.516
		(0.146)
Observations	44646	44646
Controls		X
Date FE	X	X
Register X Store FE	X	X

Notes: This table uses scanner data only from self-checkout registers. Seven of the 39 stores have self-checkout registers. Potential benefit duration is measured in weeks. Transaction expenditure is measured in dollars. Controls for each regression include indicators for whether the transaction included items from particular departments (e.g., alcohol), an indicator for whether a plastic bag tax was in place at the store, number of households participating in SNAP per state-month, lagged county-month level unemployment rate, lagged state-month level unemployment rate, and the total number of registers open during the transaction. “Date” refers to exact date (e.g., August 3, 2017). Standard errors, shown in parentheses, are clustered at state by date level.

Table 9: Is UI potential benefit duration correlated with other ATUS covariates?

	Age	Female	White	Weekly Earnings	Usual # Work Hours
Potential benefit duration	0.050	0.008	-0.001	-2.727	0.032
	(0.238)	(0.013)	(0.009)	(13.894)	(0.215)
Observations	6041	6041	6041	6041	6041
Mean of Y	41.74	0.50	0.82	880.50	41.94
State FE	X	X	X	X	X
Month-Year FE	X	X	X	X	X

	Gov't Sector	Private Sector	Max UI Benefit	Lagged UE Rate	Work Hours Prior Week
Potential benefit duration	0.007	-0.007	-0.016	1.022	-0.335
	(0.009)	(0.009)	(0.033)	(0.118)	(0.279)
Observations	6041	6041	6041	6041	5963
Mean of Y	0.19	0.81	4.06	6.40	40.33
State FE	X	X	X	X	X
Month-Year FE	X	X	X	X	X

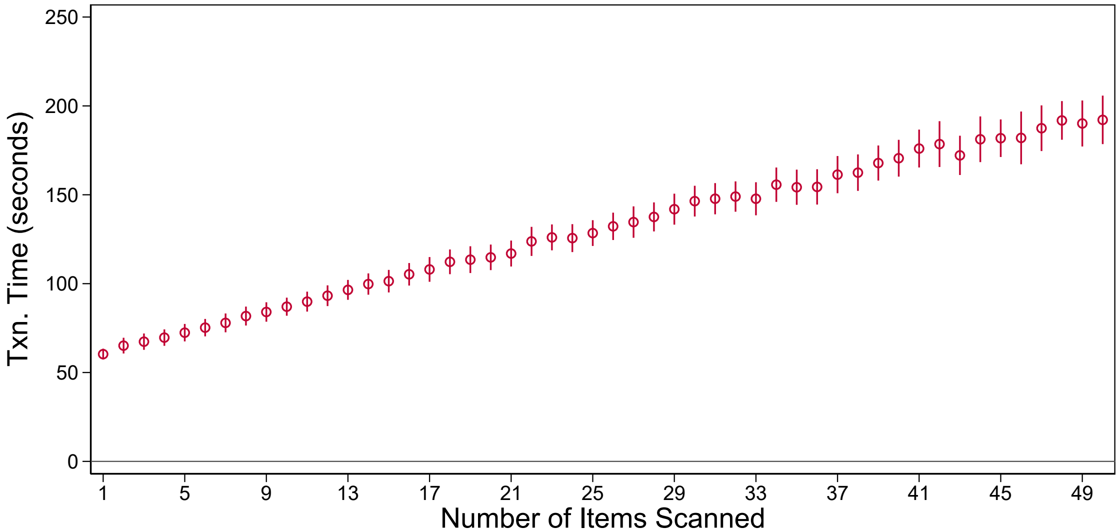
Notes: Each cell reports a coefficient from a single regression of potential benefit duration on an outcome, collapsed to the state-month-year level. Potential benefit duration is measured in weeks. "Age" is the average age of workers in our ATUS sample. "Female" is the fraction of workers who were female. "Usual # Work Hours" is the average number of self-reported weekly work hours. "Weekly Earnings" is the average worker weekly earnings in dollars. "White" is the fraction of workers who were White. "Gov't Sector" and "Private Sector" are the fraction of workers in the government vs. the private sector, respectively. "Max UI Benefit" and "Lagged UE Rate" are state-month-year maximum UI benefits and prior month unemployment rates, respectively. "Work Hours Prior Week" is the average number of work hours from the worker's week prior to completing the CPS. Standard errors, shown in parentheses, are two-way clustered at the state and month-year level.

Table 10: Results from American Time Use Survey (ATUS)

	% Time At Work Not Working			
	(1)	(2)	(3)	(4)
18-week PBD increase	0.0022	0.0024	0.0025	0.0035
	(0.0013)	(0.0017)	(0.0015)	(0.0019)
State UE rate (prior month)			-0.0001	-0.0006
			(0.0010)	(0.0010)
Maximum WBA (100s)			0.0046	0.0042
			(0.0029)	(0.0030)
Observations	30094	30094	30094	30094
Mean of Y	0.0668	0.0668	0.0668	0.0668
State FE	X	X	X	X
Month FE	X		X	
Year FE	X		X	
Month-Year FE		X		X
Controls			X	X

Notes: Controls include state unemployment rate and maximum UI benefits (in dollars), the individual's age, "usual" amount of hours worked per week, weekly earnings, hourly wage, and dummies for family income, gender, race, US citizenship, whether the individual had multiple jobs, class of worker (e.g., federal government vs. state government vs. private for profit), and general occupational category (e.g., "sales and related occupations" vs. "healthcare support occupations"). Observations weighted according to ATUS probability weights. Standard errors, shown in parentheses, clustered at state level.

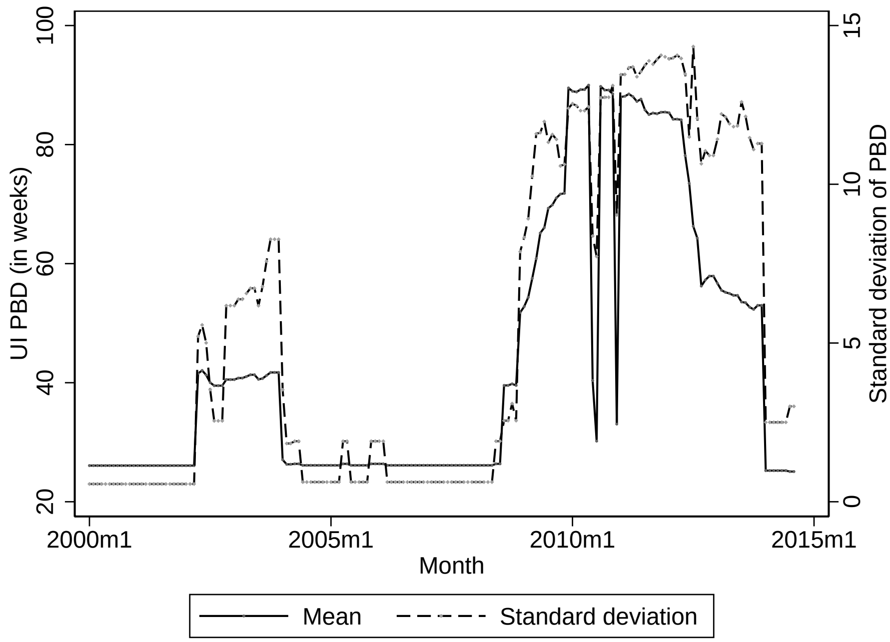
Figure 1: Relationship between the number of items scanned and average transaction time



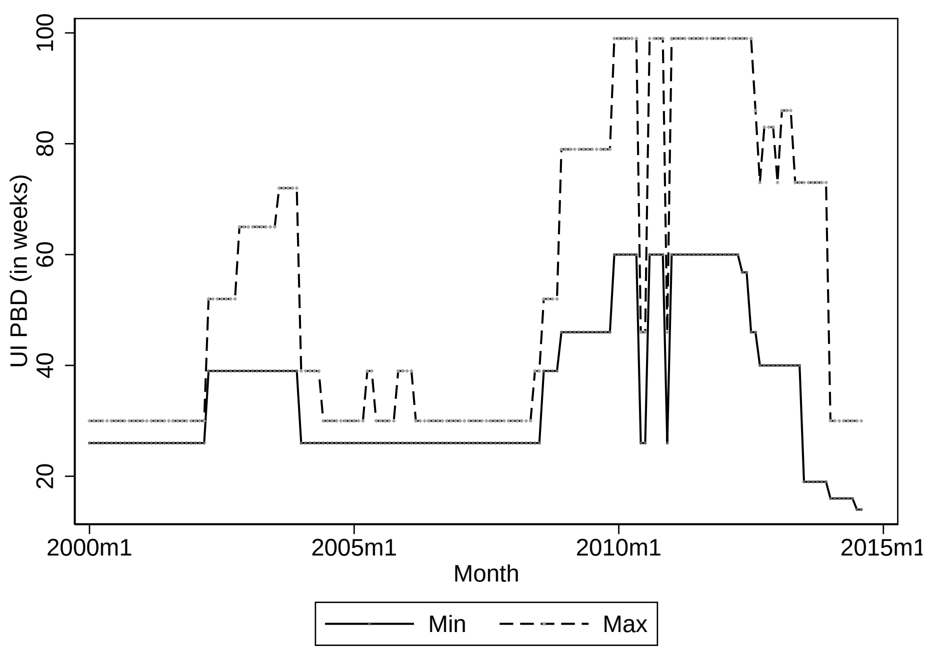
Notes: Estimates come from regressing transaction time on dummy variables for the number of items scanned per transaction, also controlling for the price and types of items purchased, as well as store, cashier, register and week-of-sample fixed effects. The first 50 dummy variables are plotted, adjusted to include the intercept coefficient. Thus, the panel shows the average length of a transaction by transaction size. Upper and lower 95% confidence intervals are depicted, estimated using two-way cluster robust standard errors on store and week-of-sample.

Figure 2: Trends in UI potential benefit duration (PBD) for ATUS sample

(a) Mean and standard deviation of PBD across states by month



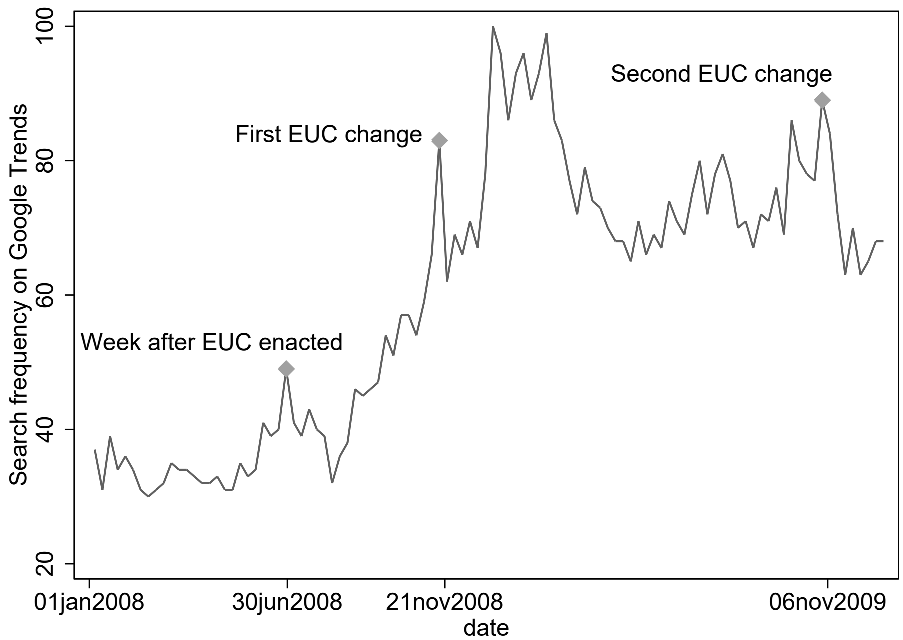
(b) Min and max of PBD across states by month



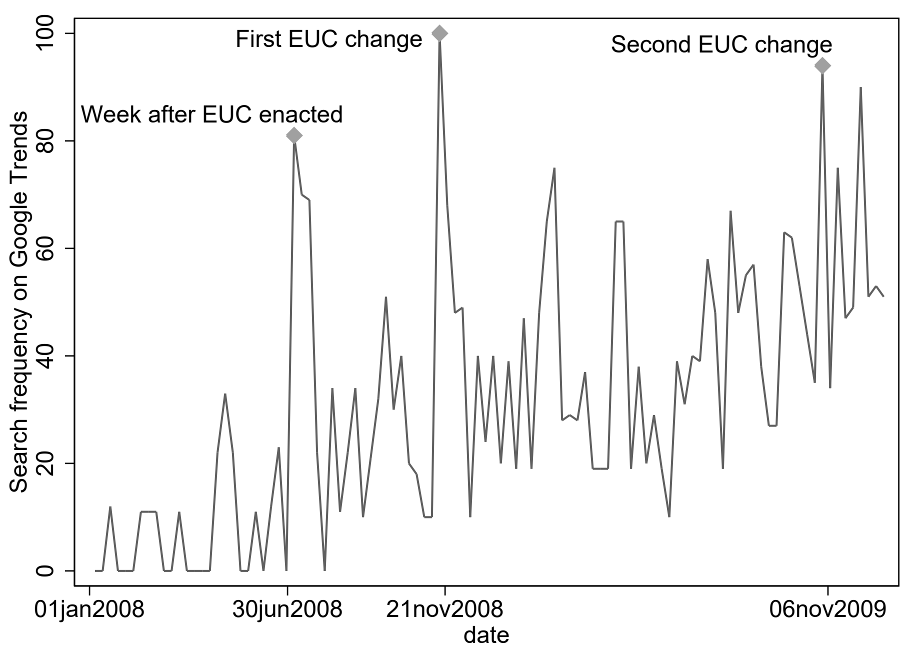
Source: Data were obtained from a replication file for Farber, Rothstein and Valletta (2015).

Figure 3: Searches on Google via Google Trends

(a) Searches for "Unemployment benefits"

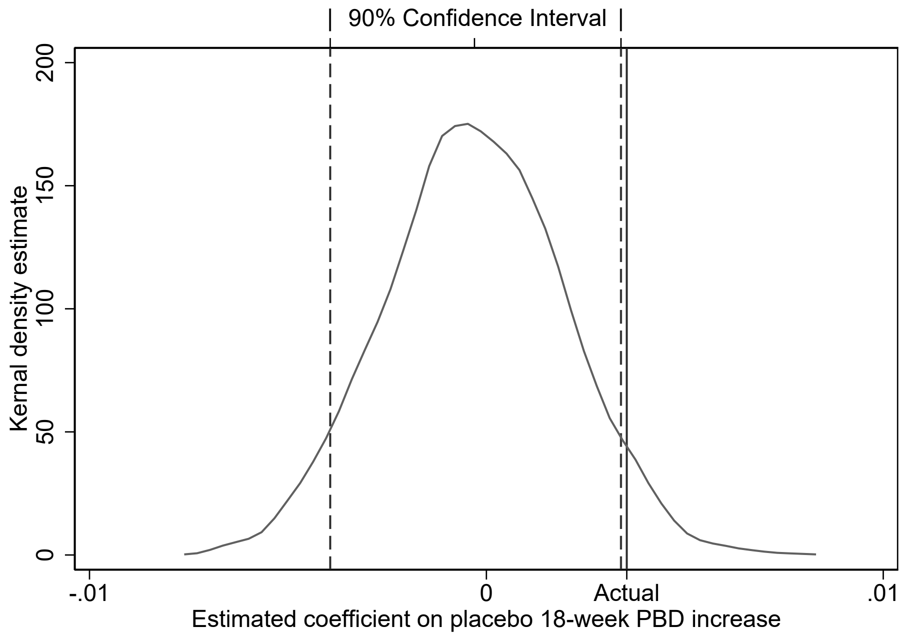


(b) Searches for "Emergency Unemployment Compensation"



Notes: Google Trends data retrieved from Google Inc. Search frequency, indexed to a 0 to 100 scale, shows how often a particular search-item on Google Search (i.e. "Unemployment benefits" and "Emergency Unemployment Compensation") is entered relative to the total search-volume for the search-item across the queried time period (January 2008 - December 2009) within the United States. An index of 100 reveals the week(s) with the highest search frequency of that item within the queried time period.

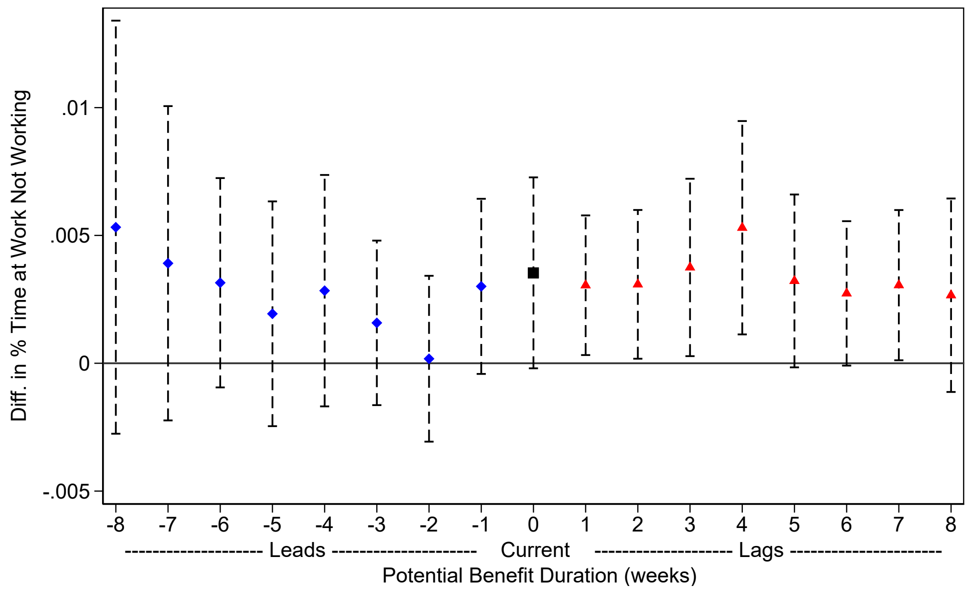
Figure 4: Permutation test of inference with ATUS - % time at work not working



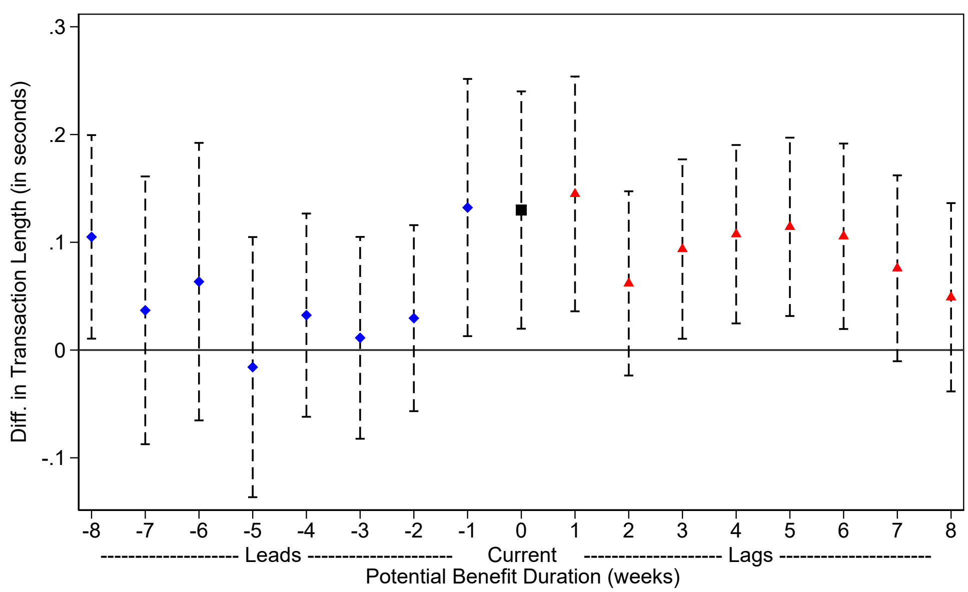
Notes: Figure plots the smoothed empirical distribution of estimated placebo treatment effects from 3,000 randomizations, where workers, by state, were randomly assigned state treatment patterns (without replacement). Dashed lines report the 90% confidence interval (the 5 and 95 percentiles of the distribution), while the solid line reports the actual point estimate. All estimates come from our fully specified model. Permutation test outlined in Bertrand, Duflo and Mullainathan (2004).

Figure 5: Impact of potential benefit duration lags and leads

(a) % Time at Work Not Working (ATUS data)



(b) Cashier processing speed (Scanner data)



Notes: Each point presents the β coefficient and its 95% confidence interval for separate regressions of an increase in PBD levels occurring w weeks ago on (a) the percentage of time the worker spent at work doing non-work activities and (b) transaction length. Lag coefficients (red triangles) represent delayed responses to PBD changes whereas lead coefficients (blue diamonds) serve as a falsification test. For example, the coefficient for w=1 corresponds to the impact of a PBD increase occurring in the week prior.

FOR ONLINE PUBLICATION

Appendix 1: Theoretical Model

Although the comparative static of interest is straightforward, and has been previously established in the literature (e.g., Shapiro and Stiglitz, 1984), in this appendix we lay out a simple theoretical model for a worker's choice of effort while on the job. The model makes clear the key assumptions required for an ex ante moral hazard effect of UI to exist, and helps to suggest the types of workers who are expected to respond ex ante to changes in UI benefits.

Consider a worker who chooses effort, e, to maximize expected utility:

$$E(U) = (1 - p(e))U(C_e) + p(e)U(C_u) \tag{2}$$

where $p(e)$ is the probability that worker is fired (decreasing in e), C_e is consumption while employed, C_u is consumption while unemployed and $U(\cdot)$ is increasing and concave. We make the following additional assumptions:

1. $p''(e) > 0$
2. $C_e = w - e$, where w is the wage
3. $\frac{\partial C_u}{\partial b} > 0$ & $\frac{\partial C_u}{\partial d} > 0$, where b is UI benefit level and d is UI benefit duration
4. $C_e > C_u$

The first order condition is:

$$(1 - p(e))U'(C_e) = -p'(e)(U(C_e) - U(C_u)) \tag{3}$$

where the left-hand side is the marginal cost of an increase in effort and the right-hand side is the marginal benefit of an increase in effort. The second order condition is:

$$p''(e)(U(C_u) - U(C_e)) + 2p'(e)U'(C_e) + (1 - p(e))U''(C_e) \equiv S(\cdot) \tag{4}$$

Applying the implicit function theorem to the FOC and denoting e^* the optimal effort:

$$\frac{\partial e^*}{\partial C_u} = -\frac{p'(e)U'(C_u)}{S(\cdot)} \tag{5}$$

The assumptions ensure that (3) and (4) are negative so that an increase in UI benefits or duration will decrease effort.

An implicit assumption, which clearly holds in the context of supermarket cashiers, is that employers partially observe effort (in order for $\frac{\partial p(e)}{\partial e} < 0$ to hold). Cases in which $p'(e)$ violates the above assumptions can provide some intuition for expected heterogeneity in equation (4). Consider a worker who cannot be fired. This worker has $p(e) = 0$ $(\forall e)$ and does not change e^* in response to ΔC_u. A worker with slightly less strong employment protection will have very small $|p'(e)|$ and a weak, but still negative, relationship between C_u and e. Although the workers in our setting are unionized, past work with data from this supermarket chain has observed that these workers can be fired if they are perceived as under-performing (see Mas and Moretti, 2009). Assumption (1) implies that there are "decreasing returns" to effort. This seems reasonable in most cases and is necessary for $\frac{\partial e^*}{\partial C_u} < 0$ to *always* hold. $\frac{\partial e^*}{\partial C_u} < 0$ will still often hold with concave $p(e)$, depending on the relative magnitude of the terms in the SOC.

We do not model the optimal e^* from the employer's or social planner's perspective. Therefore, we do not explicitly define shirking and we use the terms "a decrease in effort" and "an increase in shirking" interchangeably. A general equilibrium approach would model the employer's choice of wage offers and it is worth considering whether or not such employer responses affect the partial equilibrium relationships that we estimate. It is at least possible for both employers and customers to foresee changes in worker effort provision in response to UI benefit changes. In section 5, we investigate these possibilities by looking for changes in cashier characteristics and transaction characteristics in response to PBD changes. Concerns about employer responses are also partially reduced by observations in past work with data from this supermarket chain which suggest that workers are primarily responsible for choosing their own shifts (Mas and Moretti, 2009).

Appendix 2: Unemployment Insurance Program Extensions in the US

The Extended Benefits Program

The EB program is state run and has existed since 1970. Under EB, a state's PBD is extended by either 13 or 20 weeks if the state's 13-week average Insured Unemployment Rate (IUR) or 3-month average Total Unemployment Rate (TUR) meet certain threshold, or "trigger," levels. The TUR is simply the ratio of the number of unemployed workers to the total number of workers in the state. The IUR is the ratio of UI claimants to the total number of workers in UI-eligible jobs in the state. All states are required to provide an additional 13 weeks of UI benefits if the IUR is at least 5.0% and at least 120% of the average of the state's IURs for the same 13 week period during the past 2 years. In addition to the this, states decide whether to follow one, both, or neither of the following optional triggers:

1. If the IUR is at least 6.0% (regardless of past IURs) then an additional 13 weeks of benefits are made available. This is known as the "IUR option."

2. If the TUR is at least 6.5% and at least 110% of the same TURs in either of the prior 2 years, then an additional 13 weeks of benefits are made available. Additionally, if the TUR is at least 8% and at least 110% of the same TURs in either of the prior 2 years, then an additional 20 weeks of benefits are made available (for 20 weeks total of EB, not 33). This is known as the "TUR option."

The EB program was originally financed 50% by states and 50% by the federal government. However, starting on February 17, 2009, the American Recovery and Reinvestment Act (ARRA) temporarily made the EB program fully federally financed. This additional federal financing remained in effect through the entirety of our sample. The 2-year "look-back" timeframe present in several of the threshold rules was temporarily changed to a 3-year period in December 2010, and this change also remained in effect throughout the remainder of our sample (Whittaker and Isaacs, 2013; Marinescu, 2017).

The Emergency Unemployment Compensation Program

The EUC program was enacted by the federal government as a response to the Great Recession and was federally run and funded throughout its existence. First established by the Emergency Unemployment Compensation Act on June 30, 2008, the EUC program originally provided 13 weeks of additional eligibility in all states. The design of the EUC program was changed twice during the Great Recession. On November 21, 2008 the EUC was given a two tier structure, 20 weeks of additional eligibility was provided for all states in tier 1 and an additional 13 weeks was provided for states with a TUR $\geq 6\%$ or a IUR $\geq 4\%$. On November 6, 2009 the second tier was increased to 14 weeks and given to all states regardless of TUR or IUR, a third tier was created providing 13 weeks to states with TUR $\geq 6\%$ or a IUR $\geq 4\%$, and a fourth tier was created providing 6 weeks to states with TUR $\geq 8.5\%$ or a IUR $\geq 6\%$ (Whittaker and Isaacs, 2014; Marinescu, 2017). The tiers in each of these iterations are cumulative, so that after November 6, 2009 in a state that selected the TUR option for the EB program, the maximum possible PBD available included the original 26 weeks, 20 weeks of EB, 20 weeks of EUCI, 14 weeks of EUCII, 13 weeks of EUCIII, and 6 weeks of EUCIV (for a total of 99 weeks).

As a temporary program EUC was originally given an expiration date of March 28, 2009. Congress extended the program multiple times so that it did not expire indefinitely until well after our sample ends. However, on four separate occasions during our sample (in March, April, June, and November of 2010) Congress failed to extend the program before its previous expiration date so that there were temporary lapses in EUC availability. The first two of these lapses were short (2 and 10 days respectively) while the latter two were relatively long (nearly 2 months).

The Temporary Extension of Unemployment Compensation Program

The TEUC program, also federally run and funded, was available to new claimants between March 2002 and December 2003.[53] Benefits continued to be available for existing but unexhausted TEUC claims into early 2004. The TEUC program extended UI benefits for either 13 or 26 weeks, with the additional 13 weeks (second tier) of benefits available in states with an IUR (13 week) of at least 4% and at least 120% higher than in the same time period during the prior two years (Valletta, 2014).

Additional Detail on Extensions in Scanner Data Sample

As described briefly in section 3.1, the PBD extensions we exploit for identifying variation occur for one of three reasons: (1) a state's unemployment rate crosses a threshold or "trigger" value currently in place (see first subsection of this appendix for specific unemployment rate and trigger values used), (2) the

[53] Variation in PBD from the TEUC program is only used in our ATUS analyses, since the program does not overlap with our scanner data sample.

relevant authority (state government for EB, federal for EUC or TEUC) changes the trigger value to a level below the state's current unemployment rate, or (3) the federal government alters the (EUC or TEUC) program by changing the number of weeks available or allowing the program to expire (either temporarily or permanently). Here we provide additional narrative detail for each of the extensions occurring in our scanner data sample (ignoring EUC program lapses 4/5/2010-4/14/2010, 6/2/2010-7/21/2010, and 11/30/2010-12/16/2010). The dates and PBD levels for each of these changes are shown in Table 2. Sources for the information provided below are the EB and EUC trigger notices made available online by the US Department of Labor.[54]

1. Washington D.C., 4/12/2009, EB: Number of weeks available through the EB program increases from 0 to 20. This change resulted directly from D.C. adopting the TUR option. The TUR in D.C. exceeded both trigger values (13 week and 20 week) under the TUR option, but was below the IUR trigger value.

2. Maryland, 4/12/2009, EUC: Number of weeks available through the EUC program increases from 20 to 33. This change resulted from MD's TUR crossing the threshold value of 6%. (During this time period the second tier of EUC benefits provided an additional 13 weeks to states with TUR $\geq$ 6% (Isaacs and Whittaker, 2014).)

3. Virginia, 5/3/2009, EB: Number of weeks available through the EB program increases from 0 to 13. This change resulted directly from VA adopting the TUR option. The TUR in VA exceeded the 13 week trigger value under the TUR option, but was below the IUR trigger value and the 20 week TUR trigger value.

4. Virginia, 5/3/2009, EUC: Number of weeks available through the EUC program increases from 20 to 33. This change resulted from VA's TUR crossing the threshold value of 6%. (During this time period the second tier of EUC benefits provided an additional 13 weeks to states with TUR $\geq$ 6% (Isaacs and Whittaker, 2014).

5. Washington D.C., 11/8/2009, EUC: Number of weeks available through the EUC program increases from 33 to 53. This change resulted from a policy change at the federal level which restructured the EUC program, increasing the number of weeks available through the EUC's second tier to 14

[54] See the Office of Unemployment Insurance website, Online, *accessed 14 Sep. 2018.*

(from 13), and creating third (13 weeks), and fourth (6 weeks) tiers. The TUR in D.C. exceeded the threshold value for the third and fourth tiers (Isaacs and Whittaker, 2014).

6. Maryland, 11/8/2009, EUC: Number of weeks available through the EUC program increases from 33 to 47. This change resulted from a policy change at the federal level which restructured the EUC program, increasing the number of weeks available through the EUC's second tier to 14 (from 13), and creating third (13 weeks), and fourth (6 weeks) tiers. The TUR in MD exceeded the threshold value for the third tier but not the fourth (Isaacs and Whittaker, 2014).

7. Virginia, 11/8/2009, EUC: Number of weeks available through the EUC program increases from 33 to 47. This change resulted from a policy change at the federal level which restructured the EUC program, increasing the number of weeks available through the EUC's second tier to 14 (from 13), and creating third (13 weeks), and fourth (6 weeks) tiers. The TUR in VA exceeded the threshold value for the third tier but not the fourth (Isaacs and Whittaker, 2014).

Changes to State Regular PBD During ATUS Sample

Between 4/2011 and 8/2014 the states of Arkansas, Florida, Georgia, Illinois, Michigan, Missouri, North Carolina, and South Carolina each passed legislation reducing their regular PBDs below 26 weeks (Isaacs, 2019). This variation is not relevant for our scanner data sample but is utilized in our ATUS analyses. Here we provide additional detail on each of these policy changes, listed in order of the month that the relevant PBD change is first recorded in our data. Unless otherwise noted sources are the Department of Labor's Reports on State UI Legislation.[55].

1. Arkansas, 4/2011: Arkansas Senate Bill 593 reduced AR's PBD to 25 weeks. (See 2011 report #5.)

2. Missouri, 5/2011: Missouri House Bill 163 reduced MO's PBD to 20 weeks (Johnston and Mas, 2018).

3. South Carolina, 7/2011: South Carolina House Bill 3672 reduced SC's PBD to 20 weeks. (See 2011 report #6.)

4. Florida, 1/2012: Florida House Bill 7005 reduced FL's PBD to between 12 and 23 weeks depending on the state's unemployment rate. Specifically, the PBD of UI in FL is updated up to once annually

[55] See the Office of Unemployment Insurance website, Online, *accessed 12 Mar. 2020*

on January 1st based on the unemployment rate in the state during the third quarter of the previous year. During our ATUS sample, FL's PBD decreased from 26 to 23 weeks in 1/2012, to 19 weeks in 1/2013, and to 16 weeks in 1/2014. (See 2011 report #5.)

5. Illinois, 1/2012: Illinois House Bill 1030 reduced IL's PBD to 25 weeks. (See 2011 report #7.)

6. Michigan, 2/2012: Michigan House Bill 4408 reduced MI's PBD to 20 weeks. (See 2011 report #2.)

7. Georgia, 7/2012: Georgia House Bill 347 reduced GA's PBD to between 14 and 20 weeks depending on the state's unemployment rate. Specifically, the PBD of UI in GA is updated up to twice annually on January 1st and July 1st based on the unemployment rate in the state during the previous October and April, respectively. During our ATUS sample, GA's PBD decreased from 26 weeks to 19 weeks in 7/2012, to 18 weeks in 7/2013, and to 15 weeks in 7/2014. (See 2012 report #1.)

8. North Carolina, 7/2013: North Carolina House Bill 3672 reduced NC's PBD to between 12 and 20 weeks depending on the state's unemployment rate. Specifically, the PBD of UI in NC is updated up to twice annualy on January 1st and July 1st based on the unemployment rate in the state during the previous October and April, respectively. During our ATUS sample, NC's PBD decreased from 26 to 19 weeks in 7/2013, and to 14 weeks in 7/2014. (See 2013 report #10.)

Appendix 3: Additional Evidence of Awareness of UI Benefit Extensions

This appendix uses Google Trends and national polls to provide additional evidence of general awareness about UI benefit extensions during the Great Recession. Though Google Trends does not report raw search numbers, they do allow comparison of popularities across five search items per query.[56] By scaling across all five search items and 104 weeks, one can compare search indices within weeks to get a better sense of the absolute popularity of a particular search item. In Online Appendix Figure A2, we conduct three separate five-item searches, juxtaposing "Unemployment benefits" against four other popular searches. In the first panel, we compare searches for "Unemployment benefits" against "Disability insurance," "Food stamps," "Pell," and "Recession." Across our time frame, "Unemployment benefits" was a more popular search term than each of these four items. People searched for "Unemployment benefits" roughly twice as often as "Food stamps." The term with the largest search volume was "Recession" in January 2008 (at the onset of the Great Recession), and yet the popularity of this search was only slightly greater than the popularity for searches for "Unemployment benefits" during the ARRA implementation. In latter 2009, people searched "Unemployment benefits" at nearly three times the rate of the term "Recession." In the second panel of Online Appendix Figure A2, we compare "Unemployment benefits" to "Earned Income Tax Credit," "Social security," "Welfare," and "TANF." Again, "Unemployment benefits" was one of the more popular search items, with "Social security" being only slightly more popular on average.

Finally, in order to compare the absolute popularity of "Unemployment benefits" to non-economics terms, in the third panel, we include the search terms "Disneyland," "Eiffel Tower," "Wall-E," and "Summer camp." Once again, "Unemployment benefits" was one of the more popular search items during this time period. Wall-E was one of the most popular movies in 2008; during the week of Wall-E's peak search-popularity in June of 2008, people still searched for "Unemployment benefits" at roughly 20% the frequency of "Wall-E" (i.e. for every five searches for "Wall-E," there was one search for "Unemployment benefits"). Searches for "Summer camp" are unsurprisingly cyclical, yet during the summer of 2009, these searches seldom exceeded searches for "Unemployment benefits." During the first EUC change and the ARRA period, search volume for "Unemployment benefits" is comparable to "Disneyland." Searches for "Unemployment benefits" roughly double the amount of searches for "Eiffel Tower," despite the Eiffel Tower being the fifth

[56] Though the Google Trend's scale cannot be mapped into total search volume on Google Search, estimates do exist on the popularity of Google Search overall. For instance, roughly 3.5 billion searches are made per day. From 2008 to 2009, there were nearly 1.4 trillion total searches made on Google Search. Source: WordStream, Online, *access 31 Jul. 2019*.

most searched item on Google Maps.

To further understand workers' awareness of UI benefits during the Great Recession, we also examine polls that were conducted during these years. Since 2001, Gallup has surveyed Americans about their top concerns (e.g., crime and violence, drug use, hunger and homelessness, the economy, unemployment).[57] In March 2008 (six months before Lehman Brothers went bankrupt), 36% of respondents answered that they worry a great deal about unemployment. By March 2010, this had increased to 59%. Those worrying a great deal remained above 50% for the next three years and then steadily declined to 23% in 2018. Thus, UI benefit extensions came during a time when Americans were highly concerned about unemployment. In a poll more closely related to UI extensions, YouGov/Huffington Post surveyed 1000 U.S. adults in April 2014 about unemployment benefits extensions.[58] When asked—"How much have you heard about Congress letting unemployment benefits expire for people who have been unemployed more than six months at the end of last year?"—23% responded that they had heard a lot, 45% had heard a little, and 32% had heard nothing at all. This poll provides suggestive evidence that a majority of Americans had some level of awareness about extended UI benefits.

[57] Source: Gallup, Online, *accessed 3 May 2018*.

[58] Source: *YouGov.com*, Poll Results: Unemployment, April 18–21, 2014, Online, *accessed 3 May 2018*.

Appendix 4: Length of unemployment spells

The PBD extensions that we exploit in our analysis will only directly affect unemployed workers who remain unemployed for longer than 46 weeks. Thus, we should only expect a shirking response to these extensions if workers believed that there was a meaningful chance of suffering an unemployment spell longer than 46 weeks.

From the basic CPS monthly files for the months in our scanner data sample (December 2008 to February 2011), we extract a sample of 4,031 unemployed adult workers who resided in the Washington D.C. metropolitan area. The average duration of unemployment at the time of the survey was 29 weeks with a median of 18 weeks, while the 75th percentile of the distribution of unemployment duration was 43 weeks.[59] The lengths of unemployment spells are increasing drastically during this time (e.g., the overall mean increases from 24 weeks in the first half of our sample to 34 weeks in the second half) and this is consistent with what is seen nationally.[60] These estimates of unemployment durations are based on unadjusted samples of the stock of unemployed workers, and may be underestimating the true length of the unemployment spell due to right censoring (Kiefer, Lundberg and Neumann, 1985). Thus, it is reasonable to conclude that a low-skilled worker in the Washington D.C. metro area during the time period of our sample would have been concerned with the possibility of long term unemployment.

[59] Relevant statistics split by various subsamples are also plotted in Online Appendix Figure A4.

[60] According to the Federal Reserve Economic Data (FRED), national mean unemployment durations nationally increased from 20 weeks to 37 weeks during our sample. (US Bureau of Labor Statistics, Average Weeks Unemployed [UEMPMEAN], retrieved from FRED, Federal Reserve Bank of St. Louis; Online, *accessed 15 Mar. 2020./*)

Appendix 5: Additional Tables and Figures

Table A1: Summary statistics from ATUS sample (N=30,094)

Worker-level variable	**Mean**	**Std. Dev.**
Age (in years)	40.352	12.395
Female	0.462	0.499
Race:		
- White	0.834	0.372
- Asian	0.029	0.169
- Black	0.115	0.319
Born in the US	0.918	0.275
Works in private sector	0.831	0.375
Occupation sector:		
- Management occupations	0.111	0.314
- Sales and related occupations	0.101	0.301
- Office and administrative support	0.151	0.358
Works part time	0.121	0.326
Usual number of weekly hours	41.732	9.173
Weekly earnings (in $)	900.487	1694.676
Paid hourly (not salary)	0.454	1.635
Number of minutes at the workplace:		
- Not working (shirking)	31.833	37.55
- Working	478.613	139.776

Notes: American Time Use Survey (ATUS) data initially collected at the respondent-activity level from the years 2003 to 2014, then collapsed to the respondent level. Observation weights provided by ATUS.

Table A2: Estimated UI eligibility in Mas and Moretti (2009) sample

	mean	sd	min	max
Total hours to date	1347.01	903.61	0.0028	4414.14
Shift length (hours)	6.18	2.99	0.0008	16.08
Tenure to date (days)	384.43	189.91	0.0000	748.00
UI Eligibility Rate:				
- DC 2008	0.83	0.37	0.0000	1.00
- DC 2009	0.84	0.37	0.0000	1.00
- DC 2010	0.84	0.37	0.0000	1.00
- DC 2011	0.84	0.37	0.0000	1.00
- MD 2008	0.80	0.40	0.0000	1.00
- MD 2009	0.81	0.39	0.0000	1.00
- MD 2010	0.81	0.39	0.0000	1.00
- MD 2011	0.81	0.39	0.0000	1.00
- VA 2008	0.74	0.44	0.0000	1.00
- VA 2009	0.77	0.42	0.0000	1.00
- VA 2010	0.79	0.41	0.0000	1.00
- VA 2011	0.79	0.41	0.0000	1.00
N cashiers	412			
N cashier-shifts	55205			

Notes: This information is based on a subset of the data used in Mas and Moretti (2009) which includes every transaction for 6 stores in the same metropolitan area of the Western Census region between (roughly) 2004 and 2006. After estimating cumulative hours worked at the cashier-shift level, we drop managers from the sample and estimate UI eligibility in our state-years for all cashier-shifts worked in a store that had been in the sample for 3 or more calendar quarters.

UI eligibility rules vary by state and are based on earnings histories in the location of employment, not residence. The UI eligibility rules in our sample are as follows (Source: Department of Labor, Online, *accessed 14 Sep. 2018*): In Maryland, \$900 in wages in the first four of the last five completed calendar quarters, with $\geq$\$576 in the highest earning of those quarters, and $>$\$0 in wages in two of those quarters; In Virginia, \$2,700 in wages in either the first four or the last four of the last five completed calendar quarters, with $\geq$\$2,700 in wages during the highest two earning of those quarters; In Washington D.C., \$1,950 in wages in either the first four or the last four of the last five completed calendar quarters, and either $\geq$\$1,300 in the highest earning of those quarters or $\geq$\$1,950 in the two highest earning of those quarters.

These estimates are likely to be conservative since a cashier's first day in the Mas and Moretti (2009) sample is likely not their first day at the retailer, a cashier may have relevant earnings from other employers, and a cashier may earn more than the minimum wage.

Table A3: Results with data collapsed to cashier-register-day level

	Avg.(Transaction Length)		ln(Avg.(Transaction Length))	
	(1)	(2)	(3)	(4)
Potential benefit duration	0.187	0.184		
	(0.064)	(0.078)		
18-week PBD increase			0.019	0.017
			(0.009)	(0.011)
Total expenditures	0.082	0.060	0.000	-0.000
	(0.073)	(0.073)	(0.000)	(0.000)
Total items scanned	2.875	2.710	0.011	0.010
	(0.296)	(0.318)	(0.002)	(0.002)
Price discounts/coupons	0.559	0.679	0.005	0.005
	(0.135)	(0.146)	(0.001)	(0.001)
Local UE rate (prior month)	-0.699	-0.814	-0.010	-0.010
	(0.721)	(0.743)	(0.004)	(0.004)
State UE rate (prior month)	-1.403	-0.583	-0.002	0.002
	(2.081)	(2.236)	(0.013)	(0.013)
Observations	30179	27279	30121	27218
Controls	X	X	X	X
Date FE	X	X	X	X
Register X Store FE	X		X	
Cashier X Store FE	X		X	
Cashier X Register X Store FE		X		X

Notes: Potential benefit duration is measured in weeks. Transaction expenditure is measured in dollars. Controls for each regression include indicators for whether the transaction included items from particular departments (e.g., alcohol), an indicator for whether a plastic bag tax was in place at the store, the total number of registers open during the transaction, the cashier's experience as measured by total number of career transactions completed, the cashier's "fatigue" as measured by the number of transactions the cashier had previously completed on that shift, and the cashier's length of shift measured in both number of transactions and in minutes. "Date" refers to exact date (e.g., August 3, 2017). Standard errors, shown in parentheses, are clustered at state by date level.

Table A4: Main results – Sensitivity to cashier controls and ignoring/dropping EUC=0 weeks

	Without Cashier Controls		Ignore EUC = 0		Drop EUC = 0	
	(1)	(2)	(3)	(4)	(5)	(6)
Potential benefit duration	0.174	0.175	0.133	0.130	0.133	0.146
	(0.058)	(0.068)	(0.047)	(0.056)	(0.048)	(0.058)
Total expenditures	0.133	0.134	0.134	0.134	0.135	0.134
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Total items scanned	2.854	2.852	2.821	2.825	2.848	2.852
	(0.053)	(0.052)	(0.052)	(0.052)	(0.054)	(0.054)
Price discounts/coupons	0.730	0.735	0.720	0.726	0.717	0.723
	(0.024)	(0.024)	(0.024)	(0.024)	(0.025)	(0.025)
Local UE rate (prior month)	-0.687	-0.596	-0.678	-0.661	-0.516	-0.567
	(0.493)	(0.535)	(0.429)	(0.463)	(0.442)	(0.485)
State UE rate (prior month)	-2.542	-2.845	-1.537	-1.354	-1.568	-1.217
	(1.389)	(1.503)	(1.196)	(1.268)	(1.225)	(1.293)
Observations	515618	515433	515618	515433	471826	471647
Controls	X	X	X	X	X	X
Date FE	X	X	X	X	X	X
Register X Store FE	X		X		X	
Cashier X Store FE	X		X		X	
Cashier X Register X Store FE		X		X		X

Notes: Potential benefit duration is measured in weeks. Transaction expenditure is measured in dollars. Controls for each regression include indicators for whether the transaction included items from particular departments (e.g., alcohol), an indicator for whether a plastic bag tax was in place at the store, number of households participating in SNAP per state-month, the total number of registers open during the transaction, the cashier's experience as measured by total number of career transactions completed, the cashier's "fatigue" as measured by the number of transactions the cashier had previously completed on that shift, and the cashier's length of shift measured in both number of transactions and in minutes. "Date" refers to exact date (e.g., August 3, 2017). Standard errors, shown in parentheses, are clustered at state by date level.

Table A5: Main results by various standard error clusters

	State-Date		State-Month		Cashier		Cashier-Register		Store	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Potential benefit duration	0.133	0.130	0.133	0.130	0.133	0.130	0.133	0.130	0.133	0.130
	(0.047)	(0.056)	(0.043)	(0.046)	(0.055)	(0.059)	(0.048)	(0.053)	(0.071)	(0.076)
Total expenditures	0.134	0.134	0.134	0.134	0.134	0.134	0.134	0.134	0.134	0.134
	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.013)	(0.012)	(0.012)	(0.021)	(0.020)
Total items scanned	2.821	2.825	2.821	2.825	2.821	2.825	2.821	2.825	2.821	2.825
	(0.052)	(0.052)	(0.073)	(0.072)	(0.050)	(0.050)	(0.047)	(0.047)	(0.072)	(0.072)
Price discounts/coupons	0.720	0.726	0.720	0.726	0.720	0.726	0.720	0.726	0.720	0.726
	(0.024)	(0.024)	(0.035)	(0.034)	(0.021)	(0.021)	(0.020)	(0.020)	(0.033)	(0.034)
Local UE rate (prior month)	-0.678	-0.661	-0.678	-0.661	-0.678	-0.661	-0.678	-0.661	-0.678	-0.661
	(0.429)	(0.463)	(0.579)	(0.633)	(0.378)	(0.422)	(0.347)	(0.400)	(0.418)	(0.531)
State UE rate (prior month)	-1.537	-1.354	-1.537	-1.354	-1.537	-1.354	-1.537	-1.354	-1.537	-1.354
	(1.196)	(1.268)	(1.175)	(1.102)	(1.355)	(1.453)	(1.187)	(1.342)	(2.201)	(2.634)
Observations	515618	515433	515618	515433	515618	515433	515618	515433	515618	515433
Clusters	339	339	36	36	1,966	1,963	8,370	8,185	39	39
Controls	X	X	X	X	X	X	X	X	X	X
Date FE	X	X	X	X	X	X	X	X	X	X
Register X Store FE	X		X		X		X		X	
Cashier X Store FE	X		X		X		X		X	
Cashier X Register X Store FE		X		X		X		X		X

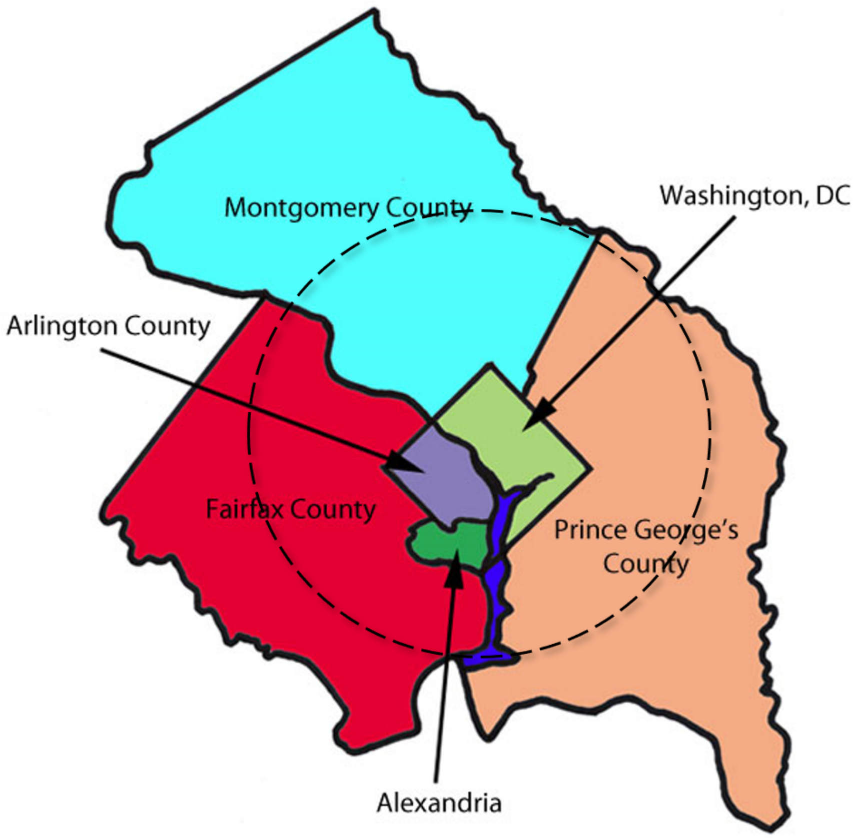
Notes: Potential benefit duration is measured in weeks. Transaction expenditure is measured in dollars. Controls for each regression include indicators for whether the transaction included items from particular departments (e.g., alcohol), an indicator for whether a plastic bag tax was in place at the store, number of households participating in SNAP per state-month, the total number of registers open during the transaction, the cashier's experience as measured by total number of career transactions completed, the cashier's "fatigue" as measured by the number of transactions the cashier had previously completed on that shift, and the cashier's length of shift measured in both number of transactions and in minutes. "Date" refers to exact date (e.g., August 3, 2017). Standard errors are shown in parentheses.

Table A6: Results from American Time Use Survey (ATUS) - Minutes spent at workplace

	Minutes spent at workplace			
	(1)	(2)	(3)	(4)
18-week PBD increase	0.2209	1.0525	0.9007	1.1862
	(1.3582)	(2.9187)	(1.4834)	(2.5087)
State UE rate (prior month)			-0.6143	-0.6460
			(0.9657)	(0.9530)
Maximum WBA (100s)			-3.7401	-3.8411
			(2.9008)	(3.0414)
Observations	30094	30094	30094	30094
Mean of Y	510.4462	510.4462	510.4462	510.4462
State FE	X	X	X	X
Month FE	X		X	
Year FE	X		X	
Month-Year FE		X		X
Controls			X	X

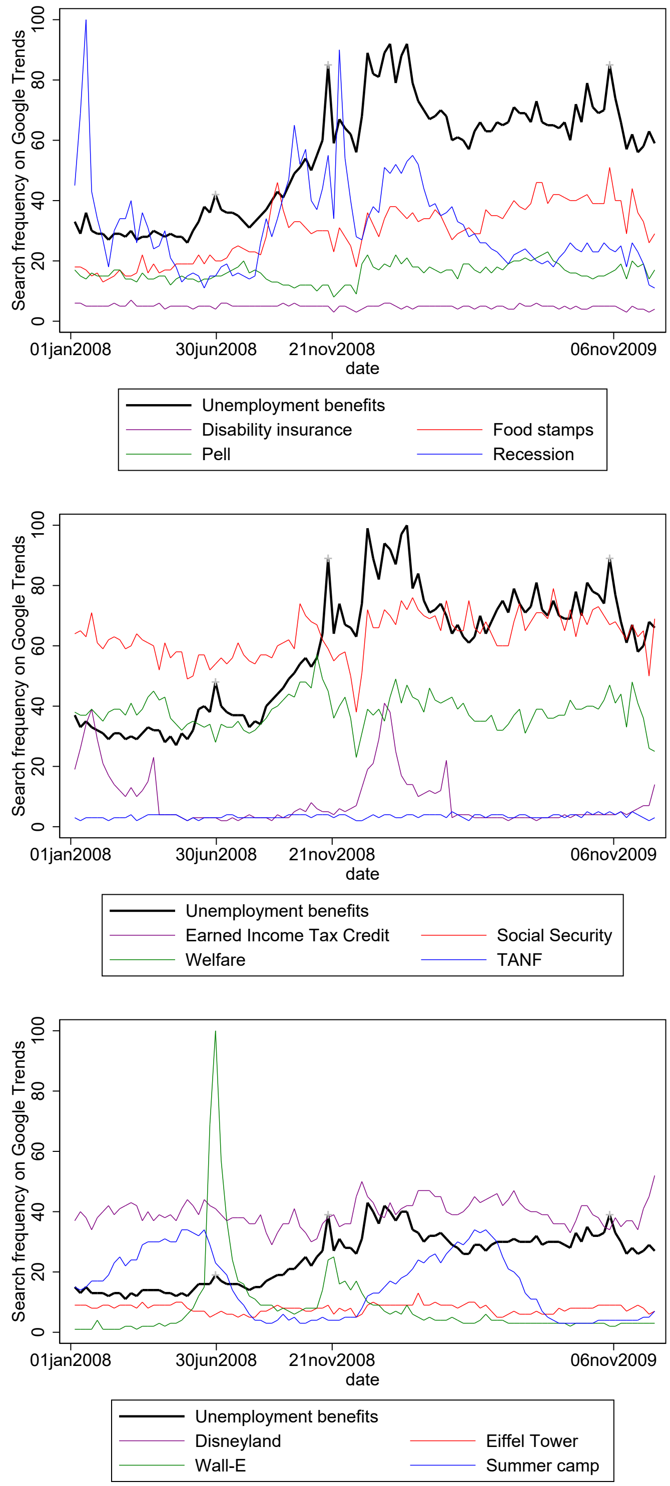
Notes: Controls include state unemployment rate and maximum UI benefits (in dollars), the individual's age, "usual" amount of hours worked per week, weekly earnings, hourly wage, and dummies for family income, gender, race, US citizenship, whether the individual had multiple jobs, class of worker (e.g., federal government vs. state government vs. private for profit), and general occupational category (e.g., "sales and related occupations" vs. "health-care support occupations"). Observations weighted according to ATUS probability weights. Standard errors, shown in parentheses, are clustered at state level.

Figure A1: Map of the Washington DC Metropolitan Area



Notes: This figure provides a stylized map of the Washington DC Metropolitan area. The circle represents the area in which the 39 stores in the scanner data sample are located. Montgomery & Prince George's Counties are in Maryland. Arlington County, Fairfax County, and the City of Alexandria are in Virginia.

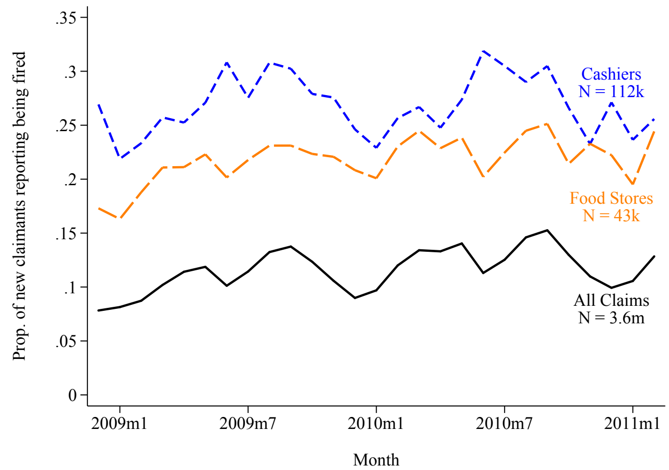
Figure A2: Searches on Google via Google Trends - “Unemployment benefits” vs. other searches



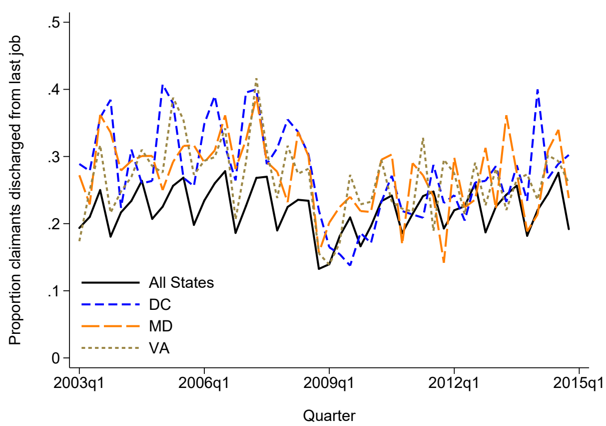
Notes: Google Trends data retrieved from Google Inc. Each panel reports a five-item query for “Unemployment benefits” (in black) vs. four other items for our time period (January 2008 - December 2009). For each term-week, Google Trends first calculates the ratio of the term’s search volume to the total number of searches (i.e. an absolute search measure for that term-week). Then, Google Trends proportionally scales all ratios across weeks and the five queried search terms to a [0,100] scale. So, within a given week, the ratio of two indices reveals the ratio of search frequency between two terms. For example, during the week of “Wall-E”’s peak search popularity, there were roughly five times the amount of searches for “Wall-E” than there were “Unemployment benefits.”

Figure A3: Time series of UI claimants

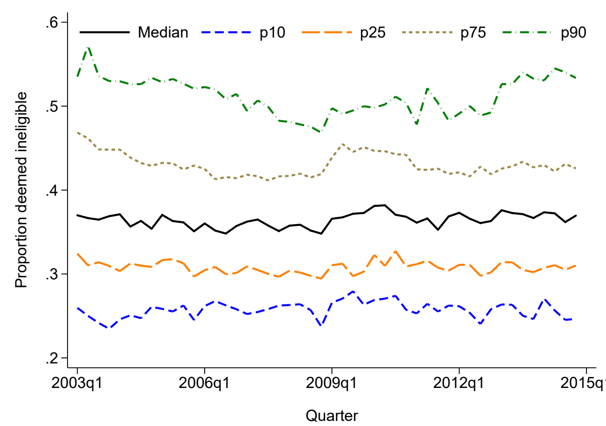
(a) Fired workers regularly file for UI (*Administrative UI claims data from California*)



(b) Over 20% of accepted UI claimants were fired (*Benefit Accuracy Measurement data*)



(c) Most claimants with misconduct determinations still receive UI (*US Department of Labor*)



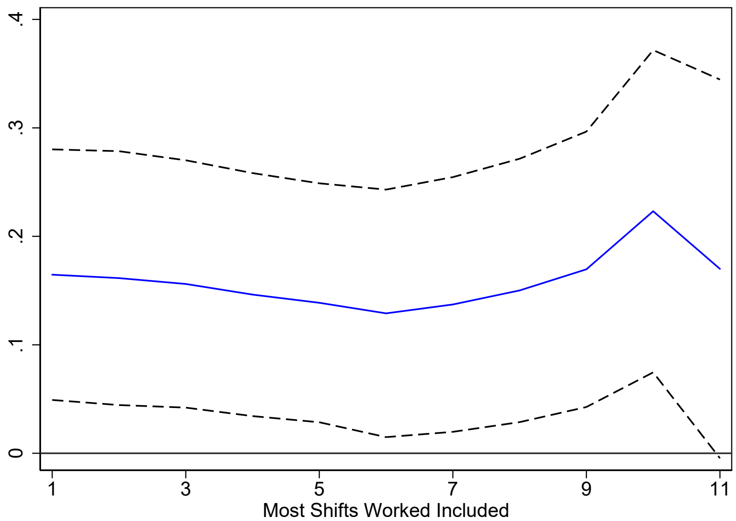
Notes: Panel (a) utilizes administrative UI claims microdata acquired from the California Employment Development Department. The graph depicts the proportion of claims filed in CA during the time period of our scanner data sample, in which the claimant was fired from their last job. Panel (b) graphs the proportion of claimants who were fired from their last job in the Department of Labor's (DOL) Benefit Accuracy Measurement (BAM) data. The BAM program audits a randomly selected subsample of claimants receiving UI benefits in every state-week. Claims with missing separation reasons are excluded from panels (a) (39%) and (b) (0.4%). Panel (c) graphs variation across state-quarters in the proportion of misconduct determinations resulting in a denial of benefits from the DOL Employment and Training Adminstration's report 207. As described in section 3.3, all claims by discharged workers result in such a determination.

Figure A4: Distribution of unemployment durations in CPS sample

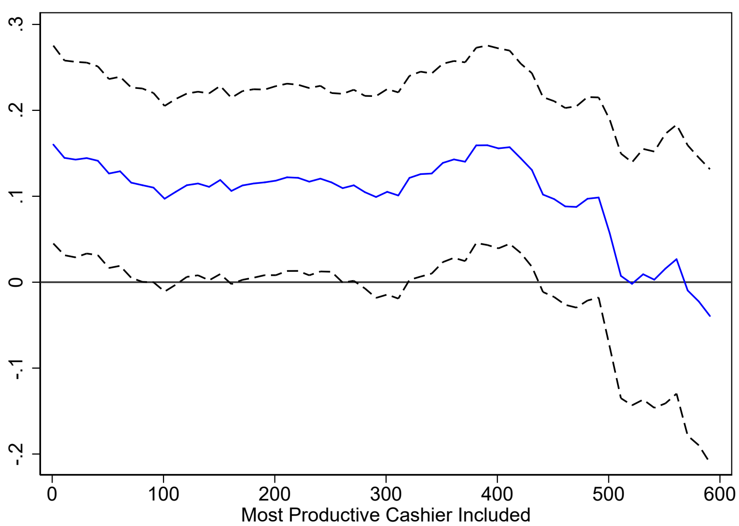
Notes: Each panel plots the distribution of unemployment spells for a cross-section of workers from the CPS monthly files for the months in our cashier sample (December 2008 to February 2011) who resided in the Washington D.C. metropolitan area. Jumps in distribution roughly correspond to (self-reported) unemployment spells of one year and two years.

Figure A5: The effect of PBD on transaction duration by cashier subsamples

(a) Cashier experience - Number of shifts worked

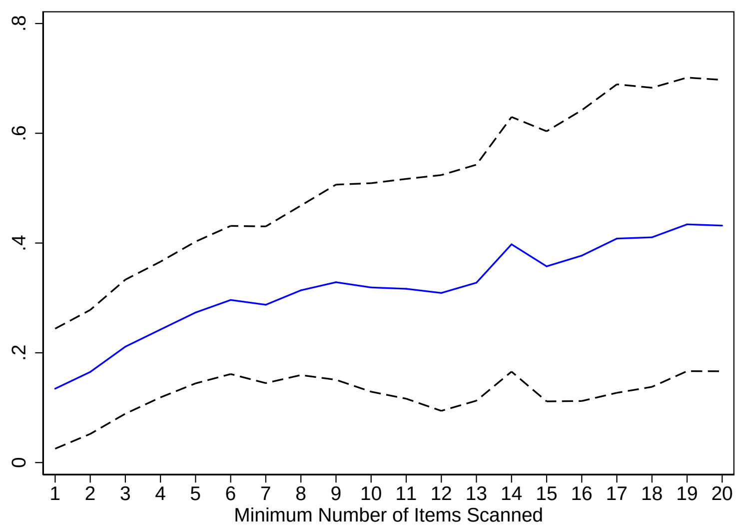


(b) Cashier productivity - Ranking of cashier pre-policy fixed effect



Notes: Point estimates (solid line) and 95% confidence intervals (dashed) for estimates of the effect of PBD on transaction duration from our fully specified model (cashier-register and day fixed effects, and controls) across numerous specifications. Each model is estimated in a different subgroup restricted to transactions completed by particular cashiers. In panel (a), starting with the full sample on the left (cashiers who worked at least 1 shift before the first policy change), estimates increase slightly as we focus on cashiers who worked, at a minimum, a higher number of shifts. In panel (b), starting with the full sample of cashiers on the left (where higher rankings correspond to higher productivity), estimates decrease as we focus on cashiers with higher rankings of pre-policy productivity.

Figure A6: Does the effect of PBD on transaction duration vary with transaction size?



Notes: Point estimates (solid line) and 95% confidence intervals (dashed) for roughly 20 estimates of the effect of PBD on transaction duration from models with cashier-register fixed effects and controls. Each model is estimated in a different subgroup restricted to transactions that included more than a certain number of items.