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

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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 job search (e.g. Katz and Meyer, 1990; Meyer, 2002; Schmieder et al., 2016) by exploiting these expansions (Rothstein, 2011; Farber et al., 2015; Farber and Valletta, 2015; Marinescu, 2017). This *ex-post* moral hazard effect has both empirical and theoretical importance given its central role in the widely used Baily-Chetty formula for the calculation of optimal UI benefit levels (Baily, 1978; Chetty, 2006).

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). 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.

To date, only a handful of studies have tested this theoretical prediction. A working paper by Burda et al. (2016) uses 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[1] paper on this question focuses on self-employed workers in Denmark, which has a relatively unique, voluntary UI system (Ejrnæs and Hochguertel, 2013).[2] They 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.[3]

[1]Theoretical work is more common where, for example, it has been shown to be a source of market failure in private UI markets (Chiu and Karni, 1998).

[2]Related empirical work has documented that inflows into unemployment spike when UI eligibility is obtained (Christofides and McKenna, 1995, 1996; Green and Sargent, 1998; Rebello-Sanz, 2012) and when benefit levels increase (Winter-Ebmer, 2003; Jäger et al., 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 the existence of an ex-ante moral hazard effect.

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

This paper aims to fill this gap in the literature by matching plausibly exogenous changes in the potential benefit duration (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[4] 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.[5] Following earlier papers using nearly identical data (Mas and Moretti, 2009; Taylor, 2017), we measure productivity as the time-length of checkout transactions processed by cashiers.[6] 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 would make them UI eligible in our state-years.[7] We argue that the typical cashier in our sample is therefore also very likely to have been UI eligible.

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

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

[5]An important point detailed further in Section 4 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.

[6]Investigating the impacts of discrimination in the workplace, Glover et al. (2017) also observe worker productivity as measured by length of supermarket cashier transactions.

[7]This data is from Mas and Moretti (2009) and includes the information necessary to estimate UI eligibility at the cashier-shift level for cashiers in a different state several years prior to our sample. Our dataset does not include this information for our cashiers.

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 2.43 seconds in transaction length for cashiers who experienced an 18-week increase in PBD.[8] With a mean transaction length of just under two minutes, this is roughly equivalent to a two percent decrease in worker productivity.[9] 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 (2017) 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.[10] Assuming a $14 hourly wage for grocery store cashiers in the US, which is the median, this would cost each store in our sample $2,016 per year in additional wages, for an estimated total cost of $6.1 million per year in the DC area.

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 any concerns regarding changes in cashier composition in response to increases in PBD levels.[11] Similarly, register fixed effects account for potential shifts in the use of different registers (e.g. express registers). Consumers' purchases do weakly respond to PBD levels, but these effects generally work in the opposite direction of our estimates. For instance, during higher PBD periods, we find that consumers buy fewer and cheaper goods per trip, and are offered fewer price discounts—all of which are generally associated with shorter transaction lengths. However, while customers purchase fewer items, we find no evidence that they make fewer (or more) shopping trips during periods with higher PBD. 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.

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 et al. (2016). Specifically, we use the ATUS to test whether the percentage of time spent at work doing non-work activities reported is affected by increased PBD levels. The ATUS is a repeated cross-sectional survey which measures the amount of time people spend doing precise activities (e.g. eating at work, childcare, socializing). Utilizing

[8]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.

[9]With a sample standard deviation just over 100 seconds, this effect is also roughly equivalent to a 0.024 standard deviation decrease in worker productivity.

[10]Additional details on these calculations are provided in the results section of the paper.

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

PBD changes across the US from 2003 to 2014, and estimating models with state fixed effects, month-year fixed effects, and state time trends, we estimate a precise increase in shirking in the ATUS sample. For our fully specified model, off a mean of 6.68%, we estimate a 0.34 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 UI benefit changes affect the social costs of UI, and therefore have important consequences for the welfare effects of UI benefit changes (Chetty, 2006). Second, they contribute to the relatively small base of empirical evidence for ex-ante moral hazard effects in *any* type of insurance.[12] 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 effect of the unemployment rate and unemployment benefits on effort) (Lazear et al., 2016). Our results provide evidence that worker effort varies over both the business cycle and corresponding policy response, rising with the unemployment rate and falling with UI generosity.

The remaining sections of this paper are as follows. Section II outlines a simple theoretical model for a worker's choice of on-the-job effort. Section III describes the data. Section IV describes our empirical specification and the quasi-experimental variation in UI benefits that we exploit. Section V details our results and section VI concludes.

2 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 section 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.

[12] 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 et al. (2017) document increases in injury length and subsequent take-up of workers compensation in response to increased benefits.

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

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

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:

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

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:

$$p''(e)(U(C_u) - U(C_e)) + 2p'(e)U'(C_e) + (1 - p(e))U''(C_e) \equiv S(\cdot) \tag{3}$$

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

$$\frac{\partial e^*}{\partial C_u} = -\frac{p'(e)U'(C_u)}{S(\cdot)} \tag{4}$$

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 equilibirum 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 will estimate. It is at least possible for both employers and customers to foresee changes in worker effort provision in response to UI benefit changes. Later, 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).

3 Data Sources and Background

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[13] for 39 stores in the District of Columbia (DC) metropolitan area (a roughly 25 mile radius around DC), including 8 store in DC, 17 stores in Maryland, and 14 stores in Virginia.[14] These data—which span from December 2008 until February 2011—were originally obtained by Taylor (2017) to study how the 2010 disposable carryout bag tax in DC affected the transaction time of supermarket checkout.[15] These data are ideal for our research question for two major 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, much like other studies using the same data source (e.g., Mas and Moretti, 2009; Taylor, 2017). Specifically, for each checkout transaction in our sample, we have information on when and where a checkout transaction

[13]There are over 2000 locations of this supermarket chain across the U.S. With revenue over $35 billion per year, this chain is one of the 15 largest retailers in the U.S.

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

[15]The SEIPR-Giannini Data Center archives and documents existing datasets from this supermarket chain (Online, *accessed 24 Sep. 2018*).

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. It is important to note 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.[16] Since there is only one time-stamp per transaction, having peak hours enables us to calculate transaction time by making the assumption that transactions in the scanner data occur back-to-back, with little or no downtime in between. Taylor (2017) 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.[17] 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. 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.

3.1 Summary statistics for scanner data

Table I 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.[18] 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 contains more shelf-stable,

[16] We drop transactions occurring at self-checkout registers because only 15 percent of stores have self-checkout during the sample period, and these lanes are not manned by an individual cashier.

[17] 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.

[18] The cost of the transaction is created by summing up the individual amounts paid per item in a transaction. This variable does not include sales tax.

fresh produce, and dairy/refrigerated items than frozen, meat/seafood, and alcohol/tobacco items. There are notable differences between the states in the average number of alcohol/tobacco items purchased, which reflects differences in state laws permitting grocery stores to sell alcohol.

Panel B of Table I 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 for measures of store age and store size. Stores in all three states are similar with respect to their year opened, year last remodeled, and store building/selling size. However, stores in DC have more registers and more cashiers than stores in Maryland and Virginia.

3.2 Variation in cashier productivity with experience

While the average transaction in our sample takes a cashier 120 seconds to complete (as shown in Table I), cashier productivity varies greatly with experience. Consistent with the concept of learning-by-doing, there is a noticeable increase in the productivity of cashiers as they gain experience within a given shift. Figure I depicts this learning curve as the average percent change in cashier productivity from the first week of tenure to all subsequent weeks. We find that between the first and second week cashiers work the 5:00-6:00pm Saturday shift, cashiers become 5% faster in completing a transaction. By week 8 they are roughly 10% faster than their first week. The quickening of checkout duration continues at a diminishing rate. By week 25 of working the 5:00-6:00pm shift, the reduction in speed ceases and cashiers remain approximately 15% faster than their first week.[19] The learning curve presented in Figure I suggests that experience within a shift may influence the ability of cashiers to shirk, with cashiers that are new—and thus still in the learning process—less able to shirk than cashiers who have mastered their position with on-the-job practice.

[19] We fit these estimates into the conventional form of a learning curve: $T_N = T_1 * N^b$, where T_N is transaction duration for the Nth week of working the 5:00-6:00pm Saturday shift, T_1 is transaction duration in the first week, and $b = \frac{ln(LearnRate)}{ln(2)}$ is the slope of the learning curve (Alchian, 1963; Argote and Epple, 1990). We estimate $T_N = 2.374 * N^{-0.041}$ which corresponds to a learning curve rate of 97.2%. This means that transactions in the second week take 97.2% the time of the first week, and transactions in the fourth week take 97.2% of the second week and so on. In comparison, the 1-year death rate for hospitals performing heart transplants follows a 79% learning curve and the production rate of aircrafts follows a 80% learning curve (Heizer and Render, 2013).

3.3 Cashier's UI eligibility

A key limitation of our data is that we do not observe either wages or the complete history of hours worked (e.g. hours per week, start date) for our cashiers. This is important because an individual's UI eligibility is determined by their earnings history, and so we cannot identify when or whether a cashier in our sample is eligible for UI benefits. In order to provide some suggestive evidence on these questions, we have obtained supplementary data from a different sample of transactions that occurred at our retailer's stores. These data are from Mas and Moretti (2009) and include all transactions in a two-year period for each of six stores between 2003 and 2006 (start dates differ by store). The stores are all in the same Western Census region metropolitan area. Since we observe all transactions for these cashiers during this time period, we can estimate the labor supply history for each cashier assuming that the first day on which they appeared in the sample was their first day of work, and that gaps between transactions of at least four hours are gaps between shifts (as opposed to downtime at work).

UI eligibility rules vary by state and are based on earnings histories in the location of employment, not residence. We apply the UI eligibility rules in each state-year in *our* sample (separately) to a subset of the cashier-shifts in the Mas and Moretti sample to estimate whether or not that cashier would have been UI eligible at the start of that shift in each state-year under the assumption that the cashier was paid the relevant minimum wage. The UI eligibility rules in our sample[20] are as follows:

- <u>Maryland</u>: \$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.
- <u>Virginia</u>: \$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.
- <u>Washington D.C.</u>: \$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.

Table II presents the results from this analysis. The average cashier-shift in our subset of the Mas and Moretti (2009) sample is worked by a cashier with roughly 13 total months and 1400 total hours of experience as a cashier at our retailer.[21] Assuming that these cashiers earned the relevant minimum wage, at

[20] Retrieved from the Department of Labor, Online, *accessed 14 Sep. 2018.*

[21] These estimates apply to 412 unique cashiers and roughly 55,000 cashier shifts. Our sample sizes are not equivalent to those reported in Mas and Moretti (2009) because we apply different restrictions. Specifically, Mas and Moretti (2009) drop transactions completed by managers, transactions completed by new cashiers, and transactions completed outside of 7AM-8PM.

least 74% of these cashiers would be UI eligible in each of our state years. These estimates are likely to be conservative since (a) a cashier's first day in the Mas and Moretti sample is likely not their actual first day at the retailer, (b) a cashier is likely to have work experience at other companies prior to their first day at the retailer, and (c) a cashier may earn more than the minimum wage. If we assume that the behavior of our cashiers was similar to Mas and Moretti (2009) in terms of shift length, shifts worked per week, and tenure, this suggests that the vast majority of transactions observed in our sample were performed by UI eligible workers.

3.4 Additional data - American Time Use Survey (ATUS)

Lastly, in additional analyses, we utilize the American Time Use Survey (ATUS).[22] The primary benefit of utilizing the ATUS is that we can measure shirking 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. ATUS 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 181,335 surveys were conducted between 2003 and 2016.

Our measure of shirking in the ATUS closely follows that of Burda et al. (2016), who investigate the relationship between shirking and state unemployment rates in the ATUS. First, we only focus on the subset of activities that were conducted "at the workplace" by US citizens with a single job aged between 18 and 65 who reported "usually" working at least 20 hours per week.[23] We identify work-related activities as those coded between 50000 and 50299 in the ATUS. 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. We are left with a sample of 30,094 workers after merging this data to UI potential benefit duration levels across the US from 2003 to 2014 (discussed in further detail in the next section).

Table III presents summary statistics for our ATUS sample. Observations are weighted using probability weights provided. The average worker in our sample is 40 years old. Approximately 46% of workers are

After estimating cumulative hours worked at the cashier-shift level, we drop managers from the sample and estimate UI eligibility for *only* those cashier-shifts worked in a store that had been in the sample for three or more calendar quarters. Since we do not observe hours worked prior to the sample, any cashier working in a store with fewer than three completed calendar quarters in the sample is guaranteed to be estimated as ineligible for at least one our state-years as per the UI eligibility rules described above.

[22] Detailed information on the American Time Use Survey can be found online (*accessed 14 Sep. 2018*).

[23] We also drop self-employed workers, since these individual are ineligible for UI.

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.

4 Identification Strategy

In this section, we first detail the expansions in Unemployment Insurance (UI) potential benefit duration (PBD) during the period of our study. Then, we investigate awareness of the PBD expansions using Google Trends and national polls, and the relevancy of the PBD expansions to unemployed individuals in the CPS. Lastly, we introduce our econometric specifications and discuss potential threats to our identification strategy.

4.1 Unemployment Insurance benefit extensions

Normally limited to between 26 and 30 weeks (depending on the state)[24], the PBD of UI benefits in the United States is regularly extended during economic downturns. During the period of our study these extensions were driven by three separate programs, the Extended Benefits (EB) program, the Emergency Unemployment Compensation (EUC) program, and the Temporary Extension of Unemployment Compensation (TEUC) program. 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.[25]

The changes in PBD due to these programs for the state-weeks in our scanner data sample as per these reports are depicted in Table IV and Figure II. Figure III depicts similar information for the state-months used in our ATUS analysis. Data for the ATUS sample extensions were collected from Farber et al. (2015) and Rothstein (2011). In each of these figures the PBDs shown are those available to new claims filed on a given date. PBD variation resulting from the EB, EUC, and TEUC programs have been utilized as identifying variation in a series of recent studies on the effects of UI benefit generosity (Farber et al., 2015; Rothstein, 2011; Marinescu, 2017; Farber and Valletta, 2015; Boone et al., 2016; Chodorow-Reich et al., Forthcoming). More detailed descriptions of these programs and related legislation can be found in these studies or in our Appendix 1.

[24] Similar to rules for UI eligibility described in Section 3.3, PBD is determined by the state of employment, not residence.

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

Here we emphasize the key point that the extensions that we exploit for identifying variation occur for one of three reasons: (1) a state's unemployment rate (specifically its average IUR over the past 13-weeks or 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) changes the trigger value to a level below the state's current 13-week Insured Unemployment Rate (IUR) or 3-month Total Unemployment Rate (TUR), or (3) the federal government allows the (EUC or TEUC) program to temporarily or permanently expire. Notably, only 2 of the 13 separate PBD changes specifically exploited in our scanner data sample (shown in Table IV and Figure II) occurred for the first of these reasons, i.e. as a direct result of changes to the relevant state's unemploment rate. 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 plausibly problematic for our design. This is further discussed in Section 4.5.2.

4.2 Awareness of UI benefit extensions

A necessary condition for the existence of a shirking effect in response to unemployment insurance is an awareness by the worker of their unemployment benefits. While it is likely that most workers in the United States are aware of UI, it could be the case that the above expansions enacted during the Great Recession went unnoticed. To investigate this possibility, we first 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 (for us, the US) and time period (January 2008 - December 2009), indexed to a range of 0 and 100.

Figure IV plots these trends. For the search item "Unemployment Benefits," we notice three jumps in search frequency that correspond to when the Emergency Unemployment Compensation (EUC) program was enacted (June 30, 2008) and subsequently adjusted (November 21, 2008 and November 6, 2009); 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 IV, 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 "Emergency Unemployment Compensation" within our sample, while the week after the enactment of the EUC program carried the fourth largest search volume overall. 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.

To further understand workers' awareness of unemployment insurance 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).[26] 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.[27] 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.[28] This poll provides suggestive evidence that a majority of Americans had some level of awareness about extended benefits.

4.3 Length of unemployment spells in the CPS

The PBD extensions that we exploit in our analysis will only directly affect unemployed workers who remain unemployed for longer than 46 weeks. In addition to being aware of a given PBD extension, a cashier in our sample must think that those extensions matter for them. In other words, they must believe that, in the event of their job loss, there is some nonzero chance that they will remain unemployed for longer than 46 weeks. We cannot provide direct evidence of the relevant expectations for the exact cashiers in our sample, but we can provide some supporting evidence for these assumptions from a sample of similar workers.

From the basic CPS monthly files for the months in our 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 (split across DC, MD, and VA), and plot the distribution of unemployment spells in Figure V. For this sample, the average duration of unemployment at the time of the survey was 29 weeks with a median of 18 weeks. The 75th percentile of the distribution of unemployment duration was 43 weeks. Limiting the sample only to cashiers (N=173) or a set of "similar"[29] occupations (N=508) does not meaningfully change

[26]Gallup asks, via phone survey, the following question: "I'm going to read a list of problems facing the country. For each one, please tell me if you personally worry about this problem a great deal, a fair amount, only a little, or not at all?" (Source: Gallup, Online, *accessed 3 May 2018*).

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

[28]Respondents were also asked whether unemployment benefit extensions should expire or be extended further. 46% responded that benefits should be extended, 32% responded that benefits should expire, and 22% weren't sure.

[29]Occupation codes similar to cashier include retail sales, tellers, customer service representatives, hotel or motel desk clerks,

these numbers. Limiting the sample to workers with educational attainment of a high school degree or less increases unemployment durations slightly (by approximately two to four weeks at each of the aforementioned points in the distribution). 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.[30] Unemployment spells are also generally 2-5 weeks longer (at each of the aforementioned points in the distribution) for the 2,360 workers located in Washington DC proper.

It is important to note that these estimates of unemployment durations are all based on unadjusted samples of the stock of unemployed workers and are therefore likely to be biased upward. However, these descriptive statistics do suggest that a meaningful number of unemployed workers in our state-years of interest had been unemployed for more than 46 weeks. It is reasonable to conclude that a rational, recently unemployed, 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.

4.4 Econometric Specification

Our primary specification estimates the following equation:

$$\text{TransactionLength}_{tdcrs} = \beta \text{UIPBD}_{ds} + \lambda_d + \lambda_{crs} + \gamma X_{tdcrs} + u_{tdcrs} \tag{5}$$

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. If cashiers shirk in response to more generous benefit durations, then we would expect β to be positive.

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, in the absence of cashier fixed effects, our estimates would be biased in the opposite direction of the expected shirking effect if more-productive workers were

receptionists, secretaries and administrative assistants, typists, and general office clerks.

[30] According to FRED, mean unemployment durations nationally increased from 20 weeks to 39 weeks during our sample.

employed relatively more frequently during higher levels of PBD. Transaction-level controls X_{tdcrs} include the total price paid on the transaction as measured in dollars, the total number of items in the transaction, 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, and 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).

4.5 Threats to identification

Given our two-way fixed effect specification, potential threats of endogeneity bias in estimating β come from time-varying factors that are correlated with both PBD and transaction length. In each of three subsections below, we address potential (a) changes in consumer purchases/composition, (b) time-varying cashier changes, and (c) other policy changes that may influence transaction length.

4.5.1 Changes in consumer purchases

Perhaps the most obvious concern for an omitted variables bias comes from changes in purchases during differing PBD levels. Given higher levels of PBD are (partly) triggered by higher state unemployment rates, it may be that during periods with higher unemployment, consumers shifted their purchasing behavior in a way that led to increases in transaction length. This would occur if, for instance, consumers take fewer trips to the grocery store, but end up purchasing more goods during their visit. Consumers may also increase their "price-consciousness" during higher PBD periods and seek out coupons and price discounts, and these characteristics could lead to increases in transaction length. It could also be the case that consumers buy more of certain types of goods during high PBD periods which take longer to scan.

In Table V, we test for each of these possibilities by collapsing our data to the store-day level and regressing a series of characteristics on PBD, conditional on month-year and store fixed effects. From the first two cells, we immediately see that consumers are *not* buying more per visit, but instead are purchasing less, both in terms of total dollars and number of items, during periods with higher PBD levels. The coefficient on price is statistically insignificant, while the coefficient on items scanned is significant at the 10% level. Shown later, both of these factors are positively associated with transaction length (more expensive, larger transactions take longer to complete), and so absent these controls, our estimates for β would be biased in the opposite direction of a shirking effect.[31]

We also do not find statistically significant changes in the number of transactions processed or 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, then there might be gaps without customers that would make transactions appear longer. However, we do not find a reduction in the number of transactions, nor do we find a reduction in the number of registers open, which we would expect if stores were experiencing fewer customers. Thus, we find no evidence that PBD extensions led to fewer customers and more downtime. 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, we find no statistically significant changes in the usage of price discounts during higher PBD periods. When we investigate across the seven product categories considered (Dairy, Bakery/Deli, Frozen Items, Meat/Seafood, Alcohol/Tobacco, Produce, Pet Food), we do not find increases in the purchases of a particular type of product, but instead find significant decreases in the purchases of dairy, bakery/deli, frozen, and produce items. In total, though it appears that there may have been small shifts in consumer behavior during higher levels of PBD, these shifts would generally be associated with *faster* transaction speeds and would thus work in the opposite direction of a cashier shirking effect.

4.5.2 Changes in time-varying 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. The evident concern here is that cashier effort responds directly to state unemployment rates. We first note that, if cashier effort were to respond directly to unemployment rates, theory clearly suggests that this response would move in the opposite direction of a response to UI benefit generosity. 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. Our results, shown later, fit with this prediction. An implication is that our estimates for β would be biased toward zero without controls

[31]This should perhaps not be surprising: higher PBD levels are triggered (partly) by higher unemployment rates, and higher unemployment rates imply less consumption overall.

for unemployment rate. Nonetheless, we present below several pieces of evidence below that links between state unemployment rates and PBD extensions do not bias our results.

Given the details of the EB and EUC programs discussed in Section 4.1, it is perhaps not surprising that lagged state unemployment rates do not display a statistically significant relationship with PBD levels (see Table V). Notably, the majority of the PBD extensions occurring in our scanner data sample resulted from changes made to the structure of the programs, as opposed to changes in state unemployment rates. Further, as described in Appendix 1, for those PBD extensions that are determined by state unemployment rates 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 address concerns related to the correlation between PBD levels and unemployment rates by controlling for both state and local unemployment rates (county level in Virginia and Maryland, ward level in DC). 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. We demonstrate in Figure II that there is substantial variation in unemployment rates across counties (or wards) within the states in our scanner data sample.

A separate possibility is that the length of cashiers' shifts corresponded to differing PBD levels. Our estimates would be biased away from zero if cashiers work longer shifts during higher PBD levels, and longer shifts correspond to reduced productivity. For instance, it may be that less productive cashiers were laid off, while more productive cashiers were given longer shifts during higher PBD periods; though cashier fixed effects control for the change in the composition of cashiers, they do not account for the possibility of increased shift length within cashiers. Given our sample contains all transactions during a single hour on Saturdays, we cannot directly test for changes in shift length. We can, however, proxy for this by observing whether the average number of open registers, the average experience of employed cashiers, or the number of employed cashiers corresponded to PBD. From Table V, 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.[32]

4.5.3 Other policy changes

Finally, another threat to the exogeneity of our estimates for β centers on other policies that may have been adopted during our time frame that are associated with greater PBD levels and influenced transaction length. For instance, it could be that more generous food stamp (officially known as Supplemental Nutri-

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

tion Assistance Program or SNAP) policies were adopted during periods with higher PBDs. Returning to Table V, in the sixth cell, we regress the number of state-month SNAP participating households on PBD levels and find no statistically significant relationship.[33] Even had SNAP been correlated with PBD levels, we find it extremely 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 the check-out counter in the same manner as a debit card 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 5% level) between the bag tax policy and PBD levels (see Table V). 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, 2017). In our primary models, we simply control for the adoption of the bag tax policy.

5 Results

5.1 Main results

Table VI presents results from four different versions of our baseline model (equation 5), all estimated via OLS with cluster-robust standard errors at the state by week level. The specification in column 1 includes day and register-store fixed effects. Column 2 adds cashier-store fixed effects, column 3 adds the full set of control variables, and column 4 includes cashier-register-store fixed effects. Across all specifications, we estimate a positive effect of potential benefits duration on transaction duration, and in our preferred specification (column 4), we estimate a 0.135 second increase in transaction length for a one-week increase in PBD.[34] In columns 3 and 4, which include both controls and fixed effects at the cashier (or cashier-register) level, coefficients on PBD are statistically significant at conventional levels.[35,36,37] We also consider an

[33]SNAP participation data come from the United States Department of Agriculture, Food and Nutrition Service (FNS-388 report, Online, *accessed 3 May 2018*).

[34]In Table A1, we show that these results remain unchanged after dropping all cashier(-shift)-level controls. A possible motivation for dropping these controls is their potential endogeneity with relation to PBD levels.

[35]In Table A2, we replicate Table VI after ignoring periods where EUC benefits temporarily dropped to zero (and coding these periods with the pre-change PBD level). Similarly, in Table A3, we replicate Table VI but after dropping all weeks where EUC benefits temporarily dropped to zero. Results remain largely the same across these two tables.

[36]We also consider the sensitivity of our standard errors to various clusters in Table A4. Results for our two primary specifications remain statistically significant after clustering (a) by state-week, (b) twoway by cashier and day, and (c) twoway by store and month.

[37]In Table A5, we replicate Table VI with the inclusion of customer-card fixed effects. At this supermarket chain, customers may opt to use a customer reward card. If a customer shops on Saturdays between 5-6pm *using their rewards card*, we can see them multiple times in the sample using the masked identification code of their card. Approximately 70% of transaction are carried out

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 Table VII and are consistent with our main results.

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. To get a sense of the magnitude of this change, one can refer to the learning curve in Figure I. A 2% slowdown in checkout speed would be similar to switching a cashier who has worked 20 weeks with a cashier who has only worked 12 weeks, or switching a cashier that has worked 5 weeks with one who has only worked 3 weeks. 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.[38] With a \$14 expected median hourly wage for grocery store cashiers in the U.S., this would cost the stores in our sample \$2,016 higher wage bills per year.[39] Aggregating further, if the 39 supermarkets in our sample are representative of the 3,014 supermarkets and other grocery stores in DC, Maryland, and Virginia,[40] the PBD extension would cost supermarkets in these states \$6.1 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 0.75 items (6.6% of the sample mean transaction size). Coefficients on the state unemployment rate are negative and statistically insignificant. This result shows that a cashier's direct response to the increasing unemployment rates is to boost effort and productivity, and so in the absence of controlling for this, our estimated shirking effect would be even larger. Coefficients on the local unemployment rate (county-level in Maryland and Virginia, ward-level in Washington D.C.) are much smaller, and statistically significant at the 10% level in our model without cashier-register fixed effects.

by rewards card members. Given customer-card fixed effects strictly rely on rewards card members who shopped on at least two separate Saturdays from 5-6pm with differing PBD periods, this model is not preferred to the fully specified model (5). Still, our estimate for β in this model (0.137) is very close to that from our fully-specified model (0.135).

[38]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 U.S. 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).

[39]Hourly wage estimates come from Salary.com, Online, *accessed January 25, 2018.*

[40]Source: Source: U.S. Census Bureau, 2012 Economic Census. Online, *accessed January 25, 2018.*

5.2 Placebo 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 et al. (2004) and utilized in several studies including Ebenstein and Stange (2010) and Chetty et al. (2009). The idea is to estimate the preferred model after reassigning treatment status, and to 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 VIII. Of the six plausible combinations of state-treatment assignments to states, the true estimate of 0.135 is the largest.

5.3 Subsamples

To look for potential heterogeneity in the effect of PBD on transaction duration, in Table IX, we estimate equation (5) for several subgroups. These subgroups are defined based on cashier or register characteristics which are expected to mediate 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). There are at least two reasons to expect that the effect of PBD on worker effort will be mediated by experience. First, since UI eligibility is based on whether or not an employee's earnings meet some minimum levels, cashiers with more extensive experience are more likely to be UI eligible. Second, prior work has established a noticeable increase in the productivity of cashiers as they gain experience (Taylor, 2017). Similarly, we find strong evidence of learning-by-doing among our sample of cashiers (see Figure I), and cashiers who are new to a shift, and still in the learning process, may be less able to shirk.

Results in Table IX suggest a slightly stronger treatment effect for more experienced 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.[41]

Next, we consider heterogeneous treatment effects by cashier productivity. Given productivity is im-

[41] Figure A2a demonstrates that 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.

perfectly observable by managers and unproductive workers are relatively 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 apathetic toward PBD levels. To identify cashier productivity independently from the effects of PBD changes, 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.[42] We then consider how PBD changes differentially influence pre-policy highly productive cashiers (above the 75% percentile of productivity) versus less productive cashiers. Our primary cashier productivity split is presented across the fourth and fifth columns of Table IX. 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.[43,44]

Lastly, we consider a set of subgroups defined by the type of register that was used to conduct the transaction—express vs. regular. Given customers who sort into express lanes are relatively time-sensitive with smaller transactions, one may expect cashiers working these registers to have little opportunity to shirk. Conversely, larger transactions conducted on regular registers plausibly present more of an opportunity for the cashier to shirk. The results from the last two columns of Table IX are consistent with this hypothesis. For transactions conducted on express registers, the estimated effect of PBD on the duration of the transaction is nearly zero. In the subgroup with regular ("Full") 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.[45]

Overall, these splits across subsamples further suggest a true shirking effect. When splitting by cashier subsamples, the types of cashiers responding to PBD levels are those who stand to gain the most from shirking. Namely, highly experienced cashiers are those who are more likely to be eligible for UI, and those who are relatively unproductive are more likely to be terminated for a decrease in productivity.[46] We also

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

[43] 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.

[44] Figure A2b 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.

[45] Ignoring register type, Figure A3 demonstrates a consistent pattern of increasing treatment effects across larger transactions. For the full sample, we estimate a treatment effect around 0.2, and as we reduce to 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.

[46] It is important to note that while the average cashier becomes more productive over their first weeks in the sample (as shown in Figure I), cashiers with a high number of shifts are not necessarily the cashiers with high productivity levels. In fact, 20 percent

find that the treatment effect is attenuated by express lanes and amplified on regular registers, the latter of which likely give cashiers a greater opportunity to shirk.

5.4 Lack of PBD effect at self-checkout registers

In this section, we consider a similar analysis as above but 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). If instead we find significant effects with this analysis, then there would be a concern that customers, and not cashiers, are driving the results.

In Table X, we estimate a variant of equation (5) without the cashier c index and using scanner data from self-checkout registers. Of the 39 stores in the sample, only 6 have self-checkout registers—2 in DC, 2 in MD, and 2 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) and column 3 adds customer fixed effects. Though this analysis suffers from considerably reduced statistical power, we find no evidence of a positive and 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.

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

In order to determine whether our results extend to other state-years, industries, and types of workers, we utilize the American Time Use Survey (ATUS) to test for shirking responses to PBD extensions that occurred between 2003 and 2014. The most notable shortcoming, however, of the ATUS is its cross-sectional nature, which disallows the ability to measure individual fixed effects. In turn, we cannot control for any potential shifts in worker (or survey-taker) composition that may have occurred across differing PBD levels. Additionally, assuming a general stigma against shirking, and given activities in the ATUS are self-reported, shirking behavior is likely to be underreported, and this may be especially so if shirking were in response to increased UI benefit generosity.

Utilizing the same variation described in section 4.1, we estimate models with various combinations

of the cashiers are high productivity with a low number of shift and 19 percent are low productivity cashiers with a high number of shifts.

of state and month-year fixed effects, a linear state time trend, and a vector of controls. These controls include state unemployment rate, the worker's age, "usual" amount of hours worked per week, weekly earnings, and dummies for 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"). To start, we test for the possibility that worker composition in the ATUS sample changed by observable characteristics to differing PBD levels. In Table XI, similar to Table V, 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.

Our main results with the ATUS sample can be found in Table XII. Across five of the six combinations of specifications considered, we estimate statistically significant (at least 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 0.0034 percentage point increase (off a sample mean of 0.0668) in time spent not working in response to an 18-week increase in PBD level.[47] In Figure VI, we consider the permutation test outlined in Bertrand et al. (2004), where we plot the smoothed empirical distribution of estimated placebo treatment effects from 3,000 randomizations. For each randomization, workers, by state, are randomly assigned a state treatment pattern (without replacement). Results from this test suggest statistical significance at the 10% level for our fully specified model. Lastly, in 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.

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 8.35 hours at their workplace on the days they work, of which 33.47 minutes (6.68%) are spent not working,[48] a 0.0034 percentage point increase in time spent not working at work translates to a 1.70 minutes increase in shirking per workday. For workers that work five days a week, this aggregates to an additional 7.39 hours of shirking per year. Further aggregating across all full-time workers in the U.S., an 18-week increase in the PBD level would lead to 823.90 million additional hours of shirking per year in the U.S.—equivalent to $14.93 billion at the median hourly

[47] 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).

[48] This estimate is similar to what the Bureau of Labor Statistics reports for full-time workers in the U.S. using the 2014 ATUS data ("The Economics Daily: Time spend working by full-and part-time status, gender, and location in 2014," *U.S. Bureau of Labor Statistics*, 2 July 2015. Online, *accessed 7 May 2018*.)

wage.[49,50]

6 Conclusions

Numerous studies have investigated the ex-post moral hazard effect of more generous UI benefits on job search activity and unemployment duration. Despite strong theoretical evidence for its importance, empirical evidence of an *ex-ante* moral hazard effect of UI remains scant. An ex-ante moral hazard effect of UI would imply that workers will reduce on-the-job effort (shirk) in response to increases in UI benefit generosity.

In this paper, we exploit state-time variation in the 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. Using a generalized difference-in-differences design, 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 estimates 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 cashiers of whom are more likely to be terminated due to shirking (lower productivity cashiers) but are 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

[49]There were 111,487,000 full-time workers 18 years or older in the U.S. in 2017, working 35 hours or more per week ("Labor Force Statistics from the Current Population Survey," *U.S. Bureau of Labor Statistics*, 19 Jan 2018. Online, *accessed 7 May 2018*).

[50]The U.S. median hourly wage across all occupations was $18.12 in 2017 ("May 2017 National Occupational Employment and Wage Estimates," *U.S. Bureau of Labor Statistics*. Online, *accessed 7 May 2018*).

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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7 Appendix 1: Unemployment Insurance Program Extensions in the U.S.

7.1 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. There are three trigger options for each state to choose from:

1. 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, then an additional 13 weeks of benefits are made available.

2. 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."

3. 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).

7.2 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, 2013; 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).

7.3 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.[51] 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).

8 Tables and Figures

[51] 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.

Table I: Summary statistics from scanner data

	Washington D.C.	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 in Transaction	12.06	11.25	12.13	11.77
	(13.32)	(12.68)	(13.26)	(13.06)
Transaction Including Returns	0.00	0.00	0.00	0.00
	(0.03)	(0.04)	(0.03)	(0.03)
Total Cost of Transaction	33.98	31.97	38.75	34.97
	(39.62)	(37.39)	(42.80)	(40.12)
# of Items by Category:				
- Alcohol & Tobacco	0.08	0.00	0.34	0.15
	(0.37)	(0.07)	(0.80)	(0.54)
- Produce	1.67	1.52	1.87	1.69
	(2.67)	(2.50)	(2.81)	(2.66)
- Shelf-Stable Grocery Items	3.52	3.29	3.16	3.30
	(4.51)	(4.28)	(4.23)	(4.32)
- Dairy/Refrigerated Items	1.17	1.10	1.25	1.17
	(1.84)	(1.83)	(1.98)	(1.89)
- Frozen Items	0.64	0.61	0.63	0.62
	(1.38)	(1.39)	(1.40)	(1.39)
- Meat/Seafood	0.84	0.75	0.65	0.73
	(1.59)	(1.40)	(1.21)	(1.38)
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				
Year Opened	1976.75	1983.12	1980.79	1980.97
	(11.34)	(9.74)	(13.12)	(11.33)
Year Last Remodeled	2004.50	2004.88	2003.50	2004.31
	(3.51)	(4.08)	(4.60)	(4.11)
Total Building Size (in sq. ft.)	38759.13	40718.29	41344.86	40541.33
	(16125.69)	(10424.40)	(11277.75)	(11751.12)
Selling Space (in sq. ft.)	24526.88	27434.59	28223.36	27121.28
	(12184.32)	(7527.55)	(7976.94)	(8653.51)
# 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 II: Estimated UI eligibility in Mas and Moretti (2009) sample

	Mean	**Std. Dev.**	**Min**	**Max**
Total hours to date	1346.55	903.37	0.0028	4412.85
Shift length (hours)	6.17	2.99	0.0008	16.08
Tenure to date (days)	384.43	189.90	0	748.00
Cashier eligible for UI:				
- Washington D.C., 2008	0.83	0.37	0	1
- Washington D.C., 2009	0.84	0.37	0	1
- Washington D.C., 2010	0.84	0.37	0	1
- Washington D.C., 2011	0.84	0.37	0	1
- Maryland, 2008	0.80	0.40	0	1
- Maryland, 2009	0.81	0.39	0	1
- Maryland, 2010	0.81	0.39	0	1
- Maryland, 2011	0.81	0.39	0	1
- Virginia, 2008	0.74	0.44	0	1
- Virginia, 2009	0.77	0.42	0	1
- Virginia, 2010	0.79	0.41	0	1
- Virginia, 2011	0.79	0.41	0	1
N cashiers = 412				
N cashier-shifts = 55,205				

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 for cashier-shifts worked in a store that had been in the sample for 3 or more calendar quarters. See section 3.3 for more detail.

Table III: 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 IV: 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	20	33	79	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
2/28/2010	20	0	46	0	0	26	13	0	39
3/2/2010	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

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 V: Is UI potential benefit duration correlated with other covariates?

	Price Paid	Items Scanned	Avg. # of Transactions	# Open Registers	Bag Tax	Food Stamps
Potential benefits duration	-0.005	-5.075	-0.045	-0.006	0.017	-623.906
	(0.022)	(2.665)	(0.034)	(0.008)	(0.008)	(539.226)
Observations	4407	4407	4407	4407	4407	4407
Date FE	X	X	X	X	X	X
Store FE	X	X	X	X	X	X

	# Employed Cashiers	Cashier Experience	Lag Local UE Rate	Lag State UE Rate	Price Discount	Dairy
Potential benefits duration	-0.007	-0.999	-0.003	-0.001	0.014	-0.647
	(0.011)	(1.151)	(0.014)	(0.003)	(0.012)	(0.350)
Observations	4407	4407	4407	4407	4407	4407
Date FE	X	X	X	X	X	X
Store FE	X	X	X	X	X	X

	Bakery/ Deli	Frozen Items	Meat/ Seafood	Alcohol/ Tobacco	Produce	Pet Food
Potential benefits duration	-0.267	-0.394	-0.312	-0.056	-0.900	-0.087
	(0.113)	(0.202)	(0.204)	(0.054)	(0.416)	(0.054)
Observations	4407	4407	4407	4407	4407	4407
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 benefits duration on an outcome, collapsed to the store-week level. Potential benefits duration is measured in weeks. "Price paid" is the average cost of a transaction measured in dollars. "Items scanned" is the total number of items purchased. "# Open Registers" is the average number of open registers during the month. "Bag Tax" is an indicator for whether a bag tax was in place. "Food Stamps" is the number of households participating in SNAP per state-month. "Cashier experience" is the average length of time the cashier appears in the sample. "Price Distance" is the average size of any price discounts received across transactions. The six department categories are sums of total products sold by category. Standard errors twoway clustered at store and month-year level are shown in parentheses.

Table VI: Main results from scanner data

	Transaction Length (in seconds)			
	(1)	(2)	(3)	(4)
Potential benefits duration	0.139	0.150	0.137	0.135
	(0.061)	(0.065)	(0.047)	(0.056)
Price paid			0.213	0.213
			(0.012)	(0.012)
Total items scanned			3.221	3.229
			(0.052)	(0.052)
Local UE rate (prior month)			-0.740	-0.747
			(0.429)	(0.467)
State UE rate (prior month)			-1.813	-1.629
			(1.200)	(1.271)
Observations	515636	515618	515618	515433
Controls			X	X
Date FE	X	X	X	X
Register X Store FE	X	X	X	
Cashier X Store FE		X	X	
Cashier X Register X Store FE				X

Notes: Potential benefits duration is measured in weeks. Price paid 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, 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 VII: Results with data collapsed to cashier-register-day level

	Avg.(Transaction Length)		ln(Avg.(Transaction Length))	
	(1)	(2)	(3)	(4)
Potential benefits duration	0.189	0.189		
	(0.065)	(0.077)		
18-week PBD increase			0.019	0.018
			(0.009)	(0.011)
Price paid	0.137	0.125	0.000	0.000
	(0.068)	(0.071)	(0.000)	(0.000)
Total items scanned	3.183	3.086	0.013	0.013
	(0.294)	(0.314)	(0.002)	(0.002)
Local UE rate (prior month)	-0.766	-0.893	-0.010	-0.010
	(0.716)	(0.740)	(0.004)	(0.004)
State UE rate (prior month)	-1.722	-0.987	-0.005	-0.001
	(2.090)	(2.237)	(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 benefits duration is measured in weeks. Price paid 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, lagged county-month level unemployment rate, lagged state-month level unemployment rate, 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 VIII: Placebo tests - Reassignment of treatments across states

		Placebos				
	Actual	(1)	(2)	(3)	(4)	(5)
Outcome: Transaction Length						
Potential benefits duration	0.135	0.012	0.064	-0.064	-0.020	-0.154
	(0.056)	(0.066)	(0.065)	(0.057)	(0.065)	(0.067)
Price paid	0.213	0.213	0.213	0.213	0.213	0.213
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Total items scanned	3.229	3.229	3.229	3.229	3.229	3.229
	(0.052)	(0.052)	(0.052)	(0.052)	(0.052)	(0.052)
Local UE rate (prior month)	-0.747	-0.838	-0.833	-0.810	-0.835	-0.793
	(0.467)	(0.468)	(0.466)	(0.469)	(0.466)	(0.467)
State UE rate (prior month)	-1.629	-1.090	-1.026	-1.309	-1.175	-0.523
	(1.271)	(1.223)	(1.231)	(1.242)	(1.297)	(1.208)
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 benefits 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. Price paid 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, 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 IX: Main results from scanner data by subsample

		# of shifts		Productivity		Register type	
	Full	High	Low	High	Low	Express	Full
Outcome: Transaction Length							
Potential benefits duration	0.135	0.170	0.139	-0.107	0.191	0.006	0.254
	(0.056)	(0.065)	(0.078)	(0.101)	(0.068)	(0.058)	(0.081)
Price paid	0.213	0.244	0.209	0.220	0.224	0.331	0.177
	(0.012)	(0.019)	(0.022)	(0.029)	(0.017)	(0.018)	(0.015)
Total items scanned	3.229	3.108	3.172	2.780	3.244	2.999	3.344
	(0.052)	(0.088)	(0.088)	(0.121)	(0.073)	(0.075)	(0.064)
Local UE rate (prior month)	-0.747	0.027	-0.669	-0.555	-0.559	-0.893	-0.655
	(0.467)	(0.667)	(0.596)	(0.610)	(0.642)	(0.468)	(0.732)
State UE rate (prior month)	-1.629	1.071	-4.442	-0.664	-2.290	-0.174	-2.957
	(1.271)	(1.934)	(2.007)	(2.953)	(1.707)	(1.356)	(1.951)
Observations	515433	163806	156865	71222	248985	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 benefits duration is measured in weeks. Price paid 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, 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 below the median fixed effect (high productivity) or above the median fixed effect (low productivity). "Date" refers to exact date (e.g. August 3, 2017). Standard errors, shown in parentheses, are clustered at state by date level.

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

	Transaction Length (in seconds)		
	(1)	(2)	(3)
Potential benefits duration	-0.478	-0.536	-0.329
	(0.508)	(0.522)	(0.669)
Price paid		0.061	0.348
		(0.096)	(0.203)
Total items scanned		8.010	7.154
		(0.469)	(0.894)
Observations	34315	34315	16155
Controls		X	X
Date FE	X	X	X
Register X Store FE	X	X	X
Customer FE			X

Notes: This table uses scanner data only from self-checkout registers. Six of the 39 stores have self-checkout registers—two in each state. Potential benefits duration is measured in weeks. Price paid 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 XI: Is UI potential benefit duration correlated with other ATUS covariates?

	Age	Female	White	Weekly Earnings	Usual # Work Hours
Potential benefits duration	0.107	0.009	-0.002	-11.427	-0.044
	(0.251)	(0.013)	(0.009)	(12.609)	(0.205)
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 benefits duration	0.009	-0.009	-2.179	1.050	-0.316
	(0.009)	(0.009)	(3.337)	(0.120)	(0.284)
Observations	6041	6041	6041	6041	5963
Mean of Y	0.19	0.81	406.18	6.40	40.33
State FE	X	X	X	X	X
Month-Year FE	X	X	X	X	X

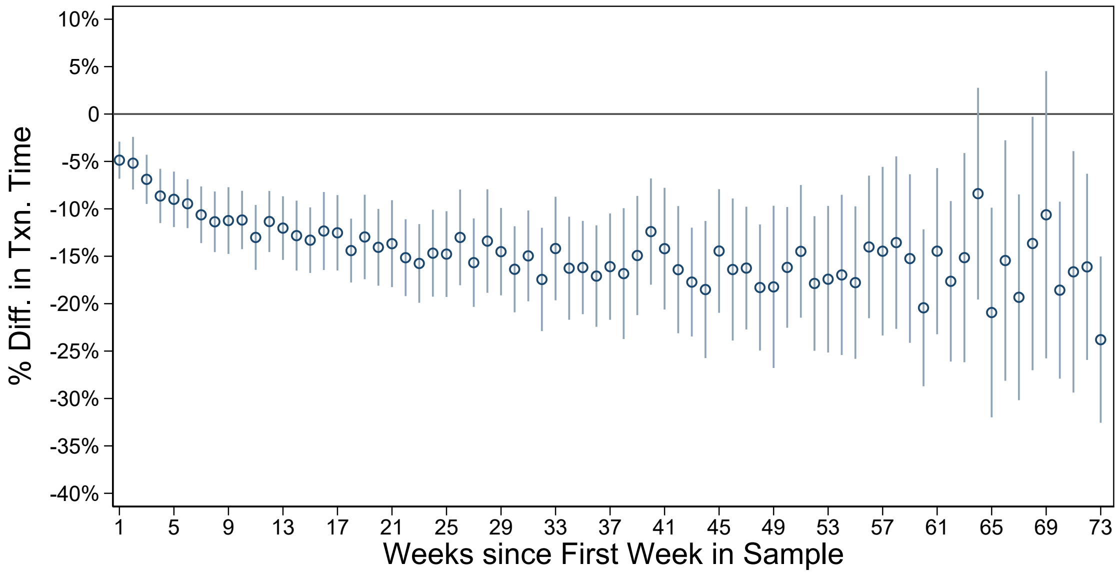
Notes: Each cell reports a coefficient from a single regression of potential benefits duration on an outcome, collapsed to the state-month-year level. Potential benefits duration is measured in weeks. "Age" is the average age of workers in our ATUS sample. "Female" is the fraction of workers who were 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 twoway clustered at the state and month-year level.

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

	% Time At Work Not Working					
	(1)	(2)	(3)	(4)	(5)	(6)
18-week PBD increase	0.0033	0.0028	0.0041	0.0040	0.0035	0.0034
	(0.0016)	(0.0017)	(0.0019)	(0.0018)	(0.0021)	(0.0020)
State UE rate (prior month)			-0.0004	-0.0007	0.0006	0.0004
			(0.0010)	(0.0010)	(0.0012)	(0.0012)
Maximum WBA (100s)			0.0044	0.0042	-0.0006	-0.0011
			(0.0029)	(0.0030)	(0.0041)	(0.0041)
Observations	30094	30094	30094	30094	30094	30094
Mean of Y	0.0668	0.0668	0.0668	0.0668	0.0668	0.0668
State FE	X	X	X	X	X	X
Month FE	X		X		X	
Year FE	X		X		X	
Month-Year FE		X		X		X
Linear State Time Trend					X	X
Controls			X	X	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.

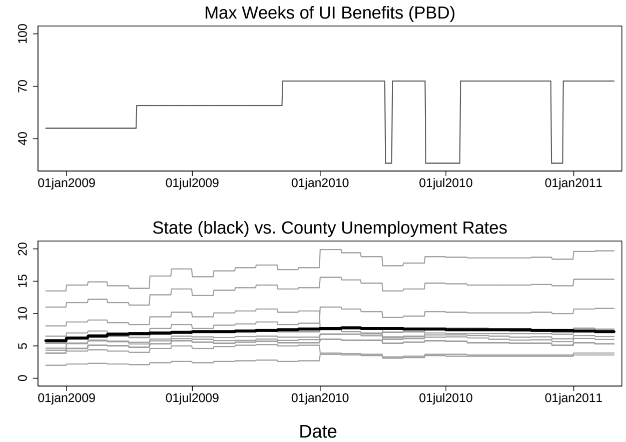
Figure I: Learning-by-doing: The relationship between cashier experience working a shift and average transaction time per shift



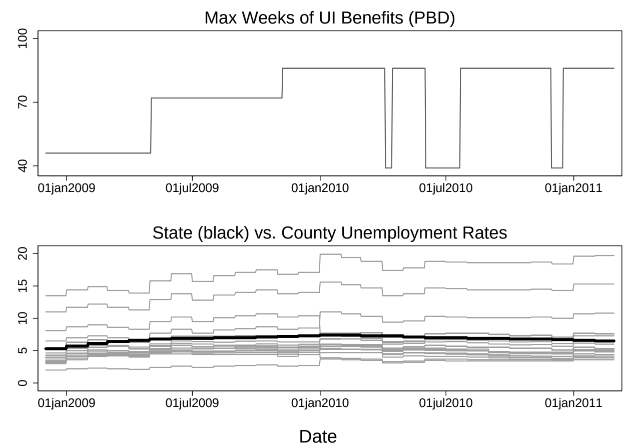
Notes: Figure presents the results from estimating learning-curve equation: $lnTxnTime_{csw} = \sum_{e=1}^{N} \eta_e E_{e,csw} + \beta_x X_{csw} + \alpha_{cs} + \delta_w + \epsilon_{csw}$. The data are averaged to the cashier-level for each Saturday 5pm hour, thus each cashier has one observation per week. $E_{e,csw}$ is a dummy variable equaling one if cashier c appears in the sample in week w for the eth time (i.e., $E_{1,csw} = 1$ for all weeks in which cashiers appear in the sample for the first time). The first week the cashiers are in the sample ($e = 1$) is the omitted dummy. In addition to cashier and week-of-sample fixed effects, we control for the average number of items scanned per transaction, average amount spent per transaction, and the average types of items purchased for cashier c in store s, and week-of-sample w. The dependent variable is logged average transaction time for cashier c, thus the y-axis can be interpreted as the percent difference in transaction time from the first week worked compared to subsequent weeks worked. The η_e estimates for cashiers learning to work a new shift are plotted with blue hollow circles. Upper and lower 95% confidence intervals are depicted, estimated using two-way cluster robust standard errors on store and week-of-sample.

Figure II: Potential benefit durations and local/state unemployment rates

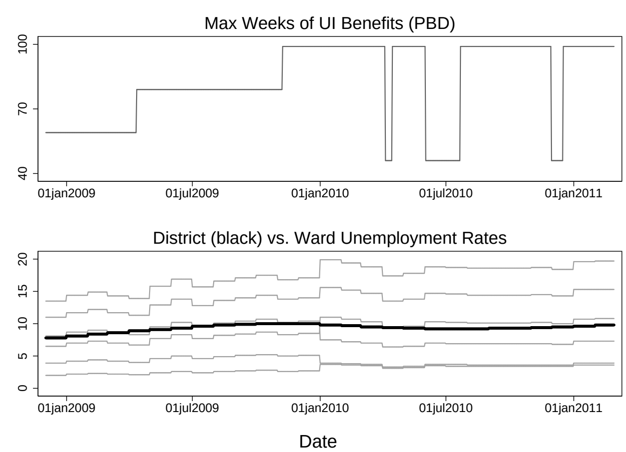
(a) Maryland



(b) Virginia



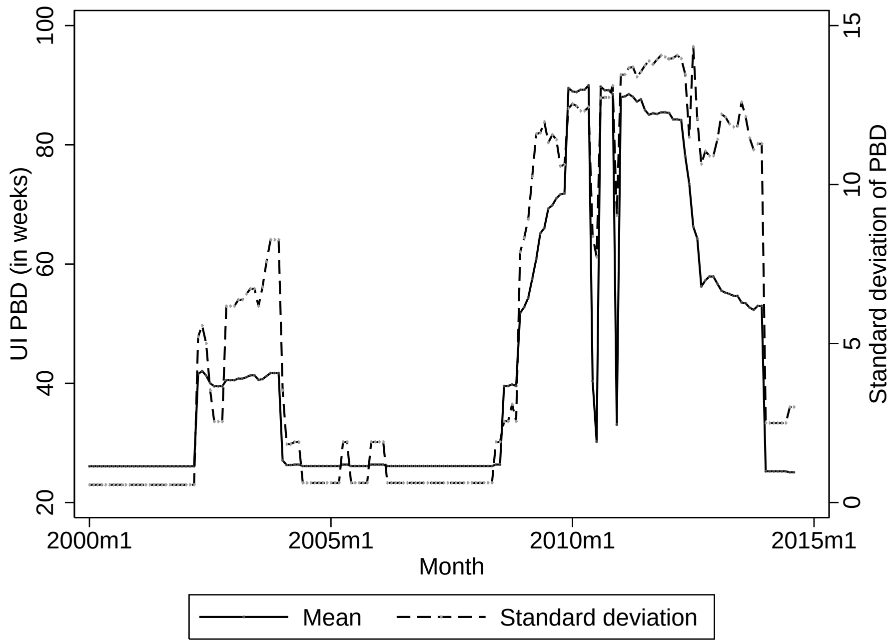
(c) Washington D.C



Notes: Figures show state (bolded) unemployment rates, county (ward for DC) unemployment rates, and PBDs during the span of our scanner data sample, in the three jurisdictions covered by the sample. County and state unemployment rates are retreived from the BLS, DC ward unemployment rates from the DC Department of Employment Services. Rates for Virginia counties, Maryland counties, and DC wards which are not represented in our scanner data sample are not shown.

Figure III: 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

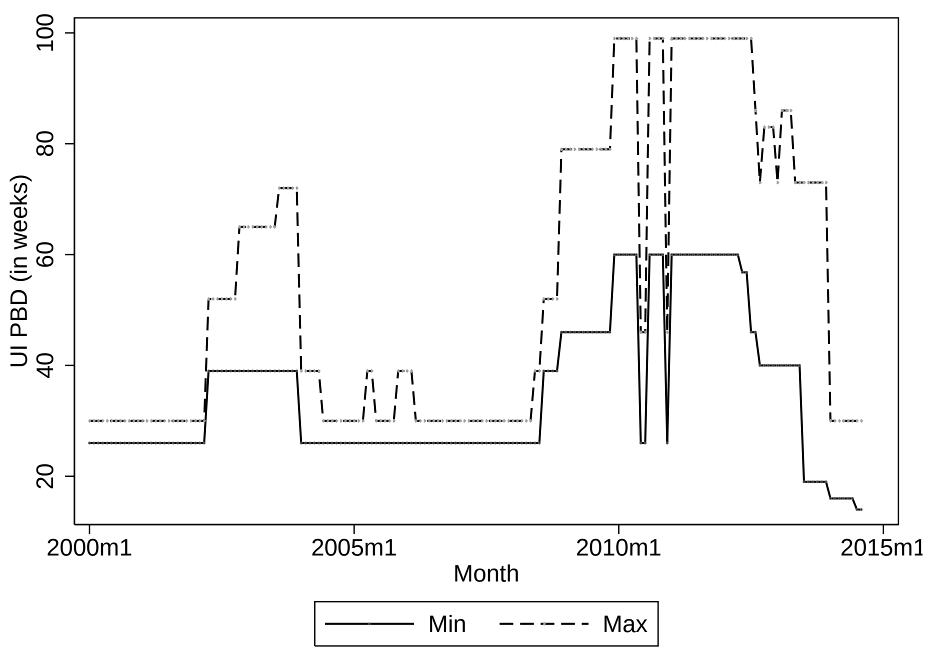
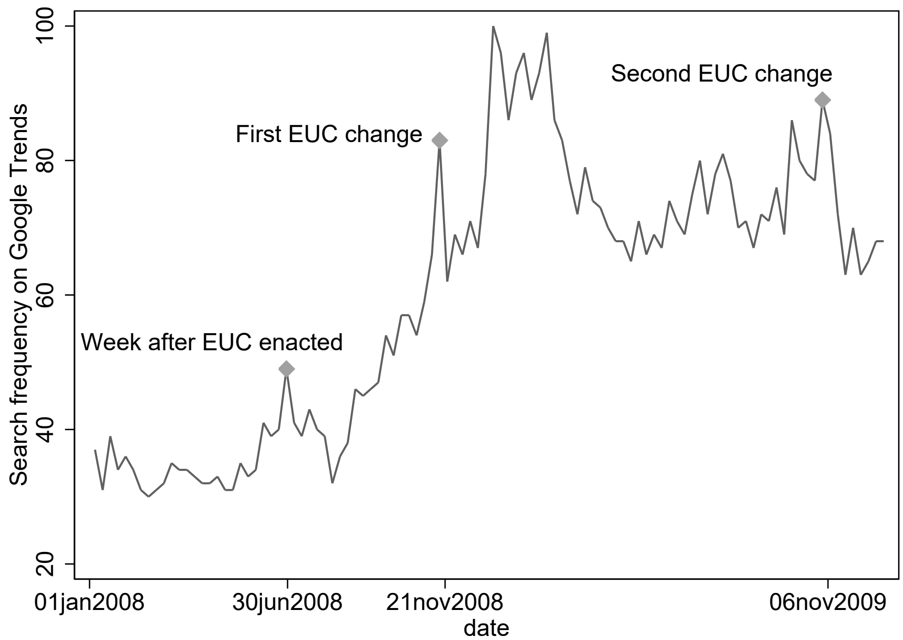
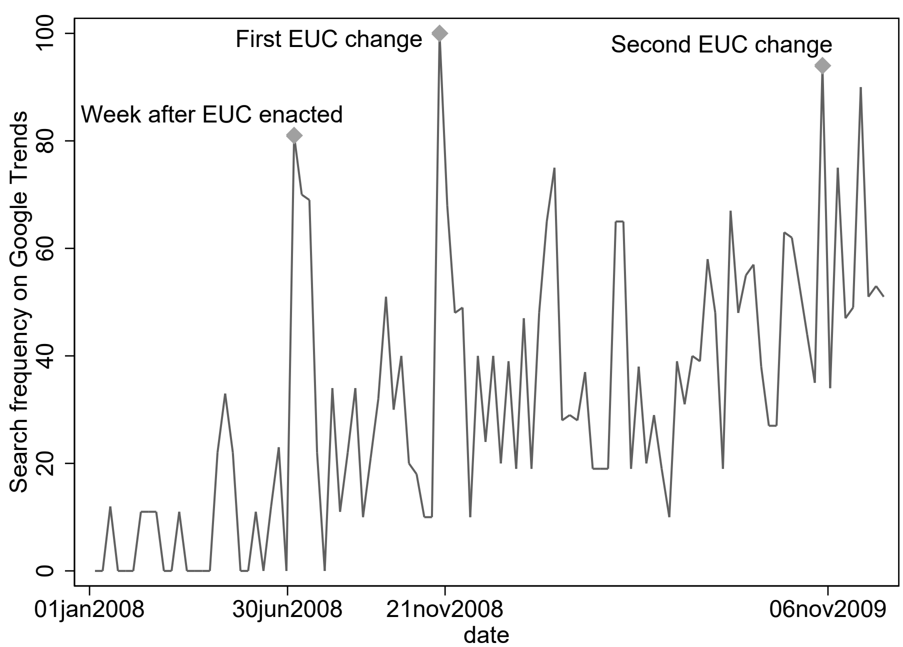


Figure IV: Searches on Google via Google Trends

(a) Searches for “Unemployment Benefits”



(b) Searches for “Emergency Unemployment Compensation”

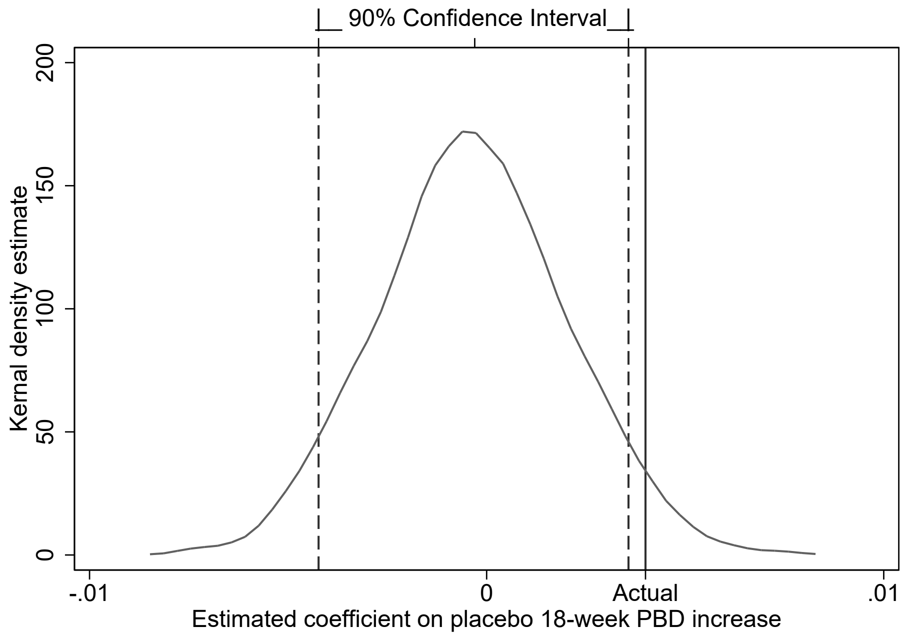


Notes: Google Trends data retrieved from Google Inc. Search frequency, indexed to a 0 to 100 scale, shows how often a particular search-item on Google Search (i.e. “Unemployment Benefits” and “Emergency Unemployment Compensation”) is entered relative to the total search-volume for the search-item across 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 V: 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 VI: 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 et al. (2004).

9 Appendix 2: Additional Tables and Figures

Table A1: Main results without cashier controls

	Transaction Length (in seconds)			
	(1)	(2)	(3)	(4)
Potential benefits duration	0.139	0.150	0.178	0.180
	(0.061)	(0.065)	(0.058)	(0.068)
Price paid			0.213	0.214
			(0.012)	(0.012)
Total items scanned			3.260	3.260
			(0.053)	(0.053)
Local UE rate (prior month)			-0.750	-0.683
			(0.494)	(0.539)
State UE rate (prior month)			-2.828	-3.132
			(1.393)	(1.505)
Observations	515636	515618	515618	515433
Controls			X	X
Date FE	X	X	X	X
Register X Store FE	X	X	X	
Cashier X Store FE		X	X	
Cashier X Register X Store FE				X

Notes: Potential benefits duration is measured in weeks. Price paid 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 A2: Main results ignoring EUC=0 weeks

	Transaction Length (in seconds)			
	(1)	(2)	(3)	(4)
Potential benefits duration	0.160	0.143	0.125	0.134
	(0.062)	(0.068)	(0.048)	(0.057)
Price paid			0.213	0.213
			(0.012)	(0.012)
Total items scanned			3.221	3.229
			(0.052)	(0.052)
Local UE rate (prior month)			-0.750	-0.751
			(0.430)	(0.468)
State UE rate (prior month)			-1.631	-1.485
			(1.202)	(1.267)
Observations	515636	515618	515618	515433
Controls			X	X
Date FE	X	X	X	X
Register X Store FE	X	X	X	
Cashier X Store FE		X	X	
Cashier X Register FE				X

Notes: Potential benefits duration is measured in weeks. Price paid 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, 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 A3: Main results dropping EUC=0 weeks

	Transaction Length (in seconds)			
	(1)	(2)	(3)	(4)
Potential benefits duration	0.149	0.160	0.139	0.152
	(0.062)	(0.068)	(0.048)	(0.057)
Price paid			0.213	0.213
			(0.013)	(0.013)
Total items scanned			3.243	3.252
			(0.055)	(0.054)
Local UE rate (prior month)			-0.576	-0.643
			(0.443)	(0.489)
State UE rate (prior month)			-1.851	-1.516
			(1.228)	(1.294)
Observations	471847	471826	471826	471647
Controls			X	X
Date FE	X	X	X	X
Register X Store FE	X	X	X	
Cashier X Store FE		X	X	
Cashier X Register X Store FE				X

Notes: Potential benefits duration is measured in weeks. Price paid 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, 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 by various standard error clusters

	State-Date		State-Month		Cashier		Store	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Potential benefits duration	0.137	0.135	0.137	0.135	0.137	0.135	0.137	0.135
	(0.047)	(0.056)	(0.042)	(0.046)	(0.055)	(0.059)	(0.070)	(0.076)
Price paid	0.213	0.213	0.213	0.213	0.213	0.213	0.213	0.213
	(0.012)	(0.012)	(0.013)	(0.012)	(0.012)	(0.012)	(0.020)	(0.020)
Total items scanned	3.221	3.229	3.221	3.229	3.221	3.229	3.221	3.229
	(0.052)	(0.052)	(0.074)	(0.074)	(0.050)	(0.050)	(0.068)	(0.068)
Local UE rate (prior month)	-0.740	-0.747	-0.740	-0.747	-0.740	-0.747	-0.740	-0.747
	(0.429)	(0.467)	(0.582)	(0.635)	(0.379)	(0.421)	(0.428)	(0.540)
State UE rate (prior month)	-1.813	-1.629	-1.813	-1.629	-1.813	-1.629	-1.813	-1.629
	(1.200)	(1.271)	(1.154)	(1.104)	(1.360)	(1.454)	(2.210)	(2.653)
Observations	515618	515433	515618	515433	515618	515433	515618	515433
Clusters	339	339	36	36	1,966	1,966	39	39
Controls	X	X	X	X	X	X	X	X
Date FE	X	X	X	X	X	X	X	X
Register X Store FE	X		X		X		X	
Cashier X Store FE	X		X		X		X	
Cashier X Register X Store FE		X		X		X		X

Notes: Potential benefits duration is measured in weeks. Price paid 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, 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 A5: Main results with customer fixed effects

	Transaction Length (in seconds)			
	(1)	(2)	(3)	(4)
Potential benefits duration	0.153	0.136	0.104	0.137
	(0.072)	(0.085)	(0.074)	(0.079)
Price paid			0.299	0.301
			(0.017)	(0.017)
Total items scanned			2.798	2.820
			(0.066)	(0.067)
Local UE rate (prior month)			-0.925	-1.045
			(0.532)	(0.579)
State UE rate (prior month)			-2.651	-3.381
			(1.745)	(1.868)
Observations	354642	354613	354613	354281
Controls			X	X
Date FE	X	X	X	X
Register X Store FE	X	X	X	
Cashier X Store FE		X	X	
Cashier X Register X Store FE				X
Customer FE	X	X	X	X

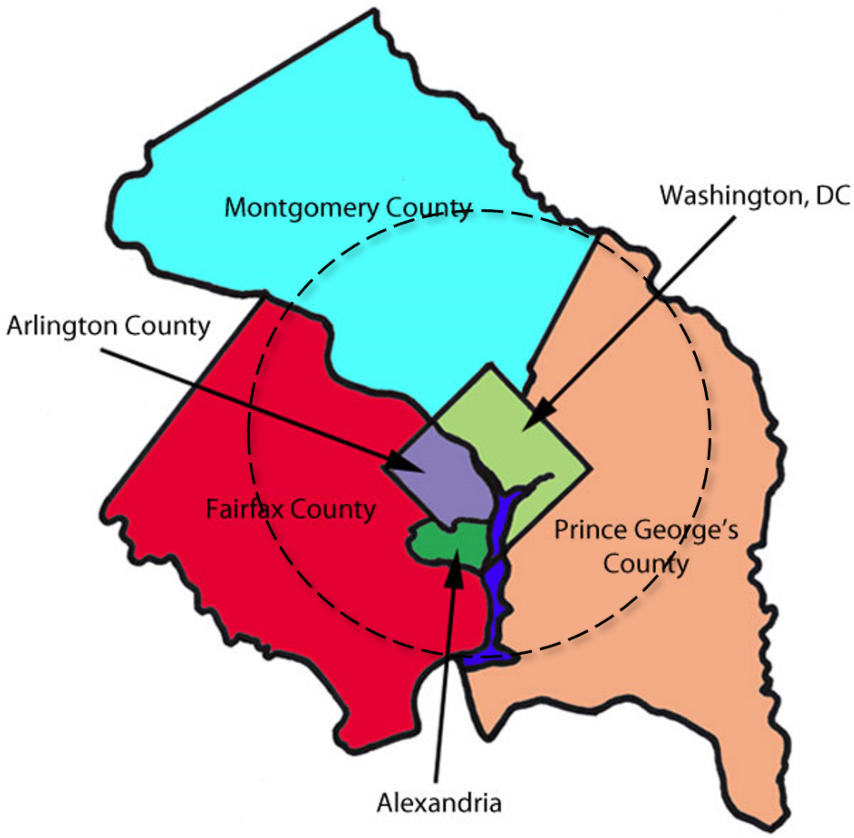
Notes: Potential benefits duration is measured in weeks. Price paid is measured in dollars. Controls for each regression include indicators for whether the transaction included items from particular departments (e.g. alcohol), an indicator for whether a plastic bag tax was in place at the store, number of households participating in SNAP per state-month, lagged county-month level unemployment rate, lagged state-month level unemployment rate, 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 twoway clustered at store and day level.

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

	Minutes spent at workplace					
	(1)	(2)	(3)	(4)	(5)	(6)
18-week PBD increase	0.9369	0.6609	1.6966	1.3460	2.2621	1.2495
	(1.7352)	(2.9854)	(1.7751)	(2.6092)	(1.9864)	(2.8702)
State UE rate (prior month)			-0.8106	-0.6795	-2.5283	-2.6023
			(0.9579)	(0.9479)	(1.1556)	(1.1757)
Maximum WBA (100s)			-3.8527	-3.8503	0.8921	1.2373
			(2.8738)	(3.0681)	(6.0693)	(6.3900)
Observations	30094	30094	30094	30094	30094	30094
Mean of Y	510.4462	510.4462	510.4462	510.4462	510.4462	510.4462
State FE	X	X	X	X	X	X
Month FE	X		X		X	
Year FE	X		X		X	
Month-Year FE		X		X		X
Linear State Time Trend					X	X
Controls			X	X	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, are clustered at state level.

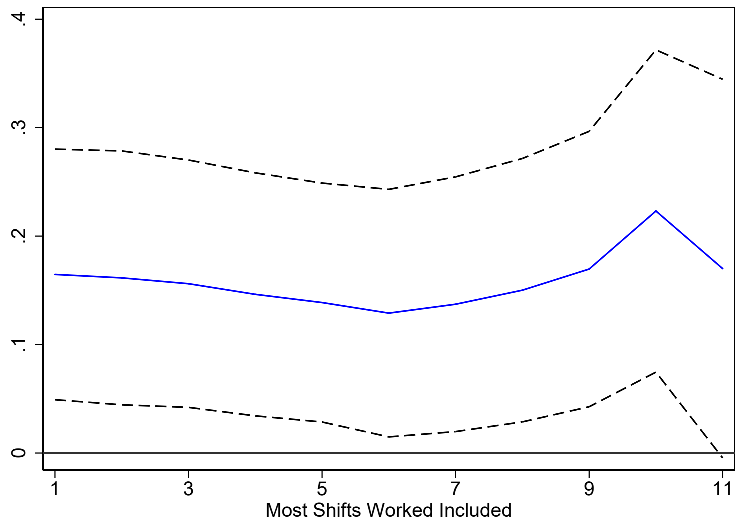
Figure A1: Map of the Washington DC Metropolitan Area



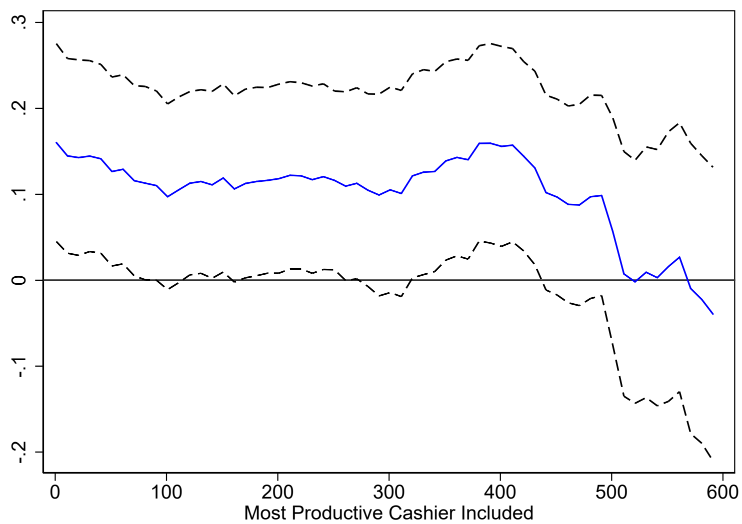
Notes: This figure provides a stylized map of the Washington DC Metropolitan area. The circle represents the area in which the 39 stores in the scanner data sample are located. Montgomery & Prince George's Counties are in Maryland. Arlington County, Fairfax County, and the City of Alexandria are in Virginia.

Figure A2: The effect of PBD on transaction duration by cashier subsamples

(a) Cashier experience - Number of shifts worked

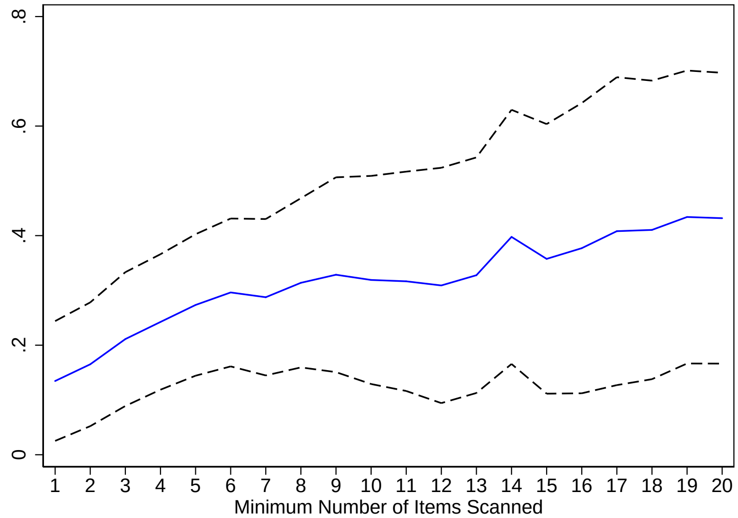


(b) Cashier productivity - Ranking of cashier pre-policy fixed effect



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 A3: 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.