Unemployment benefits and consumption smoothing: Cross-state study from the U.S. *

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Abstract

Unemployment insurance programs have three common characteristics: weekly benefit amount, potential benefit duration, and payment frequency. Exploiting variations in these dimensions across American states over time as well as deterministic kinks in the policy schedule, I study their effects on the ability of unemployed workers to smooth their consumption relative to that in employment periods. Using quarterly micro data, I find that weekly benefit amount plays the most important role in helping unemployed workers smooth consumption, namely in food and nondurables. Potential benefit duration also has a modest smoothing effect, while payment frequency rarely play a significant role.

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1 Introduction

Unemployment insurance (UI) is a major integral part of labor markets in most countries. However, the operation of UI programs inherently entails a trade-off. On one hand, generous UI might result in moral hazard costs because unemployed workers are disincentivized to look for a new job. On the other, UI serves the purpose of ameliorating financial setbacks that workers might face once they lose their jobs. While the specific design of UI programs and its effect on moral hazard have attracted great academic interest, less is known about the effectiveness of consumption smoothing. This paper aims to fill this gap.

In specific, state-administered UI programs in the U.S. share three common features. First, the benefit level that unemployed workers are entitled to is a function of their past labor earnings. Second, UI compensation is limited to a certain maximum duration threshold, over which workers no longer receive their benefits. Third, benefit payments are made at deterministic frequency, namely weekly or biweekly. In this paper I ask the question: how do these three features of UI programs in the U.S. affect unemployed workers' consumption smoothing ability?

My contributions are twofold. First, this paper is the first, according to my best knowledge, to study consumption smoothing effects of different dimensions of UI programs *simultaneously.* As discussed in the literature review, most studies have so far focused on each separate single dimension at a time while ignoring others. Second, earlier work on the consumption smoothing effects of UI programs use annual panel data with limited classes of expenditures. Instead, for my analysis I use rich quarterly panel data from the Consumer Expenditures Survey (CEX) to capture responses for unemployment spells that last only weeks as well as various categories of consumption in extensive detail.

For identification I exploit institutional variations in UI programs' features between American states. I first show that there are substantial variations in weekly benefit amounts, potential benefit duration, and, to a lesser extent, benefit payment frequency across states and over time. From the CEX data I know the households' state of residence as well as the primary earners' labor earnings and weeks of non-work in the span of twelve months. After controlling for other household- and state-level characteristics, I can back out the effects of interest from OLS regressions of different consumption categories on UI programs' features. One particular challenge is the possible endogeneity of UI benefit amount, which I circumvent by exploiting ad-hoc state-specific compensation ceilings.

Overall, I find that benefit level and maximum duration significantly smooth unemployed workers' expenditures for food and other (strict) nondurables, while payment frequency rarely matter. The effects wear out along the unemployment duration and are cyclical to business cycle. For total expenditures none of these dimensions affect smoothing. Two upfront caveats are in order. First, I ignore the moral hazard aspect of UI programs in my analysis. Thus, my results should be interpreted as the upper bound when labor supply incentives are not distorted. Second, since I use fully employed workers as the control group, I calculate *eligible* UI benefit amounts and *potential* duration instead of the *actual* amount and duration. Consequently, my results are intention-to-treat effects instead of treatment-on-the-treated. Nevertheless, mine are policy-relevant effects, as explained in Gruber (1997) and East & Kuka (2015).[1]

[1]Generally, we are interested in setting the benefit level or the maximum duration for the average

Related Literature: Due to the size and the importance of UI programs within the welfare system, economists have long been interested in the costs versus benefits study of unemployment compensation and the consequent optimal design of UI. Notable papers in this field are Chetty (2008), Schmieder & Von Wachter (2016), Kolsrud et al. (2018), Ganong & Noel (2019). Despite different settings, the central idea in these papers is to balance the welfare cost of moral hazard and the welfare benefit of consumption smoothing.

Several papers have examined the "cost" side of UI programs, namely the effects on unemployment duration and labor supply behaviors. For example, Landais (2015) studies UI benefit schedules across a few American states and finds the negative effects of benefit level and potential duration on unemployed workers' search effort. Card et al. (2015) also reach the same conclusion for the behavioral cost of benefit level in the context of Austria. Meanwhile, abrupt cuts in the maximum duration of UI benefit significantly push unemployed workers back to employment, as documented by Hagedorn et al. (2016) and Johnston & Mas (2018). More recently, Zhang (2021) explores the frequency dimension of UI payments, finding that workers have higher reservation wage and exert less search effort for new jobs when their states switch from biweekly to weekly UI payment schedule. My paper examines all three features of UI programs discussed by this literature strand, but focuses solely on their effects on consumption smoothing instead of their moral hazard aspect.

Much less has been done on the "benefit" side, which is the consumption smoothing effect of UI programs. My work contributes to this strand on the data front. Gruber (1997) is the pioneer in empirically studying the effects of UI benefit level on con-

worker who *might* lose her job, not only those actually exposed to unemployment.

sumption during unemployment in the U.S. context. He finds evidence supporting the substantial smoothing role of UI benefits. Later East & Kuka (2015) revisit the question and show that the effect varies over time, stronger in the 1970s than in the 1990s due to the relative low unemployment rate and less generosity in UI during the latter. Both studies use the Panel Study of Income Dynamics (PSID) and therefore are restricted to observing food consumption only. Meanwhile, using more recent CEX data, I can evaluate multiple categories of expenditures at the same time.

The rest of the paper has the following structure. Section 2 introduces the institutional background about UI programs in the U.S. and variations along the three dimensions of interest across states and time. Section 3 decribes the data source and the sample for empirical analysis. Section 4 discusses the methodological approach. Section 5 presents the main results while Section 6 delivers robustness checks. Section 7 concludes.

2 Institutional Background

Although UI regulations differ from state to state, they often share common requirements for claimants to be eligible for receiving benefit payments. First, they must have worked before the unemployment spell at least an extensive amount of time, which is called the *base period* and typically spans four consecutive months in most states. Second, during that period their total earnings from work must also have exceeded a minimum threshold which ranges from a few hundreds to a few thousands dollars. Next, they must have been laid off from their previous job instead of quitting *and* through reasons other than their own faults. Finally, every time filing a claim they must demonstrate that they have been actively searching for a new job. Not ever unemployed worker who is eligible for UI benefits decides to file a claim, however, leading to a take-up rate of less than 60% (Anderson & Meyer 1997 and Kuka & Stuart 2021).

In the later part of this section I discuss the institutional background and demonstrate the variations of UI programs in three dimensions of interest across states and time[2]. For regulations on benefit levels and maximum benefit duration, I use archived semi-annual state law publications consolidated by the Employment & Training Administration (ETA), U.S. Department of Labor. The data is published every January and July, covering every state of the U.S. and containing information about how, within each state, the weekly benefit amount as well as the potential benefit duration is calculated. For benefit claiming frequency, I use data from Zhang (2021) who summarizes historical audit information from the Benefit Accuracy Measurement program. Also recorded by the ETA, the program randomly samples UI benefit claimants every week from each state and checks if they have been incorrectly denied, underpaid, or overpaid. Relevant to my purpose, the data has information about the frequency of claims filing, which is then designated by Zhang (2021) as the state-wide benefit payment frequency.

[2]State-level data fully covers the period that I focus on in this paper, namely from 2002 to 2019.

2.1 Weekly benefit amount

The amount of UI benefit that a worker can claim for each week of unemployment depends on her labor earnings prior to the job loss. Specifically, this amount equals to a fixed fraction of the total wage received within the calendar quarter with the highest earnings in the last four or five quarters (so-called "highest quarter wage"). The fraction varies across states, but typically fall within the range from 1/26 to 1/21. Assume that the worker earn the same earning every quarter before the unemployment spell, the weekly benefit translates to roughly 50% of the pre-unemployment weekly earnings, or a *replacement rate* of 0.5.

Moreover, every state also has specific a weekly UI benefit cap. Let hqw be the highest quarter wage, τ be the fraction of UI benefit payment, and $\overline{WBA}$ be the cap, then the actual weekly UI benefit amount WBA is calculated as

$$WBA = \min\{\tau \times hqw, \overline{WBA}\}.$$

This formula implies that eligible benefit amount linearly increases in labor earnings up to the cap, over which it starts falling. Figure 1 depicts box plots summarizing the distribution of the benefit cap $\overline{WBA}$ across states over time. We can see that since 2002 there is not only an overall upward trend in the median benefit ceilings, which is due to rising costs of living, but also a widening difference in these ceilings among states. These features are attributable to the rising generosity in the upper tail of the distribution while the lower tail is generally stable.

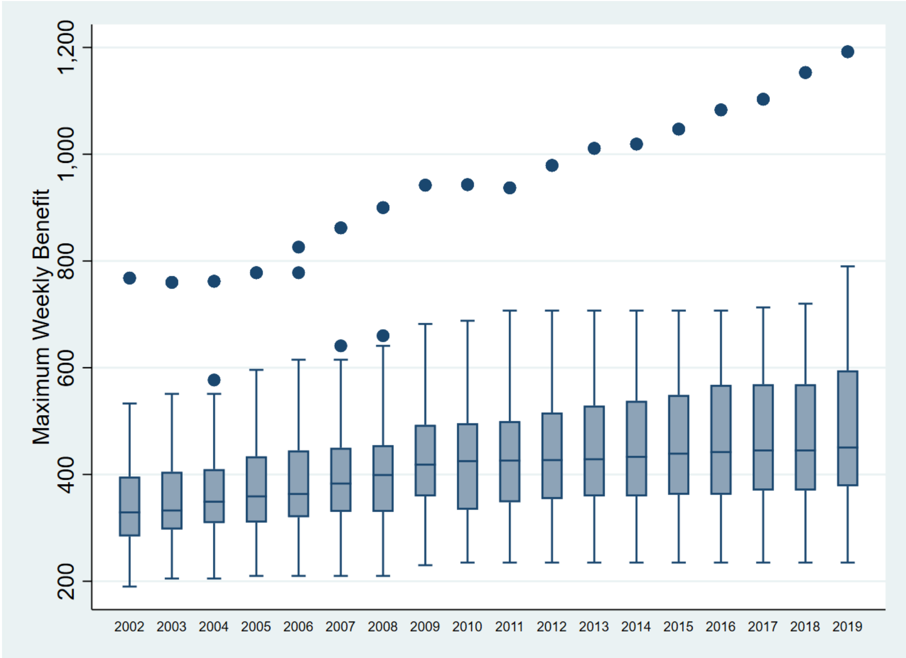


Figure 1: Weekly benefit ceilings distribution between 2002 and 2019
Source: Employment & Training Administration. Unit: Current US dollars.

2.2 Potential benefit duration

In general there are two ways in which the potential benefit duration (PBD) can be determined for a claimant. On one hand, there are hard ceilings, typically 26 weeks, stipulated by the state law that the benefit duration cannot exceed. On the other hand, some states regulate the total benefit amount payable during the unemployment spell to not be higher than a certain fraction, normally ranging from 1/2 to 1/3, of total earnings during the base period. Thus, the ceiling of potential benefit weeks in those states, if lower than 26 weeks, is technically restricted to the quotient of total benefit amount divided by the eligible WBA.

Before 2008 duration caps among states are quite uniform at 26 weeks, with only a few exceptions as in Figure 2. To ameliorate the fallout of the Great Recession, the Obama administration passed the Emergency Unemployment Compensation Act in June 2008 (EUC08) to extend the UI benefit duration in all states by federal funds. The Act was re-authorized multiple times (in "Tiers"), each with some extra weeks of benefit entitlement, by the Congress until it was terminated abruptly in December 2013. In conjunction with the EUC08, during episodes of high unemployment within a state, the Federal-State Extended Benefits (EB) also automatically came into effect with additional duration of either 13 weeks or 20 weeks after exhaustion of both regular UI and EUC08. Within my sample period, EB program was triggered on in many states between 2009 and 2012, resulting in the "99ers" cohort - unemployed workers with up to 99 weeks of UI benefits altogether. Table 1 summarizes the details of these extension programs. Since 2014 benefit duration caps have started diverging significantly, ranging from as few as 12 weeks in Florida to as many as 30 weeks in Massachusetts as in January 2019. The evolution of PBD distribution across states over time is shown in Figure 2.

Program	**Date in Effect**	**Weeks**	**Special Condition**
EUC08 Tier 1	June 30th 2008	13	
EUC08 Tier 1 Upgrade	November 21st 2008	7	
EUC08 Tier 2	November 21st 2008	13	State's unemployment rate $> 6\%$
EUC08 Tier 2 Upgrade	November 6th 2009	1	
EUC08 Tier 3	November 6th 2009	13	State's unemployment rate $> 6\%$
EUC08 Tier 4	November 6th 2009	6	State's unemployment rate $> 8.5\%$
EB	2009-2012	13 (20)	High (very high) unemployment

Table 1: UI benefit duration extension from 2008 to 2013
Source: Employment & Training Administration. EUC08 Link and EB Link

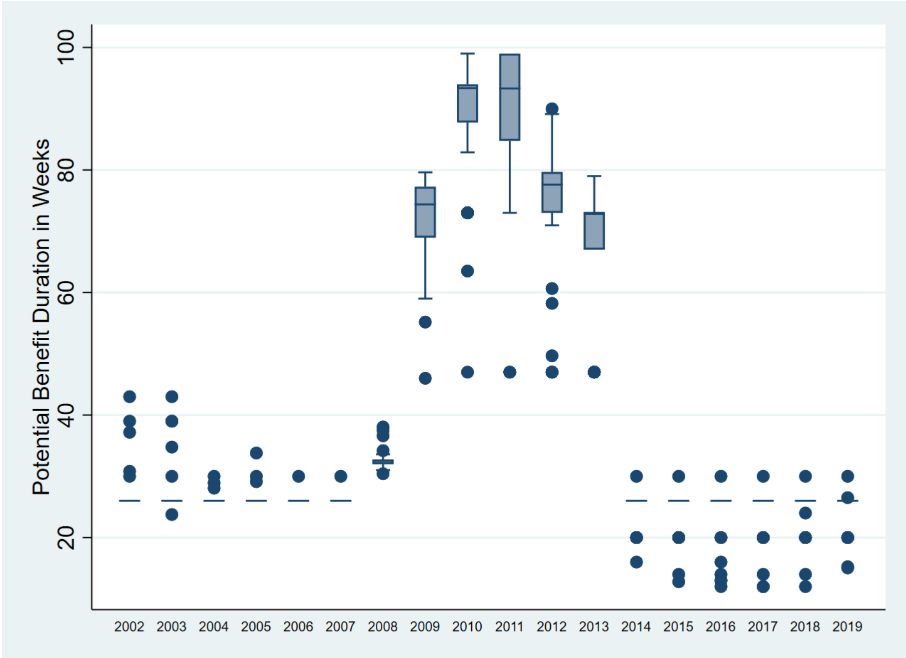


Figure 2: Potential duration distribution between 2002 and 2019
Source: Employment & Training Administration.

2.3 Payment frequency

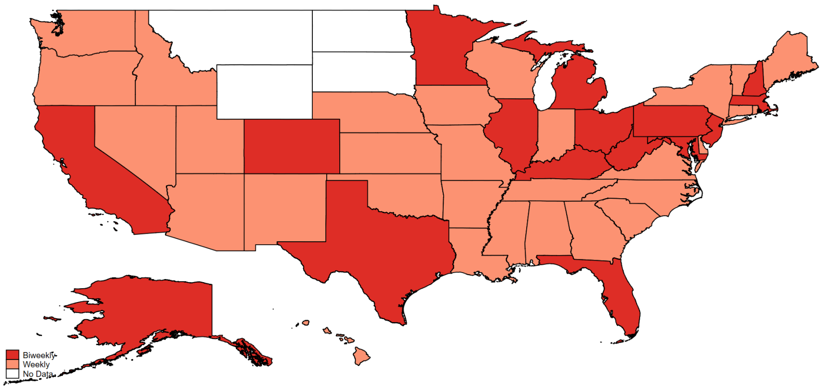
The frequency of UI benefit payments is actually the lowest rate at which unemployed workers can file their claims with the state labor authority. As one condition of continuous eligibility for UI benefits is active search for a new job, the workers must "re-certify" their claims every one or two weeks, depending on the state, in order to document their searching efforts. Payments often follow a few days after each re-certification. If workers file their claims every two weeks, each payout sum would be their entitled weekly benefit amount doubled.

Figure 3 demonstrates the distribution of UI claim filing frequency across states over a span of 18 years. At the begin of 2002, the majority of American states have already allowed eligible unemployed workers to file their claims every week. By the later half of 2019, several states in the North Eastern region have also switched from biweekly to weekly UI payouts. Nevertheless, there are still 9 states adhering to the biweekly frequency, among them some with the largest labor forces in the nation such as California, Texas, and Florida. Notably, Ohio allows both weekly and biweekly filing, but most claimants opt for the latter, making it a weekly-UI-paying state in practice (Zhang 2021).

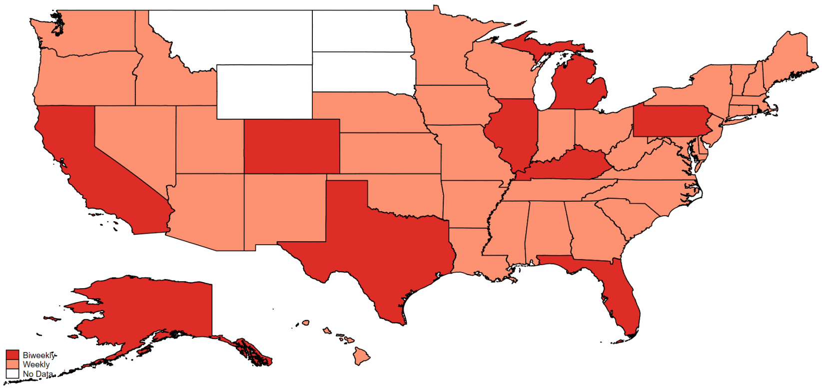
3 Data

For my analysis I use the CEX Interviews data from 2002 to 2019. I focus on this period for the practical reasons of data availability as well as avoidance of COVID-19 related restrictions on consumption. The American-representative CEX Interviews have a panel structure and are conducted every quarter for around 5,000 households. After a maximum of four such consecutive interviews, the household is rotated out of the sample and replaced by a new one. The information is collected on a recall basis, i.e. households provide details about their various categories of expenditures in the last three months.

Compared to other widely used public micro datasets, CEX's strength lies in the comprehensiveness of expenditure items, while its main weakness is the coarse coverage of income and finance. Only at the first and last rounds of interview are household members asked about their worked weeks and earnings during the last twelve months. No further information is given about their exact employment history or status at the



(a) January 2002



(b) July 2019

Figure 3: UI benefit payment frequency distribution across states
Source: Employment & Training Administration. No consistent data available for Montana, North Dakota, South Dakota, and Wyoming.

moment. Additionally, the interviewer inquire about the households' financial assets and debts at the time of the last interview and twelve months before that.

3.1 Sample Selection

To connect the expenditures data at the household level to the earnings and employment data at individual level, I focus on the primary earner in each household, defined as the one making more than half of that household's labor earnings throughout the year. If a primary earner declare themselves non-earners in subsequent interviews, I only keep those still actively looking for jobs. If there is a change of the primary earner between interviews, I drop the households entirely for consistency. Moreover, I also drop those with incomplete consumption or employment profile, self-employment income or with non-positive before-tax income or food consumption. For the sample I keep only workers who are in their prime age (from 25 to 55 years old) and at the time of their first interview report having worked 52 weeks and earned at least $4,000 in wage in the past twelve months. These filters are to make sure that those workers are in theory entitled to UI benefits if they lose their job at a later point. Since most states require a waiting-time of one week between the termination of the last job and first filing of UI claim, I exclude people reporting only one week of unemployment. Additionally, I drop observations with missing or top-coded salary and wealth data.[3] Finally, I also exclude people employed in armed forces, farming, forestry or fishing industry.

[3]Between 2004 and 2006 the CEX did not collect information on salary earned in the prior twelve months. Therefore, I impute this figure for this period from the last paycheck earned and the paycheck frequency (weekly, biweekly, or monthly). The underlying assumption is that workers earns a similar amount every working week.

3.2 Variables Construction

While we are interested in consumption, the CEX data contains only expenditures which can approximate consumption only to a limited extend. As a result, I follow the standard literature (Krueger & Perri 2006, Fernández-Villaverde & Krueger 2007) and consider four categories of observable expenditures, each one a subset of the one before it: food, strictly nondurable, nondurable, and total. Food expenditures are self-explaining, containing both at homa and away. Strictly nondurable goods and services include, besides food, also alcohol beverages, tobacco, utilities, personal care, household operations, public transportation, gas and motor oil, entertainment and miscellaneous expenditures. Nondurable goods and services further expand to health care without insurance, education, and reading. Finally, total expenditures encompasses every expenses of the households over the refered period. Each expenditure item is discounted using the corresponding category of the CPI to the 2010 U.S. dollars. Figure A2 in the Appendix summarizes the distribution of different log expenditures categories along interview rounds.

For each of the four expenditure categories above, I construct the measure of consumption changes between interview rounds, which is the main focus of my subsequent analysis, as:

$$\Delta C = \sum_{t=2}^{4} |\log C_t - \log C_{t-1}| \tag{1}$$

where C denotes expenditures in round t of interview. The formula 1 calculates the sum of absolute changes in the expenditure category between consecutive quarters. A smooth consumption profile throughout the year would imply a small, close-to-zero value of ΔC. In comparison with earlier studies which use annual panel data, this measure can capture more granular, within-quarter changes in consumption.

In order to construct variables for earnings and employment from the CEX data, I need to make two further assumptions: (1) people are paid the same amount every week during the periods in which they are employed and (2) those observing non-zero weeks of non-work in the data were all fired instead of quitting. The *eligible* WBA is then calculated for every worker in the sample *without* taking benefit caps into account. Instead of the *actual* UI benefits received only by unemployed workers, this amount is theoretically expected by everyone at the time of the first interview in case they are to lose their jobs in the next twelve months. The reason for disregarding the benefit caps is clarified in the next section.

Let hqw be the quarterly earning as implied by the salary reported in the first round[4], and τ being the UI benefit fraction as reported by the ETA, then we have

$$WBA = hqw \times \tau. \tag{2}$$

I also compute total income from other sources between the first and the fourth interview for each household, defined as family before-tax income minus the primary earner's salary. The exposure to unemployment UD in the 12 months prior to the last interview is the number of non-worked weeks. Due to lack of employment status indicator in the data, in my following analysis, I treat UD as the length of *one* unemployment spell during the year instead of the sum multiple shorter spells. Finally, net liquidity is defined as the total liquid wealth such as checking deposits and money

[4]To be exact, hqw is the highest quarter wage. As a result of the assumption made earlier, it is simply equal to prior annual salary divided by four.

market instruments minus total liquid borrowing in the form of credit card debts, both dated one year before the fourth interview.

3.3 Descriptive Statistics

Figure 4 demonstrates the distribution of non-working weeks among workers exposed to at least an unemployment spell. The majority of these workers are left without a job for less than 13 weeks, while more than 75% are unemployed for less than 24 weeks within the year, and only a small fraction face long-term or recurrent unemployment (40 weeks or more). I also account for the possibility that female workers temporarily stop working due to maternal leave, in which case they are not relevant for UI benefits analysis. There are however no such observations in the selected sample.

I compare the main characteristics of the "employed" and "unemployed" in the sample in Table 2. The former has zero exposure to unemployment as measured by UD, while the latter has at least two weeks of non-work. We can notice that despite similarities in age and family size, the group exposed to unemployment are more likely to be female and less likely to be college graduates. These factors might play a role, among others, to explain their lower average salary and, at household level, other income and net liquidity compared to the fully employed group. Among those exposed to unemployment, two noticeable patterns emerge. First, workers with medium unemployment exposure (between 12 and 23 weeks of non-work) are more likely to be female and college-educated than those with either small or large exposure. Second, the number of non-work weeks correlates positively with pre-unemployment salary but negatively with net liquidity.

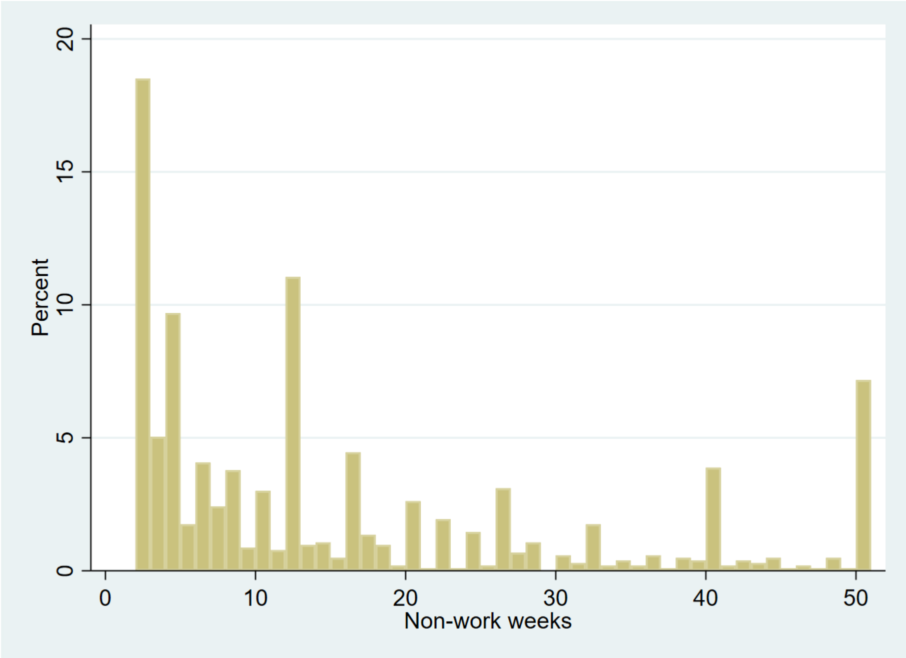


Figure 4: Number of non-working weeks in 12 months prior to the fourth interview.
Note: CEX Interview Data 2002-2019. Observation conditional on having worked for 52 weeks before the first interview (fully employed).

Characteristics	**Exposed to Unemployment** (in weeks)				**Fully employed**	**Total**
	Total	<12	12 - 23	≥ 24		
Age	40.38	40.55	39.64	40.79	40.94	40.89
Male share (%)	55.40	57.98	44.83	60.59	62.01	61.40
Family Size (persons)	2.94	2.97	2.81	3.02	2.98	2.98
College Degree (%)	47.35	44.54	57.33	43.22	52.17	51.72
Mean before-tax salary	39,782	39,048	39,609	41,433	47,208	46,523
Mean other income	33,271	32,894	32,688	34,604	34,607	34,484
Mean net liquidity	1,264	1,338	1,244	1,134	1,963	1,899
N	1,032	515	261	256	9,211	10,243

Table 2: Demographic Summary of Sample
Note: CEX first-round Interview Data 2002-2019. Observations conditional on working fully 52 weeks during 12 months before the first interview. Monetary values are in 2000 US$.

Next, I summarize in Table 3 consumption smoothing by different categories of log consumption change ΔC among the two groups by employment status and along the three dimensions of UI programs. The changes are all statistically significant for the whole sample and for every sub-samples that I look at. The implication is that consumption, especially in food, tend to vary over time. This variation holds even for the fully employed group, although to a significantly lesser extent than for the unemployment-exposed group. Among the latter, there is little evidence of differences in consumption changes around the caps on eligible benefit amount or potential benefit duration, or between a weekly and a biweekly payment schedule. Table A1 in the Appendix shows the correlation among the measures of consumption changes, unemployment exposure, and eligible weekly benefit amount.

4 Methodology

Out of the three dimensions that we are interested in, unemployment benefits are known to be haunted by the problem of endogeneity when used for assessing consumption smoothing (East & Kuka 2015). The reason is that the benefit amount is calculated by a function of pre-unemployment labor earnings, so any unobservable factors determining wage can also potentially affect consumption decicions. To circumvent this issue, I follow Card et al. (2015) and Landais (2015) and exploit the exogenous nature of state-wide benefit caps. Those deterministic ceilings impose a kink in the slope between previous wages[5], or the running variable, and the benefit

[5]To be exact, it is the uncapped eligible weekly benefit WBA, which is a fraction of previous wage earned, that I use in my analysis.

Classification	ΔC **Food**	ΔC **Strict ND**	ΔC **ND**	ΔC **Total**
Full Sample	0.992***	0.826***	0.896***	0.833***
	(0.007)	(0.006)	(0.006)	(0.007)
Fully Employed (1)	0.982***	0.822***	0.890***	0.827***
	(0.007)	(0.006)	(0.006)	(0.007)
Exposed to Unemployment (2)	1.090***	0.865***	0.949***	0.885***
	(0.025)	(0.019)	(0.022)	(0.024)
Difference (1)-(2)	-0.107***	-0.043*	-0.058**	-0.057*
	(0.023)	(0.019)	(0.021)	(0.023)
Among the Exposed				
$WBA < \overline{WBA}$ (3)	1.122***	0.847***	0.934***	0.866***
(N=519)	(0.037)	(0.031)	(0.032)	(0.032)
$WBA \geq \overline{WBA}$ (4)	1.058***	0.882***	0.964***	0.903***
(N=513)	(0.035)	(0.031)	(0.031)	(0.035)
Difference (3)-(4)	0.063	-0.035	-0.030	-0.038
	(0.051)	(0.039)	(0.044)	(0.047)
$UD < PBD$ (5)	1.088***	0.859***	0.947***	0.885***
(N=886)	(0.027)	(0.021)	(0.024)	(0.026)
$UD \geq PBD$ (6)	1.100***	0.902***	0.963***	0.881***
(N=146)	(0.069)	(0.055)	(0.056)	(0.059)
Difference (5)-(6)	-0.012	-0.044	-0.016	0.004
	(0.074)	(0.056)	(0.064)	(0.069)
Weekly filing (7)	1.081***	0.840***	0.927***	0.910***
(N=498)	(0.037)	(0.026)	(0.030)	(0.035)
Biweekly filing (8)	1.098***	0.888***	0.970***	0.861***
(N=534)	(0.035)	(0.028)	(0.032)	(0.032)
Difference (7)-(8)	-0.016	-0.048	-0.043	0.049
	(0.051)	(0.039)	(0.044)	(0.047)

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3: Consumption Smoothing Measurements ΔC

Note: Strict ND denotes strictly nondurable, ND denotes nondurable.

amount, or the policy variable. The appropriate method for causal inference in this situation is a *sharp* Regression Kink Design (RKD).[6]

[6] I assume that everyone is a complier with respect to the assignment rule.

4.1 Regression Kink Design

The main idea behind RKD is similar to that of standard regression discontinuity design (RDD): workers around an exogenous threshold are likely to be comparable but randomly assigned to different policies, which are unemployment benefits in this case. Different from RDD, in which there is a discontinuous jump in level of the treatment at the threshold, RKD focuses on the abrupt change in the treatment's *first derivative*, or a kink, at the cutoff (Card et al. 2015). Estimation also involves a local polynomial regression around the cutoff level as in the case of RDD except that the "shifting" intercept indicating exposure to treatment would be absent as we do not expect discontinuity. Formally, let Y be the outcome, V the observable running variable, and $X = x(V)$ the regressor of interest that is a deterministic function of V and has a kink at $V = 0$. Card et al. (2015) show that the weighted average marginal effect of X on Y is then

$$E[Y|V = 0, X = x(0)] = \frac{\lim_{v_0 \to 0^+} \frac{dE[Y|V=v]}{dv}|_{v=v_0} - \lim_{v_0 \to 0^-} \frac{dE[Y|V=v]}{dv}|_{v=v_0}}{\lim_{v_0 \to 0^+} x'(v_0) - \lim_{v_0 \to 0^-} x'(v_0)}. \qquad (3)$$

The identification strategy for RKD works under two assumptions. First, all covariates other than the running variable must not experience a change in slope around the kink point. I validate this assumption for the regression controls by providing graphical evidence in Figure A1 (see Appendix). Specifically, I plot the bin-averages of age, education levels, other household income, family size, weeks of non-working, and pre-unemployment net liquidity against the eligible weekly benefits normalized by the respective caps. There are no visible changes in slope of all these variables at the zero kink.

Second, the assignment variable must have a smooth density around the kink point. In our context, people should not manipulate their labor earnings in order to affect their expected unemployment benefits and sort themselves relative to the kink point. There are practical reasons to believe in the validity of this assumption. Since the cutoff level in UI benefits might be adjusted every year, it is impossible to manipulate one's earnings *one year* prior to the supposedly unexpected laid-off date.

Still, for concrete evidence, I implement a manipulation test in the spirit of McCrary (2008) and Cattaneo et al. (2020) for the log ratio of eligible weekly benefits to the benefit cap. The result of the test shows no evidence supporting the possibility of self-selection, validating the assumption. Graphically, there is no bunching of observations at either sides of the kink, as can be seen in Figure 5.

4.2 Specification

I run the following regression for household head i in state s at time t with standard error clustered at state level:

$$\begin{aligned}\Delta C_{ist} = \alpha &+ \gamma UD_{ist} + \beta_0 UD_{ist} * \widetilde{WBA}_{ist} + \beta_1 UD_{ist} * D_1 * \widetilde{WBA}_{ist} \\ &+ \beta_2 UD_{ist} * PBD_{st} + \beta_3 UD_{ist} * Freq_{st} + \delta UD_{ist} * D_2 \\ &+ \Theta_1 X_{ist} + \Theta_2 Z_{st} + \phi_t + \mu_s + \epsilon_{ist}\end{aligned} \tag{4}$$

where

$$\widetilde{WBA}_{ist} = \frac{WBA_{ist} - \overline{WBA}_{st}}{\overline{WBA}_{st}} \times 100$$

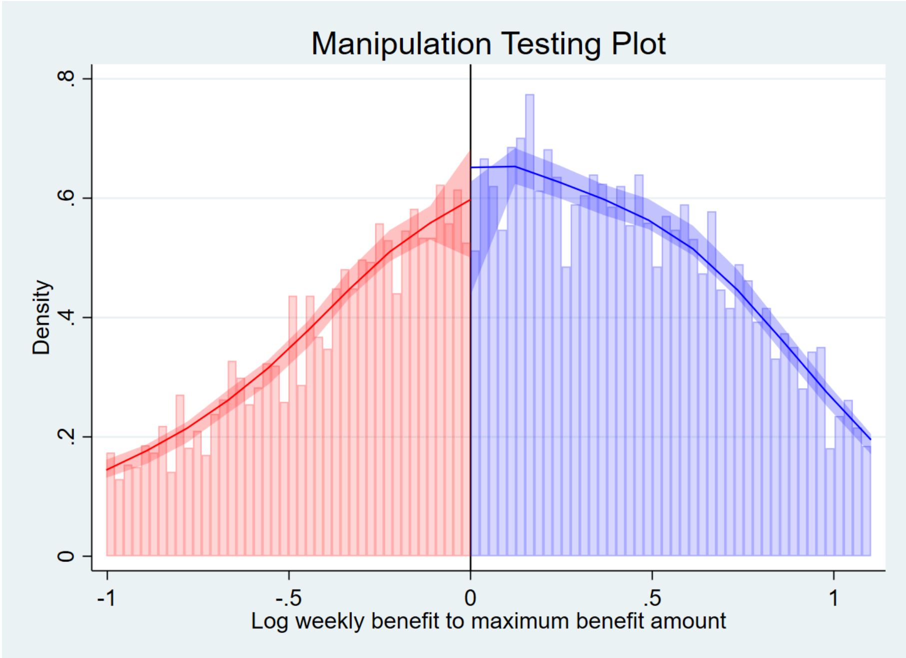


Figure 5: Graphical evidence for smooth density of running variable
Kink at 0. Eligible weekly benefits lie below the cap in the red region and above the cap in the blue region. The curves represent the kernel density. Shaded areas denote the 95% confidence interval. McCrary-type Test Statistics: -0.8733 (*p-value* = 0.3825)

denote the weekly benefit WBA normalized, as percentage, at the respective state-specific cap $\overline{WBA}$. The kink in benefit amount is therefore transformed to zero. UD is the exposure to unemployment as constructed in Section 3, PBD is the potential benefit duration, and $Freq$ is the binary indicator of UI payment frequency (0 if weekly, 1 otherwise). The two dummy variables D_1 and D_2 are indicators for WBA exceeding the benefit cap $\overline{WBA}$ and UD exceeding the duration cap PBD, respectively. Vector X contains workers' relevant controls: quadratic polynomials in age, gender, educational attainment, living in urban or rural dummy, marital status, changes in family size, number of children, income from other sources, pre- and post-unemployment salary, and pre-unemployment liquidity.[7] Meanwhile, vector Z controls for state-specific time-varying economic characteristics, namely unemployment rate and per-capita expenditures on public welfare programs *other than* UI benefits.[8] Finally, ϕ and μ correspondingly denote time and state fixed effects.[9]

The variables representing three UI dimensions all appear in interaction with the exposure to unemployment UD. This specification explicitly imposes that the treatment of interest is conditional on workers experiencing at least one unemployment spell. If worker i is fully employed during the 12 months prior to the last interview, i.e. $UD_i = 0$, she belongs to the control group as UI programs' dimensions are not relevant to her. Moreover, the effects of three UI dimensions are also dependent on the exposure to unemployment. The coefficient γ denotes the slope in consumption change along the duration of unemployment.

The first line of Equation 4 contains the RKD regression for weekly benefit amount with linear polynomial specification around the benefit cap. Coefficient β_0 is the slope of consumption change along benefit amount below the kink, while β_1 is the change in slope above the kink. The other coefficients of interest are $\{\beta_2, \beta_3\}$, which respectively measure the effects of potential benefit duration and benefit payment frequency on consumption change. If a dimension helps smooth consumption during unemployment, we expect the associated coefficients to be negative. Meanwhile, the coefficient δ denote the changes in consumption if unemployment duration exceed the

[7]For readability of estimates, I monotonically transform net liquidity a, which might be negative, into the log scale using the formula $\log(a + \sqrt{a^2 + 1})$.

[8]State-level unemployment rate is collected from the Bureau of Labor Statistics. Detailed data on personal current transfer receipts and unemployment insurance compensation by state is available on the website of the Bureau of Economic Analysis, while state population data is from the Census.

[9]Time fixed effects have two separate components: year and calendar quarter. Since we are interested in consumption changes, we should also consider seasonality.

maximum thresholds, in which case UI programs are phased out immediately.

Theoretically speaking, we could also implement the RKD procedure in a similar manner when evaluating the effect of potential benefit duration on consumption smoothing. While unemployment duration can be endogenous to the ad-hoc hard ceilings, the technical caps imposed by the total benefit payment during unemployment, as discussed in Section 2, can serve as a work-around as shown by Landais (2015). However, I abstain from this analysis because the latter type of cap rarely binds in the sample. Keep in mind that I retain only workers who are fully employed for the whole year before the first interview, so these people have a relatively high labor earnings in the base period compared to the weekly benefit amount they are entitled to. As a result, their technical duration caps are generally higher than the state-wide hard ceilings and not relevant for workers[10].

5 Main Results

5.1 Benchmark Results

I run the regression 4 for the full sample with four different measures of consumption change outlined before. I report in Table 4 only the estimates corresponding to variables of interest in this study. Note that the RKD estimates, reported here as the coefficients of the interaction term denoting the right side of the benefit kink

[10]For example, denote hqw the highest quarter wage and assume the worker earns the same amount every working quarter. The base period wage is $4hqw$, and, with a one-third fraction, the total benefit payment is $\frac{4hqw}{3}$. Dividing this amount by the typical weekly benefit of $hqw/26$ yields a duration cap of nearly 35 weeks.

$UD \times D_1 \times \widetilde{WBA}$, and their respective standard errors have been normalized by the deterministic change in slope of weekly benefits per Equation 3 to recover the real treatment effects. This value equals to $-\tau$, the UI fraction in Equation 2, as the slope before the kink is τ and after the kink is 0 due to the benefit cap. I choose a universal $\tau = 26$ for simplicity.

	Compounded changes in expenditures for...			
	Food	Strict Nondurables	Nondurables	Total
UD	1.0257 ***	0.0458	0.3882	0.1106
	(0.3556)	(0.2890)	(0.3097)	(0.3321)
UD$\times\widetilde{WBA}$	-0.0088 **	-0.0088***	-0.0117 ***	-0.0040
	(0.0042)	(0.0030)	(0.0029)	(0.0032)
UD$\times D_1 \times \widetilde{WBA}$	-0.2652**	-0.2938***	-0.3406***	-0.0832
	(0.1248)	(0.1014)	(0.1118)	(0.0962)
UD$\times$PBD	-0.0130 ***	-0.0052	-0.0095***	-0.0009
	(0.0042)	(0.0034)	(0.0035)	(0.0040)
UD$\times D_2$	-0.7782 ***	-0.0904	-0.3009	0.0733
	(0.2248)	(0.1676)	(0.1911)	(0.2712)
UD $\times$ UIFreq	-0.2014	0.1764	0.0766	0.0122
	(0.2221)	(0.1637)	(0.1778)	(0.1763)
Observations	10,243	10,243	10,243	10,243
R^2	0.033	0.035	0.038	0.019

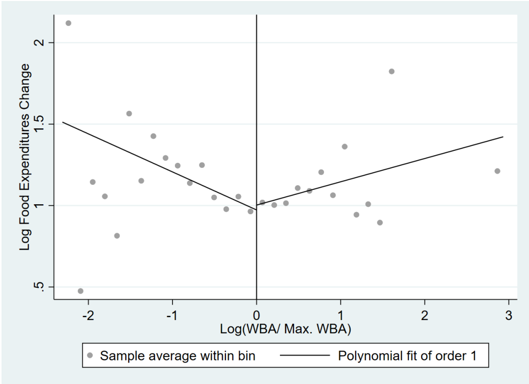
Standard errors in parentheses, clustered at state level.
Coefficients and standard errors are scaled up by 100 for readability.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Baseline Regressions - Full Sample
Note: Third row adjusted by slope change in UI schedule. $UIFreq = 0$ if weekly, $= 1$ if biweekly. $D_1 = \mathbb{1}\{WBA \geq \overline{WBA}\}$, $D_2 = \mathbb{1}\{UD \geq PBD\}$.

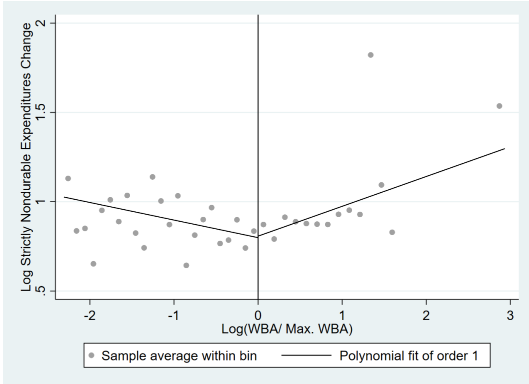
Overall the weekly benefit amount help workers smooth their consumption with statistical significance in all categories but total expenditures. The economic significance is also substantial, as a one-percentage-point increase in eligible UI benefits relative to the cap decreases fluctuations in expenditures for food by 0.27%, for strict nondurables by 0.29%, and for nondurables by 0.34% on average for every week exposed to unemployment. The potential benefit duration also plays a role in enabling consumption smoothing in food and nondurables between interview rounds. For the median worker exposed to unemployment with 12 weeks of non-work, one more week of potential benefit eligibility decreases her consumption changes between 0.11% (nondurables) and 0.16% (food). Exceeding the maximum duration correlates with further consumption smoothing, indicative of workers' adaption to long-term unemployment, but the effect is only statistically significant in the case of food. Meanwhile, UI payment frequency does not affect consumption smoothing in any clear way. The results stay fundamentally unchanged if I use a local quadratic specification for the RKD around the kinks instead of a linear one.

Figure 6 provides further graphical evidence for my RKD estimators. I plot the average changes in log of food, strictly nondurable, and nondurable expenditures within bins against the log of weekly benefit amount normalized by benefit caps for the exposed group. The general pattern is clear: the negative slope to the left side of the kink indicates that UI benefits help smooth consumption up to the cap. The discontinuity in the benefit schedule's first derivative, triggered at the kink point, leads to sharp, significant reverses in slopes of all outcomes but total expenditures changes.

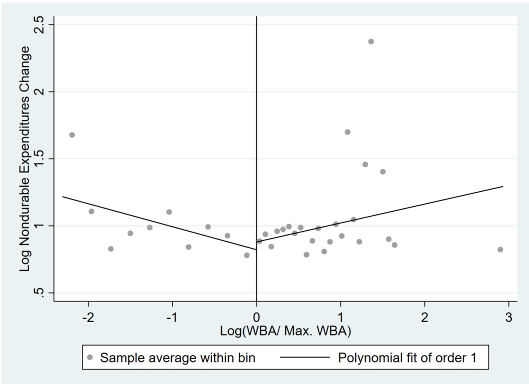
While food is an important category of expenditures smoothed by UI programs, the large magnitude of benefits' effects on nondurables suggests that it is also interesting to look at non-food items. Therefore, I also run the same regression with other



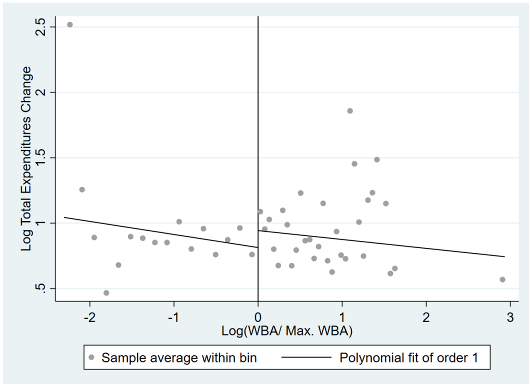
(a) Food



(b) Strictly Nondurable



(c) Nondurable



(d) Total

Figure 6: RKD Plots for log consumption changes against weekly benefit amount
Note: Kink of running variable at 0. Linear polynomial regression, full bandwidth. Only workers exposed to unemployment.

categories: strictly nondurable excluding food (Strict ND ex. Food), nondurable excluding food (ND ex. Food), and semi-nondurable, i.e. only ambiguous items such as healthcare, education, and reading (ND ex. Food and Strict ND). The results are shown in Table 5. Again, weekly benefit amount plays an significant role, both quantitatively and statistically, in reducing consumption fluctuations in all three categories. It is however noteworthy that the effects are much larger than those in the benchmark cases, ranging from 0.29% to 0.65% of reduction in expenditure changes per one-percentage-point increase in $\widetilde{WBA}$. Figure A4 in the Appendix demonstrates graphically that semi-nondurables are the most volatile categories among the unemployed. Clearly, benefits help smooth these expenditures to a greater extend than that in food. Benefit duration only affects consumption of semi-nondurables, while payment frequency still has no significant effect.

In summary, the baseline results indicate that benefit amount is by far the most important feature of UI programs in smoothing consumption, followed by benefit duration with modest effect and benefit payment frequency with no effect at all. The absence of frequency's effect can be attributed to the lack of granular variations in the data. If we keep the UI programs unchanged, the results can also be interpreted that a larger exposure to unemployment, i.e. higher UD, reduces consumption fluctuations.

5.2 Heterogeneous Effects

Next, I am interested in the heterogeneity of UI benefits' effects on workers of different exposure to unemployment. I run the regression for fully employed workers ($UD = 0$) and each of the following two groups alternately: those with less than 12 weeks of non-work ("short exposed") and those with more than 24 weeks of non-work ("long exposed").[11] The results, in Table A2, illustrates a clear difference in the consumption smoothing effect of benefit amount between these two groups. The effects are significant for strict nondurables and nondurables in general, but are more pronounced for workers with short exposure to unemployment, indicating that the effect wears

[11]The first group represents more than half of the exposed, while the second group more than one fourth.

	Compounded changes in expenditures for...		
	Strict ND *ex. Food*	ND *ex. Food*	ND *ex. Food and Strict ND*
UD	-0.3245	0.1090	3.0871 ***
	(0.3328)	(0.3419)	(0.8170)
UD$\times\widetilde{WBA}$	-0.0084**	-0.0130***	-0.0266 ***
	(0.0033)	(0.0038)	(0.0096)
UD$\times D_1 \times \widetilde{WBA}$	-0.2886**	-0.3640**	-0.6500**
	(0.1170)	(0.1430)	(0.2886)
UD$\times$PBD	0.0012	-0.0043	-0.0385***
	(0.0036)	(0.0039)	(0.0123)
UD$\times D_2$	0.0493	-0.1978	-2.3033***
	(0.2501)	(0.2605)	(0.8041)
UD $\times$ UIFreq	0.1847	0.0450	-0.8053
	(0.1860)	(0.1973)	(0.7207)
Observations	10,243	10,243	10,243
R^2	0.038	0.037	0.109

Standard errors in parentheses, clustered at state level.
Coefficients and standard errors are scaled up by 100 for readability.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Regressions with Non-Food Categories - Full Sample
Note: Third row adjusted by slope change in UI schedule. $UIFreq = 0$ if weekly, $= 1$ if biweekly.
$D_1 = \mathbb{1}\{WBA \geq \overline{WBA}\}$, $D_2 = \mathbb{1}\{UD \geq PBD\}$.

out over the length of unemployment. The short-exposed group is also, by definition, not affected by the cap on duration. Additionally, payment frequency also plays a role in smoothing strictly nondurable consumption, with weekly filing claimants able to reduce the fluctuations in this category by more than 1.4% compared to biweekly claimants.

I also want to check if UI programs' effects change along the business cycle. I divide my sample into two sub-periods: one between 2008Q3 and 2009Q4 during the fallout of the Great Recession when unemployment spiked up throughout the U.S., and one outside of this time interval. The results are reported in Table A3, and two interesting patterns stand out. For food, strict nondurable, and nondurable consumption changes, the effects of benefit amount and maximum duration are negative and only statistically significant during episodes outside the Recession. Meanwhile, during the Recession fluctuations in total expenditures significantly *increase* in eligible benefit amount and in excession of maximum duration. Overall the consumption smoothing effect of UI programs seems to be cyclical. This result stands in contrast to that of Kroft & Notowidigdo (2016), who find pro-cyclical moral hazard but non-cyclical consumption smoothing benefit of UI programs. The explanation for this difference might lie in the fact that they use average benefit level for each state instead of individual, cap-subject amounts.

6 Robustness

6.1 Imputed measures of consumption

Though quite comprehensive, the CEX data that I have used only records expenditures if there exist actual transactions for those during the interview period. That design essentially leaves out two important flows of consumption from owned housing and from owned vehicles, which both might potentially affect my results. Therefore, I construct imputed values for rent and vehicle consumption and then add them to strict nondurables, nondurables, and total expenditures.

In specific, imputed rent for homeowners are collected directly in the CEX interviews.

Households owning the dwelling in which they are living are asked how much, in their opinion, the place would have cost if rented out. Imputing vehicle consumption is more complicated, and I follow the procedure by Cutler et al. (1991). I use information from households who actually purchase new or used vehicles in the sample in order to predict total car values for the rest. Afterwards, I amortize these sum over the life span of the average car to get the flow of consumption. Details are available in Section A.2 in the Appendix.

	Compounded changes in expenditures for...		
	Strict Nondurables	Nondurables	Total
UD	0.2276	0.5006	0.1336
	(0.2767)	(0.3092)	(0.3236)
UD$\times\widetilde{WBA}$	-0.0053*	-0.0088***	-0.0043
	(0.0027)	(0.0029)	(0.0034)
UD$\times D_1 \times \widetilde{WBA}$	-0.1742*	-0.2470**	-0.0936
	(0.0962)	(0.1040)	(0.1014)
UD$\times$PBD	-0.0053	-0.0096**	-0.0008
	(0.0035)	(0.0038)	(0.0038)
UD$\times D_2$	-0.1785	-0.3677*	0.0117
	(0.1726)	(0.2114)	(0.2446)
UD$\times$UIFreq	0.1171	0.0762	0.0057
	(0.1589)	(0.1731)	(0.1681)
Observations	10,243	10,243	10,243
R^2	0.026	0.027	0.019

Standard errors in parentheses, clustered at state level.
Coefficients and standard errors are scaled up by 100 for readability.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Regressions with Imputed Rent and Vehicle Expenditures - Full Sample
Note: Third row adjusted by slope change in UI schedule. $UIFreq = 0$ if weekly, $= 1$ if biweekly.
$D_1 = \mathbb{1}\{WBA \geq \overline{WBA}\}$, $D_2 = \mathbb{1}\{UD \geq PBD\}$.

Running regression 4 with the new categories including imputed rent and vehicle consumption yields results in the same direction as the baseline, as shown in Table 6. Benefit amount helps smooth consumption in both strict nondurables and nondurables, but the magnitude of the effects is relatively smaller in both cases, suggesting that unemployed workers do not adjust their rent and car usage by any significant margin. This implication is not surprising, as these expenses are consumption commitments that normally do not fluctuate much over time (Chetty & Szeidl 2007).

6.2 Endogeneous Exposure Duration

It can also be the case that the interaction with the potentially endogenous exposure to measurement UD biases the results. After all, I have so far built my analysis on a quite strong assumption that workers stay unemployed only because they do not receive any job offer, which critically ignore the role of moral hazard in determining actual unemployment duration. To address this concern, I proceed in two different directions. In the first approach, instead of UD, I interact the three UI dimensions with a binary indicator that equals to zero if workers are fully employed during the twelve months prior to the last interview and one otherwise. Such a dummy variable is informative only about workers being separated from their last jobs, not on how long they spend in unemployment. Therefore, it is not prone to endogeneity. The results are reported in Table 7, conditional on the interactive unemployment dummy being one. We can easily see that for this specification only the effects of benefit amount on food and nondurables remain statistically significant, despite a clear fall in the economic magnitude. Earlier significant effects of the potential benefit duration disappear across the board.

	Compounded changes in expenditures for...			
	Food	Strict Nondurables	Nondurables	Total
Unemployment dummy	-0.0195	0.1319**	0.0771	0.0650
	(0.1178)	(0.0603)	(0.0729)	(0.1252)
$\widetilde{WBA}$	-0.0040**	-0.0019	-0.0028**	-0.0013
	(0.0018)	(0.0013)	(0.0010)	(0.0016)
$D_1 \times \widetilde{WBA}$	-0.1144**	-0.0624*	-0.0806***	-0.0260
	(0.0494)	(0.0312)	(0.0260)	(0.0416)
PBD	-0.0016	0.0003	-0.0008	-0.0020
	(0.0014)	(0.0014)	(0.0016)	(0.0017)
D_2	- 0.0652	0.0331	0.0055	0.0269
	(0.0647)	(0.0451)	(0.0526)	(0.0746)
UIFreq	-0.0688	0.0880*	0.0210	-0.0369
	(0.0618)	(0.0475)	(0.0509)	(0.0716)
Observations	10,243	10,243	10,243	10,243
R^2	0.035	0.038	0.041	0.020

Standard errors in parentheses, clustered at state level.

Coefficients and standard errors are scaled up by 100 for readability.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Regressions with Unemployment Dummy - Full Sample

Note: Third row adjusted by slope change in UI schedule. $UIFreq = 0$ if weekly, $= 1$ if biweekly. $D_1 = \mathbb{1}\{WBA \geq \overline{WBA}\}$, $D_2 = \mathbb{1}\{UD \geq PBD\}$.

The second approach directly targets the inherent endogeneity problem of unemployment exposure measure. I follow the two-stage strategy of Chetty (2008) and East & Kuka (2015) for the number of non-working weeks. In the first stage, I want to isolate the components in UD that are not explained by observables and potentially correlate with workers' ability to smooth consumption. To do this, I regress UD of the sub-sample exposed to unemployment on their individual characteristics vector X, the state-specific vector Z, and state and time fixed effects, i.e. variables included

	Compounded changes in expenditures for...			
	Food	Strict Nondurables	Nondurables	Total
$\hat{UD}$	0.7503*	0.3048	0.5399*	0.3881
	(0.3272)	(0.2463)	(0.2617)	(0.3222)
$\hat{UD} \times \widetilde{WBA}$	-0.0130	-0.0114*	-0.0166***	-0.0067
	(0.0071)	(0.0046)	(0.0043)	(0.0043)
$\hat{UD} \times D_1 \times \widetilde{WBA}$	-0.3874	-0.3354*	-0.4342**	-0.1352
	(0.2054)	(0.1274)	(0.1248)	(0.1274)
$\hat{UD}\times$PBD	-0.0122*	-0.0070*	-0.0095*	-0.0027
	(0.0051)	(0.0034)	(0.0040)	(0.0042)
$\hat{UD} \times \hat{D}_2$	-0.4876	-0.5867	-0.6442*	-0.0129
	(0.4539)	(0.3055)	(0.3183)	(0.2948)
$\hat{UD}\times$UIFreq	-0.1309	0.1312	-0.0904	-0.0203
	(0.2711)	(0.2334)	(0.2556)	(0.2114)
Observations	10,243	10,243	10,243	10,243
R^2	0.033	0.035	0.039	0.020

Standard errors in parentheses, clustered at state level.

Coefficients and standard errors are scaled up by 100 for readability.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Regressions with Predicted Exposure to Employment - Full Sample
Note: Third row adjusted by slope change in UI schedule. $UIFreq = 0$ if weekly, $= 1$ if biweekly. $D_1 = \mathbb{1}\{WBA \geq \overline{WBA}\}$, $\hat{D}_2 = \mathbb{1}\{\hat{UD} \geq PBD\}$.

in the baseline regression other than UI programs' three dimensions. I then use the estimated coefficients from this regression to predict exposure $\hat{UD}$ for these workers which are free of unobservable variations. In the second stage, I use this predicted exposure measure in place of UD in Equation 4, the results of which are reported in Table 8. In general, the patterns that we observe in the baseline regressions still persist here: stronger consumption smoothing effects of eligible benefit amount on wider categorization of nondurables, and modest effects of the potential benefit duration.

The size and sign of the effects found earlier in the baseline regressions are therefore robust.

7 Conclusion

In this paper I study the consumption smoothing effects of three fundamental features of UI programs across American states: the eligible benefit amount per week, the potential benefit duration, and the benefit payment frequency. The regulation regarding these three dimensions varies exogenously across states and time, creating the chance for clear identification. Moreover, I exploit kinks in the UI benefit schedule originated from state-specific ceilings in order to address the endogeneity concern about benefit amount. Using quarterly consumption data, I find clear evidence that the level of UI benefit decreases fluctuations in workers' different expenditure categories during spells of unemployment. The effects are most notable for food and semi-nondurables such as healthcare, education, and reading, while absent for durable consumption. The potential benefit duration also has modest smoothing effects on certain categories, while payment frequency does not play a significant role. These findings shed light on a so far under-studied aspect of UI programs.

One limitation of this study is the exclusion of the moral hazard aspect of UI programs. If, for example, more generous UI both in benefit amount and duration is granted, workers might actively extend their unemployment spells in length, possibly leading to a deeper drop in consumption once benefits eventually expire. Moreover, such generosity must also be viewed in conjunction with governments' budget constraints. Future work that aim to study consumption smoothing alongside endogenous unemployment duration, taking moral hazard into account either with better data or with an appropriate identification strategy, is promising from both an academic and policy-making perspective.

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A Appendix

A.1 Imputation of Vehicle Consumption

The imputation procedure consists of two steps. First, I regress total car purchases, both new and used, on demographic controls and non-car expenditures. The controls include a triangular polynomials in age, education, marital status, family size, number of earners, living in urban dummy, and time fixed effects. In the second step, I use estimated coefficients to predict the value of cars owned by each household in my sample. Finally, I impute the quartarly consumption flow of this vehicle stock by the amortization method, assuming an average 8-year life cycle of cars (Cutler et al. 1991).

A.2 Tables

Variables	ΔC **Food**	ΔC **Strict ND**	ΔC **ND**	ΔC **Total**	**UD**	**WBA**
ΔC **Food**	1					
ΔC **Strict ND**	0.4430	1				
ΔC **ND**	0.3835	0.8540	1			
ΔC **Total**	0.1454	0.3229	0.3858	1		
UD	0.0342	0.0186	0.0149	0.0123	1	
WBA	-0.0218	0.0911	0.1034	0.0250	-0.0537	1

Table A1: Correlation between measures of consumption changes, unemployment exposure, and eligible weekly benefit amount

Note: ND denotes strictly nondurable, ND+ denotes nondurable. $N = 1,032$.

	Compounded changes in expenditures for...							
	Food		Strict Nondurables		Nondurables		Total	
Exposure	Short	Long	Short	Long	Short	Long	Short	Long
UD	-0.6407	1.6371**	-0.7398	0.3135	-0.1374	0.6074	0.7548	-0.5597
	(1.1627)	(0.6247)	(0.8302)	(0.3935)	(0.8339)	(0.4751)	(0.9952)	(0.4018)
UD$\times\widetilde{WBA}$	-0.0301	-0.0049	-0.0310**	-0.0056	-0.0380***	-0.0083**	0.0151	-0.0040
	(0.0225)	(0.0038)	(0.0151)	(0.0034)	(0.0117)	(0.0035)	(0.0139)	(0.0033)
UD$\times D_1 \times \widetilde{WBA}$	-1.2740*	-0.1404	-0.9932*	-0.2054*	-1.1284***	-0.2496*	0.4888	-0.0962
	(0.7124)	(0.1040)	(0.5148)	(0.1118)	(0.4134)	(0.1248)	(0.4602)	(0.0962)
UD$\times$PBD	-0.0129	-0.0179**	-0.0075	-0.0074	-0.0108	-0.0116**	0.0037	0.0063
	(0.0129)	(0.0069)	(0.0139)	(0.0046)	(0.0126)	(0.0052)	(0.0139)	(0.0054)
UD$\times D_2$		-1.0234**		-0.2159		-0.4208		0.5144
		(0.4144)		(0.2448)		(0.3141)		(0.3481)
UD $\times$ UIFreq	0.8725	-0.3305	1.4136**	0.1783	0.5061	0.1610	0.3934	0.0743
	(0.8578)	(0.2502)	(0.5976)	(0.1671)	(0.7119)	(0.1793)	(0.6814)	(0.1892)
Observations	9840	9467	9840	9467	9840	9467	9840	9467
R^2	0.0324	0.0327	0.0355	0.0351	0.0389	0.0384	0.0193	0.0206

Standard errors in parentheses, clustered at state level.

Coefficients and standard errors are scaled up by 100 for readability.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Regressions along Unemployment Exposure Length

Note: Third row adjusted by slope change in UI schedule. $UIFreq = 0$ if weekly, $= 1$ if biweekly.
$D_1 = \mathbb{1}\{WBA \geq \overline{WBA}\}$, $D_2 = \mathbb{1}\{UD \geq PBD\}$.

	Compounded changes in expenditures for...							
	Food		Strict Nondurables		Nondurables		Total	
Recession	Y	N	Y	N	Y	N	Y	N
UD	2.9684	0.8416**	0.2339	0.1155	0.4754	0.4901	0.1172	0.2462
	(2.4052)	(0.3699)	(1.1114)	(0.3321)	(1.0424)	(0.3464)	(1.4501)	(0.3428)
UD×$\widetilde{WBA}$	0.0135	-0.0115**	0.0007	-0.0095***	0.0016	-0.0131***	0.0186**	-0.0071*
	(0.0200)	(0.0046)	(0.0108)	(0.0032)	(0.0094)	(0.0032)	(0.0085)	(0.0035)
UD×$D_1 \times \widetilde{WBA}$	0.5408	-0.3380**	0.7800	-0.3146***	0.2028	-0.3796***	0.5824*	0.1664
	(0.7826)	(0.1326)	(0.4056)	(0.1092)	(0.3692)	(0.1196)	(0.3432)	(0.1040)
UD×PBD	-0.0155	-0.0141***	0.0006	-0.0062*	0.0001	-0.0112***	0.0035	-0.0024
	(0.0205)	(0.0046)	(0.0132)	(0.0035)	(0.0118)	(0.0035)	(0.0188)	(0.0043)
UD×D_2	-1.8224	-0.6656**	0.0876	-0.1707	0.8285	-0.4443**	1.9031**	-0.1522
	(1.2390)	(0.2563)	(0.5173)	(0.2007)	(0.5551)	(0.2082)	(0.7667)	(0.2808)
UD × UIFreq	-1.6280*	-0.0405	-0.5455	0.2171	-0.6188	0.1174	-0.2270	0.0306
	(0.9467)	(0.2091)	(0.4878)	(0.1706)	(0.3919)	(0.1945)	(0.2903)	(0.1744)
Observations	851	9,323	851	9,323	851	9,323	851	9,323
R^2	0.144	0.033	0.086	0.036	0.078	0.040	0.088	0.020

Standard errors in parentheses, clustered at state level.

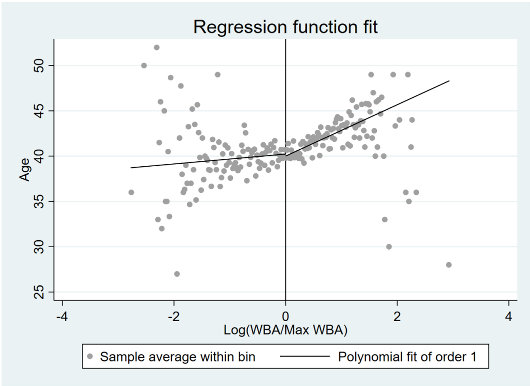
Coefficients and standard errors are scaled up by 100 for readability.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

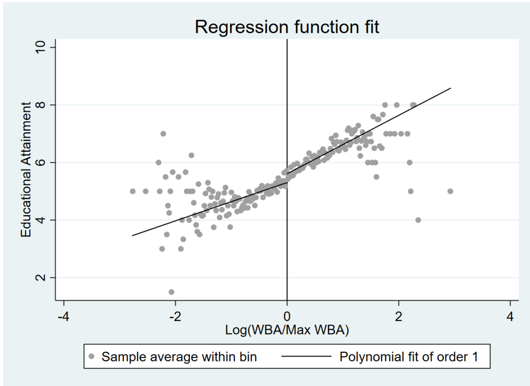
Table A3: Regressions along the Business Cycle

Note: Third row adjusted by slope change in UI schedule. $UIFreq = 0$ if weekly, $= 1$ if biweekly. $D_1 = \mathbb{1}\{WBA \geq \overline{WBA}\}$, $D_2 = \mathbb{1}\{UD \geq PBD\}$.

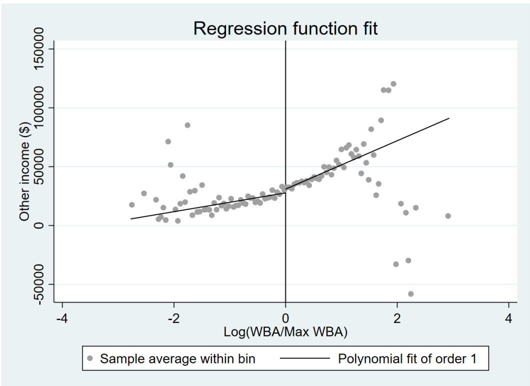
A.3 Graphs



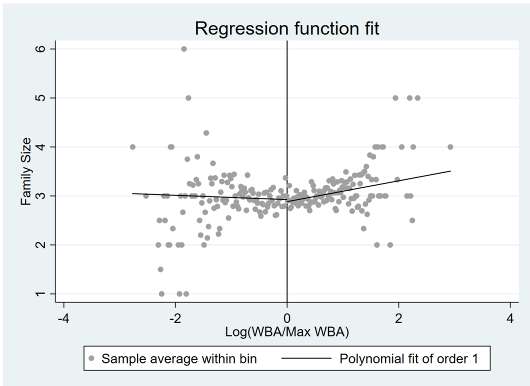
(a) Age



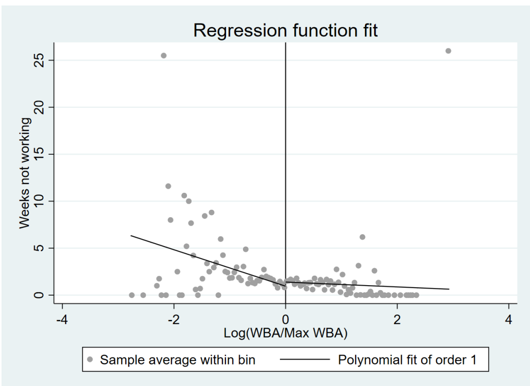
(b) Education



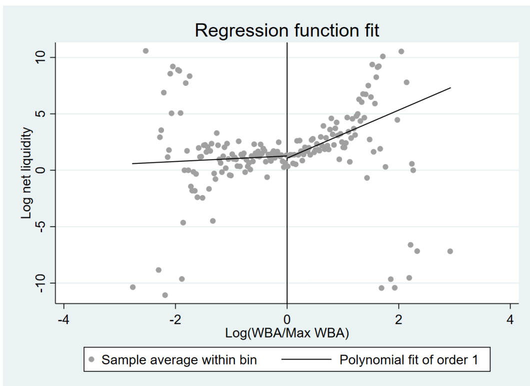
(c) Other household income



(d) Family Size



(e) Non-worked weeks



(f) Net liquidity

Figure A1: RKD Plots for Covariates

Note: Kink of running variable at 0. Education levels are categorical: (1) no schooling, (2) 8th grade, (3) 12th grade but no diploma, (4) high school diploma, (5) Some college, (6) Associate's degree, (7) Bachelor's degree, (8) Postgraduate.

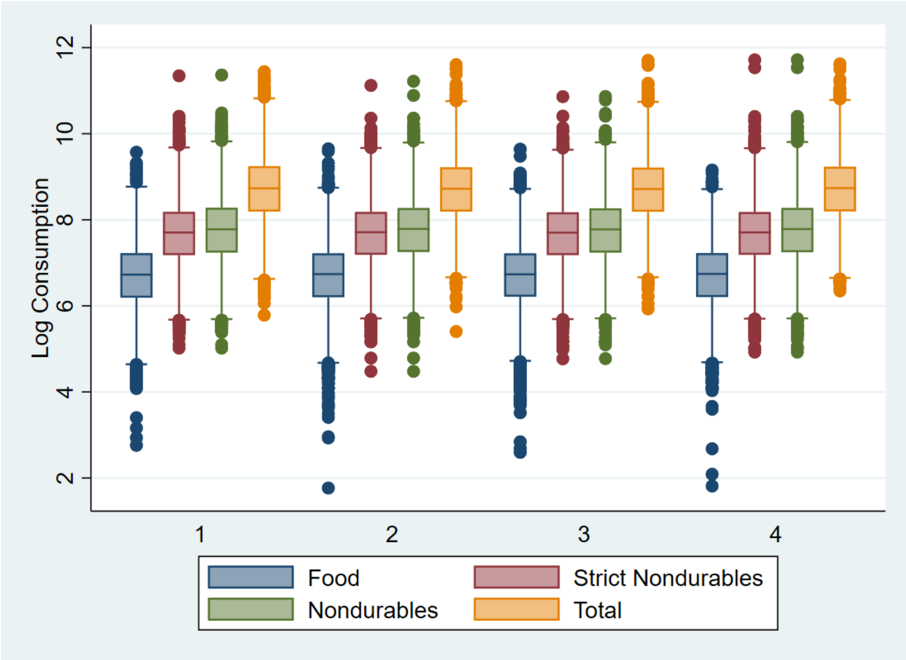


Figure A2: Box Plots - Log Consumption Distribution over Interview Rounds
Note: Full Sample

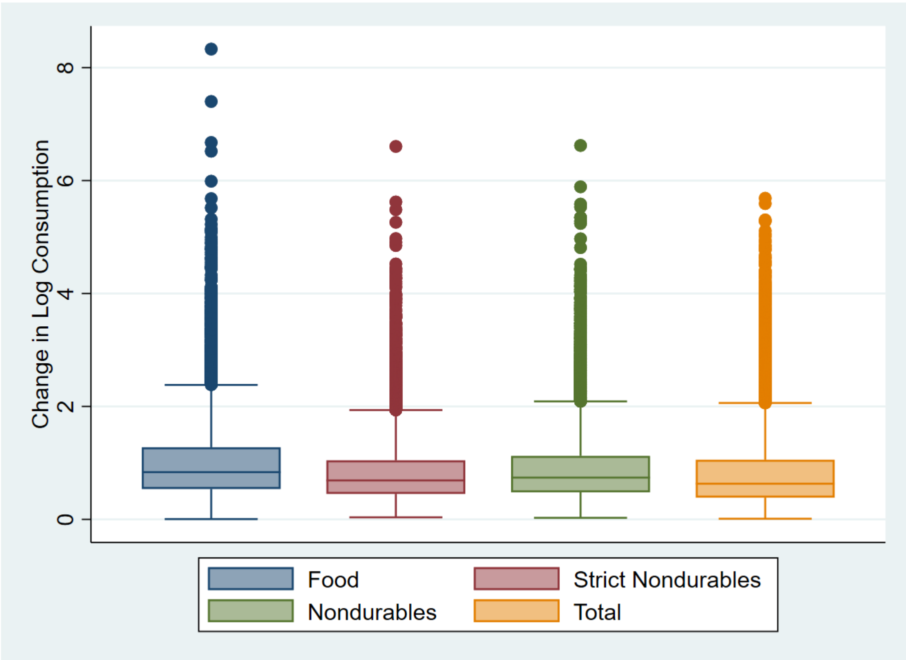


Figure A3: Box Plots - Log Consumption Change Distribution
Note: Full Sample

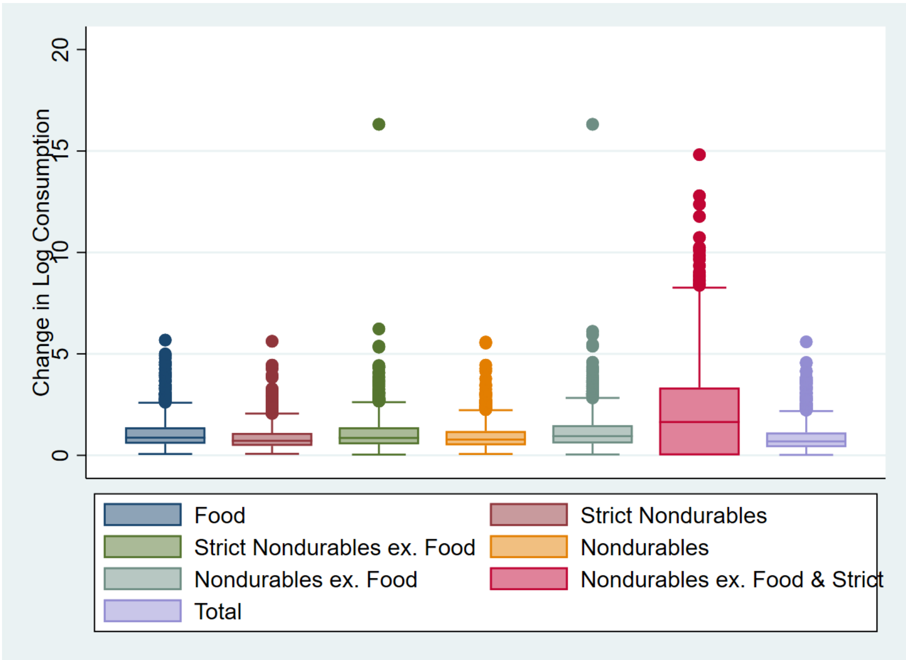


Figure A4: Box Plots - Log Consumption Change Distribution with more Categories
Note: Only Exposed Sample