Food Insufficiency During COVID-19: How Unemployment Insurance Mitigates the Effect of Job Loss

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Abstract

We examine the determinants of the food insufficiency rise during the COVID-19 crisis in 2020. We then examine the degree to which unemployment insurance benefits mitigate the effect of job loss on food insufficiency and quantify the impact of a $600 reduction in maximum weekly unemployment insurance benefits. We utilize novel Household Pulse Survey data to estimate linear fixed-effect regressions and difference-in-differences models. We find that households reporting pandemic-induced job loss have a 4.3 percentage point increase in food insufficiency risk relative to those working, and the gap grows to 6.1 percentage points in the absence of unemployment insurance. While food insufficiency rates decrease by 2 percentage points for unemployment insurance beneficiaries under the $600 weekly expansion, this effect disappears after the expiration of the additional benefits.

[1] Special thanks to Dr. Klara Peter for her guidance and assistance in conducting this research.

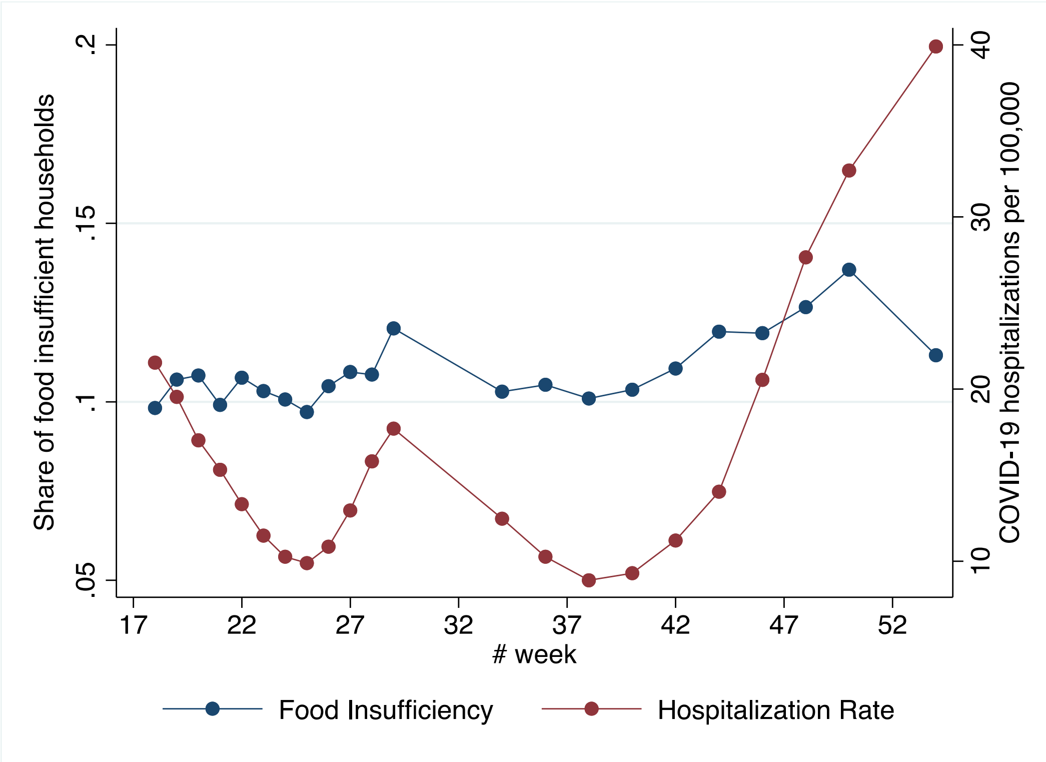
1. Introduction

While the COVID-19 pandemic has been declared a public health crisis for its direct impact on health, it also plays an insidious role in harming wellbeing by causing food insufficiency rates to skyrocket. Compared with a 36% increase after the Great Recession, food insufficiency tripled from 2019 rates during the onset of the COVID-19 pandemic (Ziliak 2020). Figure 1 illustrates the trends in food insufficiency alongside the COVID-19 hospitalization rate. While the rate of food insufficiency before the pandemic hovered around 3.5%, we observe rates between 9.7% and 13.7% during our observation period in 2020. Food insufficiency has been associated with poor nutrition (Rose 1999; Rose and Oliveira 1997; Bhattacharya et al. 2003) and harms physical and mental health (Siefert et al. 2001). Despite $2.1 trillion in government COVID-19 supplemental funding to ease the shock of the pandemic, millions of US families do not have enough food to eat.[2]

The COVID-19 pandemic also caused a sharp rise in unemployment to a maximum of 14.8% in April 2020 (Bureau of Labor Statistics 2021). Job loss incurs a higher risk of food insufficiency, which is partially offset by government policy support (see Section 2 for further discussion). Employment rates also fell more for workers in low-wage jobs (Chetty et al. 2020), who may have fewer liquid assets that protect against food insufficiency (Gundersen and Gruber 2001; Ribar and Hamrick 2003; Leete and Bania 2010). We investigate the impacts of the COVID-19 pandemic, focusing on the mechanism of job loss. In this paper, we apply an economic lens to quantify food insufficiency in the context of the COVID-19 pandemic—who is most at risk, and how effective is unemployment insurance at protecting those who it was designed to insure?

[2] Spending as of February 28, 2021. Source: https://www.usaspending.gov/disaster/covid-19

Figure 1: Food Insufficiency and COVID-19 Hospitalizations per 100,000



Notes: Shares are weighted using survey sample weights. Week 54 indicates the second week of 2021. The second round of stimulus checks was distributed to households between weeks 52 and 54.

The United States government responded to the COVID-19 crisis by supplementing the unemployment insurance (UI) program substantially, among other social safety net expansions (see Bitler et al. 2020 for a review). The Federal Pandemic Unemployment Compensation (FPUC) program extended the program duration by 13 weeks for recipients and expanded eligibility to include self-employed, gig workers, and those not earning enough to qualify for regular UI. The FPUC supplemented the maximum UI benefit amount by $600 per week until the Lost Wages Assistance (LWA) program supplanted it in August 2020. Although the LWA provides extended UI benefits retroactively for the six weeks beginning August 1, the payments were distributed at different times in each state (see Appendix Table A3). LWA claimants received no additional UI benefit until the first check arrived, typically as a lump sum of the previous weeks' backlogged payments.

Despite the US government's efforts to provide support during the crisis, we still observe exceedingly high food insufficiency levels in the population. Bitler et al. (2020) justify the disconnect between assistance and unmet need, finding that delays in program implementation and coverage gaps contribute to the inadequacy of policy response in alleviating food insufficiency.

This paper's first contribution provides an estimation of food insufficiency as a function of the COVID-19 crisis, emphasizing the role of pandemic-induced employment loss. We employ linear regression with state and week fixed effects for all estimations, favoring the linear model over the probit and logit models due to its ease of interpretation and freedom from the bias observed in the nonlinear models with fixed effects (Greene 2004). We primarily use household-level data from the United States Census Bureau's Household Pulse Survey for the analysis. We measure food insufficiency as a binary variable equal to 1 if the household sometimes or often did not have enough to eat, based on the USDA food sufficiency question. We also examine the determinants of food insufficiency defined broadly to include households that have enough but not the kinds of food wanted. Additionally, we consider the determinants of household spending on groceries and prepared meals.

We document the direct effect of the pandemic on food insufficiency: each 1% increase in the state COVID-19 hospitalization rate is associated with .3 percentage point increase in food insufficiency. We find that households headed by someone not working due to pandemic reasons have a 4.5 percentage point higher food insufficiency risk than those with employed household heads, and 3 percentage points higher food insufficiency rate than those not working due to other reasons. We also find evidence of grocery hoarding behavior in response to higher COVID-19 hospitalization rates and anticipated income loss.

For this paper's second contribution, we evaluate the role of UI in mitigating the effect of pandemic-induced job loss on food insufficiency. We begin by interacting job status with four measures of UI generosity. Then, we specify a difference-in-differences model that quantifies the effect of a $600 weekly drop in maximum UI benefits on food insufficiency. We employ a difference-in-differences framework that compares the rise in food insufficiency for UI beneficiary households after the FPUC expiration on July 31, 2020 to households that do not receive UI. While similar to the framework of Berkowitz and Basu (2021), we differentiate our work by applying inverse-propensity weights to balance covariates between UI recipient and non-recipient households. We do so to address endogeneity concerns arising from potential unobservable differences between the groups. Additionally, we consider the non-smooth introduction of the LWA program by providing short-run estimates of the program expiration effect. For these estimations, we exclude observations made after the first LWA to ensure a uniform drop in maximum benefits across all states.

We find that households with pandemic-induced job loss that don't receive UI face a 6.1 percentage point increase in food insufficiency risk. All of our UI measures were significant in reducing food insufficiency for households with pandemic-induced job loss, and the measures became significant in reducing food insufficiency for other non-working households when we broadened the food insufficiency definition. Our difference-in-differences model estimates the protection afforded by UI with the additional $600 weekly supplement to be 2 percentage points for all households and 3 percentage points for non-working households. However, this effect is offset by the expiration of the supplement. Indeed, we find that the effect of receiving UI after the supplement expires is positive and larger in magnitude than the negative effect of receiving expanded UI. We observe the rate of food insufficiency for recipient households grows as time goes on, indicating that the base UI is not enough to combat the increasing pressure of the pandemic on food insufficiency.

Our paper contributes to the nascent literature surrounding the COVID-19 crisis by applying inverse-propensity weights to make the UI recipient and nonrecipient groups similar in observed characteristics. We expect the balancing to also make the groups similar in unobservable characteristics, which may be correlated with error in our model, to address endogeneity concerns. Additionally, we know of no other papers that address the confounding effects of the LWA program in estimating the effect of UI on food insufficiency.

The rest of the paper adheres to the following structure: Section 2 provides an overview of the relevant literature and the hypotheses it has informed. Section 3 describes the data sources and presents analyses of spending inequality. Section 4 offers the empirical models, while Section 5 follows, containing the results. Section 6 concludes.

2. Literature Review and Hypotheses

In this section, we first review studies that establish the relationship between employment shocks and food insufficiency and spending, then we turn to the research that concerns policy impacts. Next, we survey literature to compare spending during the COVID-19 pandemic to past crises. Finally, we review other papers that use similar difference-in-differences estimations and/or the HPS dataset.

Unemployment and Income Shocks

Rose (1999) establishes that job loss is associated with increased risk of food insufficiency. Studies find significant associations between job loss and food insufficiency in Australia (Temple 2018), Toronto (Loopstra 2013), and Europe during the 2008 recession (Loopstra 2016). Analysis of the Survey of Income and Program Participation (SIPP) estimated the odds of food insecurity to be 78% higher for households experiencing job loss during the Great Recession (Birkenmaier 2015). In comparison, a survey of Vermont households during the pandemic found that recent job loss led to 206% higher odds of food insecurity (Niles 2020). Leete and Bania (2010) also analyze SIPP data and offer further support that negative income shocks are associated with food insufficiency. Although Heflin et al. (2007) did not find recent job loss significant in predicting food insufficiency in the Michigan Women's Employment Study, they find that hours worked are negatively associated with food insufficiency.

Food insecurity rates decline with income, yet more factors play into the experience of hardship than income alone, as many food insecure households are not poor (Gundersen et al. 2011). Analyses of the SIPP firmly establish that income volatility is associated with increased food insufficiency risk, while liquid assets are protective (Gundersen and Gruber 2001; Ribar and Hamrick 2003; Leete and Bania 2010).

Income shocks also play a role in food spending. Baker and Yannelis (2017) find a temporary shift of compensation due to the 2013 government shutdown caused households to reallocate spending across time and types of consumption. Households with liquidity and credit restraints were more sensitive to the shock, and consumption recovered quickly at the end of the shutdown. Although some households experienced similar temporary employment loss during the pandemic due to furlough, the current crisis brings much more uncertainty of long-run job security. This crisis also differs from past employment shocks in that certain industries have been hit harder than others, particularly those that require physical contact, such as restaurant service or salons, and employment in low-wage jobs has declined the most (Chetty et al. 2020). We expect food insufficiency to rise more during the pandemic than previous economic downturns since low-wage workers who are less likely to have protective assets were hit the hardest.

Recent studies of pandemic spending find that people experiencing pandemic-related unemployment have a significantly larger spending drops than those that didn't lose jobs (Andersen 2020) and that households expecting unemployment loss are less responsive to the CARES stimulus payment (Baker et al. 2020b). These observations support the consumption-smoothing hypothesis. We expect to find similar evidence of consumption-smoothing behavior in response to experienced or expected job loss. We hypothesize that households experiencing a loss of income will have higher rates of food insufficiency over time, as assets and savings deplete.

Policy

We consider previous studies that examine the impact of social protection policies on food insufficiency. In Europe during the 2008 recession, Loopstra et al. (2016) found that $1,000 increases of per-capita government spending on social protection mitigated the association between job loss and food insufficiency by .05 percentage points, and job loss became insignificant after $10,000 of per-capita spending. Ionescu-Ittu et al. (2014) found that a $100 monthly benefit for Canadian households with young children reduced food insufficiency rates by 2.4 percentage points. Borjas (2004) found that a 10 percentage point drop in the fraction of the population receiving any kind of public assistance increased food insecurity by 5 percentage points. Bitler and Hoynes (2015) find that UI offers more (or no less) protection from poverty during the Great Recession. Berkowitz and Basu (2020) and Raifman (2020) also examined the impact of UI on food insufficiency, and we discuss their work in the methods subsection.

In response to the pandemic, the US federal government allowed states to expand eligibility for Supplemental Nutrition Assistance Program (SNAP, formerly Food Stamp Program) benefits, which may be applied to food purchases at approved retail food outlets. A study examining the introduction of the Food Stamp Program found that food stamps reduce out-of-pocket food spending and increase overall food expenditures (Hoynes and Schanzenbach 2009). However, the direct effect of SNAP on food insufficiency appears to be insignificant (Gundersen and Oliviera 2001; Hashad 2020).

Spending Response to Crisis

We now turn to review the literature concerning consumer spending responses to COVID-19 and past crises. First, we consider pressures hypothesized to decrease spending.

In the wake of economic shocks, households adjust shopping behavior by switching to lower-cost retailers (Coibion 2015) or generic products, buying larger sizes or sale items, or using coupons (Nevo and Wong 2019). Griffith et al. (2015) find that UK households during the Great Recession raised shopping effort and adjusted shopping basket composition to lower real food expenditures without sacrificing the number of calories purchased. Spending more time to search for lower prices is more common among those unemployed (Kaplan and Menzio 2016) and poorer households (Arslan et al. 2020).

By April 7, 2020, all but five US states had issued stay-at-home orders. Mobility restrictions caused individuals to spend more time home and less time in restaurants and grocery stores. In response to government lockdown orders and mobility restrictions, consumers reduce restaurant spending (Andersen et al. 2020) and overall food spending (Coibion et al. 2020).

While job loss, adjusted spending behavior and lockdown orders constitute downward consumption pressures, we also consider multiple positive pressures on food spending. The US government responded to the crisis by providing large lump sum payments to taxpayers. Three studies apply OLS and 2SLS regression to the Consumer Expenditure Surveys to investigate the impacts of previous positive lump sum income shocks on household spending. In response to tax refunds, Souleles (1999) finds that spending on food increases significantly for households with income in the bottom 25% or 15% of the sample. The same regression analyses on all incomes yield positive yet insignificant increases in food spending in response to the 2001 tax rebate (Johnson, Parker and Souleles 2006) and the economic stimulus payments distributed in 2008 (Parker et al. 2013). Compared with the significantly smaller lump sum payments made in 2001 and 2008, Baker et al. (2020b) find the consumption increases in food spending in response to the April 2020 stimulus payments to be larger, more immediate, and more significant. They find the response to be stronger for households with lower incomes and lower levels of liquidity, as well as for households with larger income drops.

Disaster-induced spending on survival necessities, including nonperishable foods, increased after the 2003 SARS outbreak in China (Qiu et al. 2018), 2011 Christchurch earthquake (Forbes 2017), and after the 2011 Tohoku earthquake in Japan (Hori and Iwamoto 2014). A study of Chinese citizens living in cities affected by the COVID-19 outbreak finds that those with less food at home, poor psychological status, and women report a greater need to hoard food during the beginning of the pandemic (Wang and Hao 2020). Studies of transaction data from the United States and Denmark during the COVID-19 pandemic observe grocery spending increases consistent with stockpiling behavior (Baker et al. 2020a; Andersen et al. 2020). Loxton et al. (2020) suggest that panic buying, herd mentality, and a shift of prioritization to buy goods that satisfy physiological needs explain the spike in consumer spending on groceries.

The panic buying and food hoarding behavior exhibited by consumers increased demand at grocery retailers, which, coupled with supply chain disruptions (McKinsey 2020), contributed to the food price increases illustrated in Figure A3. Price increases necessitate higher spending to afford the same amount of food, which has adverse effects on welfare, especially among the lowest income households (Vu and Glewwe 2011; Ivanic and Martin 2008).

We hypothesize that the pressures of the COVID-19 pandemic will cause consumers to shift spending away from prepared meals, which may be considered a luxury good, towards groceries, which are essential. We expect to see evidence of food hoarding that will be strongest at the beginning of the crisis. Food price increases also put upward pressure on spending, although the greatest price increases documented in Figure A3 occurred before our sample window.

Consumers have responded to the health aspect of this crisis by adopting online grocery shopping behaviors to avoid viral exposure occurring with in-person shopping. As observed during the 2003 SARS outbreak (Kee and Wan 2004), there has been a rise in online grocery shopping in 2020, particularly among younger and higher-income households (RAND 2020; McKinsey 2020). The hesitancy of older individuals to embrace online grocery shopping, coupled with the increased risk of severe symptoms of the virus, lead us to hypothesize that older households will experience a higher risk of food insufficiency.

Methods and Data

Other studies have employed similar methods to study food spending and insufficiency. Restrepo et al. (2021) also estimate the impact of pandemic-induced job loss on food insufficiency and expenditures, as well as free food receipt and food sufficiency confidence. They find these households spend 15% less on food and were 10% less likely to report food sufficiency. We differentiate our first contribution from Restrepo et al. by accounting for the roles of household job loss expectations and pre-pandemic household food insufficiency and including households that were not working for other reasons.

Our difference-in-differences framework follows that of Berkowitz and Basu (2021), who estimate a 3.88 percentage point difference in food insufficiency risk following the expiration of the FPUC for UI beneficiary households using HPS data. However, we critique that their sample restrictions limit the interpretability of their results. While Berkowitz and Basu (2021) limit their sample to households that report a loss of household employment income since March 13, 2020, and did not use regular income sources to meet spending needs in the past week, we argue that these restrictions are opaque. It is not clear whether households reporting employment income loss have recovered earnings, changed income sources, or still have a reduced income. Additionally, the term "regular income sources" is not well defined. While Berkowitz and Basu (2021) interpret the questionnaire's phrasing of "regular income sources like those received before the pandemic" to mean earned income, households may also interpret government benefits as regular income. We attempt to address these limitations by estimating our model on the entire working-age sample, a subsample of households whose heads did not work in the past week, and households that did not use regular income for spending needs. We further differentiate our approach by balancing the two groups using inverse-propensity score weighting as done in Bitler et al. (2006). We also account for the LWA program by comparing our estimates made with the entire post-FPUC sample to those of a subsample excluding observations made after the first LWA checks were received.

Raifman et al. (2020) also estimate a difference-in-differences specification to estimate the effect of UI in mitigating the rise in food insecurity[3] among low- and middle-income households experiencing loss of employment during the pandemic. These authors used longitudinal data from the Understanding Coronavirus in America study to compare UI recipients before and after receiving benefits to those that did not receive UI, finding that UI without the $600 weekly supplement reduced food insufficiency by 3.05 percentage points (1.89 weighted) or 5.13 percentage points (5.63 weighted) with the supplement. Ionescu-Ittu et al. (2014) used a

[3] Food insecurity was defined using the food insecurity experience scale by the Food and Agriculture Organization of the United Nations

difference-in-differences framework to estimate the effect of a $100 monthly benefit increase on food insufficiency.

Difference-in-differences approaches have been used to estimate changes in food spending following the introduction of SNAP (Hoynes and Schanzenbach 2009) and the marginal propensity to consume food out of SNAP benefits (Hastings and Shapiro 2018). Baker and Yannelis (2017) use a difference-in-differences approach to compare spending of government workers affected by the 2013 shutdown to those who were not.

Luong (2020) makes use of the same HPS dataset to evaluate the effects of containment policy on food insufficiency. Luong uses an ordered probit regression to estimate the probability of food insufficiency, along with OLS estimations of expenses on food to estimate the impact of relaxing lockdown policy on food insufficiency. Luong finds that loosening lockdown restrictions is associated with increased availability of food in stores and the number of people receiving free food, but also increased prices that made food unaffordable for some families. Morales et al. (2020) use the HPS as well, applying generalized estimating equation models to conclude that non-White households were not more food insecure than White households, but larger households and those with older household heads were at higher risk. Ziliak (2020) compares the rise in food insufficiency observed in the HPS to prior trends in the Current Population Survey trends and compares the rates for adults and the elderly.

Our paper will contribute to the research concerning the role of UI in mitigating the food insufficiency risk resulting from employment loss, and we differentiate our study from others that estimate UI effects during the pandemic by accounting for the heterogeneous introduction of the LWA. We also consider that truncating the post-expiration observations will provide us with the short-term effect of the FPUC expiration, while the long-term effect of the expiration may be more severe. This is the first application of balancing weights to the HPS that we know of.

3. Data

For this study, we combined household survey data from the United States Census Bureau's novel Household Pulse Survey (HPS) with state-level economic conditions from the Opportunity Insights Economic Tracker (OIET), pandemic data from the Covid Tracking Project, and UI benefit information gleaned from state government websites. We describe the data sources and manipulations in this section. For a comparison of the HPS and the Current Population Survey, see Ziliak (2020). A complete list of variables and descriptions is outlined in Appendix Table A1, with accompanying summary statistics in Appendix Table A2.

Household Pulse Survey

The HPS dataset covers weeks ranging from April 23, 2020, to January 18, 2021, and includes 1,932,450 observations spanning the 50 states and Washington DC. The HPS is a novel three-phase survey of US households containing information concerning the social and economic impact of the COVID-19 crisis. The data were collected via online Qualtrics survey. Phase one of the survey consists of weekly data surveyed from April 23 until July 21, 2020. Household heads answered questions about their current employment and demographics, household characteristics including food sufficiency status for both the past week and before the pandemic, and spending on food, among other questions capturing pandemic-related disruption. Phase two, conducted from August 19 through October 26, 2020, consists of biweekly data with the same questions as well as additional questions about the application and receipt of government benefits and the sources of income used to meet household spending needs in the past week. Phase three currently includes eight surveys administered between October 28, 2020, and March 1, 2021. On average, the three phases yield two survey rounds per month. Although most of the questions are identical between phases two and three, 2021 observations do not include prior household food sufficiency and are therefore omitted from the estimation sample. The HPS dataset contains a panel subset of households surveyed two or three times between April 23, 2020, and July 24, 2020, containing 413,718 observations. For the purposes of this paper, we include all observations.

The HPS provides fairly detailed technical documentation related to the sample design, accuracy of estimates, and implemented procedures dealing with traditionally high non-response rates in online surveys.[4] The HPS sampling weights are designed to adjust weekly estimates for the household non-response, number of adults per household, and demographic coverage. All of our estimates use HPS sampling weights. We also note that many variables in the dataset have some missing observations; most of the variables with missing data were missing less than 3% of responses. The Census Bureau used hot deck imputations to fill in missing demographic data with responses from similar participants. Imputations were made for no more than 3% of the responses for the gender, Hispanic origin, race, educational attainment, household size, and birth year (subtracted from 2020 to generate age variable).

Measures of Food Insufficiency

The HPS includes the standard four-category measure for food insufficiency introduced by the United States Department of Agriculture (USDA) in 1977. Survey participants are asked which of the following four categories describes the food eaten in their household over the past seven days:

[4] See https://www.census.gov/programs-surveys/household-pulse-survey/technical-documentation.html

1 Enough of the kinds of food (I/we) want to eat.

2 Enough but not always the kinds of food (I/we) want to eat.

3 Sometimes not enough to eat.

4 Often not enough to eat.

The USDA defines food insufficiency as when households sometimes or often do not have enough to eat and notes that it is comparable in severity to the designation of very low food insecurity.[5] For our main dependent variable, we collapse this measure into a binary indicator equal to one if the household head selects category 3 or 4. This categorization is consistent with convention and existing literature (Gundersen et al. 2017; Berkowitz and Basu 2021; Gundersen and Oliveira 2001; Gundersen and Gruber 2001; Ribar and Hamrick 2003; Leete and Bania 2010; Rose et al. 1998; Rose and Oliveira 1997). For robustness, we also construct a broad food insufficiency definition that includes households which selected category 2 (as done by Gundersen and Ribar in 2011, Ziliak in 2020, and Luong in 2020) and estimate an ordered probit model using all four categories. For the remainder of this paper, the term *food insufficiency* refers to categories 3 and 4, while *food insufficiency (broad)* also includes category 2.

Figure 2 illustrates the trends in food insufficiency by household head employment status. Individuals who are not working are separated into two groups: not working due to pandemic reasons and not working due to other reasons.[6] We observe that food insufficiency is much more prevalent among households that report not working due to pandemic reasons (19 to 32 percent) than those employed or not working for other reasons (7 to 12 percent). Furthermore, the

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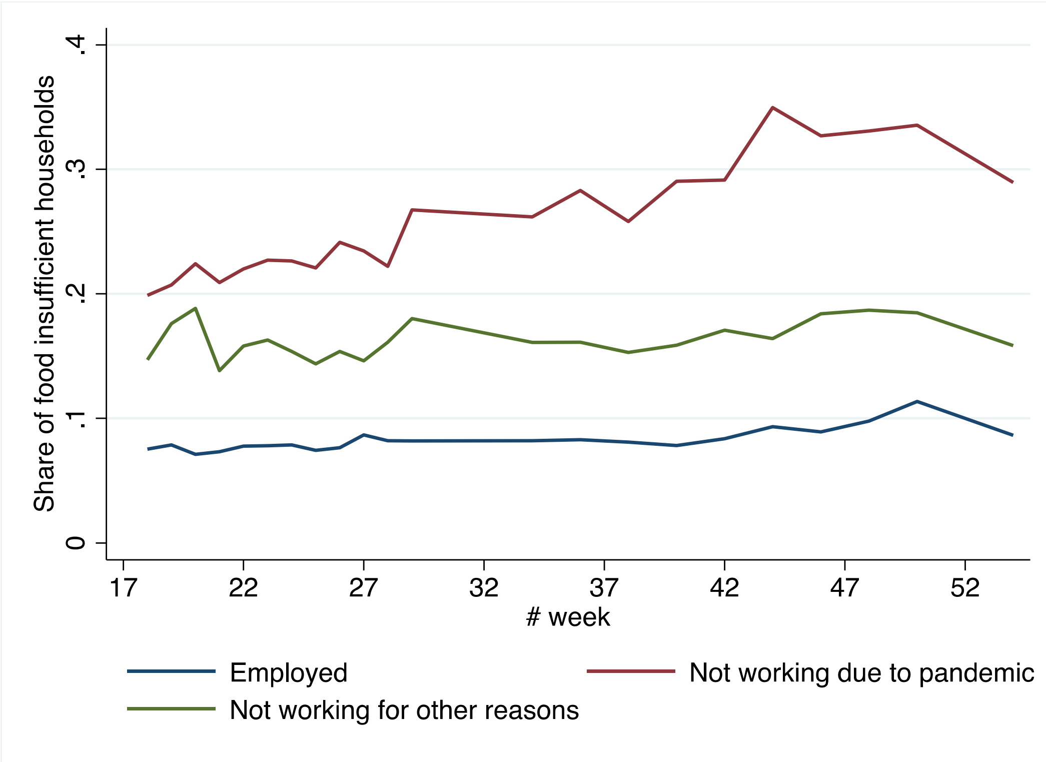
[5] For USDA classification, see https://www.ers.usda.gov/topics/food-nutrition-assistance/food-security-in-the-us/measurement

[6] Pandemic reasons include being sick with or caring for someone with coronavirus symptoms, furloughed due to pandemic-related reduction in business, and laid off due to temporary or permanent closure of business due to pandemic. Other reasons include retirement, caring for children or elderly, sick or disabled unrelated to the coronavirus, not wanting to be employed, and other reasons.

Figure 2: Food Insufficiency by Employment Status



Notes: Shares are weighted using survey sample weights. Week 54 indicates the second week of 2021. The second round of stimulus checks were distributed to households between weeks 50 and 54.

prevalence of food insufficiency significantly increased in 2020 for those not working due to the pandemic yet remained relatively steady for other households. It is noteworthy that all employment categories experienced a drop in food insufficiency rates after the second stimulus checks were distributed in 2021, with the largest drop observed for households not working due to the pandemic.

In Appendix Figure A1, we also observe a substantially higher prevalence of food insufficiency for Black and Hispanic-headed households than for White or Asian-headed households (21-24 vs. 7-10 percent in December 2020). The gender gap is not as striking as the racial gap, with a slightly higher reported food insufficiency among female-headed households.

Figure 3 visualizes the trends in the specified reasons for food insufficiency, defined broadly. Although the initial most-cited reason for food insufficiency was that stores did not have the kind of food wanted, the predominant reason quickly shifted to lack of means to afford food. This is consistent with reports of supply chain disruptions that reduced food availability. (McKinsey 2020).

In addition to food insufficiency, we also examine household food spending during the COVID-19 crisis. The HPS separately asks respondents for the amount spent in the past seven days on groceries and prepared meals for the household. We divided these disaggregated expenditure measures by an adult equivalence scale and adjusted for food price inflation.[7] Appendix Figure A3 presents the food price fluctuations for US divisions over the period of study. Appendix Figure A2 shows the natural log of the adjusted spending measure for each employment category. It is interesting that employed households and those not working due to the pandemic experience a sharp jump in spending on groceries between weeks 23 and 24, while there is a more gradual increase observed for those not working for other reasons. Weeks 23 and 24 correspond to the first trough of hospitalizations following the initial wave of the virus. Households may have deemed these weeks the safest to return to grocery stores and refill the pantry. Spending on prepared meals rises gradually for all groups until becoming steadier around week 30. Employed households spend more on prepared meals than those without employment, and those not working due to the pandemic tend to spend more than those not working for other reasons. This may be because households experiencing a recent income loss have not yet adjusted consumption patterns, or because these households have higher unobserved asset levels.

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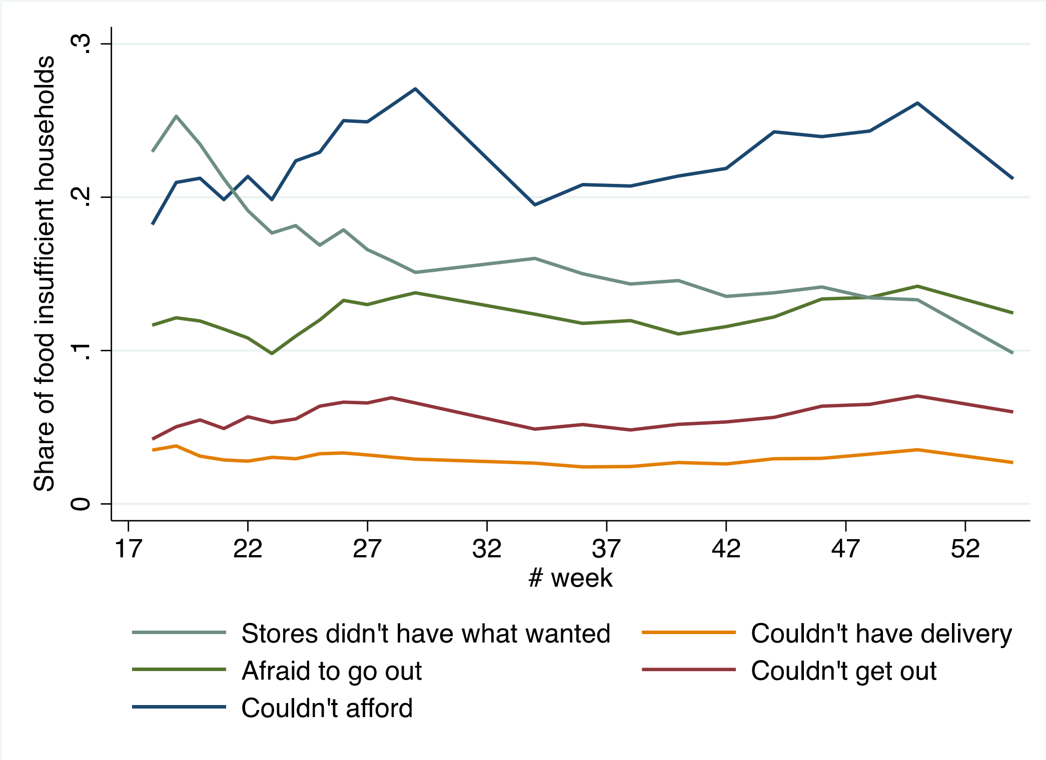
[7] The adult equivalence scale was computed to be 1 for the first adult, plus .75 for each additional adult and plus .5 for each child under 18 in the household. This accounts for the presence of economies of scale, wherein meals can be prepared more cheaply per person when more people share a meal.

Figure 2: Reasons for Food Insufficiency (Broad)



Notes: Shares are weighted using survey sample weights. Week 54 indicates the second week of 2021. In this figure, food insufficient households are broadly defined to include households that have enough to eat, but not always the kinds of food they wanted to eat.

Unemployment Insurance

We consider that UI may mitigate the impact of job loss on food insufficiency. We obtain two measures of the emerging unemployment rate—initial regular UI claim rate and the initial UI claim rate including the FPUC eligibility expansions—from the Opportunity Insights Economic Tracker (OIET). A detailed overview of the dataset can be found at Chetty et al. (2020). We further analyze the mitigating effect of UI on food insufficiency by examining the time-varying statutory maximum benefit amount. Before the expiration of the FPUC, that is, until July 31, we add $600 to the measure of pre-pandemic maximum UI benefits from the COVID-19 US State Policy Database maintained by the Boston University School of Public Health. These values were taken directly from the government websites of each state and are listed for each state in Appendix Figure

A3. Following the expiration of the FPUC, we added $300 to the pre-pandemic maximum UI benefits to capture the Lost Wages Assistance program benefits.[8]

One reservation about our calculation of maximum UI benefits arises from the observation that states did not begin to pay LWA benefits after the week of August 1. Instead, a household claiming UI for the LWA period received no extra benefits until its state began to pay out the claims, and then benefits were received as a lump sum. Additionally, states only paid up to six weeks of LWA benefits, and many states ran out of money before paying all six weeks. We cannot observe whether households qualify for back pay during the weeks prior to the initial LWA checks nor by how much the benefit amount received may differ from the statutory maximum amount. For our difference-in-differences specification that excludes observations made after the first LWA-boosted benefit checks, we used the estimated first payment date from the LWA Tracker detailed in Appendix Table A3.

Alternatively, we measure UI at the individual level. We employ a binary variable in the HPS that takes the value of 1 if the household used UI benefits to meet spending needs in the past week and 0 otherwise. We use the UI spending measure as a proxy for UI receipt to determine treatment status in our difference-in-differences model. We also use a sister variable that indicates whether or not the household used regular, pre-pandemic income sources in constructing our third difference-in-differences subsample.

Extent of the COVID-19 Crisis

We include variables to capture the extent of the COVID-19 crisis in each state for each week. We obtain a measure for new COVID-19 hospitalizations per 100,000 from The COVID

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[8] We instead added $400 of benefits for Montana, Kentucky, and West Virginia, which opted to increase the LWA benefits. We did not add any extra benefits for South Dakota, which did not apply to LWA.

Tracking Project. Although COVID-19 positive confirmed cases are often used to measure the extent of the crisis, we argue that case measurements are not as comparable among states due to differences in COVID testing rates. In contrast, hospitalizations better capture the severity of the crisis.

We also use a measure of combined credit and debit card spending from the OIET to capture the general economic conditions in each state for each week as a covariate. We hypothesize that higher consumer spending is an indicator of a stronger economic recovery and will predict lower food insufficiency and higher food spending. Although this measure excludes cash payments, we believe this bias is less pronounced during the pandemic due to a consumer shift away from cash.[9]

Summary Statistics

Appendix Table A2 provides the weighted means and standard errors of each variable by food sufficiency. Household heads that report more severe food insufficiency prior to the pandemic are more likely to report food insufficiency in the past week. We also document higher food insufficiency rates among households whose heads were not working in the past week. We can see that food insufficient households tend to be larger, report a 2019 income less than $50,000, and have heads that are female, Black, Hispanic or mixed race, unmarried, and hold less than a bachelor's degree. Households in states with higher COVID-19 hospitalization rates and initial UI claim rates are also more likely to be food insufficient. The average maximum state UI benefits are lower for food insufficient households.

[9] See https://www2.deloitte.com/ch/en/pages/consumer-industrial-products/articles/cash-is-no-longer-king-in-times-of-covid19.html

4. The Empirical Model

4.1 Baseline Model

We begin by estimating the food insufficiency equation as follows:

$$Y_{ist} = \alpha + \gamma J_{ist} + \delta Y_{i0} + \beta X_{ist} + \theta_t + \mu_s + \epsilon_{ist}. \qquad (1)$$

The subscript i indicates observations made for the individual household head and corresponding household, t indicates the week of the observation, and s indicates the state of household residence. In the base model, *Y* is a binary variable equal to 1 if the household reports sometimes or often not having enough food in week *t*. We later expand the model to explore alternative measures of *Y:* broadly defined food insufficiency and weekly household spending on both groceries and prepared meals. We also estimate equation (1) as an ordered probit model with four categories for food insufficiency.

In our specification, *J* is a categorical variable capturing the household head's job status in three categories: employed in the past week, not working due to pandemic reasons, and not working due to other reasons. Y_{i0} captures the pre-pandemic initial condition of binary household food sufficiency status prior to March 13, 2020. Since the food insufficiency question is subjectively evaluated by each household head, including this measure in our specification helps account for reporting bias. The X_{ist} vector captures three control vectors (X_{i0}, X_{it}, X_{st}). The first component captures time-constant household characteristics, including the respondent's gender, ethnicity, educational attainment, age, and household 2019 pre-tax income. The second component captures household characteristics which may vary over time, including the expectation of future income loss, marital status and household size. The last component accounts for time-varying state-wide indicators for the log of the average COVID-19 hospitalization rate, emerging unemployment rate, and combined credit and debit card spending. The term θ_t captures week fixed and μ_s captures state fixed effects. Finally, we have an unobserved time-varying determinant of food insufficiency, ϵ_{ist}, which we assume to be normally distributed and uncorrelated with the other variables in the model.

We include state and time fixed effects to counter the threat of omitted variable bias to our model. States differ in unobserved characteristics that may affect food insufficiency, such as social safety net generosity before policy extensions and employment concentration across different industries. Our state fixed effects account for these time-invariant effects, and our week fixed effects account for factors that affect all states over time.

4.2 Unemployment Insurance Policy

Next, we consider that unemployment insurance may mitigate the effect of losing employment due to the pandemic on food insufficiency. To test this hypothesis, we estimate the following expansion of equation (1):

$$Y_{ist} = \alpha + \gamma_1 J_{ist} + \gamma_2 UI_{ist} + \gamma_3 (J_{ist} UI_{ist}) + \delta Y_{i0} + \beta X_{ist} + \theta_t + \mu_s + \epsilon_{ist}. \quad (2)$$

We let *UI* be a vector that captures the state-weekly new regular and expanded eligibility UI claim rates, the log of statutory maximum UI, and an indicator for whether the household used UI benefits to meet spending needs in the past week. We critique the statutory maximum UI, which assume the LWA benefits following the expiration of the FPUC until the end of 2020 for all states. In reality, states did not begin to pay out LWA benefits until late August, and households received lump sum back pay from missing weeks until the beginning of November. See Table A4 in the Appendix for the estimated date of the LWA payment in each state. We address our critiques in the following subsection.

We refine the model by presenting a more advanced difference-in-differences framework that allows us to estimate the causal impact of the FPUC expiration and quantify how that effect changes over time.

4.3 Difference-in-Differences Framework

We expand on the difference-in-differences framework employed by Berkowitz and Basu (2021) to estimate the causal effect of a $600 reduction in maximum UI benefits on food insufficiency for UI beneficiary households. We compare this effect across three different samples: the entire sample for which we observe UI spending, a subsample including only households whose heads did not work in the past week, and a subsample including only households that did not use regular, pre-pandemic income to meet spending needs in the past week. We estimate

$$Y_{ist} = \alpha + \lambda Treated_{ist} + \gamma(Treated_{ist} Post_t) + \varphi J + \delta Y_{i0} + \beta X_{ist} + \theta_t + \mu_s + \epsilon_{ist}. \qquad (3)$$

The *Treated* variable is specified to be the UI spending variable that takes the value of 1 if household *i* used UI benefits to meet spending needs in the week prior to *t* and 0 otherwise. *Post* takes the value of 0 if *t* falls during the period of extended UI benefits and 1 after the extension expires. We account for the inconsistent transition from the FPUC to the LWA by restricting the estimation sample to observations that occurred before the LWA payments began. In doing so, we isolate a larger difference in the pre- and post-expiration periods and maintain a consistent benefit gap across states. We present the results for estimating equation (3) for each of the three groups identified above twice—once with the entire post-expiration period and once with the truncated post-expiration period.

We hypothesize that the effect of the program expiration will increase with time, and the difference-in-differences framework allows us to investigate the time-varying treatment effect. We estimate the margins of weekly effects derived from the following tweaked equation:

$$Y_{ist} = \alpha + \lambda\, Treated_{ist} + \Sigma_{j=1}^{T} \gamma_{\mathrm{j}}(Treated_{ist} * \ \mathrm{II}\{\mathrm{j} = \mathrm{t}\}) + \varphi \mathrm{J} \ + \delta Y_{i0} + \beta X_{ist} + \theta_t + \mu_s + \epsilon_{ist}. \qquad (4)$$

The term $II\{\, j = t\,\}$ takes the value of 1 if the observation occurred in week t and 0 otherwise. It differs from equation (3) in that each week j has an associated treatment effect γ_{j} rather than one treatment effect γ for all post-expiration weeks.

We consider that our model may be biased due concerns that households may select into treatment. Food insufficient households are probably more likely to seek income sources to alleviate the negative effects of lack of food. This may cause food insufficient households to seek out and apply for UI benefits, introducing reverse causality into the model. Additionally, we are concerned that households may switch between treatment groups. That is, households may exist in our untreated group that previously received UI, which may cause us to underestimate the treatment effect. Although we cannot directly address these potential sources of endogeneity, we attempt to address concerns about the correlation between treatment and the error term through use of inverse-propensity weights, which we discuss in the following subsection.

We note that FPUC-bolstered UI exceeds the weekly earnings of 76% of UI-eligible workers, and thus may disincentivize employment for these workers (Ganong et al. 2020). If this is true, we should be concerned about selection bias affecting our estimations for the sample of non-working households. We assess the validity of the concern by measuring the correlation between UI generosity and employment. We estimate two probit models of the likelihood of not working in the past week and of household employment income loss since March 13, 2020 as a function of the statutory maximum UI benefits. In Appendix Table A4, we present a *negative* correlation between not working and UI generosity and no significant association between income loss and UI generosity. Other studies have found higher UI under the FPUC had no significant impact on employment using an event-study design with HPS data (Dube 2020) and event-study and regression designs with employment data from a private firm (Finamor and Scott 2020). Therefore, we allay concerns of selection bias in response to UI generosity in our sample.

Our third sample includes only households that do not report meeting spending needs with regular, pre-pandemic income sources. This measure is not clearly defined; we consider that some households may consider UI or other government benefits "regular income." How do these households meet their spending needs, if not through regular income or UI? We examine their spending resources and find that 35% use money from savings or selling assets, 30% use credit cards or loans, 20% used money borrowed from friends or family, and 19% used money from stimulus payments. SNAP payments were used to meet needs for about 9% of these households and deferred or forgiven payments were used by about 6%. However, we consider these income-constrained households a useful comparison.

Inverse-Propensity Weights

We consider that our model may be biased if the treated households differ significantly from our untreated households in ways we cannot observe, causing a correlation between treatment status and the error term ϵ. We address this concern by applying inverse-propensity weights to make our groups more similar in terms of observable characteristics, in hopes that the groups become more similar in unobserved characteristics at the same time. We assume that treatment is independent of potential outcomes after conditioning on observed characteristics.

We balance the pre-FPUC expiration groups on the following covariates: household size, prior household income and food sufficiency status, household head's gender, age, ethnicity, marital status and education level. Appendix Table A5 shows the difference in means between the groups for each covariate before and after balancing. To calculate the weights, we employ a probit model with state and week fixed effects to regress treatment status on the aforementioned covariates. We compute weights separately for each of the three estimation samples. We weight treated households by the given survey sample weight divided by the conditional probability of being treated given the observed characteristics. We weight untreated households by the given survey sample weight divided by the conditional probability of treatment subtracted from 1. The last column of Appendix Table A5 shows the differences in group means after applying the weights, which have diminished or become insignificant for all covariates. Although inverse-propensity weighting theoretically yields no difference in observed characteristics for treated and untreated groups, we still see some differences due to the inclusion of survey sample weights in the calculation. We make the important note that our qualitative conclusions hold whether we adjust the estimates using inverse-propensity weights or not.

5. Results

We present the results from the empirical models described in the preceding section.

5.1 Baseline Model

The results from estimating equation (1) are presented in Table 1. We find the direct effect of COVID-19 on food insufficiency is significant and positive—a 1% increase in COVID-19 hospitalization rate increases food insufficiency by .3 percentage points, holding all else equal. Additionally, COVID-19 has an indirect effect on food insufficiency households that experience pandemic-related job loss, leading to a 4.3 percentage point increase in food insufficiency for those households. This increase in food insufficiency vulnerability is more than three times that of households whose heads were not working due to reasons unrelated to the pandemic, which experience a 1.3 percentage point increase in food insufficiency relative to households with working heads.

Table 1. Estimation of Equation (1) with Various Measures of Food Insufficiency

VARIABLES	Baseline Food insufficiency	(2) Food insufficiency (broad)	(3) Spending on groceries	(4) Spending on prepared meals	(5) Ordered probit
Log of COVID-19 hospitalization rate	0.003***	0.009***	0.011**	-0.040***	0.031***
	(0.001)	(0.002)	(0.005)	(0.007)	(0.006)
Not working, pandemic reasons	0.043***	0.070***	-0.088***	-0.360***	0.264***
	(0.002)	(0.003)	(0.008)	(0.013)	(0.010)
Not working, other reasons	0.013***	0.029***	-0.027***	-0.305***	0.116***
	(0.002)	(0.002)	(0.007)	(0.010)	(0.008)
Household expects income loss	0.053***	0.134***	0.036***	-0.106***	0.452***
	(0.002)	(0.002)	(0.005)	(0.009)	(0.007)
Pre-pandemic food insufficiency - 2	0.012***	0.572***	-0.017***	-0.096***	1.202***
	(0.002)	(0.002)	(0.006)	(0.009)	(0.008)
Pre-pandemic food insufficiency - 3	0.640***	0.605***	-0.108***	-0.180***	2.224***
	(0.004)	(0.003)	(0.012)	(0.019)	(0.016)
Pre-pandemic food insufficiency - 4	0.780***	0.602***	-0.418***	-0.435***	3.431***
	(0.006)	(0.004)	(0.032)	(0.039)	(0.035)
Household size	0.003***	0.006***	-0.090***	-0.085***	0.021***
	(0.000)	(0.001)	(0.002)	(0.002)	(0.002)
Female	0.002	0.015***	-0.002	-0.109***	0.045***
	(0.001)	(0.002)	(0.005)	(0.007)	(0.006)
Age	0.004***	0.009***	0.028***	0.011***	0.034***
	(0.000)	(0.001)	(0.001)	(0.002)	(0.002)
Black, non-Hispanic	0.009***	0.001	0.059***	0.358***	0.018*
	(0.002)	(0.003)	(0.009)	(0.013)	(0.011)
Asian, non-Hispanic	-0.017***	0.000	0.046***	0.181***	-0.039***
	(0.002)	(0.004)	(0.011)	(0.018)	(0.013)
Mixed or other non-Hispanic race	0.014***	0.034***	0.002	0.101***	0.123***
	(0.003)	(0.004)	(0.011)	(0.017)	(0.014)
Hispanic, any race	-0.005**	0.005	0.150***	0.362***	-0.015
	(0.002)	(0.003)	(0.008)	(0.012)	(0.010)
Married	-0.015***	-0.014***	0.095***	0.006	-0.081***
	(0.001)	(0.002)	(0.005)	(0.008)	(0.007)
High school degree or equivalent	-0.006	0.019***	-0.136***	-0.223***	0.040***
	(0.004)	(0.005)	(0.013)	(0.022)	(0.015)
Bachelor’s degree	-0.026***	-0.021***	-0.168***	-0.274***	-0.127***

	(0.004)	(0.005)	(0.014)	(0.023)	(0.016)
Graduate degree	-0.023***	-0.028***	-0.167***	-0.390***	-0.154***
	(0.004)	(0.005)	(0.014)	(0.023)	(0.016)
Income category 2	-0.020***	0.002	0.028**	0.095***	-0.037***
	(0.004)	(0.004)	(0.012)	(0.018)	(0.014)
Income category 3	-0.033***	-0.001	0.050***	0.136***	-0.067***
	(0.003)	(0.004)	(0.011)	(0.017)	(0.013)
Income category 4	-0.043***	-0.022***	0.101***	0.211***	-0.137***
	(0.003)	(0.004)	(0.011)	(0.016)	(0.012)
Income category 5	-0.060***	-0.065***	0.143***	0.332***	-0.289***
	(0.003)	(0.004)	(0.011)	(0.016)	(0.013)
Income category 6	-0.068***	-0.094***	0.220***	0.440***	-0.409***
	(0.003)	(0.004)	(0.011)	(0.016)	(0.013)
Income category 7	-0.068***	-0.132***	0.284***	0.579***	-0.566***
	(0.003)	(0.004)	(0.012)	(0.017)	(0.015)
Income category 8	-0.066***	-0.163***	0.392***	0.764***	-0.744***
	(0.003)	(0.004)	(0.012)	(0.019)	(0.016)
Unknown prior income	-0.049***	-0.057***	0.089***	0.335***	-0.241***
	(0.003)	(0.004)	(0.013)	(0.019)	(0.013)
Initial UI claim rate	0.000	0.002	0.013*	0.002	0.003
	(0.002)	(0.002)	(0.007)	(0.010)	(0.008)
Credit and debit card spending	-0.034	0.002	0.031	0.284**	-0.102
	(0.021)	(0.028)	(0.079)	(0.117)	(0.103)
/cut1					1.339***
					(0.051)
/cut2					3.004***
					(0.052)
/cut3					4.390***
					(0.054)
Constant	0.007	0.031**	3.789***	3.220***	
	(0.009)	(0.012)	(0.037)	(0.055)	
Observations	1,337,971	1,337,971	1,235,427	1,226,189	1,337,971
R-squared	0.468	0.447	0.060	0.070	

Notes: Results of columns 1-4 are from linear regression estimations of equation (1), and column 5 shows ordered probit estimates of equation (1). The cut points for the ordered probit indicate probability cutoffs between categories. Comparison groups are household head working in past seven days, prior food sufficiency, non-Hispanic White, less than high school degree, and prior household income less than $25,000. Squared age is omitted. All estimations are weighted using survey sample weights and include state and week fixed effects. Robust standard errors are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

We observe that an increase in COVID-19 hospitalizations has an opposite effect on household spending for groceries and prepared meals. That is, a 1% increase in hospitalization rate leads to a 1.1% increase in grocery spending and a 4% decrease in spending on prepared meals.

These findings support the hypothesis that households hoard groceries in response to higher pandemic severity. This change in behavior may also be motivated by fear of illness. Although we have no way of differentiating prepared meals consumed in restaurants from those consumed as take-out at home, grocery stores may be perceived as safer than restaurants.

Households expecting employment income loss experience a 5.3 percentage point increase in food insufficiency despite a 3.6% increase in spending on groceries. These households also spend 10.6% less on prepared meals than those not anticipating a loss of employment income. When taken together, these observations indicate that households are probably stockpiling groceries while conserving money by eating out less to smooth consumption in anticipation of income disruption.

Larger households and households with older, unmarried, Black, mixed race, or Hispanic heads experience greater rates of food insufficiency, while having a bachelor's degree or higher protects against food insufficiency. Although prior literature finds female-headed households are more food insufficient, we find no significant difference in conventionally defined food insufficiency. Unsurprisingly, relatively higher categories of 2019 household income are more protective against food insufficiency. Although Ribar and Hamrick (2003) found that only 79% of food insufficient households remained insufficient after a two-year period, indicating low persistence, we observe that households reporting greater degrees of pre-pandemic food insufficiency are increasingly likely to report food insufficiency during the pandemic. This indicates that the pandemic makes it more difficult for vulnerable households to escape food insufficiency.

We observe a handful of noteworthy differences in columns 2 and 6. Female-headed households are more likely to experience broad food insufficiency, and this predictor stays significant in the ordered probit model. However, we also see the differences in broad food insufficiency between White and Black, Asian, and Hispanic households disappear. In the ordered probit model, Hispanic households are not predicted to have higher food insufficiency, and Black households are barely significant in predicting higher food insufficiency. When defined broadly, the rates of food insufficiency are not statistically different for households that earned less than $50,000 in 2019.

While we expect higher food insufficiency for groups that spent less on groceries and/or prepared meals in the previous week, we find that food insufficiency rates increase despite higher mean weekly spending for households with heads who are older, Black, or mixed race. Conversely, more educated households spend less on food despite maintaining lower food sufficiency rates. This could be because more educated households may adjust shopping behavior as discussed in Section 2 more easily, while these behaviors might be less common for older, Black or mixed-race households. There is a higher concentration of minorities and those holding less than a high school degree as well lower vehicle ownership in US Census tracts classified as food deserts, where a substantial proportion of residents have low access to supermarkets or large grocery stores (Dutko et al. 2012). These populations might be particularly vulnerable to food supply shocks and have limited ability to adapt spending behavior in response to rising prices. In a study of food insufficiency in food deserts among the elderly, Fitzpatrick et al. (2015) find that not owning a vehicle is associated with food insufficiency, and this effect might be exacerbated by the reduction in public transport observed during the pandemic. While we cannot observe vehicle ownership or residency in our data, this is one possible explanation for why Black, Hispanic and older households suffer greater food insufficiency.

5.2 Mitigating Effect of Unemployment Insurance

We present the results of equation (2) in Table 2, where we interact the household head's job status with the following in turn: a binary indicator of whether the household used UI for spending needs in the past week, the maximum statutory UI benefits, in hundreds, and emerging unemployment as measured by the regular and FPUC-expanded eligibility UI claim rates. Our estimation for column 2 excludes data from the first six survey waves, as the questions concerning spending resources were added in the seventh wave.

Our estimation of γ_3 in column 1 indicates that without UI, pandemic-induced job loss is associated with a 6.1 percentage point increase in food insufficiency risk. However, receipt of UI mitigates 1.4 percentage points of this elevated risk. When food insufficiency is measured broadly, we find that pandemic-induced job loss is associated with a 7.9 percentage point increase in risk for households without UI, and the effect is reduced by 2.6 percentage points when UI is used for spending needs. It's interesting to note that UI protects households not working for other reasons from broad food insufficiency by a similar magnitude (2.3 percentage points) but has no effect on conventionally defined food insufficiency rates for these households.

We also present evidence that higher statutory maximum UI benefits provide incrementally more food insufficiency protection. We find that a \$100 increase in maximum weekly UI is associated with a .7 percentage point decrease in food insufficiency rate for households with pandemic-induced job loss. While our calculation of the maximum UI variable assumes that LWA benefits were administered for all weeks following the FPUC expiration until the end of the program in December, our understanding that the additional benefits were capped at six weeks and withheld for weeks until conferred in a lump sum causes us to question the usefulness of this estimate.

Table 2. Estimate of Food Insufficiency for Job Status Interacted with Statutory Maximum UI Benefits

	(1)	(2)	(3)	(4)
VARIABLES	UI as Spending Resource	Maximum UI (100s)	UI and PUC Claim Rate	UI Claim Rate
Panel 1. Conventional Food Insufficiency				
$\boldsymbol{\gamma_1}$) Not working, pandemic reasons	0.061***	0.115***	0.058***	0.057***
	(0.005)	(0.011)	(0.004)	(0.004)
$\boldsymbol{\gamma_1}$) Not working, other reasons	0.014***	0.021***	0.016***	0.016***
	(0.002)	(0.007)	(0.003)	(0.003)
$\boldsymbol{\gamma_2}$) UI	-0.003	0.001	0.002**	0.003**
	(0.003)	(0.003)	(0.001)	(0.002)
$\boldsymbol{\gamma_3}$) **(**Not working, pandemic reasons) * (UI)	-0.014**	-0.007***	-0.010***	-0.015***
	(0.007)	(0.001)	(0.002)	(0.003)
$\boldsymbol{\gamma_3}$) **(**Not working, other reasons) * (UI)	0.011	-0.001	-0.002	-0.003
	(0.007)	(0.001)	(0.002)	(0.003)
Observations	939,345	1,337,971	1,337,971	1,337,971
R-squared	0.455	0.468	0.468	0.468
Panel 2. Broad Food Insufficiency				
$\boldsymbol{\gamma_1}$) Not working, pandemic reasons	0.079***	0.167***	0.098***	0.096***
	(0.005)	(0.012)	(0.004)	(0.004)
$\boldsymbol{\gamma_1}$) Not working, other reasons	0.034***	0.065***	0.035***	0.036***
	(0.003)	(0.009)	(0.003)	(0.003)
$\boldsymbol{\gamma_2}$) UI	0.049***	0.006	0.005***	0.009***
	(0.005)	(0.004)	(0.001)	(0.002)
$\boldsymbol{\gamma_3}$) **(**Not working, pandemic reasons) * (UI)	-0.026***	-0.010***	-0.019***	-0.028***
	(0.008)	(0.001)	(0.003)	(0.004)
$\boldsymbol{\gamma_3}$) **(**Not working, other reasons) * (UI)	-0.023**	-0.004***	-0.005**	-0.009***
	(0.009)	(0.001)	(0.002)	(0.003)
Observations	939,345	1,337,971	1,337,971	1,337,971
R-squared	0.447	0.447	0.447	0.447

Notes: Results are from linear regression estimations of equation (2), where the UI variable interacted with job status is noted in the column header. The dependent variable for Panel 1 is households that sometimes or often did not have enough food, and for Panel 2 it also includes households without the kinds of food wanted. Comparison group is household head was working in past seven days. Omitted controls include expected loss of household income, prior food insufficiency, prior income category, household size, household head's gender, age, ethnicity and marital status, and state-level COVID-19 hospitalization rate and combined credit and debit card spending. All estimations are weighted using sample survey weights and include state and week fixed effects. Robust standard errors are shown in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Since the actual LWA benefits received were only supplemented for six weeks, we estimate the effect of a smaller reduction in UI than was truly experienced by Americans. For this reason, we believe our estimate of a .7 percentage point drop in risk to be biased upward and larger than the true effect. We further address the LWA program complications in the next subsection.

Columns 3 and 4 provide evidence that higher emerging unemployment is protective against food insufficiency. Specifically, a 1 percentage point increase in expanded eligibility UI claims is associated with a 1 percentage point decline in food insufficiency rates for households with pandemic-induced job loss. This shows that households in states that provide more UI support are more protected against the risk of food insufficiency associated with pandemic-induced job loss. This effect is robust to including only regular UI claims in the insured unemployment rate and robust to using different measures of food insufficiency.

Interestingly, we find that no measure of UI significantly offsets the risk of food insufficiency associated with not working for other reasons when food insufficiency is conventionally defined. However, all four measures are significant in mitigating the effect on broadly defined food insufficiency. This indicates that UI programs still provide some support for these households, although this effect is not as pronounced.

5.3 Difference-in-Differences Estimates

In Table 3, we present the estimated impact of a $600 per week reduction in UI benefits from our difference-in-differences framework. Column 1 presents equation (3) estimates using our full working-age sample, while Column 2 restricts the sample to households whose heads did not work in the past week. Finally, Column 3 restricts the sample to households that did not use regular, pre-pandemic income sources for spending needs in the past week.

Panel 1 includes all data from 2020 and can be interpreted as the long-term effect of the program expiration. In comparison, Panel 2 excludes observations made after the first LWA checks were received and can be interpreted as the short-term effect of the policy.

Table 3. Difference-in-Differences Estimated Change in Food Insufficiency for UI Beneficiaries After FPUC Expiration

VARIABLES	(1) Full Sample	(2) Not Working	(3) Non-Regular Income
Panel 1 – Full Time Period			
Treated	-0.020***	-0.030***	-0.049***
	(0.004)	(0.006)	(0.007)
(Treated) * (Post-FPUC)	0.032***	0.038***	0.042***
	(0.004)	(0.008)	(0.008)
F-statistic	0.877	0.783	0.880
Observations	939,345	264,582	222,061
R-squared	0.425	0.443	0.424
Panel 2 – Before LWA			
Treated	-0.015***	-0.026***	-0.046***
	(0.004)	(0.006)	(0.007)
(Treated) * (Post-FPUC)	0.021***	0.032***	0.036***
	(0.005)	(0.009)	(0.010)
F-statistic	0.877	0.779	0.878
Observations	610,594	178,003	143,048
R-squared	0.417	0.439	0.419

Notes: Column 1 includes the full sample for which we have information about UI use, while column 2 restricts the sample to household heads that did not work in the past week, and column 3 restricts the sample to households that did not use regular, pre-pandemic income sources for spending needs in the past week. Panel 2 also excludes observations made after the first LWA checks were received. Results are from linear regression estimations of equation (3). Post-FPUC effects are captured by week fixed effects and therefore omitted. Controls include expected loss of household income, prior food insufficiency, prior income category, household size, household head's job status, gender, age, ethnicity and marital status, and state-level credit and debit card spending. All estimations are weighted using inverse-propensity and sample survey weights, and all include state and week fixed effects. Robust standard errors are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

We also note that Panel 1 estimates are biased downward due to the additional benefits conferred through the LWA program, and therefore can be read as lower-bound estimates of the true effect.

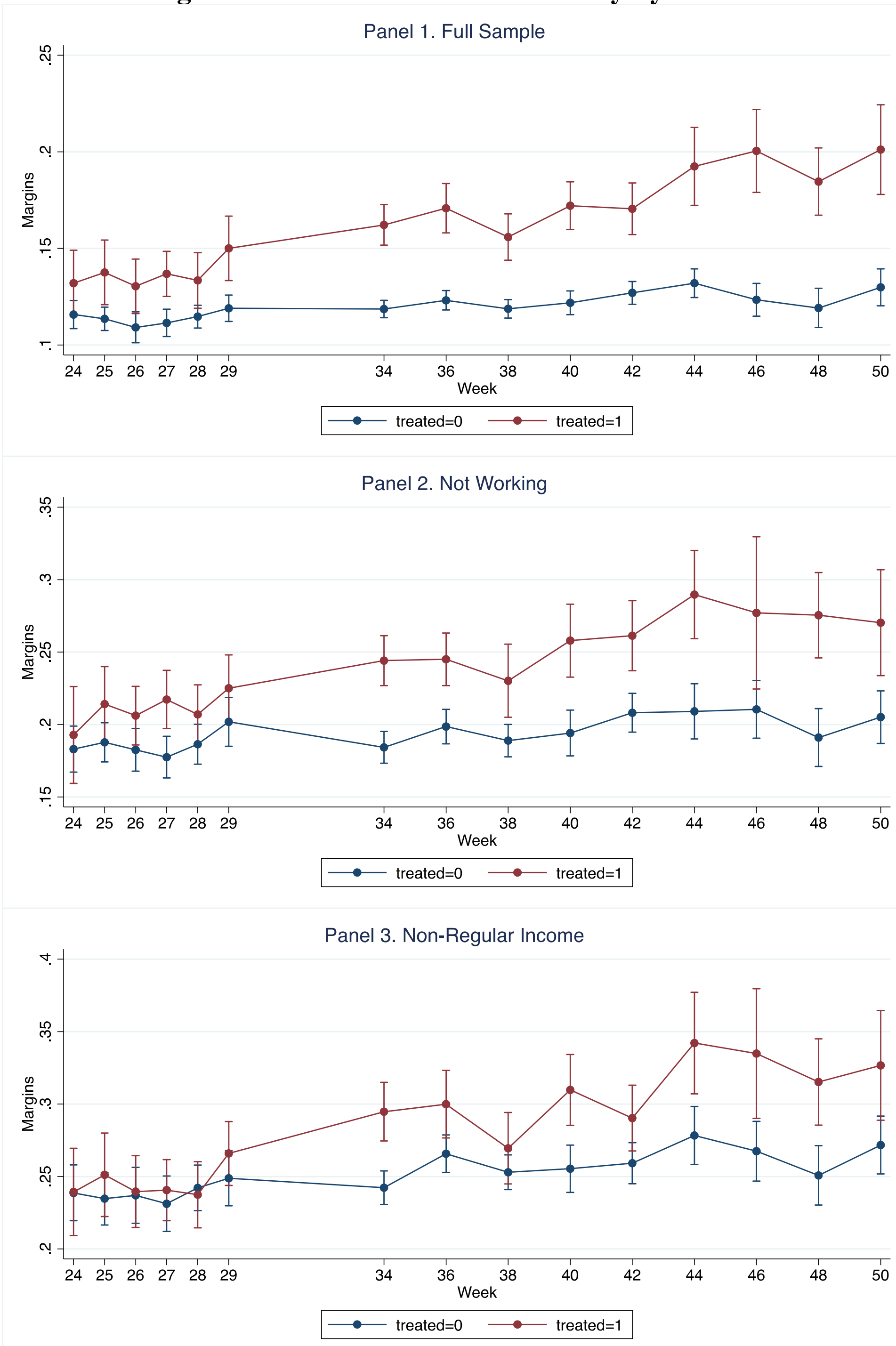
The significant negative coefficient on treatment for all estimations shows that our prior conclusion that FPUC-bolstered UI reduced food insufficiency is robust to different estimation sample restrictions, although this is no surprise. We quantify the reduced food insufficiency risk attributable to FPUC-bolstered UI receipt as between 1.5 and 2 percentage points for all working age households, 2.6 to 3 percentage points for non-working households, and 4.6 to 4.9 for households with non-regular income.

Notably, we find significant positive effects for UI receipt after the expiration of additional benefits that are larger in magnitude than the negative effects pre-FPUC expiration for all estimates except for the non-regular income groups. This indicates that base UI without the additional $600 weekly benefit is not enough to reduce food insufficiency risk for unemployed households during the pandemic. We observe this effect in the weeks before the LWA, quantifying the short-term risk to increase by 2.1, 3.2, and 3.6 percentage point increases the three groups, respectively. Our long-term estimates of increased food insufficiency risk, which may be considered lower-bound estimates, rise to 3.2, 3.8 and 4.2 percentage points for each group, respectively. We expected the increases to be larger for the non-working and non-regular income subsamples, as these groups likely face higher liquidity constraints that are associated with higher food insufficiency risk.

We compare our estimates to that of Berkowitz and Basu (2021), who found a 3.88 percentage point increase in post-expiration food insufficiency for non-regular income households that also reporting a loss of household income since the pandemic. We find slightly lower estimates for the short-term effects of the policy expiration but find a similar magnitude effect in the long-term for non-working households. We also consider the results of Raifman et al. (2020), which estimate that FPUC-bolstered UI reduces food insecurity by 5.63 percentage points, and UI without the supplement reduced food insecurity by 1.89 percentage points in households earning less than $75,000 that experienced job loss during the pandemic.

Figure 4 depicts the margins of each week by treatment status for each of the estimations presented in Table 3. Upon visual inspection, we see the food insufficiency trends before the FPUC expiration are similar between groups in all three panels, lending support for the validity

Figure 4. Predicted Margins of Week on Food Insufficiency by Treatment Status



Notes: Estimated margins of each week on food insufficiency from linear regression of treatment status interacted with dummy variable for week. Estimates use the full post-FPUC observation period. Controls include employment status, expected loss of household income, prior food insufficiency, prior income category, household size, household head's gender, age, ethnicity and marital status, and state-level COVID-19 hospitalization rate, initial UI claim rate and credit and debit card spending. Week and state fixed effects included. Estimates presented with 95% confidence intervals. All estimates weighted using inverse-propensity and sample survey weights.

of our model. We find that balancing our estimation sample on observed covariates before the treatment makes the groups similar at the start of observation. However, we still observe higher food insufficiency rates for UI-recipient households in Panels A and B, which indicates that our groups still differ by unobservable characteristics. groups In Panel C, the food insufficiency rates between the groups have considerably more overlap in the pre-expiration period, and we note that the food insufficiency rates are alarmingly high at 25%.

While we might consider that food insufficiency rates at week 29 are indicative of consumption-smoothing behavior in anticipation of the policy change, we observe a similar increase in food insufficiency for the control group. Therefore, we cannot attribute the rise in food insufficiency before the policy to anticipation. For all panels, we observe a general upward trend in food insufficiency rates for UI-reliant households, and a slighter upward trend in our control group in Panel C.

We also observe a marked dip in food insufficiency for week 38 in all panels, which is most pronounced in Panel C. Week 38 was administered between September 16 and September 28, and we note that 16 states sent the first LWA checks in this interval, and another 16 states sent out the first checks in the preceding interval corresponding to week 36. We also observe a dip in food insufficiency at week 48 for Panels A and C, but we observe the drop in our control groups as well.

For Panels A and B, we observe a significant diversion in food insufficiency rates immediately following the FPUC expiration; the gaps remain significant and grow with time. In Panel C, we also see a gap in food insufficiency following the expiration of the program, but it varies in magnitude more than the other groups. The food insufficiency rate for the treated group varies much more for Panel C in between weeks 34 and 44, indicating that these households are most impacted by benefit fluctuations from the LWA policy, likely due to liquidity restrictions that limit the households' ability to smooth consumption when faced with income volatility.

Food Spending

We now turn to examine the effect of the FPUC expiration on weekly grocery and prepared meal spending. The results of equation (3) are presented in Appendix Table A6, and the margins derived from equation (4) are displayed in Appendix Figures A4 and A5. Surprisingly, we observe higher weekly grocery spending for UI-reliant households under the FPUC for all samples despite higher food insufficiency rates for the same time period. We also observe lower weekly prepared meal spending for the full sample panel, so grocery spending may be higher for UI-reliant households because a higher the food budget may be dedicated to groceries. After the expiration of the extended benefit amount, the grocery spending gap between groups in the full sample closes and begins to reverse. For both non-working and non-regular income subsamples, the gap also lessens. In comparison, weekly prepared meal spending is similar between treatment groups for the restricted samples, and post-expiration spending falls for the treated group. For the unrestricted sample, UI-recipient households spend less before the expiration

We observe higher grocery spending in week 36 relative to other post-FPUC weeks, which may correspond to receipt or anticipated receipt of LWA checks. Since food insufficiency dipped in the following survey wave (week 38), we see higher food spending have a slightly lagged effect on reducing food insufficiency.

6. Conclusions

We use data from a novel household survey to study the role of the COVID-19 pandemic in the drastic increase in food insufficiency in the US. We quantify the effects of COVID-19 and pandemic-induced employment loss in determining household food insufficiency and the efficacy of UI in mitigating that effect. We build upon preexisting literature that estimates the mitigating effect of UI by applying inverse-propensity weights to the groups in our difference-in-differences specification. Additionally, this paper presents the first attempt that we know of to address the confounding effect of the LWA program in estimating UI efficacy during the COVID-19 crisis.

We document the direct effect of the pandemic on food insufficiency: each 1% increase in the state COVID-19 hospitalization rate is associated with .3 percentage point increase in food insufficiency. Additionally, we find that households with pandemic-induced job loss have a 4.5 percentage point higher risk of food insufficiency than those employed. Without unemployment insurance, this risk rises to 6.1 percentage points.

Although some jobs have been recovered since the beginning of the COVID-19 crisis, at the time of writing the unemployment rate in the United States remains at 6.2% (Bureau of Labor Statistics 2021). UI program expansions have been enacted to soften the economic blow of the pandemic for those affected by employment loss. Policymakers should know that under the FPUC expansion, UI reduced food insufficiency by 2 percentage points for the general population, or 3 for non-working households. However, this effect is offset by the reduction following the FPUC expiration. Policymakers should consider that base UI alone is not enough to prevent a rise in food insufficiency rates among beneficiary households.

We recognize some limitations in our study. First, UI benefit receipt may be endogenous with respect to food insufficiency. If there are unobserved factors that determine UI spending that are correlated with food insufficiency, for example, food insufficiency overpowering any effect of stigma that might deter an applicant, we would be concerned about the validity of our estimates. One way to overcome this limitation would be to instrument UI spending with UI eligibility, as suggested by Gruber (1997). The HPS was designed to provide a near real-time snapshot of the

US during the crisis, and therefore favored brevity over detail. Although our data prevents us from instrumenting UI receipt with eligibility, future research may make use of more comprehensive datasets.

The LWA program also presents challenges in measuring the maximum UI benefit receipt due to its inconsistent implementation. Although we approximate when the first checks were sent out in each state, we don't know when the households in our sample received benefits, nor for how many weeks of back pay they were eligible. Unfortunately, our data precludes us from knowing the asset level, true income, or true value of received UI benefits for each household (which is not necessarily equal to the maximum amount).

Despite these limitations, our paper offers valuable insight to the food sufficiency facet of the developing COVID-19 pandemic literature. In the future, we hope to expand our research to consider the effects of UI benefit volatility during the pandemic and consider the second increase in UI benefits enacted in 2021.

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Appendix

Appendix Table A1: Variable Descriptions

Variable Name	***Vector***	***Variable Description***	***Type***
Level of food insufficiency	Y_{ist}	Based on the survey question: In the last 7 days, which of these statements best describes the food eaten in your household? 1 = Enough of the kinds of food (I/we) want to eat 2 = Enough but not always the kinds of food (I/we) want to eat 3 = Sometimes not enough to eat 4 = Often not enough to eat	Categorical
Food insufficiency	Y_{ist}	= 1 if level of food insufficiency is 3 or 4	Binary
Food insufficiency (broad)	Y_{ist}	= 1 if level of food insufficiency is 2, 3 or 4	Binary
Spending on food at home	Y_{ist}	The log of real spending on food to prepare and eat at home per adult equivalent. Based on the survey question: During the last 7 days, how much money did you and your household spend on food at supermarkets, grocery stores, online, and other places you buy food to prepare and eat at home? The reported amount is further divided by the household adult equivalency value (1 for the first adult, 0.75 for each additional adult, and 0.5 for each child under 18 years old), and it is adjusted for inflation using the Bureau of Labor Statistics' Food CPI for Urban Consumers in prices of June 2019.	Continuous
Spending on food away from home	Y_{ist}	The log of real spending on prepared meals per adult equivalent. Based on the survey question: During the last 7 days, how much money did you and your household spend on prepared meals, including eating out, fast food, and carry out or delivered meals? All adjustments are the same as for spending on food at home.	Continuous
Pre-pandemic food insufficiency	Y_{i0}	The 4-category level of household food insufficiency prior to March 13, 2020	Categorical
Employment status	J_{ist}	HH head employment status = 0 if HH head worked for pay or profit in past 7 days = 1 if HH head not working due to pandemic reasons = 2 if HH head not working due to other reasons	Categorical
Expecting income loss	X_{ist}	=1 if HH expects a loss of employment income in next 4 weeks due to pandemic	Binary
Household size	X_{ist}	Number of persons in HH	Continuous
Female	X_{ist}	= 1 if HH head is Female	Binary
Married	X_{ist}	=1 if HH head is married	Binary
Age	X_{ist}	HH head age in 2020, calculated as 2020 – birth year	Continuous
Ethnicity	X_{ist}	HH head ethnicity = 1 if non-Hispanic White = 2 if non-Hispanic Black = 3 if non-Hispanic Asian = 4 if non-Hispanic other or mixed = 5 if Hispanic of any race	Categorical
Educational Attainment	X_{ist}	HH head educational attainment = 1 if less than high school degree = 2 if high school or equivalent degree = 3 if bachelor's degree = 4 if graduate degree	Categorical

Income Category	X_{ist}	HH pre-tax 2019 income categorized into eight groups, with one group for missing data	Categorical
Credit and debit card spending	X_{ist}	Credit and debit card spending in past week relative to January 4 – 31, 2020 Source: OIET	Continuous
Average COVID-19 hospitalization rate	X_{ist}	Log of average number of people hospitalized with COVID-19 per 100,000 in past week Source: COVID Tracker	Continuous
Initial regular UI claim rate	X_{ist}, UI	Initial claims for regular UI in past week per 100,000 Source: OIET	
Expanded-eligibility UI claim rate	UI	Initial claims for UI and PUC in past week per 100,000 Source: OIET	Continuous
Maximum UI benefits	UI	Time-varying statutory maximum UI benefit amount Source: Government websites	Continuous
Spending – UI	UI	= 1 if HH used UI benefits for spending needs in past week	Binary
Spending – Regular income		=1 if HH used regular income sources like those used before the pandemic for spending needs in past week	Binary
Household Work Loss		=1 if HH reports loss of employment income since March 13, 2020	Binary

Notes: Unless otherwise specified, all variables are from the Household Pulse Survey.

Appendix Table A2: Summary Statistics

Variables	*Food sufficient*	*Food insufficient*	*All*
Employment status			
Had work in past week	0.678	0.432	0.648
	(0.001)	(0.004)	(0.001)
Not working, pandemic reasons	0.130	0.303	0.152
	(0.001)	(0.004)	(0.001)
Not working, other reasons	0.192	0.265	0.201
	(0.001)	(0.003)	(0.001)
Household expects income loss in next month	0.303	0.625	0.343
	(0.001)	(0.004)	(0.001)
Household pre-pandemic food insufficiency			
Enough of kinds of food wanted	0.735	0.217	0.671
	(0.001)	(0.003)	(0.001)
Enough, but not always kind of food wanted	0.239	0.152	0.228
	(0.001)	(0.003)	(0.001)
Sometimes not enough to eat	0.023	0.480	0.080
	(0.000)	(0.004)	(0.001)
Often not enough to eat	0.002	0.151	0.021
	(0.000)	(0.003)	(0.000)
Household size	3.512	3.958	3.567
	(0.005)	(0.017)	(0.005)
Female	0.507	0.543	0.512
	(0.001)	(0.004)	(0.001)
Age	42.822	40.211	42.500
	(0.034)	(0.099)	(0.032)
Race/ethnicity			
White, non-Hispanic	0.626	0.434	0.603
	(0.001)	(0.004)	(0.001)
Black, non-Hispanic	0.108	0.218	0.121
	(0.001)	(0.003)	(0.001)
Asian, non-Hispanic	0.058	0.029	0.054
	(0.001)	(0.001)	(0.001)
Mixed race or other, non-Hispanic	0.039	0.056	0.041
	(0.000)	(0.002)	(0.000)
Hispanic of any race	0.169	0.263	0.181
	(0.001)	(0.004)	(0.001)
Married	0.563	0.357	0.537
	(0.001)	(0.004)	(0.001)
Level of Education			
Less than high school	0.067	0.182	0.081
	(0.001)	(0.004)	(0.001)
High school equivalent	0.585	0.722	0.602
	(0.001)	(0.004)	(0.001)
Bachelor’s degree	0.200	0.065	0.183
	(0.001)	(0.001)	(0.001)
Graduate degree	0.149	0.031	0.134
	(0.001)	(0.001)	(0.001)

Household 2019 income categories			
Less than $25,000	0.103	0.339	0.132
	(0.001)	(0.004)	(0.001)
$25,000 - $34,999	0.085	0.161	0.095
	(0.001)	(0.003)	(0.001)
$35,000 - $49,000	0.099	0.128	0.103
	(0.001)	(0.002)	(0.001)
$50,000 - $74,999	0.152	0.121	0.148
	(0.001)	(0.002)	(0.001)
$75,000 - $99,999	0.123	0.050	0.114
	(0.001)	(0.002)	(0.001)
$100,000 - $149,999	0.149	0.028	0.134
	(0.001)	(0.001)	(0.001)
$150,000 - $199,999	0.070	0.007	0.062
	(0.000)	(0.001)	(0.000)
$200,000 and above	0.076	0.004	0.067
	(0.000)	(0.000)	(0.000)
Unknown prior income	0.142	0.161	0.145
	(0.001)	(0.003)	(0.001)
COVID-19 hospitalization rate	15.125	16.363	15.277
	(0.027)	(0.091)	(0.026)
Credit and debit card spending	-0.086	-0.084	-0.086
	(0.000)	(0.001)	(0.000)
New UI claim rate	0.838	0.846	0.839
	(0.001)	(0.005)	(0.001)
Maximum unemployment insurance benefits	954.798	933.578	952.179
	(0.434)	(1.420)	(0.419)
Observations	1,234,436	103,535	1,337,971

Notes: Standard errors in parentheses. Table reports the mean and standard errors for key independent variables in the estimation sample, weighted by survey sample weights.

Appendix Table A3: Unemployment Insurance by State

State	Base Weekly Maximum UI	Extension Amount	Estimated Date of First Payment
Alabama	$275	$300	9/3/20
Alaska	$370	$300	11/6/20
Arizona	$240	$300	8/24/20
Arkansas	$451	$300	9/12/20
California	$450	$300	10/22/20
Colorado	$618	$300	9/18/20
Connecticut	$649	$300	9/17/20
Delaware	$400	$300	9/20/20
District of Columbia	$444	$300	9/24/20
Florida	$275	$300	9/8/20
Georgia	$365	$300	9/11/20
Hawaii	$648	$300	9/25/20
Idaho	$448	$300	9/10/20
Illinois	$484	$300	9/9/20
Indiana	$390	$300	9/21/20
Iowa	$481	$300	9/9/20
Kansas	$488	$300	10/9/20
Kentucky	$552	$400	9/13/20
Louisiana	$247	$300	8/27/20
Maine	$445	$300	9/12/20
Maryland	$430	$300	9/12/20
Massachusetts	$823	$300	9/1/20
Michigan	$362	$300	9/18/20
Minnesota	$740	$300	9/4/20
Mississippi	$235	$300	9/27/20
Missouri	$320	$300	8/26/20
Montana	$552	$400	8/24/20
Nebraska	$440	$300	10/6/20
Nevada	$469	$300	10/15/20
New Hampshire	$427	$300	9/10/20
New Jersey	$713	$300	10/22/20
New Mexico	$511	$300	9/9/20
New York	$504	$300	9/17/20
North Carolina	$350	$300	9/3/20
North Dakota	$618	$300	9/16/20
Ohio	$647	$300	9/15/20
Oklahoma	$539	$300	9/23/20

Oregon	$648	$300	10/1/20
Pennsylvania	$572	$300	9/17/20
Rhode Island	$732	$300	9/3/20
South Carolina	$326	$300	9/24/20
South Dakota	$414	$0	N/A
Tennessee	$275	$300	8/27/20
Texas	$521	$300	8/24/20
Utah	$580	$300	9/11/20
Vermont	$513	$300	9/24/20
Virginia	$378	$300	10/16/20
Washington	$790	$300	9/22/20
West Virginia	$424	$400	9/16/20
Wisconsin	$370	$300	10/15/20
Wyoming	$508	$300	9/16/20

Sources: Base UI gathered from government websites by the COVID-19 State Policy Database. Paydate estimates taken from the Lost Wages Assistance Tracker found at https://www.unemploymentpua.com/articles/lwatracker.html. Wisconsin, Nevada and Alaska were listed as estimated or unknown dates and replaced with dates found by author on state government websites.

Appendix Table A4: Association Between UI Generosity and Employment

Variables	(1) Not Working	(2) Work Loss
Maximum Statutory UI (hundreds)	-0.048***	0.015
	(0.015)	(0.015)
Observations	1,358,213	1,358,447

Notes: Column 1 regressed on a binary variable for whether or not the household head had work in the past week. Column 2 regressed on a binary variable for whether or not the household experienced a loss of employment income since March 13, 2020. Controls include expected loss of household income, prior food insufficiency, prior income category, household size, household head's gender, age, ethnicity and marital status, and state-level credit and debit card spending. All estimations are weighted using sample survey weights, and all include state and week fixed effects. Robust standard errors are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table A5: Differences in Covariate Means Between Treated and Untreated Groups for the Full Sample, Before FPUC Expiration

	Unadjusted Means			Adjusted Means		
	(1)	(2)	(3)	(4)	(5)	(6)
Variables	Untreated	Treated	Difference	Untreated	Treated	Difference
Household pre-pandemic food insufficiency						
Enough of kinds of food wanted	0.672	0.601	0.071***	0.659	0.663	0.004
	(0.002)	(0.006)	(0.000)	(0.002)	(0.005)	(0.808)
Enough, but not always kind of food wanted	0.231	0.291	0.060***	0.239	0.246	0.007
	(0.002)	(0.006)	(0.000)	(0.002)	(0.005)	(0.127)
Sometimes not enough to eat	0.079	0.091	0.012***	0.083	0.074	0.009**
	(0.002)	(0.004)	(0.001)	(0.002)	(0.003)	(0.031)
Often not enough to eat	0.019	0.017	0.002	0.019	0.016	0.003
	(0.001)	(0.002)	(0.438)	(0.001)	(0.002)	(0.183)
Household size	3.608	3.701	0.093***	3.636	3.598	0.038
	(0.010)	(0.027)	(0.001)	(0.010)	(0.025)	(0.218)
Female	0.508	0.518	0.010**	0.510	0.510	0.000
	(0.002)	(0.006)	(0.020)	(0.002)	(0.006)	(0.651)
Age	42.371	41.223	1.148***	42.171	42.601	0.430**
	(0.068)	(0.167)	(0.000)	(0.070)	(0.153)	(0.029)
Race/ethnicity						
White, non-Hispanic	0.600	0.540	0.060***	0.588	0.604	0.016**
	(0.002)	(0.006)	(0.000)	(0.003)	(0.006)	(0.036)
Black, non-Hispanic	0.123	0.144	0.021***	0.127	0.121	0.006
	(0.002)	(0.004)	(0.000)	(0.002)	(0.003)	(0.183)
Asian, non-Hispanic	0.051	0.069	0.018***	0.052	0.059	0.007
	(0.001)	(0.004)	(0.001)	(0.001)	(0.003)	(0.131)
Mixed race or other, non-Hispanic	0.043	0.044	0.001	0.044	0.041	0.003
	(0.001)	(0.002)	(0.613)	(0.001)	(0.002)	(0.247)
Hispanic of any race	0.183	0.204	0.021***	0.189	0.176	0.013*
	(0.002)	(0.006)	(0.000)	(0.002)	(0.005)	(0.056)
Married	0.536	0.474	0.062***	0.527	0.529	0.002
	(0.002)	(0.006)	(0.000)	(0.002)	(0.006)	(0.752)
Level of Education						
Less than high school	0.094	0.084	0.01	0.095	0.081	0.014***
	(0.002)	(0.005)	(0.140)	(0.002)	(0.004)	(0.009)
High school equivalent	0.585	0.685	0.100***	0.600	0.601	0.001
	(0.002)	(0.005)	(0.000)	(0.002)	(0.006)	(0.933)
Bachelor’s degree	0.178	0.160	0.018***	0.174	0.180	0.006
	(0.001)	(0.003)	(0.000)	(0.001)	(0.004)	(0.193)
Graduate degree	0.143	0.071	0.072***	0.131	0.138	0.007
	(0.001)	(0.002)	(0.000)	(0.001)	(0.004)	(0.121)

Household 2019 income categories						
Less than $25,000	0.150	0.174	0.024***	0.157	0.133	0.024***
	(0.002)	(0.005)	(0.000)	(0.002)	(0.004)	(0.000)
$25,000 - $34,999	0.099	0.135	0.036***	0.107	0.096	0.011***
	(0.002)	(0.004)	(0.000)	(0.002)	(0.003)	(0.005)
$35,000 - $49,000	0.105	0.135	0.030***	0.110	0.107	0.003
	(0.001)	(0.004)	(0.000)	(0.002)	(0.003)	(0.498)
$50,000 - $74,999	0.146	0.182	0.036***	0.149	0.162	0.013**
	(0.002)	(0.005)	(0.000)	(0.002)	(0.004)	(0.018)
$75,000 - $99,999	0.113	0.117	0.004	0.112	0.123	0.011***
	(0.001)	(0.004)	(0.406)	(0.001)	(0.004)	(0.008)
$100,000 - $149,999	0.135	0.102	0.033***	0.130	0.135	0.005
	(0.001)	(0.003)	(0.000)	(0.001)	(0.004)	(0.167)
$150,000 - $199,999	0.063	0.035	0.028***	0.058	0.059	0.001
	(0.001)	(0.002)	(0.000)	(0.001)	(0.003)	(0.695)
$200,000 and above	0.071	0.022	0.049***	0.064	0.064	0.000
	(0.001)	(0.001)	(0.000)	(0.001)	(0.004)	(0.856)
Unknown prior income	0.119	0.099	0.020***	0.115	0.121	0.006
	(0.001)	(0.004)	(0.000)	(0.001)	(0.004)	(0.104)
Observations	356,999	48,836		356,999	48,836	

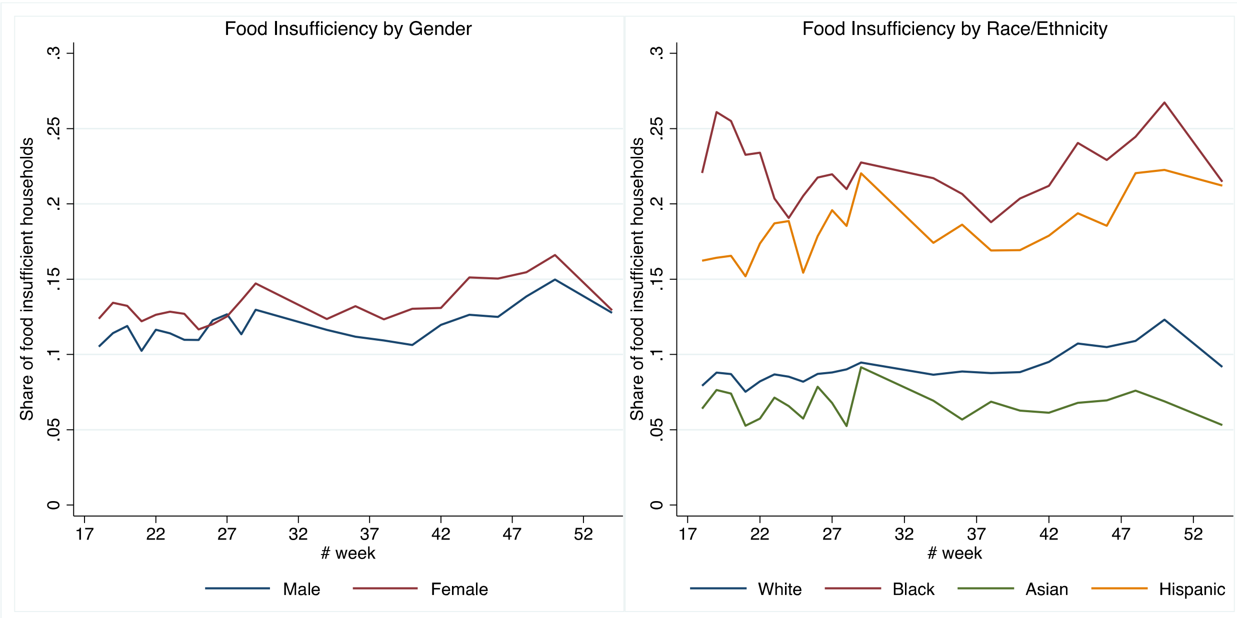
Notes: Unadjusted means are weighted by survey sample weights. Adjusted means are weighted by inverse-propensity and survey sample weights. Standard errors for the means in parentheses for columns 1, 2, 4 and 5. P-values for differences in parentheses in columns 3 and 6. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table A6. Difference-in-Differences Estimate of Change in Spending on Groceries and Prepared Meals for UI Beneficiaries After FPUC Expiration

Variables	(1) Full Sample	(2) Not Working	(3) Non-Regular Income
Panel 1 – Grocery Spending			
Treated	0.063***	0.113***	0.154***
	(0.012)	(0.020)	(0.021)
(Treated) * (Post-FPUC)	-0.065***	-0.051**	-0.089***
	(0.014)	(0.023)	(0.025)
F-statistic	0.859	0.973	0.889
Observations	870,226	244,037	202,644
R-squared	0.053	0.053	0.050
Panel 2 – Prepared Meal Spending			
Treated	0.025	0.074**	0.169***
	(0.021)	(0.033)	(0.033)
(Treated) * (Post-FPUC)	-0.115***	-0.153***	-0.185***
	(0.024)	(0.040)	(0.041)
F-statistic	0.564	0.824	0.810
Observations	862,896	241,510	200,071
R-squared	0.064	0.046	0.064

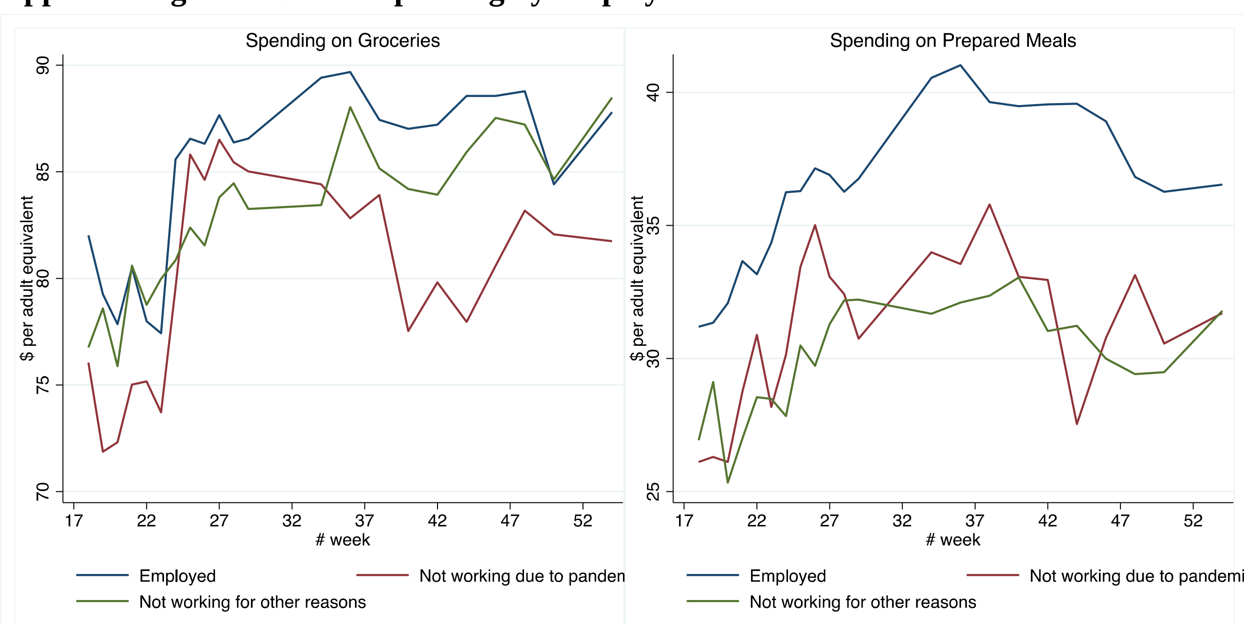
Notes: Results are from linear regression estimations of equation (3). The dependent variable for Panel 1 is the log of adult-equivalency-adjusted weekly household spending on groceries, and the dependent variable for Panel 2 is the adjusted spending on prepared meals. Both panels include all post-FPUC observations. Column 1 includes the full sample for which we have information about UI use, while column 2 restricts the sample to household heads that did not work in the past week, and column 3 restricts the sample to households that did not use regular, pre-pandemic income sources for spending needs in the past week. Post-FPUC effects are captured by week fixed effects and therefore omitted. Controls include expected loss of household income, prior food insufficiency, prior income category, household size, household head's job status, gender, age, ethnicity and marital status, and state-level credit and debit card spending. All estimations are weighted using inverse-propensity and sample survey weights, and all include state and week fixed effects. Robust standard errors are shown in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix Figure A1: Food Insufficiency by Demographics



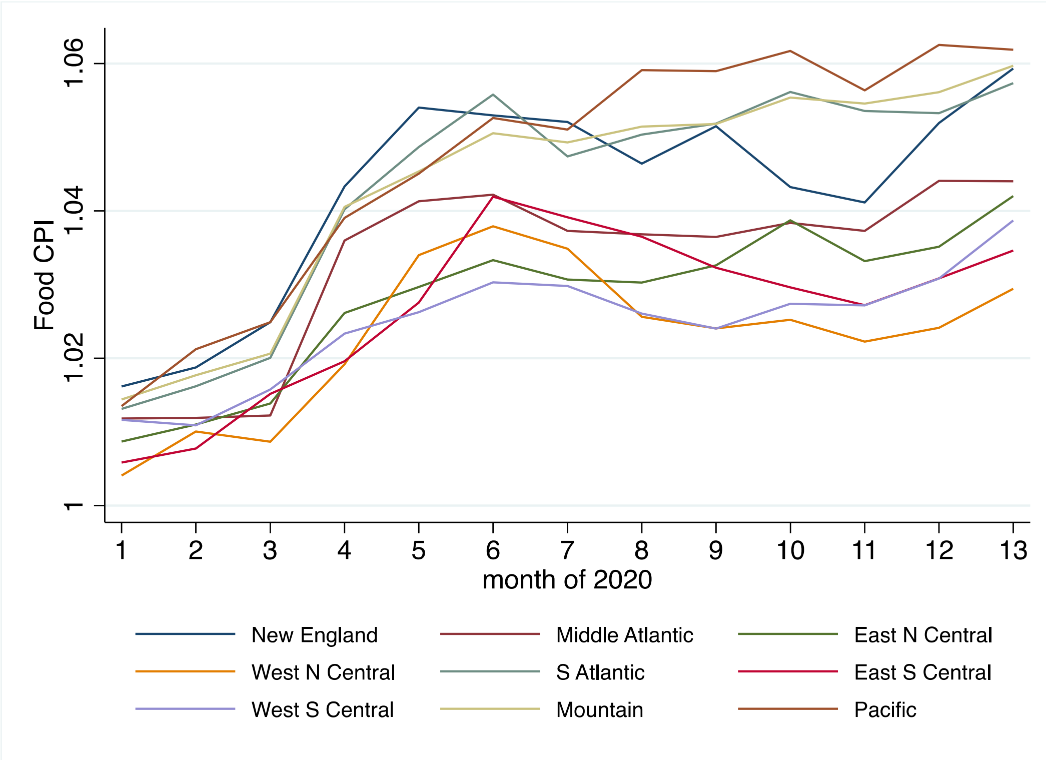
Notes: Shares are weighted using survey sample weights. Week 54 indicates the second week of 2021.

Appendix Figure A2: Food Spending by Employment



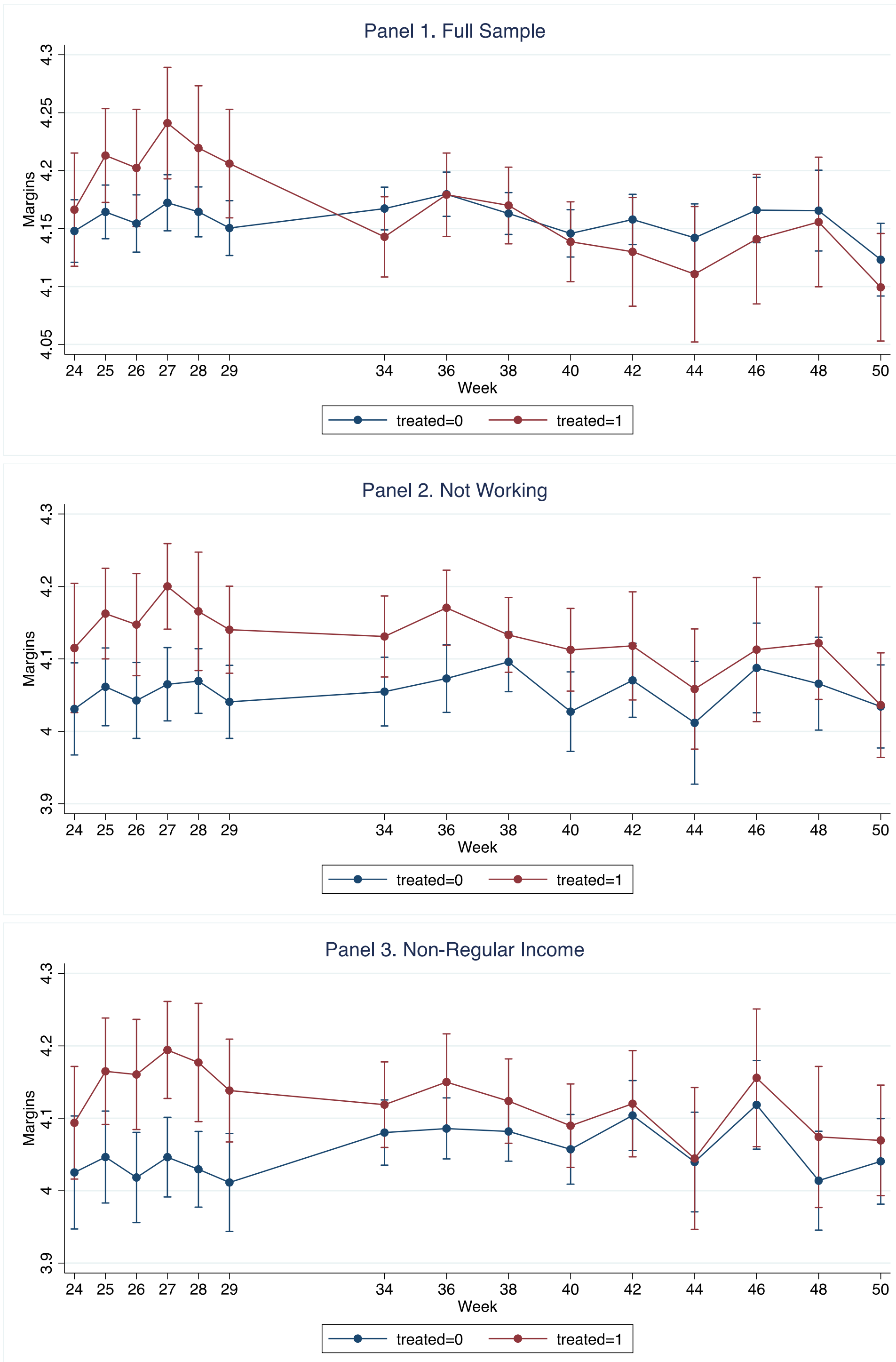
Notes: Values are weighted using survey sample weights. Week 54 indicates the second week of 2021.

Appendix Figure A3: Food CPI by Division



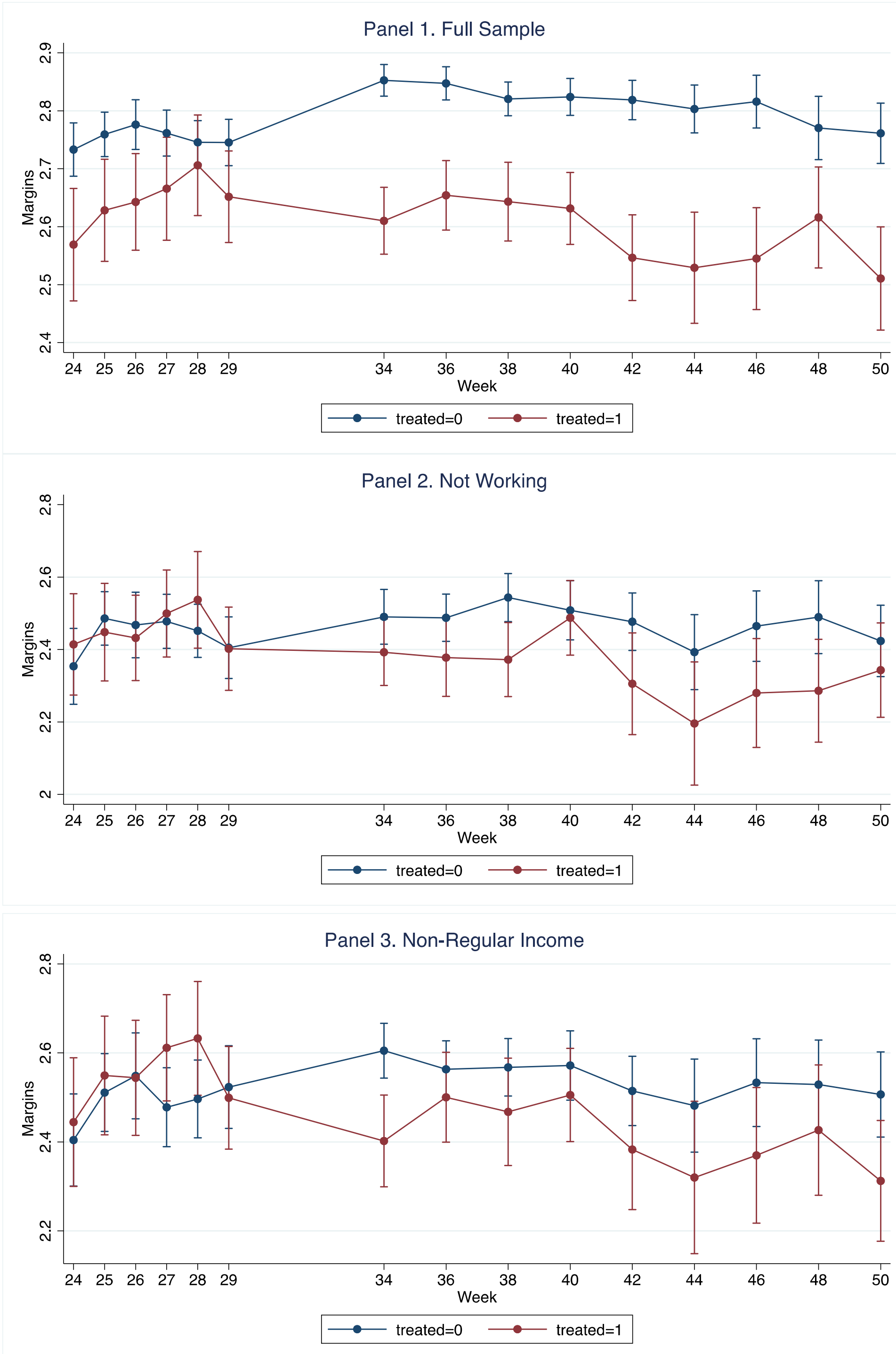
Notes: Food CPI in urban areas, June 2019=1. Source is Bureau of Labor Statistics. Month 13 indicates January of 2021.

Figure A4. Predicted Margins of Week on Weekly Grocery Spending by Treatment Status



Notes: Estimated margins of each week on food insufficiency from linear regression of equation (4). Estimates use the full post-FPUC observation period. Controls include employment status, expected loss of household income, prior food insufficiency, prior income category, household size, household head's job status, age, gender, ethnicity and marital status, and state-level COVID-19 hospitalization rate, initial UI claim rate and credit and debit card spending. Week and state fixed effects included. Estimates presented with 95% confidence intervals. All estimates weighted using inverse propensity weights.

Figure A5. Predicted Margins of Week on Weekly Meal Spending by Treatment Status



Notes: Estimated margins of each week on food insufficiency from linear regression of equation (4). Estimates use the full post-FPUC observation period. Controls include employment status, expected loss of household income, prior food insufficiency, prior income category, household size, household head's job status, age, gender, ethnicity and marital status, and state-level COVID-19 hospitalization rate, initial UI claim rate and credit and debit card spending. Week and state fixed effects included. Estimates presented with 95% confidence intervals. All estimates weighted using inverse propensity weights.