UNEMPLOYMENT INSURANCE, FOOD PREFERENCE CHANGE, AND HEALTH OUTCOMES

by

CHANDRA KANT DHAKAL

(Under the Direction of Chen Zhen)

ABSTRACT

This dissertation consists of three studies that focus on consumer behavior in food and health economics. I explore how individual and household responds to price and income changes.

In Chapter 1, I leverage the sharp drop in unemployment insurance (UI) benefits following the expiration of the Federal Pandemic Unemployment Compensation program to estimate the consumption smoothing effect of UI. I find that the consumption effect of UI is countercyclical, greater when economic conditions are weak. The UI effect is also heterogeneous over respondents differentiated by race and ethnicity, income, homeownership, presence of children, state unemployment rate, and state UI generosity. The estimated effect of UI on self-assessed food sufficiency and confidence about future food sufficiency is largely consistent with the food spending results.

In Chapter 2, I examine the causal relationship between unemployment and health. I take advantage of a spike in unemployment caused by the COVID-19 pandemic as a natural experiment and exogenous economic shock as instrumental variables to estimate the causal effect of job loss on self-reported general and mental health in the United States. While the mechanisms of health effects of unemployment remain uncertain, we argue that being unemployed increases the likelihood of having poor general and mental health. I argue that these effects are not entirely attributable to the decline in income associated with job loss, and psychological factors may be at play.

In Chapter 3, I estimate consumer demand for food Classified by the Thrifty Food Plan categories and the evolution of consumer preferences for nutritional quality using the Exact Affine Stone Index demand system. I show internet search intensity has a key role in shaping food preferences and hence the healthfulness of food purchases. I also find a strong link between increased household expenditure, nutritional information, and food choices.

INDEX WORDS: Unemployment insurance, Consumer smoothing, Health outcomes, Nutritional quality, Demand system

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by

CHANDRA KANT DHAKAL

BS, TRIBHUVAN UNIVERSITY, 2011

MS, TEXAS TECH UNIVERSITY, 2016

MS, UNIVERSITY OF GEORGIA, 2021

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CHANDRA KANT DHAKAL

Major Professor:	Chen Zhen
Committee:	Jeffrey H. Dorfman
	Travis A. Smith

Electronic Version Approved:

Ron Walcott
Vice Provost for Graduate Education and Dean of the Graduate School
The University of Georgia
August 2022

DEDICATION

I dedicate this dissertation to my family for their love and unlimited support.

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Researcher(s)' own analyses calculated (or derived) based in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business.

The conclusions drawn from the NielsenIQ data are those of the researcher(s) and do not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

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CHAPTER 1

THE IMPACT OF UNEMPLOYMENT INSURANCE BENEFIT REDUCTION ON FOOD SPENDING AND FOOD HARDSHIP

1. INTRODUCTION

The coronavirus disease 2019 (COVID-19) pandemic brought about unprecedented public health, economic, and social crises in the United States and around the world. On March 27, 2020, US President Donald Trump signed into law the Coronavirus Aid, Relief, and Economic Security (CARES) Act that, inter alia, provided an extra $600 per week under the Federal Pandemic Unemployment Compensation (FPUC) program to all individuals receiving unemployment insurance (UI) benefits. With the uniform $600 weekly supplemental benefits provided by FPUC, three-quarters of unemployed workers were eligible for UI benefits that exceeded their lost wages (Ganong, Noel and Vavra 2020). In July 2020, UI benefits accounted for 7% of US aggregate personal income (Bureau of Economic Analysis 2020), up from 1.3% during the Great Recession, making UI the largest safety net program in program outlay during the pandemic. The end of the $600 supplemental benefit on July 31 came at a time when 18 million workers were unemployed and initial UI claims had been more than 2 million for 16 consecutive weeks. The expiration of FPUC was estimated to reduce total UI benefits by 52% between July and August, 2020 (Farrel et al. 2020a). We leverage this sharp decline in July 2020 as a quasi-experiment to identify the causal effect of UI on household well-being as measured by food hardship and spending.

Food insufficiency is a broad measure of household food hardships, whose prevalence and severity are of great interest to our understanding of social welfare. An extensive public health literature has documented associations of food insufficiency in the United States with negative academic and psychosocial outcomes in school-aged children (Alaimo, Olson and Frongillo 2001), lower health status and greater odds of hospitalization among young children (Cook et al. 2004), lower women's mental health (Heflin, Siefert and Williams 2005), and lower outcomes in a host of other measures of health among the above and other demographic groups (Gundersen and Ziliak 2015). In US surveys, food insufficiency is measured by the respondent's selection in a four-option multiple-choice item that best describes the food consumed at the person's household. The options range from "enough of the kinds of food we want to eat" (sufficient) to "often not enough to eat" (most insufficient). The advantages of the single-item food insufficiency question are its clarity and simplicity, which translate to a low response burden. For these reasons, the food insufficiency question is used in the December Current Population Survey to screen eligibility for the more comprehensive 18-item and 10-items food insecurity module depending upon whether a household has kid(s) or not.

Unemployment is considered one of the main contributors to food insufficiency and insecurity (Gundersen, Kreider and Pepper 2011). In the early phase of the COVID-19 pandemic, the US unemployment rate jumped from 3.5% in February to 14.8% in April 2020 (US Bureau of Labor Statistics 2021). Schanzenbach and Pitts (2020) estimate the April food insecurity rate to be 17.3% compared to 8.5% in February and that the increase in unemployment explains more than half of the increase in food insecurity. An examination of the causal effect of UI in alleviating food insufficiency and insecurity is thus essential for a better understanding of the performance of social safety net programs in the COVID-19 pandemic.

As a self-reported and somewhat subjective measure of food hardship, the external validity of the single-item food insufficiency question has also come under scrutiny. While the food insufficiency measure is associated with objective measures of household food needs and consumption, diet quality, and economic well-being, the degree of the associations is often lower than expected (Hamilton et al. 1997; Nord and Brent 2002; Rose and Oliveira 1997; and Bhattacharya et al. 2004). For example, Gundersen and Ribar (2011) do not find a threshold in the food expenditure distribution below which self-reported food insufficiency and insecurity are ubiquitous, even though intuition suggests the existence of a threshold. They conclude that the external validity of the food insufficiency and insecurity measures may be weak (Gundersen and Ribar 2011). Gregory and Smith (2019) observe an increase in the probability of being classified as food insecure around the time of monthly benefit receipt from the Supplemental Nutrition Assistance Program (SNAP). The authors find a higher likelihood of affirmation in the salience window because 1) food consumption goes down and 2) experience of food hardship is more salient (Gregory and Smith 2019). Owing to these potential biases in food insufficiency and insecurity measures, we also use food expenditures as a more objective measure of household food hardship and economic well-being.

Using the Census Bureau's Household Pulse Survey from June 11 to December 21, 2020, we find that the expiration of FPUC reduced food expenditures, food sufficiency, and confidence about future food sufficiency. The effect on food expenditures is more pronounced among UI recipients in states and survey waves with higher UI claim rates, with lower incomes, not owning a home, or without children, although not all heterogeneities are precisely estimated. We provide strong evidence that the consumption smoothing effect of UI is countercyclical, larger during economic downturns. The heterogeneity analysis using the food sufficiency and confidence measures largely generates qualitatively similar results as the analysis on food spending, barring a few statistically insignificant differences. This leads us to conclude that the food sufficiency and confidence measures have good external validity.

The contribution of this study is threefold. First, we combine a nationally representative micro dataset with a credible identification strategy to estimate the causal effect of UI on food hardship and spending in the midst of the pandemic and to examine the extent to which this effect may be heterogeneous among population subgroups. Previous studies of the consumption smoothing effect of UI during the 2020 pandemic either use descriptive statistics and time-series plots to obtain graphical evidence (Farrell et al. 2020a, b), or are based on aggregate data from a subset of counties from one state (Casado et al. 2020). A few authors have examined the potential work disincentive provided by the CARES Act in general and FPUC in particular, but find no evidence of moral hazard associated with these COVID relief policies (Finamor and Scott 2020; Bartik et al. 2020; Boar and Mongey 2020; Dube 2021; Marinescu, Skandalis, and Zhao 2020). The focus of the present study on the potential benefit of UI in mitigating food hardship and smoothing consumption helps complete our understanding of the benefit and cost of UI during the pandemic—arguably the worst sudden economic downturn since the Great Depression in many ways.

Second, our identification strategy differs from pre-pandemic studies of the consumption smoothing effect of UI. Following the pioneering work of Gruber (1997) and in recognition that UI receipt and benefit amount are endogenous, the literature has largely used state and temporal variations in UI eligibility, the benefits for which a jobless worker is eligible, to identify the effect of UI on the eligible population. This produces the intent-to-treat effect, which has the advantage that the treatment, UI eligibility, is controlled by the government and hence the estimate is of direct relevance to policy design (Gruber 1997, p. 195). The disadvantage is that the estimate understates the effect of UI on recipients because not all eligible workers apply for UI. In comparison, we rely on the exogenous sharp decline in UI benefit amount to identify the effect on UI recipients, i.e., the average treatment effect on the treated. Because the supplemental FPUC benefit amount is uniform across all UI recipients, there is less concern of bias arising from simultaneity between the magnitude of the benefit reduction and recipient unobserved heterogeneity.

Third, the sizable reduction in UI benefits through the expiration of FPUC and the variation in UI claim rates across states provide the statistical power to test the hypothesis that the consumption smoothing effect of UI varies over the business cycle. Previous efforts to measure the cyclicality of UI consumption benefits have generated imprecise estimates (East and Kuka 2015) or imprecise and economically insignificant estimates (Kroft and Notowidigdo 2016). In conjunction with the lack of evidence on the moral hazard cost of UI during the COVID-19 pandemic in 2020, our finding of statistically and economically significant countercyclical effects of the consumption smoothing effect of UI suggests that the FPUC was likely welfare-improving for the society as a whole.

The remainder of this article proceeds as follows. The next section discusses the related literature and contributions of the present study. We then describe the data and empirical strategy. This is followed by a presentation of the empirical results. The last two sections discuss the implications of the results and concludes, respectively.

2. RELATED LITERATURE

This study is related to several strands of literature. Several studies have examined patterns of food acquisition and food security during the pandemic. Using data from the Household Pulse Survey, Restrepo, Rabbitt and Gregory (2021) estimate that involuntary unemployment due to employer shutdowns during April-June 2020 caused significant reductions in household food expenditures and food sufficiency and significant increases in charitable food receipt. Also using the Household Pulse Survey, Ziliak (2021) reports a 75% increase in food insufficiency among older adults ages 60 and above, and a 50% increase in charitable food receipt among disadvantaged adults. In a panel survey of 1370 US households, Ellison et al. (2021) report small decreases in the stated importance of price and nutrition in food purchases during the early phases of the pandemic. Using Feeding America's Map the Meal Gap food insecurity projection model, Gundersen et al. (2021) estimate an increase of 17 million food-insecure Americans in 2020 from the 2018 level with substantial variation in food insecurity rates across US counties.

We complement this line of COVID research that focuses on the effect of COVID relief efforts on consumer spending. Using a sample of six million regular users of Chase deposit accounts, Farrell et al. (2020b) find that spending of UI recipients in April, 2020 increased by 10% relative to their pre-pandemic levels, while spending of the employed was down by 10% during the same period. They speculate that the $600 UI supplement likely explains the spending differences between UI recipients and the employed (Farrell et al. 2020b). Casado et al. (2020) estimate the association between county-level consumer spending and UI replacement rate (the ratio of UI benefits to pre-unemployment wages) using data from 18 Illinois counties during the January–June, 2020 period. Based on the parameter estimates, the authors forecast that eliminating the FPUC supplement would reduce consumer spending by 44% (Casado et al.

2020). Several studies have examined the effect of the stimulus payment passed under the CARES Act on consumer spending and saving behavior. The marginal propensity to spend out of the stimulus payment is found to be inversely related to household income (Karger and Rajan 2021; Chetty et al. 2020; Sahm et al. 2020) and expectations of future negative income shocks such as job loss and UI benefit reduction (Baker et al. 2020), but positively related to local cost of living (Misra, Singh, and Zhang 2021).

Finally, this study joins a very small, but nevertheless important, collection of papers on the consumption benefits of UI. In his pioneering work on the consumption smoothing benefit of UI, Gruber (1997) estimates that a 10-percentage point increase in replacement rate reduces the decline in food expenditures by 2.8 percentage points (off an implied decline of 23 percentage points in the absence of UI) in the 1968−1987 Panel Study of Income Dynamics (PSID). Using the 1993−1995 Canadian Out of Employment Panel survey, Browning and Crossley (2001) find that the effect of Canadian UI on total expenditure is wholly concentrated on the unemployed without assets. Kroft and Notowidigdo (2016) expand Gruber's specifications with an interaction between replacement rate and state unemployment rate but do not find the consumption smoothing effect of UI to vary meaningfully over the business cycle. In their analysis of the PSID, East and Kuka (2015) extend the sample period to 2011 and find suggestive evidence that the consumption smoothing effect is larger when the state unemployment rate and UI generosity are higher. Kuka (2020) estimates that more generous UI benefits increase health insurance coverage and utilization and self-reported health, with larger effects found in periods of higher unemployment rates, for respondents to the 1996−2013 Survey of Income and Program Participation and 1993−2015 Behavioral Risk Factor Surveillance System. Our study is closely related to Raifman, Bor and Venkataramani (2021), who estimate that receipt of UI is associated with a 35% reduction in food insecurity among respondents to the longitudinal Understanding Coronavirus in America study. They, however, are careful not to interpret their estimates as causal effects because of the well-known bias of self-selection of the more disadvantaged unemployed into UI (Gruber 1997).

3. DATA AND DESCRIPTIVE STATISTICS

We use the public-use files of the Household Pulse Survey, which is a repeated cross-sectional[1] survey conducted by the Census Bureau in collaboration with eight other federal statistical and regulatory agencies. The survey is designed to understand how the COVID-19 pandemic is impacting households across the country and to aid post-pandemic recovery. The Census Bureau uses address-based random sampling to select respondents. The sampled respondent receives an email or text with a link to complete the survey that takes approximately 20 minutes to complete. The survey includes detailed demographic information, including the respondent's gender, race, age, education, household size, annual household income in 2019, indicators for the presence of children below age 18 years, indicators for homeownership, food insufficiency, health, and food spending. This is the only available data to examine the effect of unemployment insurance benefit reduction on food spending and food security.

The Household Pulse Survey has gone through three phases. Phase 1 has 12 waves, each conducted over a period of 6 days with the exception of the first wave that was collected over a 13-day period, from April 23 to July 21, 2020. Phase 2 has 5 waves from August 19 to October 26, 2020. Phase 3 started on October 28 and is ongoing. A wave in Phase 2 and 3 covers a 13-

[1] Phase 1 of the survey included respondents in one week in the following week's sample for up to three weeks total. However, only 8% of respondents participated for more than one week, making the data essentially repeated cross sections.

day period. We use data collected between June 11 (wave 7), when UI information was collected for the first time, and December 21 (wave 21), 2020, representing fifteen survey waves (see Appendix table A1 for details). For the analysis, we select respondents who (1) are working-age adults (between 18 to 65 years old), (2) reported household income disruption and job loss at some point during the COVID-19 pandemic, and (3) had an annual household income below $100,000 in 2019. The analysis sample consists of 246,051 observations. We apply the survey weights to all estimates and cluster standard errors at the state level.

[insert Table 1.1 here]

We use five outcome variables (Table 1.1) to measure household food hardship and spending. The sufficiency variable is the 1–4 ordinal response to the standard food insufficiency question. Following Coleman-Jensen et al. (2021) We recode the response such that 1, 2, 3, and 4 indicate very low food insufficiency, low food sufficiency, marginal food sufficiency and high food sufficiency, respectively. The confidence variable is a 1–4 ordinal variable measuring confidence in ascending order about food sufficiency (1=not confident at all, 2=somewhat confident, 3=moderately confident and 5=very confident) in the next four weeks. Food spending is measured by food-at-home (FAH) expenditure, food-away-from-home (FAFH) expenditure, and the sum of FAH and FAFH expenditures over the 7-day period preceding the survey. UI receipt is measured by an indicator variable on whether the respondent's household used UI benefits to meet its spending needs in the past 7 days.

[insert Figure 1.1 here]

Figure 1.1 shows the distribution of food sufficiency in the last 7 days and confidence about future food sufficiency by UI receipt status over the entire sample period. Similar proportions of UI recipients and nonrecipients are food insufficient (sometimes or often not enough to eat). About 4 percentage points more UI recipients than nonrecipients have enough but not always the kinds of food they want to eat, a situation Ziliak (2021) calls food insufficiency with reduced variety. About the same percentage point more nonrecipients than recipients are food sufficient. In terms of confidence about future food sufficiency, higher proportions of UI recipients than nonrecipients are not at all, somewhat or moderately confident about future food sufficiency. This pattern of lower food sufficiency and confidence among UI recipients is expected and consistent with the self-selection of more food-insecure unemployed persons into the UI program. Thus, it emphasizes the importance of accounting for selection bias in estimating the causal effect of UI.

[insert Table 1.2 here]

In table 1.2, we report summary statistics for food spending, food sufficiency, and a number of respondent- and state-specific characteristics. 28% of respondents report using UI benefits to meet spending needs in the last 7 days. The average new COVID-19 case and death rates are approximately 2.2 and 0.3 per 100,000 people, respectively, during the sample period. For the same period, the initial claim rate of UI of any kind is 1.1%, while the continued UI claim rate is around 15%.

UI recipients spend more on FAH and less on FAFH than nonrecipients. UI recipients and nonrecipients report nearly identical mean levels of food sufficiency and confidence about future food sufficiency, notwithstanding differences in the distributions of the two measures noted above. Of the characteristics in which ≥15% differences exist between the two groups, proportions of UI recipients who receive SNAP[2] benefits or do not have a high school diploma are 4 percentage points higher and 3 percentage points lower than nonrecipients, respectively. To make UI nonrecipient credible control group for UI recipients in terms of observed characteristics, we use the propensity scores based on a logistic regression using sociodemographic characteristics (see Appendix table A2 for the logistic regression estimates). We compute the nearest-neighbor matching based on the propensity scores. After matching, the UI recipient and nonrecipient samples become similar in most observed sociodemographic characteristics.

4. EMPIRICAL STRATEGY

We use a difference-in-differences design to estimate the causal effect of reduced UI benefit amount on food spending, food sufficiency, and confidence in future food sufficiency. Our model is specified as

$$(1) \qquad y_{ist} = \beta_0 + \beta_1 UI_{its} + \beta_2 (Post \times UI_{it}) + X'_{ist}\alpha + \delta_s + \omega_t + \varepsilon_{ist}$$

where y_{ist} is an outcome of interest for respondent i in survey wave t and state s; $Post$ is an indicator equal to 1 if t is after July 31, when FPUC expired; UI is an indicator for UI receipt; X_{ist} is a column vector of respondent characteristics; δ_s is the state fixed effect; ω_t is the wave fixed effect; the β's and α's are coefficients; and ε is the residual. The state and wave fixed effects are used to control for time-invariant unobserved state heterogeneity and seasonality, respectively. UI accounts for time-invariant heterogeneity between UI recipients and nonrecipients. Of the fifteen survey waves used in this study, six were conducted between June

[2] SNAP variable is not measured until Phase 2 of the Household Pulse Survey.

11 and July 21, 2020 when FPUC was in effect and nine were conducted between August 19 and December 21 after FPUC expired. In the fully specified model, X_{ist} includes age, gender, marital status, household size, children, SNAP participation, stimulus payment recipients status, race, education, 2019 household income, state-level Google mobility data representing time spent at various locations (time spent at retail and recreation, grocery and pharmacy, outside of the residential locations), and rates of COVID cases and deaths and UI claims in the respondent's state of residence. There are five measures of y_{ist} that are of interest to us: the inverse hyperbolic sine transformations of FAH spending, FAFH spending, total food spending, and the standardized measures of food sufficiency and confidence about future food sufficiency. We use the inverse hyperbolic sine transformation to take into account that some households do not report food spending in the 7 days preceding the survey[3]. An inverse hyperbolic sine transformation of a food spending variable is calculated as $\ln\left(z + \sqrt{z^2 + 1}\right)$, where $z =$ FAH, FAFH or total food spending. For large (no less than 10) mean values of z, the transformation, which is defined at $z = 0$, closely approximates a log-linear specification's interpretation of the coefficient on $Post \times UI$ as the proportional change in food spending following the expiration of FPUC (Bellemare and Wichman 2020).

For β_2 to measure the causal effect of the FPUC expiration on food spending and food hardship, the pretrends in UI recipients and nonrecipients' food spending and food sufficiency need to be parallel after adjusting for differences in observables. Figure 1.2 plots the raw average FAH, FAH and total food expenditures by survey wave and UI receipt status over the sample period. We observe that food expenditures by UI recipients and nonrecipients exhibit similar

[3] In our sample, 1.4%, 2.6%, and 25.1% household reported zero purchases for total food, FAH and FAFH spending at the time of survey, respectively.

trends before July 31 when FPUC expired. Although there are small differences in the level of spending, UI recipients and nonrecipients closely track each other in all three categories of food spending in the five waves leading up to the expiration of FPUC. In the weeks following July 31, trends of UI recipients' FAH and total food expenditures appear to diverge from those of nonrecipients. Overall, figure 1.2 provides graphical evidence of common food spending pretrends between UI recipients and nonrecipients.

[insert Figure 1.2 here]

[insert Figure 1.3 here]

Figure 1.3 presents the average food sufficiency and confidence about future food sufficiency by survey wave and UI receipt status. The level of food sufficiency for nonrecipients is relatively stable until wave 16 (September 30–October 12, 2020) when it starts a downward trend. By contrast, food sufficiency of UI recipients consistently declined over the sample period. There are more fluctuations in confidence about future food sufficiency for both UI recipients and nonrecipients. The confidence measure for both types of respondents moved in the same direction for much of the sample period. However, unlike the food spending measures, pretrends of the raw food sufficiency and confidence time series do not appear parallel between UI recipients and nonrecipients.

The lack of parallel pretrends in the raw time series does not invalidate the difference-in-differences design by itself because the differential pretrends may be associated with differences in observables. To formally test for parallel pretrends, we follow the literature on difference-in-differences event-study designs (e.g., Miller, Johnson and Wherry 2021; Freyaldenhoven, Hansen and Shapiro 2019; and Benzarti and Carloni 2019, to name a few) to estimate the following equation

$$(2)\quad y_{ist} = \beta_0 + \beta_1 UI_{its} + \sum_{\tau=-6+}^{-2} \theta_\tau\, UI_{its} \times I[t-E=\tau] + \sum_{\tau=0}^{8+} \gamma_\tau\, UI_{its} \times I[t-E=\tau] + X'_{ist}\alpha + \delta_s + \omega_t + \varepsilon_{ist}$$

where E denotes the first survey wave post-FPUC expiration; $t - E$ measures the number of waves between t and E; $I[t - E = \tau]$ is an indicator function equal to 1 if $t - E = \tau$, and 0 otherwise; and θ_τ and γ_τ are the coefficients on the leads and lags to E, respectively. We include + sign in the x-axis label for waves outside of the range of time over which the UI benefit is thought to affect outcomes [-6<τ $(t - E) > 8$] (see Freyaldenhoven et al. (2021) for details). We follow four major suggestions by Freyaldenhoven et al. (2021) to produce the event study plots. First, the reference period is $\tau = -1$ (normalize $\theta_{-1} = 0$) , which is the 12th wave conducted during July 16–July 21, 2020. Normalize $\theta_{-1} = 0$ means that the each estimated coeffcient θ_τ and $\gamma_\tau$measures the deviation of the average outcome of UI recipients at wave t from the common trend ω_t relative to one survey wave prior to the FPUC expiration. Second, we include a parenthetical label for the average value of the outcome corresponding to the normalized coefficient (the sample mean of the dependent variable at the 12th wave) that gives a reference value for the outcome. Including parenthetical label makes it easier to interpret the imapcts of UI benefit at any survey wave. Third, we plot a uniform $sup - t$ confidence band for the event time path for each outcome as the outler bars, in addition to 95 percent confidence interval as the inner bars. This 95% uniform confidence band gives the true value of a set of parameters at least 95% of the time. Like inner confidence interval, any estimated coefficient of event-time that did not pass entirely within the outer uniform confidence band is considered statistically insignificant. Finally, we report the p values of Wald tests for joint significance of $\hat{\theta}_\tau$'s to test the parallel pre-trend and dynamics have leveled off. We also include the p value of wald test to jointly test the significance of γ_τ's. A lack of significance in the θ_τ's would suggest parallel pretrends between UI recipients and nonrecipients. A failure to reject dynamic leveling off hypothesis suggest less dynamic effects of UI benefit changes on outcomes. Conversely, joint significance of the γ_τ's would be evidence of differential posttrends and indicative of an effect of FPUC expiration on UI recipients.

We estimate equations (1)– (2) by OLS when the outcomes are food expenditures and by ordered logit when the outcomes are food sufficiency and confidence about future food sufficiency. Figure 1.4 plots $\hat{\theta}_\tau$ and $\hat{\gamma}_\tau$, their 95% confidence bounds, and a uniform sup-t confidence band for the event-time path (survey wave) from equation (2) for each of the five outcome measures over the 15 survey waves from June 11-December 21, 2020. Panel A, B, and C illustrate the results on food spending. The magnitude of most pretrend coefficients $\hat{\theta}_\tau$ is close to zero and the $\hat{\theta}_\tau$'s are jointly statistically insignificant (F test p-values = 0.967, 0.424, and 0.820 for total food spending, FAFH, and FAH spending, respectively). In contrast, all $\hat{\gamma}_\tau$'s are negative and jointly significant (F test p-values < 0.001, 0.077, and <0.001 for total spending, FAFH, and FAH spending, respectively), indicating that food spending of UI recipients decreased relative to nonrecipients after FPUC expiration.

[insert Figure 1.4 here]

Panel D and E show the results for food sufficiency and confidence. Consistent with the food spending pattern, we observe little evidence of differential pretrends between UI recipients nonrecipients. The $\hat{\theta}_\tau$ coefficients for both the food sufficiency and confidence regressions are not statistically different from zero (F test p-values = 0.778 and 0.556 for food sufficiency and confidence, respectively), suggesting parallel pretrends. The F test for joint significance of the posttrend coefficients $\hat{\gamma}_\tau$'s are highly significant (p-values <0.001 and <0.001 for food sufficiency and confidence, respectively), indicating UI recipients' food sufficiency and confidence trends start to diverge from those of nonrecipients post-FPUC expiration. In summary, the event study results provide evidence of common pretrends for food spending and food hardship measures.

5. RESULTS

We first report results from the estimation of equation (1) by pooling all respondent types. We then estimate the equation separately for respondents differentiated by income, presence of children, homeownership, race, state UI claim rate, and state maximum UI benefit amount to examine the extent to which the estimated effect is heterogeneous across respondent types. All results in this section are based on the propensity score-matched households.

Main Results

Table 1.3 reports results from the two-way fixed effects regression (1). Three versions of equation (1) are estimated. Panel A presents results from the base model where the vector X_{ist} is omitted. Panel B shows results of the model where X_{ist} includes respondent demographics. Panel C reports results from the fully specified equation (1) where X'_{ist} includes both respondent demographics and time-varying state-specific, google mobilities, COVID case and UI claim rates.

[insert Table 1.3 here]

In the regressions of food spending, the coefficient on $Post \times UI$ measures the proportional change in food expenditure in response to the reduced benefit amount following the FPUC expiration. For food sufficiency and confidence, a negative coefficient on $Post \times UI$ indicates that food sufficiency or confidence about future food sufficiency declined following the FPUC expiration.

Results from column 1–3 of table 1.3 suggest that the FPUC expiration led to reductions in UI recipients' FAH and FAFH spending across all three specifications. The coefficient estimates in panel B are extremely close to those in panel C, suggesting the addition of state-level google mobility data, COVID case and UI claim rates as control variables has little impact on our estimates of the mean effect. Based on the fully specified model in panel C, the July 31 FPUC expiration reduced UI recipient's FAH, FAFH, and total food spending by 8.7%, 13.7%, and 10.3%, respectively.[4]

Consistent with the food spending results, column 4–5 of table 1.3 point to a reduction in food sufficiency and confidence about future food sufficiency following the FPUC expiration. The coefficient on $Post \times UI$ is negative and statistically significant at the 1% level in every

[4] The full set of coefficient estimates for the fully specified equation (1) is presented in Appendix table A3. Appendix table A4 reports results from log-linear regressions of food expenditures and ordered and binary logit regressions of food sufficiency and confidence measures. Sometimes, identifying the treatment effect in nonlinear (ordinal logit or probit) difference-in-difference models is not the same as in the linear model. The interpretation of interaction terms in the nonlinear models is not straightforward (Ai and Norton 2003). We use the Probit-OLS method to re-assign sufficiency and confidence variables (van Praag and Ferrer-i-Carbonell 2008; Perez-Truglia 2020) to address this estimation issue to check the robustness of our results. We also standardized sufficiency and confidence variables to a mean of 0 and a standard deviation of 1 to simplify interpretation. These results are reported in last two columns of Appendix table A4. We also report the estimation results using the full sample (i.e., not propensity score-unmatched) to ensure that results are generalizable to full sample (Appendix table A5 and A6). Those results are qualitatively the same as the results in panel C of table 1.3.

specification for either measure of food hardship. In our preferred specification (panel C) that includes the full set of control variables, the odds ratios for food sufficiency and confidence are 0.751 ($e^{-0.286}$) and 0.799 ($e^{-0.225}$), respectively. That is, the odds of increased food sufficiency and confidence following the UI benefit reduction is 0.751 and 0.799 times the odds when FPUC was in effect. In other words, food hardship among UI recipients increased following the benefit reduction. To further explore the impacts of FPUC expiration on each type of food sufficiency (very low, low, marginal and high food sufficiency) and confidence (not at all, somewhat, moderately and very confident), we estimate the average marginal effects of $Post \times UI$ on food sufficiency and confidence categories (Table 1.4). Based on our preferred specification (Panel C of table 1.4), very low, low and marginal food sufficiency increased by 1.3%, 3.4%, and 1.2%, respectively, while high food sufficiency decreased by 5.9% following UI benefit reduction. Similarly, among confidence outcomes, not at all, somewhat confident increased by 3.2% and 1.8%, and moderately and very confident decreased by 1.7% and 3.3% after the FPUC expiration.

[insert Table 1.4 here]

Results on Heterogeneous Effects

Figure 1.5 plots $\hat{\beta}_2$ of equation (1) and its 95% confidence interval for each of the five outcomes of interest by respondent type. The estimates are obtained by respondent type-specific regressions. For example, $\hat{\beta}_2$ in panel A of figure 1.5 is obtained by separately estimating equation (1) for respondents from state-waves with above-average UI claim rates (UI uptake rates) and for respondents from state-waves with below-average UI claim rates. The average is set to the national UI claim rate. We also calculate the statistical significance of the estimated difference between the two respondent types.

[insert Figure 1.5 here]

An inspection of figure 1.5 indicates some heterogeneity in recipients' responses to the UI benefit reduction. Panel A shows that the effect is larger in state-waves with above-average UI continued claim rates. In fact, the effect on food spending is concentrated on respondents in these state-waves, while the effect on food spending in state-waves with below-average UI claim rates is not statistically different from zero. The estimated differences in the effect of the FPUC expiration on self-reported current food sufficiency and confidence about future food sufficiency are consistent with the food spending differences: the impact is larger in state-waves with high UI claim rates. However, unlike the lack of an effect on food spending in state-waves with lower UI claim rates, the impact on current and confidence about future food sufficiency is large (is 0.771 and 0.834 times the odds when FPUC was in effect, respectively) and statistically significant even for state-waves with lower UI claim rates.

Much has been reported on racial and ethnic disparities that minorities bear disproportionately higher burdens of COVID-19 case rates and deaths (Gross et al. 2020). Little is known about the potential disparities in terms of the impact of the UI benefit reduction. Panel B attempts to shed some light on this topic. In terms of impact on total food spending, the percent reduction is the largest for non-Hispanic white UI recipients at 10.2% compared with the slightly lower 9.5% (not precisely estimated) and 9.1% for non-Hispanic black and Hispanic respondents, respectively. In terms of food sufficiency, however, non-Hispanic black UI recipients experienced the largest increase in food hardship due to the benefit reduction.

Although sometimes large in magnitude, we should note that these racial and ethnic differences are statistically insignificant.

Panel C divides the sample into those with less than $50,000 in 2019 family income and those above it. The plot shows that the lower-income group reacted more strongly to the FPUC expiration than does the higher-income group. The larger contrasts lie in FAFH spending, food sufficiency and confidence about future food sufficiency, where the point estimates of the $\hat{\beta}_2$'s of the lower-income group are 7.10, 2.49 and 4.89 times those of the higher income group, respectively. The difference in FAFH spending is significant at the 5% level. The percentage reduction in total food spending of lower-income respondents is 2.22 times that of higher-income respondents, but the difference is not statistically significant.

Panel D breaks down the effect by homeownership. We use homeownership as a proxy for liquidity and asset, with homeowners being less liquidity constrained and holding more assets than renters. Because UI benefits are transitory incomes, the consumption smoothing effect of UI is expected to be larger on unemployed workers who are liquidity constrained and owning less assets (Browning and Crossley 2001). The estimated effect of the FPUC expiration on food spending is indeed larger on renters than it is on homeowners. Only the differences in estimates for food confidence by homeownership are statistically significant at the 10% 10 levels.

Panel E illustrates the estimated effect by the presence of children. The effect on FAH spending of respondents with children is 56% of the effect on those without children, while the effect on FAFH is slightly larger for respondents with children than for those without children. Because FAH is less expensive than FAFH on a per calorie basis, households can mitigate the adverse effect of UI benefit reduction on nutrient intakes by shifting some FAFH budget to FAH. The return to this cost minimization strategy could be higher for households with children, which tend to be larger households, because of the economy of scale in household production of FAH. This may help explain the differential effect of the UI benefit reduction on FAH and FAFH spending between households with and without children. The smaller percentage decline in FAH spending in households with children than those without children may also reflect the reluctance of parents to sacrifice children's health because childhood food insecurity is shown not only adversely affect short-term (Gundersen and Ziliak 2015) but also long-term health (Hoynes, Schanzenbach, and Almond 2016). However, none of the food spending differences are statistically significant between the two household types. Panel E also shows that, contrary to the direction of spending differences, households with children experienced a larger decline in food sufficiency and confidence about future food sufficiency than did households without children following the FPUC expiration. Since child food sufficiency measure[5] is available in the HPS for our study window, we estimated equation (1) for child food sufficiency by ordered logit to formally test this hypothesis. The coefficient on $Post \times UI$ is positive and statistically significant at the 1% level which is consistent with our heterogeneity analysis. The average marginal effects for child food sufficiency categories show that odds of low and marginal food sufficiency increased by 1.6% and 3.7%, but high food sufficiency decreased by 5.3% after the expiration of FPUC (results not shown). The difference in the comparative magnitude of the UI effect by presence of children between the objective food spending and the more subjective, self-assessed food sufficiency and confidence measures may reflect the fundamental differences in preference parameters such as risk aversion, time discount, and the subsistence level of food consumption

[5] Child food sufficiency was measured based on the following question: "The children were not eating enough because we just couldn't afford enough food. 1) Often true, 2) Sometimes true, and 3) Never true." We redefine these categories such that 1, 2 and 3 represent low, marginal, and high food sufficiency, respectively.

between households with and without children. Nevertheless, only the difference in food sufficiency is statistically significant between the two household types.

UI in the United States is jointly administrated by the federal and state governments. The maximum UI benefit level varies across states. East and Kuka (2015) hypothesize that the consumption smoothing effect could be higher in states with more generous UI benefits. The reason for this heterogeneity could be that there is a threshold benefit level above which the UI effect starts to take shape.[6] Panel F divides the sample into respondents from states whose UI benefit generosity is above the national median of replacement rate of 50%, excluding the FPUC supplemental $600, and states below the national median. The effect of the July 31 benefit reduction on total food spending is slightly larger for respondents in higher-benefit states than those in lower-benefit states, although the difference is not statistically significant. The differential effect is more salient in FAFH spending, where the effect on respondents in higher-benefit states is 3 times the effect in lower-benefit states and the former is statistically significant while the latter is not. These patterns of food spending changes are consistent with the suggestive evidence provided by East and Kuka's (2015) that the UI effect is larger in higher-benefit states. Like East and Kuka's analysis, ours lacks the statistical precision to draw a more definitive inference. The differential is reversed for food sufficiency and confidence about future food sufficiency. The $\hat{\beta}_2$'s for higher-benefit states are 44% and 41% lower in magnitude than those for lower-benefit states. Again, like the results by presence of children, the subjective nature of the food sufficiency and confidence measures likely contribute to the qualitative different results

[6] East and Kuka's (2015) second explanation, which is not applicable to our estimates of treatment on the treated, is that their intent-to-treat UI effect increases with UI take up rates. As UI benefits become more generous, UI participation will increase.

from those of food spending. In lower-benefit states, the $600 cut in supplemental benefit represents a larger percentage decline in benefits than in higher-benefit states, which results in a greater reduction in perceived food sufficiency and confidence about future food sufficiency.

Robustness check

Identification of the UI effect relies on the sharp drop in benefits following the FPUC expiration. However, the difference-in-differences in matching may not eliminate differences in unobservable factors between the UI recipients and nonrecipients and between UI recipients before and after the FPUC expiration. For example, if the July benefit reduction made the less needy unemployed workers less likely to apply for UI benefits, identification of the UI effect would be in jeopardy. To test whether the July benefit reduction led to a change in UI participation, we regress the UI participation status on the benefit reduction indicator (after July 31) with state fixed effect. We find that UI participation is not associated with benefit reduction (coefficient =0.018 cluster-robust standard error = 0.011). In addition, UI participants before and after the FPUC expiration are similar in most of the observed sociodemographic characteristics in our matched sample. This provides suggestive evidence that, on average, the decision to take up UI after the expiration of FPUC is not different from the decision when FPUC was in place.

To test whether the estimated effect of the July benefit reduction is a coincidence, we employ a placebo test (e.g., Perez-Truglia 2020) to check the robustness of our main results

(3) $$y_{ist} = \beta_0 + \beta_1 UI_{its} + \beta_2(Post \times UI_{its}) + \beta_3\left(Placebo_{post} \times UI_{its}\right) + X'_{ist}\alpha + \delta_s + \omega_t + \varepsilon_{ist}$$

where $Placebo\_post$ is a "placebo" treatment indicator for an incorrect FPUC expiration date of June 30. $Placebo\_post$ takes a value 1 if June 30 < t < July 31, and 0 otherwise. In equation (3), β_3 measures the average change in y of UI recipients relative to that of nonrecipients during July

1–July 30, the last month before FPUC expiration. We expect β_3 to be close to zero and statistically insignificant if outcomes did not change until after FPUC expired.

[insert Table 1.5 here]

Table 1.5 reports results from estimating equation (3). The estimated coefficient on $Placebo\_post \times UI$ is small in magnitude and statistically insignificant for all outcomes of interest. In contrast, the estimated coefficients on $Post \times UI$ remain statistically significant and are very close to their counterparts in table 1.3. The lack of significance for $\hat{\beta}_3$ is further evidence that the estimated reduction in food spending, and confidence following FPUC expiration is unlikely to be a coincidence. It also supports the earlier event-study finding that the common pretrends assumption hold for most of the outcome variables.

6. DISCUSSION

Using the estimated coefficients on $Post \times UI$ for food spending regressions reported in panel C of table 1.3, we can calculate the marginal propensity to spend (MPS) on food out of UI benefits. After the expiration of FPUC, the Lost Wage Assistance (LWA) program was created to partially fill the gap by providing UI recipients a weekly supplemental $300 in federal contribution and, if the state chose to, an additional $100 in state contribution for up to six weeks. Four states provided the $100 per week on top of the federal $300 LWA supplement, resulting in a net decline of $200 per week for UI recipients in these states; one state declined the LWA, which meant the decline in UI benefits was the full $600 FPUC supplement; and all other states accepted the LWA but did not provide an $100 extra, resulting in a net UI benefit decline of $300. However, not all states provides LWA supplements immediately after the expiration of

FPUC because it took time for the states to set up the program and payments were made in arrears.[7] For instance, the LWA fund had run out for some states by mid-September while other states did not start paying LWA supplement until late October.[8] Considering variation in duration and timing of LWA payment across the states, we calculate the average weekly UI benefit reduction and average post-FPUC UI benefit reduction (see Appendix table A7 for details). The average UI benefit reduction in our sample is \$563, \$471, \$369, \$421, \$477, \$554, \$577, in waves 13,14, 15, 16, 17, 18, 19 respectively, and \$600 for 20 and 21 waves used in our analysis. Using the average post-FPUC UI benefit reduction of \$515 the MPS out of UI benefits is 0.060 $\left(\frac{\$300\times0.103}{\$515}\right)$, 0.036 $\left(\frac{\$212\times0.087}{\$515}\right)$ and 0.023 $\left(\frac{\$88\times0.137}{\$515}\right)$ for all food, FAH, and FAFH, respectively. Our estimate of MPS on all food is in line with the literature average MPS of 0.05 out of cash income (Beatty and Tuttle 2015, p. 402).

Dube (2021) calculates that the loss of the entire \$600 supplement by mid-September reduced the median replacement rate from 146% to 48%. Using our estimated reduction in UI benefits in waves from 13 and 19, the mean replacement rate between the end of FPUC and November 23 is 6% $\left(\frac{54+70+87+78+68+56+52}{7}\right)$. This gives a mean reduction of 84 percentage points $\left(146-\frac{667+48\times2}{9}\right)$ in replacement rate during our post-FPUC sample period. Based on the coefficient estimates on $Post \times UI$ in panel C of table 1.3, a 10 percentage point reduction in replacement rate leads to a 1.2% $\left(\frac{0.103\times10}{84}\right)$, 1% $\left(\frac{0.087\times10}{84}\right)$ and 1.6% $\left(\frac{0.137\times10}{84}\right)$ reduction in total fsood, FAH and FAFH spending, respectively (see Appendix table A8 for details for weekly reduction in food spedning due to a 10% reduction in a replacement after the expiration of

[7] We Thank anonymous reviewer for pointing this out.
[8] Lost Wages Assistance (LWA) Tracker - UnemploymentPUA.com

FPUC) . Our estimate of 1.2% for total food is less than one-half Gruber's (1997, table 1, column 4) estimate of 2.8% for US workers during the 1968–1987 period but is close to Browning and Crossley's (2001) 0.8% for total expenditures per 10 percentage point decline in replacement rate in a sample of Canadian unemployed workers during the 1993–1995 period. Our estimate is also very close to the estimate of 1.1% for the 1999–2011 period from one of East and Kuka's specifications (2015, table 5, column 1). We should note that all previous estimates of the consumption smoothing effect of UI (except those of Browning) are intent-to-treat effects, which are expected to be smaller in magnitude than our estimates of treatment on the treated effect, all else equal.

Our heterogeneity analysis compares the estimated UI effect across respondent types. We find the effect is concentrated on UI recipients in state-waves with above-average UI claim rates, while the estimated UI effect in state-waves with below-average UI claim rates is insignificant. We use UI claim rates and state unemployment rate as indicators for the rapidly evolving economic condition during the pandemic as lockdown and stay-home orders, business closures and consumer spending at brick-and-mortar stores are likely strongly associated with the severity of the local COVID situation. Firstly, to examine the link between UI claims and COVID-19 cases, we regress the state initial UI claim rate on the state COVID case rate with state and wave fixed effects. We find that each additional confirmed COVID case per 100,000 population is associated with 0.007 (cluster-robust standard error = 0.0017) new UI claims per 100 people in the 2019 labor force. Secondly, we regress the state unemployment rate on the state COVID-19 case rate with state and wave fixed effects to find a relationship between unemployment and COVID-19 cases. Result shows that each additional COVID case per 100,000 population is linked with 0.009 (cluster-robust standard error = 0.005) state unemployment rate. These results that the consumption smoothing effect of UI varies strongly with the economic conditions is a significant finding. Kroft and Notowidigdo (2016) and East and Kuka (2015) both examine the degree to which the UI consumption smoothing effect varies over the business cycle but lack the statistical precision to draw a clear conclusion. In the case of Kroft and Notowidigdo (2016), the authors conclude that they can rule out a large effect of the business cycle on the magnitude of the UI effect. In our case, the estimated effects of UI on total food spending in high-UI-claim-rate states are 2.48 times the estimates in low-UI-claim-rate states and the difference is statistically significant. The increase in the economic and statistical significance of our estimates relative to previous estimates may be attributed to the increased statistical power afforded by the sizable PFUC supplemental benefit and the unprecedented havoc on the economy caused by the pandemic. We acknowledge some limitations of this paper. First, although the HPS is a nationally representative sample of US households, and covered several topics, it is considered an experimental dataset with very low response rates. Second, variables used in the study are self-reported, which may be subject to reporting bias.

7. CONCLUSION

In this article, we leverage the sharp decline in UI benefit level following the expiration of the FPUC to estimate the effect of UI on food spending and self-reported measures of food sufficiency. We show that the mean effect of the removal of the $600 supplemental UI benefits is to reduce total food spending by 10.3%, FAH spending by 8.7%, and FAFH spending by 13.7%. In terms of the subjective measures of food sufficiency and confidence about future food sufficiency, the food sufficiency and confidence declined by 0.151 and 0.111 standard deviations, respectively, following the FPUC expiration. We find heterogeneity in the UI effect by state economic conditions and along several dimensions of respondent demographics. The estimated effect is substantially higher for respondents in states experiencing high UI claim rates and, to a lesser extent, for low-income respondents. We interpret the former result as evidence for countercyclical effects of the consumption smoothing effect of UI. That is, the consumption smoothing benefit effect is larger during economic downturns. Meaningful differences in estimated effects by the presence of children, homeownership, race and ethnicity, and state (regular) maximum UI benefit amount are also observed, although the differences are not always precisely estimated. In summary, these results suggest that the UI and the FPUC supplement had their intended effects on the consumption of the unemployed population in 2020, during the height of the COVID-19 pandemic.

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Table 1.1. Definition of outcome and UI participation variables

Variable Name	Definition
Sufficiency	Ordinal response; 1-4. Based on the following question: “In the last 7 days, which of these statements best describes the food eaten in your household? Select only one answer. 1) Enough of the kinds of food (I/we) wanted to eat, 2) Enough, but not always the kinds of food (I/we) wanted to eat, 3) Sometimes not enough to eat, and 4) Often not enough to eat”. We recode variable labels so that higher values correspond to higher food sufficiency and redefine food sufficiency categories (Coleman-Jensen et al., 2022): Food sufficiency: Enough of the kinds of food (I/we) wanted to eat Marginal food sufficiency: Enough, but not always the kinds of food (I/we) wanted to eat, Low food sufficiency: Sometimes not enough to eat, and 4) Often not enough to eat Very low food sufficiency: Often not enough to eat
Confidence	Ordinal response: 1-4, a higher value denotes higher food sufficiency confidence in the future. Based on the following question: “How confident are you that your household will be able to afford the kinds of food you need for the next four weeks? Select only one answer. 1) Not at all confident, 2) Somewhat confident, 3) Moderately confident, 4) Very confident.”
FAH spending	Continuous variable. Based on the following 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? Enter amount.”
FAFH spending	Continuous variable. Based on the following question: “During the last 7 days, how much money did you or your household spend on prepared meals, including eating out, fast food, and carrying out or delivered meals? Please include money spent in cafeterias at work or at school, or on vending machines. Enter amount.”
Total food spending	Sum of FAH and FAFH spending
Unemployment Insurance	Indicator variable that takes the value 1 if the respondent reported “unemployment insurance benefit payments” to the following question: “Thinking about your experience in the last 7 days, which of the following did you or your household members use to meet your spending needs?Select all that apply.”

Table 1.2. Summary Statistics

Variable	Unmatched		Matched	
	UI recipients	Nonrecipients	UI recipients	Nonrecipients
FAFH spending ($)	88.316	89.827	88.316	90.728
	(2.249)	(2.211)	(2.249)	(2.316)
FAH spending ($)	211.910	209.452	211.910	210.199
	(2.871)	(3.024)	(2.871)	(3.189)
Sufficiency	0.850	0.855	0.850	0.853
	(0.003)	(0.004)	(0.003)	(0.004)
Confidence	0.501	0.572	0.501	0.568
	(0.007)	(0.009)	(0.007)	(0.010)
Age	43.937	44.441	43.937	44.285
	(0.180)	(0.170)	(0.180)	(0.155)
Age squared	2078.513	2128.897	2078.513	2113.086
	(15.696)	(14.704)	(15.696)	(13.488)
Female (1/0)	0.653	0.648	0.653	0.649
	(0.005)	(0.004)	(0.005)	(0.004)
Married (1/0)	0.419	0.462	0.419	0.447
	(0.007)	(0.007)	(0.007)	(0.006)
Household size	3.020	3.122	3.020	3.079
	(0.022)	(0.026)	(0.022)	(0.025)
Presence of children (1/0)	0.415	0.433	0.415	0.425
	(0.008)	(0.005)	(0.008)	(0.005)
Stimulus payment (1/0)	0.451	0.437	0.451	0.439
	(0.009)	(0.007)	(0.009)	(0.007)
Own home (1/0)	0.508	0.580	0.508	0.557
	(0.018)	(0.013)	(0.018)	(0.014)
Hispanic (1/0)	0.139	0.140	0.139	0.139
	(0.022)	(0.021)	(0.022)	(0.022)
Non-Hispanic White (1/0)	0.738	0.776	0.738	0.764
	(0.019)	(0.016)	(0.019)	(0.016)
Non-Hispanic Black (1/0)	0.128	0.110	0.128	0.117
	(0.016)	(0.013)	(0.016)	(0.013)
Non-Hispanic Asian (1/0)	0.133	0.114	0.133	0.120
	0.030	0.037	(0.019)	(0.014)
Less than high school (1/0)	(0.001)	(0.002)	0.030	0.034
	0.166	0.162	(0.001)	(0.003)
High School Degree (1/0)	(0.006)	(0.004)	0.166	0.165
	0.442	0.418	(0.006)	(0.004)
Some college degree (1/0)	(0.006)	(0.005)	0.442	0.427
	0.362	0.383	(0.006)	(0.005)
Undergraduate and above (1/0)	(0.010)	(0.006)	0.362	0.374

	0.197	0.200	(0.010)	(0.007)
Household income <$25,000 (1/0)	(0.006)	(0.005)	0.197	0.198
	0.349	0.322s	(0.006)	(0.005)
Household income <$50,000 (1/0)	(0.003)	(0.003)	0.349	0.331
	0.454	0.478	(0.003)	(0.003)
Household income $50,000-	(0.007)	(0.006)	0.454	0.470
$99,999 (1/0)	0.454	0.478	(0.007)	(0.007)
New COVID-19 cases rate [a]	2.068	2.397	2.068	2.275
	(0.110)	(0.119)	(0.110)	(0.113)
New COVID-19 death rate [b]	0.266	0.292	0.266	0.283
	(0.015)	(0.016)	(0.015)	(0.015)
Initial UI claims rate [c]	0.011	0.010	0.011	0.010
	(0.001)	(0.001)	(0.001)	(0.001)
Continued UI claims rate [d]	0.164	0.140	0.164	0.148
	(0.017)	(0.014)	(0.017)	(0.014)
Time spent at retail and recreation	-0.149	-0.130	-0.149	-0.136
locations [e]	(0.018)	(0.014)	(0.018)	(0.015)
Time spent at grocery and	-0.027	-0.013	-0.027	-0.018
pharmacy locations [e]	(0.011)	(0.011)	(0.011)	(0.010)
Time spent outside of residential	-0.097	-0.090	-0.097	-0.092
locations [e]	(0.006)	(0.005)	(0.006)	(0.005)
N	70042	176009	70042	139332

Notes: The reported values are sample means and their standard errors in parentheses are clustered at the state level. Observations are weighted by sampling weights. State-level COVID, UI claim rates, and GPS mobility data are from Chetty et al. (2020) and the Economic Tracker at https://tracktherecovery.org.

[a]New confirmed COVID-19 cases per 100,000 people, state-level seven-day moving average.

[b]New confirmed COVID-19 deaths per 100,000 people, state-level seven-day moving average.

[c]State-level number of initial claims per 100 people in the 2019 labor force, combining Regular and Pandemic Unemployment Assistance claims

[d]State-level number of continued claims per 100 people in 2019 labor force, combining Regular, Pandemic Unemployment Assistance and Pandemic Emergency Unemployment Compensation claims

e GPS mobility data indexed to January 3-February 6, 2020.

Table 1.3: The effect of UI benefit reduction on food spending and food hardship

Specifications	Total food spending	FAH spending	FAFH spending	Sufficiency	Confidence
	OLS			Ordered logit	
	(1)	(2)	(3)	(4)	(5)
Panel A: Base model					
UI	0.076^{***}	0.085^{***}	-0.000	0.145^{***}	-0.015
	(0.023)	(0.023)	(0.052)	(0.039)	(0.049)
Post × UI	-0.069^{**}	-0.057^{**}	-0.095	-0.318^{***}	-0.256^{***}
	(0.027)	(0.028)	(0.061)	(0.049)	(0.042)
R squared	0.015	0.011	0.013	0.012	0.013
Panel B: Demographics included					
UI	0.103^{***}	0.110^{***}	0.037	0.118^{***}	-0.064^{*}
	(0.021)	(0.019)	(0.050)	(0.036)	(0.039)
Post × UI	-0.103^{***}	-0.087^{***}	-0.137^{**}	-0.283^{***}	-0.222^{***}
	(0.025)	(0.026)	(0.058)	(0.052)	(0.045)
R squared	0.100	0.084	0.052	0.052	0.051
Panel C: Demographics, COVID cases, and unemployment insurance claim rates included					
UI	0.103^{***}	0.111^{***}	0.036	0.119^{***}	-0.063
	(0.021)	(0.019)	(0.050)	(0.036)	(0.040)
Post × UI	-0.103^{***}	-0.087^{***}	-0.137^{**}	-0.286^{***}	-0.225^{***}
	(0.025)	(0.026)	(0.058)	(0.052)	(0.047)
R squared	0.100	0.084	0.052	0.053	0.054
N	209374	209374	209374	209040	209326

Note: Inverse hyperbolic sine transformation of total food, FAH, and FAFH spending applied. Panel A, B, and C present the estimation results of equation (1) with increasing number of control variables. Panel A includes state and wave fixed effects. Panel B adds respondent demographics. Panel C adds GPS mobility data, COVID case and death rates and UI claim rates for the state in which the respondent resides. Standard errors in parentheses are clustered at the state level. The sampling weight is used in estimation. *, **, and *** denote significance at 10%, 5% and 1% level, respectively.

Table 1.4. Average marginal effects for types of food sufficiency and confidence from the ordered logit

Specifications	Sufficiency				Confidence			
	Very low food sufficiency	Low food sufficiency	Marginal food sufficiency	High food sufficiency	Not at all confident	Somewhat confident	Moderately confident	Very confident
Panel A: Base model								
UI	-0.007***	-0.019***	-0.007***	0.033***	0.002	0.001	-0.001	-0.002
	(0.002)	(0.005)	(0.002)	(0.009)	(0.007)	(0.004)	(0.004)	(0.008)
Post × UI	0.015***	0.041***	0.016***	-0.072***	0.039***	0.023***	-0.022***	-0.040***
	(0.002)	(0.006)	(0.002)	(0.011)	(0.006)	(0.004)	(0.004)	(0.007)
Panel B: Demographics included								
UI	-0.006***	-0.014***	-0.005***	0.025***	0.009	0.005**	-0.005**	-0.009**
	(0.002)	(0.004)	(0.002)	(0.007)	(0.006)	(0.003)	(0.003)	(0.006)
Post × UI	0.013***	0.033***	0.012***	-0.058***	0.032***	0.017***	-0.016***	-0.033***
	(0.002)	(0.006)	(0.002)	(0.011)	(0.006)	(0.004)	(0.003)	(0.007)
Panel C: Demographics, COVID cases, and unemployment insurance claim rates included								
UI	-0.006	-0.014***	-0.005***	0.025	0.009	0.005	-0.005	-0.009
	(0.002)	(0.004)	(0.002)	(0.007)	(0.006)	(0.003)	(0.003)	(0.006)
Post × UI	0.013***	0.034***	0.012***	-0.059***	0.032***	0.018***	-0.017***	-0.033***
	(0.002)	(0.006)	(0.002)	(0.011)	(0.007)	(0.004)	(0.004)	(0.007)

Note: Standard errors in parentheses are computed from the Delta method.

Table 1.5. Placebo FPUC expiration date on food spending and food hardship

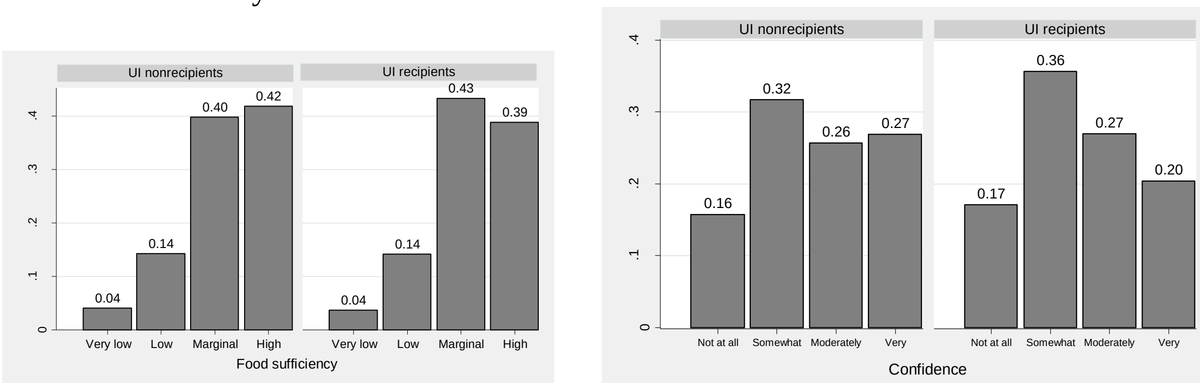
Variable	Total food spending	FAH spending	FAFH spending	Sufficiency	Confidence
	OLS			Ordered logit	
	(1)	(2)	(3)	(4)	(5)
UI	0.104***	0.108***	-0.024	0.142***	-0.037
	(0.034)	(0.036)	(0.049)	(0.033)	(0.048)
Post × UI	-0.104***	-0.084**	-0.076	-0.309***	-0.247***
	(0.036)	(0.036)	(0.059)	(0.051)	(0.051)
Placebo post ×UI	-0.003	0.005	0.117	-0.031	-0.056
	(0.044)	(0.049)	(0.082)	(0.057)	(0.050)
R-squared	0.100	0.084	0.052	0.051	0.052
N	209374	209374	209374	209040	209326

Note: Inverse hyperbolic sine transformation of total food, FAH, and FAFH spending applied. Each regression includes state and wave fixed effects, respondent demographics, GPS mobility data, and COVID case and death rates and UI claim rates for the state in which the respondent resides. Standard errors in parentheses are clustered at the state level. The sampling weight is used in estimation. *, **, and *** denote significance at 10%, 5% and 1% level, respectively.

Figure 1.1. Distribution of food sufficiency and confidence about future food sufficiency

Panel A. Sufficiency

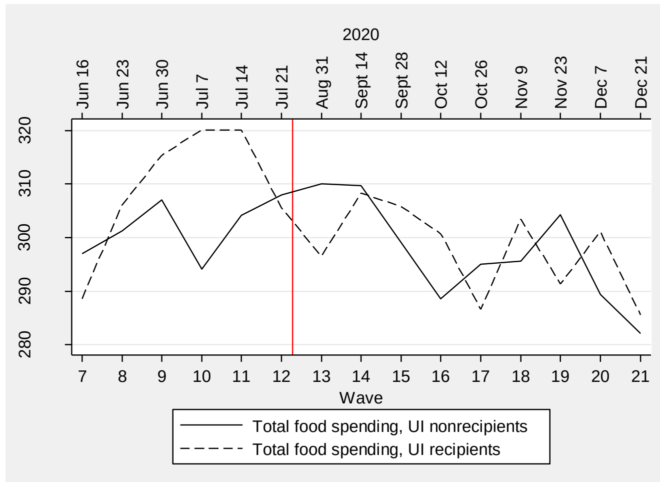
Panel B. Confidence



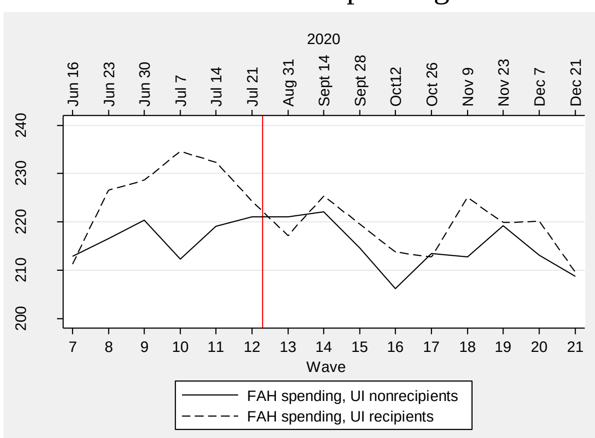
Note: Sufficiency and confidence are recoded so that a higher number represents higher food sufficiency and confidence about future food sufficiency, respectively. See table 1 for definitions of the sufficiency and confidence variables.

Figure 1.2. Raw trends of food spending by UI receipt status

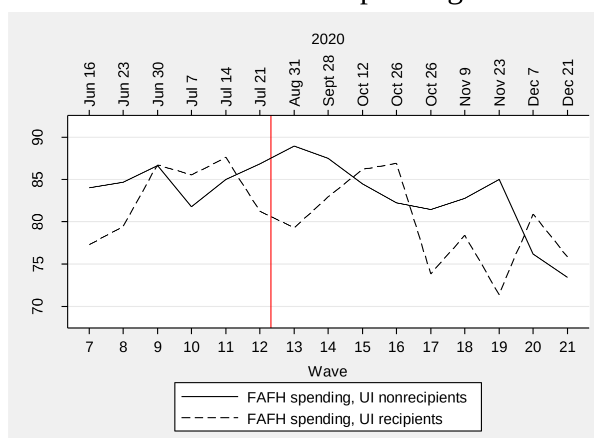
Panel A: Total food spending



Panel B: FAH spending

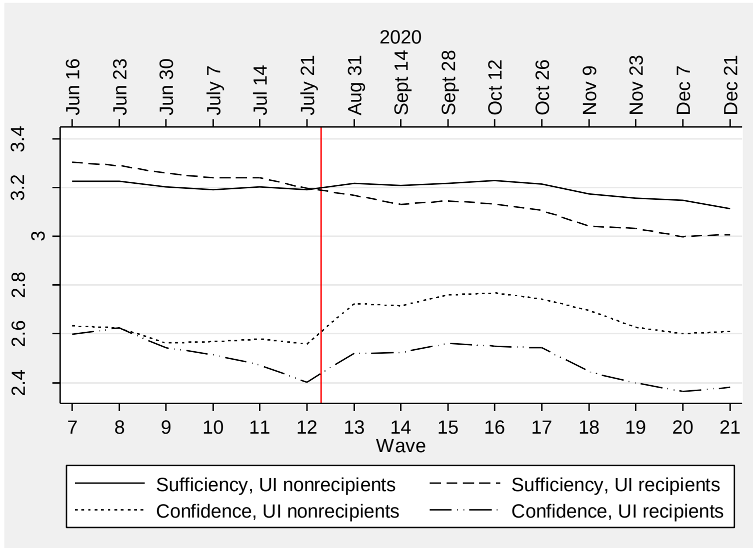


Panel C: FAFH spending



Note: Top x-axis label represents the survey wave ending date.

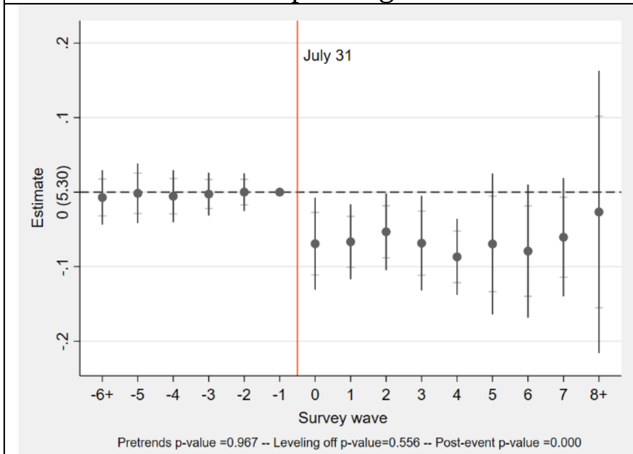
Figure 1.3. Raw trends of food sufficiency and confidence about future food sufficiency by UI receipt status



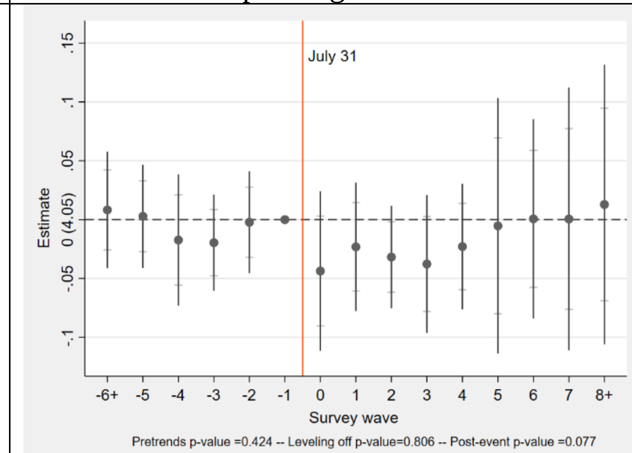
Note: Top x-axis label represents survey wave ending date.

Figure 1.4. Event studies of common pre-trends between UI recipients and nonrecipients

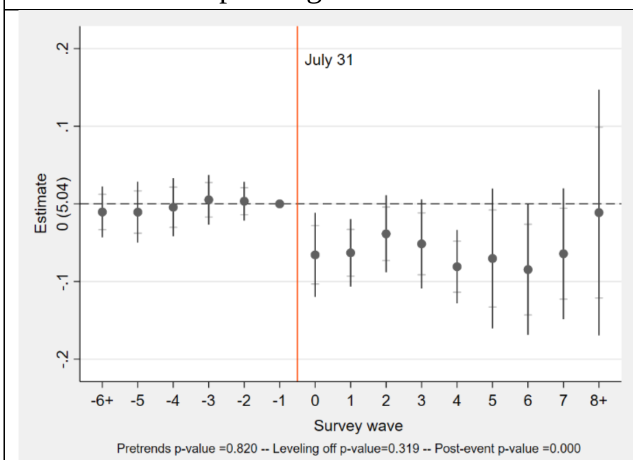
Panel A. Total food spending



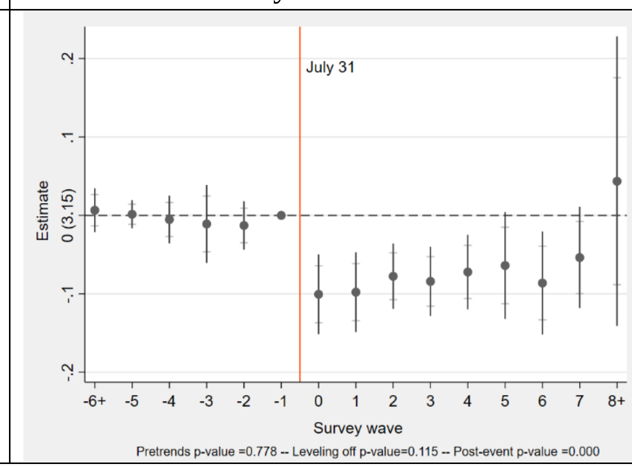
Panel B. FAFH spending



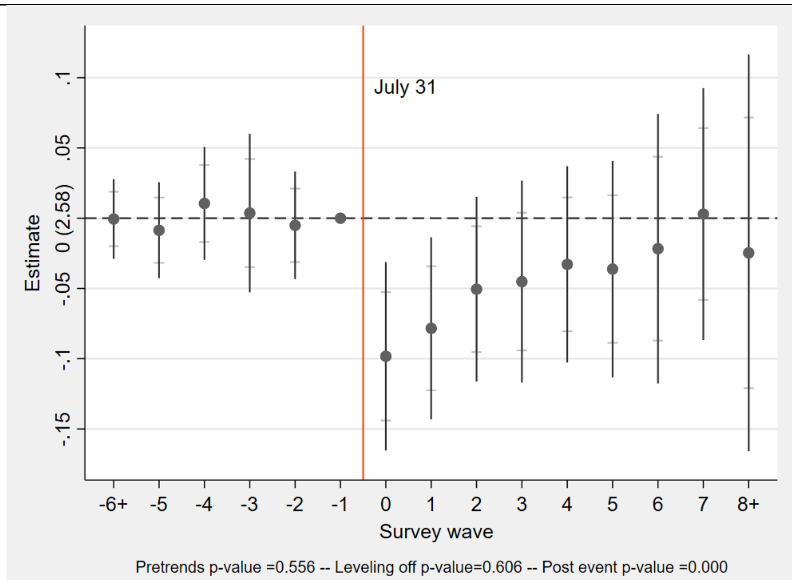
Panel C. FAH spending



Panel D. Sufficiency

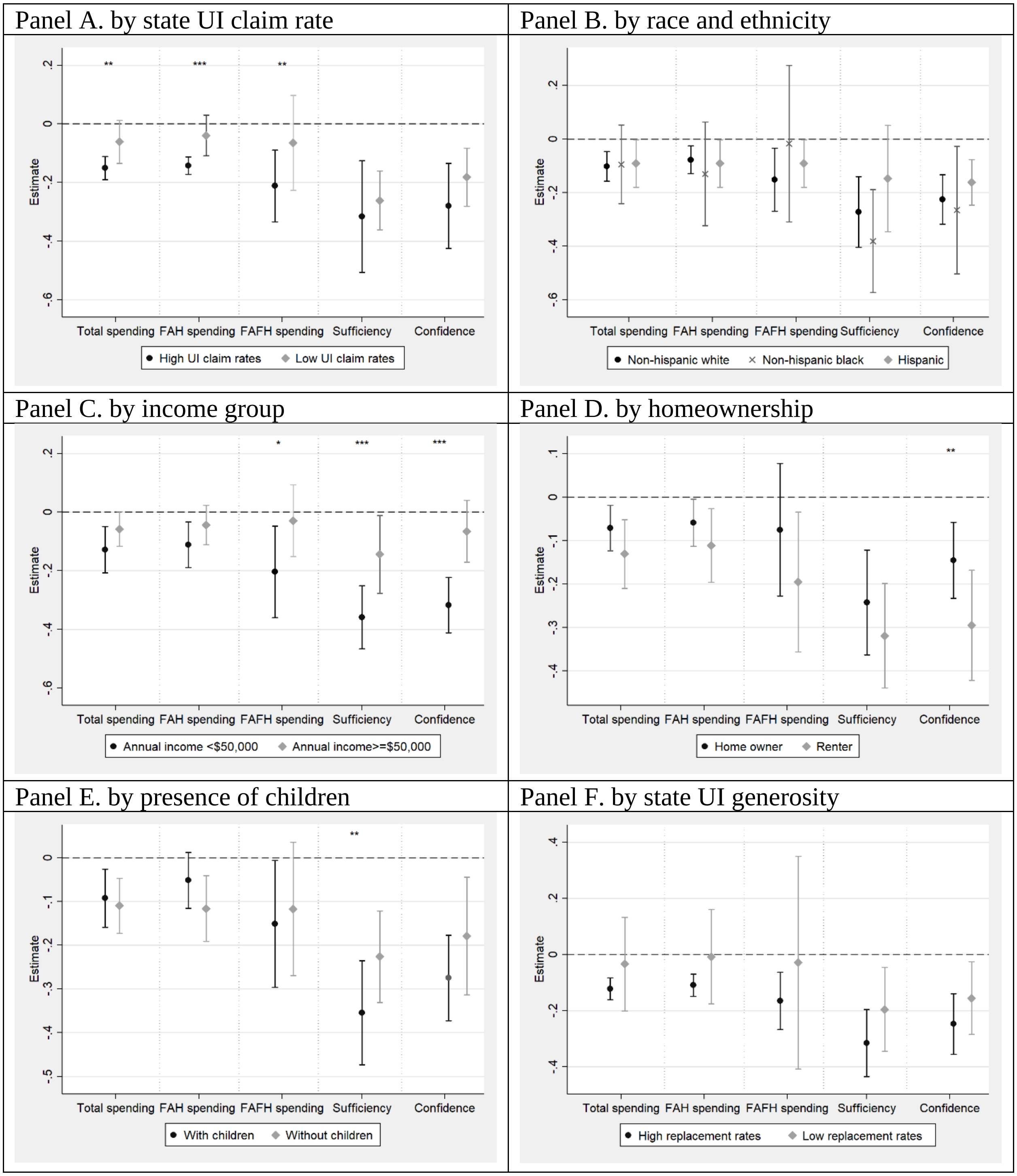


Panel E. Confidence



Each plot shows the elements of the estimated event-time path (survey waves) of the outcome, its 95 percent confidence interval, and a uniform sup-t confidence band for the individual elements of the event-time path on the y-axis, against survey waves, on the x-axis. The inner bars illustrate pointwise confidence intervals, and the uniform, sup-t confidence bands are given by the outer lines. Parenthetical labels next to the point 0 on the y axis represents the average value of the outcome corresponding to the normalized coefficient ($\tau = -1$). P-values for testing for the absence of pre-trend, joint significance of θ_τ and p-values for testing the joint significance of coefficeints γ_τ post July 31 are also added to each plot.

Figure 1.5. The heterogeneity effect of UI by respondent type



Note: Each bar illustrates the $\hat{\beta}_2$ and its 95-percentage confidence interval. When a *, **, or *** is on top of a pair of bars, the difference in $\hat{\beta}_2$ is statistically significant between two types of respondents at the 10%, 5%, or 1% level, respectively.

APPENDICES

Appendix A1**.** Data collection period of the Household Pulse Survey

Household Pulse Survey wave week no.	Start date	End date
7	June 11, 2020	June 16, 2020
8	June 18, 2020	June 23, 2020
9	June 25, 2020	June 30, 2020
10	July 2, 2020	July 7, 2020
11	July 9, 2020	July 14, 2020
12	July 16, 2020	July 21, 2020
13	August 19, 2020	August 31, 2020
14	September 2, 2020	September 14, 2020
15	September 16, 2020	September 28, 2020
16	September 30, 2020	October 12, 2020
17	October 14, 2020	October 26, 2020
18	October 28, 2020	November 9, 2020
19	November 11, 2020	November 23, 2020
20	November 25, 2020	December 7, 2020
21	December 9, 2020	December 21, 2020

Appendix A2. The propensity score matching logistic regression

Variable	UI
	logit
Age	0.021***
	(0.003)
Age squared	0.000***
	(0.000)
Female (1/0)	0.008
	(0.010)
Married (1/0)	-0.021
	(0.011)
Household size	-0.030***
	(0.004)
Presence of children (1/0)	-0.023*
	(0.013)
SNAP household (1/0)	0.334***
	(0.017)
Stimulus payment (1/0)	0.441***
	(0.035)
Own home (1/0)	-0.117***
	(0.010)
Hispanic (1/0)	-0.094***
	(0.015)
Non-Hispanic White (1/0)	0.064***
	(0.015)
Non-Hispanic Asian (1/0)	0.031**
	(0.015)
High School Degree (1/0)	0.175***
	(0.029)
Some college degree (1/0)	0.195***
	(0.028)
Undergraduate and above (1/0)	0.099***
	(0.028)
Household income <$50,000 (1/0)	0.117***
	(0.014)
Household income $50,000-$99,000 (1/0)	0.076***
	(0.014)
Observations	246051
Pseudo R-squared	0.089

Note: The dependent variable, UI, is equal to one for unemployment insurance recipients and zero otherwise. Cluster-robust standard errors in parentheses. *Significant at the 10% level; **Significant at the 5% level; ***Significant at the 1% level.

Appendix A3. Full model: Unemployment benefit cuts and food spending, and food hardship

Variable	Total food (HST $)	FAH (HST $)	FAFH (HST $)	Food sufficiency	Confidence
	OLS			Ordered logit	
	(1)	(2)	(3)	(4)	(5)
UI	0.103***	0.111***	0.036	0.119***	-0.063
	(0.021)	(0.019)	(0.050)	(0.036)	(0.040)
UI×POST	-0.103***	-0.087***	-0.137**	-0.286***	-0.225***
	(0.025)	(0.026)	(0.058)	(0.052)	(0.047)
Age	0.006*	0.009**	-0.021***	-0.104***	-0.140***
	(0.003)	(0.004)	(0.004)	(0.007)	(0.009)
Age squared	-0.000***	-0.000***	0.000	0.001***	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Female (1/0)	-0.051***	-0.028***	-0.181***	0.019	-0.010
	(0.008)	(0.009)	(0.024)	(0.015)	(0.021)
Married (1/0)	0.090***	0.112***	0.008	0.164***	0.077***
	(0.011)	(0.013)	(0.030)	(0.021)	(0.019)
Household size	0.091***	0.096***	0.069***	-0.078***	-0.053***
	(0.007)	(0.008)	(0.009)	(0.007)	(0.006)
Presence of children	0.131***	0.166***	0.040	-0.028	0.045
(1/0)	(0.015)	(0.018)	(0.031)	(0.033)	(0.028)
SNAP household (1/0)	0.213***	0.292***	-0.220***	-0.179***	-0.001
	(0.020)	(0.028)	(0.038)	(0.028)	(0.033)
Stimulus payment	0.123***	0.154***	-0.025	0.120*	0.073
(1/0)	(0.039)	(0.038)	(0.064)	(0.063)	(0.077)
Own home (1/0)	0.044***	0.062***	0.084**	0.382***	0.391***
	(0.010)	(0.010)	(0.036)	(0.015)	(0.024)
Hispanic (1/0)	0.237***	0.207***	0.457***	-0.048*	-0.235***
	(0.025)	(0.026)	(0.041)	(0.029)	(0.023)
Non-Hispanic White	0.168***	0.061**	0.543***	-0.156***	-0.288***
(1/0)	(0.027)	(0.030)	(0.042)	(0.047)	(0.042)
Non-Hispanic Asian	0.119***	0.059***	0.339***	-0.066**	-0.173***
(1/0)	(0.018)	(0.016)	(0.041)	(0.026)	(0.018)
High School Degree	-0.143***	-0.133***	-0.194**	0.128***	0.157***
(1/0)	(0.046)	(0.046)	(0.077)	(0.033)	(0.037)
Some college degree	-0.200***	-0.176***	-0.316***	0.243***	0.304***
(1/0)	(0.052)	(0.053)	(0.091)	(0.038)	(0.036)
Undergraduate and	-0.200***	-0.172***	-0.275***	0.695***	0.817***
above (1/0)	(0.054)	(0.051)	(0.099)	(0.044)	(0.038)
Household income	0.086***	0.083***	0.161***	0.370***	0.284***
<$50,000 (1/0)	(0.020)	(0.021)	(0.031)	(0.035)	(0.025)
Household income	0.230***	0.213***	0.468***	0.867***	0.856***
$50,000-$99,000 (1/0)	(0.029)	(0.024)	(0.056)	(0.034)	(0.030)

New Covid-19 cases rate	0.005	0.009*	-0.001	0.017**	0.010
	(0.004)	(0.005)	(0.010)	(0.008)	(0.008)
New COVID-19 death rate	0.002	-0.010	0.083	-0.017	0.021
	(0.023)	(0.025)	(0.075)	(0.058)	(0.058)
Initial UI claims rate	1.242	2.413*	1.450	0.723	-3.025*
	(1.093)	(1.288)	(2.076)	(1.281)	(1.802)
Continued UI claims rate	-0.075	-0.129	-0.048	0.432***	0.721**
	(0.118)	(0.110)	(0.344)	(0.103)	(0.332)
Time spent at retail and recreation locations [e]	-0.144	-0.031	-0.150	0.511	-0.044
	(0.241)	(0.236)	(0.547)	(0.399)	(0.511)
Time spent at grocery and pharmacy locations [e]	0.471	0.272	1.322**	0.167	0.760
	(0.296)	(0.309)	(0.585)	(0.549)	(0.551)
Time spent outside of residential locations [e]	-0.339	-0.461	0.264	0.367	1.400
	(0.746)	(0.754)	(1.060)	(0.857)	(1.008)
Constant	5.363***	4.804***	4.164***		
	(0.153)	(0.166)	(0.124)		
cut1 Constant				-4.375***	-3.635***
				(0.170)	(0.147)
cut2 Constant				-2.552***	-1.819***
				(0.163)	(0.149)
cut3 Constant				-0.525***	-0.504***
				(0.168)	(0.157)
R-squared	0.100	0.084	0.052	0.053	0.054
Observations	209374	209374	209374	209040	209326

Note: Cluster-robust standard errors in parentheses. *Significant at the 10% level; **Significant at the 5% level; ***Significant at the 1% level. HST represent inverse hyperbolic sine transformation. The cut1, cut2 and cut3 constants are ancillary parameters in ordered logit models (https://www.stata.com/support/faqs/statistics/cut-points/).

Appendix A4. Effects of UI benefit cuts on food spending and food hardship using logarithm transformation for food spending and binary indicators for sufficiency and confidence

Variable	Total food spending (log $)	FAH spending (log $)	FAFH spending (log $)	Sufficiency	Confidence	Sufficiency	Confidence
	OLS			Binary logit (average marginal effects)		Probit OLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
UI	0.097***	0.105***	0.031	0.044***	-0.015	0.069***	-0.031
	(0.020)	(0.018)	(0.045)	(0.006)	(0.012)	(0.017)	(0.021)
UI×POST	-0.100***	-0.086***	-0.124**	-0.049***	-0.05***	-0.151***	-0.111***
	(0.024)	(0.024)	(0.052)	(0.007)	(0.015)	(0.025)	(0.024)
R-squared	0.108	0.093	0.055	0.092	0.072	0.113	0.134
Observations	209374	209374	209374	209374	209374	209040	209326

Note: We add $1 to zero FAH, FAFH and total expenditures so that we can take the logarithm of these observations. Columns 4 and 5 of Appendix table A4 show estimated coefficients from the binary logit model. We code the food sufficiency as a binary variable which takes a value 1 if respondents report having enough of the kind of food they wanted to eat or enough but not always the kinds of food they wanted to eat, and otherwise zero (Coleman-Jensen et al., 2022). Likewise, confidence takes on value 1 if respondents are either very confident or moderately confident on food sufficiency confidence in the next four weeks of the survey. Columns 6 and 7 present coefficients from the ordered logit. We observe similar (to the specifications in the main text) effects in terms of magnitude and statistical significance on our variables of interest.

Appendix A5. Effects of UI benefit cuts on food spending and food hardship using the restricted sample without matching

Variable	Total food spending (HST $)	FAH spending (HST $)	FAFH spending (HST $)	Sufficiency	Confidence
	OLS			Ordered logit	
	(1)	(2)	(3)	(4)	(5)
UI	0.098***	0.107***	0.025	0.126***	-0.065*
	(0.021)	(0.020)	(0.049)	(0.037)	(0.038)
UI×POST	-0.098***	-0.082***	-0.126**	-0.293***	-0.218***
	(0.025)	(0.025)	(0.058)	(0.054)	(0.041)
R-squared	0.101	0.085	0.051	0.053	0.054
Observations	246051	246051	246051	245641	246000

Note: Cluster-robust standard errors in parentheses. *Significant at the 10% level; **Significant at the 5% level; ***Significant at the 1% level. HST represents inverse hyperbolic sine transformation.

Appendix A6. Effects of UI benefit cuts on food spending and food hardship using the full sample without matching

Variable	Total food spending (HST $)	FAH spending (HST $)	FAFH spending (HST $)	Sufficiency	Confidence
	OLS			Ordered logit	
	(1)	(2)	(3)	(4)	(5)
UI	0.419***	0.068***	-0.112***	-0.360***	-0.517***
	(0.030)	(0.011)	(0.031)	(0.023)	(0.032)
UI×POST	-0.149***	-0.047***	-0.087***	-0.274***	-0.322***
	(0.023)	(0.015)	(0.022)	(0.020)	(0.017)
R-squared	0.337	0.081	0.072	0.103	0.127
Observations	1325149	1150576	1140769	1245154	1183458

Note: Cluster-robust standard errors in parentheses. *Significant at the 10% level; **Significant at the 5% level; ***Significant at the 1% level. HST represents inverse hyperbolic sine transformation.

Appendix A7: Average UI benefit reduction and MPS out of UI

Household Pulse Survey wave week no.	# of states providing LWA	Total addition state benefit	Total LWA benefits in all 50 States and DC	Average LWA benefit after the expiration of FPUC	Average benefit reduction out of $600	MPS out of UI benefits		
						Total food	FAH	FAFH
13	6	$100	$1900	37	$563	0.055	0.033	0.021
14	22	$200	$6800	133	$467	0.066	0.039	0.026
15	39	$400	$12100	237	$363	0.085	0.051	0.033
16	30	$300	$9300	182	$418	0.074	0.044	0.029
17	21	$100	$6400	125	$475	0.065	0.039	0.025
18	8	0	$2400	47	$553	0.056	0.033	0.022
19	4	0	$1200	23	$577	0.054	0.032	0.021
20	0	0	0	0	$600	0.052	0.031	0.020
21	0	0	0	0	$600	0.052	0.031	0.020
Post-FPUC expiration					$513	0.060	0.036	0.023

Appendix A8: Average reduction in replacement and food spending in a 10% decrease in the replacement rate

Household Pulse Survey wave week no.	Average benefit reduction out of $600	Average Replacement rate (%)	Reduction in the replacement rate (%)	Reduction in food spending due to a 10% decrease in the replacement rate		
				Total	FAH	FAFH
13	$563	54	92	0.011	0.009	0.015
14	$467	70	76	0.014	0.011	0.018
15	$363	87	59	0.017	0.015	0.023
16	$418	78	68	0.015	0.013	0.020
17	$475	68	78	0.013	0.011	0.018
18	$553	56	90	0.011	0.010	0.015
19	$577	53	93	0.011	0.009	0.015
20	$600	48	98	0.011	0.009	0.014
21	$600	48	98	0.011	0.009	0.014
Post-FPUC	$514	62	84	0.012	0.010	0.016

CHAPTER 2

HEALTH EFFECTS OF UNEMPLOYMENT: EVIDENCE FROM FIRM CLOSURES DURING THE COVID-19 PANDEMIC

1. INTRODUCTION

The objective of this study is to leverage the spike in unemployment caused by the COVID-19 pandemic as a natural experiment to estimate the causal effect of job loss on general and mental health in the United States. The ongoing COVID-19 pandemic poses several global challenges, one of which is the issue of mass unemployment. The effects of the pandemic on employment were apparent from its onset, with more than 3.3 million workers in the United States filing for unemployment benefits in the week ending on March 21, 2020, which subsequently ballooned to 26 million claims within the next five weeks (Bureau of Labor Statistics, 2020). While the current figures for both the unemployment rate and the number of unemployed persons are significantly lower than the unprecedented highs recorded in April 2020 (5.4 percent and 8.7 million, respectively, for July 2021, compared to 14.8 percent and 23.1 million in April 2020), they still remain well above pre-pandemic levels (3.5 percent and 5.7 million, respectively, in February 2020). Moreover, the number of permanently unemployed persons remains alarmingly high. There were 2.9 million permanently unemployed persons in July 2021, which is 1.6 million more than the same figure from February 2020. Other statistics detailing the employment situation during the pandemic provide further cause for concern—compared to February 2020, the number of long-term unemployed (those jobless for 27 weeks or more) in July 2021 is up by

2.3 million, the employment-population ratio is down by 2.7 percentage points, and the number of persons not in the labor force who currently want a job is up by 1.5 million. Together, these figures characterize the devastating impacts of the COVID-19 pandemic on employment.

The pandemic also significantly impacted general and mental health, as documented by multiple studies reporting increased psychological distress, anxiety, depression, insomnia, and suicidal ideation (Holmes et al., 2020; Ran et al., 2020). For instance, during the COVID-19 pandemic, about 40 percent of the US population reported symptoms of depression and anxiety compared to 11 percent in early 2019 (Centers for Disease Control and Prevention, 2020). The pandemic-induced mental health problems could have resulted from multiple factors such as fear of contracting coronavirus, enormous death tolls, hospitalization, and public health interventions to slow the spread of the virus (stay-at-home order, lockdown, business closure), foreclosure, and massive unemployment. Although there is a large volume of descriptive research on the public health effects of the pandemic (Holingue et al., 2020; Berkowitz and Basu, 2021; Lee et al., 2021; Bierman et al., 2021), there has only been little research on the causal effect of pandemic-related unemployment on health.

While the effects of job loss on health outcomes was an active research area in the economics and public health literature even prior to the COVID-19 pandemic, studies on the connection between unemployment and health outcomes face several challenges, the most serious of which is the issue of endogeneity bias. The literature identifies three possible pathways through which poor health could be correlated with but not caused by unemployment. First, poor health may lead to job loss. This reverse causality is plausible given the finding presented by many studies that the likelihood of being unemployed is higher for those in poor health (see, for instance, Garcia-Gomez et al., 2010). Second, individuals in poor health are prone to longer spells of unemployment (Stewart, 2001), which increases the chance of observing them in the unemployed sample. This sample-selection bias is more likely to occur in cross-sectional and short panel data compared to panel data spanning longer period. Finally, it could be the case that unobserved (by the econometrician) factors cause both unemployment and poor health. For example, unobserved preference heterogeneity such as high time discount rate or hyperbolic discounting can cause risky health behaviors that may result in job loss and poor health. In sum, endogeneity, whether it is a result of reverse causality, sample selection or omitted variables, is likely to overestimate the effect of unemployment on health (Stewart, 2001).

Previous studies have used plant/firm closures (Eliason and Storrie, 2009; Schmitz, 2011), matching (Browning et al., 2006), instrumental variables (Caroli and Goddard, 2016), and individual fixed effects (Bockerman and Ilmakunnas, 2008) to control for endogeneity. Our identification strategy is closest to Schmitz (2011), who addresses reverse causality by exploiting plant closures as exogenous entries into unemployment. He uses data from the 1991-2008 German Socio-Economic Panel to find that the unemployed were less healthy than the employed. However, the study concludes that the worse health among the unemployed may only be a selection effect into unemployment and argues against a causal effect of employment status on health (Schmitz 2011). In contrast, a more recent study by Schaller and Stevens (2015) based on the US Medical Expenditure Panel Survey (MEPS) between 1996 and 2012 concludes that job loss negatively impacts self-reported general health status and mental health. Some studies use the variation in macroeconomic conditions, which can be argued to be reasonably exogenous to an individual’s labor market decisions, to identify the effect of unemployment-related economic hardship on health, and the evidence is mixed (Green 2011; Salm 2009; Ruhm, 2005). It is not clear to what extent the mixed findings can be attributed to differences in the measurement (e.g., individual versus regional-level unemployment) and classification (e.g., fired, plant closure) of unemployment, sampling variation, and study design, compared to true heterogeneity in effect. What is clear is that the lack of consensus concerning the health impact of unemployment warrants further research.

To add to the evidence base, we estimate the effect of involuntary unemployment brought about by the COVID 19 pandemic, a plausibly exogenous source of variation, on self-reported general health, mental health, and mental health utilization. The Household Pulse Survey (HPS) provides respondent-level data on unemployment and health. Using respondent-level unemployment data directly is preferred to making inferences based on the relationship between macroeconomic fluctuations and individual health, provided that the endogeneity of an individual's labor supply is controlled for. During an economic downturn, not all workers lose their jobs and experience a negative income shock. Extrapolating the health effect of macroeconomic conditions to that of unemployment likely underestimates the true effect of unemployment. Similar to Schmitz's (2011) use of plant closure as an exogenous variation in labor demand to identify the effect of unemployment on health, our research design leverages COVID-19 shutdowns to identify the impact of unemployment on health.

For identification, we take advantage of the fact that the HPS collects information on the reason for unemployment, which is not commonly collected in surveys. It allows us to identify whether an individual was laid off because his/her firm shut down due to the pandemic, if they quit voluntarily, or were fired for other reasons. Our variable of interest is an indicator that takes the value of one if an individual reported being out of work due to shutdowns related to COVID-19 and zero otherwise. For the purpose of identifying the effect of unemployment on health, this indicator does not count those who quit or were fired from their jobs as being unemployed. We estimate the effects of involuntary job loss on self-reported health status, mental health outcomes including anxiety and depression, and mental healthcare utilization measured by the use of prescription medication and professional services to treat mental health issues.

One of the potential threats to the identification is that our variable of interest may not be entirely random. That is, unobserved factors affecting unemployment and outcome variables might play a role, leading to biased estimation. We address this concern of endogeneity of involuntary job loss due to the pandemic by applying the Bartik instrumental variable (IV) approach proposed by Bartik (1991). Building on the shift in share strategy, we develop the exogenous shock as instrument variables by multiplying aggregate "shifts" with local industry shares at the state level. Specifically, we instrument the unemployment by combining changes in employment across different industries defined by the two-digit North American Industry Classification System (NAICS) code at the state level (individual's state of residence) "shifts" with state-level industry composition "shares". The baseline employment shares only affect (1) the mental health through unemployment due to job loss after controlling for the covariates and state and month fixed effects, and (2) baseline state-survey wave employment share is conditionally orthogonal to population mental health. We assume that this exogenous shock varies across the industries to isolate the local labor demand shocks and captures exogenous job losses due to the pandemic.

To examine the potential heterogeneous effects between sociodemographic subgroups, we also estimate the effects of involuntary employment separately for subsamples differentiated by household income, gender, health insurance status, accessibility to mental health professionals, and state Medicaid policy. Our results support the hypotheses that unemployment affects individual health and well-being, as well as healthcare utilization. We find that being unemployed can lead to a decline in perceived health status and an increase in the probability of experiencing anxiety or depression. Unemployed persons are also significantly more likely to use mental health prescription medication or professional therapy services than the employed. We argue that these effects are not entirely attributable to the decline in income associated with job loss and there may be psychological factors at play.

The remainder of this article proceeds as follows. Section II describes the data. Section III discusses our estimation strategy. Next, we present the results in Section IV, followed by a brief discussion in Section V. Finally, Section VI concludes.

2. DATA SOURCE

We conduct the analyses using the HPS public use microdata. The HPS survey is an online, nationally representative survey conducted by the US Census Bureau to understand the impacts of the COVID-19 pandemic on American households. The Census Bureau recruits respondents through email or phone text messaging from the Census Bureau Contact Frame and collects information online using Qualtrics. Several salient features of the HPS make it fit for use in this study. First, the survey provides high-frequency, real-time data that is nationally representative. The HPS employed the Census Bureau's Master Address Files (MFA) as the primary sampling frame to select a large sample that would be sufficient to produce estimates at the state level. The MFA is the gold standard frame for US Statistics. Second, the survey collects data from approximately 70,000 households in each survey wave since March 2020, making it the largest household survey in the US during the pandemic. Third, the survey collected not only information on employment status but also reasons for unemployment, which helps us identify whether an individual is unemployed due to the pandemic-related plant closure. Fourth, the HPS survey provides detailed individual and household characteristics about health status, mental health condition, insurance status, health care utilization, and socioeconomic status, among others. This allows us to control for a rich set of the respondent- and household-level observables, which in conjunction with the exogeneity of COVID-19 shutdowns, can help make a convincing case for identification.

We use the first 32 waves of HPS survey, covering the period from April 23 - May 5, 2020, to June 9 - 21, 2021 (see Appendix table B1 for details). We limit our analysis to respondents aged 18-65 years, excluding those not in the labor force[9]. Since low-wage workers were hit hardest by the pandemic and around 80% of job losses were among low-income households (Gould and Kandra, 2021), we also limit the sample to households with annual income below $100,000 in 2019. The Census Bureau assigns a person-weight to each survey respondent to make the sample representative of the entire US population. All regressions are weighted by the HPS sampling weights, and the standard errors are clustered at the state level.

The HPS collects information about the individuals' employment status by asking the question: "*In the last 7 days, did you do any work for either pay or profit?*" Individuals who answered "no" were asked about the main reasons for not working for pay. We identify unemployment due to COVID-19 shutdown based on responses to the survey question: "*What is your main reason for not working for pay or profit*?" Our unemployment indicator variable is equal to 1 if the respondent selects one of the following reasons: "my employer experienced a reduction in business (including furlough) due to coronavirus pandemic," "my employer closed

[9] Individuals that self-reported as being retired, or unable to work because of disability, and chronic health conditions that were not coronavirus-related were excluded from the sample.

temporarily due to the coronavirus pandemic," and "my employer went out of business due to the coronavirus pandemic."[10]

The question, "*Would you say your health, in general, is excellent (1), very good (2), good (3), fair (4), or poor (5)? Select only one answer*" assesses general health status[11]. We defined an indicator of *poor health* as 1 if a respondent reported having fair or poor health and 0 otherwise. We measure mental health status using questions adapted from the Generalized Anxiety Disorder (GAD-2) and Patient Health Questionnaire (PHQ-2) scales, representing generalized anxiety disorder and major depressive disorder screens, respectively (Kroenke et al., 2003; Kroenke et al., 2007). The GAD-2 and PHQ-2 are brief self-report mental health screening questionnaires that report the frequency of anxiety and depressive mood. The GAD-2 and PHQ-2 were based on the response to the question, "*Over the last 7 days, how often have you been bothered by the following problems ... feeling nervous, anxious, or on edge/ not being able to stop or control worrying/ having little interest or pleasure in doing things/ feeling down, depressed, or hopeless*." Potential responses for each question were rated on a 4-point Likert scale indicating, "not at all", "several days", "more than half the days", or "nearly every day." Both scales show good internal reliability, and a score of 3 or more on either scale indicates possible anxiety or depression. This standard cut-off is used to define binary indicators of anxiety and depression so that anxiety equals 1 if the GAD2 score is at least 3, while depression takes a value of 1 if the PHQ 2 is at least 3 (for details, visit Generalized Anxiety Disorder 2-item (GAD-2) - Mental Disorders Screening - National HIV Curriculum).

[10] HPS slightly modified the wording of the response items in the third phase (from October 28, 2020, to June 21, 2021) without changing the substance of the responses.
[11] Question about general health status was discontinued after 21st week of HPS.

The outcome variable, *MH service,* is from the following questions, "*At any time in the last 4 weeks, did you receive counseling or therapy from a mental health professional such as a psychiatrist, psychologist, psychiatric nurse, or clinical social worker? Include counseling or therapy online or by phone."* If the respondent responded "yes" to the question, the MH service indicator takes a value of 1, and 0 otherwise[12]. Similarly, *Prescription MH* was assessed by the question "*At any time in the last 4 weeks, did you take prescription medication to help you with any emotions or with your concentration, behavior or mental health?*" If the respondent responded "yes" to the question, the indicator *prescription MH* takes a value of 1, and 0 otherwise.

Table 2.1 provides summary statistics. Out of the 896,727 individuals in our sample, 29.7 percent reported being unemployed at some point during our study between April 23 - May 5, 2020, to June 9 - 21, 2021, corresponding to 1 to 32 HPS survey weeks. Among the unemployed, 42 percent cited firm closure due to Covid-19 as the reason for their unemployment. Henceforth, we refer to this group as Covid-unemployed, while those unemployed due to reasons other than Covid-induced firm closures are referred to as other-unemployed. A significantly larger proportion of Covid-unemployed individuals reported receiving unemployment insurance (UI) benefits than the other unemployed group (38.7 percent and 18.7 percent, respectively). In comparison, the opposite was true in the case of stimulus payments- 29.4 percent of Covid-unemployed individuals reported receiving at least one stimulus payment compared to 31.9 percent in the other unemployed group. Among the unemployed, Covid-unemployed individuals

[12] MH service and prescription MH variables were not measured until Phase 2 of the HPS (12^{th} week of survey).

were more likely, on average, to be better educated and have a high income compared to the other group.

Figure 2.1 shows the raw trend of the proportion of unemployment due to firm closures brought about by the COVID-19 pandemic. Although the trend seems to have declined to about 20 percent by the last wave of the survey, that figure might be misleading given that most of the firms that closed down during the initial days (onset) of the pandemic did not open up again.

Table 2.2 provides definitions and summary statistics of outcome variables. On average, employed individuals in our sample reported feeling better about their general health status, as shown by lower poor health scores. They were also less likely to have experienced anxiety or depression and received prescription medication or professional care regarding their mental health in the preceding four weeks. Among the unemployed, Covid-unemployed individuals had a better general health status compared to the Other-unemployed group (average poor health of 0.245 vs. 0.324). However, they performed worse in the two questionnaires pertaining to mental health, where the Other-unemployed group had lower scores. Uptake of professional care was also more limited among Covid-unemployed individuals who were less likely to have received prescription medication (by 3.2 percentage points) and professional counseling or therapy (by 2.2 percentage points). Figure 2.2 provides a graphical representation of health outcomes across employment groups.

3. ESTIMATION STRATEGY

The relationship between unemployment and general and mental health can be outlined within the human capital model of health demand (Grossman, 1972). According to this model, individuals indirectly value health, raise healthy productive time and wages, and directly value health and increase utility. As such, an individual maximizes an intertemporal utility function that includes health H_t; current investment in health I_{Ht} (health care inputs); health-promoting activities P such as physical activity, gym membership, shopping for healthy food, good sleep, and cooking; health-compromising activities (goods) C such as tobacco consumption and alcohol; working hours L and a vector of individual characteristics X, including psychological factors. Algebraically, the utility can be represented as follows:

$$U_t = U(H_t, I_{Ht}, L, P, C, X) \qquad (1)$$

This model combines the household production model of consumer behavior with human capital investment theory to assess health capital. Health production functions consider individual health at the time t, H_t is produced by the following:

$$H_t = h(I_{Ht}, L, P, C, X) \qquad (2)$$

Many potential pathways may explain the health effects of unemployment. First, the loss of wages attributable to job loss results in a change in current investment in health, health-promoting, and health-compromising activities through the income and substitution effects. A decline in income resulting from job loss would lower consumption of all normal goods, including health-promoting and deteriorating goods and services, which could be either positive or negative depending on relative spending on health-promoting and risky health behaviors. Moreover, due to job loss, individuals with low household income may substitute cheap, less healthy food for healthy food or, alternatively, forgo alcohol or other risky health behaviors to spend more on health-promoting activities. Thus, the net income and substitution effect of joblessness on health is ambiguous.

The second mechanism is through the opportunity cost of time or substitution effect as job loss increases discretionary time. The unemployed individual may use additional leisure time toward health-enhancing activities such as physical activities, cooking healthy food at home, preventive health care, doctor visits, or health-damaging activities such as drug, alcohol consumption, and sedentary life. A healthy time enters into the utility functions as a consumption good, and we could expect to improve health after displacement. In contrast, the opposite is true with engaging in unhealthy activities. This mechanism also predicts ambiguous effects on health.

Third, an individual might lose employer-sponsored health insurance coverage after displacement. Loss of health care coverage is expected to impact investment in health-related activities. Reduction in health care coverage likely decreases access to care because of high out-of-pocket costs. Less frequent doctor visits may lead to an increase in participation in unhealthy behaviors. On the other hand, the individual may be involved in health-promoting activities in the context of the ex-ante moral hazard effect after job loss (Dave and Kaestner, 2009). The net effect of health care coverage loss on health depends on the changes in both access to care and health behaviors, indicating a theoretically ambiguous impact. Finally, job loss can increase financial strain, uncertainty, social isolation, domestic violence, and unrest in a family, causing stress and poor physical and mental health. Also, the psychological effects of job loss are associated with a lifestyle change (hours), representing the relationship between job loss and health. Nevertheless, job loss and reduced income may make the unemployed person eligible for unemployment insurance benefits and other safety-net programs. Participating in these safety net programs may help reduce financial strain and enhance healthy behaviors.

To sum up, at the time of the unemployed, both forces – health-promoting and deteriorating – are in play but in opposite directions. Therefore, it is an empirical question to determine whether and to what extent unemployment brought about by the pandemic impacted the general and mental health of unemployed individuals.

We examine the extent to which a large exogenous shock to employment brought about by the COVID-19 pandemic affected individuals' general and mental health using the following regression model:

$$y_{ist} = \beta_0 + \beta_1 U\_closure_{ist} + \beta_2 U\_other_{ist} + X'_{it}\alpha + \delta_s + \omega_t + \varepsilon_{ist} \qquad (3)$$

where y_{ist} represent outcomes of interest for respondent i in survey wave t and state s. There are five measures of y_{ist} that are of interest to us: binary indicators of poor health, anxiety, depression, mental health prescription, and service indicators. The variable, $U\_closure_{ist}$, is an indicator equal to 1 if individual i in survey wave t was unemployed due to plant closure brought about by the COVID-19 pandemic; U_other_{its} is an indicator equal to 1 if individual i in survey wave t was unemployed due to other reasons than the COVID-19 pandemic induced plant closure; X_{ist} is a column vector of respondent characteristics; δ_s is the state fixed effect; ω_t is the wave fixed effect; the β's, and α's are coefficients; and ε_{ist} is the residual. In our model, β_1 is the parameter of main interest that captures the effects of unemployment due to firm closure on health status and mental health service utilization.

The state and wave fixed effects are used to control for time-invariant unobserved state heterogeneity and seasonality, respectively. In the fully specified model, X_{ist} includes age, gender, marital status, household size, children, stimulus payment recipient, race, education, and household income.

First, we exploit plant closure brought about by the COVID-19 pandemic as a plausibly exogenous source of variation to identify the causal effects of job loss on general and mental health. Our estimation strategy is based on the hypothesis that job loss due to firm closure is involuntary unemployment, which is unlikely to be related to individuals' health status. Several studies have explored this identification strategy to investigate the causal impact of job loss on several outcomes, for example, mortality (Eliason and Storrie, 2009; Browning and Heinesen, 2012), mental health of spouses (Marcus, 2013), health satisfaction, hospital visit, and mental health (Schmitz, 2011), birth weight (Lindo, 2011), and social participation of spouse (Kunze and Suppa, 2020). We estimate the binary logit models separately for all five outcome variables and then compare the results with the linear probability models (LPM).

There are good reasons to believe that COVID-19 induced unemployment is exogenous and less prone to selection problems. Workers are laid off when the plant is shut down, irrespective of their characteristics and underlying health status (Eliason and Storrie, 2009). Similarly, COVID-19 disrupted almost all sectors of the economy, and concerns about laid-off workers transferring from one plant to others in search of a job are somehow unlikely.

Instrumental Variable Approach

While our base model accounts for several estimation issues, we may still be concerned with estimating the impact of unemployment on individual health status and the potential endogeneity of unemployment status. This potential endogeneity may arise because individuals with poor health may be more likely to be unemployed due to the firm's downsizing if not shutdown. In addition, unobserved factors may be linked to unemployment, affecting general and mental health. Individuals reporting lower general and mental health may select themselves into the industries that are less likely to be affected by the economic decline. Controlling for individual characteristics may not be sufficient to address the simultaneity issue, and it can not safeguard against estimation bias due to these observed factors. Further, the HPS survey does not collect industry-level employment information before being laid off to control for industry-fixed effects. In this case, logit and LPM estimates will not be consistent. To obtain the causal effects of unemployment on an individual's health status, we need an exogenous shock that is not directly correlated with unobservables. We employ the Bartik IV approach proposed by Bartik (1991) and applied in several past studies (see, e.g., Goldsmith-Pinkham et al., 2020; Avdic et al., 2021; Derenoncourt, 2022) to tackle endogeneity concerns and uncover the causal effects of unemployment on health.

Building on the shift in share strategy, we develop the exogenous shock as instrument variables by multiplying aggregate "shifts" with local industry shares at the state level. Specifically, we instrument potential endogenous variable of interest, unemployment, by combining changes in employment across different industries defined by the two-digit North American Industry Classification System (NAICS) code at the state level (individual's state of residence) "shifts" with state-level industry composition "shares." Specifically, we first aggregate the local industry employment share at the state level in the base period, January 2020. Secondly, we calculate the national employment across industries relative to the base period. Finally, we interact the share of state-level employment across different industries and monthly growth in employment across industries compared to the base period. The IV is constructed as follows:

$$IV_{st} = \sum_j S_{bsj} \frac{E_{tj} - E_{bj}}{E_{bj}} \tag{4}$$

where S_{bsj} denotes the employment share of industry j in state s in the base period b. E_{tj} is the national employment levels of industry j and survey wave t, and E_{bj} is the national employment level of industry j in the base period b. In this case, $\frac{E_{tj} - E_{bj}}{E_{bj}}$ is the national employment growth rate of industry j in period t relative to the baseline period b. We argue that the baseline employment shares only affect (1) mental health through unemployment due to job loss after controlling for the covariates and state and month fixed effects, and (2) baseline state-survey wave employment share is conditionally orthogonal to population mental health. The identifying assumption is that changes in state-level industry growth rate (growth of industry employment shares) are uncorrelated with changes in population mental health after controlling for the individual characteristics and state and month fixed effects. This instrument captures the effects of state-employment shares on an individual's health. The exclusion restriction criterion is not formally testable, but likely, unemployment shocks can only affect individual mental health through being laid off.

The IV estimation approach in nonlinear models like binary response models with a binary endogenous explanatory variable is more challenging (Wooldridge, 2010). Two-stage residual inclusion (2SRI), two-stage predictor substitution (2SPS), control function approach, and two-stage least squares (2SLS) estimators are common methods for IV-based strategies. Terza et al. (2008) reported that 2SRI, an approach to add residuals from the first stage as a regressor to the second stage to solve the endogeneity problem, is consistent, but 2SPS is not. However, the applicability of 2SRI in nonlinear models with a binary endogenous variable remains contentious (Basu et al., 2017) as it relies on the concepts that support control function methods developed for continuous endogenous variables (Blundell and Powell, 2004). Wooldridge (2015) noted that control function methods are often used where "plug-in" approaches such as 2SRI or 2SPS are known to produce inconsistent estimation. Many applied researchers use a linear probability model instead of a binary response model due to its simplicity and claim that it delivers adequate estimates (Wooldridge, 2010). Due to their analytical simplicity, Kang and Lee (2014) recommend applying the 2SLS and the control function approach. Cameron and Trivedi (2010) suggest that using the 2SLS approach with an LPM gives consistent estimates but that the robust standard errors should be used for inference. Angrist and Krueger (2001) note that 2SLS estimates can be interpreted irrespective of the nonlinearity of the binary variables.

Given the literature, we first employ a linear 2SLS and then the control function approach, which is a modification of 2SRI. The control function approach introduces the generalized residual from the reduced form equation (first stage), rather than residuals, for the regressors as covariates in the binary response model to account for the endogeneity of unemployment status.

Using our Bartik instrument in the 2SLS yields the first stage equation is,

$$U\_closure_{ist} = \beta_0 IV_{st} + X'_{it}\alpha + \delta_s + \omega_t + \varepsilon_{ist} \tag{5}$$

where $U\_closure_{ist}$ is the binary indicator of unemployed due to firm closure, and the second stage equation is:

$$y_{ist} = \beta_0 + \beta_1 U\_\widehat{closure}_{ist} + \beta_2 U\_other_{ist} + X'_{ist}\alpha + \delta_s + \omega_t + \varepsilon_{ist}. \tag{6}$$

Under the control function approach, the first step involves the estimation of the logit model of $U\_closure_{ist}$ on $z_{ist}(z_{ist} = X_{ist}, IV_{st})$ as

$$P(U\_closure_{ist} = 1 | IV_{st}) = \Phi(IV_{st}\pi_1 + \mathrm{X}_i\pi_2) \tag{7}$$

where π_1 and π_2 are coefficients. In order to achieve identification, we impose the usual exclusion restriction. In the second step, generalized residuals are obtained as

$$\hat{r}_{ist} = U\_closure_{ist}\lambda(IV_{st}\hat{\pi}) - (1 - U\_closure_{ist})\lambda(-IV_{st}\hat{\pi}) \tag{8}$$

where $\lambda(.) = \phi\,(.)/\Phi\,(.)$ is inverse Mill's ratio (IMR), and $\phi(.)$ and $\Phi(.)$ are the standard normal density and cumulative distribution functions, respectively (Wooldridge, 2015).

Subsequently, our preferred estimating equation would be

$$y_{ist} = \beta_0 + \beta_1 U\_closure_{ist} + \beta_2 U\_other_{ist} + \rho_0 \hat{r}_{ist} + \delta_s + \omega_t + \varepsilon_{ist} \quad (9)$$

which is estimated by IV logit using IVs ($IV_{st}, \hat{r}_i$) and where β_0, β_1, β_2, $\delta_s, \rho_0, \omega_t$ are parameters. We use the Huber/White sandwich estimator for the robust heteroskedasticity standard errors clustered at the state level.

4. RESULTS

Logit Estimates

We first report results from the estimation of equation (3) by pooling all respondent types for all five outcome variables. We then estimate the sub-sample separately across different dimensions of individuals, households, and states of residents' characteristics. The main results from our base model are provided in Table 2.3, which shows the effect of unemployment due to COVID-19-induced firm closures on general and mental health. Columns 1 report the coefficients from logit models with *poor health* as the dependent variable, while column 2-5 presents the estimates for mental health[13]. Although the magnitude of the coefficients varies, results are largely consistent, with the inclusion of state and survey week fixed effects (Panel B) and demographics and state and survey week fixed effects (Panel C). The coefficient on *unemployed-firm closure* is statistically significant at the 1% level in every specification for either measure of general and mental health. For the binary logit regressions, a positive coefficient on *unemployed-firm closure* indicates that the log-odds of having poor general and mental health and receiving mental health services increased after job loss. Exponentiating the coefficient gives the odds ratio of having poor general and mental health after being unemployed due to firm closure. Our findings suggest that unemployment affects general and mental health in the labor force. Based on the fully

[13] The full set of coefficient estimates for the fully specified equation (3) is presented in appendix table A2.

specified model in panel C, the odds of having poor health, anxiety, and depression and uptake of mental health services and prescription medication for mental health are 1.383 1.766, 1.867, 1.275, and 1.221 times higher among unemployed to firm closures relative to employed individuals, respectively. Table 2.4 reports the average marginal effects of our variable interest on outcome variables after fitting binary logit models. Results from Panel C show that the probability of having poor health, anxiety, depression, and getting mental health prescription medication and services increased by 5%, 13%, 3.9%, and 1.9% of the sample mean, respectively, compared with an employed respondent. For comparison, we present corresponding LPM estimates in Table 2.5. Estimates agree in sign and statistical significance from the LPM for all outcome variables and are broadly consistent with our main specification from the binary logit models. Further, we also find that estimates of LPM are broadly similar to the average marginal effects of a variable of interest from logit models on all outcomes.

Instrumental Variable Estimates

We complement our analysis of the causal relationship between unemployment and health with the IV approach to tackle the potential endogeneity concerns. To this end, we estimate Equations (5) and (6) by 2SLS to address the potential endogeneity of our variable of interest. Table 2.6 reports these results with the fully specified model, adjusting for demographics and state and survey wave fixed effects. We cluster the standard errors at the state level to account for state-level exposure to the labor market shocks (Abadie et al., 2017). The 2SLS estimates are slightly higher than the LPM and marginal effects from the binary logit models for all outcomes. We then run the control function approach to account for the endogeneity of our variable of interest using Equations (8) and (9). We take the estimates from the control function approach as a preferred specification as it accounts for the estimation issue in a binary endogenous variable with binary outcomes. Table 2.7 reports the estimated marginal effects of unemployment on general and mental health using the control function approach. The marginal effects are 0.067, 0.145, 0.162, 0.061, and 0.021 percentage points for poor health, anxiety, depression, prescription medication, and services for mental health, respectively. The estimated results are largely consistent across the different specifications. In other words, results from the IV-based approaches are qualitatively similar to our base models. These results suggest unemployment brought about by the pandemic results in poor general and mental health and increased uptake of prescription medications or professional mental health services.

In combination, our results show the strong health effects of unemployment brough about by the pandemic on working-age group individuals. Again, since the first row of each specification only discusses unemployed individuals due to the COVID-19 pandemic, these increases in the probability of having poor general and mental health can be directly attributed to job loss.

Heterogeneity Analysis

We believe unemployment may affect overall general and mental health among some individuals more than others. To investigate the heterogeneous effects of being laid-off across groups, we divide our sample by individual, household, and state of residence characteristics, which we suspect can affect general and mental health. Figure 2.3 plots the marginal effects of $\hat{\beta}_1$ of equation (9) and its 95% confidence interval for each of the five outcomes of interest by respondent type to present results from our heterogeneity analyses. Panel A provides some cause for concern as we can see that anxiety in individuals is higher in those states with a below-median number of mental health professionals. Panel B provides suggestive evidence that issues of unemployment related anxiety and depression are concentrated among unemployed individuals in the Medicaid non-expansion states. Heterogeneities across insurance statuses (Panel C) confirm the effects of uncertainty about the future on mental health. Those without UI benefits are more likely to suffer from anxiety or depression and receive prescription medication for their mental health problems (Panel D). Panel E reveals that females are significantly more likely to feel worse about their health and more likely to have experienced anxiety or depression after being laid off. Unemployed individuals with higher household incomes (above the median income-to-poverty ratio in 2019) are more likely to report poor health, anxiety, and depression than low-income households (Panel F).

Robustness check

To ensure robustness, we now compare our results with those from other possible specifications. First, we fit the ordered logit model for ordinal measures of general health status (1-5), GAD-2 (0-6), and PHQ-2 (0-6) (as defined in Table 2.2) in appendix table B3. Results further confirm our findings- being out of work can lead to a perceived decline in health status and contribute to increases in the incidence of mental health problems such as anxiety and depression. We also fit a binary logit model for all outcome variables with our full sample without restricting the sample by household income and age (see appendix table B4). We show that results are robust to our main specification. These sensitivity analyses show that our estimates are robust to choices of functional forms.

5. DISCUSSION

This study uses the HPS to investigate the effects of unemployment on general health status, mental health, and mental healthcare utilization. We showed that unemployment is detrimental to perceived overall health status and increases the likelihood of onset anxiety and depression while also increasing uptake of professional services to tackle mental health issues. We further explored the differentials in unemployment effects according to the reason for unemployment, finding that the effects are larger for unemployed individuals due to COVID-related plant closures compared to those unemployed for other reasons.

Previous studies have reported similar results on the effect of involuntary job loss carried out across multiple regions of the world, including Germany (Clark et al., 2009), Australia (Green, 2011), and Greece (Drydakis, 2015). However, no clear consensus has emerged in the literature so far regarding the size or even the existence of unemployment effects on health outcomes. Browning et al. (2006), for instance, find no causal effect of job loss on hospitalization caused by mental stress. Salm (2009), using a linear difference in differences model on the Health and Retirement Surveys (HRS) dataset, finds no effect of unemployment on multiple health measures. Kuhn et al. (2009) find no short-run effects of job loss on public health costs associated with health care utilization. However, they do report some evidence that job loss increases hospitalization due to mental health reasons in men.

While the results discussed above may seem contradictory, a closer analysis of the methods used in arriving at those conclusions helps put them in perspective. Identifying the causal effect of unemployment is challenging due to well-established issues of reverse causality and unobserved individual heterogeneity, which may bias results obtained from cross-sectional studies (Cygan-Rehm et al., 2017). To account for the potential endogeneity of unemployment with health outcomes, previous work has employed two strategies- estimating fixed-effects models on longitudinal data to account for time-invariant heterogeneity (Bjorklun, 1985; Clark et al., 2001; Green, 2011), and exploiting exogenous variations in employment caused due to plant closures and other firm-level employment reductions (as in Browning and Heinesen, 2012; Eliason and Storrie, 2010; Kuhn et al., 2009; and Marcus, 2013). While these methods help improve the precision of estimates, Cygan-Rehm et al. (2017) argue that most of the null results may come from a lack of power rather than the absence of a causal effect. Although Salm (2009) finds no causal impact of unemployment on mental health in the US, the study is based on the HRS, which only includes information on older workers and thus may not represent the nature and size of the effect on the whole working population. This claim is supported by the findings of Schaller and Stevens (2015), who, using the more representative MEPS, conclude that involuntary job loss significantly impairs mental health. Our analysis, on the other hand, is restricted to working-age individuals. Similarly, while Schmitz (2011) reports that there is no significant correlation between unemployment and mental health after accounting for endogeneity based on limited plant-closures data from the German Socio-Economic Panel, his results are questioned by Marcus (2013) who, using non-parametric matching techniques on the same dataset, finds significant decreases in mental health due to unemployment caused by plant closures in both the individual directly affected by the closure as well as their spouses. Other recent studies using larger datasets have concluded that job loss does have detrimental impacts on mental and physical health, including increased medical expenditures due to health problems in men (Kuhn et al., 2009); increased short-run risk of suicides, alcohol-related mortality, and hospitalizations in Swedish workers (Eliason and Storrie, 2010); and increased suicide risks in Danish men (Browning and Heinesen, 2012). A closely related literature (Butterworth et al., 2012; Stewart, 2001; Stauder, 2019) also investigates the duration of unemployment on health outcomes, which is beyond the scope of this article.

Next, we discuss some of the potential channels behind these negative effects of unemployment on health outcomes. Unemployment is typically accompanied by a persistent reduction in expected income (Yong-Hwan Noh, 2009), and it is tempting to think that the decline in health among the unemployed may be entirely explained by a loss in income following job loss. However, we believe this mechanism does not satisfactorily explain our results here, considering the US federal and state governments' programs to provide unemployment benefits during the pandemic. Most states offer a 26-week benefits program with additional supplemental benefits up to $600 per week. This program provided all eligible unemployed persons, regardless of their types of unemployment, with up to 145% median statutory replacement rate (Ganong et al. 2020). The extended unemployment benefits may help offset the loss of income due to job loss and improve health status if it results from a short-term financial burden. However, the overall impact of job loss depends on the relative size of income drop and other health deteriorating effects brought about by pandemic-induced job loss. We believe a large part of the effects can be attributed to the loss of health insurance, stress, and uncertainty about the future job. Employment-based insurance is the most common source of health insurance in the United States and covers 54.4 percent of the population for some or all of the 2020 calendar year (United States Census Bureau, 2020). However, this figure has been on the decline in recent years (0.7 percentage points between 2018 and 2020). Being underinsured, particularly during an unprecedented pandemic, and the subsequent health-related uncertainty may have been a major factor in the deterioration of mental health in the unemployed. While the loss of self-esteem because of unemployment may have also been a contributor, previous studies from the life satisfaction literature have denied the existence of this relationship (Hartley 1980; Goldsmith et al., 1995). Examining the potential pathways for the effects of unemployment on mental health is an important area for future work.

6. CONCLUSION

We estimate the relationship between unemployment and health using the nationally representative data of the US household survey. Our study extends the existing literature on identifying the causal effects of unemployment on general and mental health. Following Bartik, (1991) and Goldsmith-Pinkham et al. (2020), we generate exogenous economic shocks that represent a source of variation in the local labor market as an instrumental variable to estimate the causal effect of unemployment on general and mental health. We find evidence that unemployment is associated with reporting poor health, anxiety, depression, and increased mental health services and prescription medication. These findings are robust to alternative specifications. Additionally, we find heterogeneous effects of unemployment across the state of residence and individual or household characteristics. Our findings on heterogeneity suggest that prioritizing the sub-group of the population affected more by being unemployed will allow state and federal governments to target the policies and programs to the most affected population.

Understanding the causal effects of unemployment on health outcomes is vital for informed policymaking. If unemployment does indeed affect general and mental health, then understanding the mechanism through which the effects are transmitted is crucial to know how to lessen the multidimensional unintended consequences of unemployment on health. Further, a positive causal relationship between unemployment and poor health would imply that any policy that decreases employment rates should also account for the social and financial costs of job loss.

In addition, health policy aimed at reducing the incidence of mental health problems in the population should also consider avenues for reducing unemployment.

Our findings further suggest that despite the huge investment in social safety programs and other policies to improve the labor market conditions during the pandemic, these programs could not fully protect against the effects of economic downturns on mental health and individual mental health were severely impacted. The study of these mechanisms may provide important policy implications on effectively dealing with individual mental health issues arising from the economic decline and public health crisis. Since the literature on the health effects of unemployment in the pre-pandemic era could be qualitatively different from during the pandemic, this study provides essential contextual nuance to the literature on the relationship between economic downturn and health.

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Table 2.1. Summary statistics of explanatory variables

Variables	Unemployed-firm close	Unemployed-other	Employed
Age (years)	41.128	40.756	40.985
	(0.248)	(0.261)	(0.176)
Age squared	1861.743	1841.954	1844.792
	(20.557)	(21.209)	(14.247)
Female (1/0)	0.494	0.617	0.519
	(0.004)	(0.007)	(0.002)
Married (1/0)	0.397	0.435	0.471
	(0.009)	(0.008)	(0.007)
Household size (numbers)	3.607	3.856	3.418
	(0.062)	(0.065)	(0.060)
Children (<18 years)	0.431	0.519	0.424
(1/0)	(0.008)	(0.008)	(0.006)
Own home (1/0)	0.466	0.503	0.577
	(0.020)	(0.017)	(0.018)
Stimulus payment recipient	0.294	0.319	0.278
(1/0)	(0.004)	(0.004)	(0.002)
Hispanic (1/0)	0.257	0.255	0.200
	(0.046)	(0.047)	(0.040)
Non-Hispanic Black (1/0)	0.185	0.174	0.138
	(0.021)	(0.019)	(0.014)
Non-Hispanic White (1/0)	0.667	0.683	0.746
	(0.016)	(0.015)	(0.015)
Non-Hispanic Asian/Other	0.148	0.143	0.116
(1/0)	(0.022)	(0.018)	(0.016)
Less than high school	0.122	0.161	0.075
	(0.013)	(0.015)	(0.011)
High school graduate or	0.370	0.375	0.314
equivalent	(0.015)	(0.012)	(0.009)
Some college or associate	0.344	0.324	0.345
degree	(0.006)	(0.006)	(0.005)
Bachelor's degree and above	0.164	0.140	0.266
	(0.005)	(0.004)	(0.005)
Income less than $25,000	0.285	0.369	0.144
	(0.006)	(0.006)	(0.004)
Income $25,000 - $49,999	0.367	0.333	0.333
	(0.004)	(0.005)	(0.004)
Income $50,000 - $99,999	0.348	0.298	0.523
	(0.007)	(0.006)	(0.008)
N	111908	154487	630332

Robust Standard errors clustered at the state level are in parentheses. Observations are weighted by sampling weight.

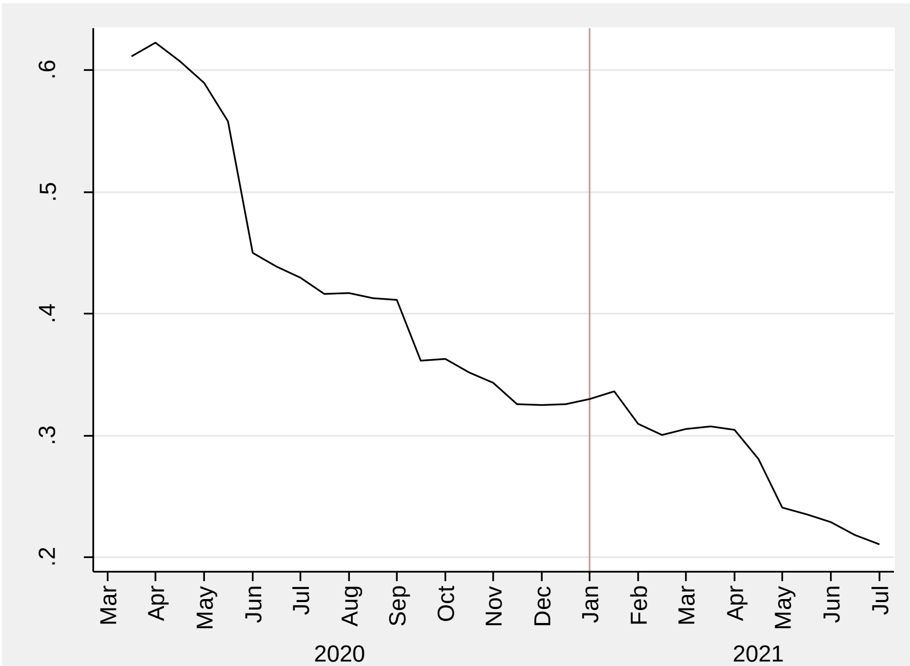

Figure 2.1. A raw trend of the proportion of unemployment due to firm closure brought about by the COVID-19 pandemic

Table 2.2. Summary statistics of outcome variables

Variables	Definition	Unemployed-firm closure	Unemployed-other	Employed
Health status	General health status, ordinal scale. 1-excellent, 2-very good, 3-good, 4-fair, and 5 poor	2.757 (0.013)	2.926 (0.019)	2.540 (0.012)
Poor health	Poor health=1 if health status is 4 or 5, and 0 otherwise	0.245 (0.006)	0.324 (0.007)	0.169 (0.004)
GAD2	Generalized Anxiety Disorder 2-item, 0-6 score, lower is better	2.943 (0.031)	2.765 (0.026)	2.265 (0.018)
PhQ2	Patient Health Questionnaire-2, 0-6 score, lower is better	2.601 (0.023)	2.417 (0.030)	1.864 (0.018)
Anxiety (1/0)	Anxiety=1 if GAD2 score is 3 or greater, and 0 otherwise	0.487 (0.006)	0.459 (0.006)	0.361 (0.003)
Depression (1/0)	Depression=1 if the PHQ2 score is 3 or greater, and 0 otherwise	0.431 (0.005)	0.397 (0.006)	0.284 (0.003)
Prescription MH (1/0)	Whether an individual takes prescription medication related to mental health at any time in the last four weeks.	0.221 (0.012)	0.253 (0.013)	0.204 (0.007)
MH service (1/0)	Whether an individual receives counseling or therapy from a mental health professional at any time in the last four weeks.	0.112 (0.007)	0.134 (0.005)	0.103 (0.003)

Robust Standard errors clustered at the state level are in parentheses. Observations are weighted by sampling weight. MH represents mental health.

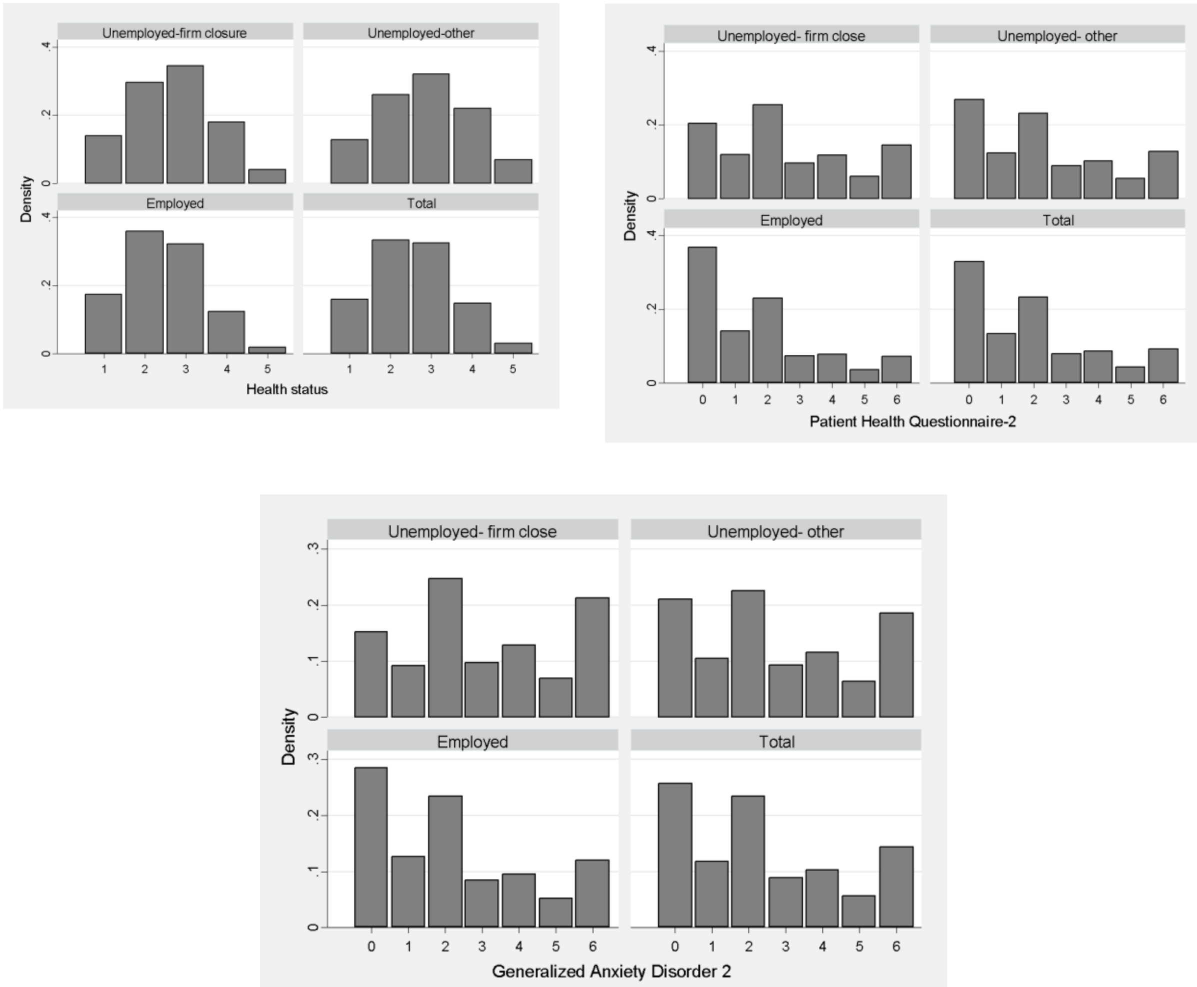

Figure 2.2. Distribution of ordinal outcome variables by employment status

Table 2.3: The effects of unemployment on general and mental health from the baseline model

Specifications	Poor health (1/0)	Anxiety (1/0)	Depression (1/0)	Prescription MH (1/0)	MH services (1/0)
	(1)	(2)	(3)	(4)	(5)
Panel A: Base model					
Unemployed-firm	0.467***	0.579***	0.673***	0.101***	0.099**
closure	(0.025)	(0.025)	(0.020)	(0.033)	(0.046)
Unemployed-other	0.858***	0.414***	0.496***	0.275***	0.299***
	(0.021)	(0.024)	(0.024)	(0.034)	(0.035)
Panel B: State and survey week fixed effects included					
Unemployed-firm	0.512***	0.599***	0.694***	0.167***	0.113***
closure	(0.024)	(0.025)	(0.022)	(0.025)	(0.044)
Unemployed-other	0.858***	0.404***	0.486***	0.303***	0.311***
	(0.021)	(0.024)	(0.024)	(0.026)	(0.035)
Panel C: Demographics and fixed effects included					
Unemployed-firm	0.324***	0.569***	0.625***	0.243***	0.200***
closure	(0.018)	(0.022)	(0.018)	(0.030)	(0.040)
Unemployed-other	0.600***	0.332***	0.395***	0.333***	0.397***
	(0.018)	(0.018)	(0.017)	(0.024)	(0.028)
N	661646	894177	893942	483561	483596

Panel A, B, and C present the estimation results of equation (3) with an increasing number of control variables. Panel A includes indicators for unemployed due to firm closure and others. Panel B adds state and survey week fixed effects. Panel C adds respondent demographics and fixed effects. Standard errors in parentheses are clustered at the state level. The sampling weight is used in estimation. *, **, and *** denote significance at 10%, 5% and 1% level, respectively. MH represents mental health.

Table 2.4. The marginal effects of unemployment on general and mental health after logit specification

Specifications	Poor health (1/0)	Anxiety (1/0)	Depression (1/0)	Prescription MH (1/0)	MH services (1/0)
	(1)	(2)	(3)	(4)	(5)
Panel A: Base model					
Unemployed-firm	0.077***	0.136***	0.146***	0.017***	0.010***
closure	(0.004)	(0.006)	(0.004)	(0.006)	(0.005)
Unemployed-other	0.141***	0.098***	0.107***	0.046***	0.029***
	(0.004)	(0.006)	(0.005)	(0.005)	(0.004)
Panel B: State and survey week fixed effects included					
Unemployed-firm	0.083***	0.141***	0.150***	0.028***	0.011***
closure	(0.004)	(0.006)	(0.006)	(0.004)	(0.004)
Unemployed-other	0.139***	0.095***	0.105***	0.051***	0.030***
	(0.003)	(0.006)	(0.005)	(0.004)	(0.003)
Panel C: Demographics and fixed effects included					
Unemployed-firm	0.050***	0.130***	0.131***	0.039***	0.019***
closure	(0.003)	(0.005)	(0.004)	(0.005)	(0.004)
Unemployed-other	0.093***	0.076***	0.083***	0.054***	0.037***
	(0.017)	(0.004)	(0.003)	(0.004)	(0.002)
N	661646	894177	893942	483561	483596

Table 2.5: The effect of unemployment due to firm closure on health status and health care utilization using the linear probability model

Specifications	Poor health (1)	Anxiety (2)	Depression (3)	Prescription MH (4)	MH services (5)
Unemployed-firm closure	0.049***	0.133***	0.138***	0.039***	0.017***
	(0.003)	(0.005)	(0.004)	(0.006)	(0.004)
Unemployed-other	0.106***	0.077***	0.084***	0.055***	0.037***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.003)
R2	0.075	0.049	0.052	0.048	0.038
N	661646	894177	893942	483561	483596

Standard errors in parentheses are clustered at the state level. The sampling weight is used in estimation. *, **, and *** denote significance at 10%, 5% and 1% level, respectively.

Table 2.6: Two-stage least square to estimate the impact of unemployment due to firm closure on general and mental health

Specifications	Poor health (1)	Anxiety (2)	Depression (3)	Prescription MH (4)	MH services (5)
Unemployed-firm closure	0.062***	0.158***	0.151***	0.041***	0.020***
	(0.004)	(0.006)	(0.004)	(0.005)	(0.004)
Unemployed-other	0.104***	0.075***	0.083***	0.055***	0.037***
	(0.005)	(0.004)	(0.004)	(0.005)	(0.003)
N	661646	894177	893942	483561	483596
R2	0.074	0.050	0.054	0.050	0.040

Standard errors in parentheses are clustered at the state level. The sampling weight is used in estimation. *, **, and *** denote significance at 10%, 5% and 1% level, respectively.

Table 2.7: Control function approach to estimate the impact of unemployment due to firm closure on general and mental health

	Poor health	Anxiety	Depression	Prescription MH	MH services
Specifications	(1)	(2)	(3)	(4)	(5)
Unemployed-firm	0.067^{***}	0.145^{***}	0.162^{***}	0.061^{***}	0.021^{***}
closure	(0.015)	(0.018)	(0.016)	(0.023)	(0.006)
Unemployed-other	0.104^{***}	0.075^{***}	0.083^{***}	0.055^{***}	0.037^{***}
	(0.005)	(0.004)	(0.004)	(0.005)	(0.003)
N	661646	894177	893942	483561	483596

Standard errors in parentheses are clustered at the state level. The sampling weight is used in estimation. *, **, and *** denote significance at 10%, 5% and 1% level, respectively.

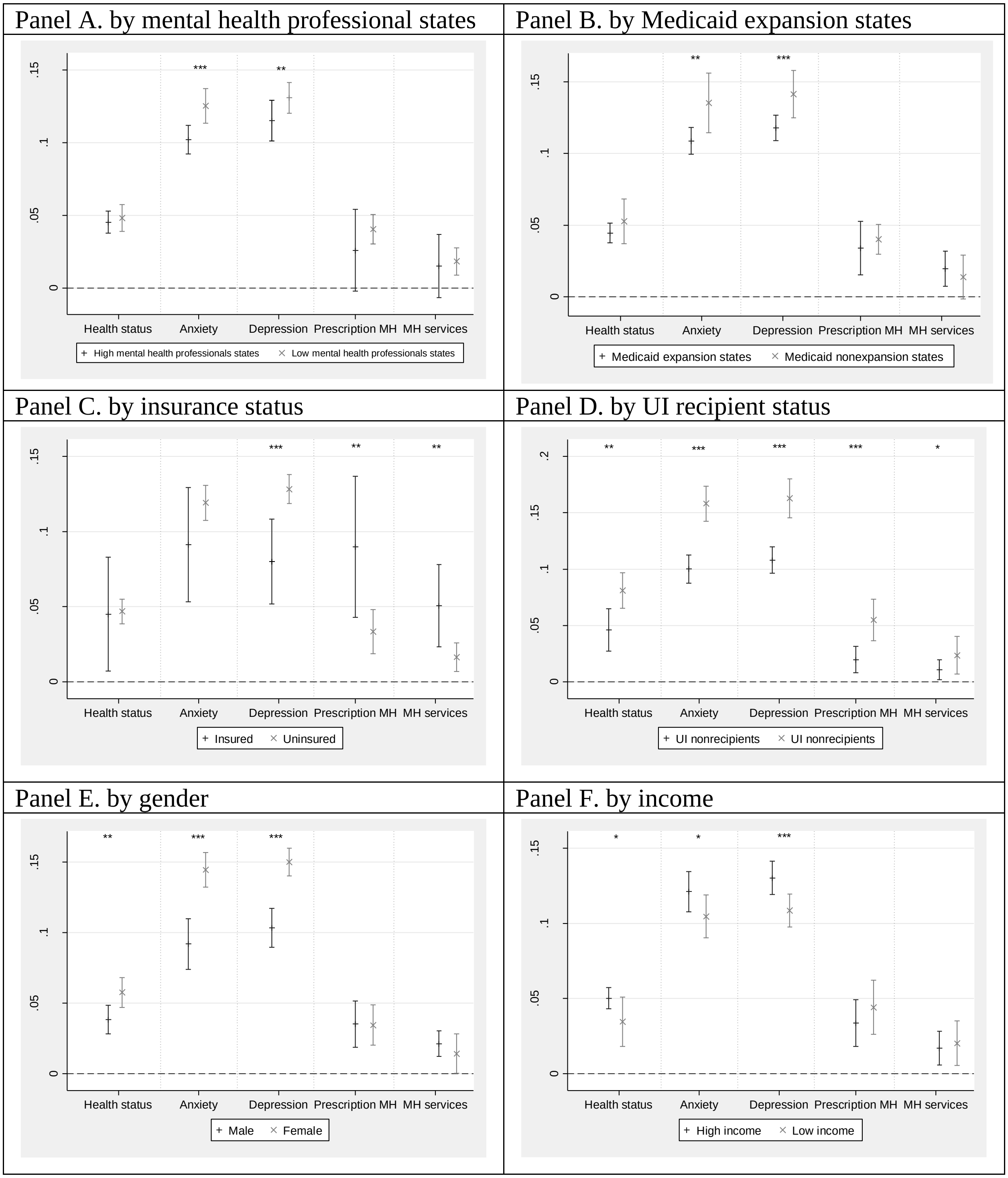

Figure 2.3. The effect of unemployment by respondent type

Note: Each bar illustrates the coefficient of the main variable of interest, unemployed-firm closure, and its 95 percent confidence interval.

APPENDICES

Appendix B1**.** Data collection period of the Household Pulse Survey

Household Pulse Survey wave week no.	Start date	End date
1	April 23, 2020	May 5, 2020
2	May 7, 2020	May 12, 2020
3	May 14, 2020	May 19, 2020
4	May 21, 2020	May 26, 2020
5	May 28, 2020	June 2, 2020
6	June 4, 2020	June 9, 2020
7	June 11, 2020	June 16, 2020
8	June 18, 2020	June 23, 2020
9	June 25, 2020	June 30, 2020
10	July 2, 2020	July 7, 2020
11	July 9, 2020	July 14, 2020
12	July 16, 2020	July 21, 2020
13	August 19, 2020	August 31, 2020
14	September 2, 2020	September 14, 2020
15	September 16, 2020	September 28, 2020
16	September 30, 2020	October 12, 2020
17	October 14, 2020	October 26, 2020
18	October 28, 2020	November 9, 2020
19	November 11, 2020	November 23, 2020
20	November 25, 2020	December 7, 2020
21	December 9, 2020	December 21, 2020
22	January 6, 2021	January 18, 2021
23	January 20, 2021	February 1, 2021
24	February 3, 2021	February 15, 2021
25	February 17, 2021	March 1, 2021
26	March 3, 2021	March 15, 2021
27	March 17, 2021	March 29, 2021
28	April 14, 2021	April 26, 2021
29	April 28, 2021	May 10, 2021
30	May 12, 2021	May 24, 2021
31	May 26, 2021	June 7, 2021
32	June 9, 2021	June 21, 2021

Appendix B2. Full model; Unemployment effects on general and mental health

Variable	Poor health (1/0)	Anxiety (1/0)	Depression (1/0)	Prescription MH (1/0)	MH services (1/0)
	(1)	(2)	(3)	(4)	(5)
Unemployed-firm closure	0.324***	0.569***	0.625***	0.243***	0.200***
	(0.018)	(0.022)	(0.018)	(0.030)	(0.040)
Unemployed-other	0.600***	0.332***	0.395***	0.333***	0.397***
	(0.018)	(0.018)	(0.017)	(0.024)	(0.028)
Age (years)	0.065***	0.022***	0.000	0.056***	0.062***
	(0.008)	(0.003)	(0.003)	(0.004)	(0.005)
Age squared	-0.000***	-0.000***	-0.000***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Female (1/0)	0.119***	0.315***	0.122***	0.618***	0.464***
	(0.024)	(0.013)	(0.012)	(0.033)	(0.028)
Married (1/0)	-0.182***	-0.196***	-0.287***	-0.143***	-0.293***
	(0.018)	(0.011)	(0.012)	(0.019)	(0.033)
Household size (numbers)	0.055***	0.033***	0.032***	0.001	-0.001
	(0.005)	(0.004)	(0.005)	(0.005)	(0.009)
Children (1/0)	-0.216***	-0.145***	-0.225***	-0.113***	-0.101***
	(0.018)	(0.019)	(0.015)	(0.015)	(0.031)
Own home (1/0)	-0.253***	-0.206***	-0.186***	-0.075***	-0.188***
	(0.026)	(0.015)	(0.020)	(0.019)	(0.017)
Stimulus payment recipient (1/0)	0.156***	0.156***	0.096***	0.075***	0.113***
	(0.035)	(0.013)	(0.018)	(0.021)	(0.024)
Hispanic (1/0)	0.091***	-0.217***	-0.221***	-0.459***	-0.338***
	(0.027)	(0.014)	(0.018)	(0.040)	(0.036)
Non-Hispanic Black (1/0)	-0.058**	-0.331***	-0.225***	-0.794***	-0.386***
	(0.028)	(0.019)	(0.022)	(0.043)	(0.028)
Non-Hispanic Asian/Other (1/0)	0.119***	-0.137***	-0.052***	-0.426***	-0.320***
	(0.034)	(0.020)	(0.019)	(0.041)	(0.047)
High school graduate or equivalent	-0.223***	-0.001	-0.009	0.074**	0.057
	(0.058)	(0.029)	(0.041)	(0.033)	(0.056)
Some college or associate degree	-0.334***	0.136***	0.073*	0.439***	0.549***
	(0.049)	(0.033)	(0.039)	(0.027)	(0.057)
Bachelor's degree and above	-0.813***	0.036	-0.171***	0.420***	0.898***
	(0.052)	(0.037)	(0.049)	(0.034)	(0.058)
Income $25,000 - $49,999	-0.336***	-0.125***	-0.130***	-0.132***	-0.273***
	(0.021)	(0.019)	(0.018)	(0.019)	(0.020)
Income $50,000 - $99,999	-0.739***	-0.297***	-0.353***	-0.211***	-0.313***
	(0.031)	(0.021)	(0.018)	(0.017)	(0.033)
Constant	-2.625***	-0.692***	-0.374***	-2.692***	-3.315***
	(0.155)	(0.073)	(0.065)	(0.112)	(0.106)
N	661646	894177	893942	483561	483596

Appendix B3: The effect of unemployment due to firm closure on health status and health care utilization using ordered logit

Specifications	Health status (1-5)	Anxiety (0-6)	Depression (0-6)
	Ordered logit		
	(1)	(2)	(3)
Unemployed-firm closure	0.230***	0.549***	0.590***
	(0.021)	(0.022)	(0.020)
Unemployed-other	0.452***	0.333***	0.378***
	(0.021)	(0.014)	(0.016)
N	661646	894177	893942

Standard errors in parentheses are clustered at the state level. The sampling weight is used in estimation. *, **, and *** denote significance at 10%, 5% and 1% level, respectively.

Appendix B4: The effect of unemployment due to firm closure on health status and health care utilization using binary logit with full sample

	Poor health	Anxiety	Depression	Prescription MH	MH services
Specifications	(1)	(2)	(3)	(4)	(5)
Unemployed-	0.339***	0.644***	0.703***	0.269***	0.176**
firm closure	(0.045)	(0.054)	(0.043)	(0.055)	(0.074)
Unemployed-	0.895***	0.314***	0.434***	0.427***	0.478***
other	(0.022)	(0.017)	(0.021)	(0.018)	(0.036)
N	1546636	2156121	2155750	1216358	1216231

Standard errors in parentheses are clustered at the state level. The sampling weight is used in estimation. *, **, and *** denote significance at 10%, 5% and 1% level, respectively.

CHAPTER 3

FOOD DEMAND AND EVOLUTION OF CONSUMER PREFERENCES FOR NUTRITIONAL QUALITY

1. INTRODUCTION

The US economy experienced a significant economic recession from December 2007 through June 2009 (NBER, 2012). Median household income fell in 2007 and did not rise until 2012, indicating a prolonged recovery (Dominguez and Shapiro, 2013). The 2008–2010 period is marked by the aggregate unemployment rate rising from 5.8 percent to 9.6 percent. The rise in the unemployment rate and a decline in real wages squeezed household budgets, particularly disposable income. Also, there was a substantial increase in the price of food relative to other goods**.**

Food is a large share of the household’s total spending; on average, household expenditures on food and beverages account for 12.9 percent of total spending in the United States (USDA, 2020). An individual’s decision to eat a particular type of food depends on many factors: income, price, preferences, etc. In the utility maximization framework, individuals choose the amount and type of foods that maximize their utility given budget constraints. Changes in household income alter the household’s food consumption decisions. For instance, Kumcu and Kaufman (2011) find that food spending by US households dropped 5 percent between 2006 and 2009 during the Great recession of 2008. This decline in food spending can alter purchasing and consumption patterns within food groups and types of food (Griffith et al., 2016; Dave and Kelly, 2012). Households may alter the food basket's composition, replacing expensive and healthy food with cheaper ones, which can have important implications for diet and nutrition.

An extensive literature on the relationship between income decline and nutritional quality has shown mixed (Dave and Kelly, 2012; Ng et al., 2014). Griffith et al. (2016)examine the effect of the Great Recession on the composition of food purchases/sales. Authors report that households maintained calorie requirements and nutritional quality by adjusting their shopping behaviors. Similalry, Kozlova (2016) finds that households do not improve the nutritional quality of their food purchases when their budget relaxes because of exogenous heating bill changes. As healthful food is more expensive per calorie basis than energy-dense foods (fruits and vegetables vs. oil and fats) (Drewnowski and Barratt-Fornell, 2004; Drewnowski, 2010), it seems intuitive that low-income households spend less on food and buy more energy-dense but less healthful products (Drewnowski et al., 2007) while adjusting their food budget.

Previous studies reported an improvement in U.S diet quality since the mid-1990s (Beatty et al., 2014; Wilson et al., 2016; Smith et al., 2019). Increased use of nutritional information and awareness is one of the key drivers of improving nutritional quality (Ollberding et al., 2011; Smith et al., 2019). Evidence suggests that consumer food preferences have continued to evolve in the last two decades. Despite the importance of food preference/consumer awareness in shaping food purchase decisions, there is inadequate evidence of the impacts of consumer awareness/knowledge on food demand at the national level. Only a few studies empirically establish a link between nutritional information and the healthfulness of food purchases (e.g., Variyam, 2008; Barreiro-Hurlé et al., 2010).

Household food purchases and then the nutritional quality of food purchased is, in part, driven by the underlying household's awareness about healthy food and increased use of nutritional information. Consumer information and awareness of healthy food, income, and prices are important factors in shaping consumer food preferences. We also aim to estimate how this increased awareness about healthy food has affected the quality of food purchased. There are various means that consumers can utilize to be informed about the healthfulness of food purchases, Google search being one of them. Understanding how a household's food purchase quality changes with the internet search intensity are crucial for designing policies to promote diets because it provides an important health information source. To do so, we use monthly Google search volume, commonly known as Google Trends, which is readily available, and real-time data. It is suggested that over 65% of the search queries are performed through Google (Afkhami, 2021). Google Trends, a freely available service by Google, provides the location, time, and subject-specific time-series data on the search volume of any requested keyword. Google provides an index of search volume that lies between 0 to 100. The value 100 indicates the maximum number of search queries reached, and 0 represents search volume below the threshold. Our measure of awareness index comes from the average of all Google indices based on Google search volume for several keywords, including "whole grain", "whole grain", "low-fat milk", "low sugar", low carb food", "zero sugar coke", "diet coke", "no sugar coke", low carb", "high fiber", "low sodium", "healthy food", "healthy diet", and "diet coke zero." Multiple past studies have used this data to answer various questions (Chen et al., 2015; Eichenauer et al., 2021). Google trend was used to predict mental health and well-being during the lockdown (Brpdeur et al., 2021), stock market performance (Huang et al., 2020; Takyi and Bentum-Ennin, 2021), unemployment insurance claims in the US (Goldsmith-Pinkhah and Sojourner, 2020; Larson and Sinclair, 2022), employment indicators (Caperna et al., 2022), and the degree of economic anxiety during the pandemic (Fetzer et al., 2020). We assume that the Google search index provides representative information about the individuals who use Google to search for healthy food. Despite the likely importance of Google search to provide information on healthy food choices, there is a lack of evidence as to the contribution of the Google search index to shaping household food preferences and the nutritional quality of food purchases.

Given this background, we seek to explore how household food preferences change with information on healthy food in addition to food price and income changes and their implications for dietary quality. We estimate the demand system to study causal estimates of price and income effects on food choices. Specifically, this study estimates consumer demand for foods classified by the Thrifty Food Plan (TFP) categories and the evolution of consumer preferences for taste and nutrition using the Exact Affine Stone Index (EASI) system (Lewbel and Pendakur, 2009; Zhen et al., 2014). Incorporating the Google search intensity variable in the EASI demand helps us to understand whether or not nutritional information changes food demand for a particular food group. This study also explores whether and how households adjust their food basket (healthy vs. unhealthy food purchases) over time and the pathways underlying this relationship.

Food groups in the TFP can be broadly categorized into two categories, healthy and unhealthy food groups. Including both health and unhealthy food groups allow us to understand better the shift in food preference in response to changes in prices and income. The health food group includes whole grains, vegetables, fruits, low-fat milk products, chicken, fish and fish products, eggs, and nuts, which are relatively more expensive. In contrast, unhealthy food groups are refined grains, whole milk and products, red meat, processed meat, sugar sweetener beverages, fats, and oils are unhealthy and energy-dense foods. Consumption of unhealthy food can lead to poor diet quality and health if people substitute away from low calories for high-calorie and less nutritious food. Because what people eat has an important health consequence, it is important to understand what factors (price, income, or preferences) greatly influence their decisions. Understanding what factors contribute to improvement in diet quality can help better shape nutrition policy (Wilson et al., 2016). Similarly, the substitution and complement between TFP food groups can have implications for diet quality and present another avenue for health outcomes.

The remainder of this article proceeds as follows. The next section discusses the data sources and descriptive statistics. We then describe the empirical strategy. This is followed by a presentation of the empirical results. The last section concludes the present study

2. DATA AND DESCRIPTIVE STATISTICS

We utilize data from the Nielsen Consumer Panel (Homescan) for 2007-2018, a national representative set of consumers. The data has information on food purchased and brought into the home by over 60,000 households annually. Nielsen recruits households in the sample via mail and online. Nielsen offers incentives such as monthly prize drawings, gift points, and sweepstakes to households to join and remain active in reporting transactions. Nielsen removes households failing to report their transactions regularly and adds new households to the panel to replace households who were filtered out or left the panel to ensure data quality. In doing so, Nielsen maintains a national representative sample. The households record where the product was purchased and the date and quantity purchased at the Universal Product Code (UPC) level. For each UPC, the data contains the UPC description, the price paid, and the amount purchased. In addition, data has information about the household panelist's demographics, including the head(s) of the household’s age, sex, race, education, region of residency, family composition, and household income. The panelist might report the price paid or a price Nielsen obtains directly from the store as part of their store-level survey. We also use Retailer scanner data (Scantrack) to create price instruments for the price indexes. Scantrack data consist of weekly prices and sales volumes for each UPC sold at approximately 42,000 unique stores from 160 retail chains from 2006–2017.

Although Homescan data has several advantages over government survey data, it does not have nutritional information on products. Therefore, we can not directly evaluate the nutrition quality of food purchases. Using the UPCs and product descriptions, we categorize household food purchases into 29 categories that largely align with the TFP food groups used in the US Department of Agriculture (USDA) Food Plans. and estimate the demand system at the food group level. This classification helps to construct the Grocery Purchase Quality Index (GPQI) developed by Brewster et al. (2017) as a tool for assessing the quality of household grocery purchases. The GPQI score (0-75) is the sum of 11 component scores. The GPQI performed similarly to the Healthy Eating Index (HEI)-2015 (Brewster et al. 2017).

Figure 3.1 depicts monthly daily average Google search results for all topics of interest and GPQI total score from January 2006 to December 2017. Searches for healthy food in the United States have experienced a gradual increment while the GPQI score is fairly constant, at least with the visual inspection. We investigate whether the total GPQI score increases over the years using the regression approach adjusting for the household fixed effects. Column 1 of Table 3.1 shows the regression of the total GPQI score on the panel year to examine the trend of GPQI score over the years. All coefficients of the indicator of panel years are significant, and these results provide suggestive evidence that there is an improvement in the household food purchase quality compared to the base year 2006. As the visual assessment does not provide a clear relationship between the GPQI score and the Google trend, we run the OLS regression to examine their association. Column 2 of Table 3.1 provides the estimates from the regression of the Google trend index on the total GPQI score, adjusting for the household characteristics and year-fixed effects. The coefficient on the Google trend is precisely estimated, meaning that the Google trend is associated with the GPQI score and improvement in diet quality likely attributable to the increase in individual awareness of healthy eating.

Table 3.2 provides descriptive statistics for the Nielsen household sample. In our subsample analysis, we classify households into lower-income and higher-income households, where households below the median income-to-poverty ratio are considered lower-income. The lower-income households are less likely to be married or Black and college-educated female head, but more likely to have at least one child, and female household heads below age 35. Higher-income households have larger household sizes than lower-income households

Table 3.3 summarizes the unit value and monthly food budget share for each food group by income level. A unit value is the ratio of the spending on that food group to the quantity purchased. There are some important caveats from unit values and food budget shares. The average unit values for all food groups, except red meat and nuts, is higher for higher-income households than those for lower-income households. These results are consistent with past studies that observed lower-income families use several cost minimization approaches to reduce the food budget (e.g. Beatty, 2010). Lower-income households reported higher monthly food budget shares for all food groups. On average, lower-income households' total food budget share is around 3 times higher than that of higher-income households.

3. ESTIMATION STRATEGY

A censored EASI incomplete demand system is estimated for 29 food groups and a *numéraire* good. The EASI model signifies superiority over other common demand specifications such as the Almost Ideal Demand System (AIDS) (Deaton and Muellbauer, 1980) and its variants. First, it is not restricted by the rank three limitations (Gorman, 1981) and allows for more flexible Engel curves. Second, the error term in the EASI can be interpreted as unobserved preference heterogeneity (Pendakur, 2009). This is important for the welfare analysis using household-level data because the error terms absorb some unobserved variations that demographics and prices cannot explain. Third, it allows for arbitrary Engel curves. The original EASI model was modified by Zhen et al. (2014) to account for censored purchases and endogenous prices.

Let a consumer maximize her utility by choosing over a vector of J-products that results in the budget shares $\boldsymbol{w} = [w^1, \dots, w^J\ ]$. The choice is constrained by the vector of prices $\boldsymbol{p} = [p^1, \dots, p^J\ ]$ and by the total amount of nominal expenditure x. The consumer minimizes acost function $C(\boldsymbol{p}, u) = x$ to reach a target utility level u. From this minimization of the cost, we can derive a Hicksian budget share demand function that depends on utility and prices: $w^j = w^j(\boldsymbol{p}, u)$. Hicksian demand is useful in measuring welfare changes as it allows the measurement of utility. However, the utility is difficult to observe in a reality where we usually observe paired quantities and prices. The relationship between the prices and quantities is described by a Marshallian demand function where the consumer maximizes utility while being constrained by prices and expenditures$w^j = w^j(\boldsymbol{p}, x)$. The Implicit Marshallian demand proposed by Lewbel and Pendakur (2009) depends not only on prices and expenditures but also on budget shares: $w^j =$

$w^j(\boldsymbol{p}, x, \boldsymbol{w})$. The implicit Marshallian demand function is flexible enough to allow for nonlinear Engel curves and random utilityparameters, accounting for preference heterogeneity.

The EASI model (Zhen et al. 2014; Zhen et al., 2021) is given by:

$$w_{iht}^{*} = \sum_{j=1}^{J} \alpha_{ij} \ln{(p_{jht})} + \sum_{j=1}^{J} \alpha_{ijy}\, y_{ht} \times \ln{(p_{jht})} + \sum_{j=1}^{J} \alpha_{ijz}\, z_{hK} \times \ln{(p_{jht})} + \sum_{r=1}^{L} \beta_{ir}\, y_{ht}^{r}$$

$$+\ \beta_{iz} z_{hK} \times y_{ht} + \sum_{k=1}^{K} g_{ik}\ z_{hkt} + \varepsilon_{hit,}$$

$$h = 1, \dots, H; i = 1, \dots J - 1; t = 1, \dots, T; \tag{1}$$

where w_{iht}^{*} is the latent budget share of food group I in period t for household h; J (=30) is the number of goods including the J^{th} composite numeraire good; H is the number of households; y_{ht} is real household income; L is the highest order of polynomial on y_{jht} to be determined empirically; p_{jht} is the price index of the jth good; K is the number of exogenous demand shifters; z_{hkt} is the kth demand shifter with z_{h1t} being a constant. The last demand shifter z_{hK} is the Google trend. $\alpha_{ij}, , \alpha_{ijy}, , \alpha_{ijz},\ \beta_{ir}, \beta_{iz}, g_{ik}$ are parameters, and ϵ_{hit} is residual. The EASI demand in Equation (1) is two-way because of the interactions between y_{ht} and P_{jht}. This allows the Hicksian price effects to vary with total expenditures and is unique to the EASI functional form. In contrast, AIDS allows only Marshallian price effects to differ for lower- and higher-income households through the income effects. The latent share w_{iht}^{*} is related to observed budget share w_{iht} according to $w_{iht} = \max{\{0, w_{iht}^{*}\}}$, where w_{iht} is calculated as the monthly food group expenditure divided by monthly household income. The variable y_{ht} is specified as the Stone price-deflated real income: $lnx_{ht} - \sum_{j=1}^{J} w_{jht} \ln{(p_{jht})}$, and lnx_{ht} is nominal monthly household income. The variable y_{ht} is endogenous because the food budget shares w_{iht} are in the Stone index. We use the extended AGLS estimator for censored equation systems (Zhen et al. 2014) to control for endogeneity in y_{ht} and p_{jht}. We instrument y_{ht} by $\bar{y}_{ht} = lnx_{ht} - \sum_{j=1}^{J} \bar{w}_j \ln(\bar{p}_{jht})$, where $\bar{w}_j$ is the sample mean budget share and $\bar{p}_{jht}$ is the instrumental variables for p_{jht} to be discussed in the following section.

In addition to a constant, we specify the demand shifters z_{hkt} to include household size, seven Census Division dummies, panel year dummies, and indicators for the presence of a female household head; female household head with a college degree; female household head below age 35; households with children; Black and White households. Incorporating Google trends-awareness measures for healthy food in the EASI allow us to examine whether or how preference evolves and the effects of food preference on food demand. Specifically, the interaction terms $z_{hK} \times p_{jht}$ and $z_{hK} \times y_{ht}$ allow the price and expenditure elasticities to change by the Google trend variable. As such, α_{ijz} and β_{iz} measures the changes in the impact of Google trend on food budget share and alter the price and expenditure elasticities, respectively.

Endogeneity

The price of this flexible demand system is that there are budget shares on both sides of the demand function. This introduces endogeneity in the model, which requires instruments to address. On the other hand, prices could be endogenous. Price endogeneity may be caused by supply-demand simultaneity, omitted variables, and measurement errors. Omitted variable bias may occur when households with preferences for certain foods and beverages are better than others to minimize food budget; or when households who value quantity over quality would rather purchase less expensive products than more expensive ones. Similarly, we have a case of reverse casualty if households have a higher demand for a food search for the lower process.

These behaviors are correlated with household food preferences, resulting in price endogeneity. Another potential reason for the price endogeneity is the unit value bias (Deaton, 1988). The price endogeneity would bias estimates of the causal effect of prices on demand. Using instrumental variables, we address potential price and real income/expenditure endogeneity bias in the EASI demand equation.

Price Indexes

We first constructed the Tornqvist price indexes for 29 food groups to calculate at the household month-level price index. The Tornqvist price indexes are defined as

$$P_{jht}^{0j} = \exp\left\{(0.5 \sum_{v \in v_{oj}} (s_v^0 + s_v^j) \ln (p_v^j / p_v^0)\right\} \quad (2)$$

where t is a quad week (month), s_v^0 and s_v^j are budget shares of product v in base 0 and entity j, respectively. The Törnqvist index is exact for the translog total or unit cost function, and the Fisher ideal index is exact for the quadratic mean of order two-unit cost function (Diewert 1976).

The price index for the *numeraire* is calculated. The Bureau of Economic Analysis produces the annual regional price parities (RPPs) to measure the cost-of-living differences across metropolitan statistical areas (MSA) in a given year. We multiply the 2006 RPPs at the MSA level with the Bureau of Labor Statistics' monthly consumer price index (CPI) for all items to create a panel of RPPs. We then link panel RPP with households based on county of residence and date of the survey. For households living in counties (home counties) outside of an MSA, we impute their RPP using the average RPP from other counties within a 500-mile radius, called donor counties, weighted by the inverse distance between the home county and donor counties[14]. Then back out the price index for the *numeraire* good J for household h in period t by

$$\ln(p_{jht}) = \frac{\ln(RPP_{ht}) - \sum_{j=1}^{J-1} \ln(p_{jht})}{\overline{w}_J} \tag{3}$$

where the first $J - 1$ good are the food groups of interest, and $\overline{w}_J$ is the sample mean budget share of the *numeraire*. The reason we use $\overline{w}_J$ rather than the household-and time-specific budget share w_{jht} in backing out p_{jht} is that any outliers in w_{jht} will create "out-of-whack" values of p_{jht}. Using $\overline{w_j}$ eliminates this problem.

Price Instrument

Following Allcott et al. (2019), we use retailer scanner data to create price instruments for price indexes. Using same-chain prices in other counties to increase instrument strength and can be argued that much of the between-chain price variations come from the differences in supply costs that help to identify demand curves (Allcott et al., 2019; DellaVigna and Gentzkow, 2019). Let $(prct, -m)$ denote the retailer r from county c in time t in all markets excluding market (MSA) m. The instrument for the price index p_{jht} is defined as

$$\bar{p}_{jht} = \frac{\sum_{r\in m} \sum_{r=1}^{R} \sum_{c=1}^{C} S_{rcht} \cdot (p_{rct}, -m)}{\sum_{r\in m} \sum_{r=1}^{R} \sum_{c=1}^{C} S_{rcht}} \tag{4}$$

where S_{rcht} is total spending of household h residing at county c at retailer r in time t.

The instruments for the *numeraire* good J for household h and month t is calculated as

[14] We obtain county distances from the National Bureau of Economic Research website: http://data.nber.org/data/county-distance-database.html

$$\ln(\hat{p}_{jht}) = \frac{\ln(\widehat{RPP}_{ht}) - \sum_{j=1}^{J-1} \ln(\hat{p}_{jht})}{\overline{w}_J} \tag{5}$$

where $\hat{p}_{jht}$ is the Allcott-type price instruments using the average price at other stores of the same chain visited by household h in period t., $\widehat{RPP}_{ht}$ is average RPP in other MSA in period t weighted by their inverse distance to h's home MSA.

4. RESULTS

To better understand the structural difference in the evolution of consumer preferences and examine how household food purchase decisions respond to changes in prices and income, we estimate demand models for 29 food groups and *numeraire*. These models provide a matrix of own and cross-price elasticities and income effects for the different food groups.

We first find the optimal value of log real total expenditure polynomials y_{ht}^{r} employing a series of tests on the joint significance of the β_{ir} beginning with $L = 2$. We chose a 4th order polynomial ($L = 4$) as the preferred specification for the Engel curves. We test whether the preference heterogeneity is associated with the Google trend variable. We also test for the joint significance of coefficients (α_{ijy}) on the interaction between price and total expenditure ($y_{ht} \times p_{jht}$) without imposing symmetry and homogeneity conditions. The test is significant with $\chi^2(870\ df) = 48392$ (870 degrees of freedom comes from the 29 budget share equations, each with 30 interaction terms). These test statistics suggest that Hicksian demand varies with total expenditure. Further, a test of joint significance of the coefficients (α_{ijz}) on the interaction between prices and Google trends ($z_{hK} \times p_{jht}$) strong reject the null of no interaction effects without imposing the symmetry and homogeneity restrictions on parameters of the latent budget share ($\chi^2(29\ df) = 664$, p-value <0.0001). This result indicates that the frequency of search for healthy food or items affects consumer preferences for food by changing the coefficients on total expenditure and price in the budget share equations. The coefficient of each own price instrument is positive and significant from the first stage regression of price indexes on a set of price instruments, demographics, real expenditure instruments and Census Division dummies.

Table 3.4 report the median Marshallian price elasticities of demand for the full sample. All own-price elasticities are negative, consistent with consumer theory, and precisely estimated. Household consumption for all food groups is more sensitive to own-price than cross-price changes, which is consistent with other demand model results and prior expectations. Twenty-seven out of 29 (exception being non-whole grain and sweets and candies) have median own-price elasticities greater than unity in absolute value; therefore, demands for these food groups are elastic. The estimated own-price elasticities are relatively higher (in absolute value) for dark green vegetables, whole grains, soups, poultry, and fruit juice, indicating these food groups are relatively sensitive to their own prices compared to other groups. In contrast, the demand for nonwhole grains, sugar, sweets and candies, other vegetables, and processed meat are less sensitive to own price.

Figure 3.2 plots the median own-and cross-price elasticities in the heat map for the lower-income households, while Figure 3.3 depicts the median price elasticity matrix for higher-income households. Demand for all food groups is own-price elastic irrespective of household groups. One noteworthy observation is that own-price elasticities become less negative as household income increases. We find this pattern for most food groups, except for non-whole grains and other goods. Lower-income households are more responsive to own-price changes than higher-income households, which is consistent with our expectations. Many cross-price elasticity estimates are consistent with previous studies and prior expectations. For example, we found substitution between low/reduced fat and whole fat milk and whole-grain and non-whole grains.

Figure 3.4 depicts expenditure elasticities of food demand with respect to total expenditure for the overall sample and by lower-and higher-income households. It shows that expenditure elasticity estimates for all food groups are inelastic, that is, an increase in total expenditure leads to a positive but less than proportionate increase in demand. The inclusion of Polynomial expenditure in the demand system allows the slope of Engel curve and expenditure elasticity to be less affected by the function forms. Except for diet soft drink, the median expenditure elasticities are less than unity, which is consistent with the theory and prior expectation that foods are necessities.

We observe considerable differences in the effects of total expenditure on food demand among lower-and higher-income households. Expenditure elasticities for all food groups are greater for the lower-income household than for higher-income households, which is consistent with Engel's law. That is, food demand for higher-income households is less elastic with respect to changes in total expenditure than that of lower-income households. Results show that expenditure elasticities for non-whole grains, sweets, and candies, fats and oils, spices, potato products, milk drinks, and cheeses are relatively small, while demand for orange vegetables, diet soft drinks, dark green vegetables, whole grains, and nuts are greater for both income groups. These results are intuitive as the household income increases; they tend to spend more on healthy food and shift away from unhealthy food.

Next, we seek to understand the role of Google trends in changing food preferences. Figure 3.5 plots coefficients of the interaction between Google trend and prices (α_{ijz}) of the EASI demand in equation (1). It shows that the Google trend has contributed to changes in household responsiveness to price changes in the food demand. All coefficients on the diagonal are statistically significant. The positive value of α_{ijz} on the diagonal suggest an increase in Google trend likely reduces the magnitude (less negative) of own-price elasticities. The following food groups have positive coefficients of interaction between Google trend and prices: all whole grain categories, all vegetables except canned vegetables, whole fruits, fruit juice, low-fat milk and milk products, cheese, nuts, and diet soft drinks. The positive coefficients on these food groups make the overall effects of an increase in own price less negative; that is, household demand for the above-mentioned food group tends to be less responsive to own price changes when Google search volume increases. Not surprisingly, coefficients on red meat, processed meat, and regular soft drinks are negative, indicating that households are likely to become more sensitive to their own price changes as the Google trend increases.

5. CONCLUSION

We estimate the EASI incomplete demand system with nationally representative data to estimate the 29 food groups and *numeraire* that accounts for all food at home categories. We create price instruments using same-chain prices in other locations to address the issue of price endogeneity in the demand system.

Consumer information and awareness of healthy food, income, and prices are important factors in shaping consumer food preferences. There are various means that consumers can utilize to be informed about the healthfulness of food purchases, Google search being one of them. In the last decade, Google search representing healthy food items such as sugar-free, diet coke, low calorie, zero sugar, low carbohydrate, low fat, whole grains, whole food, high fiber, and low sodium has increased considerably. At the same time, household food preferences and diet reportedly shifted from regular soft drinks to low-sugar soft drinks, non-whole grains to whole grains and whole milk to low-fat milk. This increase in Google search volume for healthy food items may likely contribute to a change in household food preferences leading to an improvement in diet quality. Hence, there is a need to examine this relationship using an empirical approach.

This study analyzes the impact of Google search on healthy food items on household food preferences in the United States by incorporating the Google trend variable into the EASI demand system. This process allows the price and expenditure elasticities to change with the Google trend variable, thereby altering the price and expenditure elasticities. Our results indicate that the Google trend plays an important role in shaping household food preferences. Specifically, an increase in Google search for healthy food items increases the demand for whole grains, fruit and vegetables, low-fat milk and milk products, buts and diet soft drinks while reducing the demand for regular soft drinks and red and processed meat. Google-induced household food preference changes likely have policy implications. Given the differences in internet use across the household characteristics and geographical locations, sub-sample analysis among the different population subgroups for preference heterogeneity may provide insight into the role of Google search for food choices.

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Table 3.1. Association between GPQI score and Google search index

	GPQI score	
Variable	(1)	(2)
Google trend		0.002***
		(0.000)
Panel year		
2007	0.023***	
	(0.001)	
2008	0.029***	
	(0.001)	
2009	0.023***	
	(0.001)	
2010	0.036***	
	(0.001)	
2011	0.039***	
	(0.001)	
2012	0.059***	
	(0.001)	
2013	0.064***	
	(0.001)	
2014	0.059***	
	(0.001)	
2015	0.056***	
	(0.001)	
2016	0.045***	
	(0.001)	
2017	0.037***	
	(0.001)	
Household fixed effects	X	X
N	8334756	8334756
R2	0.421	0.422

Table 3.2. Characteristics of Nielsen Consumer Panel households

Variable	Lower-income	Higher-income
Age of HH head	57.138 (13.957)	55.504 (11.927)
Household size	2.505 (1.477)	2.248 (1.054)
Share of HH with at least one child	0.268 (0.443)	0.185 (0.388)
Share of HH are married	0.566 (0.496)	0.693 (0.461)
Share of HH are Black	0.095 (0.293)	0.098 (0.297)
Share of HH are White	0.839 (0.367)	0.822 (0.382)
Share of HH with female head below age 35	0.171 (0.376)	0.162 (0.368)
Share of HH with college educated female head	0.263 (0.440)	0.481 (0.500)
Household income $	34703.430 (18290.820)	84898.040 (26666.750)
N	4166424	4168352

Note: Mean values presented are followed by standard deviation. HH represents household size.

Table 3.3. Unit value and budget shares by household types

	Unit value ($/100 gram)				Food budget share			
	Lower-income		Higher-income		Lower-income		Higher-income	
Food group	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Whole grains-flour	0.4058	0.0677	0.4097	0.0677	0.0018	0.0042	0.0006	0.0009
Whole grains-cereals	0.7094	0.0880	0.7110	0.0901	0.0006	0.0025	0.0002	0.0006
Whole grains-grains	0.9051	0.1343	0.9096	0.1370	0.0019	0.0051	0.0007	0.0012
Non whole grains	0.6534	0.1079	0.6617	0.1096	0.0113	0.0174	0.0036	0.0034
Potato products	0.5351	0.0898	0.5414	0.0882	0.0026	0.0055	0.0008	0.0011
Dark green vegetables	0.6997	0.0761	0.7000	0.0788	0.0002	0.0012	0.0001	0.0003
Orange vegetables	0.3044	0.0476	0.3055	0.0485	0.0003	0.0013	0.0001	0.0003
Canned vegetables	0.2430	0.0444	0.2442	0.0444	0.0009	0.0026	0.0003	0.0006
Other vegetables	0.7149	0.1318	0.7213	0.1294	0.0045	0.0088	0.0019	0.0023
Whole fruits	0.7763	0.1451	0.7839	0.1476	0.0041	0.0096	0.0017	0.0025
Fruit juice	0.2168	0.0312	0.2170	0.0314	0.0021	0.0059	0.0008	0.0013
Whole milk & products	0.3187	0.0445	0.3208	0.0433	0.0020	0.0071	0.0006	0.0013
Low fat milk & products	0.1845	0.0294	0.1848	0.0289	0.0039	0.0081	0.0014	0.0018
Cheese	0.9623	0.1744	0.9689	0.1736	0.0048	0.0090	0.0017	0.0020
Milk drinks	0.6847	0.1107	0.6899	0.1116	0.0031	0.0071	0.0010	0.0015
Red meat	1.8389	0.2491	1.8354	0.2380	0.0033	0.0105	0.0011	0.0026
Poultry	0.9628	0.1056	0.9663	0.1050	0.0016	0.0055	0.0006	0.0013
Seafoods	1.3415	0.1949	1.3462	0.1943	0.0017	0.0062	0.0006	0.0016
Processed meat	2.1128	0.3674	2.1406	0.3707	0.0088	0.0204	0.0029	0.0059
Nuts	1.0755	0.1729	1.0750	0.1753	0.0022	0.0063	0.0009	0.0016
Eggs	0.2736	0.0633	0.2751	0.0615	0.0011	0.0026	0.0003	0.0005
Fats and oils	0.5860	0.1111	0.5930	0.1126	0.0033	0.0065	0.0011	0.0014
Spices	0.8080	0.1411	0.8178	0.1451	0.0039	0.0075	0.0013	0.0016
Coffee & tea	5.0760	0.7405	5.0988	0.7420	0.0031	0.0084	0.0011	0.0021
Regular soft drink	0.1724	0.0274	0.1736	0.0271	0.0045	0.0118	0.0011	0.0022
Sweets & candies	0.6550	0.1127	0.6641	0.1152	0.0089	0.0153	0.0028	0.0032
Soups	0.4619	0.0822	0.4681	0.0832	0.0020	0.0054	0.0006	0.0011
Diet soft drinks	0.1090	0.0142	0.1098	0.0151	0.0021	0.0081	0.0008	0.0019
Frozen entries	0.7148	0.1149	0.7200	0.1147	0.0018	0.0042	0.0006	0.0009
N	4166423		4168352					

Table 3.4. Median Marshallian price elasticities over all households

	With respect to the price of food group														
Demand for food group	Whole grains-flour	Whole grains-cereals	Whole grains-grains	Non whole grains	Potato products	Dark green vegetables	Orange vegetables	Canned vegetables	Other vegetables	Whole fruits	Juice	Whole milk & products	Low fat milk & products	Cheese	Milk drinks
Whole grains-flour	**-1.982**	-0.052	-0.154	0.337	0.021	-0.027	0.102	-0.190	-0.004	-0.002	-0.143	0.035	-0.154	-0.371	-0.062
Whole grains-cereals	-0.129	**-4.515**	-0.075	0.522	0.082	0.027	-0.063	-0.089	-0.005	-0.055	-0.189	-0.005	-0.171	-0.168	0.077
Whole grains-grains	-0.069	-0.103	**-2.465**	0.310	0.016	0.091	0.081	-0.096	0.085	0.014	-0.103	-0.004	-0.097	-0.256	-0.107
Non whole grains	-0.004	-0.001	-0.172	**-0.734**	-0.019	0.120	0.102	-0.097	0.053	0.032	-0.114	-0.035	-0.081	-0.250	-0.102
Potato products	0.006	0.147	-0.157	0.277	**-1.593**	0.136	0.114	-0.138	0.024	0.093	-0.097	-0.040	0.015	-0.322	-0.101
Dark green vegetables	-0.197	0.025	-0.081	0.384	0.052	**-5.865**	0.174	-0.112	0.037	-0.076	-0.134	0.057	0.028	-0.233	0.100
Orange vegetables	-0.106	-0.183	-0.112	0.304	-0.026	-0.292	**-2.634**	-0.202	-0.070	0.148	-0.280	-0.038	-0.101	-0.509	0.055
Canned vegetables	-0.074	0.034	-0.079	0.425	-0.035	0.154	0.031	**-3.074**	0.016	0.086	-0.109	-0.014	-0.017	-0.346	0.035
Other vegetables	-0.034	0.081	-0.081	0.252	0.001	-0.128	0.038	-0.203	**-1.341**	0.021	-0.123	-0.035	-0.060	-0.458	0.002
Whole fruits	-0.017	0.046	-0.088	0.229	0.029	-0.079	0.013	-0.221	-0.092	**-1.541**	-0.188	-0.078	-0.169	-0.375	-0.072
Juice	-0.014	-0.031	-0.050	0.418	-0.012	0.066	-0.024	-0.077	0.027	0.016	**-3.037**	-0.076	-0.114	-0.204	0.070
Whole milk & products	0.058	0.017	-0.021	0.305	0.007	-0.138	0.064	-0.087	-0.024	0.066	-0.137	**-2.859**	0.503	-0.361	0.018
Low fat milk & products	-0.084	-0.193	-0.143	0.216	0.025	0.053	0.060	-0.122	-0.006	-0.012	-0.201	0.251	**-1.745**	-0.270	-0.101
Cheese	-0.031	0.087	-0.125	0.318	0.029	0.016	0.073	-0.093	-0.045	0.069	-0.078	-0.024	-0.001	**-2.264**	0.002
Milk drinks	-0.022	0.167	-0.142	0.306	0.002	0.329	0.194	-0.119	0.117	-0.072	-0.064	0.003	-0.104	-0.244	**-2.224**

Table 3.4. Median Marshallian price elasticities over all households (Continued)

Demand for food group	With respect to the price of food group														
	Red meat	Poultry	Seafoods	Processed meat	Nuts	Eggs	Fats and oils	Spices	Coffee & tea	Regular soft drink	Sweets & candies	Soups	Diet soft drinks	Frozen entries	Other goods
Whole grains-flour	0.024	0.002	0.104	0.099	-0.075	-0.070	-0.167	-0.012	0.099	0.155	0.294	-0.057	0.026	-0.215	-0.112
Whole grains-cereals	0.212	0.067	0.200	0.234	-0.138	0.063	-0.107	0.082	0.094	0.244	0.341	-0.161	0.160	-0.108	0.083
Whole grains-grains	0.003	-0.006	0.055	0.071	-0.183	-0.047	-0.081	-0.041	0.013	0.087	0.209	-0.031	-0.040	-0.224	-0.406
Non whole grains	-0.049	0.041	0.040	0.054	-0.042	-0.028	-0.125	0.001	0.026	0.041	0.161	-0.111	-0.043	-0.232	-0.162
Potato products	-0.025	0.059	0.138	0.035	0.034	-0.039	-0.220	-0.047	0.052	0.055	0.218	-0.220	-0.024	-0.278	-0.847
Dark green vegetables	0.316	-0.119	0.221	0.093	0.011	-0.104	-0.158	0.015	0.188	0.257	0.416	-0.099	0.304	-0.133	0.378
Orange vegetables	0.265	0.177	0.246	0.331	-0.048	0.060	-0.336	-0.010	0.116	0.298	0.282	-0.477	0.122	-0.268	-0.423
Canned vegetables	0.048	0.098	0.330	0.158	0.036	-0.017	-0.219	-0.025	0.107	0.205	0.349	-0.370	0.186	-0.153	-0.737
Other vegetables	0.112	0.144	0.059	0.158	-0.018	-0.060	-0.282	-0.044	0.040	0.170	0.290	-0.097	0.040	-0.248	0.240
Whole fruits	0.113	0.131	0.069	0.142	-0.124	-0.122	-0.268	-0.039	0.030	0.080	0.211	0.145	-0.067	-0.169	0.296
Juice	0.082	0.133	0.044	0.180	-0.029	-0.024	-0.143	0.068	0.103	0.119	0.310	-0.086	0.138	-0.156	0.338
Whole milk & products	0.088	0.164	0.128	0.108	0.009	0.067	-0.243	-0.002	-0.007	0.092	0.226	-0.230	0.126	-0.165	0.028
Low fat milk & products	0.079	0.109	0.045	0.137	-0.066	-0.029	-0.138	0.028	0.043	0.103	0.165	-0.045	-0.031	-0.276	-0.143
Cheese	0.085	0.081	0.114	0.113	0.010	-0.017	-0.206	-0.012	0.098	0.174	0.280	-0.180	0.067	-0.255	-0.326
Milk drinks	-0.001	0.014	0.079	0.070	0.004	-0.165	-0.139	0.028	-0.026	0.023	0.251	0.161	-0.119	-0.233	-0.179

Table 3.4. Median Marshallian price elasticities over all households (Continued)

Demand for food group	With respect to the price of food group: Whole grains-flour	Whole grains-cereals	Whole grains-grains	Non whole grains	Potato products	Dark green vegetables	Orange vegetables	Canned vegetables	Other vegetables	Whole fruits	Juice	Whole milk & products	Low fat milk & products	Cheese	Milk drinks
Red meat	0.004	0.375	-0.140	0.305	-0.160	0.257	0.155	-0.333	-0.076	0.145	-0.094	0.042	0.054	-0.705	-0.269
Poultry	-0.053	0.064	-0.112	0.390	-0.085	-0.022	0.166	-0.269	-0.197	0.103	-0.169	0.055	0.042	-0.569	-0.159
Seafoods	-0.044	0.109	-0.074	0.317	-0.057	0.106	0.130	-0.032	-0.097	0.053	-0.172	-0.004	-0.050	-0.399	-0.020
Processed meat	-0.054	0.192	-0.100	0.212	-0.030	0.206	0.117	-0.094	0.047	0.059	-0.062	0.034	0.048	-0.298	-0.071
Nuts	-0.055	-0.058	-0.096	0.350	0.020	-0.049	-0.060	-0.059	-0.017	-0.039	-0.098	-0.088	-0.054	-0.301	0.070
Eggs	-0.056	0.025	-0.045	0.301	-0.091	-0.137	-0.021	-0.121	-0.134	0.096	-0.181	-0.057	0.078	-0.459	0.068
Fats and oils	0.010	0.154	-0.040	0.368	-0.005	-0.022	0.035	-0.144	-0.073	0.062	-0.116	-0.082	0.011	-0.439	0.018
Spices	0.003	0.148	-0.102	0.335	-0.043	0.011	0.049	-0.171	-0.048	0.068	-0.122	-0.073	0.009	-0.431	0.010
Coffee & tea	-0.022	0.015	-0.068	0.347	0.046	0.022	0.133	-0.125	0.059	0.057	-0.083	-0.085	0.054	-0.278	-0.106
Regular soft drink	0.031	0.318	-0.174	0.235	-0.086	0.296	0.149	-0.160	0.142	-0.014	-0.123	-0.065	0.098	-0.166	-0.193
Sweets & candies	0.017	-0.076	-0.071	0.198	-0.046	-0.077	-0.110	0.055	0.034	0.051	-0.101	-0.119	-0.014	-0.191	0.034
Soups	-0.009	-0.315	-0.034	0.410	-0.058	-0.039	-0.109	0.047	-0.051	0.307	-0.160	-0.058	0.021	-0.254	0.191
Diet soft drinks	-0.018	0.141	-0.237	0.212	-0.026	0.223	0.150	-0.084	0.106	0.019	0.080	0.121	-0.068	-0.312	-0.177
Frozen entries	0.030	-0.001	-0.135	0.314	-0.002	-0.019	0.185	0.006	0.062	0.130	-0.115	-0.012	-0.053	-0.256	-0.059
Other goods	0.006	-0.002	0.023	-0.045	0.005	-0.004	-0.010	0.021	0.001	-0.004	0.025	0.011	0.005	0.056	0.014

Table 3.4. Median Marshallian price elasticities over all households (Continued)

Demand for food group	With respect to the price of food group: Red meat	Poultry	Seafoods	Processed meat	Nuts	Eggs	Fats and oils	Spices	Coffee & tea	Regular soft drink	Sweets & candies	Soups	Diet soft drinks	Frozen entries	Other goods
Red meat	**-2.297**	0.412	0.079	0.038	0.214	-0.135	-0.381	-0.135	0.208	0.062	0.362	-0.184	0.032	-0.399	-0.631
Poultry	-0.091	**-3.208**	0.077	0.173	0.070	-0.095	-0.336	-0.041	0.095	0.136	0.363	-0.246	0.131	-0.232	0.192
Seafoods	0.162	0.126	**-2.754**	0.124	0.015	-0.087	-0.248	0.019	0.127	0.187	0.390	-0.133	0.170	-0.277	0.804
Processed meat	-0.055	-0.022	0.055	**-1.398**	0.006	-0.091	-0.124	-0.041	0.072	0.048	0.261	-0.052	-0.013	-0.232	-0.215
Nuts	0.175	0.195	0.116	0.139	**-3.006**	0.000	-0.200	0.012	-0.015	0.177	0.236	-0.112	0.024	-0.120	-0.188
Eggs	0.051	0.098	0.045	0.068	-0.030	**-1.294**	-0.386	-0.006	0.063	0.174	0.231	-0.312	0.109	-0.185	-0.059
Fats and oils	0.174	0.085	0.124	0.156	-0.016	-0.069	**-2.021**	-0.077	0.078	0.176	0.322	-0.149	0.098	-0.143	-0.314
Spices	0.032	0.076	0.132	0.122	-0.018	-0.016	-0.295	**-1.515**	0.057	0.137	0.310	-0.224	0.065	-0.172	-0.263
Coffee & tea	0.017	0.061	-0.016	0.094	-0.077	-0.033	-0.171	-0.006	**-2.388**	0.145	0.294	-0.093	0.026	-0.180	-0.047
Regular soft drink	-0.130	-0.095	0.054	-0.077	0.050	-0.087	-0.137	-0.084	0.063	**-1.507**	0.202	0.111	-0.062	-0.136	-0.316
Sweets & candies	0.060	0.128	-0.043	0.051	-0.168	0.073	-0.172	0.000	-0.114	0.090	**-0.990**	-0.272	0.014	-0.170	-0.332
Soups	0.144	0.173	0.167	0.206	-0.006	0.207	-0.084	0.061	0.036	0.299	0.293	**-3.254**	0.276	-0.275	-0.282
Diet soft drinks	-0.026	0.031	0.060	-0.052	-0.078	-0.048	-0.123	-0.048	0.044	0.196	0.161	0.035	**-2.490**	-0.307	-0.847
Frozen entries	-0.102	-0.005	0.024	0.071	0.029	0.043	-0.078	0.056	0.040	0.093	0.210	-0.242	-0.038	**-2.304**	0.060
Other goods	0.006	-0.002	-0.002	-0.007	0.012	0.006	0.032	0.005	0.001	-0.013	-0.038	0.025	0.005	0.046	**-0.975**

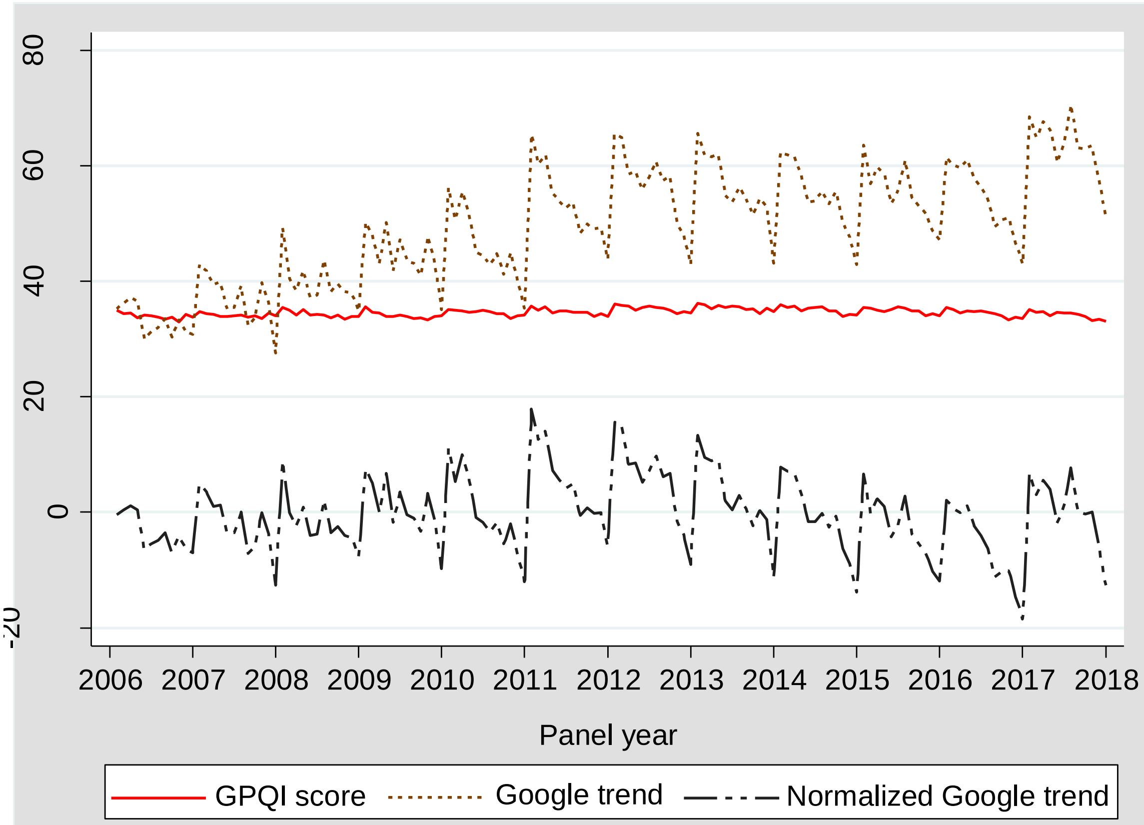

Figure 3.1. Time series of GPQI score and Google search index for healthy food topics from January 2006 to December 2017

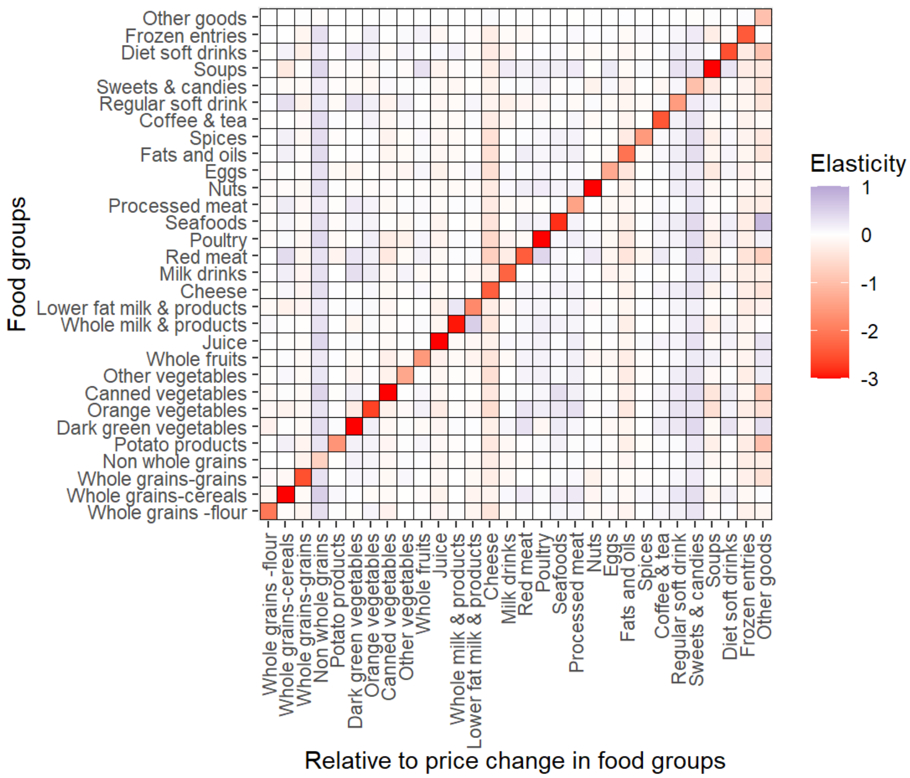

Figure 3.2. Median Marshallian price elasticities for lower-income households

Note: The cross-price elasticities depict the % change in quantity demanded of the row's food group in response to a 1% increase in the price of the column's food group.

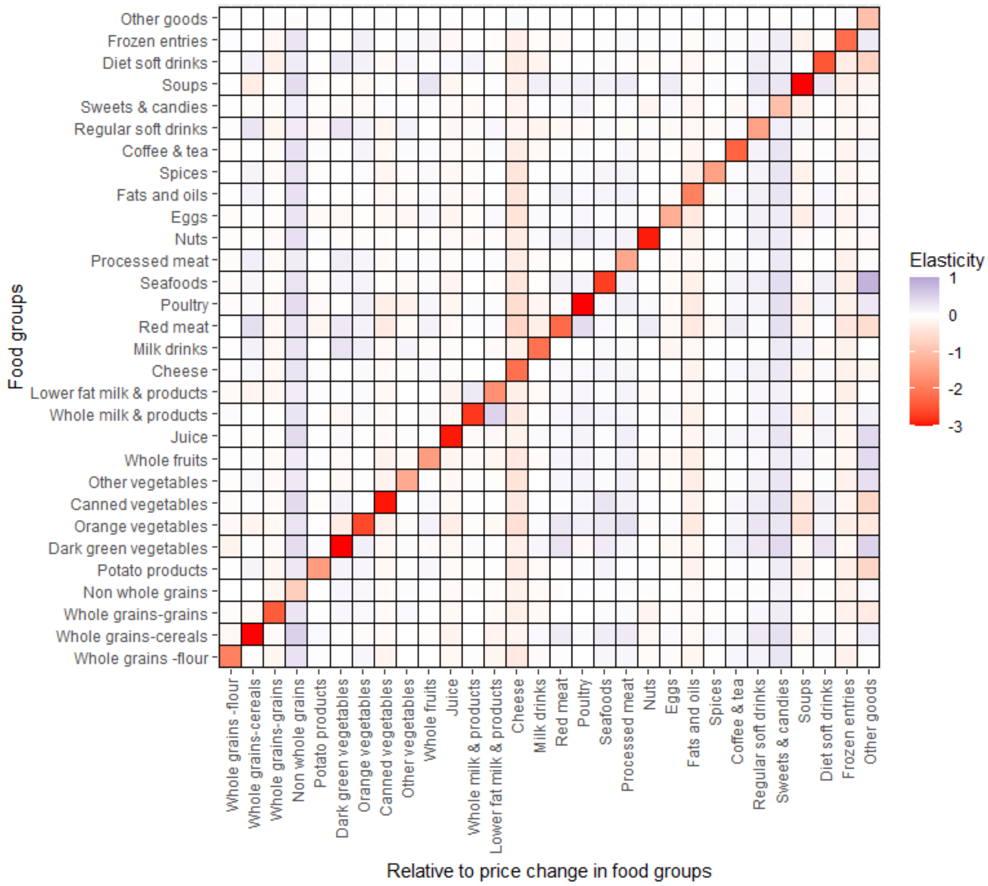

Figure 3.3. Median Marshallian price elasticities for higher-income households

Note: The cross-price elasticities depict the % change in quantity demanded of the row's food group in response to a 1% increase in the price of the column's food group.

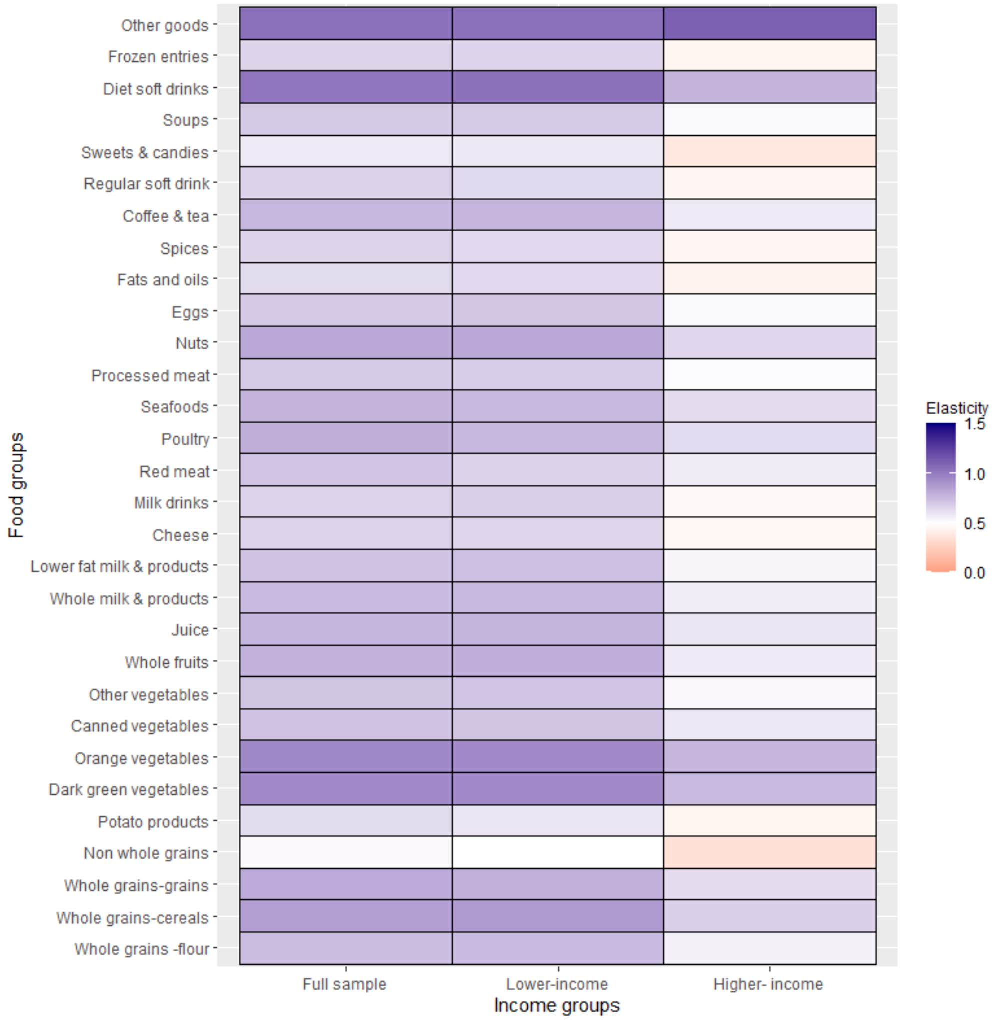

Figure 3.4. Elasticities of food demand with respect to changes in total expenditures by total by lower- and higher-income households

Note: Figure shows the median % change in quantity demanded of each food group with respect to a 1% change in total expenditures.

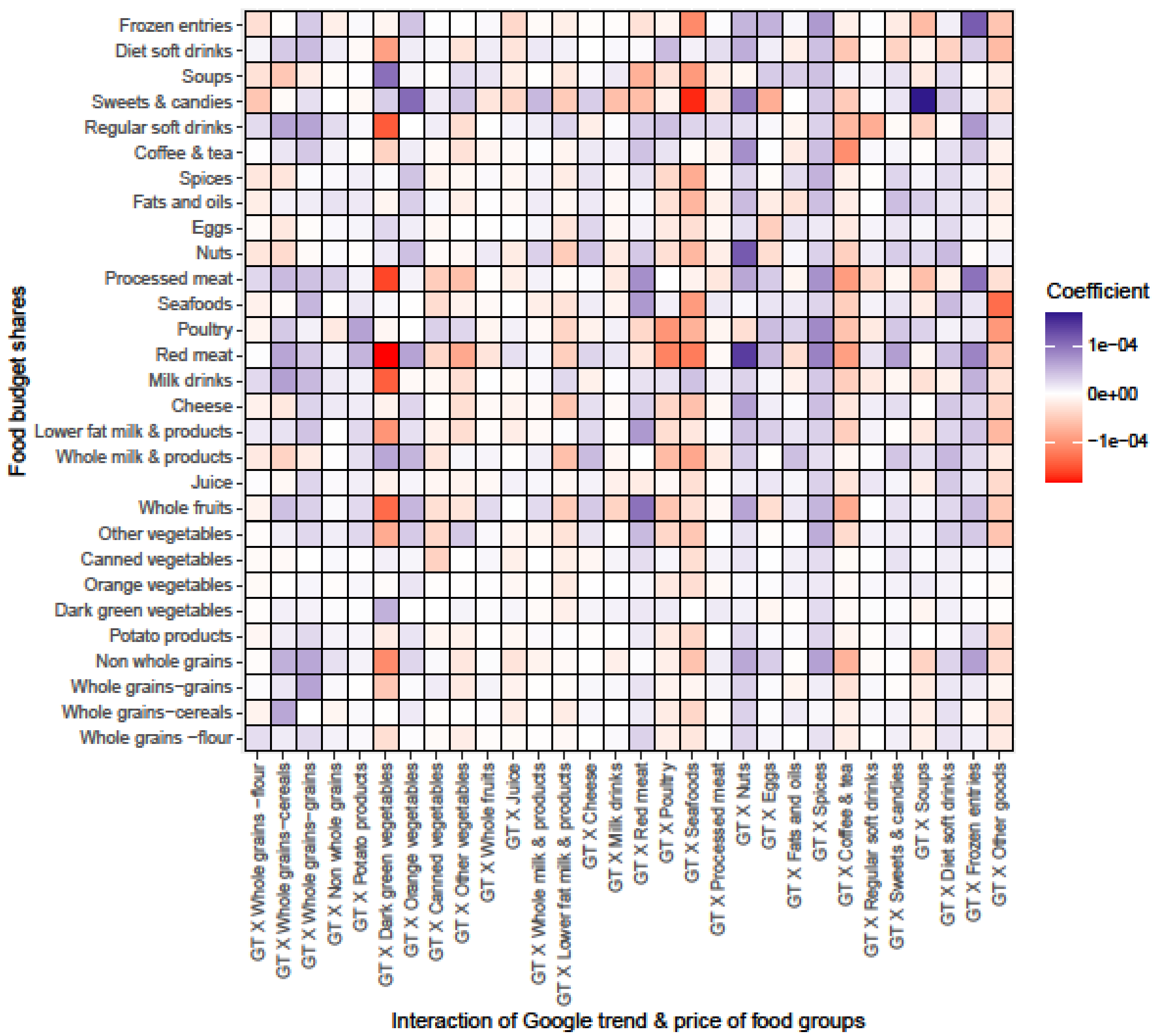

Figure 3.5. Coefficients of interaction between Google trends and prices in the budget share equation of the demand system