The Minimum Wage and Consumer Nutrition

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ABSTRACT

The USDA estimates that 1 in 9 U.S. households is "food insecure": unable to purchase sufficient, or healthy food. Public policy advocates and politicians have pointed to the prevailing federal minimum wage as a culprit, labeling it a "starvation wage." This study examines whether and to what extent increases to the minimum wage have improved the quantity and nutritional quality of food purchased by minimum wage earners, and what implications these potential changes in consumer behavior have for marketers. We show that households likely to be earning the minimum wage increase their calories purchased in response to minimum wage increases, and that these gains are predominantly found among households purchasing the least amount of food prior to the minimum wage rising. While we do not find evidence that the average household improves the nutritional content of calories purchased, we do find evidence that the least healthful households (as measured by past purchases) buy more healthful foods in response to rising minimum wages. Overall, our findings suggest that higher minimum wages may not only help households afford more calories, but also encourage some households to purchase more healthful calories. Additionally, we find an increased openness among minimum wage households to purchasing new grocery items. This openness to trying previously unpurchased products offers promotion and product line planning opportunities to manufacturers. It also offers retailers with a nutrition-friendly brand image an opportunity to nudge consumers towards purchasing more healthful foods.

Keywords: minimum wage, nutrition & nudges, food insecurity, nutritional inequality, marketing & public policy

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In 2018, the US department of agriculture estimated that approximately 1 in 9 U.S. households[1] (and 1 in 7 U.S. households with children) was "food insecure" at some point during the year, defined as being unable to acquire healthy food ("low food security") or a sufficient amount of food ("very low food security").[2] Based on 2020 Current Population Survey estimates, this pattern has grown worse since - with nearly 1 in 4 households with children experiencing food insecurity.[3] Among food insecure households, nearly 40% struggle to consume sufficient calories (Coleman-Jensen et al., 2014). Food insecurity has been linked to a wide variety of negative health outcomes, including iron deficiency anemia (Eicher-Miller et al., 2009), depression and anxiety (Whitaker et al., 2006), asthma (Kirkpatrick et al., 2010), diabetes (Seligman et al., 2007), chronic disease (Seligman et al., 2010), and obesity (Holben and Taylor, 2015). The harm food insecurity can do to a household also extends beyond these direct effects on health. A lack of sufficient or healthy food has been linked to lower cognitive function in children (Hoyland et al., 2009), along with diminished academic performance (Gassman-Pines and Bellows, 2018) and higher rates of disciplinary infractions at school (Gennetian et al., 2016).

A central tenet of the work on food insecurity is that low- and poor-calorie consumption is driven by constrained financial resources (Newell et al., 2014). The USDA reports that 98% of the respondents to the December 2018 Current Population Survey Food Security Supplement who suffered from very low food security worried that their food would run out before they obtained money to purchase more, and 96% who suffered from either low or very low food security reported that they could not afford to eat balanced meals (Figure 1). Public policy advocates and politicians alike have brought attention to the link between wages and food insecurity, branding the prevailing hourly federal minimum wage of $7.25 a "starvation wage."[4] In spite of the critical connection between wages and food insecurity, research exploring the link between the minimum wage and

[1] https://www.ers.usda.gov/data-products/ag-and-food-statistics-charting-the-essentials/food-security-and-nutrition-assistance/?topicId=14875 - retrieved Nov 15, 2019.

[2] https://www.ers.usda.gov/topics/food-nutrition-assistance/food-security-in-the-us/definitions-of-food-security/ - retrieved Nov 15, 2019.

[3] https://www.brookings.edu/blog/up-front/2020/07/09/about-14-million-children-in-the-us-are-not-getting-enough-to-eat/ - retrieved Sep 1, 2020.

[4] https://thehill.com/blogs/ballot-box/dem-primaries/240871-sanders-calls-minimum-wage-a-starvation-wage - retrieved Nov 15, 2019.

consumer nutrition is surprisingly scant.

This study seeks to document *whether* and to what extent increases to the minimum wage allow minimum wage-earning households to purchase more and/or healthier food, and *how* their shopping baskets are affected as a result.[5] Prior research has shown that as households spend more on food, they purchase a wider variety of foods, perhaps as a means of combating diminishing returns to quantity (Li, 2013). Research has also suggested that some low-income households may be averse to spending money on new foods that they may not like (Daniel, 2016). We examine whether minimum wage households that purchased more food (in response to the minimum wage rising) did so by buying universal product codes (UPCs) they had not purchased before. We also examine whether households that purchased more healthful food (in response to rising minimum wages) did so by changing the types of food they were eating (e.g., by eating more vegetables), or by purchasing more healthful versions of the foods they were already eating (e.g., buying more healthful UPCs without changing which categories they buy from). We discuss how answers to these questions may be of interest to marketers.

To our knowledge, only two research papers have previously tried to examine the relationship between the minimum wage and food purchases. Using cross-sectional data from phone surveys of households with a high-school degree or less (a proxy for minimum-wage earners), Pohl et al. (2017) find a modest relationship between the minimum wage and self-reported consumption of a very narrow subset of food purchases - fruit and vegetables. Newell et al. (2014) estimate how much money minimum wage-earning households would in theory need in order to become food secure, but do not explore whether the minimum wage itself is causally related to food purchases.

A broader stream of research on food insecurity has focused on the USDA's Supplemental Nutrition Assistance Program ("SNAP", or "food stamps") and has concluded that SNAP reduces self-reported instances of food insecurity (Mabli et al., 2013; Nord and Golla, 2009; and Kreider et al., 2012). However, SNAP assistance is a restricted form of funding that can only be used to purchase food. In fact, Hastings and Shapiro (2018) estimate the marginal propensity to consume

[5] Our data only contain information on food purchases, precluding our ability to credibly comment on food consumption (or food waste) at the household level.

food from SNAP payments to be 0.5 to 0.6, which they note is quite a bit larger than previously reported estimates of the marginal propensity to consume food from cash (0.1). It is therefore unclear whether higher *wages* would be allocated to food to the same extent as SNAP, especially given other financial burdens lower-income households may face (e.g., rent, bills). Notably, Aaronson et al. (2012) find that minimum wage earners have tended to allocate a dominant share of increased wages towards the purchase of durable goods. More recently, Australian citizens who withdrew retirement funds early amid the COVID-19 pandemic spent more on gambling than at the grocery store (the third largest category of spending). For these households, spending at the grocery store, restaurants, and cafes together made up only 18.4% of their spending from these funds.[6] Thus, it is not necessarily obvious that rising minimum wages will encourage the purchase of more food and alleviate food insecurity. It is similarly unclear whether higher minimum wages could motivate such households to choose different foods in a grocery setting (e.g., new/previously unpurchased or healthier UPCs).

To explore this, we combine ten years of data from the Nielsen Homescan panel data set with a proprietary dataset on the content of nutrition labels. We examine within-household changes in (1) calories purchased, and (2) the nutritional content of those calories, as measured by two previously established health indices: a) the Healthy Eating Index (Allcott, Diamond, Dube, Handbury, Rahkovsky and Schnell, 2019) which is based on the U.S. Dept. of Agriculture (USDA)'s Healthy Eating Index guidelines (hereafter referred to as the "USDA" health index), and b) the Nutrient Profiling Model, which is based on the U.K. Food Standards Agency (hereafter referred to as the "UK FSA" health index). We estimate the impact of the minimum wage on these measures by comparing the purchase behavior of households that earn up to the minimum wage (our "treatment" group) with that of households that earn just above the minimum wage (our "control" group).

We show that the minimum wage has a strong impact on minimum wage earners' ability to purchase calories—we estimate the elasticity of calories purchased with respect to the minimum wage to be around 0.4. We also show that this effect persists for those households in the data that

[6]https://www.abc.net.au/news/2020-06-01/superannuation-withdrawals-spent-on-gambling-alcohol-takeaway/12306710 - retrieved Jan 15, 2021.

reported never receiving SNAP assistance, who are presumably better off financially than those who have received SNAP. While we cannot observe whether or not a household has a sufficient number of calories to eat, the effect of the minimum wage on calories purchased appears to be driven primarily by the households that were purchasing the fewest calories prior to the minimum wage rising. The calories purchased by these households are more than unit-elastic with respect to the minimum wage.

On the other hand, we find little evidence of a change in the nutritional value of calories purchased in response to a minimum wage increase. We estimate the average impact of the minimum wage on nutritional content, as represented by two holistic health indices, to be near-zero. Nonetheless, we find evidence suggesting that some minimum wage households (those that previously had the least healthful shopping baskets) do improve the nutritional content of their shopping basket in response to rising minimum wages. This improvement appears to be counteracted by a worsening of the dietary health index of households that had the most healthful shopping baskets before the minimum wage in their area rose.[7] These opposing effects lead to a null overall effect of rising minimum wages on the dietary health of minimum wage households.

The finding that few households see an improvement in the average healthfulness of their purchased food when their *internal* capacity to buy food improves (due to rising wages) complements a growing body of literature showing that households are not easily nudged to eat healthier by a host of *supply-side* forces. Bollinger et al. (2021) find only a modest increase in the purchase of nutritious foods in Canada after the implementation and promotion of the Guiding Stars nutrition labeling system. Relatedly, using a field experiment, Dubois et al. (2020) find that the impact of front-of-package nutrition labels was 17 times smaller than in comparable lab studies. The most successful of the labels under study (Nutri-Score) improved the nutritional score of labeled foods purchased by only 2.5%. Allcott, Diamond, Dube, Handbury, Rahkovsky and Schnell (2019) find that neighborhood environments do not contribute meaningfully to nutritional inequality between income groups. In fact, the entry of supermarkets into "food deserts" has a meager effect on healthy

[7]We discuss subsequently that these patterns are unlikely to be a result of potential regression to the mean, which we control for, as recommended by Daw and Hatfield (2018) .

eating. The authors find that providing low-income households access to the same products/prices available to higher-income households would reduce nutritional inequality by only 9%.

Our conclusion is also consistent with findings in the SNAP literature. The USDA reports limited differences in the foods purchased by recipients of SNAP assistance when compared to those purchased by non-recipients.[8] Hastings et al. (2019) perform counterfactual simulations showing that additional SNAP payments aimed at closing the gap between high- and low-socio-economic status households with respect to food spending would not reduce the (large) difference in healthfulness of food consumed by much (estimates were both small and indistinguishable from zero). We find that a similar tendency to avoid making substantial changes to one's food purchases manifests among minimum-wage earners when the minimum wage rises. Nonetheless, even holding fixed the nutritional content of households' calorie purchases, an increase in the purchase of calories has the potential to help households reach recommended values of important nutrients.

Next, we try to systematically examine changes to households' shopping baskets following minimum wage increases. We find that minimum wage households that purchase more calories do not merely buy more of previously purchased food; one out of every three additional UPCs that they buy in response to the minimum wage rising is a UPC they are purchasing for the first time. We also find that minimum wage households that *do not* purchase more calories buy more new UPCs in response to the minimum wage rising, but do so at the expense of UPCs that they had previously purchased, keeping their total volume of food purchased constant. Moreover, we find that the households that improve the healthfulness of their shopping baskets do so without drastically changing which categories they source their food from.

The finding that rising minimum wages may partially alleviate minimum wage households' risk-aversion to previously unpurchased UPCs suggests an opportunity exists for retailers when the minimum wage rises: some of their consumers are buying a larger volume of new products in a grocery setting. Retailers with a nutrition-friendly brand image could leverage this openness to "new" UPCs to nudge consumers towards more healthful products—e.g., via simple nutritional

[8] https://fns-prod.azureedge.net/sites/default/files/ops/SNAPFoodsTypicallyPurchased-Summary.pdf - retrieved Nov 15, 2019.

scoring systems (Nikolova and Inman, 2015). Our finding also complements Becerril-Arreola et al. (2021), which finds that income dispersion in a region decreases category offerings, especially for larger brands. Raising the minimum wage serves to reduce income dispersion, making the market capable of bearing more category offerings—consistent with the notion that households are more amenable to purchasing new foods after the minimum wage rises.

In sum, the main contributions of our study are to a) measure the causal effect of minimum wage changes on calories purchased and the nutritional composition of calories purchased by minimum wage-earning households, and b) systematically document how these changes manifest in households' shopping baskets. Our work adds to the literature on food insecurity and consumer nutrition, showing that minimum wage increases *do* help minimum wage-earning households afford more calories, and that they help at least some households improve the average healthfulness of their shopping basket. In light of these findings, we also offer some commentary on the role that minimum wages may play in alleviating the growing levels of nutritional inequality in the United States (Wang et al., 2014).

Our study adds to a nascent but growing stream of the marketing literature that focuses on the behaviors of low-income consumers, an under-investigated segment of society. Other recent work has found, for example, that low-income households are judged as immoral for spending SNAP vouchers on "moral goods," such as organic food (Olson et al., 2016); that low-income households' ability to take advantage of various promotions is impeded by liquidity constraints (Orhun and Palazzolo, 2019); and that "soda taxes" reduce low-income households' consumption of sugary beverages more than it does for higher-income households, because low-income households are less able to cross-shop in cities without soda taxes (Seiler et al., 2020). Finally, our study also aims to spur further research in the Marketing domain on the topic of minimum wages as a policy, which has primarily been explored in Economics and Public Health circles.

The rest of the paper is organized as follows. First, we describe the data we leverage to answer our research questions. Next, we examine the impact of minimum wage changes on calorie consumption, followed by an examination of the nutritional content of those calories. We then test the robustness of our estimates to accounting for unobserved selection and other alternative explanations. Subsequently, we examine heterogeneous patterns in households' response to minimum wage revisions. Our final set of analyses characterizes how households change the composition of their shopping baskets. We conclude with a discussion of the implications of our findings for policy makers and marketers.

Data

We utilize two data sets: The Nielsen Homescan Panel data set, provided by the Kilts Center for Marketing at the University of Chicago Booth School of Business, and Label Insight's Open Data database containing nutrition label information for food UPCs sold in the United States. We merge the nutrition label data set with ten years (2007-2016) of Nielsen Homescan Panel data to examine how: (1) calories purchased and (2) the nutritional content of calories purchased change in response to rising minimum wages.[9] In the following three subsections, we describe the variation in minimum wages across locations and over time, outline our sampling criteria, and describe our dependent variables and relevant summary statistics.

The Minimum Wage

There are three levels of aggregation at which the minimum wage can be set: federal, state, or sub-state (typically a city or county). We use the term "locality" to refer to any self-contained region—state or sub-state—where the minimum wage changed between 2007 and 2016. There are 41 states in our data for which no city or county set a higher minimum wage than the state's between 2007 and 2016; these states each serve as their own locality. For states where cities or counties overrode the state minimum wage at some point between 2007 and 2016, each individual city or county that did so is treated as its own locality, while all remaining cities and counties which adopted the state's minimum wage are grouped together as a single locality. For example, in California, the state minimum wage was $10 per hour at the start of 2016, but the city of San

[9]Rising minimum wages contribute similar benefits to (say) voluntary raises afforded by employers. However, the nature of our household income data does not allow us to quantify the effect of more general income changes on consumer nutrition. We further discuss how our estimates may be interpreted in light of this in the results section.

Francisco set a higher minimum wage of $13; San Francisco is classified as its own locality.

We observe 309 minimum wage increases for the localities in our data. Of these changes, 107 were for individual cities or counties. Overall, minimum wages changed noticeably during the decade under study (Figure 2): the average locality increased its minimum wage by 29% between 2007 and 2016. However, many *individual* minimum wage increases were small. The median and average minimum wage increases were $0.50 and $0.55, respectively, affording a mere $80-$88 extra per month (before taxes) to a fully-employed minimum wage earner. While the smallest individual increase was $0.04 (Florida's minimum wage rose from $7.21 to $7.25 with the passing of the Fair Labor Standards Act of 2009, which changed the federal minimum wage to $7.25), the largest individual state-level increase was $1.25 (South Dakota's minimum wage rose from $7.25 to $8.50 in 2015).

Nielsen Data Sample

We leverage grocery purchase activities tracked via the Nielsen Homescan panel to measure within-household changes in food purchases as a function of rising minimum wages.[10] We adopt a difference-in-differences (DiD) based identification strategy to measure the causal effect of rising minimum wages. We measure changes in the purchase behaviors of minimum wage households in localities that experienced rising minimum wages (our treatment group). We compare these changes to changes in the purchase behaviors of one of two control groups: (a) households living in the same locality as treated households that are likely earning just above the minimum wage, or (b) minimum wage households in other localities where the minimum wage did not change. The purchase activities of control households allow us to control for time trends driven by unobserved factors influencing households' grocery purchases.

In order to identify which households are earning the minimum wage, we first calculate a "minimum wage annual salary equivalent" (hereafter "MWASE") for each locality and month.

[10]The Nielsen dataset does not include information on food choices away from home (e.g., at restaurants). However, past research suggests that grocery purchases are not a systematically biased measure of overall diet healthfulness (Allcott, Diamond, Dube, Handbury, Rahkovsky and Schnell, 2019). We used MRI Simmons survey data to further explore this and found no evidence that out-of-home food purchasing differed systematically after minimum wages increased - please see Web Appendix G for details.

The MWASE is defined as the amount an individual could have earned by working 40 hours per week, for 52 weeks, if they were paid their locality's minimum wage. For example, the minimum wage in Michigan during September 2014 was $8.15/hr; the corresponding MWASE is calculated as $\$8.15 \times 40 \times 52$, or $16,952.[11]

We classify a household as a "minimum wage household" (i.e., a part of our treatment group) if, *for every month that the household participated in the Nielsen panel*, their reported income bracket did not exceed the income bracket that included their locality's MWASE. For illustration, consider Washington D.C. between 2014 and 2015. In 2014, Washington D.C.'s minimum wage was $9.50 per hour, placing its MWASE in the [$15,000-$20,000] income bracket. In 2015, D.C.'s minimum wage rose to $10.50 per hour, placing its MWASE in a higher [$20,000-$25,000] bracket. Households living in D.C. during this two year period would only be classified as a minimum wage household if their reported income bracket was no higher than [$15,000-$20,000] in 2014 and no higher than [$20,000-$25,000] in 2015. Similarly, we classify a household as earning above the minimum wage (i.e., a part of our control group) if, for every month that they participated in the Nielsen panel, their reported income bracket exceeded the income bracket that includes their locality's MWASE, but was no higher than the $35,000-$40,000 income bracket.

In Web Appendix A, we show that our findings are highly robust to several alternative definitions of the control group, including more flexible, locality-specific income thresholds that are allowed to rise along with the locality's minimum wage. We also show that our findings are robust to excluding "borderline" households—those households that lie within the income bracket containing the MWASE. Households that appear to be switching between earning the minimum wage and earning more than the minimum wage are excluded from our sample, to preserve the integrity of our DiD-style identification strategy.

Our two control groups have complementary strengths. Identification using households earning above the minimum wage from the same locality as minimum wage earners allows for locality-

[11] Only 3.6% of households that participated for more than a year in the Nielsen panel reported two heads being fully employed for their entire time in the sample. Given these low rates of dual-employment, we chose to use the MWASE for a single, fully-employed minimum wage earner. We show the robustness of our results to alternative approaches—with particular attention given to the possible misclassification of households with two minimum wage earners as control households—in Web Appendix A.

specific controls for time trends. However, this approach relies on an assumption that there are no spillover effects of minimum wage changes on households earning above the minimum wage. Some research has suggested this assumption may not be innocuous in this setting. While Cengiz et al. (2019) note that households that fall just above the minimum wage category may also experience a modest wage boost from rising minimum wages (making our estimates conservative), Neumark et al. (2000) and Clemens et al. (2018) argue that rising minimum wages may have a negative net income effect on households in our control group. Furthermore, some households may be potentially misclassified as "treatment" or "control", since our income data are reported in buckets. Prior work has pointed out that measurement error in the form of misclassification may be difficult to to distinguish from spillover effects between the minimum wage and higher income groups (Autor et al., 2016). To assuage these concerns, we exploit model specifications that utilize purchase activities of *only* minimum wage earning households—comparing households in localities where the minimum wage is changing to those in localities where it is not (an alternate control group). However, in these specifications we forego the benefit of including locality-specific time controls, because households that are part of the alternate control group do not live in the same localities as those from our treatment group. We show that our results are robust to using either approach (we discuss this formally in the next section).

Screening Criteria

We employ a few simple criteria to screen out households that were unlikely to have been active members of the labor force. Critically, this does not mean that we retain only employed households. Prior work has documented the impact of rising minimum wages on labor force participation and employment (Card and Krueger, 1993; Wessels, 2005; Neumark and Wascher, 2006; Meer and West, 2016). Some households may experience a job loss or a reduction in employment hours due to rising minimum wages – this should be (and is) factored in to our estimates. We employ a mild screen, retaining households that were employed for at least 25% of their time in the panel. Following a similar logic, we retain households for which the household heads were not of retirement age for at least 25% of their time in the panel. Our conclusions are not sensitive to these screens (see Web Appendix D).

For households that were in the panel for only a single year, we retain only those households that reported their purchases for more than 9 months (75% of their time in the panel). This ensures that our analyses focus on households who show a reasonable level of reliability with reporting purchase activities. Dropping households for which this last condition does not hold reduces the number of households by only 2.3%, and the number of observations in our sample by only 0.6%. Our conclusions are not sensitive to the inclusion or exclusion of these households. Our final sample contains 560,717 observations corresponding to 19,375 households, of which 3,301 are classified as minimum wage households.

Dependent Variables

We make use of two main components of nutrition label information to construct our dependent variables: a UPC's (1) total calories and (2) total volume of "healthy" and "unhealthy" macronutrients. We construct two composite health indices based on the USDA's Healthy Eating Index and the UK Food Standards Agency's Nutrient Profiling Model.

Our dependent variables are calculated using only the UPCs for which we have nutrition label information. The nutrition label data contains UPCs that were carried in stores in 2018 (the year the data was acquired), but the Nielsen data runs from 2007-2016. Consequently, not all UPCs in the Nielsen data set can be matched to the nutrition label data set. We are able to match UPCs to roughly half of household spending on food by our sample. The match rates are predictably lower for earlier years (39% for 2007) than for later years (54% for 2016). However, crucially, they do not vary differentially across the treatment and control groups over time (see Table WA.18 in Web Appendix I), alleviating concerns that differences in nutrition label match rates may induce biases in our estimates.

Calories

We construct two DVs pertaining to calories purchased by a household. The first is the natural log of household h's average calories purchased per day during month t: $ln(AvgDCal_{ht})$. The second is the natural log of household h's average calories purchased *per adult equivalent*, per day during month t: $ln(Cal\,pAdult_{ht})$.[12] We follow Allcott, Diamond, Dube, Handbury, Rahkovsky and Schnell (2019)'s approach for constructing the number of "adult equivalents" for a household, scaling children's caloric needs (by age group) to a percentage of an adult's caloric needs. For minimum wage households, the mean observations for our two dependent variables are 1,483 calories per day and 849 calories per adult equivalent per day.[13]

Recall that in the USDA's Current Population Survey Food Security Supplement, many low-income households report an inability to afford a sufficient number of calories. Several patterns in our raw data are consistent with these surveys. First, over the course of a year, the median minimum wage household purchases 12% fewer calories per adult equivalent when compared to a control household of the same size and from the same locality.[14] Second, the number of calories purchased by minimum wage households declines sharply over the course of the month, as they (plausibly) face higher liquidity constraints: the number of calories purchased is fairly similar during the first two weeks (1,603 per day, on average), but drops to 1,296 calories per day (a 19.2% decline) in the final week of the month (see Figure 3, Left Panel).[15] Control households (who, despite earning more than the minimum wage, are still near the bottom of the income distribution) also purchase fewer calories at the end of the month, but their decline (a mere 7.4%) is less pronounced.

Previous research has linked obesity among lower-income households to *excess* caloric consumption (Drewnowski, 2009; Claassen et al., 2019). These findings are also consistent with our data. While the *median* minimum wage household purchases fewer calories than comparable control households, the *average* (mean) minimum wage household does not, because caloric purchases among minimum wage households are skewed. Minimum wage households at the upper tail of the caloric distribution differ dramatically from comparable control households: the 90th percentile of

[12] We add a small constant (1) to all measures before taking logs since some contain zeros.

[13] These numbers are based on *matched UPCs only;* roughly half of households' food purchases.

[14] For each locality, year, and household size, we compute the average calories per adult purchased by control households, and calculate each minimum wage household's percentage difference from this value.

[15] Orhun and Palazzolo (2019) find a decline in spending by low income households over the course of the month. Consistent with their approach, we define the first three "weeks" as the first three 7-day intervals (first through seventh and so on), with the fourth "week" defined as the 22nd day onward. Other work has also established that the drop in calories over the course of the month holds for SNAP recipients (Damon et al., 2013; Kuhn, 2018).

minimum wage households by this metric purchases *80% more* calories per adult equivalent; the 95th percentile purchases *119% more* than the comparable control household. In a later section, we systematically examine whether the responsiveness of households to rising minimum wages varies as a function of their ex ante (i.e., pre-minimum wage increase) calorie purchases.

Health Indices

To gauge the healthfulness of households' food purchases, we use two composite health indices which have precedence in the literature and are based on government-established guidelines: the USDA's Healthy Eating Index (or "HEI") and the UK Food Standards Agency's Nutrient Profiling Model ("NPM"). We discuss each of these in sequence.

The HEI is a measure of diet quality that is aligned with the agency's "Dietary Guidelines for Americans." It was developed in 1995 and has often been used in conjunction with retail panel data to evaluate the healthfulness of food purchases (e.g., Carlson et al., 2014; Allcott, Diamond, Dube, Handbury, Rahkovsky and Schnell, 2019; Hastings et al., 2019). The HEI scores the healthfulness of a household's diet based on a set of macronutrients (fruit, vegetables, dairy, fiber, protein, saturated fat, sugar, and sodium).[16]

We follow Allcott, Diamond, Dube, Handbury, Rahkovsky and Schnell (2019) in adapting the HEI for our analyses. The USDA provides a "recommended" consumption rate per 1,000 calories for the five healthy macronutrients (1.3 cups of dairy, 1.2 cups of fruit, 1.3 cups of vegetables, 26g of protein, and 14.3g of fiber), and a recommended limit per 1,000 calories for the three unhealthy macronutrients (15.6g of sugar, 17.8g of saturated fat, and 2g of sodium).[17]

We use these guidelines to construct the first composite health index measure used as a dependent variable in our analyses: HEI_{ht}. In each month t, we calculate the amount of each macronutrient purchased per 1,000 calories by household h, then divide that number by the respective threshold recommended by the USDA. The resulting ratios are a measure of the household's standing on

[16]Even though fruit, vegetables, and dairy are technically not "macronutrients", we use the term "macronutrients" while referring to all nutrition components for expositional ease.

[17]Based on the macronutrient information available in our data, we use slight variants of the HEI (Healthy Eating Index) and NPM, as described in detail in the Appendix.

each healthy or unhealthy macronutrient, relative to USDA guidelines. The ratios for unhealthy macronutrients are multiplied by negative one, ensuring that a reduction in a household's purchase of an unhealthy macronutrient will lead to a less negative number for its corresponding ratio. E.g., a household purchasing 15.6g of sugar per 1,000 calories (the recommended limit) would have a score of -1.0, while a household purchasing 7.8g would have a less negative score of -0.5.

Our dependent variable HEI_{ht} is the average of these macronutrient-specific ratios. A household that has consumed their full recommended value of healthy macronutrients (and receives a value of 1 on each) and consumed the recommended limit for all unhealthy macronutrients (and receives a value of -1 on each) would have a health index of 0.25 (2/8). An increase of 0.1 to HEI_{ht} represents an average improvement (over all eight macronutrients) of 10% of the USDA's prescribed consumption threshold, where "improvement" implies an increase in consumption of healthy macronutrients and a decrease in consumption of unhealthy macronutrients.

The Nutrient Profiling Model (hereafter "NPM") has, like the HEI, been widely adopted in prior research (Trichterborn et al., 2011; Poon et al., 2018; Andre et al., 2019; Dubois et al., 2020). The UK's Food Standards Agency developed the NPM to allow the UK Office of Communications to score the nutritional quality of foods advertised to children.[18] Our NPM Index assigns each UPC a score between -40 to 15, based on the UPC's volume of three healthy components (protein, fiber, and fruit/vegetables/nuts) and four unhealthy components (saturated fat, sugar, sodium, and calories) per 100 grams.[19] We calculate the score for each UPC in our dataset, and then construct a weighted average (by each UPC's size in grams) for all matched UPCs purchased by household h during period t: NPS_{ht}.

We provide further discussion on the similarities and differences between the HEI and NPM in the Appendix. Overall, both indices show that lower-income households eat less healthfully than higher-income households (Table 1), consistent with past research on food content and obesity. For example, Drewnowski (2009) suggests that obesity is more prominent among lower-income households because calorie-dense foods (foods high on calories per gram) tend to be both nutrient-poor

[18] https://www.gov.uk/government/publications/the-nutrient-profiling-model

[19] To keep the interpretation consistent across the two indices, we reverse the sign on our Nutrient Profiling Model's scores; in our index, lower numbers are worse, consistent with the HEI. Please see the Appendix for further details.

and cheaper. Appelhans et al. (2014) argues that the combination of reduced access to home-prepared dinner supplies and caregiver attitudes towards cooking contribute to greater nutritional inequality and childhood obesity rates among low-income households. The difference in scores on the health indices between minimum wage households and those earning just above is small, as one would expect given that both sets of households are at or near the bottom of the income distribution. However, minimum wage earners purchase far less healthful food than high-income households. The HEI for these two groups of households differs by 0.11 - a difference that could result from (say) minimum wage households purchasing an additional 11% of each healthy macronutrient's prescribed consumption amount, as well as reducing their purchase of each unhealthy macronutrient by 11% of the USDA's recommended limit.

Relatedly, in the USDA's Current Population Survey Food Security Supplement, many low-income households express an inability to afford healthful meals. The implicit suggestion here is that households not only eat less overall when they have less money, but they also eat less healthfully. While we found that minimum wage households purchased fewer calories during the later weeks of the month (when they are more likely to face liquidity constraints), we do *not* see a similar pattern with the health indices: the nutritional composition of food purchases over the course of the month is highly stable (Figure 3, Right Panel). This is seemingly inconsistent with the premise that a lack of funds inhibits the purchase of healthy foods. However, the USDA has suggested that low-income households' aversion to purchasing more healthful foods like fruit and vegetables may be driven by the *perception* that they are more expensive, even though some research suggests that healthful foods are not, on average, more expensive.[20] In light of this, it is perhaps understandable that any plausible changes in liquidity constraints over the course of the month did not change the healthfulness of a household's shopping basket.

Interestingly, the nutritional composition of grocery baskets for minimum wage households differed substantially from that of control households among those that ranked below the median on a health index, but start to disappear among those that ranked above the median (Table 2).

[20] https://www.ers.usda.gov/amber-waves/2008/november/can-low-income-americans-afford-a-healthy-diet/

Impact of the Minimum Wage

As discussed earlier, responses by food-insecure households to questions on the December 2018 Current Population Survey suggest that they are inhibited from eating as much/as healthily as they would in the absence of monetary constraints. If this premise is accurate, then increases to the minimum wage should allow minimum wage households to both purchase more food and healthier food. In this section, we test whether this is the case, and quantify the extent to which households purchase more calories and/or improve the nutritional content of calories purchased when the minimum wage rises.

Calories Purchased

We begin by examining the basic relationship between the minimum wage and household h's calories purchased during month t. We use the following model specification:

$$ln(Cal_{ht}) = \alpha_{hl} + \beta I[MWASE]_h ln(MW_{lt}) + \sum_{l=1}^{L}\sum_{t=1}^{T} \lambda_{lt} + \varepsilon_{ht} \tag{1}$$

where Cal_{ht} represents one of our two dependent variables (average daily calories or average daily calories per adult equivalent) and $I[MWASE]_h$ is an indicator variable designating the household's treatment group membership. The term $ln(MW_{lt})$ is the log of minimum wage in locality l in month t, and is analogous to the post-treatment indicator variable in a two-period differences-in-differences model. Our coefficient of interest β captures the causal effect of minimum wage changes on calories purchased by minimum wage households. We include household-locality fixed effects (α_{hl}) to control for both household-level heterogeneity and possible evolution of food preferences due to household migration (Bronnenberg et al., 2012).[21]

We also include locality × month fixed effects (λ_{lt}) to capture any locality-specific changes over time in factors that affect the purchase of food—e.g., price changes in locality l. These fixed

[21] The DV Average Daily Calories Purchased Per Adult Equivalent implicitly accounts for household size changes over time (e.g., due to marriage, the birth of a child, or a roommate moving out), while the DV Average Daily Calories Purchased does not. We therefore include household size fixed effects in specifications for the latter DV to control for such changes. Our estimates are robust to excluding these fixed effects.

effects are estimated from the purchase behavior of our control group. The inclusion of household fixed effects and locality $\times$ month fixed effects obviate the need to separately include main effects for $I[MWASE]_h$ and $ln(MW_{lt})$. The effect of interest (β) is identified by changes in the purchase behavior of the treatment group over and beyond the influence of locality-specific time trends (λ_{lt}). Our identification strategy, therefore, relies exclusively on comparisons of households *within the same locality* - i.e., minimum wage households in locality l are compared to households earning just above the minimum in that locality. We verify that the parallel trends assumption holds.[22] In line with Bertrand et al. (2004) and Abadie et al. (2017), we cluster our standard errors at the locality level (the level at which "treatment"—i.e., minimum wage change—occurs).

As we estimate the regression model in logs, β represents the elasticity of calories purchased (or calories purchased per adult equivalent) with respect to the minimum wage. We estimate these elasticities to be 0.459 (S.E.=0.175) and 0.589 (S.E.=0.170), respectively.[23] These estimates imply that the median minimum wage change (an increase of 6.6%) leads minimum wage households to increase their calories purchased by 3.0%, and increase their calories purchased per adult equivalent by 3.9%.

Past research has argued that minimum wage earners have tended to allocate a dominant share of increased wages towards the purchase of durable goods (Aaronson et al., 2012). Furthermore, households' estimated marginal propensity to purchase food from cash has been shown to be quite low (roughly 0.1, Hastings and Shapiro, 2018). Taken together, this might suggest that increases to the minimum wage might not be a particularly meaningful instrument for addressing food insecurity. In light of this, our estimates of 0.459 for the elasticity of calories purchased with respect to the minimum wage and 0.589 for calories purchased per adult equivalent are encouraging.

The identifying assumption behind the DiD-based identification strategy is that households who are assigned to the control group are not influenced by changes to the minimum wage. This assumption may be violated if increases to the minimum wage also influence behaviors of house-

[22] We show results from the test of parallel trends and additional falsification tests in Web Appendix B.

[23] These results are robust to incorporating the projection factors available in the Nielsen data to allow for scaling measures to the national level annually: the corresponding elasticities with projection factors for calories and calories per adult are 0.411 (S.E.=0.187) and 0.548 (S.E.=0.197) respectively.

holds earning just above minimum wages (Cengiz et al., 2019; Neumark et al., 2000; Clemens et al., 2018). An additional identification challenge stems from household income being reported in buckets, potentially leading to measurement error: some households may be incorrectly classified as either treatment or control. Autor et al. (2016) have also noted that a spillover effect of rising wages onto other income groups may be difficult to distinguish from measurement errors in the classification of minimum wage earners.

To alleviate these concerns, we specify two models where we drop households earning above minimum wages from the analyses and use only purchase activities of minimum wage households. These models exploit alternative sources of identifying variation available in our data: the difference in timing of minimum wage changes between localities. Our parameters of interest are identified from instances where the minimum wage changes in one locality (e.g., Arizona), but not another (e.g., Utah). In effect, minimum wage households in localities where the minimum wage is not changing serve as a control group for minimum wage households in localities where the minimum wage is changing.

We construct two alternate model specifications by tweaking the specification discussed in detail above (let us call this "specification 1"). In a second specification, we replace the locality $\times$ month fixed effects used in specification 1 with month fixed effects. These fixed effects capture any unobserved time varying influences on our DV that are common to minimum wage households in all localities. In a third specification, we refine specification 2 by using region $\times$ month fixed effects in place of month fixed effects. In specification 3, the control group is comprised of minimum wage earners in other localities *within the same geographic region* of the U.S. (e.g., Midwest, West, Northeast, South).

As is common in models based on the DiD identification strategy, our model specifications assume that the *timing* of minimum wage changes in specific regions is exogenous to the treatment outcome. Goodman-Bacon (2020) shows that causal inference without an untreated group (as in specifications 2 and 3) that leverages differences in treatment timing amounts to a weighted average of all DiD estimates generated from each pairwise locality combination—e.g., comparing San Francisco to Los Angeles, to Nevada, to Arizona, etc. He shows that the treatment effect is unbiased in multiple period DiD designs conditional on the treatment effect being stable over time (e.g., the effect on household h is similar four months post-treatment as it is two months post-treatment). In a later section, we present a robustness check showing the estimated effects of the minimum wage using short windows of 2-4 months on either side of the treatment. We find that these effects are substantively similar to one another and comparable with our primary estimates, suggesting that the impact of the minimum wage on our DVs turns on nearly instantaneously and is stable over time.

A limitation of not using households earning above the minimum wage as the control group in specifications 2 and 3 is that the fixed effects used to control for changes in local market conditions are less rigorous than in specification 1. Our first specification used locality $\times$ month fixed effects to control for any unobserved changes in local market conditions (e.g., due to factors such as changes in the local costs of living over time). To better account for such local time-trends in specifications 2 and 3, we include an index for the cost of food at the local market level (specified as the logarithm of the average cost per 1000 calories of food purchases in locality l during time period t) as an additional control variable. We computed this price index using prices for product purchases in the Nielsen data made by households with an annual salary greater than \$40,000 (i.e., these households are neither members of our treated nor control group) to alleviate any concerns that such purchases may be influenced by minimum wage changes.[24]

The estimates of calories purchased from specification 2 and specification 3 are substantively similar to those from specification 1 for both average daily calories and average daily calories per adult equivalent (see Table 3).[25]

These elasticities represent the average effect of rising minimum wages on the calorie purchases of households likely to be part of the labor force affected by minimum wage changes. Note that

[24]To confirm the orthogonality of these price indices to the minimum wage, we estimate the following equation: $ln(PricePer1000cal)_{lt} = \alpha_l + \beta_t + \delta ln(MW_{lt})$. The parameters α_l and β_t are locality and month fixed effects. The parameter δ represents the elasticity of our locality-specific price controls with respect to the minimum wage, and is not significantly different from zero (0.0007; SE 0.0027).

[25]To further test the robustness of specification 1, we run a placebo test for the effect of wage changes on calories purchased by households earning wages just above the minimum (the control group for specification 1). We estimate specifications 2 and 3 using just those households and find no statistically significant changes in their purchase behaviors (although the confidence interval spans effects of potentially large economic magnitude). Please see Web Appendix A for details.

rising minimum wages may not only influence hourly wages, but also minimum wage households' level of employment (Jardim et al., 2017). Recall that our analyses include households that may have experienced job losses or reduced hours due to minimum wage increases. Such potential adverse effects of minimum wage revisions on employment could blunt the overall benefits of minimum wage increases. Minimum wage households' income can grow by, at most, as much as their locality's minimum wage grew—and possibly less, if their hours were reduced. Thus, our estimate of the elasticity of calories with respect to the minimum wage is likely a lower bound for the analogous effect with respect to income (for minimum wage earners). We explore differences in the responsiveness to minimum wages among households with varying levels of employment more formally in a subsequent section of the paper.

Health Indices

We next test whether the minimum wage has a causal effect on the healthfulness of calories purchased by minimum wage households. We regress our Health Indices on the same set of variables from equation 1, with one exception: we use the minimum wage for a given locality l and month t instead of the log of the minimum wage, as our health indices can take either positive or negative values.[26]

$$HIndex_{ht} = \alpha_{hl} + \beta I[MWASE]_h MW_{lt} + \sum_{l=1}^{L}\sum_{t=1}^{T}\lambda_{lt} + \varepsilon_{ht} \tag{2}$$

Specifying the model in levels (as opposed to using the log of the minimum wage) allows us to compute the effect of a one dollar increase in the minimum wage on households' dietary health indices. This provides an intuitive measure of the effect given that the median minimum wage increase over our study window was about 50 cents. We estimate the effect of a one-dollar change in the minimum wage on the health indices of minimum wage-earners (β) to be very close to zero

[26] We weight each observation by calories purchased by household h during month t, since some monthly health indices are calculated over a larger number of calories than others. Our findings are not sensitive to whether or not we weight our estimates by calories purchased. They are also robust to using the log of minimum wages instead of minimum wages - see Web Appendix H.

(see Table 4).[27] This suggests that, *on average*, minimum wage revisions appear to have neither a statistically significant nor economically meaningful impact on the nutritional value of calories purchased by minimum wage earners. This effect is fairly precisely measured: for specification 1, the 95% confidence intervals are (-0.020,0.013) for the HEI and (-0.0052,0.168) for the NPM. By contrast, the interquartile range for the indices for minimum wage households are 0.414 (HEI) and 4.305 (NPM). Estimates for specifications 2 and 3 are similarly close to zero and non-significant.

As noted previously, the potential for household misclassification—treated as control, or control as treated—may bias estimates toward zero, likely rendering our estimates of the elasticity of calories purchased with respect to the minimum wage conservative. With respect to health indices, however, the potential for misclassification means that a null effect should be viewed with caution, especially in light of the possibility that households may prioritize *getting enough to eat* over purchasing healthy calories. Such a prioritization is implicit in the USDA's classification of households' food security status: low food security refers to a scenario where households cannot afford a balanced meal, while *very* low food security denotes households not being able to afford sufficient calories.

Moreover, the interquartile ranges for households' average health indices are quite sizable, suggesting that the healthfulness of households' shopping baskets differed meaningfully. Recall, for example, that the magnitude of the HEI's inter-quartile range (IQR) of 0.414 corresponds to 41.4% of each macronutrient's USDA threshold. In a later section of this paper, we explore whether rising minimum wages may have had a heterogeneous impact on minimum wage earners based on differences in their nutritional status.

[27] Consistent with this pattern, on average, we find no systematic shifts in households' purchases of the different macronutrients underlying the two dietary health indices. Please see Web Appendix F for details.

Robustness Checks

Model Validation

In this section, we test the validity of our identification strategy by performing a few robustness checks. First, we test for the existence of differential trends in calorie purchases between the treated and control group during the 12 month period before minimum wages rose. We find no evidence that the parallel trends assumption is violated. Second, as a falsification test, we compare the trendlines of minimum wage earners to those earning above the minimum in a set of 37 localities where minimum wages did not change over a five-year period (2010 through 2014). We find no difference between these trendlines (please see Web Appendix B).

Testing for Unobserved Selection

To investigate the influence of unobservables, we begin by examining short, n-month pre/post windows surrounding each minimum wage change ($n = 1$, 2, 3 and 4 months pre/post) in the style of a regression discontinuity, and employ a difference-in-differences design to estimate the effects of interest. Focusing on a shorter temporal window on each side of the treatment makes it less likely that the treatment group was influenced by factors besides the treatment (Imbens and Lemieux, 2008; Hartmann et al., 2011). Here too, we find elasticities that are very similar to those from our focal approaches (please see Table WA.7 in Web Appendix B). The effects are strongly significant for n=2, 3, and 4 months. While the effect is similar in magnitude during the narrow one-month window before/after minimum wage revisions, the effect is not statistically significant due to the smaller sample size. This suggests that changes to the minimum wage are likely to have a near-immediate effect on the purchase of calories.

As a final check, to assess the potential importance of unobserved confounders in explaining our effects, we follow the approach proposed by Oster (2019). Building on the logic of Altonji et al. (2005), Oster (2019) argues that the robustness of estimates to omitted variable bias can be examined by observing movements in: (a) the coefficient of interest, and (b) model R-squared from specifications that either include or exclude control variables in a regression. Under the rationale that including "relevant" control variables (those that plausibly contribute to improving model R-squared, e.g., period fixed effects) would help alleviate omitted variables bias in a regression model (compared to the case when they are excluded), this approach enables researchers to comment on how large the influence of selection on unobservables would need to be, relative to selection on observables, to nullify the treatment effect of interest. Following Oster (2019)'s recommendations, we find that the degree of selection on unobservables would need to be 1.61 times that on observables in order to overturn our effect, above the generally accepted threshold of 1.0 (which corresponds to equal proportional selection on observables and unobservables).[28] This increases our confidence that our estimate of the treatment effect is unlikely to be driven by selection on unobservables. Taken together, the robustness of our estimates across these analyses increases our confidence that our effects are unlikely to be driven by selection on unobservables.

Accounting for differences in the cost of living

Here, we test the robustness of our results to controlling for different measures of inflation. We begin by replacing the nominal minimum wage used in our primary specification with an inflation-adjusted measure.[29] As we use a decade long observation window (2007-2016), it is important to control for potential changes in the value of a dollar over time. The fixed effects specified at the locality × month level included in the model are intended to help control for any unobserved time-varying influences in households' food purchases at the local market level (such as changes to the cost of living). While these fixed effects allow us to rigorously control for unobserved influences at the local level, a downside of using the fixed effects approach is the inability to articulate precisely

[28] We use the STATA routine 'psacalc' authored by Oster and follow her suggestions with setting the maximum model R-squared (R^2_{max}) to 1.3 the R-squared of a model employing the full set of available controls. The estimate of the proportional selection parameter that would explain away our effect of wages on calories (i.e., reduce to zero) is estimated as 1.61. Under equal proportional selection on observables and unobservables, we still find a lower bound of 0.152 for the treatment effect.

[29] We deflate the minimum wages based on the monthly regional consumer price index for "all items" (series ID: CUURxx00SA0 downloaded from the BLS website: bls.gov). The BLS reports monthly information on CPI in urban areas at various levels of aggregation - U.S. National, regional (East, West, Northeast, Midwest) and hyper-local (Metropolitan Statistical Area level). Because the hyper-local CPI data suffers from missingness at the monthly level, we used the national- and regional-level CPI measures for deflating minimum wages and found virtually identical estimates.

what they are controlling for. To be conservative, we also adjust for inflation, thereby providing an additional measure of control for such influences. Our elasticity estimates using real wages remain statistically significant and are of similar magnitude to our primary estimates (e.g., 0.408 for calories and 0.515 for calories per adult equivalent, for specification 1). We provide additional details in Web Appendix C.

Ruling out Alternative Explanations

We also examine whether our elasticities of interest differ when controlling for subsidies that low-income households may have received—specifically, SNAP ("food stamps") or the earned income tax credit (EITC). One could hypothesize that changes to the minimum wage might be correlated (within a locality) with changes to food stamp laws, given that both instruments are aimed at helping the less fortunate. Moreover, food stamps eligibility is inversely proportional to wages. Prior research has documented that households' food purchases are influenced by their participation in federal safety net programs such as the SNAP (Hastings and Shapiro, 2018). Using panelists' self-reported data on whether they have received SNAP, we test whether our results are contaminated by any unobserved correlation between SNAP assistance and minimum wage changes. We find that elasticity estimates do not differ between households that did and did not receive SNAP assistance.

In addition to the SNAP, minimum wage households may qualify for / benefit from the EITC (Earned Income Tax Credit) to a greater extent than control households, though past research has argued that the EITC is primarily spent on paying bills or on durable goods.[30] To alleviate concerns that EITC refunds are contaminating our estimates, we perform a conservative test by re-running our primary analyses excluding the months in which EITC is received (February and March) and one month thereafter (April, to account for any potential carryovers in spending from EITC receipts the month prior) from each year of our data. Our results are largely unchanged by the exclusion of these months (please see Table WA.9 in Web Appendix C for details).

We show a consolidated view of our estimates from our focal analyses and all the robustness

[30]https://www.chicagofed.org/~/media/publications/economic-perspectives/2008/ep-2qtr2008-part2-goodman-etal-pdf.pdf - accessed Sep 1, 2020.

checks in the form of specification charts in Figures 4 and 5. Across all model specifications, we find substantively similar effects. The average elasticity estimate of calories (calories per adult) with respect to the minimum wage across these specifications is 0.477 (0.575). Overall, all estimates are statistically significant at the 0.05 level; many are also significant at the 0.01 or 0.001 level. We replicate these robustness tests using the two health indices as DVs and find that our substantive conclusions are supported - on average, the minimum wage does not appear to be shifting households' nutritional quality choices.

Heterogeneity in Households' Responsiveness to Minimum Wages

Our results thus far indicate that rising minimum wages appear to be motivating an increase in households' calories purchased. However, might some minimum wage households have differed from others in their responsiveness to rising minimum wages? For example, an increase in calories purchased could have been a net positive for a household suffering from food insecurity, but a net negative for a household already eating relatively many calories—a major driver of obesity among low-income households (Drewnowski, 2009). It is plausible that both types of households participate in the Nielsen panel: while the *median* minimum wage household (with respect to calories purchased) purchased fewer calories than similar control households, the *average* minimum wage household does not. As noted earlier, caloric purchases are heavily skewed among minimum wage households.

Calories Purchased

We begin by dividing households into quartiles based on their (pre-treatment) values for average daily calories per adult equivalent. Each household's "pre-treatment" period is the set of months prior to the first minimum wage change in their locality, during their time in the panel.[31] We then tweak our three model specifications in two specific ways to compute quartile-specific estimates (β_q) for households. First, we interact the focal variable of interest $I[MWASE]_h ln(MW_{lt})$ with a dummy variable ($I[Qrt_h = q]$) designating the membership of household h in quartile q (for a given dependent variable). Second, we include quartile $\times$ month fixed effects (ψ_{qt}) to control for quartile-specific time trends to account for potential regression to the mean.[32] With these modifications, the equation corresponding to specification 1 (for example) is specified as:

$$ln(Cal_{ht}) = \alpha_{hl} + \sum_{q=1}^{Q} \beta_q I[Qrt_h = q] I[MWASE]_h ln(MW_{lt}) + \sum_{l=1}^{L}\sum_{t=1}^{T} \lambda_{lt} + \sum_{q=1}^{Q}\sum_{t=1}^{T} \psi_{qt} + \varepsilon_{ht} \qquad (3)$$

We find large, significant elasticities of calories purchased and purchased per adult equivalent with respect to the minimum wage for the bottom 25% of households (the quartile that purchased the least pre-treatment amount of food per adult equivalent) across all three specifications and both calorie DVs (Table 5).[33] Only households which ranked below the median on pre-treatment calories purchased appear to be showing a growth in calories following minimum wage revisions. Further, these households' calorie purchases seem to be more than unit-elastic with respect to minimum wages, and noticeably higher than the average elasticity of 0.35-0.47 estimated across all minimum wage households. Although we cannot directly identify whether households are overeating or experiencing food insecurity in our data, this finding is more consistent with the possibility that the effect of the minimum wage on calories is driven by the latter than the former.

[31] We control for the fact that match rates change over time by dividing each DV by the average value of that DV during period t. This division is done only for the purpose of sorting households into quartiles. We do not sort households into quartiles based on total calories purchased because doing so is equivalent to sorting them by household size; we use only calories per adult equivalent, instead. Demographic differences across households did not explain variations in households' responses to the minimum wage - we discuss this in detail in Web Appendix D.

[32] Daw and Hatfield (2018) discuss the importance of including such controls to avoid biased treatment effect estimates arising from possible reversion to the mean in settings where pre-treatment outcomes govern the treatment grouping.

[33] Though we only sort households into quartiles using average daily calories purchased per adult equivalent, we use these quartiles for estimating regressions on both calorie DVs.

Dietary Health

It is plausible that while the average minimum wage household did not purchase more healthful food in response to the minimum wage, this average effect might have obscured important underlying behavioral differences among households.[34] Might purchases of healthful foods also vary across households based on differences in their ex-ante tendencies to purchase healthful foods? Recall that we see meaningful differences between households on the $HIndex$ in the raw data: the average minimum wage household purchases less healthful food than the average control household, and this difference is primarily driven by the least healthful households (Table 2). Against this backdrop, we set out to systematically explore whether rising minimum wages may differentially influence the nutritional choices of households who ate healthfully vs. less healthfully prior to wage revisions.

As we did with calories, we divide households into quartiles based on their (pre-treatment) values for the Healthy Eating Index and Nutrition Profiling Model. We again use quartile-specific time trends to control for potential regression to the mean (but using health index-based quartiles) and measure quartile-specific effects of the minimum wage (not logged, consistent with our previous dietary health specifications) on our health indices:

$$HIndex_{ht} = \alpha_{hl} + \sum_{q=1}^{Q} \beta_q I[Qrt_h = q] I[MWASE]_h MW_{lt} + \sum_{l=1}^{L} \sum_{t=1}^{T} \lambda_{lt} + \sum_{q=1}^{Q} \sum_{t=1}^{T} \psi_{qt} + \varepsilon_{ht} \tag{4}$$

We find an interesting asymmetry in behaviors of minimum wage earners at the extremities of the distribution of households' ex-ante health index. Households whose shopping baskets were the least healthful prior to experiencing a minimum wage change (the bottom quartile) appear to make *more healthful* choices after the minimum wage increased, while households whose shopping baskets were ex ante the most healthful (the top quartile) seem to be making *less healthful*

[34] Interestingly, which quartile a household belongs to with respect to health indices is not predictive of which quartile they belong to with respect to calories purchased; a household that purchases fewer calories is not especially more likely to purchase more or less healthful calories than a household that purchases relatively more calories.

choices after.[35] Across our three model specifications, a one dollar change to the minimum wage is estimated to lead households in the bottom quartile to increase their USDA health index by 0.032 to 0.068. This change amounts to 8% to 16% of the interquartile range (or "IQR") of the health index (0.414), and 50% to 100% of the gap between bottom quartile minimum wage and control households' USDA health index (-0.824 and -0.761, respectively). Thus, the typical minimum wage change (of about 50 cents) is likely to help minimum wage households bridge the "healthfulness gap" between the foods purchased by minimum wage and control households (for at-home consumption) by between 25% to 50%. We find even stronger results with the UKFSA health index: an increase in response to the minimum wage by 0.459 to 0.765, representing 11% to 18% of the DV's IQR (4.305). This change corresponds to more than the entire gap on the UKFSA health index between minimum wage and control households in the bottom quartile (0.366). Supplementary analyses in Web Appendix F suggests that this movement may be primarily driven by reductions in how much sugar and saturated fat households purchase.

By contrast, households whose shopping baskets were the most healthful before the minimum wage increased in their locality purchased less healthful foods after. Specifically, a one dollar change in the minimum wage leads the top quartile to decrease their USDA health index by about 0.050—roughly 12% of the IQR. Their scores on the UKFSA health index dropped by 0.2 to 0.3—roughly 5% to 6.5% of the IQR. Interestingly, these changes occur in spite of the limited differences between the healthfulness of shopping baskets of minimum wage and control households from the top quartile. Minimum wage households in this quartile have an HEI score of 0.338 (vs 0.326 for the control group) and a NPM score of -3.72 (vs -3.65 for the control group).[36]

Next, we try to better characterize these shifts in health index in more tangible terms for households in the most and least healthful quartiles. To do this, we classify UPCs using a simple median split of the UKFSA's NPM health index (which is specifically designed to measure the healthful-

[35]Households in the third quartile are sometimes estimated to be purchasing less healthful food in response to the minimum wage, but not consistently so across specifications.

[36]Recall that we control for mean reversion using quartile-specific time trends in these analyses. Additionally, it is worth noting that the "pre-treatment" period for 95% of households is 6 months or longer. Having a lengthy six-month (or longer) window of purchases to classify households ensures that the classification is representative of households' typical behaviors and not driven merely by short-term, random demand shocks.

ness of UPCs), labeling UPCs whose score lies above the median as "healthful." Our back of the envelope calculations suggest that a minimum wage earner from the bottom (i.e., least healthful) quartile shifts about 1,140 calories per month from unhealthful UPCs to healthful UPCs in response to a one dollar change in the minimum wage. In illustrative terms, such a shift is equivalent to replacing two meals consisting of frozen pizza with healthier frozen meals over the course of a month.[37] By contrast, we find that for a minimum wage earner from the top (most healthful) quartile, a one dollar increase to the minimum wage is associated with a shift of 1,200 calories from healthful UPCs to unhealthful UPCs per month—akin to (for example) replacing about two healthful meals with 1.2 pints of Ben & Jerry's Ice Cream (1,000 calories per pint).

Changes to the Shopping Basket

With minimum wage changes potentially on the horizon, it is incumbent upon retailers and manufacturers to understand how such changes may affect the composition of their customers' shopping baskets. Previous research has shown that as households spend more on food, they purchase a wider variety of foods, perhaps as a means of combating diminishing returns to quantity (Li, 2013). Marketers may consider it worthwhile during these times to expand product recommendations to provide consumers easier access to items to help them maximize variety in product purchases (Carlson et al., 2015). On the other hand, Daniel (2016) has suggested that some low-income households may be averse to spending their limited funds on new foods that they may not like. These findings suggest that as the minimum wage rises, minimum wage earners may change the nature of foods they buy. Developing a systematic understanding of changes to consumers' shopping baskets is important for marketers. In this section, we explore whether and how minimum wage households that purchase more food, or more healthful food, change the composition of their shopping basket.

[37] The details of these calculations are available in Web Appendix E.

Households that purchase more calories

Some households purchased considerably more food after the minimum wage in their locality increased. Are these households merely buying more of the UPCs they had purchased previously, or are they becoming more open to trying new UPCs?

To answer this question, we examine the extent to which households purchase UPCs that they had never purchased prior to a given month t. We find that the lowest quartile of households with respect to their ex ante purchases of calories (those that purchased more calories as the minimum wage increased) are not merely buying more of the same UPCs they had purchased before. Instead, they purchase more of both "new" (previously unpurchased) and "old" (previously purchased) UPCs. Specifically, the median minimum wage increase in our data (of about 50 cents per hour, which affords a mere $80 extra per month before taxes) leads these households to purchase about three additional UPCs per month, of which one is a UPC they had never previously purchased.[38]

Interestingly, households above the median with respect to their ex ante purchases of calories—who *did not* purchase more calories in response to the minimum wage rising—increase their *percentage* of spending on new UPCs, by modestly re-allocating their purchases of old UPCs towards new UPCs. Specifically, they purchase one additional "new" UPC every three months, and one fewer UPC from the previously purchased set of UPCs. From a behavioral perspective, this is consistent with Daniel (2016)'s findings. New funds may encourage households that likely already had enough to eat (before the minimum wage rose) to experiment a little with new foods in the grocery store. Thus, even those households that do not buy more food once minimum wages increase seem willing to try UPCs in a grocery setting that they had not previously purchased.

Households that purchase more healthful calories

We also examine how households that purchased more healthful food (in response to the minimum wage rising) did so. We first examine whether such households changed the types of food they were eating (e.g., substituting purchases of frozen food for fresh produce) or, alternatively, purchased

[38]Details of these calculations can be found in Web Appendix E.

more healthful versions of the foods they were already eating (e.g., buying more healthful UPCs without changing which categories they buy from).

Using the same specification from equation 4, we examine whether changes to the minimum wage affect how households allocate their spending across the six major food departments in the Nielsen data (dairy, deli, dry grocery, fresh produce, frozen foods, and package meats) for the least healthful (bottom quartile by health index) or most healthful (top quartile by health index) households. Interestingly, we do not find any cross-department purchase substitution among minimum wage earners (we show detailed results in Web Appendix E). This indicates that households don't appear to be making any drastic changes to the types of food they are purchasing in response to rising minimum wages. For example, households do not seem to be substituting frozen meals for vegetables (or vice-versa).

Given that households are not changing their spending among departments, any observed shifts on a health index for households in the bottom (top) quartile must be from purchasing more healthful (less healthful) items from a given department (e.g., choosing a healthier dairy option). But have rising minimum wages encouraged households to experiment by choosing new (i.e., previously unpurchased) items within a grocery department? Or when households purchase items for the first time, are they selecting more healthful foods than they used to? Alternatively, are they selecting more healthful foods when repurchasing UPCs they have tried before?

To begin exploring this, we construct the DV $PctNew_{ht}$, which measures the percentage of UPCs purchased by household h in month t that were previously unpurchased. We test whether households in the top and bottom quartiles are buying previously unpurchased UPCs at a higher rate in response to minimum wage changes. We do not find any consistent evidence that they are doing so: we see evidence of a small increase in new UPCs purchased by the lowest quartile when using the NPM health index (1.3%, SE=0.66%), but find no effect using the HEI. We also find no change in the percent of new products purchased for the highest quartile using either index.

Though these groups of households may not be purchasing new UPCs more frequently than they were before, might the new UPCs that they are purchasing be more healthful? We examine whether the minimum wage influences the average healthfulness of new and/or previously purchased foods in households' shopping baskets. To do this, we calculate the HEI and NPM for the subset of household i's shopping basket during month t that (i) were and (ii) were not purchased previously, and use these new DVs in regression equation 4. Interestingly, we find that households appear to be making modifications to the healthfulness of their purchases of both new UPCs and previously purchased UPCs (full details of these analyses can be found in Web Appendix E).

In sum, shifts in the healthfulness of UPCs being purchased is not driven by a large influx of *new*, more (in quartile 1) or less (in quartile 4) healthful UPCs; the proportion of new UPCs being purchased remains the same. Rather, it appears that the (ex ante) least healthful households are both (i) modestly re-allocating their purchases within the set of UPCs they had previously purchased within each department, selecting to repurchase more healthful items more frequently, and (ii) also purchasing more healthful products when buying foods they had not previously purchased. Likewise, the (ex ante) most healthful households also see a negative shift in both the health indices of previously purchased and new UPCs.

Discussion, Implications and Conclusion

In 2018, food insecurity affected 1 in 7 households with kids in the United States. 2020 Current Population Survey estimates have indicated that the global pandemic has made the situation much worse, with nearly 1 in 4 U.S. households with kids experiencing food insecurity. A central tenet of the work on food insecurity is that low- and poor-calorie consumption is driven by constrained financial resources (Newell et al., 2014). Raising the minimum wage has long been proposed as a means for low income households to battle starvation and food insecurity. Relatedly, to encourage higher wages and address the prevailing scourge of hunger, President Biden issued an executive order requiring federal contractors to provide a minimum wage of $15 per hour to their employees by 2022.[39]

However, empirical evidence supporting the claim that raising the minimum wage might be

[39] https://www.whitehouse.gov/briefing-room/statements-releases/2021/04/27/fact-sheet-biden-harris-administration-issues-an-executive-order-to-raise-the-minimum-wage-to-15-for-federal-contractors/ - retrieved May 3, 2021.

effective at addressing food insecurity is scant. Prior work has indicated that minimum wage earners are likely to allocate a sizable share of increased wages to the purchase of durable goods (Aaronson et al., 2012). Furthermore, the responsiveness of households' food purchases to cash has been shown to be far smaller than to alternative government administered non-cash subsidies such as SNAP (Hastings and Shapiro, 2018). In our study, we measure the extent to which revisions to the minimum wage might influence changes in minimum wage households' calories purchased as well as their dietary health.

Our work addresses recent calls from the Marketing academic community encouraging new research on how governments, public planners, and firms can work together for societal betterment. More generally, it contributes to a broader discussion within the literature pertaining to what interventions (by policy makers or marketers) can encourage healthier diets among low-income households. A sizable body of research has focused on supply-side factors that may influence households' choice of healthful foods (without directly influencing households' *internal capacity* to buy more/better food). For example, strategies such as presenting nutritional information on product packages (Bollinger et al., 2021), or providing consumers better access to healthy grocery options (Allcott, Diamond, Dube, Handbury, Rahkovsky and Schnell, 2019) have been found to have only limited success in influencing changes in consumer purchase behavior.

On the other hand, Cooksey-Stowers et al. (2017) suggest that restricting access to unhealthy foods can be effective. They find that food swamps—areas with a "high-density of establishments selling high-calorie fast food and junk food"—are more predictive of obesity rates in the population than food deserts. Furthermore, the literature on "sin taxes" has largely suggested that making unhealthy food more expensive can succeed in deterring its consumption among households (e.g., Khan et al., 2015; Silver et al., 2017; Allcott, Lockwood and Taubinsky, 2019), although strategic behavior among consumers may also threaten such deterrence (Seiler et al., 2020). Similarly, a meta-analysis of field experiments for healthy eating nudges (Cadario and Chandon, 2020) found that healthier diets can be successfully encouraged, but that interventions are more effective at reducing unhealthy eating than increasing healthy eating.

Our study contributes to this stream of research by documenting that households do tend to purchase more calories, but not more healthful calories (on average), when their *internal capacity* to buy food increases (i.e., demand-side changes in response to minimum wage revisions). Moreover, the growth in calorie purchases following minimum wages increases is driven entirely by the behaviors of households that ex ante purchased the least amount of food—households that were more likely to be food insecure.

While increases to the minimum wage help households buy more food, we find that most households do not improve the average nutritional content of their calories in response to the minimum wage rising. This finding is consistent with past research: an increase in consumer spending on food has not been found to be associated with improvements in the nutritional quality of their diets (Carlson et al., 2014; Hastings et al., 2019). It also supports the view that healthful and unhealthful foods are usually not seen as substitutes (Epstein et al., 2006). More generally, our results are consistent with prior work that has shown that while consumers with different product preferences exhibit divergent behaviors in the marketplace (Bronnenberg et al., 2015; Rao and Wang, 2017), these product preferences do not readily change—even when consumers move to states with populations that have different product preferences (Bronnenberg et al., 2012).

Interestingly, however, we find some asymmetries in the response of households' dietary health to the minimum wage that would be obscured by focusing on the "average" household. We find that households who (ex ante) ranked within the worst quartile on the dietary health indices actually purchase more healthful foods once minimum wages increase. This finding is promising because improvements in the purchase behaviors of these households is often difficult to encourage via alternative interventions such as nutrition labeling. Our estimates imply that the typical minimum wage increase during our study period (a mere 50 cents per hour) helped minimum wage earners in the bottom-most dietary health quartile bridge a large proportion of the gap between them and the lowest dietary health quartile of households earning above minimum wage (25-50% of their gap on the HEI, and their entire gap on the NPM). This is encouraging as well, and suggests that higher minimum wages may at least be a start for addressing the high levels of nutritional inequality in the United States. It is also worth noting that, even among households that keep the nutritional content of their calories constant—as much of our sample appears to do—an increase in calories purchased may help them reach recommended values of important nutrients. On the other hand, minimum wage households who purchased the most healthful foods ex ante actually purchase less healthful foods after, perhaps undoing some of the societal good achieved by the least healthful households purchasing more healthful food.

It is worth noting that the minimum wage changes observed in our data (averaging about fifty cents) are small. If households prioritize getting enough calories over getting healthful calories, it is possible that past minimum wage changes were not sufficient to allow very many minimum wage households to improve the average healthfulness of their shopping basket. Given the relatively sizable magnitude of minimum wage changes currently being proposed at the national level, it is possible that future minimum wage changes may have more success with improving the average healthfulness of minimum wage earners' shopping baskets. On the whole, from a policy perspective, our findings—that increases to the minimum wage are successful in enabling households' purchase of more food, and that at least some households are now observed to purchase more healthful food—suggest that raising the minimum wage does offer promise for alleviating food insecurity and nutritional inequality in the United States.

Our research also highlights a potential opportunity for retailers: increases to the minimum wage not only encourage some minimum wage households to buy more food, but also to try new (i.e., previously unpurchased) foods. For minimum wage households in the bottom quartile (with respect to their pre-treatment calories purchased), roughly one in three of the additional UPCs they buy when the minimum wage rises are UPCs that they have never purchased before. Even households that keep their caloric purchases constant (perhaps because they already had enough to eat before the minimum wage rose) allocate a larger proportion of their spending towards previously unpurchased UPCs. From a simple promotional planning standpoint, consumers being more receptive to trying new foods may allow retailers and manufacturers to more effectively encourage brand-switching via targeted couponing or promotions. Becerril-Arreola et al. (2021) report that income dispersion in a region decreases category offerings, especially for larger brands. Rising minimum wages may therefore not only encourage households to become more open to purchasing new UPCs, but, by virtue of their serving to reduce income dispersion, also make the market capable of bearing more category offerings.

Importantly, we find that minimum wage households increase their purchase of food almost immediately after the minimum wage rises. Retailers and manufacturers can therefore anticipate when increases in demand are likely to occur by monitoring the timing of federal/local minimum wage changes, and plan accordingly. Additionally, because heterogeneous responses appear to be correlated with *behavioral* (as opposed to demographic or attitudinal) factors, retailers should be able to easily identify which of their consumers are changing their shopping behavior (e.g., using grocery loyalty card data).

For retailers with a nutrition-friendly brand image (such as Sprouts, Trader Joe's, and Raley's), consumers' greater willingness to try new foods may present an even larger opportunity. While our data suggests that most households do not purchase more healthful foods *on their own* when the minimum wage increases, a concurrent nudge from retailers (e.g., in the form of targeted coupons encouraging purchases of healthy products) might be able to increase the number of households that do. The Marketing literature has identified ways in which retailers can potentially shift their customers' purchase behavior towards more healthful foods. For example, Nikolova and Inman (2015) show that simplified and more accessible nutritional scoring systems can help consumers make healthier food choices in grocery stores. Brimblecombe et al. (2010) find that restricting the availability of unhealthful foods (e.g., removing 600 mL soft drinks from the store's refrigerators) improved the healthfulness of food purchased without harming profits. Prior work has documented that poor diets are associated with a wide-ranging host of medical conditions (e.g., diabetes, obesity, anxiety/depression, anemia and lower cognitive function). In light of this, we hope that our findings will encourage NGOs (e.g., Feeding America) and public health agencies focused on promoting healthy eating among low-income households to partner with like-minded retailers (who have access to behavioral data on consumers) to create marketing campaigns aimed at maximizing the benefits from minimum wage revisions for creating meaningful social change.

Such interventions need not be limited to retailers. For example, Lim et al. (2020) find that front-of-product labeling by manufacturers encourages competitors to improve the actual healthfulness of their foods (not only tweak claims made on labels). Some of these strategies may be even more effective in an environment when minimum wage households are becoming more receptive to trying new foods. If such efforts are successful, these brands can potentially reap rewards in the form of loyalty: research on Corporate Social Responsibility has found that CSR engenders attitudinal loyalty in a larger proportion of consumers when the firm's CSR directly influences consumers (*intrinsic* CSR; Ailawadi et al., 2014) than when CSR benefits the world at large.

Our research has a few limitations. First, while our results based on the household calorie quartile splits imply that the increase in calories consumed is less likely to be driven by households' binging on unnecessary calories, we cannot explicitly observe which calories are "necessary" or "unnecessary." Second, we cannot observe downstream consequences of malnutrition or unhealthy eating. Future research may consider it valuable to leverage detailed data on public health outcomes to help paint a more complete picture of the downstream effect of changes to food purchases (spurred by minimum wage revisions) on societal health. Finally, we also encourage future research to explore the use of alternative estimation approaches (e.g., the synthetic control method) by leveraging the rich geographic variation in minimum wages at the locality level.

In sum, our study adds to the literature on food insecurity, consumer nutrition, and the minimum wage, showing that revisions to the minimum wage do help minimum wage-earning households afford more calories, and encourage improvements in the healthfulness of foods purchased for at least some households (arguably those most in need of more healthful food). Our findings contribute to an important national discussion about the prospective benefits of raising the minimum wage, by shining a light on their potential for alleviating food insecurity and nutritional inequality problems in the United States. Given that additional minimum wage hikes may well be on the horizon, our findings also herald an impending opportunity for retailers and manufacturers looking to encourage consumers to improve the healthfulness of their shopping basket.

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Tables and Figures

Table 1: Household Health Index Scores by Income

	USDA HEI	UKFSA NPM
Minimum Wage	-0.565	-5.799
Control Group	-0.520	-5.478
$40-70,000	-0.491	-5.256
$70-100,000	-0.473	-5.108
> $100,000	-0.453	-4.905

Table 2: Summary of Monthly Shopping Basket Health Indices

	USDA HEI		UKFSA NPM	
Percentile	MW	Control	MW	Control
1st	-2.13	-1.96	-19.44	-17.74
10th	-0.99	-0.92	-10.62	-9.85
25th	-0.69	-0.65	-7.53	-7.09
Median	-0.47	-0.44	-5.09	-4.86
75th	-0.28	-0.27	-3.22	-3.15
90th	-0.13	-0.13	-1.88	-1.86
99th	0.15	0.15	0.53	0.53

Table 3: Effect of Minimum Wage on Calories

Specification:	(1)	(2a)	(2b)	(3a)	(3b)
Elasticity: Daily Calories	0.459**	0.367*	0.392*	0.362*	0.410*
St. Error	0.175	0.165	0.166	0.176	0.176
$Adj-R^2$	0.447	0.428	0.428	0.428	0.428
Specification:	(1)	(2a)	(2b)	(3a)	(3b)
Elasticity: Calories Per Adult	0.589***	0.492**	0.517**	0.471*	0.521**
St. Error	0.170	0.183	0.183	0.195	0.194
$Adj-R^2$	0.397	0.387	0.387	0.388	0.388
Fixed Effects:					
Locality × Month	Yes				
Month		Yes	Yes		
Region × Month				Yes	Yes
Cost of Food Controls:			Yes		Yes
Sample Size	560,197	93,930	93,930	93,930	93,930

* Significant at .05 level, ** Significant at .01 level, *** Significant at .001 level

Table 4: Effect of Minimum Wage on Nutrition

Specification:	(1)	(2a)	(2b)	(3a)	(3b)
Estimate: USDA HEI	-0.004	0.009	0.008	0.016	0.015
St. Error	0.008	0.013	0.020	0.018	0.018
$Adj-R^2$	0.017	0.016	0.016	0.018	0.018
Sample Size	553,416	92,530	92,530	92,530	92,530
Specification:	(1)	(2a)	(2b)	(3a)	(3b)
Estimate: UKFSA NPM	0.058	0.112	0.109	0.084	0.079
St. Error	0.055	0.061	0.061	0.067	0.067
$Adj-R^2$	0.255	0.252	0.252	0.252	0.252
Sample Size	551,715	92,127	92,127	92,127	92,127
Fixed Effects:					
Locality × Month	Yes				
Month		Yes	Yes		
Region × Month				Yes	Yes
Cost of Food Controls:			Yes		Yes

* Significant at .05 level, ** Significant at .01 level, *** Significant at .001 level

Table 5: Calories Purchased: Quartiles and Median Split

	Quartiles				Median Split	
Elasticity: Avg Daily Calories	Fewest	Q2	Q3	Most	Fewest	Most
Specification 1	1.292***	0.526	-0.051	-0.280	0.978***	-0.195
St. Error	0.351	0.283	0.422	0.233	0.210	0.238
Specification 2	1.410**	0.533	-0.433	-0.475	1.046***	-0.462
St. Error	0.501	0.482	0.437	0.314	0.282	0.285
Specification 3	1.420**	0.542	-0.469	-0.443	1.046***	-0.465
St. Error	0.463	0.525	0.449	0.297	0.285	0.287
Elasticity: Calories Per Adult	Fewest	Q2	Q3	Most	Fewest	Most
Specification 1	1.434***	0.656*	0.111	-0.262	1.122***	-0.115
St. Error	0.374	0.273	0.404	0.242	0.230	0.221
Specification 2	1.630**	0.656	-0.322	-0.484	1.225***	-0.418
St. Error	0.515	0.436	0.442	0.361	0.285	0.308
Specification 3	1.602***	0.650	-0.376	-0.462	1.197***	-0.434
St. Error	0.476	0.484	0.447	0.348	0.299	0.308
Sample Size	560,197					

* Significant at .05 level, ** Significant at .01 level, *** Significant at .001 level; All regressions include quartile $\times$ month fixed effects.

Table 6: Health Indices: Quartiles and Median Split

	Quartiles				Median Split	
Effect of $\triangle$\$1 on USDA HEI	Fewest	Q2	Q3	Most	Fewest	Most
Specification 1	0.032*	-0.013	-0.026	-0.046*	0.012	-0.035**
St. Error	0.012	0.011	0.013	0.021	0.008	0.012
Specification 2	0.055**	0.001	0.008	-0.050*	0.028**	-0.016
St. Error	0.017	0.012	0.040	0.024	0.010	0.024
Specification 3	0.068**	0.007	0.021	-0.047	0.036**	-0.009
St. Error	0.021	0.014	0.047	0.024	0.013	0.029
Effect of $\triangle$\$1 on UKFSA NPM	Fewest	Q2	Q3	Most	Fewest	Most
Specification 1	0.459***	-0.045	-0.174	-0.214*	0.248**	-0.191**
St. Error	0.112	0.099	0.100	0.089	0.077	0.069
Specification 2	0.765***	0.055	-0.158	-0.247*	0.248**	-0.191**
St. Error	0.170	0.088	0.122	0.106	0.077	0.069
Specification 3	0.711***	0.028	-0.208	-0.285**	0.412***	-0.210**
St. Error	0.168	0.100	0.135	0.105	0.086	0.078
Sample Size	551,054					

* Significant at .05 level, ** Significant at .01 level, *** Significant at .001 level; All regressions include quartile $\times$ month fixed effects.

Figure 1: Food Insecurity Summary

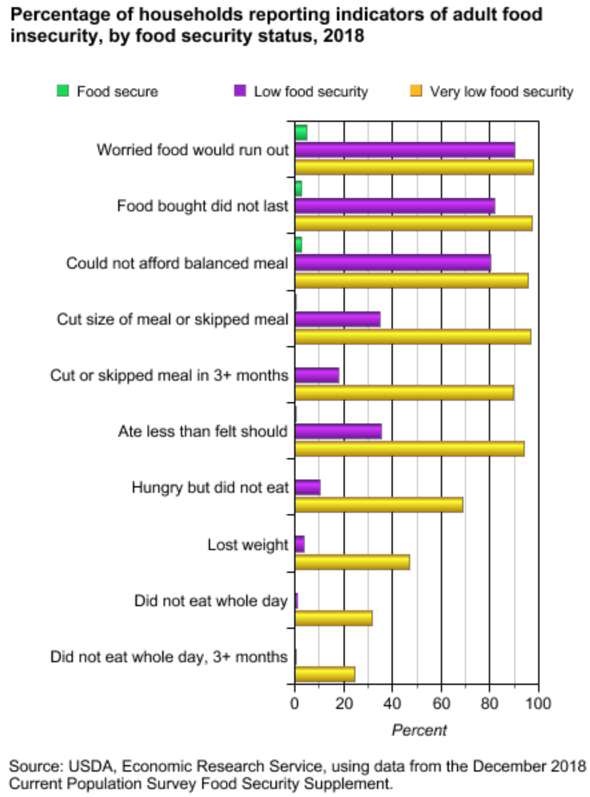


Figure 2: Minimum Wages in the different Localities in our dataset

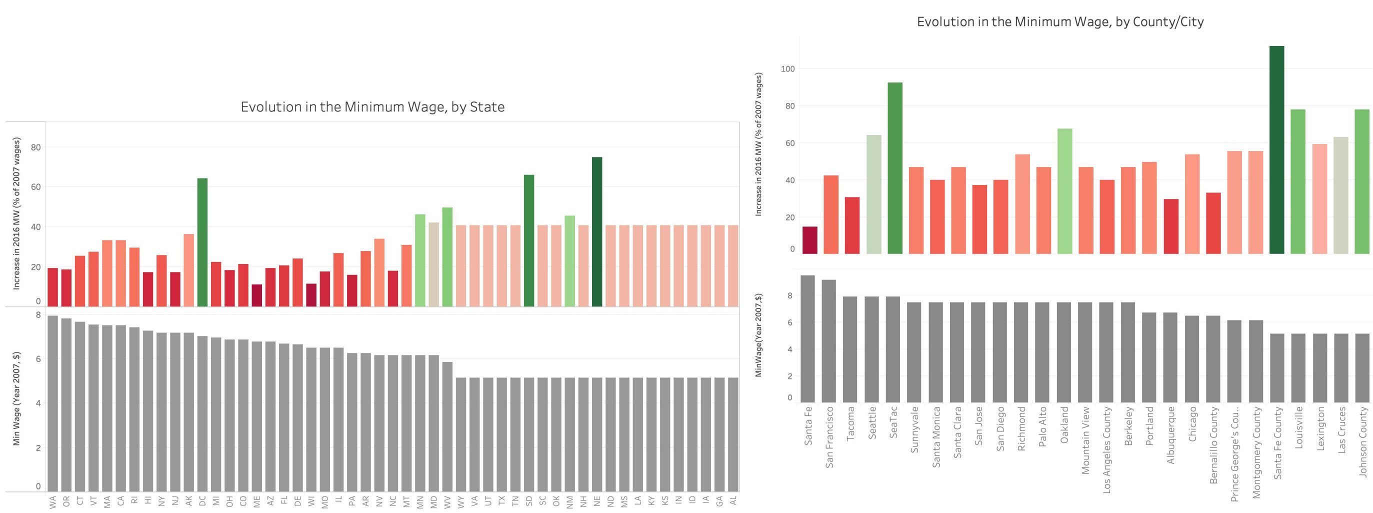


Figure 3: Average Daily Calories Purchased (Left Panel) and Health Indices (Right Panel) by Week of the Month

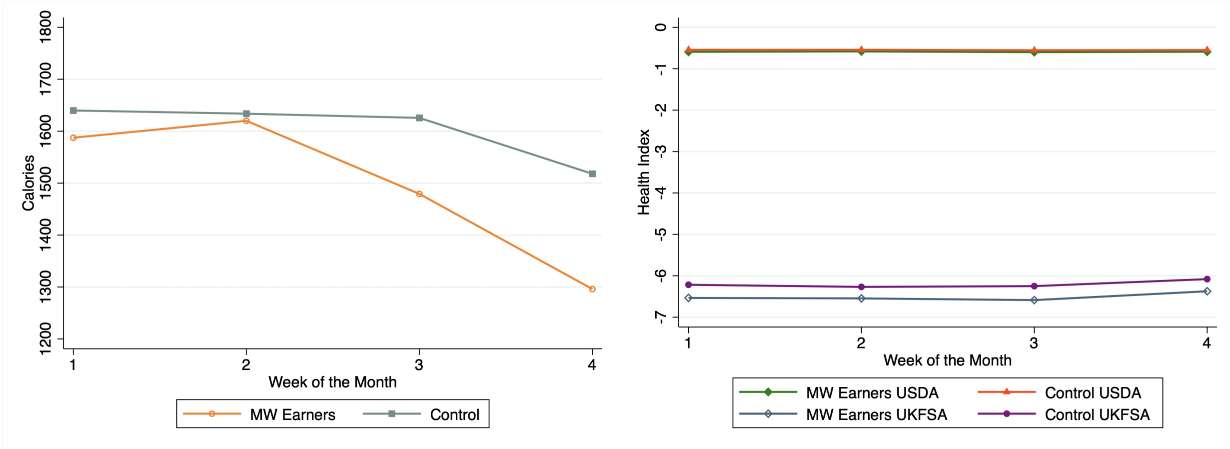


Figure 4: Robustness of the Effect of Minimum Wages on Calories

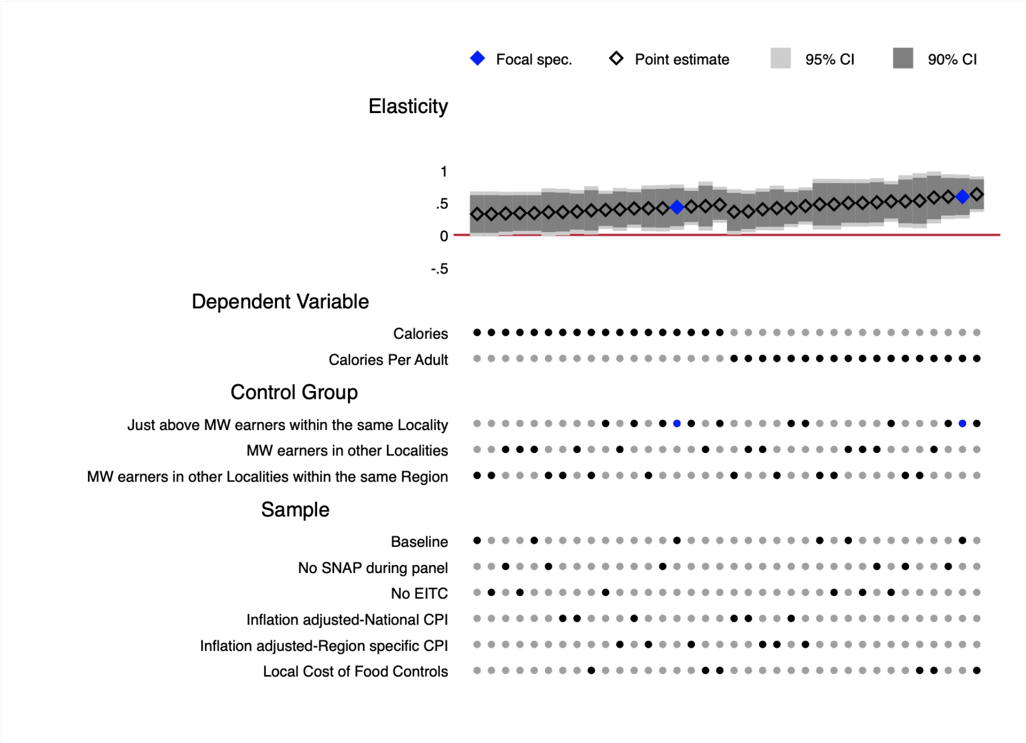
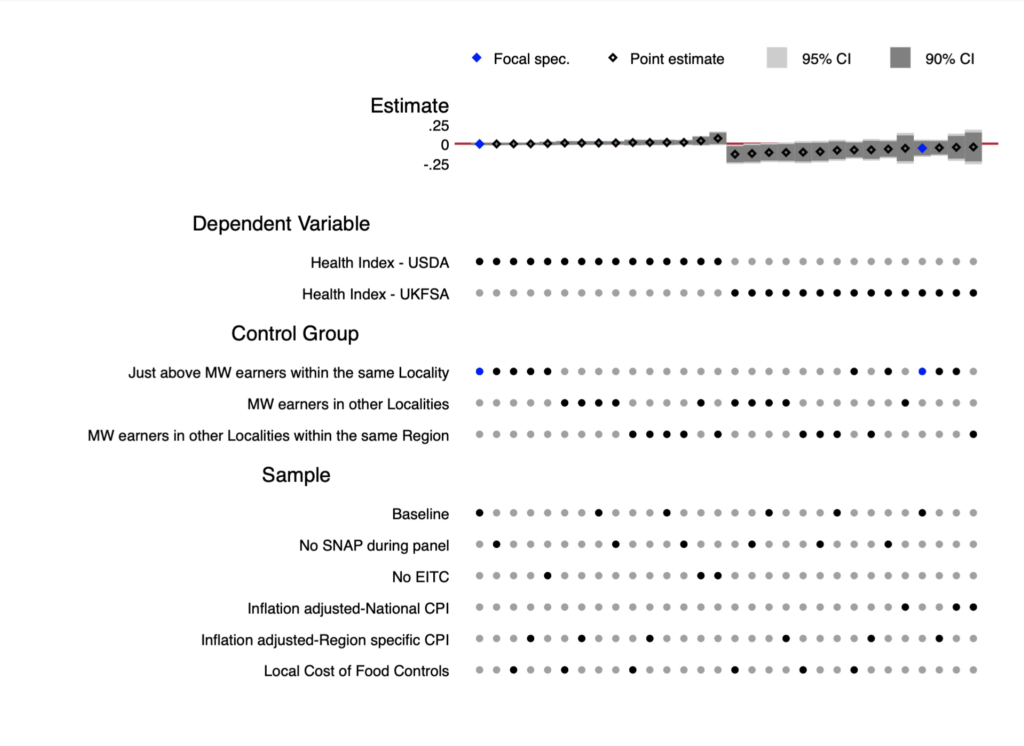


Figure 5: Robustness of the Effect of Minimum Wages on Dietary Health



Appendix: Additional Details on the Health Indices

Based on the macronutrient information available in our data, we use a variant of the HEI (Healthy Eating Index). We include Fiber in our index as we do not observe information on grains and refined grains; the primary distinction between the two is that refined grains have fiber stripped out. We are also unable to view the fatty acid content of purchases, and therefore exclude this component, just as Allcott, Diamond, Dube, Handbury, Rahkovsky and Schnell (2019) do. Further, as our data include information on sugar (but not added sugar specifically), we use the American Heart Association's recommended limit for sugar intake in place of the USDA's recommended limit for added sugar intake. Finally, the HEI distinguishes between different types of proteins, fruit, and vegetables; our data do not allow us to observe such differences.

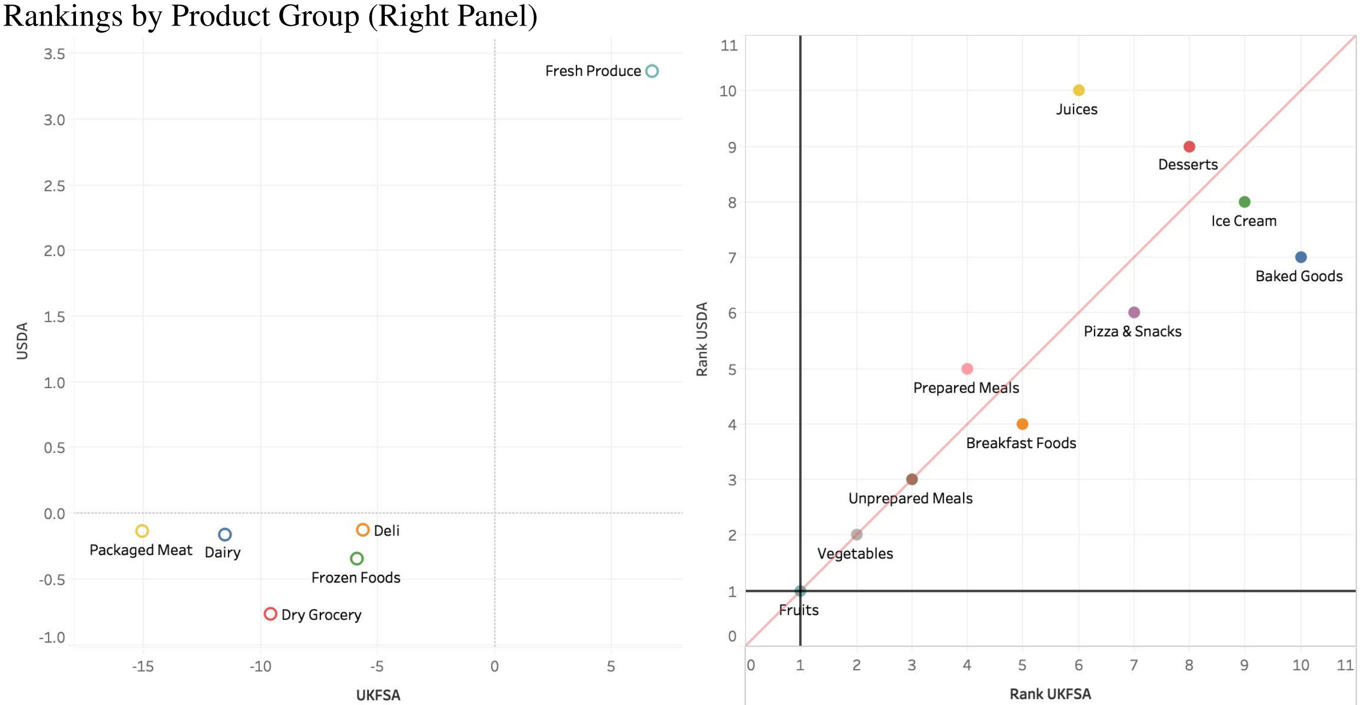
While the USDA's HEI is bounded, we construct a linearized version following Allcott, Diamond, Dube, Handbury, Rahkovsky and Schnell (2019) with no restrictions at the upper- or lower-bounds. We employ an "average" health index which corresponds to 1/8 * the health index that Allcott, Diamond, Dube, Handbury, Rahkovsky and Schnell (2019) construct by summing over all macronutrients. We use this "ratio" approach for constructing the HEI as it is relatively easy to interpret. However, our results are robust to adopting the summation HEI approach used in Allcott, Diamond, Dube, Handbury, Rahkovsky and Schnell (2019) (which is basically a summation counterpart of our ratio measure).

The scores generated from the UKFSA's Nutrient Profiling Model actually range between -15 and 40, where higher numbers are worse. By contrast, lower numbers are worse in the HEI's index. To keep the interpretation consistent across the two indices, we reverse the sign on the NPM's scores; in our index, lower numbers are worse, consistent with the HEI. This makes it easier to interpret conclusions based on our two indices. We refer the reader to the "Nutrient Profiling Technical Guidance" (UK OfCom, 2011) for the full description of how NPM's scores are calculated.

The HEI and NPM differ in a few ways. First, the HEI was designed to measure the healthiness of one's overall diet, and so measures the prevalence of macronutrients per 1,000 calories. By contrast, the NPM was designed to measure the prevalence of nutritional components per 100g for a specific UPC. Second, while many components are common to both indices (fruit, vegetables, protein, fiber, saturated fat, and sodium), two are not: dairy is only factored into the HEI, and calories are factored into a UPC's NPM as a dietary component (akin to protein or fiber). Third, the relative weight assigned to fruit and vegetables differs between the indices. The HEI treats fruit and vegetables as separate components, each given equal weight with other components such as protein, while the NPM treats them as a single component. Consequently, fruit and vegetables are weighted more strongly in the HEI.

The HEI and NPM, by virtue of their differences in construction, produce non-trivially different healthfulness ratings for UPCs across the six major food departments in the Nielsen data (Figure 6, Left Panel). The two indices agree that fresh produce contains the most healthful UPCs, but they diverge in their appraisal of other departments. The two indices also show reasonable agreement within-department. For example, within the Frozen Foods department, both indices largely agree on the most healthful groups (e.g., frozen fruit and vegetables), but do not agree as strongly on the less healthful groups (Figure 6, Right Panel).

Figure 6: UPC Health Index Scores by Department (Left Panel), Frozen Food UPC Health Index Rankings by Product Group (Right Panel)



Web Appendix A: Household Classification

The primary specification used for our analyses classifies households into one of two groups based on their reported income for a given year. However, income is only reported in buckets in the Nielsen data, and are therefore not precisely observed. This introduces a risk that some households might be misclassified as minimum wage households. To address this concern, we test the robustness of our primary estimates to alternate specifications of the treatment group, wherein we drop households most likely to be misclassified. Additionally, we test the robustness of our estimates to different definitions of our control group.

Potential Misclassification of "Border" households in Primary Specification

Our analyses in the main text classify a household as a "minimum wage earning" household if their income never exceeded the income bucket that included their locality's Minimum Wage Annual Salary Equivalent (MWASE). However, the income bucket containing a locality's MWASE will likely include some households earning above the minimum wage. For example, the income bucket that includes Michigan's 2014 MWASE of $16,952 consists of households that earn from $15,000-$20,000. Consequently, households earning between $16,952 and $20,000 were also classified as "minimum wage earning households." We refer to households in the income bucket containing their locality's MWASE as "border" households; it is unclear what side of the border (between households earning the minimum wage and those not) the household lies on. As a robustness check, we re-estimate our primary effects after dropping these "border" households from our sample. We use three (increasingly strict) screens for defining border households, excluding households that were on the "border" (1) 50% or more of their time in the sample, (2) 25% or more of their time in the sample, or (3) ever. Elasticity estimates for the calorie DVs are larger when we exclude border households than when we use the full sample, suggesting that the full sample provides a conservative estimate of the elasticity of calories purchased with respect to the minimum wage:

Table WA.1: Removal of "Border" Households

	[1]	[2]	[3]	[Full Sample]
Daily Calories	0.635***	0.669***	0.519*	0.459*
St. Err.	0.191	0.192	0.206	0.175
Daily Calories per Adult Equiv.	0.773***	0.794***	0.628**	0.589***
St. Err.	0.183	0.192	0.194	0.170
"Border" cut-off	50%	25%	Ever	n/a
Pct of MW HH from full sample	67%	60%	57%	100%
Observations	535,997	525,055	516,237	560,197

* Significant at .05 level, ** Significant at .01 level, *** Significant at .001 level

We also estimated the models for the Health Indices using the same screens for border households, and found that the results were highly insignificant ($p > 0.30$ for all three regressions, for each health index), similar to our primary health index analyses; they are omitted from the table above in the interest of space, but are readily available from the authors upon request.

Potential Misclassification of Households with Multiple Earners

Next, we conducted a test to address the possibility that minimum wage households with multiple earners might be misclassified into the control group, thereby biasing our estimates for specification 1 (specifications 2 and 3 do not utilize above minimum wage earners, and are therefore not contaminated by possible misclassification of control group households). Here, we systematically screen out households from within the control group (comprising just above minimum wage earners) based on how often they reported having more than one earner during their years on the Nielsen panel.[40] This excludes even those households whose earners report being employed part-time (for fewer than 30 hours per week). We use five such (increasingly strict) screens, removing households that report having more than one earner: (i) during every year that they were in the panel (ii) more than 75% of their time on the panel, (iii) more than 50% of their time on the panel, (iv) more than 25% of their time on the panel, and (v) *ever* during their time on the panel. Our

[40]A caveat to this test is that the treatment group may comprise households with multiple earners while the control group might not.

results are substantively robust to the use of these screens (see Table WA.2).

Next, we also perform a more conservative test wherein we only focus on a subsample of household years for which each household specifies having only a single earner. This screen retains 77% of our observations. For each of our four DVs (Calories, Calories per Adult Equivalent, the HEI, and the NPM), we re-estimate specification 1 on this subsample. We find that while the effect on calories per adult is significant at the conventional 5% level, the effect of calories is marginally significant at the 10% level - see Table WA.3. Furthermore, the estimates of the elasticities for calories are at the lower end of our robustness check estimates. This is consistent with our intuition, as the above analysis not only excludes potentially misclassified households from the control group, it also excludes all multi-earner households from the treated group. Critically, the excluded treated households are not misclassified, as they report jointly earning no more than a single, fully-employed minimum wage earner (our definition of the MWASE). Moreover, multi-earner households earning below the MWASE are more stably employed than single-earner households earning below the MWASE; multi-earners have at least one household head employed 96.2% of their years in the panel, while single-earner households are employed only 85.4% of their years in the panel. This approach therefore excludes the minimum wage households likely to benefit most from minimum wage increases. For these reasons, any possible misclassification of multi-earner households into the control group would likely render our estimates conservative.

Table WA.2: Removal of Dual Earners from the Control Group

	Calories		Calories Per Adult		HEI		NPM	
	Est.	St. Err.	Est.	St. Err.	Est.	St. Err.	Est.	St. Err.
Primary Sample	0.459*	0.178	0.589***	0.170	-0.004	0.011	0.054	0.070
Removing "control" HHs who reported having:								
>1 earner in HH in all years in panel	0.401*	0.180	0.527**	0.174	-0.005	0.011	0.058	0.071
>1 earner in HH for >75% of the time in panel	0.389*	0.180	0.519**	0.174	-0.006	0.011	0.055	0.071
>1 earner in HH for >50% of the time in panel	0.386*	0.182	0.508**	0.176	-0.006	0.011	0.060	0.071
>1 earner in HH for >25% of the time in panel	0.378*	0.186	0.512**	0.181	-0.007	0.011	0.053	0.070
ever having >1 earner in HH	0.380*	0.193	0.537**	0.187	-0.007	0.011	0.049	0.072

* Significant at .05 level, ** Significant at .01 level, *** Significant at .001 level

Table WA.3: Robustness Test considering only Single-Earner Households

	Calories	Calories per adult	HEI	NPM
Estimate	0.313+	0.413*	-0.015	0.029
St. Err.	0.180	0.185	0.013	0.085
p-value	0.086	0.029	0.235	0.731
Sample Size	430,959	430,959	425,297	423,901
Locality x Month FE	Yes	Yes	Yes	Yes

+ Significant at the 0.1 level, * Significant at .05 level

Alternate Control Group Definitions in Primary Specification

Our analyses in the main text classify a household as a "control" household if their income was (a) always above the income bucket that included their locality's Minimum Wage Annual Salary Equivalent, and (b) never above $40,000. Our analyses are robust to a wide range of plausible values for the upper-bound income threshold used for defining households' control group membership. Here, we provide the results for our primary calorie analyses using the following alternative thresholds: $45,000 [column 2], $10,000 above the MWASE [column 3], $15,000 above the MWASE [column 4], and $20,000 above the MWASE [column 5]. We show results from our primary specification in column [1] for reference.

Table WA.4: Alternate Control Group Definitions

	[1]	[2]	[3]	[4]	[5]
Daily Calories	0.459*	0.440**	0.817***	0.379*	0.396*
St. Err.	0.175	0.155	0.229	0.182	0.175
Daily Calories per Adult Equiv.	0.589***	0.557***	0.991***	0.553**	0.529**
St. Err.	0.170	0.156	0.212	0.179	0.192
Observations	560,197	755,972	193,758	325,352	503,904

* Significant at .05 level, ** Significant at .01 level, *** Significant at .001 level

Placebo Test of the Effect of Minimum Wages on Households Earning just above Minimum Wages

Previous research has noted that rising minimum wages may also influence the behaviors of households earning just above minimum wages (e.g., Cengiz et al., 2019; Neumark et al., 2000; Clemens et al., 2018). To alleviate concerns that our estimates may be contaminated due to potential spillovers of rising minimum wages on households in the above minimum wage group, we showed the robustness of our results to dropping households from the above minimum wage group from our analyses (Specifications 2 and 3 in the main text).

In this section, we also conduct a placebo test to examine changes in behaviors of above minimum wage households due to rising minimum wages. We estimate specifications 2 and 3 (with and without cost controls) but with only above minimum wage households included. If rising minimum wages spill over to influence purchase behaviors of households earning just above minimum wages, we should see a change in their purchase behaviors in the period surrounding minimum wage changes. We see no such changes in their behaviors, and estimated effect sizes are small, although the confidence intervals do span effects of potentially large economic magnitude (e.g., the 95% confidence interval for the elasticity of Calories per Adult Equivalent in column 3a is [-0.318, 0.101]) - see Table WA.5.

Table WA.5: Placebo Tests with Households earning just above Minimum Wages

	[2a]	[2b]	[3a]	[3b]
Daily Calories	-0.064	-0.061	-0.104	-0.097
St. Err.	0.098	0.100	0.103	0.103
Daily Calories per Adult Equiv.	-0.063	-0.059	-0.108	-0.102
St. Err.	0.097	0.099	0.105	0.106
Locality × Month Controls	-	-	-	-
Month Controls	Yes	Yes	-	-
Region × Month Controls	-	-	Yes	Yes
Locality-Month Food Cost Controls	-	Yes	-	Yes
Observations	466,787	466,787	466,787	466,787

+ Significant at .10 level, * Significant at .05 level, ** Significant at .01 level, *** Significant at .001 level

Web Appendix B: Model Validation

Assessing Parallel Trends between the Treated and Control Groups

We examine pre-treatment trends in (the natural log of) Daily Calories Purchased, (the natural log of) Calories per Adult Equivalent, and both Health Indices (USDA's Healthy Eating Index and the UK FSA's Nutrient Profiling Model) to ensure that they do not significantly differ between minimum wage earners and households earning just above the minimum wage, prior to minimum wage changes. We examine pre-treatment trends for both 9- and 12-month pre-treatment windows. Because some minimum wage changes occur very close together, the "pre-treatment period" for one minimum wage change may include a locality's previous minimum wage change. For this reason, we include in this test minimum wage changes for which the period prior to treatment does not overlap with a prior minimum wage change. This approach retains 97% of all minimum wage changes (299 out of 309) when using a 9-month window, and 89% (275 out of 209) when using a 12-month window.

We test the parallel trends assumption for each of the four DVs (Y_{ht}, for household h in period t) with the following regression:

$$Y_{ht} = \alpha_{hc} + \sum_{m=2}^{M} \pi_m I[t-m] I[MWASE]_h + \sum_{l=1}^{L} \sum_{c=1}^{C} \sum_{t=1}^{T} \lambda_{lct} + \varepsilon_{ht} \tag{5}$$

We include: (1) a separate fixed effect for each household for each minimum wage change (c) they experienced (α_{hc}), effectively allowing each household to have a different baseline that is specific to each minimum wage change, and (2) fixed effects for each locality (l) $\times$ minimum wage change (c) $\times$ Month (t). These fixed effects control for time trends (for a given DV Y_{ht}) among the control group prior to each minimum wage change in each locality. We cluster our standard errors at the locality level (the level at which wage changes are observed). The results are also robust to clustering standard errors at the level of each locality's individual minimum wage changes. The π_m parameters semi-parametrically test whether the "trends" in our DVs for minimum wage households differ from those for our control group. We also adopt a simpler parametric approach to test for pre-trends. Here, we use a linear time trend for the months prior to treatment in place of the interaction terms of pre-treatment month-specific dummy variables and the "minimum wage household" dummy ($\sum_{m=2}^{M} \pi_m I[t-m]$) used before. We find that none of the values of π_m are significantly different from zero for the Calorie DVs and the USDA Healthy Eating Index, and only one parameter (corresponding to three months prior to treatment for the Nutrient Profiling Model) is statistically significant at the 5% level. The linear pre-trend term is non-significant for all four DVs. Thus, pre-treatment trends do not appear to systematically differ between our treated and control groups.

Table WA.6: Test of Parallel Trends – Difference between Treatment and Control Group Trends

	Months Prior to MW Change (π_m): 12-Month Window											
	12	11	10	9	8	7	6	5	4	3	2	Linear
Daily Calories	-0.02	-0.02	-0.02	0.03	-0.01	0.06	0.02	-0.01	-0.04	0.03	0.00	0.00
St. Err.	0.04	0.04	0.04	0.04	0.04	0.04	0.03	0.04	0.04	0.03	0.03	0.00
Calories/Adult	-0.02	-0.02	-0.02	0.02	-0.01	0.06	0.01	-0.02	-0.04	0.03	0.00	0.00
St. Err.	0.04	0.04	0.04	0.04	0.04	0.04	0.03	0.04	0.04	0.03	0.03	0.00
USDA HEI	0.02	-0.07	0.00	-0.01	-0.01	0.02	0.01	-0.02	0.02	0.02	0.03	0.00
St. Err.	0.03	0.06	0.03	0.03	0.02	0.03	0.02	0.02	0.02	0.02	0.03	0.00
UKFSA NPM	0.00	-0.19	0.00	-0.09	-0.18	-0.06	0.02	-0.11	-0.01	-0.27*	-0.10	0.00
St. Err.	0.17	0.16	0.14	0.14	0.14	0.17	0.13	0.12	0.14	0.11	0.11	0.01
	Months Prior to MW Change (π_m): 9-Month Window											
				9	8	7	6	5	4	3	2	Linear
Daily Calories				0.00	-0.02	0.03	0.03	0.00	-0.02	0.03	-0.01	0.00
St. Err.				0.04	0.04	0.04	0.03	0.04	0.04	0.03	0.03	0.00
Calories/Adult				0.00	-0.02	0.02	0.02	0.00	-0.02	0.03	-0.01	0.00
St. Err.				0.04	0.04	0.04	0.03	0.03	0.04	0.03	0.03	0.00
USDA HEI				0.00	0.00	0.03	0.02	0.00	0.02	0.02	0.01	0.00
St. Err.				0.03	0.02	0.02	0.02	0.02	0.02	0.02	0.03	0.00
UKFSA NPM				0.03	-0.03	-0.15	0.04	-0.16	-0.06	-0.22*	-0.08	-0.01
St. Err.				0.13	0.14	0.19	0.11	0.12	0.12	0.09	0.10	0.01

* Significant at .05 level, ** Significant at .01 level, *** Significant at .001 level

DiD Approach using Minimum Wage "Windows"

The two-way-fixed-effects approach used in our primary analyses is akin to a difference-in-differences design. As a robustness check, for each minimum wage change observed in the data, we examine short, n-month pre/post windows surrounding each minimum wage change (n =1, 2, 3 and 4 months pre/post) in the style of a regression discontinuity, and employ a difference-in-differences design to estimate the effects of interest. Only households that were in the data for the entire pre/post window are retained for the analyses. This approach ensures that the pre- and post-treatment period are of equal length, and that we have a balanced panel (albeit at the cost of losing some data). Additionally, we only include minimum wage changes for which the pre/post window does not overlap with another minimum wage change's pre/post window.[41] We use the following specification:

$$Y_{ht} = \alpha_{hlc} + \beta I[MWASE]_h ln(MW_{lt}) + \sum_{l=1}^{L}\sum_{c=1}^{C}\lambda_{lct} I[POST]_{ht} + \varepsilon_{ht} \tag{6}$$

We include: (1) a separate fixed effect (α_{hlc}) for each minimum wage change (c) experienced by household (h) in a given locality (l), allowing a household's baseline to differ prior to each minimum wage change (the fixed effects α_{hc} are included in place of the household fixed effects used in our main analyses); and (2) a separate fixed effect for each locality (l) $\times$ minimum wage change (c) (λ_{lct}) combination. As in our main specification, the elasticity of interest is captured by β. We conduct our analyses for both (1) average daily calories and (2) calories per adult equivalent using 1-, 2-, 3-, and 4-month pre/post windows. The results for each analysis can be found in Table WA.7, alongside the estimated elasticities from our primary analyses for comparison. The estimates are similar to one another, and to the elasticities from our primary analyses–though the elasticity estimates from using a narrow 1-month pre/post window are not statistically significant, perhaps due to the smaller sample size.

[41] As the window used grows, a minimum wage change's window becomes more likely to overlap with a second minimum wage change's window. Further, recall that households choose to participate in the Nielsen panel at the beginning of each calendar year, while minimum wages may change at any time during the calendar year. A growing window size also increases the likelihood that a household was not in the panel for the entire length of the pre/post window. Thus, while the sample grows with window length, the number of minimum wage changes and number of households in the sample drop. We also confirmed that households' entry and exit from the Nielsen panel are not systematically related to the timing of minimum wage revisions in the data.

Table WA.7: Difference-in-differences using Minimum Wage "Windows"

	Shortening the Analysis Window				
Months before after each MW change:	1	2	3	4	Primary
Daily Calories	0.420	0.485*	0.362*	0.419*	0.459*
St. Err.	0.440	0.202	0.180	0.184	0.175
Daily Calories per Adult Equiv.	0.454	0.487*	0.368*	0.406*	0.589***
St. Err.	0.438	0.203	0.183	0.177	0.170
Observations	32,936	63,352	92,100	107,864	560,197
Households	8,694	8,416	8,204	7,618	19,375
Minimum Wage Changes (out of 309)	309	307	305	275	309

+ Significant at .10 level, * Significant at .05 level, ** Significant at .01 level, *** Significant at .001 level

Falsification Test

Between the years of 2010 and 2014, minimum wage changes were less common. Only 79 of 309 minimum wage changes (26%) occurred during this five-year period (half our sample period), and 37 of 74 localities did not change their minimum wage. We run a falsification test to further examine whether trends between the treatment group (minimum wage households) and the control group (households earning just above minimum wages) are truly parallel during the period when no minimum wage changes are occurring. We test whether trends in the four primary DVs differ between the treatment and control group during this period in localities where the minimum wage was constant. We regress our DVs on an interaction term of the household's treatment membership status and a linear month trend ($I[MWASE]_h \times m$). As before, we include household $\times$ locality fixed effects to control for changes in product preferences possibly arising from household migration across localities, and locality $\times$ Month fixed effects to control for unobserved locality-specific time trends. The coefficient on the interaction term captures any increasing/decreasing patterns in our DVs over time among minimum wage earners, *above and beyond their locality's time trend* (as estimated from the control group's purchase activities). We find no significant difference in trends between the two groups for any of the four variables: Calories (-0.001, SE 0.001), Calories per adult equivalent (-0.001, SE 0.001), USDA Healthy Eating Index (0.000, SE 0.001), or UKFSA NPS (-0.002, SE 0.002).

Web Appendix C: Accounting for Cost of Living differences and Ruling out Alternative Explanations

Accounting for differences in the cost of living

We conduct two robustness checks using alternative approaches to control for regional cost-of-living differences: We (1) use real (rather than nominal) minimum wages in the three specifications discussed in the text, and (2) include controls for food prices that vary at the locality level to control for changes in the cost of living.[42] For (1), we deflate the minimum wages in each month using National and region-specific CPI (consumer price index) measures compiled from the Bureau of Labor Statistics website (www.bls.gov). For (2), we compute the average price paid per 1,000 calories in locality l and month t, and include that (in natural-log form) as a covariate in the regression to proxy for changes to the cost of living in each locality. We interact the cost of food controls with the treatment membership indicator to allow for a possibly differential influence of changes in the cost of food, on calories purchased by minimum wage households. As before, the "control group" households' purchase activities serve to capture the influence of general time trends unrelated to the cost of food. We again find a strong positive impact of minimum wages on calorie purchases, even after including additional controls for changes in the cost of food over time (Table WA.8 and Table 3).

[42] The effect of changes in the cost of food in each locality cannot be separately identified from the locality-month fixed effects in this regression. So we use month fixed effects together with the cost of food controls in specification 1 for this robustness test. Since one should not use the same data to construct both the independent and dependent variables, we construct a measure of a locality's cost of food using all purchases made by households earning more than $40,000 per year (the cut-off for our "control" group).

Table WA.8: Cost of Living Controls

	Nominal Wages - adjusted using National CPI			Nominal Wages - adjusted using Region-specific CPI		
Specification:	1	2	3	1	2	3
Daily Calories	0.385**	0.336*	0.322^{+}	0.407**	0.357*	0.351^{+}
St. Err.	0.147	0.164	0.174	0.149	0.166	0.179
Daily Calories per Adult Equiv.	0.515**	0.491**	0.471*	0.414**	0.361*	0.355*
St. Err.	0.157	0.183	0.195	0.148	0.162	0.175
Observations	560,197	93,930	93,930	560,197	93,930	93,930

+ Significant at .10 level, * Significant at .05 level, ** Significant at .01 level, *** Significant at .001 level

Ruling out Alternative Explanations

It is plausible that some minimum wage households benefited from membership in the SNAP (food stamps) program or the Earned Income Tax Credit (EITC) during months where the minimum wage in their locality rose. In general, households' eligibility for benefiting from either policy should decrease with rising wages. However, controlling for such influences is important given that these instruments are especially aimed at helping the less fortunate (such as minimum wage earners). We assess the influence of each of these in sequence.

Prior research has documented a robust effect on food consumption of household participation in federal safety net programs such as the SNAP (Hastings and Shapiro, 2018). We use information available in the Nielsen dataset on whether households received SNAP assistance during their time in the panel, and re-estimate our models only on a group of households who did not. We find similar elasticities, helping us alleviate concerns about households' SNAP membership interfering with our estimate of the treatment effect of minimum wages.

We also modify our primary specification, including both our primary variable of interest ($I[MWASE]_h ln(MW_{lt})$) and that same variable interacted with a dummy variable equal to one if a household *ever* received SNAP assistance. This allows us to estimate the elasticities of interest for households that did not receive SNAP, and test whether these elasticities were significantly different for households that did receive SNAP at some point (about 20% of our minimum wage households in our data). We find that the elasticities do not differ between households that did and did not receive SNAP assistance (Table WA.9).[43]

More importantly, these results also show that the benefits conferred by rising minimum wages persist among households who, after experiencing wage increases, may no longer be eligible for, or choose not to request SNAP assistance.

Next, we discuss the role of the Earned Income Tax Credit (EITC). The EITC is primarily received by households in the months of February and March. Past research has argued that the EITC is primarily spent on paying bills or on durable goods.[44] Nonetheless, to alleviate concerns that EITC refunds may be contaminating our estimates, we re-ran our primary analyses excluding the months of February, March, and April from each year in our data. Our results are largely unchanged by the exclusion of these months (Table WA.9).

Table WA.9: EITC and SNAP Robustness Checks

	No SNAP	SNAP (dif)^	No EITC	Primary
Daily Calories	0.519**	-0.132	0.441*	0.459*
St. Err.	0.174	0.312	0.178	0.175
Daily Calories per Adult Equiv.	0.631***	-0.099	0.588***	0.589***
St. Err.	0.169	0.271	0.173	0.170
Observations	560,197		419,120	560,197

^ The difference between the elasticity of SNAP and No SNAP households.
* Significant at .05 level, ** Significant at .01 level, *** Significant at .001 level

Web Appendix D: Heterogeneity

In this section, we explore whether households' responsiveness to rising minimum wages systematically differs along demographics or employment levels. Intuitively, we may expect that demographic differences among households (such as household size, the presence/absence of kids,

[43]Nielsen provides a caveat about the possible noisiness in households' SNAP membership indicator in the dataset. It could be that SNAP participation rates are under-reported in the Nielsen data. Nonetheless, as the minimum wage increases, minimum wage households' eligibility and reliance on SNAP can be expected to only decline, which should reduce the salience of such contamination.

[44]https://www.chicagofed.org/~/media/publications/economic-perspectives/2008/ep-2qtr2008-part2-goodman-etal-pdf.pdf - accessed Sep 1, 2020.

education level of the head of household etc.) and differences in employment levels may influence how responsive households' calorie purchases are to minimum wage changes. However, we do not find significant differences along any of these dimensions, possibly due to the relatively small size of our sample of minimum wage households (3,301). Nonetheless, we present our results here to encourage future research on the topic, which may help uncover some interesting differences among minimum wage households.

Differences in Employment and Income among Minimum Wage Earners

Past research has documented that changes in the minimum wage laws may also motivate changes to households' employment status or employment hours. Recall that for our primary analyses, we screened out households that were unemployed more than 75% of their time in the Nielsen panel. We did so under the premise that such households were unlikely to be active participants in the labor force. This screen is conservative. As one might expect, many households at the bottom of the income distribution (that we retain for our analyses) do not work full time and are not employed consistently throughout the panel. They consequently report annual incomes well below that of the locality's MWASE (which corresponds to the wage for a household working full-time, for all 52 weeks of the year).

In this subsection, we discuss how our estimates of the elasticity of calories purchased with respect to the minimum wage vary by households' frequency of employment in the panel, and their reported annual income. Our model specification remains identical to the one discussed in the main text; we merely alter the screening criteria governing households' inclusion in our analysis sample for the analyses.

We begin by exploring the relationship between elasticities and employment frequency. We re-estimate our model on a sample of households that were employed more than X% of the time, where X is increased from 0% to full employment in 20% intervals. All estimates are statistically significant, and there does not appear to be a meaningful relationship between these elasticities and employment frequency during a household's time in the panel.

Table WA.10: Elasticities | Employment Frequency

	> 0%	> 20%	> 40%	> 60%	> 80%	100%
Daily Calories	0.373*	0.405*	0.445*	0.482**	0.424*	0.463*
St. Err.	0.161	0.167	0.193	0.159	0.178	0.185
Daily Calories per Adult	0.514**	0.539**	0.557**	0.520***	0.411**	0.451**
St. Err.	0.165	0.167	0.185	0.157	0.187	0.190
Observations	582,869	572,363	553,006	525,060	499,413	476,667

* Significant at .05 level, ** Significant at .01 level, *** Significant at .001 level

We next examine the relationship between elasticities and a household's reported income (among minimum wage earners), comparing households that reported earning half of the MWASE or less, to those who reported earning more than half. Given that minimum wages govern the lower bound of wages paid to any member of the labor force, as one might expect, households that report earning less than half the MWASE are more likely to report being under-employed or unemployed than households that earn more than half. We have a total of 3,301 minimum wage earning households in our data. For these analyses, we exclude households that report being at the "border" more than 50% of their time in the panel, as such households would be primarily lumped into the "above half the MWASE" group. This criterion leaves us with 2,202 households, of which 1,110 report having earned below half their locality's MWASE 50% or more of the time they were in the panel. We estimate the elasticities of interest - on both (1) the 1,110 households who typically (i.e., 50% of the time, or more) earn below half the MWASE, and (2) the remaining households, who typically (50% of the time or more) report earning above half the MWASE, but are not "border households". Overall, our estimates for the two groups are both strongly significant, but are not significantly different from each other (though the elasticity estimate for households earning below half the MWASE is larger than that for those earning above using the "Daily Calories" DV).

Table WA.11: Elasticities | Reported Income

	Below Half	Above Half
Daily Calories	0.745**	0.492*
St. Err.	0.227	0.226
Daily Calories per Adult Equiv.	0.786***	0.763***
St. Err.	0.229	0.222
Observations	502,338	499,827

* Significant at .05 level, ** Significant at .01 level, *** Significant at .001 level

Demographic Differences

We test for differences in households' responsiveness to minimum wage changes along five demographic variables: gender, household size, presence of children, education, race, and age. While we do not find statistically significant differences between groups, we do notice some large differences in the effect sizes. As mentioned earlier, the lack of statistical significance may be due to insufficient power: We have 3,301 minimum wage-earning households spanning 10 years and 74 localities. Recall that our estimates of the elasticity rigorously account for time trends, some even specific to each locality or region of the United States (West, Midwest, Northeast, South). In these analyses, treatment households are compared to control households within-locality or within-region, further limiting the degrees of freedom available for uncovering demographic differences across a rather small household size within each of these geographic units. Nonetheless, we present our results for completeness and transparency.

Recognizing possible sample size limitations, we study the role of each demographic variable in a separate regression. For each demographic variable, we identify a reference group (e.g., for household size, we use a household size of 1 as the reference group) and its associated estimate β_1, which serves as a baseline for interpreting the elasticity of calories purchased with respect to the minimum wage for all other demographic groups (see equation 7 below). Therefore, for all groups $d \neq 1$, the difference between their elasticity and the reference group's elasticity is given by β_d. If the estimate of β_d is significant, then we have evidence that group d's elasticity differs from the reference group's.

Because households can change demographic groups year-over-year (e.g., changes in family size over time), we include a household $\times$ locality $\times$ demographic group fixed effects (rather than merely household $\times$ locality). When a household changes demographic groups (e.g., when a household goes from having merely a male head to having a male and female head, perhaps due to marriage) they are treated as a new household in this analysis. Time trends for each demographic group are also controlled for with demographic $\times$ time fixed effects (λ_{dt}). Our regression equation is given by:

$$Y_{hlt} = \sum_{d=1}^{D} \alpha_{hld} + \beta_1 ln(MW_{lt}) I[MWASE]_h + \sum_{d=2}^{D} \beta_d ln(MW_{lt}) I[MWASE]_h + \sum_{l=1}^{L} \sum_{t=1}^{T} \lambda_{lt} + \sum_{d=1}^{D} \sum_{t=1}^{T} \lambda_{dt} + \varepsilon_{ht} \tag{7}$$

Our results are shown in Table WA.12. While we find no salient statistically significant differences, some differences are perhaps large enough to highlight. Households with a single head typically have a lower elasticity (consistent with later findings that larger households have larger elasticities). Among single-head households, female-led households have a much lower elasticity (this difference is marginally significant). The data are insufficient to make a strong claim as to why this may be the case, but single female heads are more likely to have children (18% vs 5%) despite not differing substantially from single male heads in terms of employment level (they are slightly more likely to be under-employed but also slightly less likely to be unemployed). Households with kids are, in general, less responsive to minimum wage changes than households without kids (possibly due to competing monetary constraints associated with raising kids, such as the cost of their education etc). Larger households are noticeably more responsive than smaller households, among households without kids; among households with kids, we see a u-shaped relationship, though this may be driven by the fact that households of four or more people typically include children among their members, and households with kids are generally less responsive to minimum wage changes.

Households with a college degree (among at least one of the household heads) are virtually indistinguishable from those without a college degree. With respect to age, we split households based on the oldest head's age. We use an age of 40 for the cut-off, since that is the middle of the age range BLS defines as "prime working age" (25-54). Here, too, households above and below the cut-off do not appear to differ with respect to their response to the minimum wage.

When examining race, we compare white households to non-white households, because non-white households make up a very small proportion of the Nielsen panel. Here we see another large difference in effect size (one that is close to significant using the Calories Per Adult DV), with white households estimated to have a much larger elasticity than non-white households.

Table WA.12: Elasticities | Demographics

	Avg Daily Calories			Calories Per Adult		
	Est.	St. Err	p	Est.	St. Err	p
Reference: Two Heads	0.744*	0.308	0.018	0.717*	0.282	0.013
Male Head Only	-0.289	0.518	0.578	-0.223	0.529	0.674
Female Head Only	-0.645^{+}	0.364	0.081	-0.462	0.306	0.136
Reference: No Kids	0.495**	0.173	0.006	0.624***	0.171	0.001
Kids	-0.358	0.520	0.493	-0.557	0.488	0.258
Reference: HH Size 1	0.285	0.192	0.142	0.288	0.193	0.141
HH Size 2	0.106	0.298	0.724	0.055	0.293	0.851
HH Size 3	0.634	0.472	0.184	0.583	0.455	0.205
HH Size 4+	-0.454	0.696	0.516	-0.335	0.638	0.602
Reference: HH Size 1, No Kids	0.241	0.192	0.215	0.241	0.193	0.215
HH Size 2, No Kids	0.206	0.330	0.535	0.174	0.321	0.589
HH Size 3, No Kids	0.984	0.950	0.304	0.866	0.853	0.314
HH Size 4+, No Kids	1.065	0.959	0.270	1.017	0.913	0.269
Reference: No College Degree	0.439*	0.175	0.015	0.581***	0.170	0.001
College Degree	0.089	0.370	0.811	0.083	0.377	0.827
Reference: Max Head Age 40+	0.429^{+}	0.223	0.058	0.566**	0.198	0.006
Max Head Age < 40	-0.003	0.529	0.996	0.000	0.478	0.999
Reference: White	0.548***	0.164	0.001	0.722***	0.154	0.000
Non-White	-0.362	0.349	0.303	-0.674^{+}	0.345	0.055

+ Significant at .10 level, * Significant at .05 level, ** Significant at .01 level, *** Significant at .001 level

Web Appendix E: Tracking Shifts in Households' Grocery Purchases

New (Previously Unpurchased) UPCs

Given that some households are purchasing considerably more food, one might wonder whether households are purchasing more of the same UPCs, or trying UPCs they had not previously purchased. Previous research has shown that low-income households are reluctant to try new foods because they fear spending money on items they may not like (Daniel, 2016). Does an increase in the minimum wage make these households more open to purchasing new UPCs?

To address this question, we construct three new variables: (1) $NewUPCs_{ht}$, the total number of previously unpurchased UPCs that household h purchased in month t, (2) $OldUPCs_{ht}$, the total number of previously purchased UPCs that household h purchased in month t, and (3) $PctNew_{ht}$, the *percentage* of UPCs purchased by household h in month t that were previously unpurchased. Together, the first two DVs tell us the volume of UPCs households purchased in month t that were and were not purchased prior to month t, while the third measures the share of purchased UPCs that were new UPCs. Tracking changes on either measure is potentially important for managers.

We use our quartile-specific regressions to estimate the effect of the minimum wage on our three dependent variables, for each calorie quartile, with one modification: we add fixed effects for the number of months that household h had been in the panel at month t ($I[Months_{ht} = m]$). These fixed effects control for a cold-start problem: we cannot observe which UPCs a household purchased before they began participating in the panel, and the DV of interest is therefore decreasing over a household's time in the panel (as the total number of unique UPCs purchased by households can only increase with time).

We find that households in the lowest quartile of households with respect to their ex ante purchases of calories—those that also purchased more calories as the minimum wage increased—purchase more of both "new" and "old" UPCs in fairly similar proportions. Their elasticity of new

UPCs w.r.t. the minimum wage is 0.87, while their elasticity of old UPCs w.r.t. the minimum wage is 0.70; their relative percentage of new (versus old) UPCs did not change (Table WA.13).

What do these elasticities translate to in terms of raw UPCs purchased? The median minimum wage change in the data is 6.6%; the median minimum wage household in the data purchases 50 UPCs per month. In steady-state (after the aforementioned "cold start"), about a third of UPCs purchased in a given month were never previously purchased by a household. Based on a simple back of the envelope calculation, this suggests that the median minimum wage increase of 50 cents led the median household to purchase an additional 1.1 UPCs per month that they had never purchased before (vs an additional 2.2 UPCs per month that they had purchased before).

Interestingly, however, households above the median with respect to their ex ante purchases of calories—who *did not* purchase more calories in response to the minimum wage—also purchase more new UPCs, but do so by modestly shifting their purchases way from old UPCs towards new UPCs. For these households, a $1 increase in the minimum wage corresponds to a 1.3% increase in new UPCs purchased per month. Based on similar back of the envelope calculations, we find that the median minimum wage change (50 cents) would encourage these households to purchase one more new UPC (and one fewer old UPC) once every 3 months (0.65% of the 50 UPCs purchased per month get shifted from "old" to "new"—0.325 UPCs per month).

Table WA.13: New and Old UPCs: Quartiles and Median Split

	Quartiles			
Total UPCs Purchased	Fewest	Q2	Q3	Most
Elasticity: New UPCs	0.867***	0.199	0.295	0.226
St. Error	0.215	0.174	0.204	0.133
Elasticity: Old UPCs	0.704**	0.376	-0.184	-0.258
St. Error	0.227	0.208	0.278	0.220
Percent of UPCs that were "new"	Fewest	Q2	Q3	Most
Effect of $\triangle$$1 to the Min. Wage	0.43%	-0.66%	1.50%**	1.21%*
St. Error	0.62%	0.73%	0.46%	0.57%
Sample Size	560,197			

* Significant at .05 level, ** Significant at .01 level, *** Significant at .001 level

Substitution between food departments

Are households changing the healthfulness of what they eat by changing how much they eat from the six major food "departments" in the Nielsen data (dairy, deli, dry grocery, fresh produce, frozen foods, and package meats)? We test to see whether the percentage of household h's spending in month m on foods from department d ($Spend_{hmd}$) changed in response to minimum wage increases:

$$Spend_{hmd} = \alpha_{hl} + \sum_{q=1}^{Q} \beta_q I[Qrt_h = q] I[MWASE]_h MW_{lt} + \sum_{l=1}^{L}\sum_{t=1}^{T} \lambda_{lt} + \sum_{q=1}^{Q}\sum_{t=1}^{T} \psi_{qt} + \varepsilon_{ht} \tag{8}$$

Neither the least healthful (bottom quartile by health index) nor most healthful (top quartile by health index) households change what percentage of their spending is allocated to any food department in response to the minimum wage rising. This suggests that changes to healthfulness must be occurring within-department—e.g., eating more healthful frozen foods, rather than substituting fresh produce for frozen foods.

Table WA.14: Percentage of Spending by Department

	Dairy		Deli		Dry Grocery		Frozen		Produce		Meat	
Q. 1	HEI	NPM	HEI	NPM	HEI	NPM	HEI	NPM	HEI	NPM	HEI	NPM
Est.	0.0%	-0.1%	0.4%	-0.1%	-0.8%	0.7%	0.4%	-0.4%	-0.1%	-0.3%	0.0%	0.1%
St. Err	0.2%	0.7%	0.8%	0.3%	1.0%	0.6%	0.4%	0.3%	0.2%	0.3%	0.1%	0.2%
Q. 4	HEI	NPM	HEI	NPM	HEI	NPM	HEI	NPM	HEI	NPM	HEI	NPM
Est.	0.1%	-0.4%	-0.4%	0.8%	0.2%	-0.1%	0.1%	0.0%	-0.2%	0.0%	0.2%	-0.3%
St. Err	0.7%	0.4%	0.4%	0.6%	0.6%	0.8%	0.4%	0.5%	0.4%	0.2%	0.2%	0.2%
Obs.	560,027											

No results are significant at .10 level or better

"Healthful" and "Unhealthful" UPCs

Our primary analyses show that some minimum wage households at the extremities of the index representing their dietary health purchase more or less healthfully in response to the minimum wage rising. However, such shifts in health indices are not driven by substitution between product departments. In an effort to translate our causal estimates of minimum wage's impact on households' health indices to tangible terms, we examine the extent to which households became more or less inclined to purchase UPCs classified as "healthful" or "unhealthful" in response to the minimum wage rising.

We classify UPCs using a simple median split of the UKFSA's NPM health index (which is specifically designed to measure the healthfulness of *UPCs*, rather than diets) and label UPCs whose score lies above the median as "healthful." We then calculate, for each household h and each month t, the percentage of calories purchased from UPCs classified as "healthful". On average, minimum wage households acquire 42% of their calories from "healthful" UPCs and 58% from "unhealthful" UPCs.

We estimate the relationship between this DV and the minimum wage using the following regression equation (identical to equation 4, but with a different DV):

$$PctGood_{ht} = \alpha_{hl} + \sum_{q=1}^{Q} \beta_q I[Qrt_h = q] I[MWASE]_h MW_{lt} + \sum_{l=1}^{L}\sum_{t=1}^{T} \lambda_{lt} + \sum_{q=1}^{Q}\sum_{t=1}^{T} \psi_{qt} + \varepsilon_{ht} \tag{9}$$

We find that for quartile 1, a \$1 increase to the minimum wage is associated with 1.9% of calories being switched from "bad" UPCs to "good" UPCs (SE=0.59%). For quartile 4, we see roughly the opposite effect—a \$1 increase to the minimum wage is associated with 2.0% of calories being switched from "good" UPCs to "bad" UPCs (SE=0.48%). We try to translate these estimates to more tangible terms, scaling these percentage terms up to the monthly level by computing the number of calories that shift from the "unhealthful" to the "healthful" bin of UPCs (for quartile 1) and vice-versa (for quartile 4) for a household that purchases 2,000 calories per day. Our back of the envelope calculations suggest that a minimum wage earner from the bottom (i.e., least healthful) quartile shifts 1,140 calories from unhealthful UPCs to healthful UPCs per month in response to a one dollar change in the minimum wage. For illustration, over the course of a month, such a shift is equivalent to replacing two meals consisting of frozen pizza with healthier frozen meals.

By contrast, we find that for a minimum wage earner from the top (most healthful) quartile, rising minimum wages are associated with a shift of 1,200 calories from healthful UPCs to unhealthful UPCs per month—akin to (for example) replacing slightly over two healthful meals with 1.2 pints of Ben & Jerry's Ice Cream (1,000 calories per pint).

Healthfulness of "new" (and "old") UPCs

We examine whether the minimum wage influences the average healthfulness of new and/or previously purchased foods in households' shopping baskets. To do this, we calculate the HEI and NPM for the subset of UPCs in household i's shopping basket during month t that (i) were and (ii) were not purchased previously. We use four new DVs—HEI_{ht}^{New}, HEI_{ht}^{Old}, NPM_{ht}^{New}, and NPM_{ht}^{Old}—in regression equation 4.

Across our three specifications and two health indices, our estimates are directionally similar to each other (Table WA.15), and consistent with our estimates of the effect of the minimum wage on each quartile's health indices for all UPCs: the (ex ante) least healthful households purchase more healthful UPCs in response to the minimum wage rising, while the (ex ante) most healthful households purchase less healthful UPCs in response to the minimum wage rising. Interestingly, this pattern holds regardless of whether households are buying a UPC for the first time, or buying UPCs they had previously purchased. Households appear to be making modifications to the healthfulness of their purchases both when they are buying new UPCs, and when they are selecting from within the set of UPCs they had previously purchased. However, the estimated effect using the NPM appears to be stronger for previously purchased UPCs. For example, for the (ex ante) least healthful households, the average (across the three specifications) change in NPM for new UPCs is 0.905 for previously purchased UPCs and 0.450 for new UPCs. However, even though the (average) magnitude of the change in the NPM for new UPCs is twice the size of that for previously purchased UPCs, this difference is not statistically significant.

For completeness, we also performed the above analyses using households stratified into quartiles based on their (pre-minimum wage increase) calorie purchases. We find no consistent evidence of systematic shifts in the dietary health index for old or new UPCs, for households ranking within the top or bottommost quartiles on calories purchased (see Table WA.16).

Table WA.15: New and Old UPCs: Healthfulness

	Spec. 1		Spec. 2		Spec. 3	
Effect of a 1$ increase in the MW on USDA HEI	Q1	Q4	Q1	Q4	Q1	Q4
New UPCs	0.052**	-0.029**	0.042**	-0.045+	0.046**	-0.045+
St. Error	0.019	0.011	0.013	0.026	0.015	0.025
Old UPCs	0.028	-0.068**	0.058**	-0.068**	0.057**	-0.075***
St. Error	0.022	0.027	0.019	0.025	0.020	0.022
Effect of a 1$ increase in the MW on UKFSA NPM	Q1	Q4	Q1	Q4	Q1	Q4
New UPCs	0.493***	-0.046	0.475***	-0.327+	0.383**	-0.401*
St. Error	0.123	0.121	0.139	0.170	0.136	0.171
Old UPCs	0.670***	-0.445**	1.046***	-0.420**	0.999***	-0.464**
St. Error	0.186	0.139	0.246	0.146	0.248	0.153
Sample Size	560,197					

+ Significant at .10 level, * Significant at .05 level, ** Significant at .01 level, *** Significant at .001 level

Table WA.16: New and Old UPCs: Healthfulness for Households Stratified by Calories Consumed

	Spec. 1		Spec. 2		Spec. 3	
Effect of a 1$ increase in the MW on USDA HEI	Q1	Q4	Q1	Q4	Q1	Q4
New UPCs	0.024	-0.012	0.002	-3.5e-6	0.004	9.71e-5
St. Error	0.019	0.012	0.011	0.010	0.012	0.011
Old UPCs	-0.02	-0.030	-0.033	-0.021	−0.037+	-0.026
St. Error	0.02	0.020	0.020	0.018	0.022	0.018
Effect of a 1$ increase in the MW on UKFSA NPM	Q1	Q4	Q1	Q4	Q1	Q4
New UPCs	-0.033	0.078	-0.213	-0.103	-0.307*	-0.177
St. Error	0.110	0.113	0.133	0.151	0.140	0.144
Old UPCs	-0.090	-0.003	-0.254	0.052	-0.284	0.048
St. Error	0.257	0.161	0.257	0.177	0.271	0.179

+ Significant at .10 level, * Significant at .05 level.

Web Appendix F: Effect of Minimum Wages on the Purchase of Specific Macronutrients

The benefit of using the composite health index measures (developed by the USDA and UK's Ofcom, which aggregate over all the different macronutrients) is that they provide an intuitive interpretation of how overall dietary health evolves in response to minimum wage changes. Nonetheless, in this section we also discuss whether and to what extent the purchase of specific macronutrients are influenced by the minimum wage.

We calculate the log of the quantity of each component of each health index (per 1,000 calories for HEI; per 100g for NPM) purchased by household h in month t. On average (across all minimum wage households), we find no material changes in the purchase of any specific macronutrient (see Figure WA.1). As a next step, we examine whether we see any shifts in the macronutrient purchases for households belonging to the (ex ante) healthiest and unhealthiest quartile on the dietary health indices (the two quartiles which exhibit movements in dietary health following minimum wage increases). We utilize our quartile-specific regression specifications to estimate the (ex ante) most and least healthful quartiles' elasticity of each measure with respect to the minimum wage. Note that while we were unable to take the log of the composite health indices, as they could take either positive or negative values, we are able to take the log of the absolute value of each macronutrient's index. In the estimates below, a positive coefficient indicates an increase in the *quantity* of a macronutrient consumed; sign in this context is *not* an indicator of impact on overall healthfulness.

To summarize the key results in the table below, we find evidence that the (ex ante) least healthful households may improve their overall health indices by decreasing their calories-per-gram (which is only included in the NPM index; significant or marginally significant in all three specifications), increasing their fiber (significant in all three specifications for HEI; n.s. for NPM), decreasing their sugar (significant in specifications 2 and 3 for both HEI and NPM; n.s. for specification 1), and decreasing their saturated fat (significant for all three specifications for the NPM, n.s. for specification 2 and 3 of the HEI, and significant in the opposite direction for specification

1 of the HEI). We also find evidence that the (ex ante) most healthful households may see a decline in their overall health indices by increasing their sugar (significant for both indices and all specifications except specification 1 for NPM) and saturated fat (significant for specifications 2 and 3 for NPM only). There is not much evidence of movement with respect to produce, protein, or dairy for either set of households. There is weak evidence that households shift their purchase of sodium (which is included only in the HEI), with the least healthful households reducing their purchase of sodium and the most healthful households increasing their purchase of sodium.

Figure WA.1: Shifts in the purchase of specific macronutrients (across all minimum wage households)

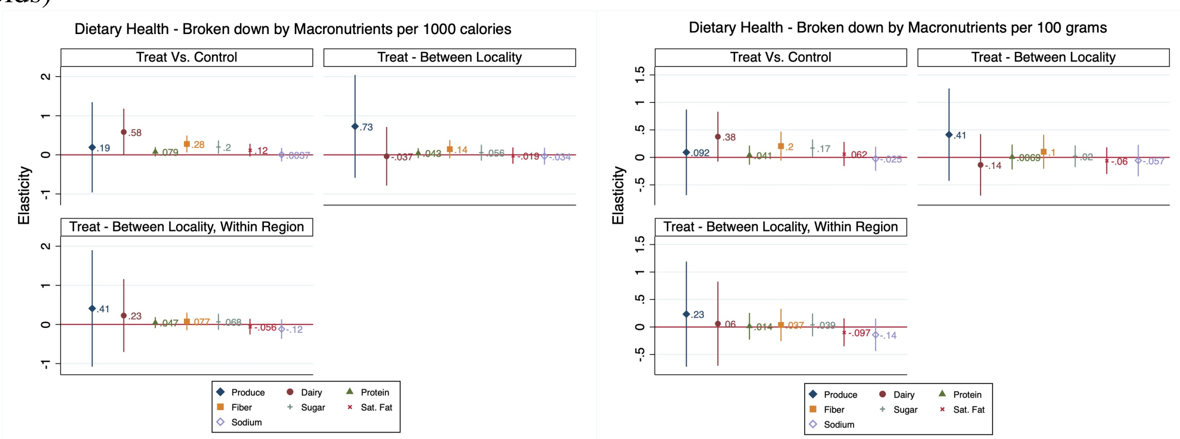


Table WA.17: Shifts in the purchase of specific macronutrients (Top and Bottom Quartiles)

	Spec. 1		Spec. 2		Spec. 3	
	Q1	Q4	Q1	Q4	Q1	Q4
Calories per 100 grams (HEI)	-0.257^{+}	0.199	-0.394*	0.287	-0.360*	0.308
p-value	0.058	0.225	0.028	0.139	0.025	0.110
Cups of produce per 1kcals (HEI)	0.460	1.696^{+}	1.182	0.491	0.815	0.286
p-value	0.543	0.091	0.272	0.668	0.481	0.807
Cups of produce per 100g (NPM)	0.625	0.090	1.475^{+}	0.161	1.214	-0.015
p-value	0.337	0.904	0.091	0.865	0.139	0.988
Cups of dairy per 1kcals (HEI)	1.150^{+}	0.761	-0.122	0.921	0.149	1.117
p-value	0.088	0.298	0.869	0.320	0.832	0.224
Cups of dairy per 100g (NPM)	-0.394	0.407	-0.664	0.686	-0.395	0.866
p-value	0.298	0.529	0.224	0.179	0.514	0.097
Protein per 1kcals (HEI)	0.156	0.005	0.184	-0.061	0.200	-0.050
p-value	0.221	0.952	0.243	0.607	0.200	0.699
Protein per 100g (NPM)	-0.079	0.132	-0.131	0.126	-0.102	0.146
p-value	0.540	0.498	0.429	0.571	0.554	0.503
Fiber per 1kcals (HEI)	0.500**	0.330	0.555**	-0.309	0.535**	-0.370
p-value	0.008	0.184	0.009	0.287	0.009	0.205
Fiber per 100g (NPM)	0.213	0.416	0.183	0.414	0.161	0.346
p-value	0.300	0.210	0.470	0.224	0.504	0.293
Sugar per 1kcals (HEI)	0.036	0.620**	-0.416**	0.712*	-0.452**	0.696*
p-value	0.779	0.003	0.002	0.026	0.001	0.027
Sugar per 100g (NPM)	0.036	0.228	-0.581**	0.390^{+}	-0.527*	0.431^{+}
p-value	0.839	0.191	0.009	0.080	0.018	0.052
Saturated Fat per 1kcals (HEI)	0.479**	0.237	0.174	0.345	0.162	0.326
p-value	0.007	0.238	0.443	0.175	0.463	0.205
Saturated Fat per 100g (NPM)	-0.364*	0.364	-0.710***	0.571*	-0.682***	0.532*
p-value	0.036	0.222	0.000	0.016	0.000	0.035
Sodium per 1kcals (HEI)	-0.165	0.354*	-0.262	0.222	-0.393*	0.180
p-value	0.300	0.039	0.100	0.291	0.041	0.354

+ Significant at .10 level, * Significant at .05 level, ** Significant at .01 level, *** Significant at .001 level

Web Appendix G: Out-of-Home Food Purchases

Past research has highlighted that the Nielsen dataset does not include information on households' food choices away from home (e.g., restaurants, fast food locations etc.). However, Allcott, Diamond, Dube, Handbury, Rahkovsky and Schnell (2019) note that for all income brackets in the U.S., the share of healthy and unhealthy macronutrients (protein, carbohydrates, saturated fat, etc.) consumed away from home tends to be about the same as the share of calories consumed at home. This suggests that grocery purchases are not a systematically biased measure of overall diet healthfulness. To confirm this, we utilize data on household food spending and self-reported restaurant visitation behaviors available as part of Mediamark Research's (MRI-Simmons©) Annual Survey of the American Consumer, accessible via the "SimplyAnalytics" database, to explore whether households' out-of-home food purchasing differed systematically in the period after minimum wages rose.

The MRI survey produces local estimates of usage and consumption (propensity) for thousands of specific and detailed products and services. We estimate the relationship between the following annual ZIP code-level measures and the minimum wage: a) household-average spending on meals at restaurants, carry outs etc., b) percentage of households that reported visiting fast food and drive-in restaurants once or more over the last 6 months, c) percentage of households that reported visiting the following relatively "healthy" out-of-home venues (i) Chipotle, ii) Panera Bread, iii) Subway) and relatively "unhealthy" out-of-home venues (iv) Krispy Kreme Doughnuts, v) McDonalds, vi) Cheesecake Factory).

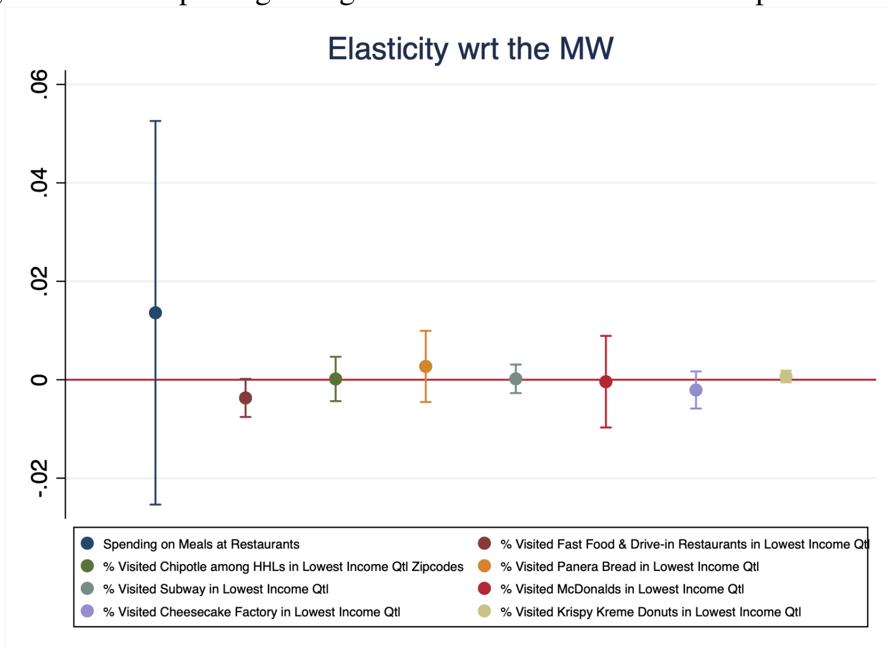
Given the nature of these data, our focus here is not on making causal claims, but to examine whether data patterns for households' out-of-home food purchases might contradict our primary conclusions.

As a further test, we drill down to focus only on a subsample of ZIP codes that rank within the lowest quartile in the country on average household income (as of 2016). These ZIP codes likely have a higher proportion of minimum wage earners, and so serve as a proxy for out-of-home food purchase activity for minimum wage earning households.[45] We find no evidence to suggest that the directionality of our main conclusions based on households' grocery purchases is likely to be overturned based on out-of-home purchase activity (see Figure WA.2). We find no

[45] We also find no significant effects when we consider only the zip codes that rank within the second lowest quartile on average household income. Similarly we find no differential effects among the two lowest wage quartile zip codes. These results are omitted due to space but are readily available with the authors on request.

statistically significant correlations at the ZIP code level between households' out-of-home food purchase activities and minimum wage revisions. This boosts our confidence that our view of households' food purchase activities is unlikely to be a biased measure of households' calorie intake patterns and diet healthfulness.

Figure WA.2: Exploring changes in out-of-home food consumption activities



Web Appendix H: Robustness of the Effect of Minimum Wages on Dietary Health to Functional Form

In this section, we examine the robustness of our conclusions about dietary health to using the logarithm of minimum wages in place of minimum wages specified in levels in Equations (2) and (4) of the paper. We see that that our substantive conclusions are verified when we use the natural log of minimum wages in place of minimum wages specified in levels. In fact, the significance of our estimates is stronger when we use the log of minimum wages (MW).

	Quartiles				Average effect
Effect of a 1% MW increase on HEI	Fewest	Q2	Q3	Most	
Specification 1	0.225*	-0.093	-0.197	-0.346*	-0.029
St. Error	0.096	0079	0.100	0.146	0.059
Specification 2	0.417**	0.034	0.054	-0.419*	0.070
St. Error	0.135	0.094	0.318	0.169	0.100
Specification 3	0.531***	0.091	0.165	-0.385*	0.122
St. Error	0.153	0.107	0.376	0.169	0.137
Effect of a 1% MW increase on NPS	Fewest	Q2	Q3	Most	Average effect
Specification 1	3.239***	-0.505	-1.799	-1.733*	0.412
St. Error	0.829	0.805	0.747	0.676	0.431
Specification 2	5.674***	0.239	-1.073	-2.377**	0.858
St. Error	1.255	0.716	0.991	0.809	0.498
Specification 3	5.291***	0.074	-1.369	-2.583**	0.710
St. Error	1.255	0.781	1.087	0.794	0.526
Sample Size	553,416				91,672

* Significant at .05 level, ** Significant at .01 level, *** Significant at .001 level; All quartile regressions include quartile $\times$ month fixed effects.

Web Appendix I: Match Rates for UPCs in our data

We show the evolution of match rate of UPCs in our dataset. Recall that the nutrition label data only contains UPCs that were carried in stores in 2018 (the year the data was acquired), but the Nielsen data runs from 2007-2016. Consequently, not all UPCs in the Nielsen data set can be matched to the nutrition label data set, and the match rates are predictably lower for earlier years than for later years. While the match rates increase systematically over time, crucially they do not vary differentially by income group over time (see Table WA.18), alleviating concerns that differences in nutrition label match rates may induce biases in our estimates.

Table WA.18: Match rates by year and income

	Spend Match#		*UPC Match*##	
	MinWage	Control	MinWage	Control
2007	39%	39%	39%	42%
2008	41%	42%	42%	44%
2009	45%	45%	46%	47%
2010	48%	48%	50%	50%
2011	50%	50%	51%	52%
2012	47%	47%	51%	52%
2013	49%	49%	54%	55%
2014	52%	52%	58%	58%
2015	54%	54%	61%	61%
2016	54%	54%	61%	61%

Spend Match: Percent of dollars spent on purchased UPCs matched to nutrition label data set. ## UPC Match: Percent of purchased UPCs matched to nutrition label data set.