

UNCOVERING THE ROLE OF MORAL HAZARD IN HEALTH INSURANCE*

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Abstract

This paper investigates whether there is evidence of ex-ante moral hazard in health insurance—i.e., whether lower out-of-pocket costs lead to risky health behaviors. Understanding the role of ex-ante moral hazard is crucial, given that it can contribute to preventable health issues. I leverage the staggered rollout of U.S. state policies lowering insulin out-of-pocket costs and focus on privately insured households with diabetes. Using household-level grocery purchase data, I find that reduced insulin out-of-pocket costs result in increased purchases of sugar, a nutrient closely tied to insulin needs and linked to long-term health risks. Sales of diabetes supplies also rise, highlighting a shift from lifestyle management to treatment.

Keywords: moral hazard, risky behaviors, health insurance, dietary choice, diabetes.

JEL: D12, H31, H51, I12, I18.

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1 Introduction

Healthcare spending in the U.S. has risen steadily over recent decades, averaging 17.5% of GDP between 2016 and 2019 (CMS, 2023), with similar trends in other OECD countries (OECD, 2019). Due to aging populations, both care demands and costs are expected to rise even further. Therefore, policymakers face the challenge of balancing the financial sustainability of the healthcare system and access to care. Policies that reduce out-of-pocket costs for patients, such as the U.S. Inflation Reduction Act, aim to improve access and affordability. However, their effect on patient behavior is ambiguous. While such policies may lead to greater healthcare utilization (Brot-Goldberg et al., 2017), they may also increase incentives to engage in risky behaviors due to reduced financial exposure to medical costs (Ehrlich and Becker, 1972).

In this paper, I evaluate the effects of reducing out-of-pocket costs on households' healthcare utilization and risky behaviors, particularly diet choices. I focus primarily on risky behaviors as they can provide evidence of ex-ante moral hazard in health insurance. In the health economics literature, ex-ante moral hazard refers to the idea that insurance, by lowering the out-of-pocket price of care, may reduce incentives to maintain a healthy lifestyle (Ehrlich and Becker, 1972).¹ Understanding whether ex-ante moral hazard affects patient behavior is crucial, as it might lead to preventable health issues and, consequently, to higher healthcare spending. Causal evidence of the effect on changes in out-of-pocket costs on ex-ante moral hazard is lacking, while the effect on healthcare use has been extensively studied (Einav and Finkelstein, 2018).

To provide causal evidence, I exploit a natural experiment provided by the staggered implementation of U.S. state-level policies. Between 2020 and 2022, 23 states reduced the out-of-pocket costs for insulin for insured individuals. The policies set a monthly cap on out-of-pocket costs, ranging from \$25 to \$100, with any excess covered by insurance. In a staggered difference-in-differences approach à la Callaway and Sant'Anna (2021), I compare the grocery purchasing behavior of households with diabetes and private health insurance in states with the cap to those in states without the cap. I find that lowering insulin costs leads to riskier nutritional behaviors, consistent with ex-ante moral hazard. Households increase their sugar purchases, particularly among those who have more control over their condition through lifestyle. The results also show a sustained increase in the sales of devices essential for daily diabetes management. Taken together, the findings highlight a shift from lifestyle management to treatment.

These results provide new evidence for understanding the role of ex-ante moral hazard in health insurance along three dimensions. First, by analyzing the nutritional content of purchased food—rather than relying on product categories—I reduce reliance on subjective definitions of risky behavior. Nutritional content allows me to precisely quantify changes in the purchase of health-relevant nutrients (e.g., sugar) and avoid product misclassification. Second, I focus on individuals with chronic conditions, such as diabetes, whose higher healthcare requirements may cause them to react differently to financial incentives than people in better health. Third, while the existing literature primarily examines the extensive margin of insurance (i.e., obtaining insurance coverage) in relation to risky behaviors, this paper focuses on the intensive margin.

¹In the literature, ex-ante moral hazard also refers to preventive measures, such as doctor check-ups. In this paper, I focus mainly on lifestyle-related behaviors. Note that ex-post moral hazard refers to the notion that insurance coverage may lead to increased healthcare use (Pauly, 1968).

Empirical evidence at the intensive margin is limited due to the challenges of finding a plausibly exogenous identification strategy and observing risky behaviors, health status, and insurance plan information simultaneously. In this paper, I overcome these empirical challenges by exploiting recent policies that reduce insulin costs and constructing a dataset with the necessary information.

I use a rich dataset constructed by combining several data sources. The primary source is the 2019–2022 NielsenIQ Consumer Panel provided by the Kilts Center. This is a representative panel data on grocery purchases for over 83,000 unique households. I complement this dataset with extensive information on the nutritional content of the products purchased. To identify households potentially affected by the policy, I match the Consumer Panel with the NielsenIQ Annual Ailments, Health, and Wellness Survey, which provides health-related information, including diabetes diagnoses, diabetes types, and insurance coverage.

In line with the scope of the policy, I focus on individuals with diagnosed diabetes. This group provides a relevant setting for answering my research questions for three main reasons. First, diabetes is diagnosed in 11.3% of the U.S. population and is expected to rise due to demographic trends (Ong et al., 2023). In 2021, it was the eighth leading cause of death, mainly due to its association with multiple comorbidities (ADA, 2018). Second, diabetes management depends heavily on both prescription drugs and diet. Tracking carbohydrate intake, especially sugar, is essential for regulating blood glucose and determining insulin dosage, which increases with higher carbohydrate consumption (CDC, 2023). Third, the distinction between diabetes types provides important insight into ex-ante moral hazard. While type I diabetes has a strong genetic component and requires insulin use regardless of an individual’s lifestyle, type II diabetes—the most prevalent form that affects 95% of individuals with diabetes—can often be managed through changes in diet and exercise. Consequently, individuals with type II diabetes have greater scope for behavioral responses to financial incentives, making them more susceptible to ex-ante moral hazard.

I find that the reduction in insulin out-of-pocket costs leads to a 4.8% increase in carbohydrate purchases in the first quarter following the policy implementation. While one possible alternative explanation is that households are correcting previously suboptimal consumption constrained by limited insulin access, the composition of carbohydrate purchases suggests otherwise. Specifically, I document a sustained increase in sugar purchases (9.3%), a carbohydrate subtype that can increase the long-term risk of comorbidities, such as cardiovascular and kidney disease (Holman et al., 2008). These patterns are consistent with ex-ante moral hazard, whereby reduced short-term financial costs weaken incentives for preventive behavior. Supporting this interpretation, the increase in sugar purchases is primarily driven by households with type II diabetes, a condition often manageable through lifestyle changes. Heterogeneity analysis further supports the ex-ante moral hazard interpretation, as households placing low importance on a low-carbohydrate diet exhibit the most significant increases in sugar purchases.

I examine the extent of ex-ante moral hazard by studying the effect of the policy on other dietary components that could lead to comorbidities. I observe short-term increases in nutrients, such as fat, cholesterol, and calorie purchases. The results further highlight the shift toward less healthy nutritional behavior. However, the results are temporary, unlike sugar, showing that

households return to the pre-policy level in the second quarter. Hence, they suggest that ex-ante moral hazard is primarily focused on behaviors most closely related to the prescription drug affected by the cost change. Finally, I can exclude an alternative explanation for the observed increase in risky behaviors, namely that an income effect is at play rather than ex-ante moral hazard. I find no evidence of an income effect, as households do not increase their grocery expenditures.

Additional analyses show that the policy leads to an increase in medication utilization and sales of testing devices. For these analyses, due to limitations in Consumer Panel data, I utilize diabetes device sales data from over 30,000 stores in the NielsenIQ Retail Scanner Data. The findings are in line with the ex-ante moral hazard interpretation. In particular, they suggest that individuals rely more on medication and monitoring to manage the effects of their dietary choices. While this shift from diet-based to medication-based disease management may be effective in the short term, it raises concerns about long-term health risks, such as the development of diabetic kidney disease (de Oliveira et al., 2024). To assess whether this behavioral shift has adverse consequences, I examine ketone strip sales—a proxy for disease management—and find a decline following the policy implementation. This result suggests that, at least in the short term, sugar levels are well-managed despite changes in diet.

These results suggest that improving access to insulin might lead to behavioral distortions that need to be considered to improve long-term health outcomes. Indeed, reduced costs lead to lower incentives for a healthy lifestyle, as shown by the rise in sugar consumption. To mitigate the unintended consequence of ex-ante moral hazard and to fully achieve the health benefit potentially due to improved insulin access, complementary behavioral interventions may be necessary. For instance, providing nutritional counseling to groups that are identified as being prone to engaging in risky behaviors could be beneficial. These findings might also inform decisions in states planning to introduce the cap. They might also extend to other diseases requiring a combination of diet and medication for effective management, such as high cholesterol, which is typically managed with statins alongside lifestyle adjustments. Moreover, these results might also be relevant for setting the out-of-pocket costs for new drugs, such as semaglutides (e.g., Wegovy and Ozempic).

Related Literature This paper contributes to four strands of literature. First, it contributes to research on government interventions and other factors that shape dietary choices. Much of this literature focuses on sin taxes on sugary drinks, which are designed to reduce sugar consumption, and finds mixed results. While such taxes reduce soda consumption on average (Dubois et al., 2020), individuals may compensate by substituting toward other sugary foods (Lozano-Rojas and Carlin, 2022) or by purchasing taxed products in nearby, untaxed areas (Cawley et al., 2019). Other interventions, such as food labeling, have been found to lower sugar and calorie consumption by improving awareness (Barahona et al., 2023). Vouchers for healthy food also show diet improvements but their effects tend to diminish over time (Griffith et al., 2018; Hinnosaar, 2023). Prior work also highlights the persistence of dietary habits over time (Hut, 2020), the importance of preferences beyond prices (Dubois et al., 2014), and the role of information, which are often not accurate (Belot et al., 2020; Vitt et al., 2021). I contribute

to this literature by showing that government policies can have unintended consequences on unhealthy eating among households with chronic conditions. I also show that out-of-pocket costs for prescription drugs influence dietary choices. This result is relevant for further understanding which factors influence diet, especially for individuals with nutrition-related diseases.

Second, I contribute to the literature linking food purchasing behavior to health outcomes. Prior work shows that lower prices for sugar-rich foods increase the risk of obesity and related conditions (Gračner, 2021). My paper is most closely related to Oster (2018) and Hut and Oster (2022), who find that households adjust their diets following a diabetes diagnosis. However, these changes are short-lived and limited to specific food categories. In this paper, I examine a policy setting that alters financial incentives—rather than health information—and demonstrate how reductions in prescription drug out-of-pocket costs affect dietary behavior. This offers new evidence on how insurance design can shape health-related consumption, particularly for individuals with diet-related chronic diseases.

Third, I relate to papers examining whether insurance coverage increases risky behaviors due to ex-ante moral hazard.² Existing papers investigate the effect of insurance coverage (i.e., the extensive margin) on risky behaviors—such as alcohol and snack food consumption—and find mixed evidence (Dave and Kaestner, 2009; Simon et al., 2017; Cotti et al., 2019; Chen et al., 2023). My contribution to this literature is twofold. First, I study the intensive margin, specifically how individuals who are already insured respond to a reduction in out-of-pocket costs. Empirical evidence at this margin remains limited and Newhouse (1993) is a notable exception. Second, to the best of my knowledge, I am the first to use nutritional content to measure risky behaviors in the moral hazard literature. This allows me to capture specific nutrients, such as sugar. Thus, I can quantify the risky behavior more precisely and can link the behavior to the disease.

Fourth, I relate to the literature investigating the impact of changes in out-of-pocket costs. Following a decrease (increase) in out-of-pocket costs for other diseases, the existing evidence mainly suggests an increase (decrease) in the use of prescription drugs and healthcare (e.g., Chandra et al. (2010); Ziebarth (2010); Chandra et al. (2014); Shigeoka (2014)). However, some scholars find no effect on prescription drug and healthcare consumption (e.g., (Puig-Junoy et al., 2016)). Recent medical studies examine the effects of insulin out-of-pocket caps, finding consistent reductions in patient costs but mixed evidence on usage behavior (e.g., Anderson et al. (2024); Garabedian et al. (2024); Giannouchos et al. (2024)). Research from middle-income countries indicates that expanding coverage or subsidizing diabetes care can improve health outcomes (Americo and Rocha, 2020; McNamara and Serna, 2022; Barkley, 2023). Contributing to this literature, I provide new evidence on how reduced out-of-pocket costs affect not only medication use but also non-prescription, disease-related purchases, and dietary behavior, a key component of chronic disease management.

This paper is organized as follows. Section 2 reports background information on diabetes and the insulin out-of-pocket cost policy. Section 3 describes the data. Section 4 illustrates the empirical strategy, and Section 5 presents and discusses the results. Section 6 concludes.

²This literature also studies preventive care, e.g., Barros et al. (2008) and Spenkuch (2012).

2 Background

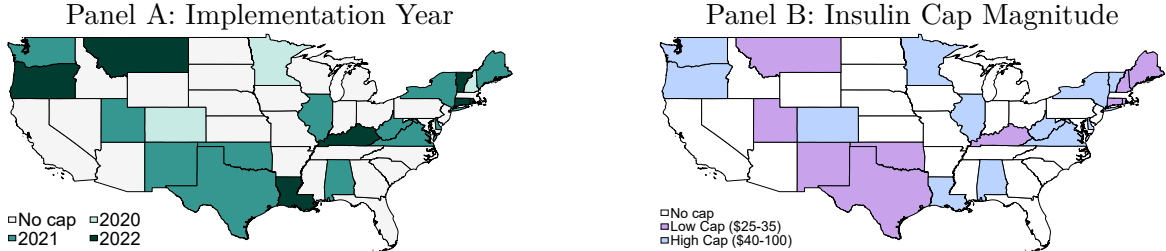
In this section, I discuss the policy exploited for identification, as well as diabetes, and its management. I conclude by discussing the potential effects of the policy.

2.1 The Cap on Insulin Out-of-Pocket Costs

Starting in 2020, several states in the U.S. passed laws to reduce insulin out-of-pocket costs for individuals with private insurance, who made up the majority (68%) of the population in 2019 (U.S. Census Bureau, 2020). Colorado was the first U.S. state to pass such legislation, which became effective in January 2020 and imposed a \$100 cap. Between September 2020 and January 2022, 22 other states adopted similar legislation, establishing caps ranging from \$25 to \$100.³ The policy caps out-of-pocket spending for privately insured individuals at \$25 to \$100 per 30-day insulin supply, depending on the state, with insurers covering any costs above the cap. On average, the policy led to a \$20–27 decrease in out-of-pocket costs per month (Giannouchos et al., 2024).

In 2019, before the policy was implemented, the average monthly out-of-pocket costs for insulin was \$82, according to a report by the Health Care Cost Institute (2024), which analyzed prescription drug claims data from privately insured patients. The report also estimates that 70% of individuals would benefit from a \$35/month cap, while 25% would benefit from a \$100 cap. These high costs led to decreased insulin use, as well as poor blood sugar control.

Figure 1: Insulin Out-of-Pocket Cost Cap Policy



Notes: Own data collection from state-level bills. Panel A reports the year of policy implementation. Panel B indicates whether the cap set by each state falls below (low) or above (high) the median cap level.

Figure 1 illustrates the timing of the policy implementation (Panel A) and the size of the cap (Panel B).⁴ In Panel A, the white-colored states did not implement an insulin cap policy or did so after 2022. The shades of green indicate when the policy was implemented, ranging from light green for states that implemented the policy in 2020 to darker green for those that implemented it in 2022. In Panel B, purple-colored states are those with a “low cap” (i.e., a below-median cap, ranging from \$25 to 35) and blue-colored states are those with “high cap” (i.e., an above-median cap, ranging from \$40 to 100). The maps highlight geographical variation in policy implementation, both in terms of timing and cap magnitude.

³To date, 26 states have passed legislation that caps insulin out-of-pocket costs. However, I consider only 23 states as I exclude some of the states, as I discuss in Section 4. Medicare also introduced a \$35 cap on out-of-pocket costs starting from January 2023. As the data extends until December 2022, I am not able to analyze the effect of this policy.

⁴Table A1 reports, for each state, the cap amount, the date the bill was signed and became effective.

2.2 Diabetes

Diabetes is a chronic disease where blood glucose levels are too high. There are two main types of diabetes: type I and type II.⁵ Type I affects only 5% of individuals with diabetes and is due largely to genetic factors. Type I diabetes cannot be cured, and all individuals with type I diabetes must rely on insulin for disease management. Instead, type II diabetes, the most common form, is strongly predicted to age and obesity. Nearly 80% of individuals with type II are overweight or obese (Iglay et al., 2016). Differently from type I diabetes, type II can be improved or even reversed through sustained lifestyle changes. Moreover, it may require insulin if lifestyle changes and oral medications are insufficient.⁶

These difference between type I and type II diabetes have important implications for studying ex-ante moral hazard. Type I patients face less scope for engaging in risky behaviors. In contrast, individuals with type II diabetes—whose condition can be managed or improved through diet and exercise—may have incentives to adjust their behavior when financial constraints on insulin use are relaxed.

For managing type II diabetes diet is crucial. In particular, patients with diabetes need to count carbohydrates, which affect insulin dosage.⁷ In addition, they should also pay attention to the type of the carbohydrates, which can be either fiber or sugar (Ley et al., 2014). While fiber-rich carbohydrates have a slow impact on glucose levels, foods high in sugars can lead to blood sugar spikes. Frequent blood sugar spikes, in turn, are associated with the development of comorbidities for diabetic patients (Holman et al., 2008). Comorbidities, such as hypertension, hyperlipidemia, and cardiovascular diseases, are common for individuals with diabetes. In the U.S., four out of five individuals with type II diabetes have hypertension (Landsberg and Molitch, 2004). These risks highlight the role of preventive behavior and adherence to dietary recommendations, which may be influenced by the changes in out-of-pocket insulin costs. In Section 5, I examine the effect of the policy on nutrients correlated with comorbidities.

2.3 Potential Policy Effects and Ex-Ante Moral Hazard

Reducing insulin out-of-pocket costs might affect diet, as insulin dosing is closely tied to carbohydrate intake. To illustrate the potential effects of the cap, consider two scenarios in which, holding income, preferences, and insurance constant, the amount of insulin an individual can afford per day varies. In the first scenario, the individual can buy 20 units per day. In this case, the individual may need to limit carbohydrate consumption to avoid risking elevated blood glucose levels. In the second scenario, the same individual can afford 100 units of insulin per day. In this scenario, the higher insulin access could affect nutritional choices because the additional insulin can be used to compensate for higher carbohydrate intake. The two scenarios mimic the pre- and post-period of the policy, which increases insulin affordability through the insulin cap. While the cap directly benefits those who previously paid above the threshold, it may also

⁵Other, less common forms of diabetes are excluded in the analyses because they are not observed in the data. Nevertheless, they account for a small share of cases.

⁶Overall, 23% of patients with diabetes use insulin to control blood sugar levels (ADA, 2022).

⁷The exact insulin dosage depends on the insulin-to-carbohydrate ratio. If an individual consumes 45 grams of carbohydrates and has a 1:15 insulin-to-carb ratio, they would require three units of insulin. In addition, the individual must also consider their current blood glucose level relative to their target.

affect individuals who were already paying at or below it by allowing them to purchase more insulin at no additional cost.

The scenarios are stylized to highlight a potential shift in individual incentives, which is the central question addressed in this paper. The scenarios do not exclude that some individuals may have been consuming suboptimally low levels of insulin prior to the policy, not out of preference but due to affordability constraints. Nevertheless, if individuals respond to lower insulin costs by increasing carbohydrate consumption in ways that raise long-term health risks, such behavior would be consistent with ex-ante moral hazard.

In this context, ex-ante moral hazard refers to a reduction in preventive effort through dietary choices because the immediate financial consequences of risky behavior are lowered. Although the long-term costs of diabetes-related complications remain unchanged, the policy lowers the short-term financial cost of managing blood glucose. This reduction may alter incentives for nutritional choices and increase the risk of developing comorbidities over time. Besides carbohydrate purchases, the policy may also affect dietary decisions more broadly, including total caloric intake. Among others, increased consumption of calories can lead to weight gain, which may exacerbate diabetes-related symptoms and comorbidities.

The reduction in insulin’s out-of-pocket cost might also lead to an income effect. The magnitude of this effect depends on pre-policy spending and the cap in the state of residence. Households already paying below the cap would observe no savings. However, if a household was spending \$200 per month in a \$35-cap state, it could save approximately \$2,000 annually. These savings correspond to 2.9% of the average gross household income in the dataset (\$67,000). Being sizable, these spending could influence grocery purchases.

3 Data

This section first describes the food purchase data, then presents the health and insurance information, and concludes with details on the retailer-level data on diabetes device sales.

3.1 Household-Level Purchases

Food Purchases I utilize the NielsenIQ Consumer Panel (henceforth NCP) data from 2019 to 2022, which is available through the Kilts Center at the University of Chicago Booth School of Business. The NCP contains data on consumer purchases, as panelists collect information on their purchased items with at-home scanner technology upon returning home from a shopping trip. Shopping trips included are those from supermarkets, drug stores, large and small retailers, and online outlets.

Previous studies support the validity of the NCP data. [Einav et al. \(2010\)](#) found that information on shopping trips, products, and quantities in the NielsenIQ Consumer Panel is generally accurate. For example, they report that approximately 20% of shopping trips are not captured in the data. Most discrepancies are related to prices, which are not central to my analysis, as I primarily rely on data concerning trips, products, and quantities. Moreover, [Oster \(2018\)](#) finds that NCP is representative of the individuals’ calorie consumption. She reports that the calorie levels in the NCP correspond to around 80% of the calories reported

in the National Health and Nutrition Examination Survey, where panelists keep food diaries. Finally, note that NielsenIQ incentivizes survey participation through monthly prize drawings, redeemable gift points, and gift cards.

The data contain individual demographic information, such as household size, structure, income, education level of the household heads, and ages of all household members. The NCP also includes information on the state of residence, which is relevant for the identification strategy. For each purchase, the NCP reports the Universal Product Code (UPC)—a unique 12-digit identifier for each product—along with the quantity purchased, store information, and pricing details. Prices are either recorded by panelists or, when available, retrieved from NielsenIQ’s store-level data. For a subset of products, NielsenIQ provides additional detailed information on nutritional content.⁸ The matched data allow me to construct dietary outcomes, such as total grams of carbohydrates purchased per quarter, normalized by household size.

Health and Insurance Status The Annual Ailments, Health and Wellness Survey (henceforth, the Ailment Survey) is administered to NCP panelists in the first quarter of the corresponding year. Panelists are asked several questions about their ailments, general health, and diet. Instrumental for my analysis, the survey records whether participants have type I or II diabetes or prediabetes.⁹ I include the first two in my analysis and exclude the latter, yielding 9,598 unique households where at least one member has diabetes. The survey also captures information on households’ comorbidities, average exercise frequency, and the importance they give to a low-carbohydrate diet. Starting in 2020, I also observe whether a household member has private health insurance.

While the data offer rich details on household behaviors, a key limitation is the partial coverage of health and insurance information due to voluntary participation in the Ailment Survey. Only a subset of households (64%) ever participates. To address this limitation, I utilize information from participating households to impute the diabetes status of the remaining panelists based on their purchasing behavior. Specifically, I employ variables observed for all NCP households. I implement a rule-based classification approach using (i) the annual amount in USD spent on diabetic products, (ii) the number of trips made to purchase diabetic products each year, and (iii) the number of packages of diabetic products purchased. Conditional on positive values, I compute quartiles for each variable and classify a household as having diabetes if it falls in the top two quartiles on at least one measure.¹⁰ As shown in Table B1 in Appendix B.1, this method correctly identifies 67% of households with diabetes and allows me to recover diabetes status for an additional 1,970 households. Approximately 10% of the households have diabetes, which is similar to the share of diabetes in the U.S. population (11.3%).

Additionally, I impute the insurance status for non-respondents using a logit model with random effects. I use household-level information, such as the head of household age, house-

⁸NielsenIQ started providing this dataset in 2021. Using the UPC, I can assign this information to purchases made in 2019 and 2020. For all years 2019–2022, I have the information for 60% of the transactions, amounting to 42% of the household spending on groceries. Prior work by Felipe Lozano-Rojas informed the construction of nutritional outcomes.

⁹An individual with prediabetes is in a stage where medication is not yet required, but it may become necessary if the condition is not reversed through lifestyle changes, such as improved diet and increased physical activity.

¹⁰I use this method for its strong performance. See Appendix B.1 for alternatives and comparisons.

hold size, household income, education in years, and hours worked per week. The imputation results, reported in Table B2, show that 85% of households with private insurance are correctly identified. In Appendix B.1, I provide a detailed explanation of the two imputations, and I show that respondents and non-respondents are largely comparable. In Appendix B.4, I show that the results are robust to the exclusion of households with imputed status.

Final Sample and Descriptives Alaska and Hawaii are not included in the NielsenIQ dataset, and I exclude two additional states: Louisiana because the law became effective in August 2022, and I only observe a few months in the post-period; Oregon because it passed various insulin-related laws during the period, which were different from the policies implemented in other states. I also exclude 100 households that move to a different state between 2019 and 2022. The final sample comprises a total of 47 states (including Washington DC as a separate state), of 21 are treated and 26 are controls. This corresponds 69,969 observations and 8,651 unique households, all of which have at least a member with diabetes and private insurance. These restrictions are in line with the policy being effective for households with diabetes who hold private insurance.

Table 1 reports households and shopping behavior descriptives.¹¹ I show the descriptives for households in control states (in columns 2 and 3) and in treated states (columns 4 and 5). States that did not implement the policy are classified as the control group, while those that did are classified as the treated group.¹² Panel A summarizes household characteristics from the first year each household appears in the panel. On average, the heads of household are nearly 61 years old, and households consist of 2.5 individuals. The average years of education is 14.5. In terms of income, households in treated states earn an average of \$66,900, while those in control states earn slightly less, at \$66,162. The share of marital status (72% married, 7% single), presence of children under six (1%), and racial composition (78% white) are similar across the treatment and control groups.

Panel B reports purchasing patterns for households with diabetes. Quarterly food expenditures are similar across treatment and control states, averaging around \$1,300 per household, which corresponds to \$200 per person per month. This aligns with the \$154 per-person monthly food spending reported by the [Bureau of Labor Statistics \(2020\)](#). The table also presents nutritional content descriptives, reported as average per-person quarterly quantities. On average, each quarter, households with diabetes buy food containing 8,500 grams of carbohydrates, including 500 grams of fiber and 1,400 grams of sugar. The food purchases contain 3,300 grams of fats and 1,700 grams of proteins. These quantities correspond to roughly 70,000 total calories, which represents about 37% of the recommended daily caloric intake of 2,100 calories for a moderately active 61-year-old, i.e., 1,800 for women and 2,400 for men ([Cleveland Clinic, 2023](#)).

¹¹Table A2 in Appendix A reports similar statistics comparing the whole panel to the selected sample.

¹²This classification helps mitigate concerns related to the imputation of diabetes status and insurance coverage. Defining the control group as households without diabetes would increase the risk of bias, as the imputation model produces a relatively high number of false negatives—households that have diabetes but are not identified as such. As a result, treated households (i.e., with diabetes) could be mistakenly included in the control group (i.e., without diabetes), introducing a downward bias in the estimated treatment effect.

Table 1: Panel and Retailer Data Descriptives

	Control States		Treated States	
	Mean	S.D.	Mean	S.D.
Panel A: Household (HH) Characteristics				
HH Heads Age	60.95	11.28	60.85	11.50
HH Size	2.32	1.12	2.37	1.18
HH Heads Education (years)	14.39	2.19	14.43	2.20
HH Income	66,162	25,872	66,900	26,023
HH Income (per person)	33,414	18,172	33,411	18,345
Married	0.72	.	0.71	.
Single HH	0.18	.	0.18	.
Presence of Children <6 yrs	0.01	.	0.01	.
White	0.78	.	0.79	.
Observations	5,517		3,134	
Panel B: Purchasing Behavior				
<i>General Behavior</i>				
Total Spent \$/Quarter	1,261	781	1,245	770
Total Spent \$/Quarter (per person)	623	425	606	417
<i>Nutrients and Calories (per person)</i>				
Carbohydrate (g)	8,534	6,943	8,472	7,299
Fibers (g)	498	393	499	392
Sugar (g)	1,433	1,917	1,426	1,809
Proteins (g)	1,729	1,219	1,726	1,194
Fats (g)	3,340	2,941	3,306	2,845
Cholesterol (mg)	23	97	20	91
Sodium (mg)	175	333	174	329
Calories	71,115	51,780	70,550	51,996
Observations	44,841		25,128	
Panel C: Diabetes Device Sales				
<i>Insulin Syringes</i>				
Units	215.28	435.67	223.46	447.20
Price	0.62	0.28	0.61	0.29
<i>Glucose Monitors</i>				
Units	8.95	19.09	9.08	19.52
Price	11.52	7.90	11.43	8.03
<i>Glucose Test Strips</i>				
Units	387.31	494.84	388.81	490.51
Price	2.18	2.01	2.24	2.05
<i>Ketone Test Strips</i>				
Units	26.17	54.39	28.61	56.19
Price	0.24	0.04	0.24	0.04
Observations	894,624		528,240	

Notes: Own calculations on 2019–2022 NielsenIQ Panel and Retailer Data. Treated states implemented a cap on out-of-pocket costs for insulin between 2020 and 2022, while control states did not. In Panel A, the observation is at the household level using information from the first year the household enters the panel. In Panel B, the observation is at the household-quarter level, and values are in grams (g) or milligrams (mg). In Panel C, the observation is at the retailer month-level. Prices are unit prices in USD.

3.2 Retailer Sales

I use sales data from the NielsenIQ Retail Scanner Data to examine the impact of the policy on sales of diabetes devices. The data contain the weekly units sold, prices, and discounts (if any) for the products. The data also include a unique store identifier, the type of store (e.g., supermarket or drugstore), and the state in which the store operates. The data includes only devices that are sold without a prescription. Because insulin requires a prescription, it is not observed in this dataset. However, I do observe several non-prescription diabetes devices—such

as glucose meters and testing strips—that are essential for managing the disease.

The sample is restricted to diabetes-related devices sold by drug stores. I construct two primary outcome variables: the average sale price and the number of units sold. To account for differences in package sizes, the unit measure reflects the number of individual product units contained in a package (e.g., whether an insulin syringe package contains 10 or 100 syringes). I construct outcomes for four categories of diabetes devices, identified using NielsenIQ product descriptions.¹³ First, I examine insulin syringes, which are used to inject insulin.¹⁴ Second, I study the purchase behavior around glucose monitors, which are small devices that are used to measure blood glucose levels several times a day. Third, I assess the effect of the policy on glucose testing strips, which are used together with glucose monitors. Together, these two categories provide insight into testing behavior. Fourth, I examine ketone strip sales, which are used to monitor high levels of ketones, typically during illness or prolonged hyperglycemia.

Panel C of Table 1 reports descriptive statistics on diabetes supplies sales from the monthly retailer-level data from over 30,000 drug stores for control states (in columns 2 and 3) and in treated states (columns 4 and 5). The values are similar across treated and control states, especially in terms of prices. In treated states, around 223 syringes, nine glucose monitors, 389 glucose test strips, and 29 ketone test strips are sold per month in each store. Prices per unit range from an average of \$0.24 for ketone test strips to \$11 for glucose monitors.

4 Estimation and Identification

The implementation of the policy is staggered and is rolled out at nine different points in time. To address the “forbidden comparison” problem that arises when using a two-way fixed effect in such a setting, I employ the staggered difference-in-differences approach à la [Callaway and Sant’Anna \(2021\)](#).¹⁵ This approach is useful in my setting since the short-run treatment effects may be stronger than the long-run effects, and there are potentially heterogeneous treatments across cohorts that might vary over time. Given that the policy was implemented over two years, its effects may change over time.

To describe the research design, I employ the potential outcome framework à la [Robins \(1986\)](#) and define D_{it} as an indicator equal to one if household i resides in a state that has implemented the policy cap in quarter t . The outcome of interest Y_{it} can be written as $Y_{it} = Y_{it}(1)D_{it} - Y_{it}(0)(1 - D_{it})$, where $Y_{it}(1)$ and $Y_{it}(0)$ are the potential outcomes with and without the policy, respectively. The typical challenge in this setting is the inability to observe a unit with and without the treatment simultaneously, i.e., it is not possible to observe the counterfactual.

¹³Up to 2020, NielsenIQ reports two broad diabetes-related categories: insulin syringes and testing products. Glucose monitors can be identified using keywords such as “BG K” and “MNSYS.” From 2021 onward, more detailed product classifications allow for the identification of additional products in earlier years.

¹⁴Although insulin pumps and pens are alternatives to syringes, 65% of individuals with diabetes in the U.S. do not use them ([Endocrine News, 2014](#)).

¹⁵This problem was highlighted in the recent staggered difference-in-differences literature. When estimating a two-way fixed effect such $y_{it} = a_i + b_t + \theta(\text{treat}_i \cdot \text{post}_t) + e_{it}$, the main coefficient of interest is θ , being the effect for treated individuals in the post period (i.e., after the policy implementation). However, θ is estimated by comparing the same unit across time and comparing different units with and without the treatment at the same time t . This second difference is problematic because one of the comparisons involves later-treated units versus earlier-treated units. This difference is a “forbidden” comparison, especially if the treatment effects are expected to be heterogeneous, as the parallel assumption no longer holds.

To overcome this challenge, difference-in-differences approaches rely on comparing individuals in the treated states to those in the control states.

Callaway and Sant’Anna (2021) approach this comparison by estimating group-time average treatment effects. This group-time effect is defined as the average treatment effect (ATT) for group g at time t , and a group is defined by the period in which units are first treated. If an observation is never treated, then $G = \infty$. Formally,

$$ATT(g, t) = \mathbb{E}[Y_t(g) - Y_t(0) | G_g = 1] \quad (1)$$

where G_g is an indicator equal to one if an observation is treated for the first time in period g . For each $g \in \mathcal{G}$ and $t \in \{1 \dots, \mathcal{T}\}$,¹⁶ the regression estimated to obtain $ATT(g, t)$ is equivalent to:

$$Y_{it} = \alpha_1^{g,t} + \alpha_2^{g,t} \cdot G_{g,i} + \alpha_3^{g,t} \cdot I\{T = t\} + \beta^{g,t}(G_g \times I\{T = t\}) + \gamma^{g,t} \cdot X_i + \varepsilon^{g,t} \quad (2)$$

where Y_{it} is an outcome of interest for household i at quarter t . $\alpha_1^{g,t}$ is the intercept, while $\alpha_2^{g,t}$ is the group fixed-effect for group g and $\alpha_3^{g,t}$ the time t fixed effect. $\beta^{g,t}$ is equal to $ATT(g, t)$ and thus is the coefficient of interest, capturing the treatment effect of the policy implementation. X_i controls for the state-level share of Democratic votes in the 2016 presidential election. This control is included to account for the probability that a state implements the insulin cap or other health-related policies that might influence the outcome.¹⁷ $\varepsilon^{g,t}$ is the normally-distributed error term. As several states never implement the policy, I employ the households in never-treated states as the control group, defined as $C = 1$. The $ATT(g, t)$ for each $t > g$ is then given by:¹⁸

$$ATT(g, t) = \mathbb{E} \left[\left(\frac{G_g}{\mathbb{E}[G_g]} - \frac{\frac{p_g(X)C}{1-p_g(X)}}{\mathbb{E} \left[\frac{p_g(X)C}{1-p_g(X)} \right]} \right) \times (Y_t - Y_{g-1} - \mathbb{E}[Y_t - Y_{g-1} | X, C = 1]) \right] \quad (3)$$

Equation (3) provides a doubly robust method to estimate the ATT, building on Sant’Anna and Zhao (2020). The method is doubly robust since the ATT is identified even if either (but not both) the propensity score model or the outcome regression models are misspecified. This doubly robust method has two building blocks, namely the inverse probability weighting and the outcome regression. The first block adjusts the distribution of covariates between the treated and untreated groups. It involves estimating the propensity score $p_g(X) = p_{g,T}(X) = P(G_g = 1 | X, G_g + C = 1)$, which represents the probability of being first treated in period g , conditional on covariates and on either being a member of group g or not participating in the treatment at any time. The propensity score is estimated via logit using the share of Democratic votes. The

¹⁶Note that $\bar{g} = \max_{i=1, \dots, n} G_i$ is the maximum G observed in the data and that $\mathcal{G} = \text{supp}(G) \setminus \{\bar{g}\} \subseteq \{2, 3, \dots, \mathcal{T}\}$ denotes the support of G excluding $\bar{g}$. $\mathcal{T}$ is the maximum number of period observed.

¹⁷Given that the treatment itself might influence the post-treatment covariates, I use the pre-treatment values from the 2016 presidential elections. I obtain the state-level Democratic vote shares from <https://www.fec.gov/resources/cms-content/documents/federaelections2016.pdf>.

¹⁸When the pre-treatment covariates play no role in identification, the ATT can be rewritten as $ATT(g, t) = \mathbb{E}[Y_t - Y_{g-1} | G_g = 1] - \mathbb{E}[Y_t - Y_{g-1} | C = 1]$.

term $\frac{G_g}{\mathbb{E}[G_g]}$ represents the resulting weight for the treated, while $\frac{\frac{p_g(X)C}{1-p_g(X)}}{\mathbb{E}\left[\frac{p_g(X)C}{1-p_g(X)}\right]}$ is the weight for the control group. The second block, instead, represents the difference in outcomes adjusted by the expected outcome difference for the comparison group.¹⁹

Identification Assumptions and Potential Threats For the identification strategy to be valid and for a causal interpretation of the results, three assumptions need to be satisfied.²⁰ First, the approach relies on the *irreversibility of treatment assumption*. Formally, this is defined as follows: $D_1 = 0$ almost surely (a.s.), and for $t = 2, \dots, T$, $D_{t-1} = 1$ implies that $D_t = 1$ a.s. This means that no unit is treated in the first period of the data and that once a unit becomes treated, it stays treated. This assumption is satisfied in my setting, given that states implement the policy and do not remove it.²¹

Second, the method relies on a (conditional) *no-anticipation* assumption. This assumption requires that the household does not actively choose the treatment status.²² Given that the bill was approved a few months before being enacted, there could be potential anticipation effects, which might lead households to modify their behavior in three possible ways.²³ First, households could move to take advantage of the policy. However, in my data, only around 100 households moved during the analysis period. Hence, this seems to be a minor concern and I also exclude those households from the analysis. Second, households may switch insurance status to take advantage of the cap. I do not find any evidence that a significantly larger share of the population takes up insurance after the policy is implemented.²⁴ Moreover, in my analysis, I limit my sample to individuals who already have insurance before the reform, a selection that partially addresses these concerns. Third, households may change their behavior in ways that directly affect the outcomes of interest. For example, households could ration insulin a few months before the policy is implemented to save money, knowing that they will soon be able to buy more insulin. Consequently, they may also reduce their carbohydrate purchases. The event study estimates show no evidence of this.

Third, the method relies on the (conditional) *parallel trends assumption*. This assumption states that households' dietary outcomes in treated states would have evolved parallel to those in control states, had they not been treated. Given that parallel trends in the *post* period cannot be directly tested due to the absence of a counterfactual, a common approach to evaluating the credibility of this assumption is to check whether they hold in the pre-period. I show that they do in Section 5.

Nevertheless, the COVID-19 pandemic could be a threat to this assumption. While some shocks likely affected both treated and control states similarly—and so the time-fixed effects partially account for them—, treated states reacted differently than untreated states. In Ap-

¹⁹I implement the method using the `csdid` command in Stata (Rios-Avila et al., 2023).

²⁰An additional assumption is having panel data, which is the case in my setting. The case with cross-section data is an extension in Callaway and Sant'Anna (2021).

²¹Rollout is described in more details in Appendix B.2 and represented in Figure B1.

²²Under this assumption, it follows that $ATT(g, t) = 0$ for all pre-treatment periods $t < g$.

²³In Colorado, the law was passed in May 2019 and was enacted in January 2020.

²⁴I use yearly data on insured individuals by state to check whether individuals take up more insurance in the first and second years after the policy is implemented. In Figure A1 in Appendix A, I plot the share and confidence intervals of insurance coverage from two years before the policy implementation to two years after.

pendix B.4, I present various robustness checks that address and mitigate such concerns.²⁵

Finally, a potential threat to the identification strategy is the possibility of spillover effects, such as individuals crossing state borders to benefit from the insulin price cap implemented in a neighboring state. However, this is unlikely, as the cap applies only to individuals who are enrolled in insurance plans in the states enacting the law.

5 Results

In this section, I begin by examining the effects of the policy on the purchases of carbohydrates and two key subtypes: fiber and sugar. I then assess the scope of ex-ante moral hazard by analyzing additional dietary outcomes and exploring the heterogeneity of the effects across groups. Finally, I study the impacts on medication use, diabetes management, and short-term disease management.

5.1 Increase in Risky Behaviors

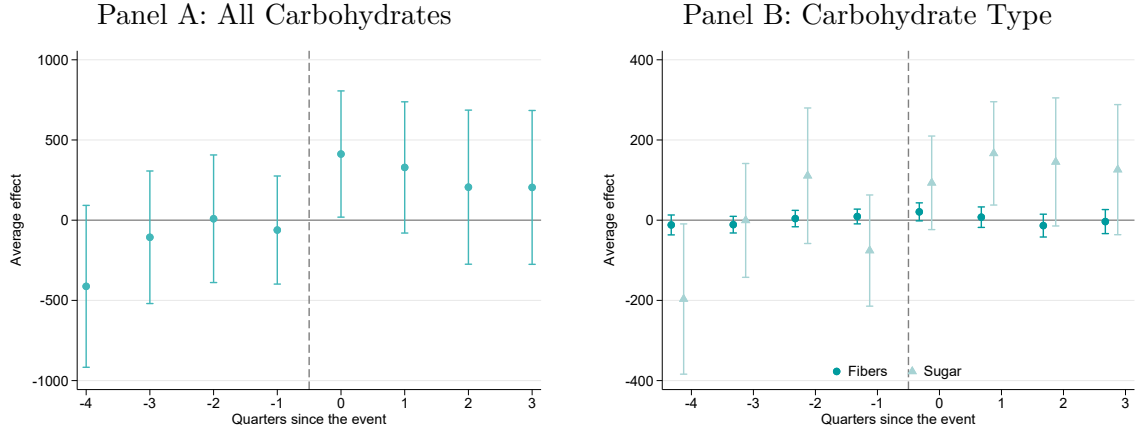
As highlighted in Section 2, carbohydrates and insulin consumption are tightly linked. Panel A of Figure 2 shows the dynamics of the treatment effect for all types of carbohydrates for four quarters before and after the policy implementation. In particular, it plots the event study for grams of carbohydrates purchased per quarter, adjusted by household size. Considering both panels and all three outcomes, all but one of the coefficients of the pre-period are close to zero and are not significant. Table A3 in Appendix A shows that the average coefficient in the pre-treatment periods is not statistically significant. Moreover, I test the parallel trend hypothesis in the pre-period, which cannot be rejected. These results suggest that there are no pre-trends prior to the policy implementation.²⁶

Panel A of Figure 2 reports the effect of the policy on total carbohydrate purchases. In the first quarter following implementation, households increased their carbohydrate purchases by 412.5 grams. This gram amount corresponds to the carbohydrate content of 11 cups of cooked white rice. Given a baseline average of 8,472 grams per quarter in the pre-period, this represents a 4.8% increase. This increase is consistent with the scenarios discussed in Section 2.3. This change might be due to lower incentives in healthy nutritional choices, consistent with an ex-ante moral hazard interpretation. However, an alternative explanation for this effect could be that households have been consuming suboptimal levels of carbohydrates prior to the policy due to limited access to insulin. If so, the observed increase could reflect optimal consumption rather than a behavioral distortion.

²⁵While individuals with diabetes may have been more affected by COVID-19, this concern is mitigated in my context, as the analysis is restricted to diabetic households.

²⁶The interpretation of the coefficients in Figure 2 deviates slightly from that of a standard two-way fixed effects (TWFE) event study. Table A3 in Appendix A reports the results with coefficients normalized at $t - 1$, comparable to the TWFE approach. The key difference lies in the coefficient for period $t - 1$, which is typically omitted in TWFE specifications and serves as the reference period. This difference is due to the identification proposed by Callaway and Sant'Anna (2021). In the post-period, as in the TWFE, the coefficient for each period $t + k$ is interpreted relative to $g - 1$, the last period before treatment for group g . The same logic applies to the control group. This is analogous to the TWFE framework, where $t - 1$ is the baseline period. Thus, the coefficient at $t + 1$ captures the difference between $t + 1$ and $g - 1$. In contrast, pre-treatment coefficients are identified using adjacent-period differences, such as between t and $t - 1$.

Figure 2: Carbohydrates, Purchased Grams



Notes: The figure reports the event study estimates and the 95 percent confidence intervals using the [Callaway and Sant'Anna \(2021\)](#) estimator. I use NielsenIQ Panel Data and the observation is at the household-quarter level.

To understand whether the change in consumption reflects ex-ante moral hazard or improved carbohydrate purchase, I examine whether there are changes in the composition of carbohydrates purchased. An increase in carbohydrates, such as fiber, could suggest that the policy leads to optimal consumption, while a rise in sugar could indicate evidence of ex-ante moral hazard. Fiber is generally considered a beneficial carbohydrate, as it does not cause glycemic spikes and is associated with improved glycemic control, reduced body weight, and decreased risk of comorbidities ([Reynolds et al., 2020](#)). These positive effects are observed for both types of diabetes and across a range of fiber intakes. In contrast to fiber, excessive sugar consumption can lead to blood sugar spikes, which require additional insulin to regulate. In particular, repeated blood sugar spikes can lead to both short-term adverse health effects and long-term development of comorbidities, such as heart disease ([Holman et al., 2008](#)). Therefore, sugar purchases capture a risky behavior that increases the long-run probability of developing comorbidities, consistent with the concept of ex-ante moral hazard.

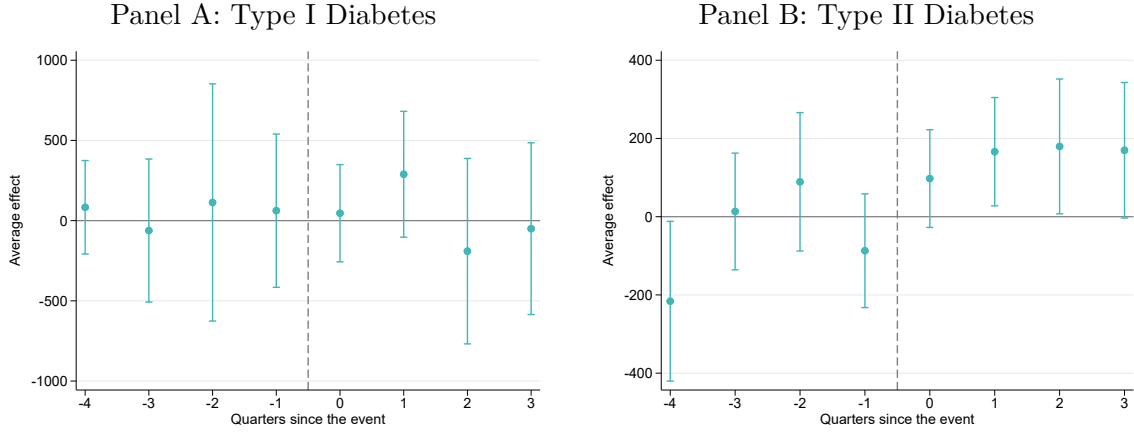
Panel B of Figure 2 plots the estimates for two carbohydrate subtypes: fiber and sugar. The dark-green circle estimates indicate that fiber purchase increases by 20.7 grams per person (4.1%). The light-green triangles of Panel B in Figure 2 show that there is a statistically significant increase in sugar purchases. The average effect in the entire post-period corresponds to an increase of 132.8 grams (9.3%), as shown in Table A3 in Appendix A.

5.2 Is Ex-Ante Moral Hazard the Channel?

Higher Effect with Higher Scope for Moral Hazard To further investigate whether ex-ante moral hazard is the channel driving the observed behavioral changes, I examine whether there are heterogeneous effects by diabetes type. As discussed in Section 2.2, there are strong differences between the two main types of diabetes, where Type I is due to genetic factors while type II can often be managed through diet and exercise. Type II may thus have greater scope for ex-ante moral hazard. If ex-ante moral hazard is driving the behavioral changes observed,

one would expect the response to be driven type II diabetes, especially on sugar.

Figure 3: Sugar, Purchased Grams, by Diabetes Type



Notes: The figure reports the event study estimates and the 95 percent confidence intervals using the [Callaway and Sant'Anna \(2021\)](#) estimator. I use NielsenIQ Panel Data and the observation is at the household-quarter level.

Figure 3 shows the estimated effects on sugar purchases by diabetes type.²⁷ Panel A shows no statistically significant change in sugar purchases among households with Type I diabetes. While these null effects partially reflect the relatively small share of Type I households in the sample (around 10%), they are consistent with the idea that this group has limited scope to adjust their dietary choices in response to changes in insulin out-of-pocket costs. In contrast, Panel B reports the estimates for households with type II diabetes. For this group, considering the baseline 1,426 grams for the control states in the pre-period, sugar purchases increase by 11% (153 grams) in the post-policy period, with the effect both sustained over the four periods and statistically significant.

Figure A2 in Appendix A reports the results for carbohydrates and fiber. Households with type II diabetes increase their overall carbohydrate purchases by 4.3% in the first quarter of the post-period. Instead, households with type I diabetes exhibit a non-statistically significant upward trend in fiber purchases. These results highlight differences in trends between type I and type II diabetes. The different responses in sugar purchases support the interpretation that the policy led to higher engagement in risky behaviors, which is consistent with an ex-ante moral hazard interpretation. Since households with type II diabetes are driving the results, I restrict the analysis to this subgroup in the rest of this section.

Heterogeneity by Demographics and Lifestyle Decisions To further rationalize these findings, I investigate whether there are heterogeneous effects across six households' characteristics.²⁸ For five characteristics, I split the sample based on whether the value falls below

²⁷Note that I do not impute the diabetes type. Therefore, this sample includes only households that responded to both surveys. In Section B.4, I show that results based on this subsample are comparable to the baseline results.

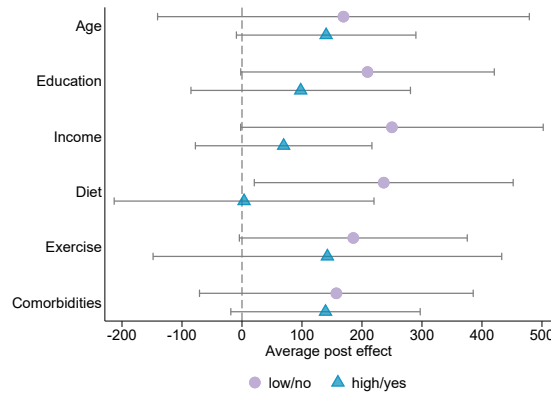
²⁸For treated households, I take the value of the year prior to the policy implementation. For other households, I use the value from 2019, as the first states were treated in 2020.

(“low”) or above (“high”) the median. For one characteristic, i.e., comorbidities, I use a binary indicator that takes the value of one if the household reports any comorbidity.

In Figure 4, I report the average coefficient from the first quarter of the event study for sugar purchases. Additionally, in Figure A4, I report the coefficients for all carbohydrates and fibers. Overall, although not all of the estimated effects are statistically significant, I find that all but one group increases their sugar purchases. Across outcomes, there are no statistically significant differences between groups. However, there is variation when examining specific household characteristics and dietary outcomes.

First, I consider three demographic characteristics, namely household heads’ age, heads’ education, and household income. These are reported in the first three rows of Figures 4. First, I find that households with older heads exhibit a statistically significant coefficient (at the 10% level) for sugar purchases, while those with younger heads do not. Second, similar results are observed for households whose head has lower education level. Third, lower-income households exhibit a significant increase in sugar purchases. Conversely, higher-income households do not significantly change the purchase. These different reactions likely reflect varying food choices driven by differences in income and budget constraints.

Figure 4: Sugar - Heterogeneity



Notes: The figure reports the first-quarter event study estimates and the 95 percent confidence intervals using the [Callaway and Sant’Anna \(2021\)](#) estimator. Source of the data is the NielsenIQ Panel. The sample is restricted to households in which at least one member has type II diabetes.

In addition, I examine heterogeneity along dimensions of health status and health consciousness, using self-reported information from the Ailment Survey. First, I consider the importance of the household following a low-sugar diet. Survey responses are on a Likert scale from one, “Not important at all,” to five, “Very important,” with values above four classified as above the median. Second, I use information on exercise frequency to classify households relative to the median. Households that exercise three times per week or less are categorized as below the median. Physical activity is broadly defined to include yoga, walking, and gardening, among other activities. Panelists report their exercise frequency by selecting from eight response options, ranging from “never” to “every day.” Third, I construct an indicator equal to one if the

household has other comorbidities related to diabetes and diet, such as hypertension, heart disease, and high cholesterol. These heterogeneity dimensions allow further to corroborate the ex-ante moral hazard interpretation of the findings.

An interpretation consistent with ex-ante moral hazard would imply that households placing less importance on a low-sugar diet to respond more strongly to the policy. These households already appear to place lower value on dietary disease management. Conversely, households that place greater importance on sugar reduction should exhibit little or no change. The results support the ex-ante moral hazard interpretation and provide evidence against the alternative explanation of optimal consumption after the policy implementation. I find that households below the median increase their purchases of sugar and carbohydrates more than those above the median. Notably, households that report assigning high importance to a low-sugar diet do not change their purchasing behavior in response to the policy.

A similar hypothesis applies to exercise frequency. If ex-ante moral hazard is the driving mechanism, households with lower exercise frequency, i.e., those less engaged in preventive health behaviors, should respond more strongly to the policy. In line with this hypothesis, I find that households below the median in exercise frequency increase their purchases of sugar.

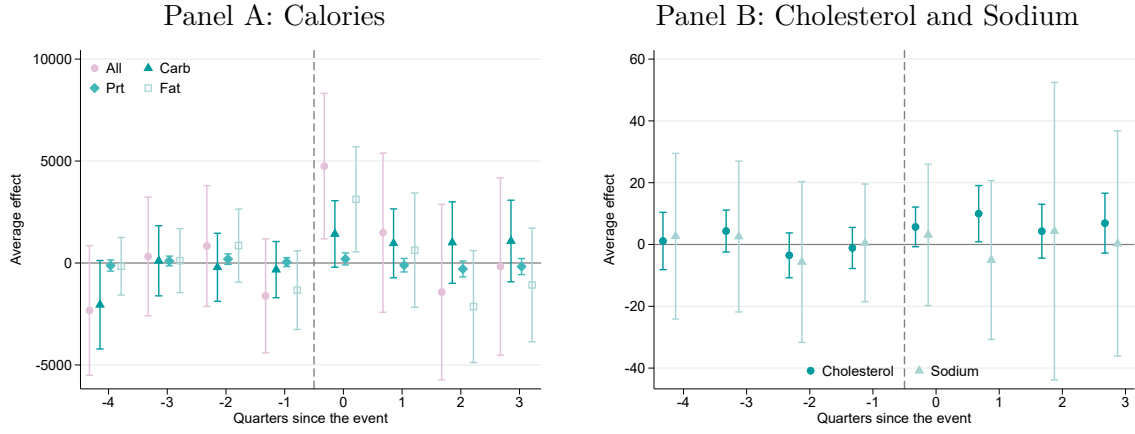
Finally, households affected by comorbidities might be more cautious in their dietary choices. Conversely, households without comorbidities might have a larger scope for ex-ante moral hazard and risky behaviors, as they are healthier. I find almost no difference in sugar purchases across the two groups.

Extent of Ex-Ante Moral Hazard In this subsection, I investigate whether reducing the out-of-pocket expenses of insulin affects dietary habits that extend beyond increased consumption of sugar and carbohydrates. These findings provide more information on the extent of the policy’s impact. In particular, it helps assess whether behavioral responses extend beyond sugar—which is directly linked to insulin use—to dietary choices less closely associated with the prescription drug.

In Panel A of Figure 5, I plot the coefficients both for the overall calories (pink circle) and the calories from the three macronutrients, i.e., carbohydrates, proteins, and fats. Calories serve as a comprehensive measure of dietary intake, as they represent a weighted sum of the three macronutrients: carbohydrates, proteins, and fats. One gram of fat corresponds to nine calories, while one gram of carbohydrates and one gram of protein each correspond to four calories. Therefore, small changes in each of these nutrients could lead to more substantial differences in the overall caloric intake. Importantly, weight loss accompanied by substantial calorie restriction can even reverse the progression of type II diabetes (Wilding, 2014). Calorie purchases increase by 7.1% in the first quarter, exhibiting only a temporary effect. The overall calorie coefficients follow a pattern similar to that of fats, which is consistent with fats exerting a higher impact on calories.

In Panel B of Figure 5, I plot the coefficients for cholesterol (dark green) and sodium (light green). These outcomes allow me to investigate the policy’s effect on essential micronutrients, which can provide additional information on households’ responses to the policy. Cholesterol can contribute to cardiovascular diseases, and excessive sodium intake can lead to hypertension.

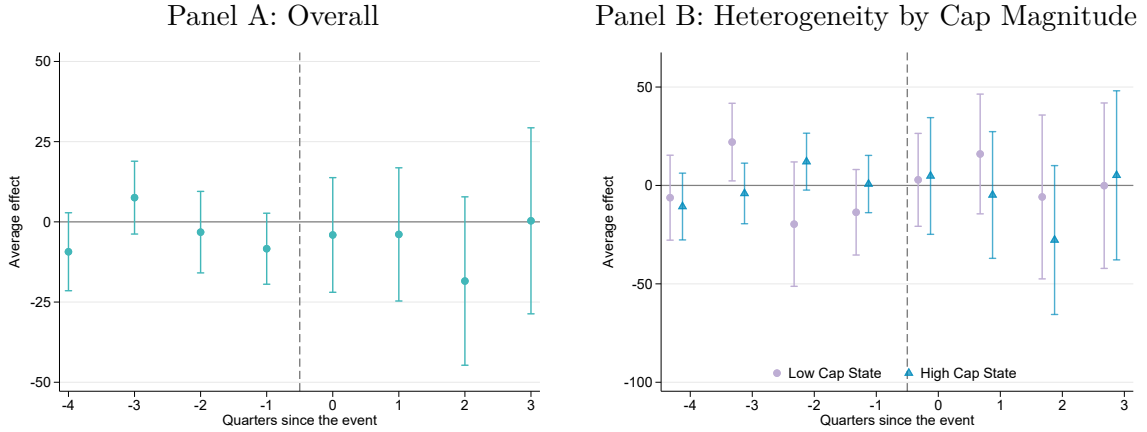
Figure 5: Other Nutritional Outcomes



Notes: The figure reports the event study estimates and the 95 percent confidence intervals using the [Callaway and Sant'Anna \(2021\)](#) estimator. Panel A reports protein and fat measured in grams (g), while the y-axis in Panel C is in milligrams (mg). The source of the data is in the NielsenIQ Panel data. The observation is at the household-quarter level. The sample is restricted to households in which at least one member has type II diabetes.

While I find no significant changes in sodium purchases, I observe a 6.7 mg increase in cholesterol in the post-period, which is significant at the 10% level. This represents a substantial increase of 33% in percentage terms. The effect is temporary and returns to pre-policy levels in subsequent periods.

Figure 6: Total Grocery Spending



Notes: The figure reports the event study estimates and the 95 percent confidence intervals using the [Callaway and Sant'Anna \(2021\)](#) estimator. I use NielsenIQ Panel Data and the observation is at the household-quarter level. I only consider households with type II diabetes.

Absence of an Income Effect An alternative explanation for the results presented in the previous subsections is the presence of an income effect. Specifically, the reduction in out-

of-pocket insulin costs may increase households’ disposable income, which could lead to higher overall grocery spending and, consequently, changes in the nutritional content of food purchases. To exclude this alternative explanation, I construct an outcome measuring per capita household grocery expenditures.

As shown in Panel A of Figure 6, I find no statistically significant change in grocery spending following the policy implementation, suggesting that an income effect does not seem to be at play. Given that the size of the income effect may vary with the size of the cap, Panel B reports results separately for states with caps below the median (purple circle) and above the median (blue triangle). The differences between the two groups are minor and do not suggest the presence of income effects that are different across states. Figure A5 in Appendix A shows heterogeneity analyses by household characteristics. Overall, I find no significant differences in grocery spending across groups, including when comparing households with below- and above-median income levels.

5.3 Diabetes Devices for Disease Management

I now turn to the analysis of medication and testing devices sales. These outcomes contribute to a broader assessment of the policy by offering insight into how it influences healthcare utilization and disease management. To do so, I use store-month-level data because diabetes devices are purchased infrequently at the household level, which results in few observations. Nevertheless, the identification strategy discussed in Section 4 is unchanged.

I study the effect of the insulin policy on the sales of diabetes devices. Even though the insulin cap policy does not cover these devices, their usage may still be indirectly affected by the policy, as they are complements to insulin usage.²⁹ The four panels of Figure 7 report the results on the inverse hyperbolic sine transformation of units sold for the considered devices. I report the coefficients for the twelve months preceding and following the policy implementation.

Increased Treatment The first outcome of interest for evaluating medication utilization is insulin consumption, which is not observed in the data. In the absence of these data, purchases of insulin syringes serve as a proxy for medication utilization, as they are necessary for injecting insulin. Thus, an increase in syringe purchases can plausibly reflect higher insulin consumption, as well as increased injection frequency. If anything, using syringe sales as a proxy for medical utilization might underestimate the true effect. This might be the case if individuals increase their insulin dosage without increasing the number of syringes used. For example, individuals might use larger dose per injection.

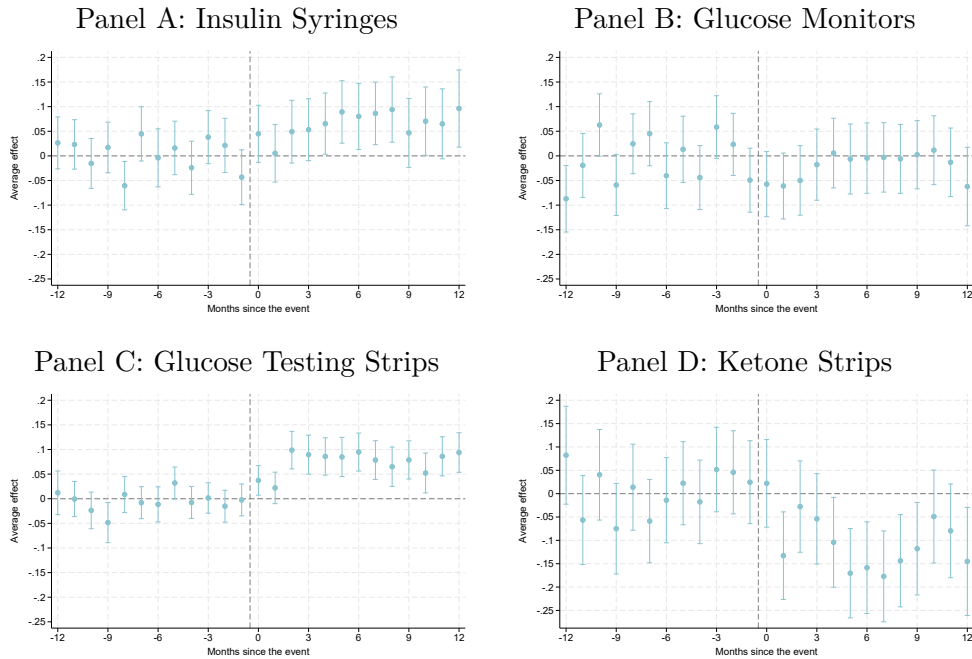
Panel A shows that, in treated states, there is a sustained increase in insulin syringe sales following the implementation of the policy. While the upward trend begins in the first months of the post-policy period, the effects become statistically significant starting in the fifth month. One possible explanation for this delay is a gradual adjustment in insulin consumption. Individuals may initially make only marginal increases in usage, which may not immediately translate into detectable increases in syringe sales.

²⁹In Connecticut, some devices are covered. Results remain robust when excluding the state (see Section B.4).

Additionally, I investigate the policy’s effect on diabetes monitoring behavior by analyzing purchases of glucose monitors and glucose testing strips. The latter are inserted into a glucose monitor with a drop of blood. Glucose monitors display blood glucose levels.³⁰ Panel B shows no change in glucose monitor sales, while Panel C shows an increase in testing strips already in the first month following implementation. Because glucose monitors are durable devices that can last up to five years, individuals are unlikely to purchase them frequently, which may explain the absence of a detectable policy effect on this outcome. In contrast, glucose testing strips are disposable and intended for single use. The observed increase in purchases of testing strips, therefore, suggests that individuals are testing their glucose levels more frequently.

Taken together, the results show an increase in insulin syringe sales—suggesting either an increase in consumption or frequency of injections, or both—, as well as an increase in testing device sales. These findings are consistent with the evidence of ex-ante moral hazard presented in the previous section: lower out-of-pocket insulin costs appear to incentivize greater sugar consumption, with patients managing the short-term consequences by increasing their use of diabetes-related devices.

Figure 7: Units Sold of Diabetes Devices



Notes: The figure reports the event study estimates and the 95 percent confidence intervals using the [Callaway and Sant’Anna \(2021\)](#) estimator. The outcome variables are the inverse hyperbolic sine transformation of monthly units sold of insulin syringes (Panel A), glucose monitors (Panel B), glucose testing strips (Panel C), and ketone testing strips (Panel D). The observation is at the store-month level. Source of the data in the NielsenIQ retail-level scanner data.

³⁰The NielsenIQ datasets do not include purchases of continuous glucose monitors (CGMs), as these devices require a prescription. However, their adoption remains limited, as recent estimates suggest that only 13% of individuals with type II diabetes use them ([Mayberry et al., 2023](#)). CGMs are monitors that allow patients to continuously track glucose levels by inserting a needle with a sensor in the body, usually in the upper arm. This sensor is connected to an external device or an app on the phone, which can send a notification in case of abnormal blood glucose values.

Decreased Extreme Health Effects One might argue that, although individuals appear to be shifting from diet-based to medication-based disease management, they might be able to manage well the diseases, through increased injections and blood glucose testing. Although the data do not allow me to rule out this concern fully, I investigate the effect of the policy on a proxy for disease management.

I study whether there is a decrease in the frequency of spikes in glucose levels for individuals with diabetes. I proxy for this by examining ketone strip sales, which are used to monitor the level of ketones in the blood or urine. High ketone levels can occur when the body burns fat instead of carbohydrates, often due to insufficient insulin. For individuals with diabetes, elevated ketones can signal an increased risk of diabetic ketoacidosis, a potentially life-threatening complication (Misra and Oliver, 2015). Medical guidelines recommend ketone testing when blood glucose is high and accompanied by symptoms such as nausea, vomiting, or fever (Albanese-O'Neill et al., 2017).

In Panel D of Figure 7, I plot the sales of ketone strips. During the post-period, I observe a decreasing trend, which becomes significant four months after the end of the period. The effect is stable and quite persistent over time. The decrease in sales suggests that individuals are testing their ketone levels less frequently. The delayed effect may stem from a period of adjustment as individuals adjust to their changes in behaviors. These results suggest that, at least in the short-run, the disease is well managed.

A limitation is that ketone strips sales capture only very extreme events. Therefore, other outcomes should be considered for a more precise measure of diseases management. In particular, there may be long-term health consequences of repeated blood sugar spikes, which can result from high sugar intake. For instance, they could lead to an increased risk of diabetic kidney disease and other complications (de Oliveira et al., 2024).

Heterogeneity and Prices Given that the magnitude of the cap varies across states, the responses may differ depending on the size of the cap. To examine the heterogeneous effects of the insulin out-of-pocket cap, I split the sample into two groups: states with above-median caps and those with below-median caps. For these two groups, Figure A6 in Appendix A reports the average treatment effect in the post-period for all four devices considered in Figure 7. Overall, I find that the policy's effects are stronger in low-cap states, where insulin cost reductions are greater and more individuals are affected. These states exhibit larger increases in insulin syringe, glucose testing strip, and ketone strip purchases. As in the main analysis, glucose monitor sales remain unchanged.

Finally, one potential concern is that, all the analyses use data on overall sales, which include all consumers. However, the products considered—such as insulin syringes, glucose monitors, and testing strips—are specific to diabetes management and are not typically used for other conditions, which mitigates this concern. A related limitation is that I cannot distinguish privately insured individuals (who are directly affected by the policy) from those with other forms of insurance. This limitation raises concerns that changes in diabetes device sales may reflect unrelated price changes rather than being driven by the policy.

To address this, Figure A7 in Appendix A reports the results on prices. The results show

no statistically significant post-policy price changes, except for glucose testing strips and ketone strips, where prices begin to decline in the fifth month after the policy implementation. However, the increase in testing strip sales is evident from the first month after implementation and grows further by the third month, suggesting that price changes are unlikely to be the primary driver. If anything, if the price were driving the results of ketone strips, a lower price should increase the demand for strips. Therefore, it seems unlikely that a change in price drives the sales effect.

6 Conclusion

This paper studies the role of ex-ante moral hazard in health insurance at the intensive margin, i.e., when the out-of-pocket cost changes. In a staggered difference-in-differences approach, I leverage the exogenous variation in out-of-pocket costs provided by the staggered implementation of policies by 23 U.S. states. These policies reduced the out-of-pocket costs for insulin for insured individuals with diabetes, the focus of this study. I use grocery purchase data from 2019 and 2022, combined with the nutritional content and households' health status information.

My findings address important gaps in the economics literature on health insurance and risky behaviors in three ways. First, I can evaluate changes in health-related nutrient purchases to assess the degree of risky behaviors. Second, I focus my analysis on individuals who have chronic conditions, in particular diabetes. Given the central role that both lifestyle and medication play in disease management, this group is particularly relevant for studying moral hazard. Moreover, the distinction between type I and type II diabetes further allows for heterogeneity analysis, which shows that those with more behavioral discretion (type II) are more responsive to cost changes. Third, I contribute to the limited literature on ex-ante moral hazard at the intensive margin, moving beyond the commonly studied extensive margin of insurance coverage. Together, these contributions offer a more nuanced and comprehensive understanding of how cost-sharing reforms affect risky behaviors.

I find that lowering out-of-pocket costs can lead to an increase in carbohydrate (4.8%) and sugar (9.3%) purchases. This result is primarily driven by households with type II diabetes, who have greater discretion over their lifestyle choices and disease management. I also find evidence that certain groups—such as those giving lower importance to low-carbohydrate diets and those with lower incomes—are also more responsive to the policy. These results are consistent with ex-ante moral hazard. Particularly, when financial exposure to medical costs declines, individuals may reduce their effort in maintaining a healthy diet. The increase in sugar consumption and greater reliance on medical supplies for diabetes management indicate a behavioral shift from lifestyle management toward treatment. Importantly, I rule out alternative explanations, such as income effects, which support the interpretation of a moral hazard response.

In recent years, policies in the U.S. have been implemented to reduce out-of-pocket costs for patients, such as the U.S. Inflation Reduction Act. At a time of high healthcare spending, these policies aim to improve affordability. However, the findings from this work emphasize the importance of understanding and considering behavioral responses to the policy. Indeed, to achieve the full health benefits that may be attained through improved access, it may be necessary to implement complementary policies, such as nutritional education, for those who are

more likely to engage in risky behaviors. These findings are relevant for chronic diseases, such as diabetes or high cholesterol. For these diseases, a combination of medication and lifestyle changes is necessary for effectively managing the disease. Therefore, understanding how price changes impact incentives is crucial in order to guarantee both access and the long-term financial sustainability of healthcare systems as new drugs continue to emerge on the market.

Finally, because of the nature of the available data, this paper has two limitations. First, I cannot directly link individual food purchases to diabetes device usage, as the latter data are frequent only at the retailer level. This data limitation restricts my ability to establish a clear connection between dietary behavior and disease management practices. Second, I am unable to assess the long-term health outcomes of the policy, which prevents a comprehensive welfare analysis from interpreting the adverse effects of ex-ante moral hazard. Nevertheless, this work provides an initial step in documenting the role of ex-ante moral hazard through behavioral reactions to lower insulin expenses.

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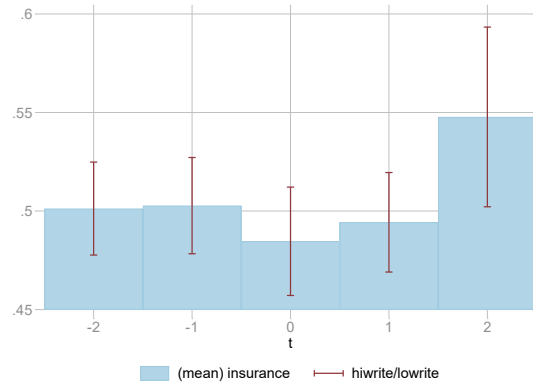
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A Appendix: Additional Figures and Tables

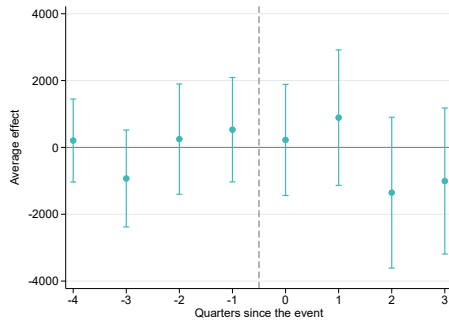
Figure A1: Employer-sponsored Insurance Share over the Years



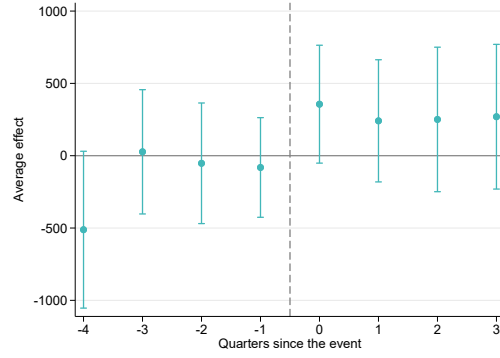
Notes: Own calculation on 2018–2022 KFF insurance count data at the state-year level. This histogram reports the share of individuals having an employer-sponsored insurance (a type of private insurance). The states are stacked around the year zero, i.e., the year in which they implemented the insulin out-of-pocket cost cap. Note that at $t = 2$, only CO, MN and NH.

Figure A2: Carbohydrate (All and Fibers): Heterogeneity by Diabetes Type

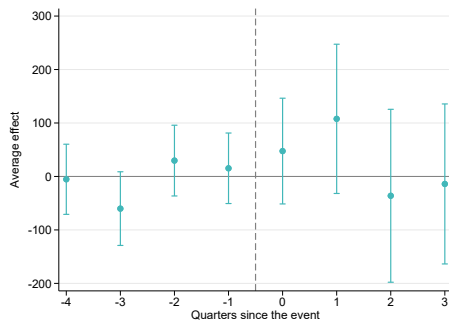
Panel A: Carbohydrates - Type I



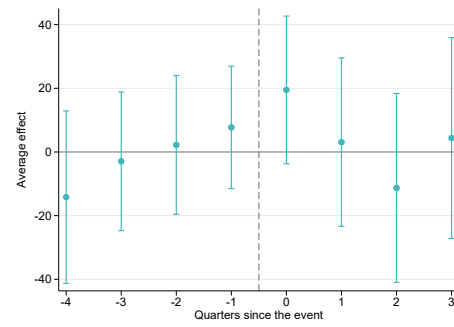
Panel B: Carbohydrates - Type II



Panel C: Fiber - Type I

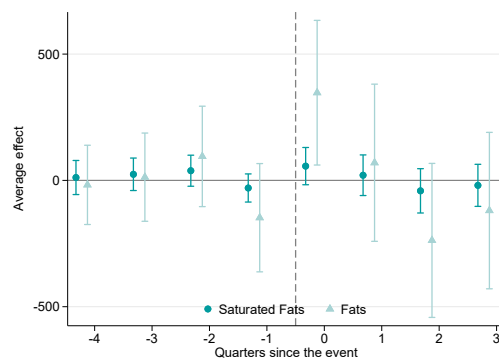


Panel D: Fiber - Type II



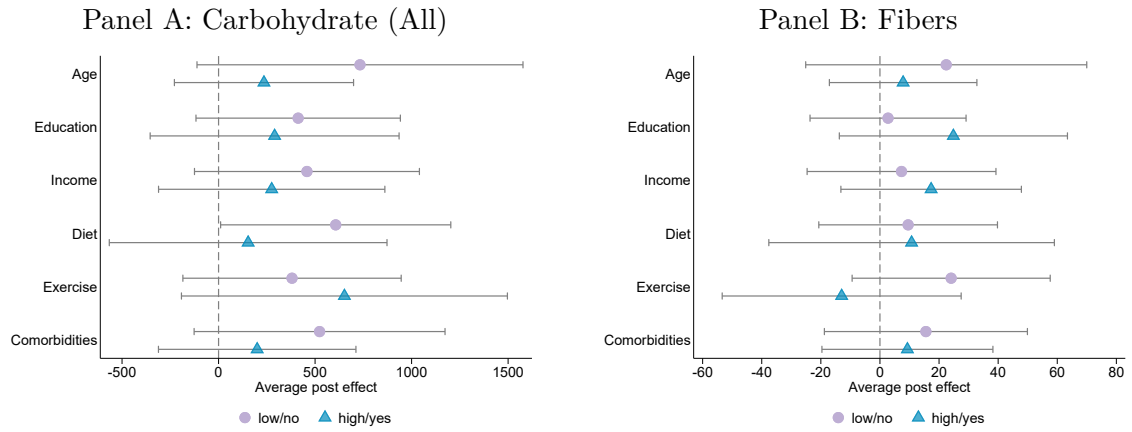
Notes: The figure reports the average post-period event study estimates and the 95 percent confidence intervals using the [Callaway and Sant'Anna \(2021\)](#) estimator. The observation is at the household-quarter level. Source of the data is the NielsenIQ Panel and the Ailment Survey.

Figure A3: Effect by Fat Types, Purchased Grams



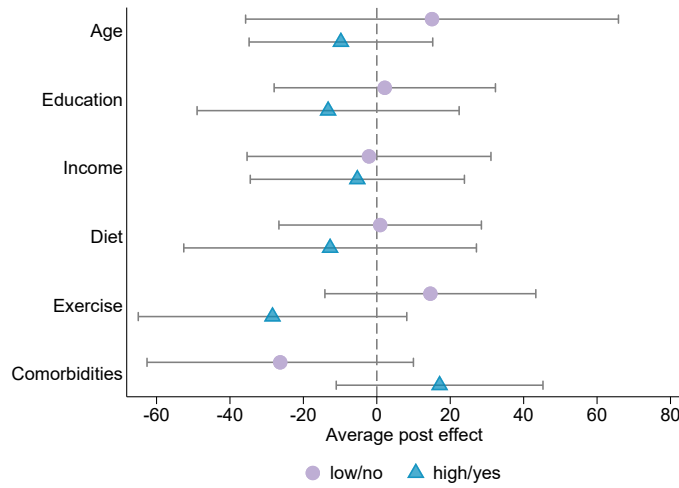
Notes: The figure reports the event study estimates and the 95 percent confidence intervals using the [Callaway and Sant'Anna \(2021\)](#) estimator. Source of the data is the NielsenIQ Panel and the Ailment Survey. The observation is at the household-quarter level. The sample is restricted to households in which at least one member has type II diabetes.

Figure A4: Carbohydrates and Fibers - Heterogeneity



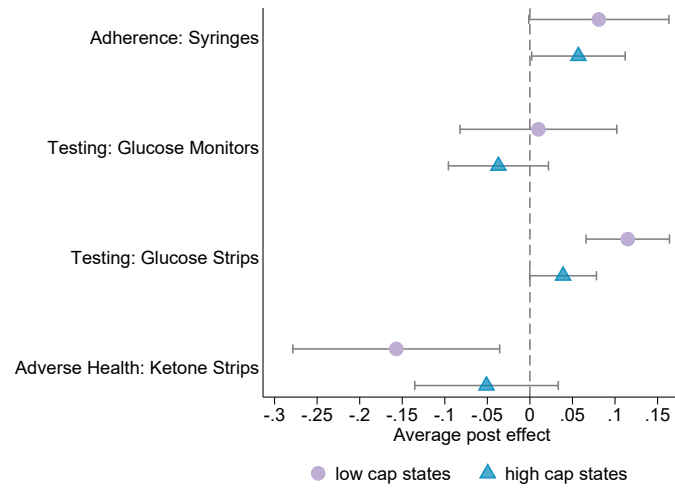
Notes: The figure reports the average post-period event study estimates and the 95 percent confidence intervals using the Callaway and Sant'Anna (2021) estimator. The observation is at the household-quarter level. Source of the data is the NielsenIQ Panel and the Ailment Survey. The sample is restricted to households in which at least one member has type II diabetes.

Figure A5: Total Grocery Spending - Heterogeneity



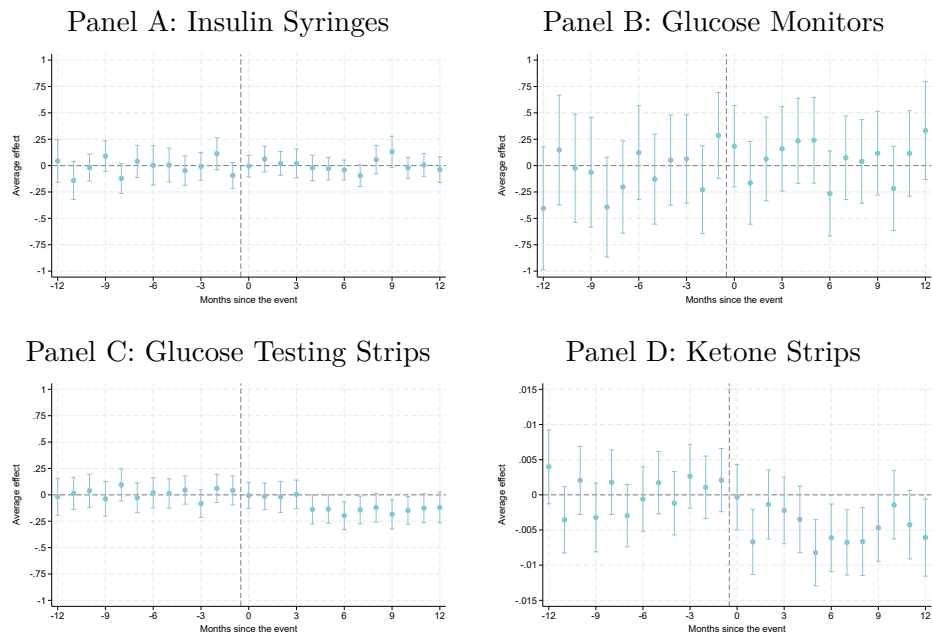
Notes: The figure reports the average post-period event study estimates and the 95 percent confidence intervals using the Callaway and Sant'Anna (2021) estimator. The observation is at the household-quarter level. Source of the data is the NielsenIQ Panel and the Ailment Survey. The sample is restricted to households in which at least one member has type II diabetes.

Figure A6: Heterogeneity by Cap Size



Notes: The figure reports the average post-period event study estimates and the 95 percent confidence intervals using the [Callaway and Sant'Anna \(2021\)](#) estimator. The observation is at the store-month level (drug stores only). Source of the data is the NielsenIQ retail-level scanner data.

Figure A7: Unit Prices of Diabetes Devices



Notes: The figure reports the event study estimates and the 95 percent confidence bands using the [Callaway and Sant'Anna \(2021\)](#) estimator. The outcome variables are unit prices. The observation is at the store-month level (drug stores only). Source of the data in the NielsenIQ retail-level scanner data.

Table A1: State-Level Out-of-pocket Cost Legislation

State	State Code	Diabetes %	Legislation	Date Effective	Date Signed
Alabama	AL	12.9	\$100	October 1, 2021	April 13, 2021
Colorado	CO	7.1	\$100	January 1, 2020	May 22, 2019
Connecticut	CT	8.2	\$25	January 1, 2022	July 1, 2020
Delaware	DE	10.5	\$100	January 1, 2021	July 16, 2020
Delaware	DE	10.5	\$35	January 1, 2023	October 26, 2022
Illinois	IL	9.3	\$100	January 1, 2021	January 24, 2020
Kentucky	KY	11.4	\$30	January 1, 2022	March 22, 2021
Louisiana	LA	12.7	\$75	August 1, 2022	June 18, 2022
Maine	ME	8.3	\$35	January 1, 2021	March 31, 2020
Maryland	MD	9.1	\$30	January 1, 2023	May 16, 2022
Minnesota	MN	7.8	\$50 (90-day supply)	July 1, 2020	April 15, 2020
Montana	MT	7.8	\$35	January 1, 2022	April 29, 2021
Nebraska	NE	8.9	\$35	January 1, 2024	September 1, 2023
New Hampshire	NH	7.5	\$30	September 14, 2020	July 16, 2020
New Jersey	NJ	8.6	\$30	January 1, 2023	June 29, 2022
New Mexico	NM	11.0	\$25	January 1, 2021	March 4, 2020
New York	NY	9.1	\$100	January 1, 2021	April 3, 2020
North Dakota	ND	9.3	\$25	July 1, 2023	April 10, 2022
Oklahoma	OK	11.9	\$30	November 1, 2021	April 20, 2021
Oregon	OR	8.4	\$75	January 1, 2022	March 31, 2021
Rhode Island	RI	9.0	\$40	January 1, 2022	July 7, 2021
Texas	TX	12.1	\$25	September 1, 2021	June 14, 2022
Utah	UT	8.5	\$30	January 1, 2021	March 30, 2020
Vermont	VT	6.7	\$100	January 1, 2022	October 2, 2020
Virginia	VA	9.8	\$50	January 1, 2021	April 8, 2020
Washington	WA	7.9	\$100	January 1, 2021	March 31, 2020
Washington	WA	7.9	\$35	January 1, 2023	March 4, 2022
Washington D.C.	DC	8.3	\$30	January 1, 2021	March 31, 2020
West Virginia	WV	13.1	\$100	July 1, 2021	March 6, 2020

Notes: Column 3 report diabetes state-level prevalence percentages. Source is the KFF and the Behavioral Risk Factor Surveillance System (BRFS). Columns 4-6 report details on the state-level out-of-pocket cost legislation. In particular, column 4 reports the cap on the out-of-pocket cost for insulin, column 5 when such cap became effective and column 6 when the bill was signed. Columns 4-6 are based on own data collection from each state House Bills. Note that Alaska and Hawaii are not included in the NielsenIQ dataset, and I exclude two additional states: Louisiana and Oregon. The working sample includes a total of 47 states (including Washington DC as a separate state), of 21 are treated and 26 are controls.

Table A2: NCP Descriptives

	All HH		HH with Diab. & Ins.	
	Mean	Median	Mean	Median
Panel A: Household (HH) Characteristics				
HH Heads Age Heads	53.97	55.00	60.87	62.00
HH Size	2.47	2.00	2.36	2.00
HH Heads Education (years)	14.50	14.00	14.41	14.00
HH Income	61,467	65,000	66,409	65,000
HH Income (per person)	30,060	25,000	33,064	28,333
Married [N/Y]	0.63	.	0.72	.
Single HH [N/Y]	0.24	.	0.17	.
Presence of Children <6 yrs [N/Y]	0.04	.	0.01	.
White [N/Y]	0.78	.	0.78	.
Diabetes Share [N/Y]	0.10	.	1.00	.
N	83,401		8,651	
Panel B: Purchasing Behavior				
<i>General Behavior</i>				
Total Spent \$/Quarter	1,057	900	1,256	1,089
Total Spent \$/Quarter (per person)	533	441	617	525
<i>Nutrients and Calories</i>				
Carbohydrate	2,972	2,399	3,146	2,612
Fibers	194	141	205	153
Sugar	494	307	510	331
Fats	1,527	1,005	1,657	1,152
Proteins	831	660	899	732
Calories	28,596	22,175	30,668	24,587
N	944,593		69,937	

Notes: Own calculations on NielsenIQ Panel Data. Observation is at the household (HH) level using information from the first year in which the household enters the panel. *HH with Diab. & Ins.* are households with diabetes and private insurance (both declared and predicted).

Table A3: Event Studies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Carbs	Fib	Sgr	Prt	Fat	Sat Fat	Chol	Sod	Cals
<i>Average Estimates</i>									
Pre Average	142.4 (167.6)	-6.867 (9.604)	24.56 (52.03)	-41.74 (28.01)	80.34 (84.40)	-1.850 (24.32)	3.039 (2.573)	3.872 (8.743)	1125.7 (1275.3)
Post Average	287.9 (196.9)	2.829 (11.27)	132.8** (66.32)	-26.88 (36.84)	6.652 (111.1)	3.155 (31.41)	6.080* (3.275)	4.615 (12.95)	1104.0 (1578.5)
<i>Leads and Lags</i>									
$q - 4$	261.7 (243.1)	2.526 (13.71)	8.544 (75.52)	-53.50 (39.62)	53.15 (99.14)	-19.74 (36.19)	1.057 (3.236)	10.67 (13.57)	1310.9 (1698.0)
$q - 3$	104.1 (212.1)	-13.92 (11.85)	-10.60 (65.29)	-50.83 (34.42)	48.28 (104.5)	-3.551 (29.75)	6.836** (3.413)	4.122 (11.44)	647.5 (1590.3)
$q - 2$	61.42 (171.9)	-9.204 (9.383)	75.74 (70.73)	-20.88 (26.96)	139.6 (107.5)	17.74 (27.29)	1.223 (3.164)	-3.176 (9.288)	1418.6 (1392.3)
q	412.5** (200.8)	20.77* (11.46)	93.16 (59.52)	50.14 (35.97)	329.9** (137.0)	52.99 (35.40)	5.697* (3.057)	6.065 (10.87)	4819.1** (1710.9)
$q + 1$	328.9 (208.7)	7.529 (13.04)	166.6** (65.72)	-15.58 (40.43)	67.10 (147.6)	27.32 (38.39)	9.476** (4.289)	-1.294 (12.02)	1857.3 (1878.0)
$q + 2$	205.9 (245.0)	-13.50 (14.52)	145.3* (81.48)	-79.29* (47.15)	-246.7* (144.4)	-46.55 (42.52)	3.391 (4.149)	7.273 (22.28)	-1714.0 (2063.5)
$q + 3$	204.4 (244.7)	-3.487 (15.31)	126.0 (82.77)	-62.77 (47.92)	-123.6 (147.3)	-21.15 (39.89)	5.755 (4.483)	6.416 (17.19)	-546.3 (2089.2)
<i>t statistics in parentheses</i>									
* $p < 0.10$, ** $p < 0.05$									

Notes: N = 55,534. The table reports the event study estimates and the standard errors intervals using the [Callaway and Sant'Anna \(2021\)](#) estimator. All specifications have household and quarter fixed effects. $q - 1$ is the reference period. The outcome variables are in grams columns 1–6, and columns 7–8 in milligrams: (1) Carbohydrates (2) Fibers, (3) Sugar, (4) Protein, (5) Fat, (6) Saturated Fats, (7) Cholesterol, (8) Sodium, and (9) Calories. The observation is at the household-quarter level. Source of the data is the NielsenIQ Panel.

B Appendix: Additional Details and Checks

B.1 Diabetes and Insurance Status Imputation

In this appendix, I provide additional details on the diabetes and insurance status imputation. As a first step, I impute diabetes status by using past survey responses in the survey, as households do not participate every year. Given that diabetes is a chronic disease, this is a reasonable assumption. In a second step, I impute the diabetes diagnosis for the individuals who do not reply to the Ailment Survey using information available for all NCP panelists. I explore several methods for the imputation, i.e., logit, probit, and random forest, accounting for the panel data structure with either random effects or time fixed effects. In addition, I also consider a simple rule based on observed diabetes-related purchases. To verify the accuracy of these imputations, I estimate the model on 80% of the sample and impute the diabetes status for the remaining 20% of the sample. The sample is composed only by observations that appear both in the NCP and in the Ailment Survey. For the imputation, I use demographic information—such as age, education, and race—and diabetes device purchases. The logit, probit, and random forest models perform well in correctly determining true positives and true negatives. However, these methods fail to capture a significant number of true positives and, thus, to significantly increase the sample size. For example, a random-effect logit model would increase the number of unique households with diabetes by 4.4%.³¹

Table B1: Diabetes Status Imputation

		Diabetes		
		No	Yes	Total
Diabetes Imputation	No	110,314	23,041	133,355
	Yes	1,564	3,169	4,733
		111,878	26,210	138,088

Notes: This table reports the confusion matrix for the diabetes status imputation rule. Observation is at the household-year level. The tables has two rows—indicating whether the imputation rule identifies the household as having diabetes—and two columns—indicating whether the household actually has diabetes. The last columns and rows report the total number of observations per group.

Instead, using a simple rule, I identify the status of 1,970 additional observations, corresponding to around 1,261 unique households, leading to a 13.1% increase in unique households observed. This rule considers three aspects of purchasing insulin syringes and testing products. I employ (i) the annual amount in USD spent on diabetic products, (ii) the number of trips made to purchase diabetic products each year, and (iii) the number of packages of diabetic products purchased. I calculate the quartiles for these three variables using only positive values. I do this for insulin and testing products separately, using six different variables in total. From the quartile calculation, values equal to zero are excluded. Hence, if the variable equals zero, I

³¹The main limiting factor is that I have so-called imbalanced classes since I impute the diabetes status, which affects only 12% of the U.S. population.

assign a quartile value of zero. If at least one of these variables is in the third or fourth quartile, I categorize that household as having diabetes. I report the results of the rule imputation in Table B1. I show that 67% of households with diabetes are correctly identified.

Similarly, I impute the insurance status for non-respondents. I employ a logit with random effects using the following information: the head of household age (a categorical variable with four different levels: below 40, between 40 and 60, between 60 and 65, between 65 and 70, and above 70), household size, household income, education in years, hours worked per week, a binary indicator equal to one for married couples, a binary indicator equal to one for being white, the type of residence (e.g., multi-family house or trailer), an indicator if the household is below the poverty line, and an indicator for the presence of children below 18. Before imputing the out-of-sample (i.e., for the non-respondents) of insurance status, I estimate the model on 80% of the Ailment Survey sample—which I randomly select—and impute the insurance status out-of-sample on the remaining 20%. The imputation results, reported in Table B2 show that 85% of households with private insurance are correctly identified.

Table B2: Private Insurance Imputation

		Insurance		
		No	Yes	Total
Insurance Imputation	No	3,848	11,141	14,989
	Yes	1,244	7,294	8,538
		5,092	18,435	23,527

Notes: This table reports the confusion matrix for the insurance status imputation I perform. I estimate a fixed effect logit on 80% of the sample and test the imputation on the remaining 20%, reported here. The tables has main two rows—indicating whether the imputation rule identifies the household as having private insurance—and two main columns—indicating whether the household has private insurance. The last columns and rows report the total number of observations.

Finally, I show that respondents and non-respondents are comparable. By construction, I can verify the validity of the imputation only for the Ailment Survey respondents. However, given that I impute the diabetes and insurance status of non-respondents, one might be concerned that the imputation for non-respondents might yield different results. As I cannot verify whether this is the case, I show that individuals participating in the Ailment Survey are similar to those only in the NCP by looking at the standardized difference between the two groups for the variables used for the imputation.

A standardized difference is defined as $\frac{\mu_a - \mu_b}{\sigma}$, where μ_a is the average for group a , μ_b for group b and σ is the common standard deviation. Standardized differences have the advantage of not being affected by different sample sizes across groups. For interpretation, a standardized difference equal to 0.5 indicates that the mean difference is half the standard deviation. In the context of covariate balance among two groups, while there is no consensus on the threshold, 0.25 is usually considered a small difference (Imbens and Rubin, 2015). I report the standardized differences for the variables used for the diabetes and insurance imputation in Panel A and B of Table B3, respectively. The two groups seem comparable as most standardized differences

are well below 0.25 (in absolute terms). While three differences are above this value, they are only marginally so and they are below 0.5, which is considered a medium difference. To further mitigate any related concerns, in Section B.4, I check that the results are robust to the exclusion of the imputed observations.

Table B3: *NielsenIQ Consumer Panel (NCP) vs. Ailment Survey*

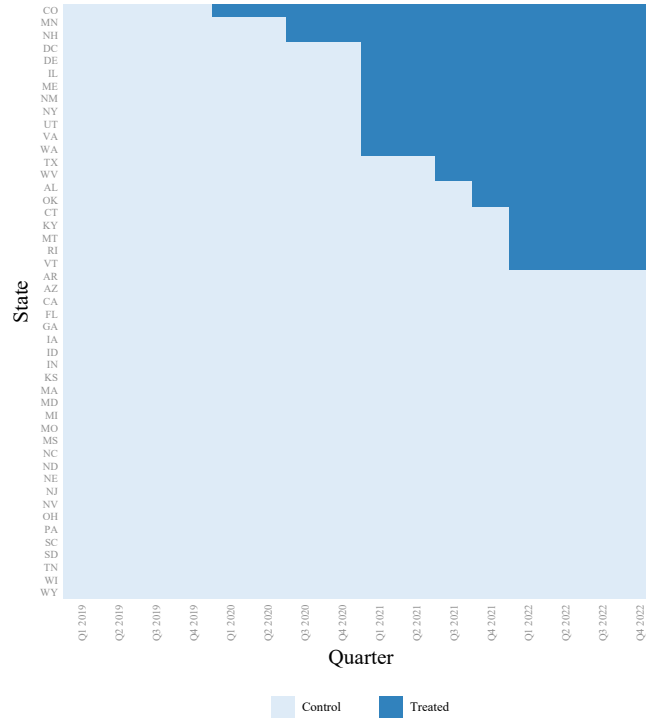
	NCP Only		NCP & Ailment Survey		St. Diff.
	Mean	S.D.	Mean	S.D.	
Panel A: Variables Used for Prediction of Diabetes					
Different products - insulin syringes	0.09	0.79	0.15	0.99	-0.060
Total spent - insulin syringes	3.03	43.61	6.29	62.95	-0.059
Quantity - insulin syringes	0.12	1.16	0.20	1.46	-0.060
Different products - testing	0.02	0.36	0.04	0.62	-0.029
Total spent - testing	0.64	12.61	1.37	27.93	-0.032
Quantity - testing	0.03	0.57	0.05	1.00	-0.026
Panel B: Variables Used for Prediction of Insurance Status					
HH Heads Age	54.87	13.10	58.61	13.31	-0.283
HH Size	2.54	1.35	2.17	1.18	0.290
HH Income	59833.55	28,788.63	55435.14	29,108.44	0.152
HH Heads Education (years)	14.50	2.19	14.56	2.20	-0.029
Employment Head	4.56	2.89	5.36	3.18	-0.264
Married	0.65	.	0.60	.	0.120
White	0.76	.	0.81	.	-0.134
Living in Trailer	0.04	.	0.04	.	-0.031
Below Poverty Line	0.09	.	0.10	.	-0.048
Presence of Children <18 yrs	0.23	.	0.14	.	0.247
Observations	116,118		120,487		
Unique HHs	51,409		53,596		

Notes: Own calculations on NielsenIQ Panel Data. Observation is at the household-year level. “NCP Only” refers to households found only in the NielsenIQ Consumer Panel, while “NCP & Ailment Survey” includes households in both the NCP and the Ailment Survey. For all variables used to impute diabetes (Panel A) and insurance status (Panel B), I report the mean, standard deviation, and standardized difference (Cohen’s d) between the two groups. Differences below 0.25 are considered small (Imbens and Rubin, 2015).

B.2 Policy Rollout

To illustrate key characteristics relevant to the staggered difference-in-differences approach, I present the staggered policy rollout in Figure B1. I report the two-letter abbreviation of the state on the y -axis and the quarter of implementation on the x -axis. Hence, each square represents a state-quarter combination. The light blue indicates no treatment, while the dark blue indicates the treatment. I observe households over 16 quarters, and I consider four quarters before the policy implementation (pre-period) for the first state and four quarters after implementation (post-period) for the last state. The figure shows that some states belong to the same group g as they implement the policy simultaneously, with nine states enacting it in the first quarter of 2021. The figure also highlights that several states never implement the policy within the considered period.

Figure B1: Policy Implementation, Staggered Timing



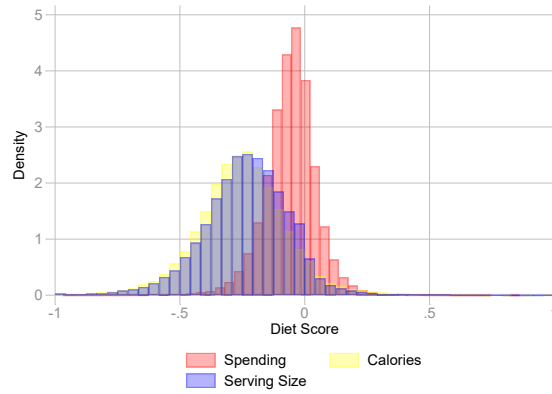
Notes: This figure shows the staggered rollout of the insulin out-of-pocket cost cap by state and quarter.

B.3 Diet Score

To assess the overall healthiness of household diets, I employ the NCP to construct three diet score measures. Following [Oster \(2015\)](#) and [Hut and Oster \(2022\)](#), I use ratings derived from a survey of 17 doctors who evaluated 59 food categories as a “good” (+1), “neutral” (0), or “bad” (-1) source of calories for diabetic patients. Using product-level purchase data, I categorize items based on product descriptions and assign an average rating to each category, informed by the doctors’ assessments. For example, ice cream received a score of -1, juice scored -0.64, eggs scored 0.7. I report full category scores in [Table B4](#). I then compute a household-level diet score by weighting each product’s rating by its share of the household’s total grocery spending.

The spending diet score offers the advantage of complete price coverage, capturing the full composition of the shopping basket. In contrast, nutritional content and serving size data are only available for a subset of purchases. However, the spending diet score approach may be sensitive to price variation, as unhealthy foods may be cheaper than healthier options. To address this concern, I construct two alternative diet scores: one based on the share of calories and another on the share of serving sizes relative to the total basket of groceries.

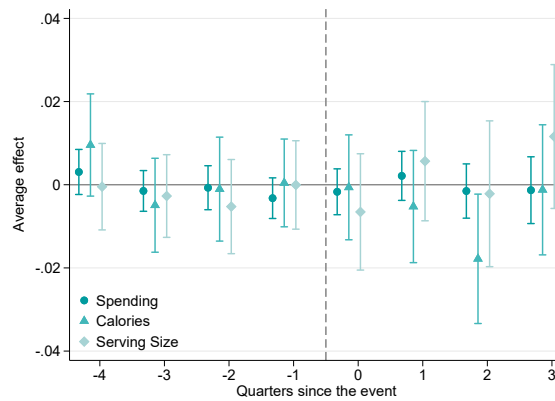
Figure B2: Diet Score Density



Notes: The figure reports the average post-period event study estimates and the 95 percent confidence intervals using the [Callaway and Sant'Anna \(2021\)](#) estimator. The observation is at the household-quarter level. Source of the data is the NielsenIQ Panel, [Oster \(2015\)](#), and [Hut and Oster \(2022\)](#).

The diet scores range from -1 (least healthy) to 1 (most healthy). Households diet score average -0.06 based on spending, -0.25 based on calories, and -0.23 based on serving size. These averages suggest that households tend to eat unhealthily, though not severely so. Notably, the diet score based on spending tends to be higher, reflecting the fact that healthier products often cost more and, therefore, account for a larger share of expenditure. Figure B2 shows the distribution of these scores.

Figure B3: Diet Scores



Notes: The figure reports the event study estimates and the 95 percent confidence intervals using the [Callaway and Sant'Anna \(2021\)](#) estimator. The source of the data is in the NielsenIQ Panel data. The observation is at the household-quarter level. The sample is restricted to households in which at least one member has type II diabetes.

Figure B3 reports the event study coefficients for three different diet scores, from darkest to lightest green, for spending, calories, and serving size, respectively. The three diet scores yield mixed results, with the findings varying depending on the specific definition employed. First, the spending diet score exhibits no effect after the policy implementation. Among the diet

scores, the spending diet score appears to be the most precisely estimated, as its coefficient is close to zero, and standard errors are smaller. Second, there is some evidence of a worsening in the households’ diet using the share of calories diet score, which becomes statistically significant two quarters after the policy implementation. Third, the diet score, calculated using the share of serving size, shows a slight upward trend in $t + 2$ and $t + 3$. Overall, the mixed results suggest that the policy does not lead to substantial changes in overall dietary quality. However, the diet score analysis also appears to mask important behavioral responses, which are shown in the nutrient-level analysis.

Table B4: Food Categories and Their Healthiness

Food Category	“Good”	“Bad”	Food Category	“Good”	“Bad”
Panel A: Unhealthier Food Categories					
Ice Cream	0	17	Potato Chips	1	16
Flavored Syrup	0	17	Jam	1	16
Cake Mix	0	17	Salad Dressing	1	15
Cookies	0	17	Pasta Dinner	1	15
Soda	0	17	Snack Crackers	1	14
Frozen Biscuits	0	17	Margarine	1	12
Frozen Pizza	0	17	Bread	1	12
Cookie mix	0	17	Juice	2	13
Slice-n-Bake Cookies	0	17	Flour	2	8
Chocolate Chips	0	17	Regular Milk	3	11
Sugar	0	17	Potatoes	3	9
Mayonnaise	0	16	Applesauce	3	8
Butter	0	14	Pasta	4	9
Spam	0	14	Rice	4	9
Creamer	0	12	Pretzels	4	9
Panel B: Healthier Food Categories					
Pickles	4	3	Lite Dressing	4	3
Cold Cereal	7	4	Olives	7	4
Canned Vegetables	8	3	Ground Beef	9	6
Canned Beans	9	5	Soup	9	5
Frozen Fruit	9	5	Natural Cheese	10	5
Breakfast Bars	10	4	Salsa	10	3
Olive Oil	11	0	Peanut Butter	12	3
Dried Fruit	12	3	Tuna	12	1
Cottage cheese	13	2	Eggs	13	1
Frozen Vegetables	14	1	Yogurt	14	0
Shrimp	15	1	Hot Cereal	15	0
Fresh Fruit	15	0	Chicken	16	0
Fish	17	0	Nuts	17	0
Low Fat Milk	17	0	Dried Beans	17	0
Vegetables	17	0			

Notes: Source of the data: [Oster \(2015\)](#) and [Hut and Oster \(2022\)](#). For each food category, the number of doctors who rated the product as a good or bad source of calories for diabetic patients is reported. For readability, Panel A reports unhealthier foods, i.e., the majority of doctors defined the food as a bad source of calories. For Panel B, healthier foods are reported. Here, the major of doctor rated the product as a “good source of calories”.

B.4 Robustness Checks

In this subsection, I test and show that my analysis is robust to different specifications. I provide four sets of robustness checks, namely on sample section, confounders from pandemic conditions and policy rollout differences, a placebo test, and alternative methodology.

First of all, I consider different sample selections. I start by addressing a limitation of the

NCP data, namely that food purchases are observed at the household level, not at the individual one. Thus, I may underestimate the effect if only the household member with diabetes changes their behavior. To address this concern, I restrict the analysis to single-person households. As shown in Panel A of Figures B4 and B5, the estimated effects for this sample (dark green circles) are larger than the baseline (gray triangles), suggesting that household-level analysis likely understates the impact. However, this approach reduces the sample size, which leads to wider confidence intervals. Additionally, I show that the results are robust to restricting the sample to households that participate in the Ailment Survey, for whom diabetes and insurance status are reported rather than imputed. Panel B of Figures B4 and B6 shows that there are no differences. The significance and magnitude are almost unchanged, as can be seen by comparing ailment only (dark green circles) to the baseline results (gray triangles).

Second, I address concerns regarding the COVID-19 pandemic and differences in policy rollout. One potential concern is that treated states may differ systematically from control states, particularly in their responses to COVID-19, which could confound the estimated policy effects. I observe that states in the control group were less likely to implement stay-at-home policies. Such policies, in turn, could affect food purchasing behavior. Therefore, as a robustness check, I exclude the states without stay-at-home policies.³² In Panel A of Figure B7 and in Figure B8, I show that the results are robust to this exclusion. In addition, the pandemic caused temporary layoffs and stimulus checks between 2020 and 2021. I report the timing of the shocks in Table B5. To account for these shocks, I exclude the three states—i.e., Colorado, Minnesota, and New Hampshire—that reduced insulin out-of-pocket costs in 2020. I find similar results, which I report in Panel B of Figure B7 and in Figure B9. Finally, I also show that the analysis is robust to the selection of included states. In Panel C of Figure B7, I exclude Connecticut, given that this state’s legislation also includes diabetes devices for the cap calculation. In Panel D, I exclude Maryland, North Dakota, and Nebraska from the analysis, as they implemented the policy in January 2023 and January 2024, respectively. In these robustness checks, the results are virtually unchanged. This is also the case for fibers and sugar, as shown in Figures B10, and B11.

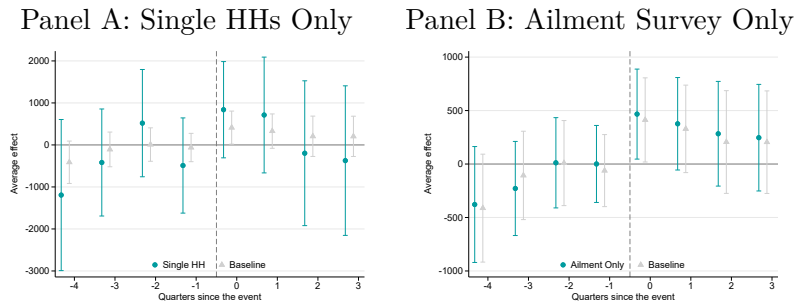
Third, I also run a placebo analysis to show that the results are explicitly driven by the policy and not by other confounding factors, some of which are discussed in the previous paragraph. I exploit the characteristics of the policy to perform the placebo test. I run the analysis on households with diabetes and Medicare. These households serve as an appropriate placebo group, as they were similarly exposed to other potential confounders, such as the COVID-19 pandemic, but were not subject to the insulin cap. I replicate the main analysis on this subgroup and report the results in Figure B12. I find no policy effect among Medicare households, supporting the identification strategy.

Fourth, I show that my results are robust to alternative difference-in-differences specifications. In particular, I employ the standard two-way fixed effects approach and the approach proposed by Sun and Abraham (2021). The latter method and Callaway and Sant’Anna (2021) are similar in accounting for differential timing in treatment. There is one main difference that could be marginally relevant in my context. Callaway and Sant’Anna (2021) can be used if the

³²I exclude Utah, Wyoming, North Dakota, South Dakota, Nebraska, Oklahoma, Arkansas, and Iowa.

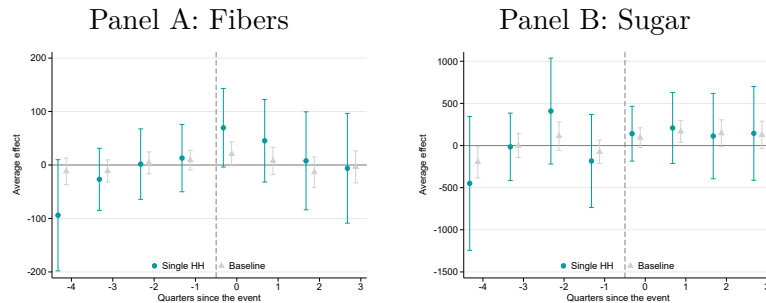
parallel trends assumption only holds after conditioning on observables. Even if, in my main specifications, I do control for the share of Democratic votes at the state level, the parallel trends also hold without the covariates. Moreover, the individual fixed effects capture most of the yearly demographics I observe. Therefore, the two methods should yield similar results. Figure B13 replicates the carbohydrate purchase results and shows similar results for all three methods considered.

Figure B4: Carbohydrate - Household Selection Robustness Checks



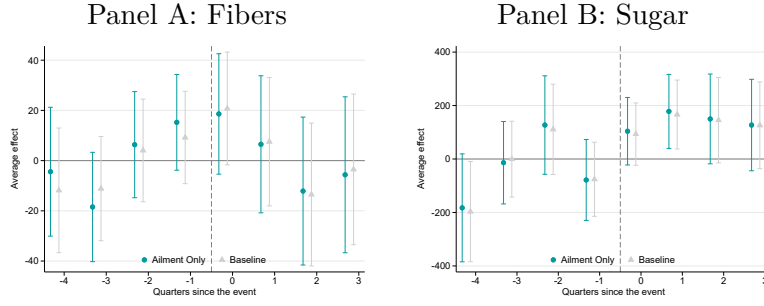
Notes: The figure reports the event study estimates and the 95 percent confidence bands using the [Callaway and Sant'Anna \(2021\)](#) estimator. Source of the data is the NielsenIQ Panel. Panel A restricts the sample to single households only, while Panel B restricts the sample to households both in the NCP and the Ailment Survey.

Figure B5: Fibers and Sugar - Single HHs Only



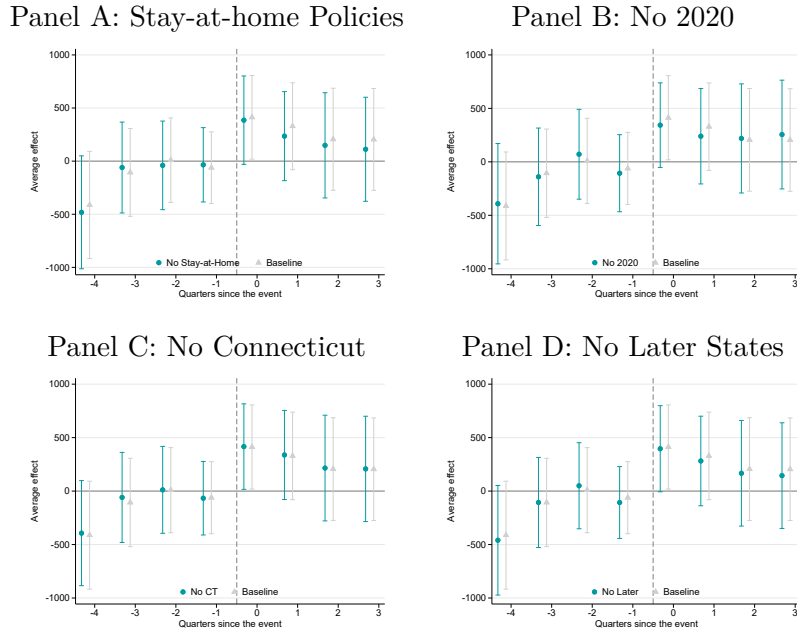
Notes: The figure reports the event study estimates and the 95 percent confidence intervals using the [Callaway and Sant'Anna \(2021\)](#) estimator. The outcome variables are the grams purchased on the different nutrients by the household. The observation is at the household-quarter level. Source of the data is the NielsenIQ Panel. I restrict the sample to single households only.

Figure B6: Fibers and Sugar - Ailment Survey Only



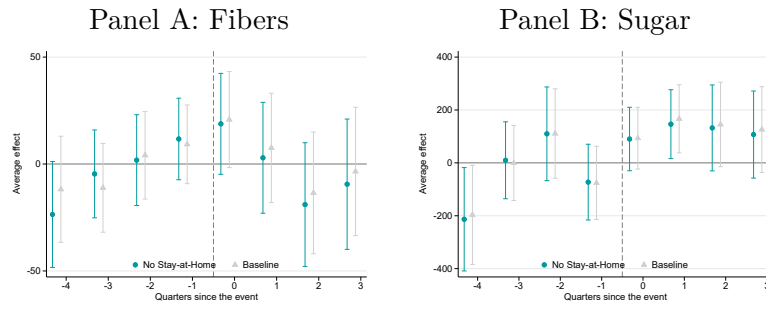
Notes: The figure reports the event study estimates and the 95 percent confidence intervals using the [Callaway and Sant'Anna \(2021\)](#) estimator. The outcome variables are the grams purchased on the different nutrients by the household. The observation is at the household-quarter level. Source of the data is the NielsenIQ Panel. I restrict the sample to households both in the NCP and the Ailment Survey.

Figure B7: Carbohydrate – COVID-19 and Other Confounders Robustness Checks



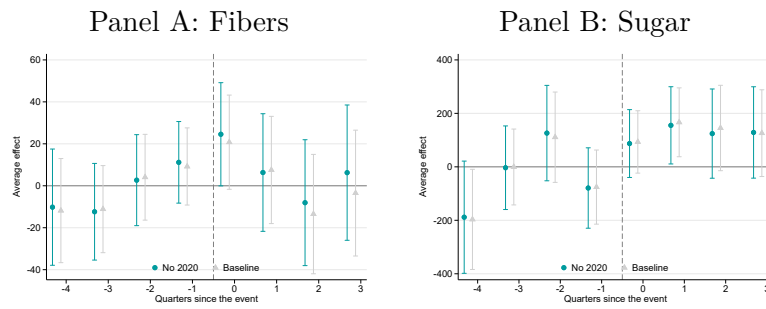
Notes: The figure reports the event study estimates and the 95 percent confidence intervals using the [Callaway and Sant'Anna \(2021\)](#) estimator. The outcome variables are the grams of carbohydrates per person per quarter. The observation is at the household-quarter level. The source of the data is in the NielsenIQ Panel data. In Panel A, I exclude the states without stay-at-home policies. In Panel B, I exclude the states that implemented the policy in 2020 (i.e., Colorado, Minnesota, and New Hampshire). In Panel C, I exclude Connecticut given the different legislation and in Panel D I exclude the states that were later treated, i.e., Maryland, North Dakota and Nebraska.

Figure B8: Fibers and Sugar – Stay-at-home Policies



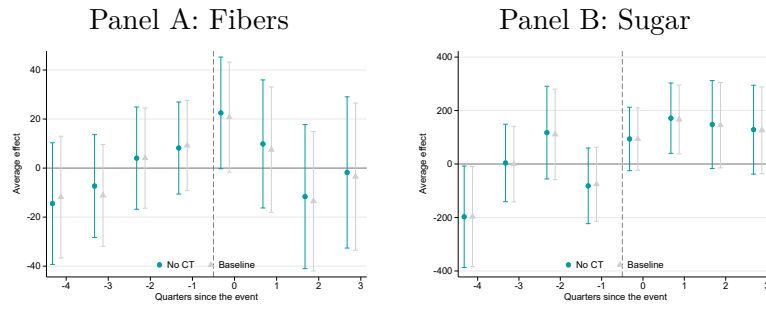
Notes: The figure reports the event study estimates and the 95 percent confidence intervals using the [Callaway and Sant'Anna \(2021\)](#) estimator. The outcome variables are the grams of carbohydrates per person per quarter. The observation is at the household-quarter level. Source of the data is the NielsenIQ Panel. I exclude the states without stay-at-home policies.

Figure B9: Fibers and Sugar - No 2020



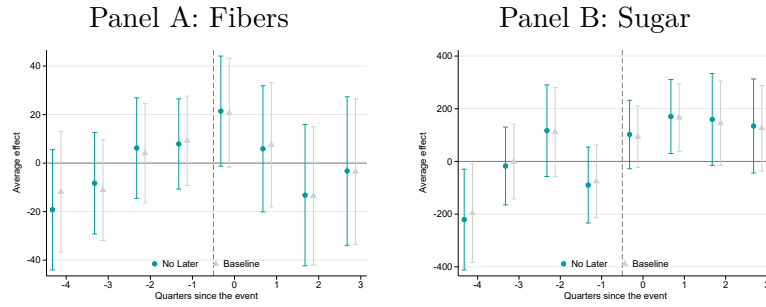
Notes: The figure reports the event study estimates and the 95 percent confidence intervals using the [Callaway and Sant'Anna \(2021\)](#) estimator. The outcome variables are the grams of carbohydrates per person per quarter. The observation is at the household-quarter level. Source of the data is the NielsenIQ Panel. I exclude states that implemented the policy in 2020.

Figure B10: Fibers and Sugar - No Connecticut



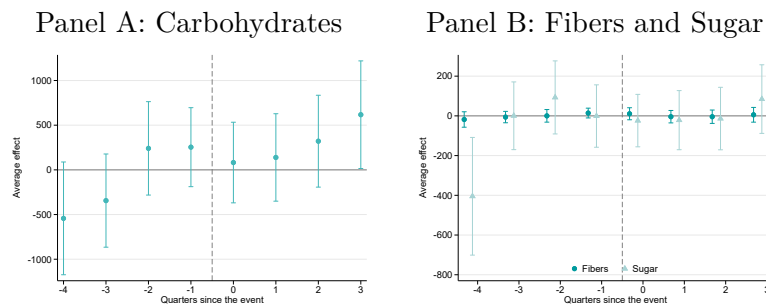
Notes: The figure reports the event study estimates and the 95 percent confidence intervals using the [Callaway and Sant'Anna \(2021\)](#) estimator. The outcome variables are the grams of carbohydrates per person per quarter. The observation is at the household-quarter level. The source of the data is in the NielsenIQ Panel data. I exclude Connecticut.

Figure B11: Fibers and Sugar - No Later States



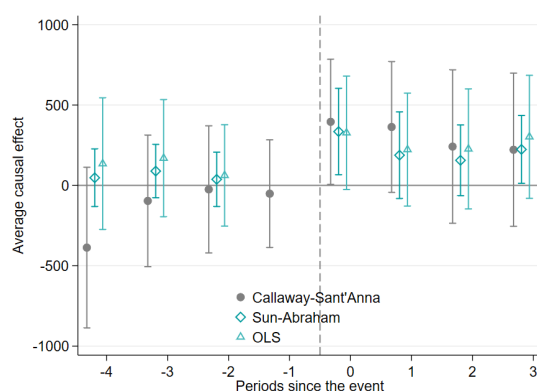
Notes: The figure reports the event study estimates and the 95 percent confidence intervals using the [Callaway and Sant'Anna \(2021\)](#) estimator. The outcome variables are the grams of carbohydrates per person per quarter. The observation is at the household-quarter level. The source of the data is in the NielsenIQ Panel data. I exclude states that were later treated, i.e., Maryland, North Dakota and Nebraska.

Figure B12: Medicare Placebo



Notes: The figure reports the event study estimates and the 95 percent confidence bands using the [Callaway and Sant'Anna \(2021\)](#) estimator. Source of the data in the NielsenIQ Panel. The figures report falsification test on households with diabetes and Medicare, which were not affected by the insulin cap policy.

Figure B13: Carbohydrate: Other Methods



Notes: The figure reports the event study estimates and the 95 percent confidence intervals using the [Callaway and Sant'Anna \(2021\)](#) estimator, [Sun and Abraham \(2021\)](#) estimator and the two-way fixed effect (OLS). The outcome variables are the grams purchased on the different nutrients by the household. The observation is at the household-quarter level. Source of the data is the NielsenIQ Panel.

Table B5: Timeline - COVID-19 Measures and Stimuli

March 13, 2020	COVID-19: National Emergency
April 2020	COVID-19 Stimulus Checks for Individuals (\$1,200)
April 2020	Temporary layoff soared from 2 million to 18 million
December 2020	COVID-19 Stimulus Checks for Individuals (\$600)
January 2021	The cap becomes effective in several other states
March 2021	COVID-19 Stimulus Checks for Individuals (\$1,400)

Notes: The table reports the timeline of the different COVID-19 measures and stimuli that were implemented between 2020 and 2021.