Uncovering the role of moral hazard in health insurance[*]

Matilde Cappelletti

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Abstract

I investigate whether a decrease in a prescription drug's out-of-pocket cost increases risky behaviors due to ex-ante moral hazard. Understanding the role of ex-ante moral hazard is crucial, given its implications for preventable health issues and healthcare expenditures. In a difference-in-difference setting, I leverage the staggered implementation of U.S. state-level policies lowering insulin out-of-pocket costs. Focusing on privately insured households with diabetes and using household-level scanner data on grocery purchases, I find that the out-of-pocket cost reduction results in an increase in risky behaviors due to ex-ante moral hazard. In particular, the findings show a temporary increase in carbohydrate purchases (4.8%) and a sustained increase in sugar purchases (9.3%), which can lead to adverse health effects. I rule out alternative explanations, such as an income effect, for the observed ex-ante moral hazard. In addition, I find evidence of a sustained increase in healthcare utilization, highlighted by higher sales of diabetes supplies.

Keywords: moral hazard, risky behaviors, health insurance, dietary choice, diabetes.
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[*]University of Mannheim and ZEW Mannheim. Email: *matilde.cappelletti@gess.uni-mannheim.de.*
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1 Introduction

Healthcare spending in the U.S. has been rising over the past decades, averaging 17.5% of GDP between 2016 and 2019 (CMS, 2023), with similar trends in other OECD countries (OECD, 2019). As expenditures are expected to rise further due to aging populations and governments are facing declining healthcare budgets, it is essential to identify cost-effective policies to ensure the financial sustainability of healthcare systems. Policies that aim to reduce out-of-pocket costs for patients, such as the U.S. Inflation Reduction Act, are of particular relevance given their broader implications for patient behavior. Indeed, such policies can increase patients' healthcare utilization (Brot-Goldberg et al., 2017) and may encourage risky behaviors by increasing ex-ante moral hazard (Ehrlich and Becker, 1972).

In this paper, I evaluate the effects of reducing out-of-pocket costs on households' healthcare utilization and risky behaviors. I focus primarily on risky behaviors to understand whether there is evidence of ex-ante moral hazard. Ex-ante moral hazard refers to the idea that insurance, by lowering the out-of-pocket price of care, might lead individuals to reduce efforts in maintaining 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, higher healthcare spending, and an unequal burden on healthcare resources if certain groups misutilize care due to low perceived personal costs. Moreover, causal evidence on the effect of 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 enacted policies *reducing* the out-of-pocket costs for insulin for insured individuals. The policies set a monthly out-of-pocket costs cap, ranging from \$25 to \$100, with any excess covered by insurance. In a staggered difference-in-difference 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 reducing insulin out-of-pocket costs for insured patients increases risky behaviors as households worsen their dietary choices. In particular, I document that this is due to ex-ante moral hazard. The results also indicate that reducing out-of-pocket costs leads to a sustained increase in the sales of devices essential for daily diabetes management.

These results provide important evidence for understanding the role of ex-ante moral hazard in health insurance in three main ways. First, I provide results on nutritional content rather than on product categories. Not only do the nutritional content outcomes rely less on the researcher's definition of risky behaviors, but they also offer precise insights into diabetes management. Second, I focus on individuals who are already sick and thus may behave differently from healthy individuals. Their more frequent need for care makes them particularly relevant to the financial sustainability of the healthcare system. Finally, while the existing literature investigates the effect of expanding

[1] In 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).

insurance coverage (i.e., the extensive margin) on risky behaviors, I consider a change in the out-of-pocket costs (i.e., the intensive margin). Empirical evidence on the intensive margin is limited due to the challenges of finding a plausibly exogenous identification strategy and observing both risky behaviors and insurance plan information simultaneously.[2] In this paper, I overcome both of these empirical challenges by exploiting recent policies reducing the out-of-pocket costs for insulin and setting up a dataset with the necessary information.

I combine information from multiple data sources. I employ representative panel data on grocery purchases for over 83,000 unique households, the 2019–2022 NielsenIQ Consumer Panel provided by the Kilts Center. To identify households potentially affected by the policy, I match the Consumer Panel with the NielsenIQ Annual Ailments, Health, and Wellness Survey, which provides information on diabetes and insurance status. Finally, I complement the dataset with extensive information on the nutritional content of the products purchased.

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 two main reasons. First, diabetes is a widespread, chronic disease that affects 11.3% of the U.S. population and, due to demographic shifts, its incidence is expected to rise further (Ong et al., 2023). It also accounts for $1 out of every $4 spent on healthcare in the U.S. (ADA, 2023; Hirsch, 2016) and is the seventh leading cause of death due to its association with several comorbidities (ADA, 2018). Second, for individuals with diabetes, prescription drugs and risky behaviors are tightly linked. Indeed, the disease is characterized by sustained high blood sugar levels due to the lack of insulin production. Carbohydrate—especially sugar—tracking is essential to diabetes management in order to control blood sugar levels and determine the amount of insulin to be injected, which increases with higher carbohydrate intake (CDC, 2023).

The reduction in insulin out-of-pocket costs leads to a 4.8% increase in carbohydrate purchases over the first post-treatment quarter. One concern might be that households have been consuming too few carbohydrates in the pre-period if they lacked access to an optimal amount of insulin. To address this concern and better understand whether ex-ante moral hazard is at play, I examine whether households engage more in additional risky behaviors, which might lead to adverse health effects. I find evidence for this, as there is a sustained increase in sugar purchases (9.3%), which can lead to blood sugar spikes. If repeated, such spikes can lead to comorbidities and increase mortality risk (Holman et al., 2008). I also observe short-term increases in fat, cholesterol, and calorie purchases, potentially exacerbating or leading to comorbidities. Although I find a 4.1% increase in fiber purchases—a recommended type of carbohydrate due to its lack of impact on blood sugar (Ley et al., 2014)—and no increase in saturated fats or sodium purchases—which are correlated with cardiovascular risks (Schwab et al., 2021), ex-ante moral hazard appears to be present. This is especially highlighted by the increase in sugar purchases.

I then investigate the effect of the policy on overall diet healthiness. To do so, I construct a diet score in which food categories are scored based on doctors' classification as good, neutral, or bad sources of calories for diabetic patients (Hut and Oster, 2022; Oster, 2015). For each

[2] Brook et al. (1983) and Newhouse (1993) are notable exceptions.

household, the food category scores are weighted by each product's spending share.[3] The diet score results suggest that households continue to exert a similar effort to maintain their health, masking underlying changes as highlighted in the nutritional content analysis.

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. This finding holds regardless of the size of the cap and households' income levels.

Additional results show that the policy leads to an increase in healthcare utilization. I find that the reduction in insulin out-of-pocket costs leads to a sustained increase in medication adherence and testing.[4] In particular, the out-of-pocket cost reduction leads to a sustained increase in insulin syringe sales. Given that insulin and syringes are complementary goods, the increase suggests that the policy has bite and that the cap affects insulin consumption behaviors. I also observe an increase in the sales of glucose strips, which are used to monitor blood sugar levels. These effects are statistically significant starting four months in the post-period and are sustained up to 12 months. The effect is stronger in states with a low cap, given that the possible savings for the households are larger and more individuals are likely to be affected.[5] Furthermore, I show that the effects are driven by the policy itself rather than by a decrease in diabetes device prices.

Finally, I find some evidence that the policy leads to a reduction in adverse health effects. In particular, I show that the sales of ketone testing strips decrease after the policy implementation. Ketone strips are used to check for high levels of ketones, which can occur during illness or prolonged periods of elevated blood sugar. High levels of ketones also indicate an increased risk of acute complications of diabetes. Therefore, the decrease in sales results suggests improved long-term health outcomes. The presented results are robust to different specifications, e.g., different methodological approaches and sample selection.

These results have important policy implications. First of all, these findings might inform decisions in states planning to introduce or lower the cap. Overall, the findings show that the policy has a sustained effect on medication adherence and testing, with a mostly moderate and temporary impact on dietary outcomes. These findings 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). Heterogeneity analyses suggest that the policy might lead to worse behaviors for specific groups, such as households that engage in less physical activity and those with no comorbidities. Therefore, policymakers might need to consider heterogeneity in responses when designing similar policies.

This paper contributes to five strands of literature. First, this paper relates to studies examining

[3] The results are similar when using the share of calories and serving size instead of the spending share.

[4] To analyze this, due to Consumer Panel data limitations, I employ diabetes device sales data for over 30,000 stores from the NielsenIQ Retail Scanner Data.

[5] I define "high cap" states as those with caps above the median ($40–$100 per month) and "low cap" states as those below the median ($25–$35 per month).

whether insurance coverage increases risky behaviors due to ex-ante moral hazard. 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.[6] Dave and Kaestner (2009) find that Medicare coverage increases the likelihood of daily alcohol consumption for men, while Chen et al. (2023) find evidence against ex-ante moral hazard. Among others, Simon et al. (2017) and Cotti et al. (2019) study the effect of the Affordable Care Act and find no increase in ex-ante moral hazard. My contribution is threefold. First, I study the effect of a reduction in out-of-pocket cost (i.e., the intensive margin) on ex-ante moral hazard.[7] Second, to the best of my knowledge, this paper is the first to use nutritional content in the moral-hazard literature. Third, while the existing ex-ante moral hazard literature studies risky behaviors for the overall population, I provide evidence on risky behaviors for individuals with a disease. The behaviors considered are relevant for disease management.

Second, the recent medical literature has examined the effects of out-of-pocket caps on insulin use and patient savings, with mixed results regarding insulin usage. Giannouchos et al. (2024) and Ukert et al. (2024) find that lower caps lead to an increase in insulin usage. Conversely, Garabedian et al. (2024) and Anderson et al. (2024) find no significant change in usage. The difference may stem from Giannouchos et al. (2024)'s ability to observe whether individuals are spending above the cap, as they show that they are the primary drivers of the results. However, all papers (except for Anderson et al. (2024)) show reductions in out-of-pocket costs. Other recent studies investigating the effect of out-of-pocket costs outside of the U.S. Americo and Rocha (2020) find a reduction in hospitalization rates due to subsidized prescription drugs for diabetes in Brazil. McNamara and Serna (2022) find that expanded coverage in Colombia leads to increased insulin consumption, and a reduction in outpatient care and hospitalization rates for diabetes. Finally, Barkley (2023) documents a decrease in insulin prices for public insurers, a consequent increase in insulin utilization, and an improvement in health outcomes. I also add to the broader literature on out-of-pocket costs, which generally examines their impact in two areas: (i) health outcomes and (ii) healthcare utilization.[8] In the first area, the evidence is quite clear that lower out-of-pocket costs improve individuals' health. Most scholars find that a decrease (increase) in out-of-pocket costs leads to positive (negative) health effects (Huh and Reif, 2017; Dunn and Shapiro, 2019; Chandra et al., 2021; McNamara and Serna, 2022; Barkley, 2023). Others find negligible (Chandra et al., 2014) or mixed (i.e., zero and positive) effects (Americo and Rocha, 2020). In the second area, Ziebarth (2010) and Shigeoka (2014) are papers most closely related to mine, as they study changes in out-of-pocket cost.[9] My contribution to this literature is threefold. First, I

[6]The literature on ex-ante moral hazard also focuses on preventive care, e.g., Barros et al. (2008), Spenkuch (2012), and Simon et al. (2017).

[7]On the intensive margin the evidence is limited, Brook et al. (1983) and Newhouse (1993) are notable exceptions.

[8]Following a decrease (increase) in out-of-pocket costs, the existing evidence mostly suggests an increases (decreases) in the use of prescription drugs (Gaynor et al., 2007; Fiorio and Siciliani, 2010; Puig-Junoy et al., 2016; McNamara and Serna, 2022; Barkley, 2023) and healthcare (Chandra et al., 2010, 2014; McNamara and Serna, 2022). Some papers find no changes in prescription drug (Martínez-Jiménez et al., 2021) and healthcare consumption (Puig-Junoy et al., 2016; Park et al., 2019).

[9]Ziebarth (2010) finds that an increase in out-of-pocket cost decreases the demand for health services. Shigeoka (2014) shows that patients' care utilization is sensitive to cost-sharing reductions.

provide evidence of diabetes-related purchases that do not require a prescription. Second, I provide additional evidence on the policy's impact on health outcomes, complementing Giannouchos et al. (2024)'s findings of a reduction in diabetes-related medical claims. Third, using a novel dataset, I provide evidence of the policy on dietary outcomes, which are essential for disease management.

A third relevant strand of research focuses on the factors influencing dietary choices. Dubois et al. (2014) find that prices and product characteristics do not entirely explain cross-country food purchase differences; different preferences are important.[10] The literature also shows that diet is persistent (e.g., Hut (2020)). With this work, I contribute by showing that the out-of-pocket costs for prescription drugs can impact diet. This is relevant for further understanding how diet is determined for individuals with diet-related diseases.

Fourth, I build on papers studying the link between food purchasing behavior and health. Gračner (2021) shows that decreased prices of foods high in sugar affect obesity and diet-related ailments. Bao et al. (2020) report that individuals with obesity are more likely to increase their food purchases when products are on promotion. My paper is most closely related to Oster (2018), showing that, upon a diabetes diagnosis, households follow doctors' recommendations to reduce calorie intake and that the change is concentrated on unhealthy foods. Hut and Oster (2022) investigate the effect of hypertension and obesity diagnoses and find that households make minor dietary adjustments concentrated in one food category. With this paper, I study a setting related to obesity and I provide evidence of households' purchase behavior when the prescription drug out-of-pocket costs decrease.

Finally, I contribute to the literature investigating how government-led interventions impact unhealthy food consumption. Sin taxes—i.e., taxes imposed on goods considered harmful, such as sugary drinks—are imposed to contain sugar consumption. Dubois et al. (2020) find that sugar taxes are well-targeted to younger individuals, though less to those with a high-sugar diet. Other scholars find mixed results: while soda taxes decrease the frequency of soda consumption, individuals also travel to areas without the tax to purchase soda (Cawley et al., 2019) or partially substitute the decrease in soda with an increase in sugary food purchases (Lozano-Rojas and Carlin, 2022).[11] I contribute to this literature by showing that government policies can have unintended consequences on unhealthy food consumption for households already affected by diabetes and, possibly, obesity.[12]

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.

[10] The role of information is also important, as many individuals may lack accurate information about diet or have no information at all (Belot et al., 2020; Vitt et al., 2021).

[11] Other papers study the effectiveness of food labeling, which provides consumers with information about a product's nutritional content. Barahona et al. (2023) find that individuals decrease sugar and calorie consumption. Papers focusing on government vouchers for healthy products also show mixed results. While Griffith et al. (2018) find vouchers to be effective, Hinnosaar (2023) finds that this effect is short-lived, disappearing two years after the program's end.

[12] Almost 80% of type II diabetic patients are overweight or obese (Iglay et al., 2016).

2 Background

In this section, I provide additional details about diabetes and its management, and on the policy I exploit in this paper.

2.1 Diabetes: Definition and Management

In healthy individuals, the pancreas produces insulin, which is needed to regulate blood glucose levels. When this process is impaired, the blood glucose levels chronically rise, leading to diabetes.[13] There are two main types of diabetes: type I and type II.[14] In type I diabetes, the pancreas produces either too little insulin or none at all (Khin et al., 2023). In type II diabetes, the body does not process insulin correctly and does not remove sugar from the bloodstream. Type I is genetic, usually appearing during childhood, and affects only a small percentage (around 5%) of individuals with diabetes. The vast majority (95%) has type II diabetes, for which advanced age and obesity are strong predictors. Almost 80% of type II diabetic patients are overweight or obese (Iglay et al., 2016). Thus, weight management plays a crucial role in managing type II diabetes and its related comorbidities. While weight gain can make diabetes harder to control, a 10% weight loss can improve diabetes management and lower blood pressure (Look AHEAD Research Group, 2007). To manage weight, lifestyle changes—such as exercise, a balanced diet, and calorie restriction—are often prescribed.

If blood sugar levels are not well-controlled, type II diabetes can require insulin for its management.[15] Overall, 23% of patients with diabetes use insulin (ADA, 2022). To avoid abnormalities in blood sugar, insulin dosing must accompanied by regular blood sugar monitoring and carbohydrate counting. Carbohydrate counting is needed since insulin dosage is based on carbohydrate intake.[16] The *type* of the carbohydrate is also crucial for managing blood sugar levels and the disease (Ley et al., 2014). Fiber-rich carbohydrates lead to a slow impact on glucose levels. At the same time, foods high in sugars can quickly elevate blood sugar levels. Frequent blood sugar spikes should be avoided as they can lead to both short-term and long-term complications in individuals with diabetes, such as the development of comorbidities (Holman et al., 2008).

Besides avoiding sugars and including fibers, a balanced diet should incorporate lean proteins, and healthy fats while limiting unhealthy fats, sugars, and sodium. Such a diet can lower the risks of diabetes-related complications. Adequate healthy fat and protein intake is recommended, as

[13]Blood glucose levels are tested to diagnose diabetes. The A1C test measures the blood glucose level over the past three months: a value less than 5.7% is considered normal, 5.7% to 6.4% leads to a prediabetes diagnosis and 6.5% to a diabetes diagnosis (ADA, 2024).

[14]There are some other types of diabetes, such as gestational diabetes, which usually has its onset during pregnancy. However, I exclude them from the discussion here since I do not observe them in the data, but also because they do not make up a significant portion of patients with diabetes.

[15]While type II diabetes can be improved or cured with lifestyle changes, type I diabetes cannot be cured. Note that for type II diabetes, there are also oral medications. All type I diabetic patients must use insulin.

[16]The exact dosage is individual-specific, as individuals have different insulin-to-carb ratios. Assume that an individual plans to eat 45 grams of carbohydrates for their meal. If their insulin-to-carb ratio is 1:15, then the insulin amount needed is three units. For the final injection, the individual must also account for the current and target blood sugar to be reached.

a reduction in protein for weight loss could result in lean muscle loss, which is not desired (Ley et al., 2014). Conversely, the consumption of saturated fats and cholesterol can increase the risk of cardiovascular diseases and hyperlipidemia (Imamura et al., 2016; Fernandez and Andersen, 2014). Additionally, high sodium intake is linked to hypertension (Grillo et al., 2019). In Section 5, to better understand whether ex-ante moral hazard is at play, I investigate the effect of the policy on nutrients that are correlated to these comorbidities.

Comorbidities, such as hypertension, hyperlipidemia, and cardiovascular diseases, are common for individuals with diabetes. In the U.S., nearly one-third of individuals with type I diabetes has hypertension. The prevalence is even higher among those with type II, with four out of five individuals being affected (Landsberg and Molitch, 2004). Hyperlipidemia—i.e., abnormally high levels of fats in the blood—affects 77% of diabetic patients (Iglay et al., 2016). Hypertension and hyperlipidemia are also risk factors for cardiovascular diseases, such as heart failure (Fuchs and Whelton, 2020). Heart failures can be deadly: a fifth of patients with diabetes die from stroke-related complications (Phipps et al., 2012).

2.2 The Policy: 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.[17] 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.[18] The policy ensures that privately insured individuals pay no more than $25–100 (depending on the state) for a 30-day supply of insulin, with any excess covered by the insurance. As discussed in Section 1, recent papers find an increase in savings. On average, the policy leads to a $20–27 decrease in out-of-pocket costs per month (Giannouchos et al., 2024; Ukert et al., 2024).

In 2019, the average monthly out-of-pocket spending on 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 shows that 70% of individuals would benefit from a $35/month cap, while 25% would benefit from a $100 cap, as shown in Figure A1 in the Appendix. These estimates are further confirmed in the medical literature. Bakkila et al. (2022) find that, in 2017–2018, 15% of diabetic patients using insulin spent nearly half of their income on insulin prescriptions. These high costs not only led to decreased insulin use but also to poor blood sugar control. Similarly, a survey by Herkert et al. (2019) shows that 25.5% of patients with diabetes underutilized insulin due to its high cost. In Section 5.3, I provide evidence that insulin syringe sales—which are a complement of insulin sales—increase after the policy.

Figure 1 shows the timing of the policy implementation (Panel A) and the size of the cap (Panel B).[19] In Panel A, the white-colored states did not implement an insulin cap policy or did

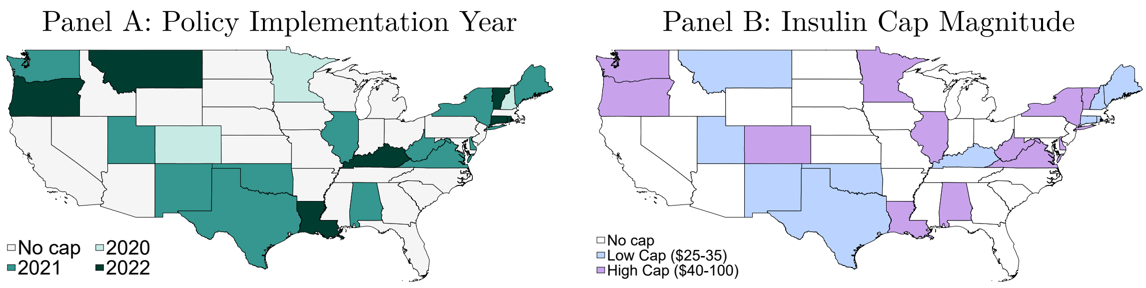
[17] In 2019, the vast majority (68%) of individuals had a private insurance (U.S. Census Bureau, 2020).

[18] To date, 26 states have passed legislation that caps insulin out-of-pocket costs. In my analysis, however, 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 cannot analyze the effect of this policy.

[19] Table A1 in the Appendix reports for each state the magnitude of the out-of-pocket cost cap, the date on which

so after 2022. The shades of green indicate when the policy was implemented, from light green for the states implementing the policy in 2020 to darker green in 2022. In Panel B, the map uses blue to highlight states that implemented a "low cap" (i.e., a below-median cap, ranging from $25 to 35) and purple for those with "high cap" (i.e., an above-median cap, ranging from $40 to 100). The map highlights that there is geographical variation in the policy implementation, as states enacted the cap at different times and with caps of varying levels.

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



Notes: Own data collection from state-level bills. Panel A reports the year each state implemented its cap on insulin out-of-pocket costs. Panel B indicates whether the cap set by each state falls below (low) or above (high) the median cap level.

Potential Effects of the Policy on Household Behavior Reducing insulin out-of-pocket costs might impact diet quality, given that insulin consumption depends on an individual's carbohydrate consumption. To illustrate the possible effect of the cap on nutrition, consider two scenarios. In the two scenarios, holding everything else equal (e.g., income, preferences, and health insurance), the number of units of insulin per day that the individual can afford changes. This is comparable to the effect of the policy. By imposing a cap on out-of-pocket costs, the policy increases the amount of insulin individuals can afford. The cap affects not only individuals who were paying above the cap in the pre-period but potentially also those paying at or below the cap, who can now afford more insulin for the same out-of-pocket cost.

In the first scenario, the individual can buy 20 units per day, corresponding to a carton of insulin per month. In this case, the individual could be budget-constrained to limit the carbohydrate intake to keep the glucose levels within range. In a second scenario, similar to the setting with an out-of-pocket cost cap, the same individual can afford 100 units of insulin per day. Potentially, the individual is now free to "indulge" and increase the number of carbohydrates consumed, knowing that they can regulate their blood sugar levels with additional insulin.

This example simplifies reality to emphasize the possible shift in incentives for the individual, which is the question at the core of this paper. Whether individuals "indulge" in the second scenario and whether this is due to ex-ante moral hazard are empirical questions. In addition to increased carbohydrate purchases, ex-ante moral hazard could be observed through additional dietary changes. Dietary changes could involve shifts in food quality (e.g., changes in the type of

the bill became effective, and when it was signed.

carbohydrates purchased or in other nutrients) and quantity (e.g., calorie intake). For instance, an increase in calories and fats could lead to weight gain. In turn, this could exacerbate the diabetes symptoms and comorbidities, as highlighted in Section 2.1.

Reducing insulin's out-of-pocket cost might also increase available income. The size of the income effect for the household depends on the out-of-pocket costs paid by the household before the policy's implementation and the cap in the state in which they live. If the household is not affected by the policy, since they were already paying below the new limit, there would be no savings. However, if the household were paying over $200 per month and were living in a $35-cap state, the yearly saving would be sizable, i.e., around $2,000. Therefore, the income effect could be substantial and affect the household's grocery and diabetes device purchasing decisions. A $2,000 yearly savings corresponds to 2.9% of the average gross household income in the dataset, which is around $67,00. Finally, the out-of-pocket cost reduction could lead to greater medication adherence. Existing research finds that patients are price sensitive (Goldman et al., 2004; Shankaran and Ramsey, 2015; McAdam-Marx et al., 2024). The income effect and improved disease management could both affect nutrition. I discuss this in detail in Section 5.3.

3 Data

In this section, I provide descriptive statistics for the three main data sources I use. First, I describe the food purchase data. I then report information on the health and insurance status data. I conclude by describing the retailer-level data on diabetes device sales.

3.1 Household Data

Food Purchase Data I use the NielsenIQ Consumer Panel (NCP) data from 2019 through 2022, available through the Kilts Center at the University of Chicago Booth School of Business. The NCP contains data on consumer purchases: Upon returning home from a shopping trip, panelists scan their purchased items with at-home scanner technology.[20] Shopping trips included are those from supermarkets, drug stores, large and small retailers, and online outlets. The data contain individual demographic information, such as household size, structure, income, education level of the heads of household, and ages of all household members. The data also include 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.[21] For a subset of products, NielsenIQ provides additional detailed information on nutritional content.[22] This allows me to construct diet outcomes, such as the per-quarter grams of carbohy-

[20] Individuals are incentivized to participate and stay in the panel in three different ways by NielsenIQ: monthly prize drawings, gift points redeemable for merchandise and gift cards and sweepstakes and contests.

[21] Prices are either recorded by panelists or retrieved from NielsenIQ's store-level data when accessible.

[22] NielsenIQ started providing this dataset in 2021. Using the UPC, I am able to assign this information to purchases 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. I am also grateful to Felipe Lozano-Rojas for sharing some of his

drates purchased. I scale these outcomes by dividing them by the number of members living in the household.

In my analysis, I also investigate the overall healthiness of the diet by constructing different measures of diet scores. I build on Oster (2015) and Hut and Oster (2022), who surveyed 17 doctors and asked them to rate 59 food categories as a “good” (+1), “neutral” (0), or “bad” (-1) source of calories for diabetic patients. Using product-level data, I classify the food items based on their descriptions. I then calculate an average rating for each product category by combining the ratings from all 17 doctors.[23] The diet score for each household is calculated by adjusting each product’s score according to the share of that product in the household’s total purchases. A diet score based on the share of spending provides both advantages and disadvantages. On the one hand, the price information is reported for all the products purchased. This allows me to capture the totality of the households’ basket.[24] On the other hand, prices could be lower for highly processed products and higher for healthier ones, and this would give more weight to healthier products and less to unhealthier ones. Therefore, I also construct two additional diet scores using different shares, i.e., (i) the share of calories of the products out of the overall basket, and (ii) the share of serving sizes for a product related to the overall purchased serving sizes.

While the data stem from self-reported food purchases, previous studies provide evidence about its validity. Einav et al. (2010) find that most of the discrepancies are in prices, while information on shopping trips, products, and quantities is mostly accurate.Therefore, these data limitations are not a particular concern in my setting, given that I primarily employ information about trips, products, and quantities. Only 20% of trips are not reported in the NCP. 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.

Health and Insurance Status Data 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 about their ailments, general health, diet, and wellness. The data are detailed, reporting whether participants have type I or II diabetes or prediabetes.[25] I include the first two in my analysis and exclude the latter, yielding 9,598 unique households where at least one member has diabetes. Starting in 2020, I also observe whether a household member has private health insurance.[26] In heterogeneity analyses, I use information on other diseases the households have, the number of hours per week they exercise, and whether they follow a low-carbohydrate diet. Only a subset of the households ($\sim$64%) ever participates in the Ailment

work on nutritional content data that guided my construction of the outcomes.

[23]For example, ice cream received a score of -1, eggs scored 0.7, and juice scored -0.64. The complete list of products and the corresponding rating is reported in Table A2 in the Appendix.

[24]Indeed, additional information, such as nutrients and serving size, is available for only a subsample of the data.

[25]An individual with prediabetes is at a stage in which they do not need medication but might need it in the near future if they do not cure the disease with lifestyle changes, such as exercise and diet.

[26]Other possible answers, which I do not include in my analysis, are Medicare, Medicare, and uninsured.

Survey, as panelists are not required to do so.[27] Therefore, the missing information on diabetes and health insurance status are data limitations that need to be addressed. I discuss below how I overcome these limitations and how I use the information from the Ailment households to predict the diabetes and insurance status of the rest of the panelists.

I predict the diabetes diagnosis for the individuals who do not reply to the Ailment Survey using information available for all NCP panelists. To do so, I consider a simple rule based on observed diabetes-related purchases.[28] 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. If at least one of these variables is in the third or fourth quartile, I categorize the household as having diabetes. In Table B1 in the Appendix B, I show that 67% of households with diabetes are correctly identified. Using this rule, I identify the diabetes status for 1,970 additional households.

Moreover, I predict the insurance status for non-respondents via logit with random effects using household-level information, such as the head of household age, household size, household income, education in years, and hours worked per week. The prediction results, reported in Table B2 in the Appendix B, show that 85% of households with private insurance are correctly identified. In Appendix B, I provide a detailed explanation of the prediction, and I show that respondents and non-respondents are largely comparable. In Section 5.5, I also show that the results are robust to the exclusion of the predicted observations.

Final Sample and Descriptives In my sample, I consider only households with at least one member with diabetes and at least one member covered by private health insurance, as the policy is effective for households with diabetes holding private insurance.[29] The *treatment* group is composed of households with diabetes with private insurance living in a state with the policy, while the *control* group is given by households with diabetes with private insurance living in a state *without* the policy.[30]

From my sample I exclude 100 households that move to a different state between 2019 and 2022. Additionally, I exclude two 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. The final sample consists of 69,969 observations and 8,651 unique households, all of which have diabetes and private insurance, and reside in one of the considered states.[31]

[27] Some households might participate in one year but not the following year.

[28] As reported in details in Appendix B, I test different methods. I use the simple rule as it performs well and allows me to increase the sample significantly.

[29] Around 10% of the sample has diabetes, which is similar to the share in the U.S. population (11.3%).

[30] This selection of treatment and control group allows me to overcome some concerns that might arise from the prediction of diabetes status and insurance. Due to the presence of a relatively high number of false negative households in my prediction, treated households might end up in the control group, which could create a downward bias in my estimates.

[31] Note that Alaska and Hawaii are not included in the dataset.

Table 1 reports households and shopping behavior descriptives.[32] Panel A reports household characteristics for the first year in which the household appears in the panel, both for households in control states (in columns 2 and 3) and in treated states (columns 4 and 5). The average head of the household is almost 61 years old, and the average household size is 2.5 individuals. Households have studied for an average of 14.5 years. Furthermore, the average household income is $67,278. Households in control states are slightly less wealthy, earning $66,579 yearly on average. The share of married (72%), single (7%), and with children below six years (1%) and white (78%) households is similar across the groups.

Panel B of Table A3 reports purchasing patterns for all households and for households with diabetes. Quarterly reported expenditures are similar across the states (around $1,300), also when accounting for the average household size ($600).[33] The table also provides information on the nutritional content, such as the average per-person quarterly grams and calories contained in the groceries purchased by the household. On average, each quarter, households with diabetes buy food containing ∼8,500 grams of carbohydrates, of which ∼500 are fibers and ∼1,426 are sugars. The food purchases contain ∼3,300 grams of fats and ∼1,700 grams of proteins. Such values are equivalent to ∼70,000 calories per quarter.[34] The household diet scores take a value between -1 and 1, where -1 indicates the least healthy diet. On average, households have a diet score of -0.06, -0.25, and -0.23 for spending, calories, and serving size, respectively. This suggests that households eat unhealthily but not remarkably so, depending on the specific diet score. In particular, this highlights that the diet score using the share of spending does indeed give more weight to healthier products due to their higher prices. I plot the three diet score densities in Figure A2 in the Appendix.

3.2 Diabetes Devices Purchase Data

To study the effect of the policy on diabetes device purchases, I employ sales data from the NielsenIQ Retail Scanner Data. The regressions are run at the store-month level, and the identification strategy discussed in Section 4 remains unchanged. The data contain products' weekly units sold, prices, and discounts (if any).[35] I also observe a store identifier, the store type (e.g., supermarket or drug store), and the state in which the store is located.

My sample includes diabetes devices sold without a prescription by drug stores. This implies that I do not observe insulin sales, which require a prescription. However, the data include several diabetes devices that do not require a prescription and are critical to disease management. Such

[32]Table A3 in the Appendix provides these descriptives comparing the entire panel with the selected sample.

[33]The per month average is $200 per person, which is in line with the $154 per-person average food spending reported by the Bureau of Labor Statistics (2020).

[34]This corresponds to 37% of the recommended daily intake of 2,100 calories for a moderately active 61-year-old, with women in this group needing 1,800 calories per day and men requiring 2,400 (Cleveland Clinic, 2023).

[35]I do not use the household-level data, given that households do not buy often enough such devices, resulting in a low number of observed instances. However, the analysis at the retailer level has the main drawback that I cannot disentangle customers affected by the policy and those who are not (e.g., those with other types of insurance). However, a change in treatment after the implementation of the policy in a difference-in-difference is driven by the affected individuals.

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				
General Behavior				
Total Spent $/Quarter	1,261	781	1,245	770
Total Spent $/Quarter (per person)	623	425	606	417
Nutrients and Calories (per person)				
Carbohydrates (g)	8,534	6,943	8,472	7,299
Fibers (g)	498	393	499	392
Sugars (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
Diet Score (Share Spending)	-0.06	0.10	-0.06	0.11
Observations	44,841		25,128	
Panel C: Diabetes Device Sales				
Insulin Syringes				
Units	215.28	435.67	223.46	447.20
Price	0.62	0.28	0.61	0.29
Glucose Monitors				
Units	8.95	19.09	9.08	19.52
Price	11.52	7.90	11.43	8.03
Glucose Test Stripcs				
Units	387.31	494.84	388.81	490.51
Price	2.18	2.01	2.24	2.05
Ketone Test Strips				
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, observation is at the household level using information from the first year in which the household enters the panel. In Panel B, observation is at the household-quarter level, and values are in grams (g) or milligrams (mg). In Panel C, observation is at the retailer month-level. Prices are unit prices in USD.

devices also account for an important part of monthly out-of-pocket costs: Chua et al. (2020) report that people spend more per month on devices than on insulin and that 36% of diabetic patients in high-income countries have rationed testing supplies.

I consider diabetes device sales aggregated to the monthly level and construct two main outcomes: the average sale price and the units sold. I examine diabetes device price adjustments, given that stores might change them if they anticipate increased demand after the insulin's out-of-pocket cost reduction. If this were the case, this would threaten my identification strategy, as individuals might change their behavior due to device prices and not the policy. In Section 5.3, I show that this is not the case. The units sold consider how many product units are contained in a single package—e.g., whether an insulin syringe package contains 10 or 100 syringes. I construct these outcomes for four device categories.[36] First, I consider insulin syringes, which are used to inject insulin.[37] 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 on glucose testing strips, which are used together with glucose monitors. Investigating glucose monitors and testing strip purchases can be informative regarding testing behavior. Fourth, I look at ketone strip sales, which are used to monitor high levels of ketones, which can occur during periods of sickness or extended periods of high blood glucose levels.

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

Staggered Difference-in-Difference Approach 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, I employ the staggered difference-in-difference approach à la Callaway and Sant'Anna (2021).[38] This approach is particularly 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 might change over that time.

To describe the research design, I use the potential outcome framework à la Robins (1986) and define D_{it} as an indicator equal to one if a household i lives in a state that has implemented the policy cap in quarter t. The outcome of interest Y_{it} can be written as $Y_{it} = Y_{it}(1)D_{it} -$

[36] I identify the categories by the product description provided by NielsenIQ. Up to 2020, NielsenIQ provides two product categories for diabetes-related products: insulin syringes and testing products. Among those, glucose monitors can be identified by the keywords "BG K" and "MNSYS." From 2021, they provide more detailed categories that allow me to retrieve additional products for 2019 and 2020.

[37] Although insulin pumps and pens are alternatives to injecting insulin with a syringe, the vast majority (65%) of the individuals with diabetes in the U.S. do not use them (Endocrine News, 2014).

[38] This problem was highlighted in the recent staggered difference-in-difference literature. When estimating a two-way fixed effect such $y_{it} = a_i + b_t + \theta(treat_i \cdot post_t) + e_{it}$, 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 arises between later-treated units and earlier-treated units. This is a "forbidden" comparison, especially if the treatment effects are expected to be heterogeneous, as the parallel assumption no longer holds.

$Y_{it}(0)(1-D_{it})$, 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. To overcome this challenge, difference-in-difference approaches rely on the comparison of 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,

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

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 $t \in \{1 \ldots, \mathcal{T}\}$,[39] the regression estimated to obtain $ATT(g,t)$ is equivalent to:

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

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 is to account for the probability that a state implements the insulin cap or other health-related policies that might influence the outcome.[40] $\varepsilon^{g,t}$ 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:[41]

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

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 group. 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

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

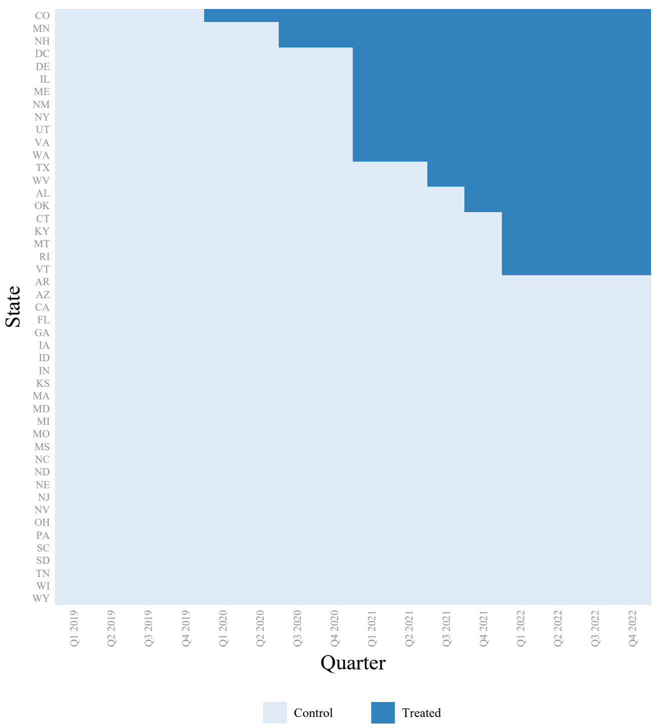
[40] Given that the post-treatment covariates might be influenced by the treatment itself, I use the pre-treatment values from the 2016 presidential elections. I obtain the state-level Democratic vote shares from https://www.fec.gov/resources/cms-content/documents/federalelections2016.pdf.

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

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 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.[42]

To illustrate key characteristics relevant to this approach, I present the staggered policy rollout in Figure 2. I report the two-digit code of the state on the y-axis and the quarter of implementation on the x-axis. Therefore, each square represents a state-quarter combination. The light blue indicates no treatment, while the dark blue indicates the treatment. As the figure shows, 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 2: Policy Implementation, Staggered Timing



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

Identification Assumptions For the identification strategy to be valid and for a causal interpretation of the results, three assumptions need to be satisfied. First, the approach relies on

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

the *irreversibility of treatment assumption.* Formally, this is defined as follows: $D_1 = 0$ almost surely (a.s.), and for $t = 2, \ldots, \mathcal{T}, \quad D_{t-1} = 1$ 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, as it is also represented in Figure 2.[43]

Second, the method relies on a (conditional) *no-anticipation* assumption. Formally, for all g $\in \mathcal{G}, t \in \{1 \ldots, \mathcal{T}\}$ such that $t < g$:

$$\mathbb{E}[Y_t(g)|X, G_g = 1] = \mathbb{E}[Y_t(0)|X, G_g = 1] \quad a.s. \tag{4}$$

This assumption requires that the household does not actively choose the treatment status.[44] 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 different ways.[45] First, households could move to take advantage of the policy. However, in my data, only around 100 households moved in the period of the analysis, so this seems to be a minor concern.[46] Second, people might switch insurance status to benefit from the cap. I do not find any evidence that a significantly larger share of the population takes up insurance after the policy.[47] In my analysis, I limit my sample to individuals who 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 might reduce their carbohydrate purchases as well. The event study estimates, both on nutritional outcomes and on diabetes device sales, show no evidence of this.

Third, the method relies on the (conditional) *parallel trends assumption*, based on a never-treated group:

$$\mathbb{E}[Y_t(0) - Y_{t-1}(0)|X, G_g = 1] = \mathbb{E}[Y_t(0) - Y_{t-1}(0)|X, C = 1] \quad a.s. \tag{5}$$

for all g $\in \mathcal{G}, t \in \{2 \ldots, \mathcal{T}\}$ s.t. $t > g$. 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 evaluate 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. Some shocks likely affected both treated and control states in a similar fashion, and so the time fixedeffects partially account for them. However, in certain cases, treated states reacted differently than untreated states. Therefore, in Section 5.5,

[43] An additional assumption is that the researcher has access to panel data, which is the case in my setting. The case with cross-section data is possible and is an extension in Callaway and Sant'Anna (2021).

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

[45] In Colorado, the law was passed in May 2019 and was enacted in January 2020.

[46] I exclude those households from the analysis.

[47] I 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 A3 in the Appendix, I plot the share and confidence intervals of insurance coverage from two years before the policy implementation to two years after.

I provide different robustness checks to control for such concerns.[48]

Finally, one might be concerned about spillover effects, such as individuals crossing state borders to benefit from the cap in a neighboring state. However, this is not possible, as the cap applies only to those insured within the state where the law is enacted.

5 Results

In this section, I start by analyzing the effects of the policy on carbohydrate purchases. I further investigate the effect of ex-ante moral hazard on dietary outcomes. I then show that the effects are heterogeneous, and I conclude by discussing additional results and mechanisms.

5.1 Baseline Results: Carbohydrates Purchase

Panel A of Figure 3 shows the main results of the household-level analysis. It plots the event study for grams of carbohydrates purchased per quarter by the household (adjusted by the number of members in the household), showing the dynamics of the treatment effect. I report the coefficients for four quarters before and after the policy implementation. All of the coefficients of the pre-period are close to zero and are not significant. I also test that all the pre-treatment periods are *jointly* equal to zero, and I cannot reject this hypothesis, suggesting no discernible pre-trends prior to the policy implementation. I report these tests in Table A5 in the Appendix.

The interpretation of coefficients in Figure 3 partially differs from a standard two-way fixed effect (TWFE) figure. The main difference is that I report a coefficient for $t-1$, which is usually omitted in a TWFE and used as a reference period. This difference is due to the identification suggested by Callaway and Sant'Anna (2021). In the post-period, as in the TWFE, the reference period is $t-1$, i.e., the quarter before the policy implementation.[49] However, in the pre-period, the identification comes from the difference between two adjacent periods, such as t and $t-1$, as reported in equation (5). In Table A5 in the Appendix, I report the coefficients with the normalization at $t-1$ as it is used in the TWFE.

The reduction in insulin out-of-pocket costs leads to a temporary increase of 412.5 grams of carbohydrates purchased in the first quarter of the post-period. This gram increase is equivalent to the carbohydrate content of 11 cups of cooked white rice (per quarter). Considering a baseline average in the pre-period of 8,472 grams per quarter, this corresponds to a 4.8% increase.

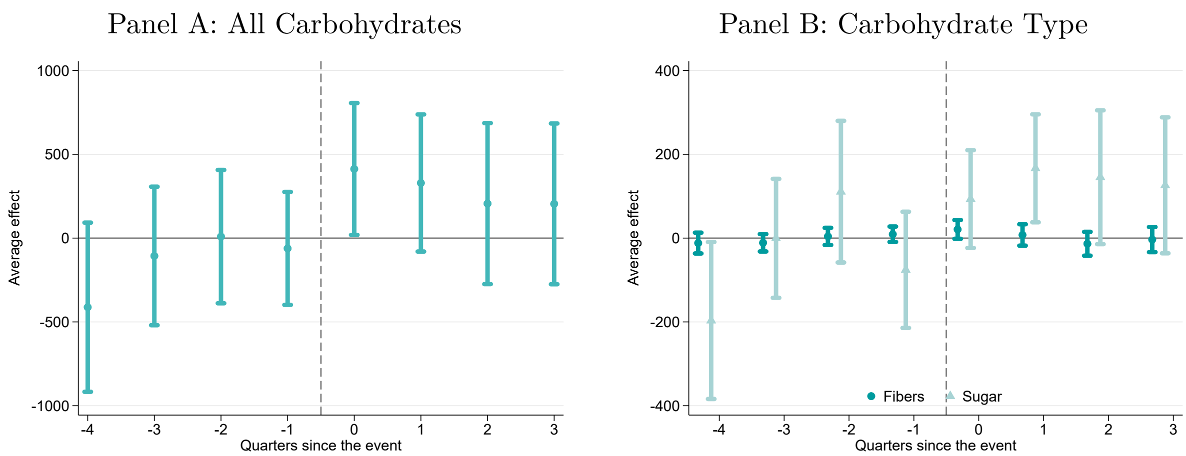
An alternative explanation for ex-ante moral hazard for the detected increase might be that households were consuming too few carbohydrates in the pre-period. This might be the case if they lacked access to an optimal amount of insulin. To exclude this alternative explanation, I investigate the effect on the type of carbohydrates. In the medical literature, observational and clinical studies

[48] An additional concern regarding the pandemic might be that individuals with diabetes might be more strongly affected by COVID-19 than healthy individuals. Given that my analysis is run only on households with diabetes, this is not a concern in my setting.

[49] In the post-period, the coefficients are estimated with respect to $g-1$, which is the $t-1$ for the group g, as described in equation (3). The same is also done for the control group. The coefficients in the post-period at $t+1$, can be interpreted as the difference between $t+1$ and $g-1$.

have highlighted that the quality of carbohydrates and fats consumed is extremely important (Ley et al., 2014). Panel B of Figure 3 plots the coefficients for two types of carbohydrates purchased—fibers (dark green) and sugar (light green). It shows that fiber purchase increases by 20.7 grams per person ($\sim$ 4.1%). This is considered a "good" carbohydrate since its consumption does not cause sugar spikes. The consumption of foods high in fiber is actually encouraged as they are found to improve several measures of glycemic control, body weight, premature mortality (Reynolds et al., 2020), and the development of comorbidities (McRae, 2018). These positive effects are found for both types of diabetes and different ranges of fiber intakes.

Figure 3: 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.

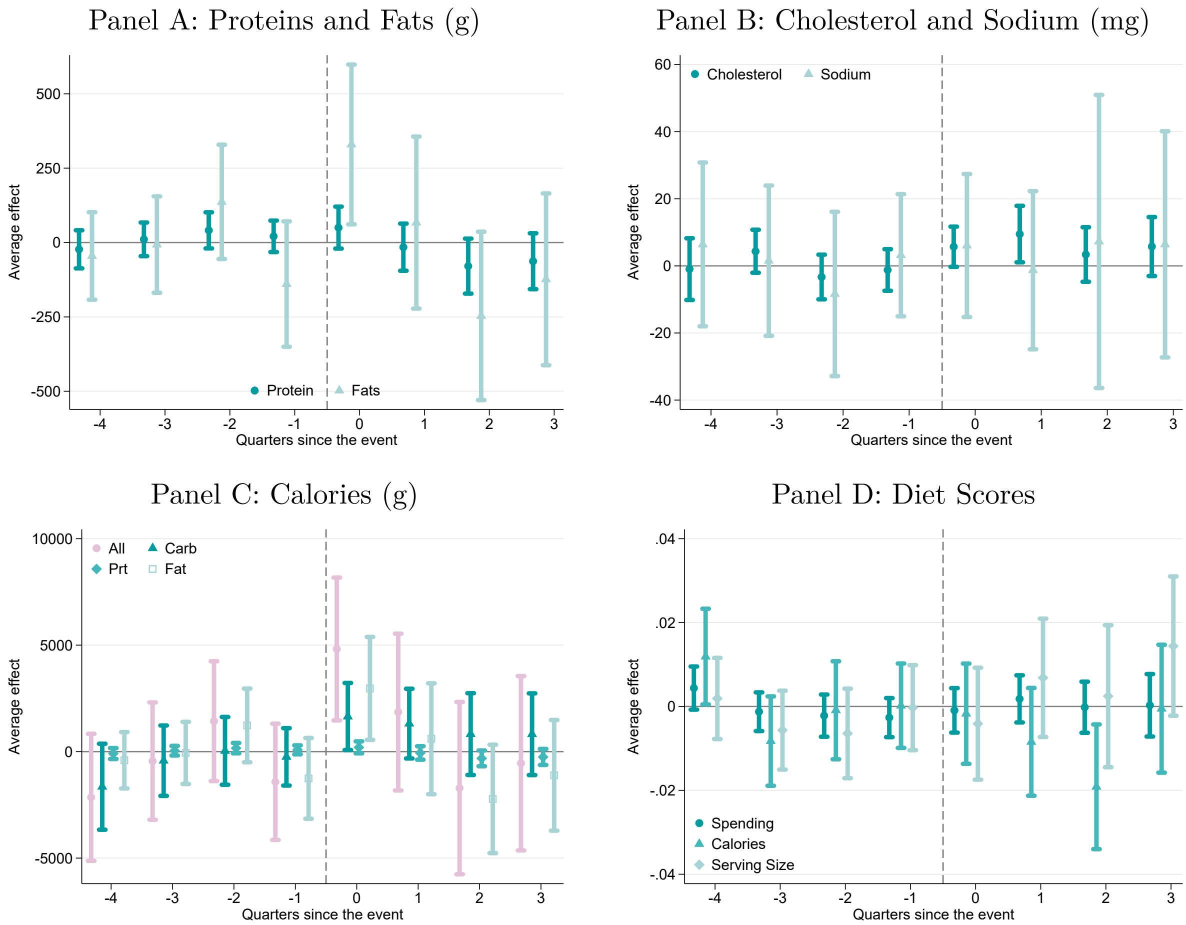
However, sugar consumption is more problematic, as it can lead to sugar spikes, which require additional consumption of insulin. Moreover, repeated blood sugar spikes can lead to both short-term adverse health effects and long-term development of comorbidities. Sugar consumption is found to be associated with an increased risk of developing heart disease together with diabetes (Holman et al., 2008; Malik et al., 2010). The light green triangles Panel B in Figure 3 show that there is a statistically significant increase in sugar purchases in the post period. The average effect in the post-period corresponds to an increase of 132.8 grams ($\sim$ 9.3%), as shown in Table A5 in the Appendix. The effect, therefore, is sustained over time. Moreover, the sugar results suggest that households substitute towards products with a higher sugar share, given that carbohydrate purchases revert to levels similar to the pre-period.

5.2 What is the Role of Ex-Ante Moral Hazard?

To further understand the role ex-ante moral hazard plays, I first study additional nutritional outcomes that are correlated with diabetes management and comorbidities. Next, I evaluate the overall healthiness of the diet by constructing a diet score. Finally, I investigate whether the responses across groups are heterogeneous.

Other Nutrients, Calories and Diet Healthiness Alongside carbohydrates, the other two macronutrients necessary for a balanced diet are proteins and fats. While carbohydrates are tightly linked to insulin consumption, other nutrients are mainly associated with comorbidities, such as cardiovascular diseases, that are common for individuals with diabetes. Therefore, if the role of ex-ante moral hazard is large, it might also impact other nutritional outcomes. Panel A of Figure 4 plots the coefficients for proteins (dark green) and fats (light green) purchased. Protein purchases exhibit a 50.1 gram ($\sim 2.8\%$) increase in the first quarter after the policy is implemented. However, these results are not statistically significant. An increase in protein consumption could be interpreted as a positive outcome.[50]

Figure 4: Other Nutrients, Calories, and Diet Score



Notes: The figure reports the event study estimates and the 95 percent confidence intervals using the Callaway and Sant'Anna (2021) estimator. The y-axis in Panel A is measured in grams (g), while the y-axis in Panel B is in milligrams (mg). The source of the data is in the NielsenIQ Panel data. The observation is at the household-quarter level.

[50]Note that while protein consumption is encouraged, high-protein diets are not found to be a better disease management method than other diets (Larsen et al., 2011).

The light green triangle reports the coefficients for grams of fat, showing a significant increase of 329 grams ($\sim 9.9\%$) in the first quarter of the post-period. As for carbohydrates, the quality of fats is also crucial (Ley et al., 2014). In particular, saturated fats should be avoided. In different meta-analyses, a reduction of saturated fats in the diet was associated with an improvement in glucose management and a decrease in cardiovascular risks (Imamura et al., 2016; Schwab et al., 2021). Figure A4 in the Appendix shows that there is no change in the purchase of saturated fats. This suggests that households with diabetes follow doctors' guidelines on this aspect. Later in this section, I also analyze the effect on total calories, as a fat increase can also lead to a higher caloric intake, which in turn can lead to weight gain. As discussed in Section 2, weight management is vital for type II diabetes.

I also investigate the policy's effect on essential micronutrients, which can provide additional information on households' responses to the policy. Cholesterol can cause cardiovascular diseases, and sodium can lead to hypertension. Moreover, the vast majority (more than 70%) of sodium intake comes from processed and packaged foods (Quader, 2017). Therefore, an increase in these outcomes could also signal that individuals are buying more processed food. I plot the coefficients for cholesterol (dark green) and sodium (light green) in Panel B of Figure 4. While I find no significant changes in sodium purchases, I observe a 7.6 mg increase in cholesterol over the first two quarters.[51] In percentage terms, this is quite a large increase, corresponding to a 38% rise. However, the effect is also temporary and returns to levels similar to the pre-policy purchase.

Finally, I study the effect of the policy on calories purchased. Calories can be a comprehensive measure, as they summarize in one value the effect on the different macronutrients considered so far. Indeed, calories are a weighted sum of the three macronutrients: 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 changes in the caloric intake. Moreover, excessive calorie consumption can lead to weight gain, and weight management is critical for managing diabetes. Weight loss can improve glycemic control and, accompanied by strong calorie restriction, can even reverse the progression of type II diabetes (Wilding, 2014). It is, therefore, relevant to consider whether there are changes in caloric intake.

In Panel C of Figure 4, I plot the coefficients both for the overall calories (pink circle) and the calories from the different nutrients, i.e., carbohydrates, proteins, and fats. The overall calorie coefficients follow a pattern similar to fats, given the higher weight that fat exerts on calories. Calorie purchases increase by 6.8% in the first quarter, exhibiting only a temporary effect.

Analyzing nutrients provides an objective way to investigate the effect of the policy on diet. However, it does not provide a comprehensive measure of the overall healthiness of the households' diet. To do so, I employ diet score measures to investigate this aspect. Panel D of Figure 4 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 provide somewhat mixed results, depending on the definition used. First, the spending diet score shows no changes after the policy implementation. It appears to be the most precisely estimated, as its coefficient is close

[51] It is significant at the 10% level in the first quarter.

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 using the share of serving size shows a slight upward trend in $t + 2$ and $t + 3$. Overall, given the mixed results, it seems there are no substantial or permanent changes in the overall healthiness of the diet. Moreover, the diet score analysis appears to mask important behavioral responses, which are evident in the nutritional content analysis.

Heterogeneity in Diet Responses To obtain a deeper understanding of the mechanisms driving the effect, I investigate whether there are heterogeneous effects across households' characteristics. I consider six characteristics by dividing the sample based on whether the variable takes a value below ("low") or above ("high") the median. For the last variable, I consider a binary variable, taking "no" or "yes" values.[52]

In Figure 5, I report the average coefficient from the first quarter for the event study for carbohydrates. Additionally, in Figure A5 and A6, I report the coefficients for the other macro- and micronutrients considered, as well as calories. All groups increase their carbohydrate purchase, although not for all the coefficient is statistically significant. Overall, when looking at each outcome in isolation, I do not observe statistically significant differences across groups. However, there are differences in response to the policy when considering the various characteristics and additional nutrients. Higher-educated and higher-income households mostly improved their diets after the policy implementation. Lower-income households, however, although they partially follow doctors' recommendations, also purchase more unhealthy products. In particular, households with no comorbidities appear to worsen their purchases the most, as their changes in purchases negatively affect the diet score.

In the first three rows of Figure 5, I consider demographic characteristics, namely age, education, and income. First, I find that households led by younger heads exhibit a statistically significant coefficient for carbohydrate purchases, while those with older heads do not. However, older households appear to buy slightly more fat. These effects result in higher calorie purchases for both groups. The overall diet healthiness is also unchanged.

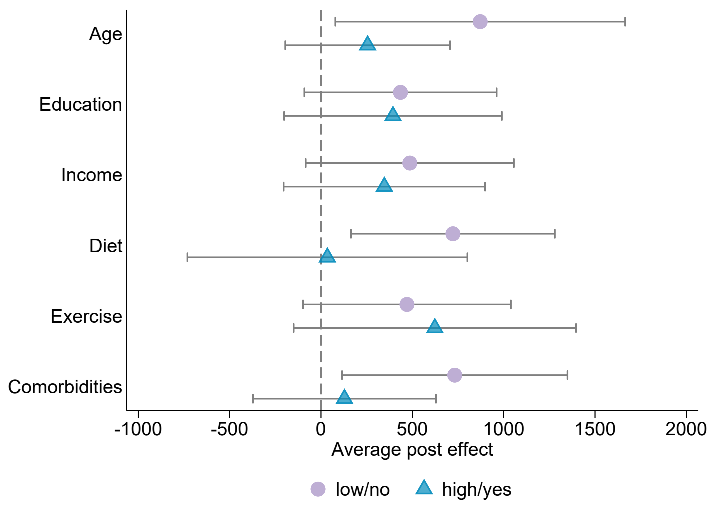
The differences in education status and income are more pronounced and offer important insights for mechanisms and policy evaluations. For higher-educated households, I observe an increase in "good" nutrients—i.e., fibers—but not in "bad" ones—i.e., fats and calories. For lower-educated households, instead, I observe an increase in the purchase of sodium, which is usually found in processed and packaged foods (Quader, 2017). Higher-educated households' response may be due to a better understanding of the link between insulin and carbohydrates and better disease management.

Both income groups increase their carbohydrate purchases. However, lower-income households also increase significantly the purchase of fats, leading to a slight decrease in the diet score. Con-

[52]For 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.

versely, higher-income households do not increase the purchase of any other nutrients, leading to no changes in calories and even a slight improvement in the diet score. The differences are likely due to the different food choices, given different income availability.

Figure 5: Carbohydrates - 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.

In addition, I investigate heterogeneity across health and health consciousness dimensions, provided in the Ailment Survey.[53] First, I consider how important it is for the household to follow a low-sugar diet.[54] Second, I use the information on exercise frequency and take the median. Households exercising up to three times per week are below the median.[55] Third, I construct a dummy indicating whether the household has other comorbidities related to diabetes and/or diet, such as hypertension, heart disease, and high cholesterol.

Households assigning below-median importance to low-sugar diets increased their carbohydrate and fiber purchases more than those above the median. They do not significantly increase sugar consumption after the policy. Hence, they also appear to be aware of the doctors' recommendations concerning sugar. However, their dietary choices are not entirely consistent with the recommendations, as below-median households also increase fat purchases but not protein purchases.

Similar patterns are observed when looking at exercise frequency. In particular, households with below-median exercise frequency purchase more carbohydrates and fibers but also more fats and calories. Unlike the other groups, they also increase the purchase of saturated fat and cholesterol. Households that exercise less frequently, similar to those that give less importance to diet, might

[53]Given the source of these data, the sample is different from the other analysis, as I can only use households that respond to both surveys. In Section 5.5, I show that the results on this sample are similar to the baseline.

[54]Values are on a Likert scale from 1 "Not important at all" to 5 "Very important."

[55]Note that there are eight possible answer options, from "never" to "every day." Physical activities are broadly defined and include different types of movement. Examples provided for the answer are yoga, walking, and gardening.

rely less on diet and more on insulin for diabetes management.

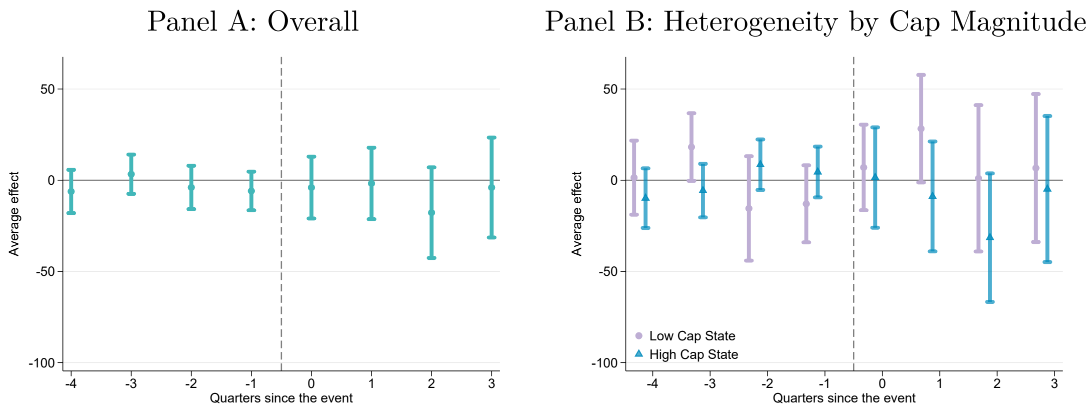
I observe that those affected by comorbidities do not increase their carbohydrate consumption. However, they increase the amount of fat and calorie purchases. Nevertheless, households with comorbidities do not increase cholesterol purchases, which could have a negative effect. Households without comorbidities do so and also purchase more carbohydrates and calories. For them, there is weak evidence of ex-ante moral hazard, as all measures of the diet score decrease.

5.3 Additional Results

In this subsection, I investigate whether the policy leads to a positive income effect, changes in disease management and medication adherence, and health effects. These outcomes also provide further understanding of the effect of reducing prescription drugs.

Income Effect A reduction in out-of-pocket costs could lead to a positive income effect for treated households. The increase in income could be channeled into grocery purchases, which would provide an alternative explanation for the results on nutrients observed in the previous subsection.

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.

To study this alternative explanation, I construct an outcome accounting for the overall household's spending (per person) on groceries. As reported in Panel A of Figure 6, I find no statistically significant differences in spending on groceries after the policy implementation. This result suggests that an income effect does not seem to be at play for the overall sample. Especially for income effects, differences in the magnitude of the caps might be relevant. Panel B reports the coefficients for the states with caps below the median (purple circle) and above the median (blue triangle). The coefficients are higher for lower cap states, and there is a small and statistically significant (at the 10% significance level) increase in spending in one quarter after the policy was implemented.

However, these differences are minor and do not suggest the presence of strong income effects that are different across states.

In Figure A6 in the Appendix, I turn to household characteristics and plot the average post-period coefficient over the four quarters after the policy implementations. I observe no differences in spending on groceries in the different groups, not even by income, considering below- and above-median income levels. The only group that exhibits some income effect are those that engage less in physical activities. They spend an additional $60 per quarter on groceries.

Adherence and Testing I study the effect of the insulin policy on the sales of diabetes devices. These devices are *not* affected by the policy as they are not included in the monthly cap.[56] These outcomes are relevant *per se* as they provide additional information to evaluate the effects of a reduction in prescription drugs. Due to data limitations discussed in Section 3.2, I do not use the household-level data, but rather store-month-level data. Figure 7 reports the results on the inverse hyperbolic sine transformation of units sold for the considered devices. It reports the effect twelve months before and after the policy implementation. For all outcomes, the parallel trend assumption is satisfied as the pre-trends are flat. Furthermore, unlike the nutritional outcomes, the effect of all these outcomes is sustained over time, suggesting a permanent effect.

To study medication adherence, an outcome of interest would be insulin consumption, which I do not observe as I do not observe any device or medication for which a prescription is needed. I can study, however, insulin syringe sales, which are a complement of insulin. An increase in the latter plausibly indicates that the consumption of insulin has increased. If anything, using insulin syringe sales as a proxy for insulin sales might cause a downward bias in my estimates. This could be the case if individuals increase their insulin dose by using more insulin in a syringe. Panel A shows that, in treated states, the number of insulin syringes purchased increases after the policy. While an increase is evident from the beginning of the post-period, the effects are statistically significant from the fourth month after the policy implementation. An explanation for the delayed detected effect can be that there is an incremental reaction in insulin consumption. For example, if, in the first few months, individuals are only minimally adjusting their insulin consumption upward, this would not show up in increased insulin sales.

Additionally, I investigate the effect of the prescription drug's out-of-pocket reduction on the management of the disease. To do so, I start by looking at glucose monitors, which are devices with a small display that are used to monitor glucose levels. They show the glucose levels and sometimes provide some additional information through color-coded signs, such as whether the value is low or high for the specific individual.[57] Glucose testing strips, which are inserted into a glucose monitor with a drop of blood, are also important devices for diabetes management.

[56] For Connecticut, some of these devices are also included in the $100 cap. In Section 5.5, I show that the results are robust to its exclusion.

[57] In this data, I do not observe continuous glucose monitors, as a prescription is needed to buy them. However, they are not yet widely used, as estimates report only 13% of individuals with type II diabetes employing them (Mayberry et al., 2023). Continuous glucose monitors are modern 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.

Panel B shows no change in glucose monitor sales, while Panel C shows a jump in testing strips already in the first period of the policy implementation. Given that glucose monitors are durable—they can last up to five years—individuals might not buy them very often. On the other hand, glucose testing strips are disposable and can be used only once. These results suggest that individuals increase the frequency of their testing.

One concern might be that sales changes in diabetes devices are driven by price changes for diabetes supplies rather than being due to the policy. Therefore, in Figure A8 in the Appendix, I report the event study price estimates for each of the products considered in Figure 7. Prices do not appear to be a driver of the changes in purchases as there are no statistically significant changes in treated states after the policy, with the exception of glucose testing strips. For the latter, there is a decrease in prices starting four months after the policy. However, the increase in testing strips is also evident from the first period and even larger starting from the second quarter after the policy implementation. Another possible concern is that for these analyses, I use the overall sales, not only the sales to those affected by the policy. However, this is not a major concern as the products considered are specific to diabetes and are not used for other diseases.

Finally, caps of different magnitudes might have different effects. To examine the heterogeneous effects of the insulin out-of-pocket cap, I split the sample into states with above-median and below-median caps. For these two groups, Figure A9 in the Appendix reports the average treatment effect in the post-period. Overall, although the difference is not statistically significant, I find that the policy's impact is more pronounced in states with lower caps, where the cost reduction for insulin is more substantial and where more individuals are affected.

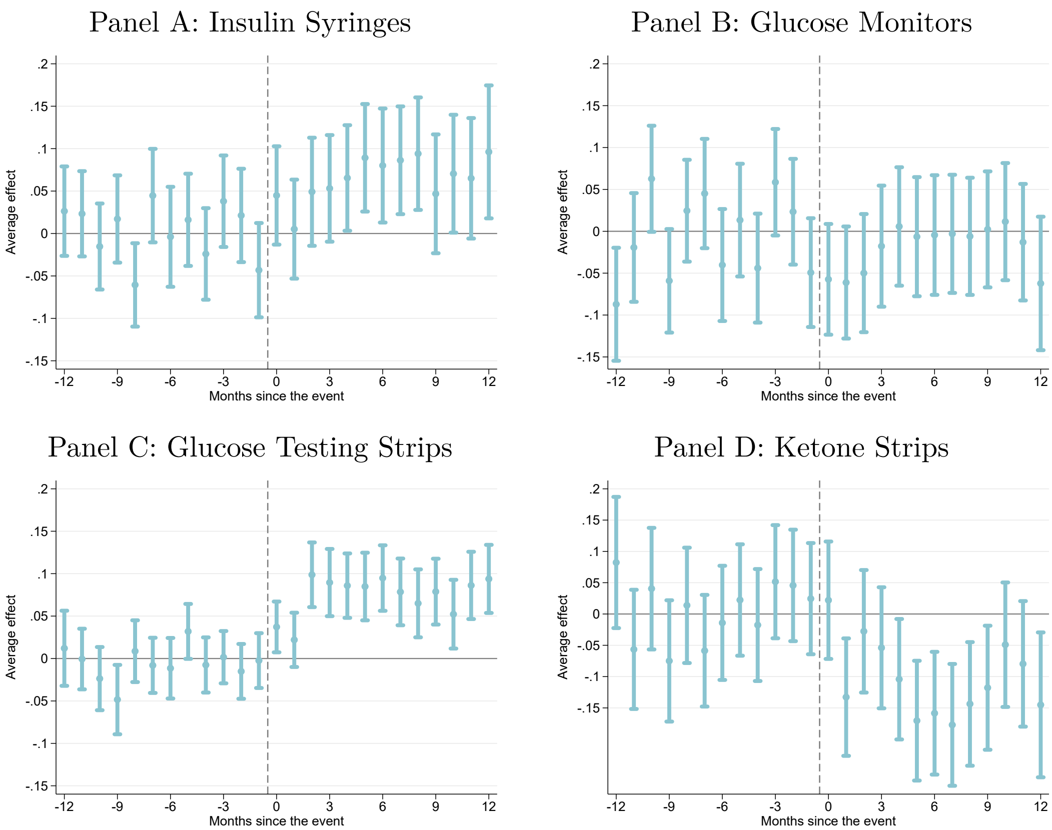
The magnitude of the cap matters for the size of the effect, as the increase in insulin syringe purchases is larger in the low-cap states than in high-cap ones. In particular, individuals in states with lower caps are more sensitive to the out-of-pocket cost reduction and adjust their adherence and testing behavior. As in the overall analysis, there is no increase in glucose monitor sales. For glucose testing strips, the rise in purchases is larger in low-cap states. Patients in low-cap states seem to test more often than in high-cap states.

5.4 Health Effects

A relevant question is the health effects of the policy. One way to measure this is to study whether there is a decrease in the number of adverse health effects for individuals with diabetes. I can proxy for this in the data by examining ketone strip sales, which are used to monitor the level of ketones in the blood or urine. Ketones are produced when the body burns fat for energy instead of carbohydrates, which can occur if insulin levels are insufficient, as an insufficient amount of carbohydrates reaches the cell. For individuals with diabetes, elevated ketone levels, therefore, indicate a lack of insulin and an increased risk of diabetic ketoacidosis, which is a severe and acute complication of diabetes, which can potentially be deadly (Misra and Oliver, 2015).[58] Guidelines encourage people with diabetes to use ketone strips with high blood glucose when nauseated and/or

[58] Although traditionally associated with type I diabetes, diabetes ketoacidosis has been found to become increasingly common also for type II diabetes (Puttanna and Padinjakara, 2014).

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.

vomiting and when fever is detected (Albanese-O'Neill et al., 2017).

In Panel D of Figure 7, I plot the sales of ketone strips. In the post-period, I observe a decreasing trend, which becomes significant four months into the post-period. The effect is stable and quite persistent over time. The decrease in sales suggests that individuals are testing less frequently, likely as a result of increased insulin access and improved health status. The lagged effect can be due to individuals having to first adjust to the new insulin consumption before learning that they do not need ketone testing as much as before. Figure A9 shows that the effects are stronger in low-cap states. Finally, Panel D of Figure A8 in the Appendix reports a decreasing trend for prices. If the price were driving the results, a lower price should increase the demand for strips. Therefore, I can exclude that a change in price drives the sales effect.

5.5 Robustness Checks

In this subsection, I test and show that my analysis is robust to different specifications. I provide three sets of robustness checks, namely on sample section, methodology, and falsification tests.

First of all, I consider different sample selections. I start by addressing a limitation of the NCP data, namely that I cannot observe which household member consumes specific foods or the quantity consumed. I only observe what is purchased at the household level. By focusing on household-level consumption, I may underestimate the effect if the household member with diabetes changes their eating habits after the policy implementation, while overall household purchasing patterns remain unchanged. To address such concerns, I restrict the analysis to single-person households. I report the results for carbohydrates in Panel A of Figure A10 in the Appendix, while Figure A11 in the Appendix reports the plots for fibers and sugar. The coefficients for carbohydrates for single households only (dark green circles) are larger than in the baseline results (gray triangles). The figures highlight that, indeed, the analysis at the household level underestimates the results. However, when considering only single households, I lose a large share of the sample and, therefore, I have larger confidence intervals.

Additionally, I show that the results are robust when considering only the individuals in the Ailment Survey. For the panelists who do not participate in the survey, I might incorrectly predict diabetes and insurance status. I report the results in Panel B of Figure A10 in the Appendix and show that there are no discernible 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). I also report the results for fibers and sugar in Figure A12 in the Appendix.

Furthermore, I address the concern that COVID-19 might confound the reported effects.[59] One might be concerned that treated states are different from control states in that treated states might react differently to COVID-19 than untreated states. I indeed observe that states in the control group were less likely to implement stay-at-home policies. Whitman et al. (2024) find that purchasing behaviors changed in states with stay-at-home policies compared to those that did not. Therefore, as a robustness check, I exclude the states without stay-at-home policies.[60] In Panel A of Figure A13 in the Appendix, I show that the results are robust to this exclusion. The pandemic also caused temporary layoffs and stimulus checks that were implemented between 2020 and 2021.[61] To account for this, 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 A13 in the Appendix. Similar results are obtained for fibers and sugar, as reported in Figure A14 and A15 in the Appendix, respectively.

I also show that the analysis is robust to the selection of included states. In Panel C of Figure A13, 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 Figure A16, and A17 in the Appendix, respectively.

Second, I show that my results are robust to alternative difference-in-difference specifications. In particular, in Figure A18, I use Sun and Abraham (2021)'s approach instead of Callaway

[59] O'Connell et al. (2022) find no changes in food quality due to the pandemic in the U.K.
[60] I exclude Utah, Wyoming, North Dakota, South Dakota, Nebraska, Oklahoma, Arkansas, and Iowa.
[61] I collect and report the timing of these policies and shocks in Table A4 in the Appendix.

and Sant'Anna (2021)'s, my preferred specification. The estimators are similar in accounting for differential timing in treatment. There are two main differences, which, however, should be marginally relevant in my context. One is that—in the case that all units are treated—the former uses only the last cohort as the treated group instead of the not-yet-treated. However, as there are states that are never treated, I do not use this difference in the estimators. A second difference is that Callaway and Sant'Anna (2021) can be used if the 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. I find robust and similar results. This is shown in Panel A of Figure A18 in the Appendix, which replicates the carbohydrate purchase results from Equation 1 reported in Figure 3 and shows similar patterns. In terms of magnitude, the two methods show slightly different coefficients. This difference is, however, not statistically significant as the confidence intervals overlap. I also report the results for fibers and sugar in Figure A18 in the Appendix.

Third, I also run a series of placebos to show that the results are explicitly driven by the policy and not by other confounding factors. I exploit the characteristics of the policy to perform the first placebo test. I run the same analysis on households with diabetes and Medicare, given that individuals with Medicare are not affected by the policy. I report the results in Panel A of Figure A19 in the Appendix, which show no effect on households with Medicare. Second, in Panel B, I randomly assign a different treatment date to treated states. In Panel C, I randomly assign the control states to one of the cohorts, and I employ the treated states as controls. Both leads and lags plots indicate no significant post-treatment effect. For all three placebos, I also report the falsification tests for fibers and sugar in Figure A20 in the Appendix.

6 Conclusion

This paper studies the role of ex-ante moral hazard in health insurance at the intensive margin through behavioral health choices, an aspect mostly overlooked in the existing literature, which focuses on insurance coverage, i.e., the extensive margin. Due to ex-ante moral hazard, individuals might engage more in risky behaviors when covered by insurance or when there are changes in out-of-pocket costs. In turn, ex-ante moral hazard might lead to preventable health issues or higher healthcare spending. Therefore, this paper contributes to the debate regarding cost-effective health policies to ensure the financial sustainability of healthcare systems.

I examine the effects of a reduction in insulin out-of-pocket costs on dietary behaviors and healthcare utilization. To provide causal evidence, I exploit a plausibly exogenous variation in the out-of-pocket cost. I combine different data sources to obtain a household-level dataset with information on risky behaviors, health, and insurance status. In doing so, I investigate the role of ex-ante moral hazard in a comprehensive way, investigating food purchases, their nutritional content, and the sales of diabetes supplies.

The empirical analysis shows that the policy leads to an increase in risky behaviors. These effects are mostly temporary, with the exception of sugar purchases and diabetes supplies, whose effects appear sustained over time. The results show that this increase is due to ex-ante moral hazard, and I rule out alternative explanations for ex-ante moral hazard. Among others, I do not find evidence of an income effect playing a role, or at least not through increased grocery spending. Nevertheless, I find a decrease in adverse health effects, as shown by the decrease in ketone strip sales. Heterogeneity analyses across household types suggest that certain groups, e.g., those less physically active, might react more strongly to the policy and increase their risky behaviors. These are relevant insights for policymakers considering the implementation of similar policies, especially as new diabetes drugs enter the market. Additionally, these results provide evidence for other chronic diseases that rely on a combination of lifestyle changes and medication.

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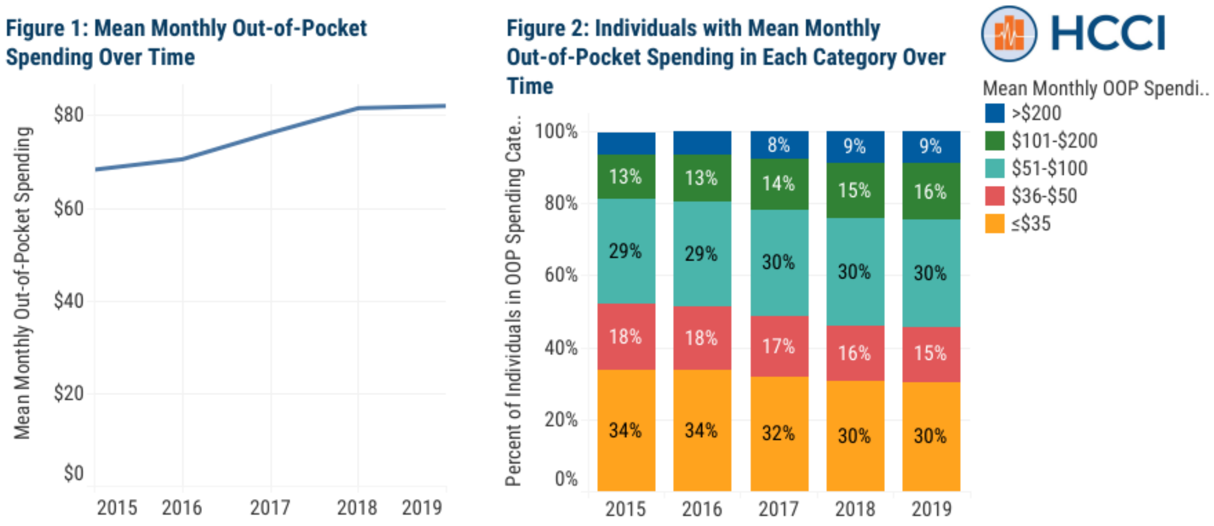
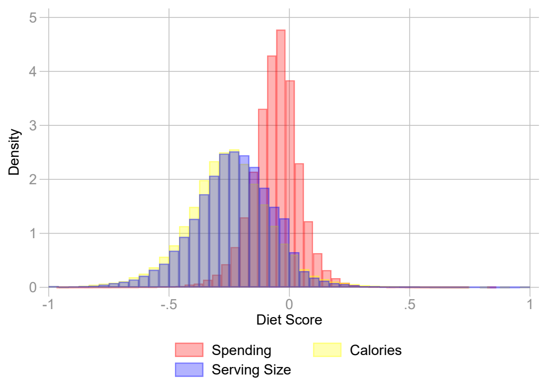
A Appendix: Additional Figures and Tables

Figure A1: Out-of-pocket cost over the years



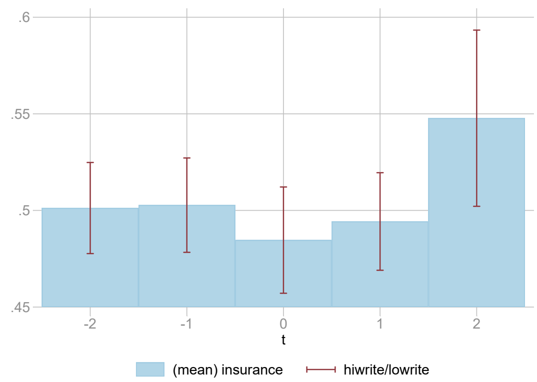
Source: HCCI report, see Health Care Cost Institute (2024).

Figure A2: Diet Score Density



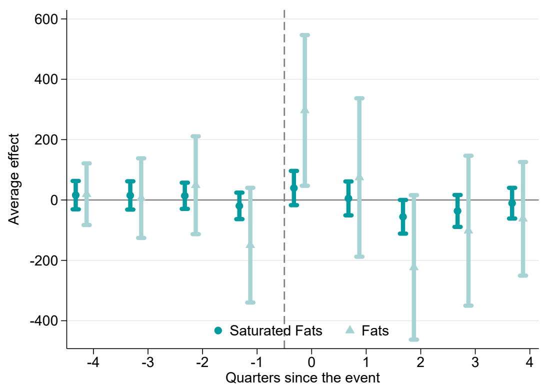
Notes: The figure reports the average post-period event study estimates and the 95 percent confidence intervals using the Callaway and Sant'Anna (2021) estimator. The observation is at the household-quarter level. Source of the data is the NielsenIQ Panel.

Figure A3: Employer-sponsored Insurance Share over the Years



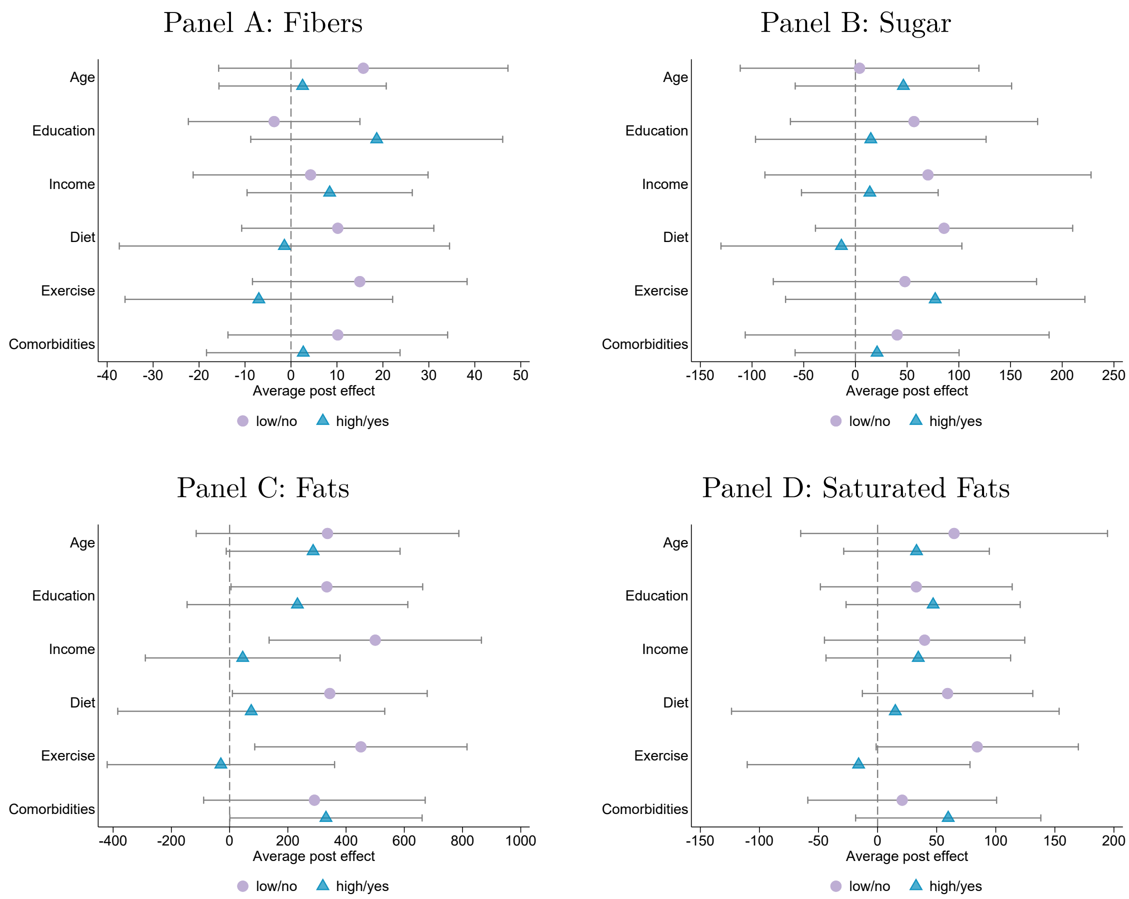
Notes: Own calculation on 2018–2022 KFF insurance count data at the state-year level. This histogram reports the share of individuals having an employer-sponsored insurance (a type of private insurance). The states are stacked around the year zero, i.e., the year in which they implemented the insulin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

Figure A4: Effect by Fat Types, Purchased Grams



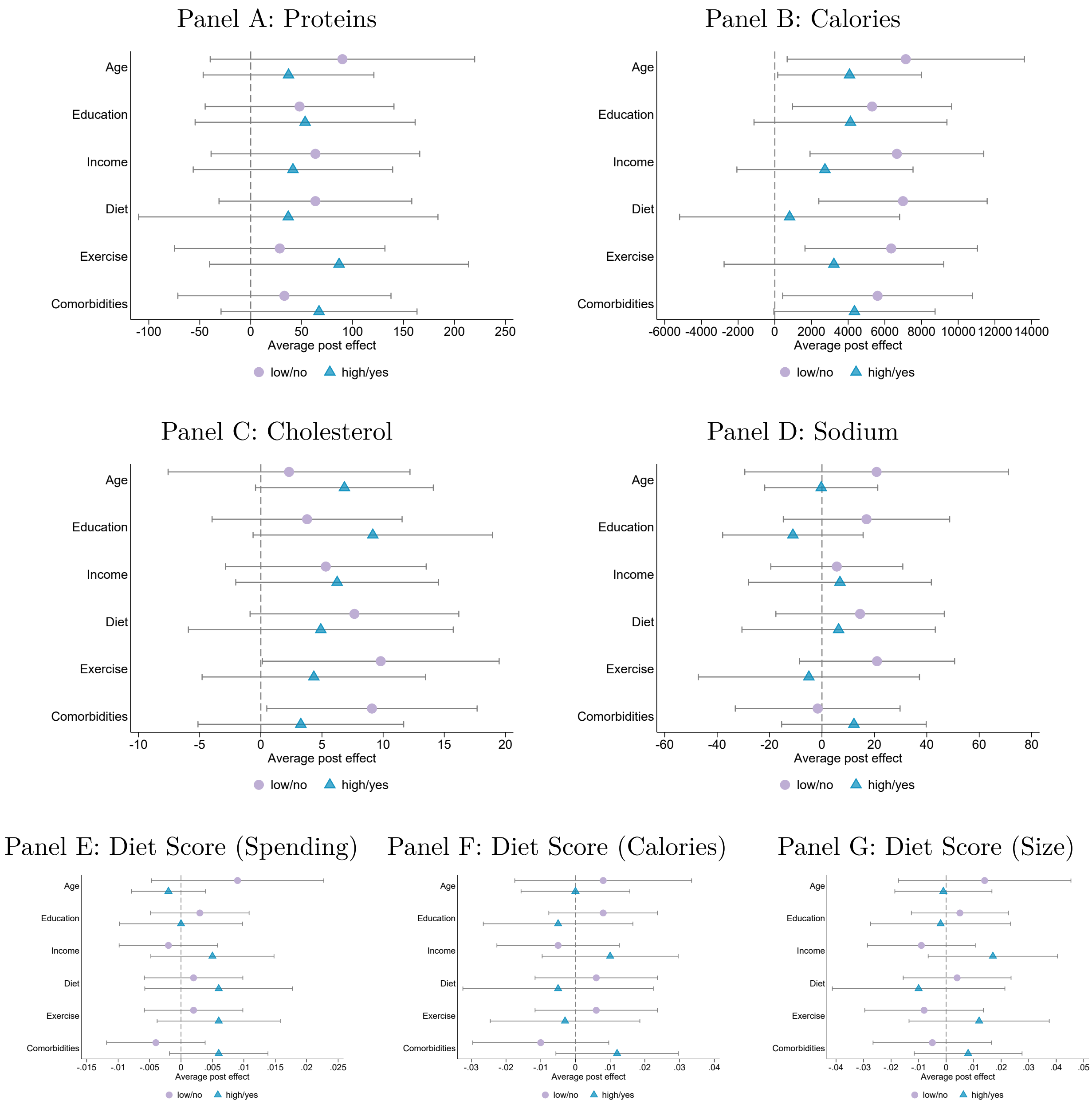
Notes: The figure reports the event study estimates and the 95 percent confidence intervals using the Callaway and Sant'Anna (2021) estimator. Source of the data is the NielsenIQ Panel. The observation is at the household-quarter level.

Figure A5: Nutrients - Heterogeneity



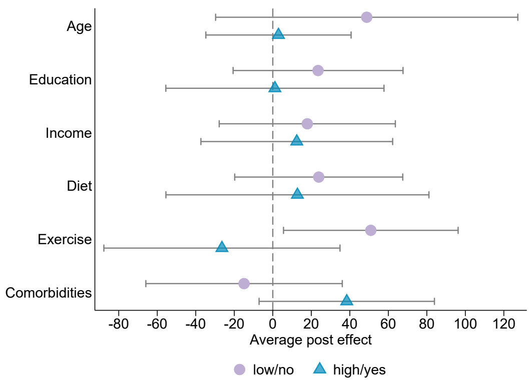
Notes: The figure reports the average post-period event study estimates and the 95 percent confidence intervals using the Callaway and Sant'Anna (2021) estimator. The observation is at the household-quarter level. Source of the data is the NielsenIQ Panel.

Figure A6: Nutrients - Heterogeneity (II)



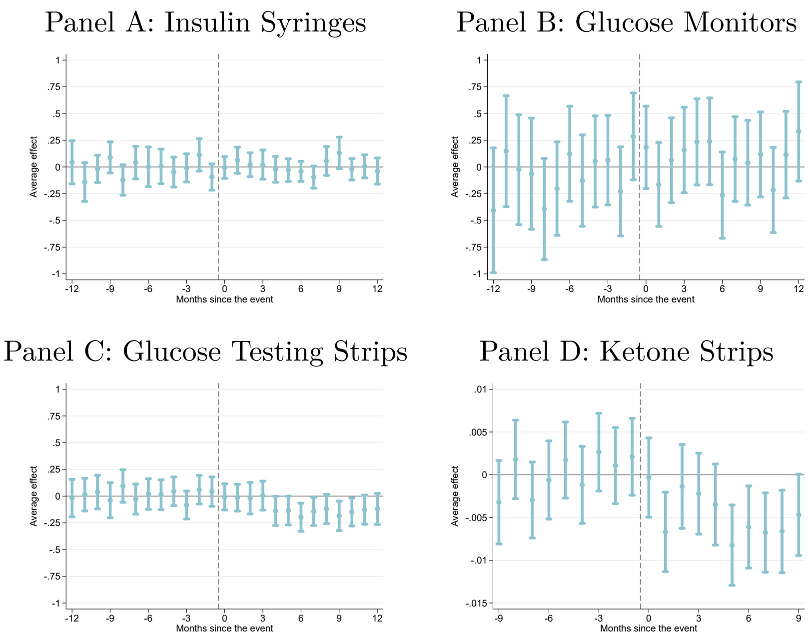
Notes: The figure reports the average post-period event study estimates and the 95 percent confidence intervals using the Callaway and Sant'Anna (2021) estimator. The observation is at the household-quarter level. Source of the data is the NielsenIQ Panel.

Figure A7: Income - Heterogeneity



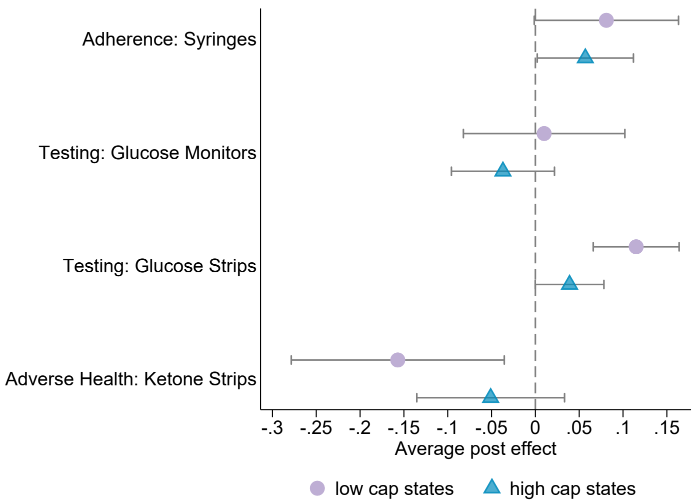
Notes: The figure reports the average post-period event study estimates and the 95 percent confidence intervals using the Callaway and Sant'Anna (2021) estimator. The observation is at the household-quarter level. Source of the data is the NielsenIQ Panel.

Figure A8: Unit Prices of Diabetes Devices



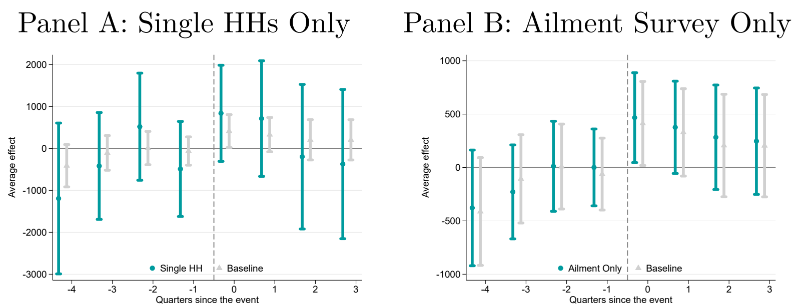
Notes: The figure reports the event study estimates and the 95 percent confidence bands using the Callaway and Sant'Anna (2021) estimator. The outcome variables are unit prices. The observation is at the store-month level (drug stores only). Source of the data in the NielsenIQ retail-level scanner data.

Figure A9: 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 household-quarter level. Source of the data is the NielsenIQ Panel.

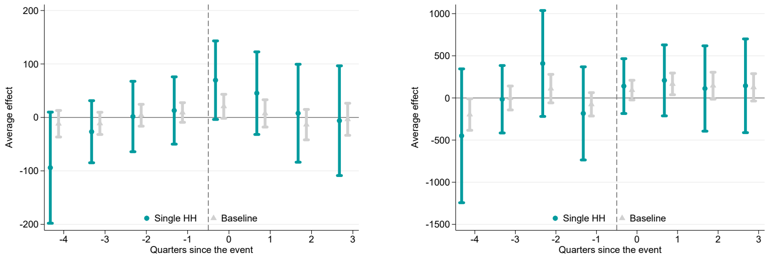
Figure A10: Carbohydrates - Household Selection Robustness Checks



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 A11: Fibers and Sugar - Single HHs Only

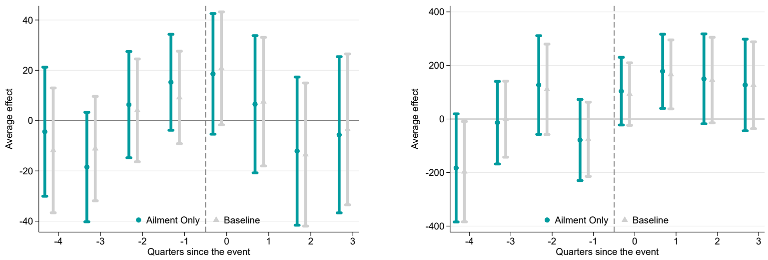
Panel A: Fibers Panel B: Sugar



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 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 replicate Panel B of Figure 3 using households with only one member.

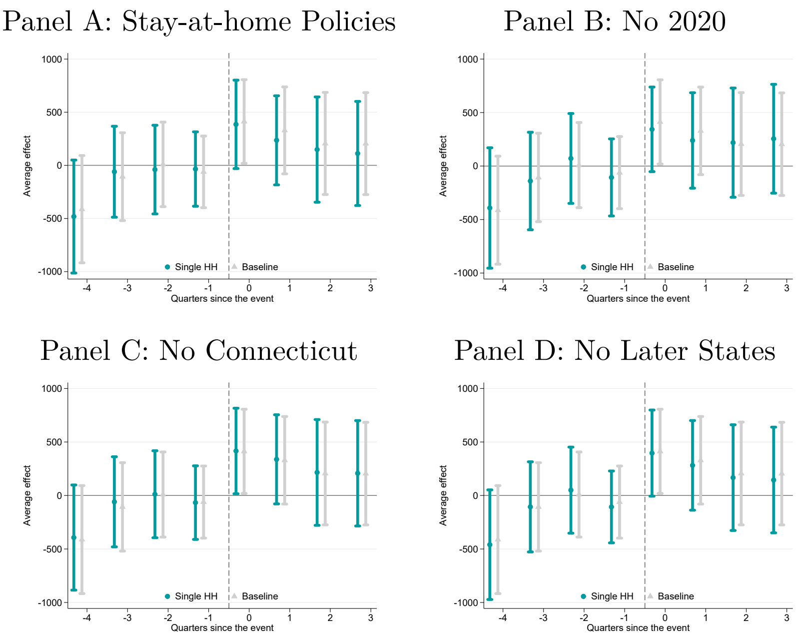
Figure A12: Fibers and Sugar - Ailment Survey Only

Panel A: Fibers Panel B: Sugar



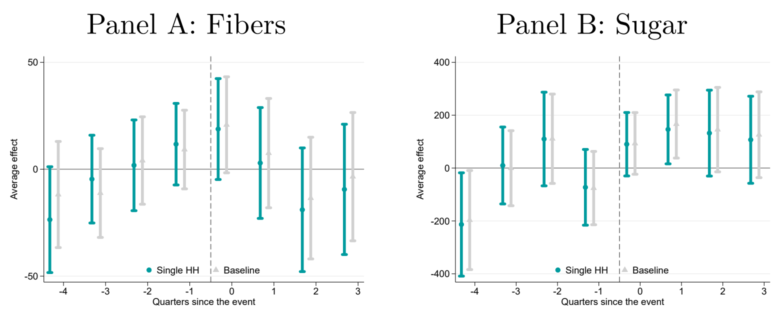
Notes: The figure reports the event study estimates and the 95 percent confidence intervals using the Callaway and Sant'Anna (2021) estimator. The outcome variables the grams purchased on the different nutrients by the household. The observation is at the household-quarter level. Source of the data is the NielsenIQ Panel. I replicate Panel B of Figure 3 using households with only households that responded to the Ailment Survey.

Figure A13: Carbohydrates – COVID-19 and State Selection 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.

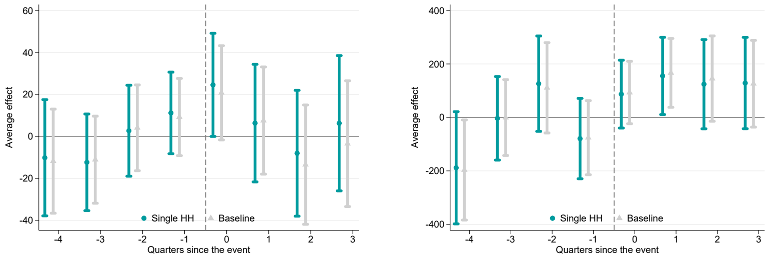
Figure A14: Fibers and Sugar – Stay-at-home Policies



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 A15: Fibers and Sugar - No 2020

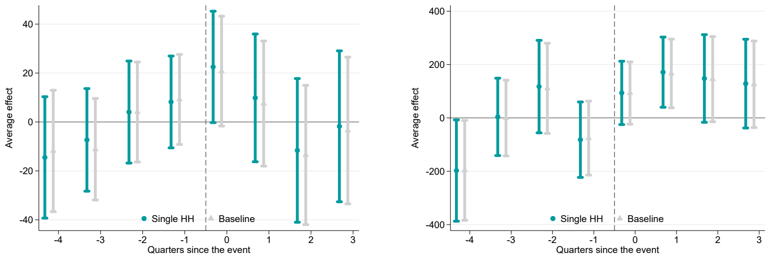
Panel A: Fibers Panel B: Sugar



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 A16: Fibers and Sugar - No Connecticut

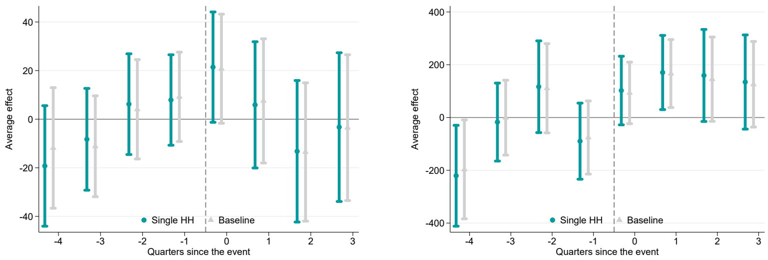
Panel A: Fibers Panel B: Sugar



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.

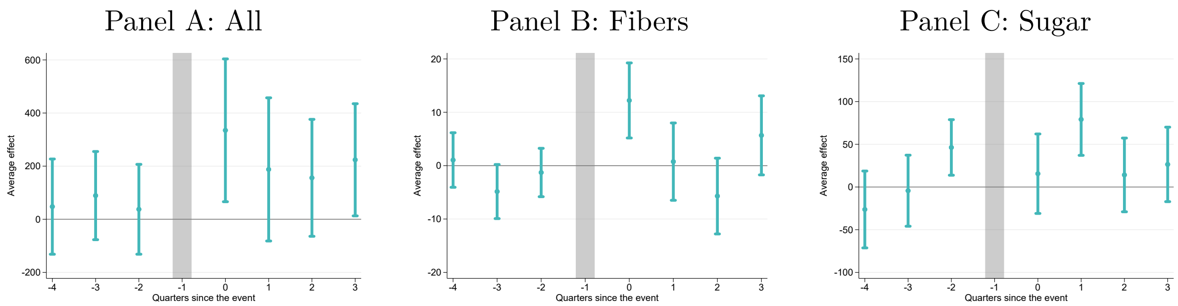
Figure A17: Fibers and Sugar - No Later States

Panel A: Fibers Panel B: Sugar



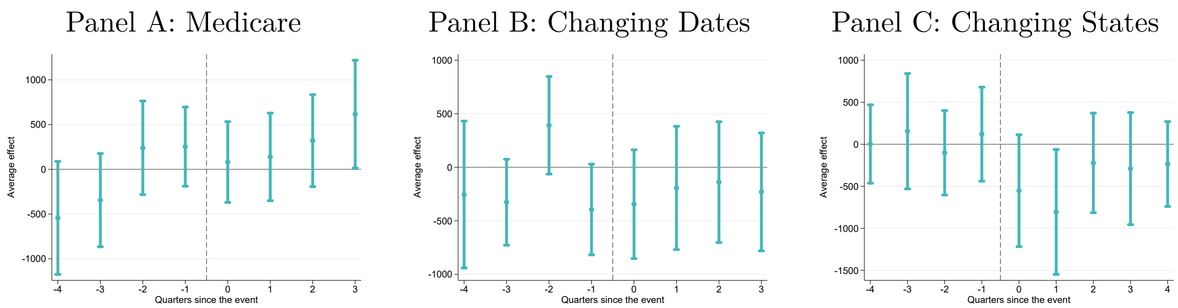
Notes: The figure reports the event study estimates and the 95 percent confidence intervals using the Callaway and Sant'Anna (2021) estimator. The outcome variables are the grams of carbohydrates per person per quarter. The observation is at the household-quarter level. The source of the data is in the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

Figure A18: Fibers and Sugar - Sun and Abraham (2021)



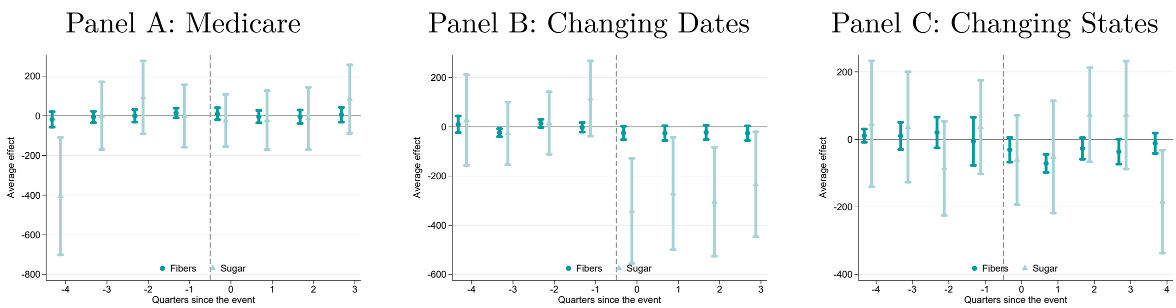
Notes: The figure reports the event study estimates and the 95 percent confidence intervals using the Sun and Abraham (2021) estimator. The outcome variables the grams purchased on the different nutrients by the household. The observation is at the household-quarter level. Source of the data is the NielsenIQ Panel.

Figure A19: Carbohydrates - Placebos



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 tests. Panel A run the analysis on households with diabetes and Medicare. Panel B uses the data as in the baseline and changes the date in which states are treated, while Panel C uses the correct dates but changes the states that are treated.

Figure A20: Fibers and Sugar - Placebos



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 tests. Panel A run the analysis on households with diabetes and Medicare. Panel B uses the data as in the baseline and changes the date in which states are treated, while Panel C uses the correct dates but changes the states that are treated.

Table A1: State-Level out-of-pocket cost Legislations

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.

Table A2: 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".

Table A3: 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				
General Behavior				
Total Spent $/Quarter	1,057	900	1,256	1,089
Total Spent $/Quarter (per person)	533	441	617	525
Nutrients and Calories				
Carbohydrates	2,972	2,399	3,146	2,612
Fibers	194	141	205	153
Sugars	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
Diet Score	-0.06	-0.05	-0.06	-0.05
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 A4: 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 sates
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.

Table A5: Event Studies

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

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$

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 colums 1–6, and columsn 9–10 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 store-month level. Source of the data is the NielsenIQ Panel.

B Appendix: Diabetes and Insurance Status Prediction

In this appendix I provide additional details on the diabetes and insurance status prediction. 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 predict 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 prediction, 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 predictions, I estimate the model on 80% of the sample and predict 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 prediction, 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%.[62]

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 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 prediction in Table B1. I show that 67% of households with diabetes are correctly identified.

Table B1: Diabetes Status Prediction

		Diabetes		
		No	Yes	Total
Diabetes Prediction	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 prediction rule I perform. Observation is at the househol-year level. The tables has two rows—indicating whether the prediction rule identifies the household as having diabetes—and two columns—indicating whether the household actually has diabetes. It reports the number of true negative (top left), false negatives (top right), false positives (bottom left), and true positives (bottom right). The last columns and rows report the total number of observations per group.

Similarly, I predict 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

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

line, and an indicator for the presence of children below 18. Before predicting 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 predict the insurance status out-of-sample on the remaining 20%. The prediction results, reported in Table B2 show that 85% of households with private insurance are correctly identified.

Table B2: Private Insurance Prediction

		Insurance		
		No	Yes	Total
Insurance Prediction	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 prediction I perform. I estimate a fixed effect logit on 80% of the sample and test the prediction on the remaining 20%, which is reported here. The tables has main two rows—indicating whether the prediction rule identifies the household as having private insurance—and two main columns—indicating whether the household actually has private insurance. It reports the number of true negative (top left), false negatives (top right), false positives (bottom left), and true positives (bottom right). The last columns and rows report the total number of observations per group.

Finally, I show that respondents and non-respondents are comparable. By construction, I can verify the validity of the prediction only for the Ailment Survey respondents. However, given that I predict the diabetes and insurance status of non-respondents, one might be concerned that the prediction 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 prediction.

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 prediction 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 5.5, I check that the results are robust to the exclusion of the predicted observations.

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

	NCP Only		NCP & Ailment Survey		
	Mean	S.D.	Mean	S.D.	St. Diff.
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 predict diabetes (Panel A) and insurance status (Panel B), I report the mean, median, and standardized difference (Cohen's d) between the two groups. Differences below 0.25 are considered small (Imbens and Rubin, 2015).