Choosing coverage, using care: Disentangling direct and indirect effects of premium subsidies on healthcare demand

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Abstract

Health insurance premium subsidies are designed to alleviate the financial burden that mandatory premium payments exert on low-income individuals. We analyze the effects of newly receiving subsidies on health plan choice and healthcare demand in Switzerland's mandatory health insurance framework. Using rich administrative data on healthcare costs, health plan choice, and income, we combine a heterogeneity-robust difference-in-differences estimator with a control function approach that models deductible choice in the first stage and healthcare demand in the second stage. We document a two-fold effect of premium subsidies. First, consistent with prior evidence, insurance decisions are sensitive to premium changes: Subsidy claimants are more likely to select the lowest deductible once they begin receiving subsidies. Second, factoring out the indirect effect through the deductible channel, we find a direct positive effect of subsidies on healthcare costs at the intensive margin. However, our results do not indicate subsidy-induced moral hazard. Moreover, out-of-pocket costs decrease significantly, indicating that subsidies effectively enhance financial protection for low-income households. Our findings inform the behavioral and welfare consequences of income-targeted premium support and underscore how subsidy generosity and administrative design shape outcomes.

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Keywords: health plan choice; deductibles; premium subsidies; healthcare demand; out-of-pocket expenditures; difference-in-differences; control function approach.

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1 Introduction

Mandatory health insurance in regulated competition market structures aims to achieve universal healthcare access, with government interventions ensuring that health insurance remains affordable for all. In addition to risk adjustment for insurers, a critical element of this design involves subsidies for low-income individuals to support their premium payments. As premiums represent a significant expense for many households, subsidies are intended to reduce financial burdens and address equity concerns (Einav, Finkelstein, & Tebaldi, 2024; Schmid, Schreiner, & Stutzer, 2022). Amid rising healthcare costs and fiscal constraints, policymakers are increasingly reconsidering the opportunity to change the allocation of premium subsidies, both at the extensive (who is eligible) and intensive (how much support is provided) margins. A body of literature has documented the effects of premium subsidies on enrollment behavior, focusing mainly on the United States, a large but distinctive insurance market without a strict universal mandate (Finkelstein, Hendren, & Shepard, 2019; Frean, Gruber, & Sommers, 2017; Saltzman, 2019; Tebaldi, 2025). This literature has emphasized the extensive margin of enrollment responses, estimating how premium subsidies influence individuals' decisions to enter or exit the market (Finkelstein et al., 2019; Frean et al., 2017; Saltzman, 2019; Tebaldi, 2025). In contrast, in settings with a universal mandate, the central question shifts from market entry to health plan choice and subsequent healthcare demand, and the behavioral effects of subsidies along these margins remain less well understood. Examining the way in which subsidies and coverage levels affect healthcare demand is, therefore, critical for designing efficient and equitable support policies in mandatory insurance systems (Tebaldi, 2025).

In this paper, we examine the role of premium subsidies for eligible individuals by uncovering the effects of newly receiving such subsidies on health plan choices and healthcare demand. Our setting is Switzerland, which presents two salient features in this context. First, health insurance is mandatory[1], offered by private insurers and with a uniform benefits package to all insured individuals under the compulsory scheme. Second, insurers are not allowed to reject applications for mandatory health insurance or make profits from compulsory health plans, thus reducing adverse selection (Gruber, 2022; Schmid, Beck, & Kauer, 2018). We link rich administrative datasets fully covering the permanent residents, focusing on working-age adults (aged 26 to 64).[2] To our knowledge, we are, along with Zou (2023), among the first to examine the effects of premium subsidies on healthcare demand by combining administrative data on health insurance premium subsidies, income, health plan choices, and healthcare costs. While the majority of the literature on subsidies, health plan choices, or healthcare demand focuses on a subset of the population, this data linkage provides access to the entire population.

We exploit within-canton changes in eligibility thresholds as quasi-experimental variation in receiving subsidies. Eligibility, however, also depends on changes in household type and income. We address

[1] Art. 3 Abs. 1 KVG (Federal Health Insurance Act, *Bundesgesetz über die Krankenversicherung*, KVG)

[2] Health insurance premiums are lower for children and young adults (aged 19 to 25). In addition to premium subsidies, adults older than 64 are more likely to receive further transfers such as old-age benefits and other social assistance payments. For this reason, we focus on the main working-age population here.

potential selection and endogeneity in treatment decisions through several restrictions. First, we drop future observations once an individual changes canton or household type (e.g., through childbirth) as eligibility thresholds vary across these dimensions. Second, we limit the change in gross household income in the year determining eligibility to within 5%, attributing small fluctuations to noise rather than strategic behavior.[3] Third, to mitigate anticipation, we define the baseline as two periods before treatment changes and disregard the interim year. Finally, we drop individuals once they experience a second treatment switch, thereby avoiding interference from treatment reversals.

To address staggered treatment timing and potentially heterogeneous treatment effects, we employ the difference-in-differences (DID) estimator proposed by de Chaisemartin and D'Haultfœuille (2024). First, we identify the dynamic causal effect of newly receiving subsidies on choosing the lowest deductible by constructing counterfactuals for newly eligible individuals using those whose eligibility status will change in future periods and individuals who never receive subsidies in the observed period. Second, we examine the causal effect of subsidies on healthcare demand by employing a control function approach, whereby we estimate a first-stage ordered probit model for the choice of insurance deductible. The applied control function procedure, adapted from Wooldridge (2015) and combined with the staggered DID estimator of de Chaisemartin and D'Haultfœuille (2024), aims to remove endogeneity arising from the inclusion of the deductible level in the estimation of the healthcare demand model. Our instrument used to identify the effect of deductibles relies on an individual's relative position within the health insurance premium distribution in the year preceding subsidy receipt. We define a binary variable whether the individual's premium lies above or below the median premium within the corresponding premium region[4], conditional on household type, deductible level, and health plan features. The intuition is that this measure captures heterogeneity in an individual's ability to select insurers offering lower premiums. Those more attentive, or less inertial, in comparing premiums across insurers should, upon receiving subsidies, exhibit a greater propensity to adjust their deductible. We find that this instrument is a strong predictor of deductible choice, and present suggestive evidence for the validity of the exogeneity assumption.

The results reveal that the receipt of health insurance premium subsidies affects healthcare demand through two distinct mechanisms. First, we identify an indirect effect that operates via deductible choice: Upon receiving subsidies, individuals are more likely to select the lowest deductible. This shift in deductible choice is associated with higher healthcare expenditures, consistent with previous studies (e.g., Boes & Gerfin, 2016; Gerfin, Kaiser, & Schmid, 2015; Kaiser & Gerfin, 2017; Zabrodina, Dusheiko, & Moschetti, 2020). Similarly, Brot-Goldberg, Chandra, Handel, and Kolstad (2017) find that increasing deductibles reduces healthcare consumption. Second, we find evidence of a direct effect on healthcare demand that persists after accounting for changes in deductible choice. This positive effect offsets pre-treatment differences between treatment and control group trends. While the control group exhibits an upward trend in healthcare costs prior to receiving the subsidy, the treatment group shows a decline in

[3]We also demonstrate robustness of our results with alternative income ranges.
[4]Cantons have up to three premium regions.

costs, relative to the control group's trend. Assuming that this negative pre-treatment trend would have continued absent the subsidy, our estimates suggest that receiving subsidies counteracts this divergence. Importantly, this pattern is unlikely to imply subsidy-induced moral hazard, that is, individuals consuming more healthcare merely because the effective price falls. Although out-of-pocket spending (including both health care payments and the net premium after subsidy) decreases following subsidy receipt, we find no evidence that cost-sharing itself changes. Hence, the observed increase in total costs appears to arise from an income effect that offsets pre-existing downward trends, rather than from moral hazard.

Our analysis systematically considers potential violations of the common trend assumption (Rambachan & Roth, 2023) given the short pre-treatment period. The results remain qualitatively robust to alternative sample definitions and to specifications that restrict identification to within-canton changes in subsidy eligibility thresholds. To investigate potential selection-into-subsidies effects, we exploit institutional heterogeneity across cantons: some automatically enroll eligible individuals, whereas others require active application. Splitting the sample along this margin, we find that the subsidy-induced increase in the probability of choosing the lowest deductible is concentrated in non-automatic cantons, confirming the incentive effect of premium subsidies to adjust health plan choice, while in automatic cantons, the evidence points to a predominant income effect, with significant reductions in out-of-pocket spending without corresponding changes in deductible choice.

Our findings contribute to the growing literature on the effects of health insurance premium subsidies. In Switzerland, much of the literature examines the 2014 reform requiring cantons to switch from cash to in-kind transfers (Schmid et al., 2018). Whereas cash transfers are paid directly to the consumer, in-kind transfers are disbursed to insurers on behalf of the insured to lower premium costs. Evidence suggests that in-kind transfers increase the likelihood of choosing a low deductible and reduce both the risk of late premium payments and the need for government debt collection in cases of prolonged nonpayment (Kaufmann, Schmid, & Boes, 2017; Schmid et al., 2022; Vaidya, 2021). Although Kaufmann et al. (2017) do not find significant effects on healthcare utilization, Andersen, Dusheiko, and Grassi (2015) shows that subsidy reductions can adversely affect health outcomes, and Zou (2023) finds that larger subsidies generate strong income effects and raise the likelihood of selecting more generous health plans. Our findings similarly emphasize the influence of premium subsidies on deductible choice and add to the literature by documenting their effects on healthcare demand.

The United States presents a contrasting case, where regulated competition was partially introduced through the Affordable Care Act (ACA) marketplaces with premium subsidies (Einav et al., 2024), despite the system allowing for the possibility of remaining uninsured. Under the ACA, a temporary federal health insurance mandate with limited enforcement was established in a fragmented insurance environment (e.g., Kim et al., 2025). Subsidies appear to drive enrollment among low-income individuals who are particularly sensitive to premium changes (Goldin, Lurie, & McCubbin, 2020; Lurie, Sacks, & Heim, 2021; Saltzman, 2019), with higher coverage leading to increased healthcare utilization (Courtemanche, Marton, Ukert, Yelowitz, & Zapata, 2018; Dunn, Knepper, & Dauda, 2021; Finkelstein et al., 2012). Finkelstein et al.

(2019) find a rapid decline in insurance take-up for low-income individuals in Massachusetts as premium subsidies decrease, consistent with evidence that even with high subsidies, take-up would be incomplete, as enrollees' willingness to pay is less than half of the expected costs. Similarly, Frean et al. (2017) conclude that the subsidy rate significantly affects the probability of being uninsured. Tebaldi (2025) also finds that the willingness to pay for insurance is less than the expected cost for most low-income households. In relation to our findings, this suggests that the design of health insurance premium subsidies, the structure of health plans, and the nature of price regulation jointly shape both coverage decisions and healthcare demand. Careful calibration of these policy levers is therefore essential for achieving efficient and equitable outcomes in mandatory insurance systems.

The remainder of the paper is organized as follows. Section 2 introduces the institutional background and provides the theoretical framework underlying our study. Section 3 presents the data, including the sample construction and relevant descriptive statistics. Section 4 describes the empirical strategy. Section 5 reports the results and the robustness checks. Section 6 discusses and concludes.

2 Background and setting

2.1 Health system context: Swiss mandatory health insurance

Since 1996, health insurance in Switzerland has been mandatory[5] and regulated in the Swiss Federal Health Insurance Act (Gerritzen, Martínez, & Ramsden, 2014; Schmid et al., 2018). The mandatory health insurance system can be best described as a managed or regulated competition aimed at maintaining risk solidarity, health plan affordability, and efficiency (Schmid et al., 2018). Regulated competition is not unique to the Swiss health insurance system. For example, the United States, the Netherlands, Germany, or Chile also have components of regulated competition (McGuire & van Kleef, 2018).

In total, 39 insurers offered mandatory health insurance plans in 2024 (BAG, 2025b). Insurers must offer the standard health plan in all regions where they operate, which comes with a deductible of CHF 300, a free choice of general practitioners (GPs) and specialists for outpatient care, and a hospital list defined by the cantonal authority to choose from for inpatient care (Schmid et al., 2018). Additionally, a 10% coinsurance rate is in place for all healthcare costs that exceed the deductible, with a stop-loss amount of CHF 700 for adults and CHF 350 for children[6] (Schmid et al., 2018).

While the average yearly health insurance premium for 2024 amounted to CHF 5,081 for adults aged 26 and older (BAG, 2025c), premiums can vary between consumers for several reasons. On the one hand, variation can originate from the insurer's side. The levied premiums must be approved by the Federal Office of Public Health (FOPH) and are required to both cover costs and ensure financial solvency of insurers, implying that the insurers' expected costs should be reflected in their premium

[5] Art. 3 Abs. 1 KVG

[6] There are special rules in place for families with more than two children as well as exceptions for cost-sharing for specific types of care, for example, for expenditures related to maternity.

setting (Gerritzen et al., 2014; Schmid et al., 2018). Moreover, premiums are community-rated, but can vary by age group and location: Per canton, up to three premium regions are possible, and premiums can vary between children (18 years or younger), young adults (19 to 25 years), and adults (26 years or older) (Schmid et al., 2018).[7] On the other hand, variation in premiums can also come from consumers' choices, which are determined at the individual rather than the household level: There are no family plans; each individual holds their own insurance contract. One of these choices is a higher voluntary deductible, where premium rebates, which are capped by law, are granted in exchange for choosing a deductible of CHF 500, 1,000, 1,500, 2,000, or 2,500 in place of the standard deductible of CHF 300 (Schmid et al., 2018).[8][9] Premium rebates are also issued for managed care plans involving gatekeeping, such as Health Maintenance Organization (HMO) plans, telemedicine plans, or preferred provider organization (PPO) plans (Schmid et al., 2018). In 2024, only 12.8% of consumers chose the standard deductible without managed care, 6.7% chose a voluntary deductible without managed care, and a vast majority of 80.5% enrolled in managed care plans with either the standard or a voluntary deductible (BAG, 2025c). Furthermore, premium variation can originate from the consumer's choice of the pool of insurance companies with premiums varying due to the reasons mentioned above.[10]

Subject to specific deadlines, consumers can switch insurers (open enrollment, applications may not be turned down by insurers), deductible levels, and health plans by the end of a calendar year (BAG, 2025d). Between December 31, 2022, and January 1, 2023, 8.9% of consumers changed their insurer, which appears to be motivated by a premium reduction of 0.9% on average, while consumers who did not switch their insurer incurred an average premium increase of 6.6% (BAG, 2025a). 20.6% of consumers who changed insurers also selected a different deductible level, whereas only 4.9% of consumers staying with the same insurer altered their deductible level (BAG, 2025a). Although information on health plan choices to support decision-making is available, a third of the Swiss population struggles to find it; however, the most significant challenge related to health plan choice is understanding and managing financial information related to insurance (Bardy, 2024).

In proportion to the gross domestic product, Swiss health system costs are among the highest in the world (Papanicolas, Woskie, & Jha, 2018). Compared to other OECD countries, the share borne by private households is high in Switzerland (OECD, 2023). Thus, health insurance premium payments amount to a significant expenditure (Schmid et al., 2022). Even more so, the community-rated premiums exercise disproportionate financial pressure on households in lower-income groups (Gerritzen et al., 2014).

[7] Insurers are not allowed to make profits from compulsory health insurance plans, and there is a risk adjustment scheme in place to account for different risk pools of insurers (De Pietro et al., 2015; Schmid et al., 2018). However, insurers can offer supplementary health insurance plans, such as insurance for dental care. Supplementary insurance is regulated separately from mandatory insurance (Schmid et al., 2018) and not considered in this study.

[8] In this setting, young adults count as adults. For children, the voluntary deductibles range from CHF 100 to 600, with the standard deductible being CHF 0.

[9] In most cases, the standard deductible level of CHF 300 and the highest deductible level of CHF 2,500 dominate the middle deductible levels for the entire range of expected healthcare costs.

[10] Consumers can also reduce premiums by opting out of accident insurance if they are covered by their employer (Art. 8 Abs. 1 KVG). The services specified by the standard health plan would also cover accidents, yet all consumers working more than eight hours per week are insured against accidents through their employer (Schmid et al., 2018).

For this reason, cantons grant premium subsidies to enrollees in modest economic circumstances.[11] In 2024, 29.5% of consumers received premium subsidies with an average subsidy of CHF 2,484 per recipient and CHF 4,187 per household (BAG, 2025c).

The eligibility for and amount of health insurance premium subsidies can differ regarding the significant income type[12], income thresholds, and considerations of family structures as per the cantons' discretion (Schmid et al., 2018). Even though federal regulations exist, the cantons are jointly responsible for their healthcare systems together with the federation and the municipalities (e.g., Schmid et al., 2018). Health insurance premium subsidies are jointly financed by the cantons and the federal government, with the cantonal share amounting to 49.4% in 2024 (BAG, 2025c). As the previous year's tax declaration states the income relevant for determining subsidy eligibility for most cantons, most individuals could know in advance whether they will be eligible (Kaufmann et al., 2017). Furthermore, if individuals are on social welfare or receive supplementary benefits, they do not qualify for health insurance subsidies as social security payments cover the insurance premiums (Kaufmann et al., 2017). Because authority lies with the cantons, heterogeneous subsidy systems emerge across the cantons, featuring step functions, linear functions, non-linear functions, or even mixed systems (Gerritzen et al., 2014; Vaidya, 2021). Since 2014, the subsidies have been paid directly to the insurer (in-kind transfer scheme), thereby reducing the out-of-pocket premiums for subsidy recipients (Schmid et al., 2018). Furthermore, in some cantons, premium subsidies are automatically transferred to eligible households, respectively, to their insurers, based on their tax declaration, while in other cantons, individuals must apply for the subsidy.

The analysis that follows rests on the assumption that the objective of the premium subsidy system is to alleviate the premium burden for low-income individuals. Therefore, cross-cantonal variation in premium loads should reflect differences in healthcare prices, living costs, and social policies (Gerritzen et al., 2014). However, De Pietro et al. (2015) conclude that the various subsidy systems in place do not achieve the desired effect of sufficiently reducing the financial burden on low-income individuals, and therefore give rise to additional variability in the premium load depending on the canton. The issue is also salient in the current policy debate: Although Swiss citizens rejected a popular initiative to cap health insurance premiums at 10% of household income in June 2024, the canton of Vaud had already implemented such a cap in 2019, the parliament of the canton of Basel-Stadt decided in June 2025 to introduce the cap as well, and more cantons are considering following suit (Eichkorn, 2025).

2.2 Theoretical framework

The relevant theoretical framework for the decision-making process of health plan and healthcare choices under the influence of subsidies is provided by Andersen et al. (2015), using the two-stage model by Cardon and Hendel (2001). We use the two-stage model to formulate our hypotheses.

First, the expected-utility-maximizing consumer chooses the health plan with the highest expected

[11] Art. 65 Abs. 1 KVG

[12] Table B1 in the Online Appendix summarizes the types of significant income, additions, and deductions by canton.

utility (Cardon & Hendel, 2001). Health plan characteristics and interactions of individual and health plan characteristics determine the consumer's utility from a given plan (Atherly, Dowd, & Feldman, 2004). Premium subsidies influence this stage by altering policy prices; a higher subsidy, for example, could be used to select a more comprehensive health plan (Andersen et al., 2015). Therefore, our first hypothesis is that, on average[13], health insurance premium subsidies increase the probability of choosing a more comprehensive health plan, measured by the probability of selecting the lowest deductible.

Second, after realizing their uncertain health state, consumers choose healthcare consumption and all other goods (Cardon & Hendel, 2001). Here, the subsidies alter individuals' disposable income and influence overall consumption decisions at this stage, potentially affecting healthcare demand (Andersen et al., 2015). Thus, our second hypothesis is that, on average, health insurance premium subsidies lead to higher healthcare demand due to a higher disposable income.

3 Data

3.1 Sources

Our primary datasets originate from Swiss administrative records and comprise population and household statistics[14] (2010-2022) from the Federal Statistical Office (FSO), statistics on compulsory health insurance[15] (2018-2022) from the Federal Office of Public Health (FOPH), and individual earning accounts[16] (2010-2022) from the Central Compensation Office (CCO).

3.2 Outcomes

The primary outcomes of this study are the deductible choice and healthcare costs. In the first stage, we focus on a binary indicator for choosing the lowest deductible (CHF 300). In the second stage, to analyze healthcare demand, we consider gross healthcare costs, which describe all incurred healthcare costs as reported to the insurance company. One important drawback is that, in Switzerland, not all healthcare costs are automatically reported to insurers. On the one hand, in the *tiers garant* framework, the consumers settle the invoices of the service providers and report the incurred costs to the insurer, possibly leading to reimbursement upon reaching the deductible threshold.[17] On the other hand, in the

[13]On average, because some individuals' expected health status implies no healthcare costs at all. In such cases, subsidies become a pure income effect. Furthermore, consumers might not behave in an expected-utility-maximizing way.

[14]This dataset includes registry data on household types and is supplemented with experimental imputations (approximately 13%). Specifically, around 1.45% of households are estimated to be attributed to the wrong household type (BFS, 2022). The majority of these households are assigned to the household types "consensual couple with no children", "other household with multiple people", or "same-sex couple with no children" (BFS, 2022). We do not use any of these household types in our analysis due to some legal ambiguity in the assignment to the premium subsidy regulations; see Section B.2.1 in the Online Appendix for more details on the household types.

[15]We identify the premium subsidy amount by subtracting net premiums from gross premiums. Net premiums are only available from 2019 onwards; therefore, our analysis assumes receipt of the subsidy (binary) for 2018 to be equivalent to 2019 if specific rules are fulfilled; see Section B.2.4 in the Online Appendix for further details.

[16]The dataset includes gross income from various sources. See Online Appendix B.2.2 for more details.

[17]Art. 42 Abs. 1 KVG

tiers payant framework, the insurer deals directly with the service provider in settling the invoices.[18] Under *tiers garant*, consumers may decide not to report their healthcare costs to their insurers, especially if they anticipate that their overall healthcare costs will not reach the yearly deductible associated with their health plan. Hence, our measure of healthcare costs may underestimate the actual costs borne by enrollees, particularly for those with the highest deductible level (CHF 2,500).[19]

We winsorize the right tail of gross healthcare costs at the 99th percentile to reduce the influence of outliers. Furthermore, we define additional cost-related outcomes, namely, a binary variable indicating non-zero healthcare costs, a second binary variable indicating reporting of healthcare costs greater than CHF 1,000, as well as out-of-pocket healthcare costs defined as the sum of the net premium, which is the gross premium minus the subsidy, and the share of healthcare costs borne by the consumer.

3.3 Building the sample

We employ sample-building steps on the household-type level before combining individuals from different household types into a joint dataset.[20] We focus on single adults (parents) with 0 to 3 children and married couples with 0 to 3 children and further restrict the sample to adults aged 26 to 64.[21]

In our setting, the treatment group consists of individuals newly receiving subsidies, while the control group includes individuals who are not yet receiving subsidies or are never-treated. Our design targets treated and control units that are similar along observed characteristics and differ solely in subsidy receipt, allowing us to exploit quasi-random variation in treatment status. We formalize this by imposing further sample restrictions to improve comparability across groups and to enhance the plausibility of exogeneity.[22] First, we exclude all future years for units once they move to a different canton or change household type (e.g., by having a child or getting married). Such changes can directly impact treatment, as eligibility thresholds vary significantly by canton and household type, but they may also be endogenous to healthcare costs. Second, we exclude significant income shocks and restrict (past) gross[23] household income growth in the period of the switch to be within -5% and $+5\%$. For example, if, in year t, income from year $t-2$ is used to determine subsidy eligibility, we compare household income in year $t-2$ to that in year $t-3$.[24] We demonstrate the robustness of our results with alternative ranges of income variation. We interpret this small income change as idiosyncratic noise rather than strategic responses,

[18] Art. 42 Abs. 2 KVG

[19] This does not affect inpatient services, which are billed under the *tiers payant* framework (Art. 42 Abs. 2 KVG, Art. 42 Abs. 3 KVG). Furthermore, a survey from 2023 reports that for most insurers, the *tiers payant* system is dominant, handling around 80% to 90% of all bills (Manz, 2023), i.e., we do not expect that this affects our results by much.

[20] The procedure is detailed in Section B.2 in the Online Appendix.

[21] Due to an additional set of heterogeneous rules by canton, we exclude young adults (aged 19 to 25) from our analysis, and focus on the main working-age population up to age 64.

[22] One limitation of our strategy is that we cannot account for the possibility of suddenly applying for (and receiving) subsidies even though the household would have been eligible already before in non-automatic cantons, i.e., in cantons where households have to apply to receive subsidies. In such cases, the switch would be treated as exogenous even though it is a choice. In the robustness checks, we will present results for automatic and non-automatic cantons separately.

[23] As we do not observe significant income, we use gross income; see Online Appendix B.2.2 for further details.

[24] We observe the year used to determine subsidy eligibility for every canton and account for differences in years between cantons. See Table B1 in the Online Appendix for more details on the years used to determine eligibility.

such as slight variations in secondary income sources or salary adjustments due to inflation. Third, we account for the possibility that individuals may already know their change in eligibility status for year t by year $t-1$. In most cantons, the income used to determine subsidy eligibility is from a prior year[25], and information about the cantonal premium subsidy schemes in year t is published in year $t-1$. To address this issue, we define two periods before the treatment first changes as the baseline, and do not consider the effect in the period in between as a treatment effect but rather as an anticipation effect. This practice requires that any used observations have no treatment change in the year after individual i was first observed. Fourth, we exclude individuals from our analysis before they switch treatment for a second time in our observation period to isolate the initial treatment effect.

To ensure the comparability between treated and control units, we impose an additional income restriction. We define minimum and maximum thresholds for current household gross income for every canton and household type and exclude households outside this range at least once during their observation period. The ranges vary by canton to account for cantonal differences in eligibility thresholds, types of income used to determine subsidy eligibility, and cantonal income distributions. The ranges are set to the 5th and 95th percentiles of the treatment groups' income distribution[26] and are also applied to the never-treated. The final distributions of the corresponding treatment and control groups by household type mostly overlap, which supports our comparability assumption.[27]

3.4 Descriptive statistics

Table 1 shows descriptive statistics by subsidy receipt for the constructed sample. As expected, the table documents significant socioeconomic differences between those who receive subsidies and those who do not. In particular, adults receiving subsidies are more likely to be younger, female, and non-Swiss. By design (see Section 3.3), our sample shows no significant differences in income. For 23.8% of all consumers in the sample, their household receives subsidies. For the subsidy-receiving households, the average subsidy is CHF 2,602. The gross household premium is, on average, higher (CHF 7,304) for subsidy recipients than for individuals whose households do not receive subsidies (CHF 6,348).

35.0% select the lowest deductible in total; the share is approximately 2 percentage points (pp) higher for subsidy recipients than individuals not receiving subsidies. An average adult in our sample who receives subsidies reports gross healthcare costs of CHF 2,233 compared to CHF 2,624 without subsidies. However, the probability of reporting non-zero healthcare costs is slightly higher for recipients of subsidies. Nevertheless, their likelihood of reporting costs over CHF 1,000 is lower again compared to their counterparts who do not receive subsidies. Unsurprisingly, the out-of-pocket costs are lower for individuals receiving subsidies on average, at CHF 3,809, compared to CHF 4,661 for those without a

[25] See Table B1 in the Online Appendix for more details.

[26] We use the income distribution after all other restrictions have been applied. See Section B.2.4 in the Online Appendix for a more detailed description; step 17.

[27] Figure B1 in the Online Appendix visualizes the distributions. For simplification purposes, these distributions are based on the level of Switzerland, rather than on the cantonal level, on which the income ranges used can vary.

Table 1: Descriptive Statistics (Sample)

	Total		Subsidies		No Subsidies		
	Mean	SD	Mean	SD	Mean	SD	p
Individual Characteristics							
Age	46.20	10.30	42.97	8.64	47.21	10.57	< 0.001
Female	0.516	0.500	0.540	0.498	0.508	0.500	< 0.001
Swiss Nationality	0.777	0.416	0.707	0.455	0.799	0.400	< 0.001
Chronically Ill	0.118	0.323	0.091	0.287	0.127	0.333	< 0.001
HH Gross Income	88,751.3	29,613.7	88,836.0	28,624.8	88,724.9	29,916.1	0.219
HH Subsidy Yes/No	0.238	0.426					
HH Subsidy	626.7	1,422.3	2,601.8	1,805.4			
HH Gross Premium	6,575.8	2,461.0	7,304.4	2,319.1	6,348.3	2,459.9	< 0.001
Health Plan Choice							
Lowest Deductible	0.350	0.477	0.366	0.482	0.345	0.475	< 0.001
Healthcare Demand							
Healthcare Costs	2,531.0	7,451.4	2,233.2	6,528.1	2,624.0	7,715.0	< 0.001
Non-Zero Costs	0.742	0.437	0.753	0.431	0.739	0.439	< 0.001
Costs $>$ CHF 1,000	0.417	0.493	0.404	0.491	0.421	0.494	< 0.001
Out-Of-Pocket Costs	4,453.8	1,265.5	3,808.6	1,345.1	4,660.7	1,165.6	< 0.001
Observations		592,610		141,114		451,496	

Notes: HH: Household. Monetary variables are in CHF. Observations are pooled over the observation period (2018 to 2022). The sample is based on the inclusion criteria we define for identification. The p-value in the last column compares the mean of "Subsidies" to "No Subsidies" (t-test for non-binary variables, chi-squared test for binary variables). We assume subsidy receipt for 2018 (see Chapter B.2 in the Online Appendix). Table B3 in the Online Appendix additionally shows population statistics. The binary variable indicating chronic illness is constructed using data on pharmaceutical cost groups.

subsidy.

4 Empirical strategy and estimation approach

4.1 First stage: Health plan choice

To estimate the effects of health insurance premium subsidies on health plan choice in Switzerland, we use the heterogeneity-robust DID estimator by de Chaisemartin and D'Haultfœuille (2024) in a design with staggered treatment timing. We reduce the premium subsidies to a binary indicator and focus on the binary outcome of choosing the lowest deductible. We examine the outcomes of treated units relative to not-yet-treated and never-treated individuals by employing the staggered DID approach. It has been shown that two-way fixed effect (TWFE) regression estimates for staggered treatment and heterogeneous treatment effects can be biased and incorrectly signed due to negative weights (de Chaisemartin & D'Haultfœuille, 2022; Goodman-Bacon, 2021; Roth, Sant'Anna, Bilinski, & Poe, 2023). Recent literature discusses various heterogeneity-robust DID estimators, such as those by Callaway and Sant'Anna (2021) and Borusyak, Jaravel, and Spiess (2024) (Roth et al., 2023). The estimator by de Chaisemartin and D'Haultfœuille (2024) seems particularly suitable for our setting. This heterogeneity-robust estimator corresponds to the estimator by Callaway and Sant'Anna (2021) in binary and staggered designs with a particular choice of weights when not-yet-treated units are used as controls without covariates (de

Chaisemartin & D'Haultfœuille, 2024; Roth et al., 2023).[28] The *did_multiplegt_dyn*-package in Stata (de Chaisemartin et al., 2025), which implements the de Chaisemartin and D'Haultfœuille (2024) estimator, furthermore allows us to compute our estimates by only comparing treated individuals to control units with the same household type and canton, allowing for canton-household-type-specific trends.

More formally, denote the indicator $C_{i,t}$ for the status of individual i receiving subsidies at time t. f_i is the year corresponding to the first change in receiving subsidies, i.e., switching into treatment. To compute the dynamic treatment effects at time t, we estimate:

$$\begin{aligned} ATET_t = & \frac{1}{N_t^S} \sum_{i: C_{i,f_i-1} \neq C_{i,t+f_i-1}} Y_{i,t+f_i-1} - Y_{i,f_i-1} \\ & - \frac{1}{N_t^{Non-S}} \sum_{i': C_{i',f_i-1} = C_{i',t+f_i-1}} (Y_{i',t+f_i-1} - Y_{i',f_i-1}), \end{aligned} \tag{1}$$

where $Y_{i,t}$ is the outcome variable of individual i in t, N_t^S the number of switchers we observe in t, and N_t^{Non-S} the number non-switchers (not-yet-treated or never treated) we observe in t. The $ATET_t$ estimator from de Chaisemartin and D'Haultfœuille (2024) retrieves the dynamic effects by comparing the outcome evolution between year $f_i - 1$ and subsequent years $t + f_i - 1$ until 2022, between individuals who first switched in f_i and those whose subsidy claiming status remains unchanged at $t + f_i - 1$.

In addition to the aforementioned canton-household-type specific trends, our estimated model in Equation 1 includes individual and year fixed effects. Standard errors are clustered at the household level, which is the level of treatment. Furthermore, we control for current inflation-adjusted gross household income in logs, Swiss citizenship, and squared age in our estimations.

4.2 Second stage: Healthcare demand

Depending on the health plan selected in the first stage, healthcare consumption, among other goods, is chosen. Our goal is to causally estimate the effect of newly receiving subsidies on healthcare demand. Empirically challenging is the two-fold role of premium subsidies. First, a subsidy may directly impact healthcare demand by reducing insurance premiums and, consequently, increasing available income. Second, premium subsidies may impact healthcare demand indirectly through changes in the selected health plan. Deductible choice, in particular, must be considered as endogenous in a model for healthcare demand, and, therefore, we employ a control function approach to remove endogeneity arising from including the deductible level in the model (e.g., Heckman & Robb, 1985; Wooldridge, 2015).

We adapt the control function procedure in Wooldridge (2015) by specifying an ordered probit reduced form, including unit means in the sense of the Mundlak (1978) correction. The generalized residuals for the ordered probit, as defined, for example, by Vella (1993), are then inserted into the second stage healthcare demand estimation. Appendix A.2 shows the detailed procedure. First, we report the reduced form

[28] In our application, we obtain comparable results using both estimators, e.g., Figure A1 in the Appendix shows the event study for the ATET of receiving subsidies on the probability of choosing the lowest deductible for both estimators.

results, omitting the deductible choice in the model and applying the de Chaisemartin and D'Haultfœuille (2024) staggered DID estimator. Second, we report results from the control function procedure, which involves an ordinary least squares (OLS) regression in the second stage. Third, we report results from the control function procedure using the de Chaisemartin and D'Haultfœuille (2024) staggered DID estimator in the second stage, which additionally accounts for staggered treatment timing. Bootstrapped standard errors with 200 replications are reported for the control function applications.

As an instrument for deductible choice in the demand equation, we construct a measure that summarizes whether someone selects a relatively cheap or expensive health plan in year $t-1$. Within subgroups of similar individuals with similar choices, we compute the percentile of the gross premium in $t-1$ for every individual. A low percentile, for example, would indicate that the person has chosen a relatively cheap health plan compared to others with the same choice set (i.e., who live in the same premium region), the same household type, the same type of health plan, the same deductible level, and inclusion (or not) of accident coverage. On the one hand, a high premium percentile could indicate a "worse" choice: many others have obtained a health plan with the same features for a lower price. On the other hand, a high percentile could also show stronger preferences, e.g., towards a specific insurer (even though differences between insurers are limited), and thus more price-inelastic behavior. We expect that patterns in the gross premium percentile capture individual decision-making characteristics, which also affect the deductible choice. Thus, we define our instrument as a binary variable indicating whether someone's gross premium in $t-1$ is, within the subgroup, above the 50th percentile, i.e., above the median. This instrument seems plausibly exogenous and relevant; see Online Appendices B.4.1 and B.4.2 for supporting evidence.

4.3 Honest DID

The DID strategy used to identify the impact of newly receiving premium subsidies relies on the common trend assumption, i.e., treated and control would have followed the same outcome trend in the absence of the treatment. Testing solely on pre-trend differences has its limitations, including the low power of such tests, pre-trend differences that may not be significant even if the common trend assumption is violated, or the known pre-test bias when an analysis is conditioned on the results of pre-trend tests (Roth et al., 2023). We apply the honest DID approach introduced by Rambachan and Roth (2023) and report the estimates in the results tables. Post-treatment violations of common trends could be bounded on relative magnitudes if concerns about shocks affect treated and control units differently, thus generating violations to the common trend assumption (Rambachan & Roth, 2023). We do not apply bounds on relative magnitudes, as our setting should account for potential shocks given that we control for cantonal non-parametric trends and income. Instead, we impose smoothness restrictions that require violations in the post-treatment periods to remain within linear pre-trend extrapolations, as we cannot rule out trends that may differently affect our treatment and control groups (Rambachan & Roth, 2023). These smoothness restrictions include a parameter M, where $M = 0$ allows for linear violations of parallel trends, and $M > 0$ allows for deviations from linearity (Rambachan & Roth, 2023).

5 Results

The presentation of results is divided into three parts. First, we confirm the results from the previous literature that health insurance decisions are sensitive to premium changes by presenting the results from the first stage, specifically the effect of newly receiving subsidies on choosing the lowest deductible. Second, we present novel insights on the effect of subsidies on healthcare demand, i.e., the second stage in our setting. Third, we show complementary analyses to assess the robustness of our findings.

5.1 First stage: Health plan choice

Table 2 shows the estimated average treatment effect on the treated (ATET) of receiving health insurance premium subsidies on the probability of choosing the lowest deductible from the staggered DID setup. Figure 1 visualizes the event study estimates.

During the observation period, the probability of choosing the lowest deductible increased over time in both the complete sample and the treatment group. Within the control group, the increase persisted until 2020, after which the trend flattened.[29] Under the common trend assumption, we find that newly receiving subsidies reinforces the upward trend of choosing the lowest deductible by 1.5 pp (around 5% of the pre-treatment mean), on average, compared to similar consumers who do not receive health insurance premium subsidies. The event study estimates show that the ATET has steadily increased over time, indicating some adaptation time in the choice of insurance deductibles by the treated individuals. We do not find any evidence of pre-trend violations (Figure 1) and confirm our first-stage result on the deductible choice by imposing smoothness restrictions as defined by Rambachan and Roth (2023), where the estimate remains comparable with 1.6 pp.

Table A1 and Figure A2 in the Appendix show results for additional outcomes on deductible choice, namely choosing the lowest, the two lowest (CHF 300, 500), the three lowest (CHF 300, 500, 1,000), or the four lowest (CHF 300, 500, 1,000, 1,500) deductible levels, reflecting the ordinal nature of the deductible choice variable. Under the common trend assumption, the estimates for the additional outcomes are slightly higher than those for selecting the lowest deductible, ranging from 1.6 pp to 1.8 pp, but remain largely stable and are all statistically significant at the 1% level. The magnitude of the coefficients estimated with honest DID is somewhat lower, but we also observe larger standard errors.

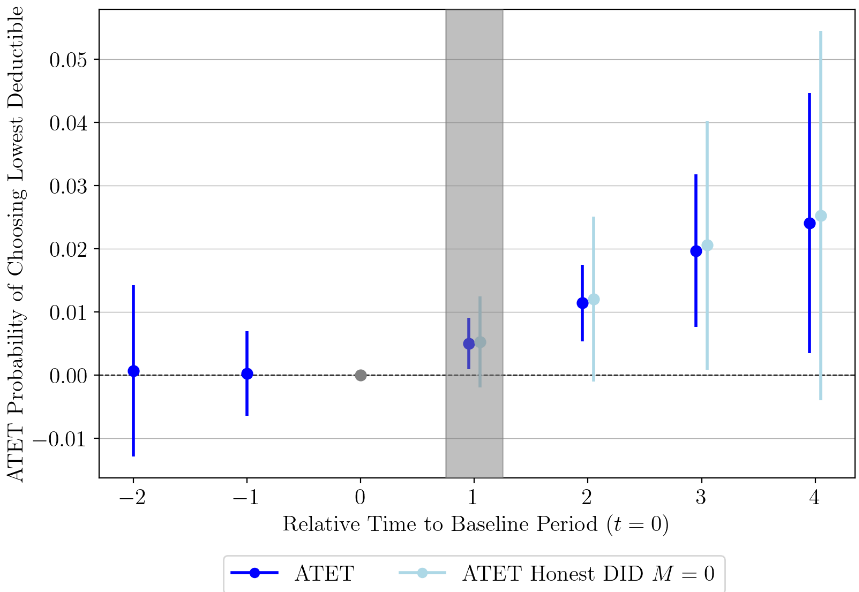
[29] Figure B2 in the Online Appendix illustrates the mean trends for the treatment and control groups for all outcomes.

Table 2: ATET of Receiving Subsidies on Probability of Choosing Lowest Deductible

ATET	0.015***
	(0.004)
ATET Honest DID $M = 0$	0.016**
	(0.008)
Honest DID Breakdown	< 0.001
Unit FE	✓
Year FE	✓
Individuals	210,160
Switchers	10,913

Notes: Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Coefficients represent $ATET_t$ estimates by de Chaisemartin and D'Haultfœuille (2024). Standard errors in parentheses (clustered at the household level). Individual and year fixed effects are included. Treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. Average effect considers periods $t = 2$ to $t = 4$ and excludes the anticipation period $t = 1$. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends; $M > 0$ accounts for deviations from linearity; breakdown value is the highest M for which $p < 0.05$; breakdown value search from 0 to 0.05 in intervals of 0.001.

Figure 1: ATET of Receiving Subsidies on Probability of Choosing Lowest Deductible, Event Study



Notes: Visualization of $ATET_t$ estimates by de Chaisemartin and D'Haultfœuille (2024) with 95% confidence intervals. Individual and year fixed effects are included. Treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends.

5.2 Second stage: Healthcare demand

5.2.1 Reduced form

Table 3 shows the ATET of receiving subsidies on four outcomes related to healthcare demand, with the same estimation approach as used for the choice of the lowest deductible. Here, we refer to this approach as the reduced form, since we will employ a control function approach at a later stage to include deductible choice in the demand equation to disentangle direct and indirect effects of newly receiving premium subsidies. Figure 2 shows the corresponding event study estimates.

In the control group, mean reported healthcare costs have been increasing over the observation period,

Table 3: ATET of Receiving Subsidies on Healthcare Demand, Reduced Form

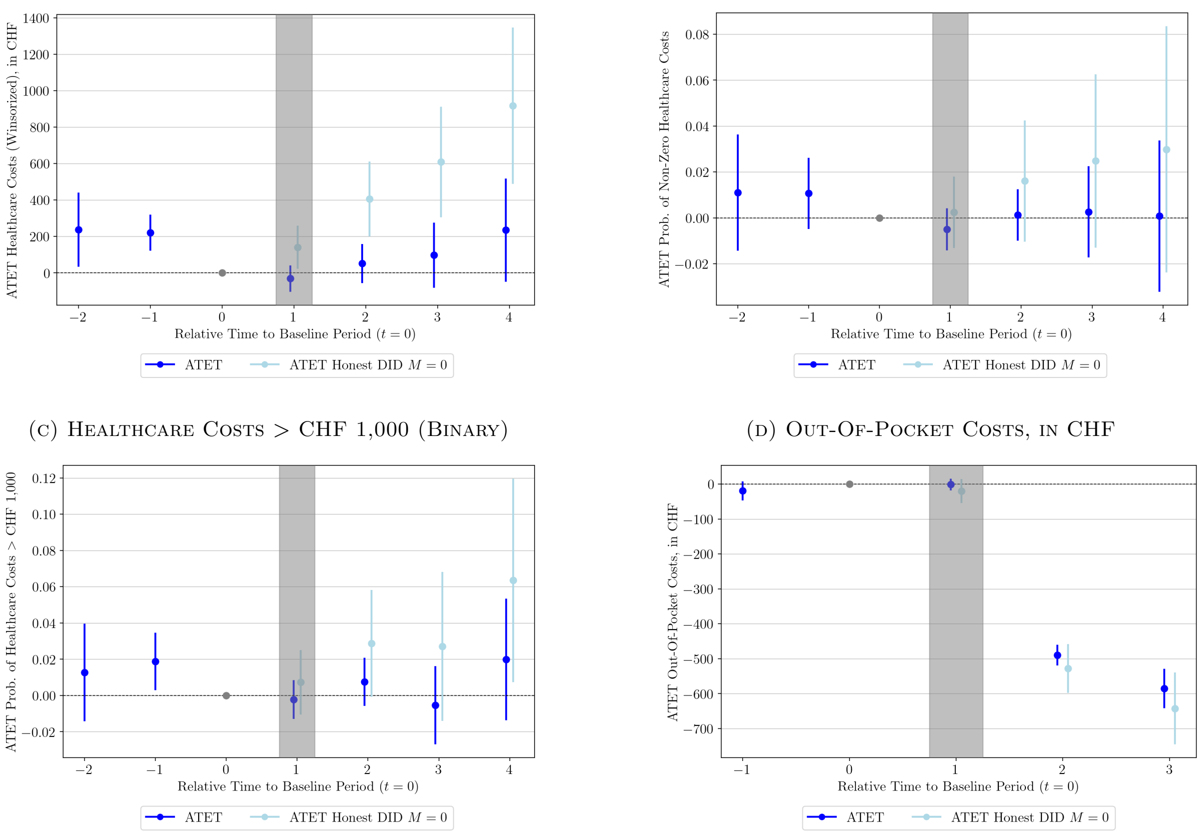
	(1) Healthcare Costs (Winsorized), in CHF	(2) Non-Zero Healthcare Costs (Binary)	(3) Healthcare Costs > CHF 1,000 (Binary)	(4) Out-Of-Pocket Costs, in CHF
ATET	89.4	0.002	0.006	-511.3^{***}
	(58.6)	(0.007)	(0.007)	(16.3)
ATET Honest DID $M = 0$	531.8^{***}	0.020	0.033^{*}	-553.8^{***}
	(124.1)	(0.016)	(0.018)	(38.5)
Honest DID Breakdown	> 50	–	–	> 50
Unit FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Individuals	210,014	210,015	210,014	104,701
Switchers	10,913	10,913	10,913	4,751

Notes: Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Coefficients represent $ATET_t$ estimates by de Chaisemartin and D'Haultfœuille (2024). Standard errors in parentheses (clustered at the household level). Individual and year fixed effects are included. Treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. Average effect considers periods $t = 2$ to $t = 4$ and excludes the anticipation period $t = 1$. Honest DID Rambachan and Roth (2023): $M = 0$ allows for linear deviations of parallel trends; $M > 0$ accounts for deviations from linearity; breakdown value is the highest M for which $p < 0.05$; (1) and (4) breakdown value search from 0 to 50 in intervals of 1; (2) and (3) breakdown value search from 0 to 0.05 in intervals of 0.001. Due to data availability restrictions regarding out-of-pocket costs for 2018, periods $t = -2$ and $t = 4$ cannot be computed. The average effect on out-of-pocket costs considers periods $t = 2$ and $t = 3$.

with one exception for 2020, where the upward trend temporarily flattened (Figure B2 in the Online Appendix). Under the common trend assumption, we do not find evidence that healthcare costs are affected by newly receiving subsidies relative to the control group. However, pre-treatment estimates in both periods, $t = -2$ and $t = -1$, are positive and significant, indicating that, compared to the upward trend of the control group, the costs in the treatment group have been declining before treatment. We account for such trends in healthcare costs affecting the treatment and control groups differently by imposing smoothness restrictions (Rambachan & Roth, 2023). In other words, we assume that the treatment group's negative trend relative to the control group would continue in the absence of treatment, thereby relaxing the common trend assumption in the basic DID specification. By permitting a linear continuation of the trend only by setting $M = 0$, we obtain a coefficient of CHF 531.8, indicating that the subsidy counteracts the negative trend relative to the control group. The estimate remains significant even if we allow for linearity deviations greater than CHF 50 between consecutive periods. The event study results show that the honest DID point estimates become statistically significant from the first year upon receiving subsidies ($t = 2$), and continue to increase over time.

We do not find evidence that the probability of having non-zero reported healthcare costs is affected by newly receiving subsidies, neither under the common trend assumption, nor under smoothness restrictions. In other words, among individuals with zero pre-subsidy expenditures, subsidies do not appear to increase the likelihood of initiating health spending. Similar to the probability of incurring non-zero costs, the probability of reported costs exceeding CHF 1,000 is increasing over time in the control group (Figure B2 in the Online Appendix). Again, 2020 is the exception, where the probability of having healthcare costs that are non-zero (greater than CHF 1,000) temporarily flattens (decreases). For the probability

Figure 2: ATET of Receiving Subsidies on Healthcare Demand, Reduced Form, Event Study



(a) Healthcare Costs (Winsorized), in CHF

(b) Non-Zero Healthcare Costs (Binary)

(c) Healthcare Costs > CHF 1,000 (Binary)

(d) Out-Of-Pocket Costs, in CHF

Notes: Visualization of $ATET_t$ estimates by de Chaisemartin and D'Haultfœuille (2024) with 95% confidence intervals. Individual and year fixed effects are included. Treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends. Due to data availability restrictions regarding out-of-pocket costs for 2018, periods $t = -2$ and $t = 4$ cannot be computed in panel (D).

of costs exceeding CHF 1,000, the pattern of estimates is similar to that for healthcare costs: We find no statistically significant effect under common trends; however, we estimate a significantly positive pre-treatment effect in $t = -1$, indicating differential trends in the control and treatment groups. When imposing smoothness restrictions and allowing for linear violations of parallel trends, we estimate a positive effect of 3.3 pp, i.e., relative to the control group, the subsidy manages to counteract the negative trend in the probability of having healthcare costs above CHF 1,000. However, this estimate is less robust compared to the estimate for healthcare costs, as it is only significant at the 10% level.

For the out-of-pocket costs, we can only estimate one pre-treatment effect due to data availability restrictions in 2018. The control group trend is flat from 2019 to the end of our observation period. We do not estimate a significant effect for the pre-treatment period and find negative estimates of similar magnitude under the common trend assumption and under honest DID (with significance withstanding linearity deviations of more than CHF 50).

5.2.2 Structural equation with control function

To isolate the impact of subsidies and deductibles in the healthcare demand equation, we employ the control function approach, which enables us to account for the endogeneity of deductible choice. Table 4 presents the results, both with a second-stage OLS regression and with the de Chaisemartin and D'Haultfœuille (2024) staggered DID estimator in the second stage, which additionally accounts for staggered treatment timing. Figure 3 visualizes the event study estimates for both specifications.

The control function estimates of the effect of subsidies on healthcare costs imply a similar pattern to the reduced form estimates. Imposing smoothness restrictions with $M = 0$ without considering staggered treatment timing gives an effect estimate of CHF 313.1, compared to CHF 531.8 in the reduced form, suggesting that the reduced form overestimated the effect. The control function estimate remains significant for linearity deviations between consecutive periods of up to CHF 23. The significant coefficient for the generalized residuals indicates the presence of endogeneity in the deductible choice, emphasizing the importance of the control function approach. Compared to the lowest deductible of CHF 300, which serves as the baseline, the coefficients on choosing higher deductible levels are negative and increase in magnitude as the deductible increases. As expected, consumers with higher deductibles tend to incur fewer healthcare costs. Extending the specification further to control for staggered treatment timing yields a comparable estimate of CHF 288.6, allowing for linear violations of common trends. This estimate remains significant for deviations from linearity between periods of up to CHF 8. Altogether, our findings suggest that premium subsidies raise healthcare demand both indirectly, by increasing the probability of selecting the lowest deductible, and directly, through their effect on healthcare costs.

Regarding the probability of reporting non-zero healthcare costs, we estimate a significant negative coefficient under parallel trends. However, when accounting for staggered treatment timing, the effect is no longer significant. Applying honest DID changes the signs for both specifications and returns insignificant estimates. Thus, we do not find conclusive evidence that the probability of having non-zero healthcare costs is affected by newly receiving subsidies. Nevertheless, we find a significant coefficient on the generalized residuals, highlighting the presence of endogeneity in the choice of deductibles at the extensive margin of healthcare costs. As expected, the deductible coefficients suggest lower estimated probabilities of reporting healthcare costs, the higher the chosen deductible level.

The pattern of results is similar for the probability of reporting healthcare costs greater than CHF 1,000. While we estimate significant negative coefficients under common trends, allowing for linear deviations with honest DID again changes the sign and returns insignificant estimates. This suggests that the adverse effect found under common trends represents a continuation of an already existing downward trend for the treatment group relative to the control group. Thus, we do not find evidence that the probability of healthcare costs being above CHF 1,000 is significantly affected by newly claiming subsidies. This is contrary to the reduced form findings, where we estimated a positive and significant effect. By not including the deductible choice in the reduced form model, this leads to an overestimation of the

Table 4: ATET of Receiving Subsidies on Healthcare Demand, Control Function

	(1) Healthcare Costs (Winsorized), in CHF		(2) Non-Zero Healthcare Costs (Binary)		(3) Healthcare Costs > CHF 1,000 (Binary)		(4) Out-Of-Pocket Costs, in CHF	
Control Function	✓	✓	✓	✓	✓	✓	✓	✓
Staggered DID	✗	✓	✗	✓	✗	✓	✗	✓
ATET	−25.3	−77.3	−0.012**	−0.007	−0.012**	−0.016**	−547.3***	−534.3***
	(40.4)	(66.9)	(0.005)	(0.008)	(0.006)	(0.008)	(16.6)	(18.9)
ATET Honest DID $M = 0$	313.1***	288.6**	0.001	0.013	0.026	0.011	−577.7***	−582.2***
	(109.4)	(142.3)	(0.014)	(0.019)	(0.016)	(0.019)	(34.7)	(41.0)
Honest DID Breakdown	23	8	–	–	–	–	> 50	> 50
Deductible CHF 500	−1,136.8***		−0.098***		−0.243***		−46.6	
	(317.4)		(0.029)		(0.029)		(98.2)	
Deductible CHF 1,000	−1,578.9***		−0.163***		−0.366***		−273.2**	
	(394.9)		(0.036)		(0.037)		(125.0)	
Deductible CHF 1,500	−1,781.4***		−0.195***		−0.448***		−505.6***	
	(487.4)		(0.043)		(0.043)		(145.7)	
Deductible CHF 2,000	−2,088.8***		−0.234***		−0.536***		−818.4***	
	(552.3)		(0.050)		(0.048)		(170.9)	
Deductible CHF 2,500	−2,982.3***		−0.308***		−0.695***		−1,274.1***	
	(851.8)		(0.071)		(0.070)		(252.2)	
Generalized Residuals	920.4**		0.079***		0.204***		123.8	
	(373.0)		(0.030)		(0.030)		(110.7)	
Bootstrapped SE	✓	✓	✓	✓	✓	✓	✓	✓
Unit FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Individuals	167,151	190,608	167,151	190,609	167,151	190,608	157,337	96,652
Switchers	11,107	10,045	11,107	10,045	11,107	10,045	5,090	4,422

Notes: Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Coefficients represent ATET estimates from a control function approach with an ordered probit for deductible choice in the first stage and OLS or the de Chaisemartin and D'Haultfœuille (2024) staggered DID estimator in the second stage. Standard errors in parentheses (clustered at the household level and bootstrapped with 200 repetitions). Individual and year fixed effects are included. For the staggered DID, treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. Average effect considers periods $t = 2$ to $t = 4$ and excludes the anticipation period $t = 1$. The instrument for the control function approach is a binary variable indicating whether someone's gross premium in $t - 1$ is, within the subgroup of individuals with the same choice set and choices, above the 50th percentile, i.e., above the median. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends; $M > 0$ accounts for deviations from linearity; breakdown value is the highest M for which $p < 0.05$; (1) and (4) breakdown value search from 0 to 50 in intervals of 1; (2) and (3) breakdown value search from 0 to 0.05 in intervals of 0.001. Deductible levels, generalized residuals, and other control variables are included in the columns employing staggered DID, but the coefficients are not reported. Due to data availability restrictions regarding out-of-pocket costs for 2018, periods $t = -2$ and $t = 4$ cannot be computed. While the *did_multiplegt_dyn*-package in Stata (de Chaisemartin et al., 2025) considers this automatically, we manually exclude units first treated in 2020 for the out-of-pocket costs in the specification that does not account for staggered treatment timing, as we do not observe their outcome at baseline. The average effect on out-of-pocket costs considers periods $t = 2$ and $t = 3$.

Figure 3: ATET of Receiving Subsidies on Healthcare Demand, Control Function, Event Study

Notes: Visualization of ATET estimates with 95% confidence intervals from a control function approach with an ordered probit for deductible choice in the first stage and OLS or the de Chaisemartin and D'Haultfœuille (2024) staggered DID estimator in the second stage. Individual and year fixed effects are included. For the staggered DID, treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. The instrument for the control function approach is a binary variable indicating whether someone's gross premium in $t-1$ is, within the subgroup of individuals with the same choice set and choices, above the 50th percentile, i.e., above the median. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends. Due to data availability restrictions regarding out-of-pocket costs for 2018, periods $t = -2$ and $t = 4$ cannot be in panel (D). While the *did_multiplegt_dyn*-package in Stata (de Chaisemartin et al., 2025) considers this automatically, we manually exclude units first treated in 2020 for the out-of-pocket costs in the specification that does not account for staggered treatment timing, as we do not observe their outcome at baseline.

subsidy effect. There is endogeneity present in this specification as well, and the predicted probability of reporting healthcare costs exceeding CHF 1,000 decreases as the chosen deductible increases.

Finally, the results with the control function approach confirm the reduced form estimates for the out-of-pocket costs. While we estimate, on average, an effect of newly receiving subsidies on out-of-pocket costs of CHF −553.8 in the reduced form specification with honest DID, we obtain slightly larger effect estimates of CHF −582.2 (CHF −577.7) with (without) accounting for staggered treatment timing in the control function specification. The coefficient on the generalized residuals is insignificant, i.e., bias due to the endogeneity of the deductible variable is less relevant here. The coefficients for the deductible levels imply lower out-of-pocket costs as the deductible level increases. This pattern of effects, i.e., increased total healthcare costs and decreased out-of-pocket costs, will be further discussed below.

5.2.3 Subsample analysis

The results observed so far may be partly driven by specific subgroups of the population. For instance, younger individuals tend to be more price-sensitive in their health plan choices (Tebaldi, 2025), while older age tends to be associated with higher health insurance literacy (Bardy, 2024) and higher healthcare needs, which could, in turn, affect health plan choice and healthcare utilization. Furthermore, female consumers may be more risk-averse than male consumers, and, hence, make different health plan and healthcare choices (Agnew, Anderson, Gerlach, & Szykman, 2008; Borghans, Golsteyn, Heckman, & Meijers, 2009; Hartog, Ferrer-i-Carbonell, & Jonker, 2002; Mauvais-Jarvis et al., 2020).

Table 5 explores potential heterogeneity in treatment effects with control function estimates by sex and birth cohort, alongside the baseline (full sample) results from Table 4 for comparison. For all reported coefficients, we apply the honest DID smoothness restrictions, allowing for linear deviations of parallel trends with $M = 0$. The breakdown values are reported in square brackets. For conciseness, we focus on the results accounting for the staggered treatment timing. Figures A3 to A6 in the Appendix show the complementary event study results by subsample.

Concerning healthcare costs, applying the de Chaisemartin and D'Haultfœuille (2024) staggered DID estimator reveals striking differences: First, the positive coefficient in the complete sample is driven by female consumers (CHF 467.9), whereas for men, the effect is much smaller (CHF 74.8) and statistically insignificant. For both sex subgroups, the mean reported healthcare costs have increased over the observation period. Event study results suggest that the downward-sloping pre-treatment trend relative to the control group is observed only in the female subsample. Second, members of the middle age group (born between 1969 and 1982) appear to be another driver of the effect on healthcare costs, along with the female subgroup, although with a lower magnitude (CHF 443.2). Healthcare costs in the corresponding control groups generally increased over the observation period for all three age groups, except in 2020.[30] In the youngest age group, trends differ in the treatment and control groups: Relative to the trend of the control group, the treatment group's trend has been decreasing prior to treatment. When permitting the common trend violation to continue linearly, we find no effect of newly receiving subsidies. In the middle age group, the treatment group's trajectory before treatment has also been declining, although not as pronounced as for the youngest age group. Here, we find that subsidies mitigate this downward pattern for this age group. For the oldest age group, we do not find that the treatment group's pattern differs significantly from that of its control group before and after treatment.

Regarding the probability of reporting healthcare costs above CHF 0 or CHF 1,000, we find positive and significant effects for the oldest age group between 5.1 and 7.8 pp relative to the controls. However, for the indicator of costs exceeding CHF 1,000, the effect is less precisely estimated when accounting for staggered treatment timing. These findings nonetheless suggest that older individuals are more likely to

[30] In the control group for the youngest age group, mean healthcare costs decreased from 2018 to 2020; however, the median increased from 2018 to 2019. Thus, we still conclude that healthcare costs generally increase over the observation period.

Table 5: ATET of Receiving Subsidies on Healthcare Demand, Control Function, Honest DID, Subsamples

	(1) Healthcare Costs (Winsorized), in CHF		(2) Non-Zero Healthcare Costs (Binary)		(3) Healthcare Costs > CHF 1,000 (Binary)		(4) Out-Of-Pocket Costs, in CHF	
Control Function	✓	✓	✓	✓	✓	✓	✓	✓
Staggered DID	✗	✓	✗	✓	✗	✓	✗	✓
Complete Sample	313.1***	288.6**	0.001	0.013	0.026	0.011	−577.7***	−582.2***
	(109.4)	(142.3)	(0.014)	(0.019)	(0.016)	(0.019)	(34.7)	(41.0)
	[23]	[8]	[–]	[–]	[–]	[–]	[> 50]	[> 50]
Female	295.6*	467.9**	−0.004	−0.025	0.048*	0.051*	−516.8***	−523.7***
	(167.7)	(202.4)	(0.019)	(0.024)	(0.025)	(0.028)	(53.9)	(57.4)
	[–]	[28]	[–]	[–]	[–]	[–]	[> 50]	[> 50]
Male	330.1**	74.8	0.008	0.045	0.004	−0.028	−646.7***	−658.2***
	(143.3)	(186.8)	(0.023)	(0.029)	(0.020)	(0.028)	(56.8)	(60.7)
	[17]	[–]	[–]	[–]	[–]	[–]	[> 50]	[> 50]
Birth Year 83-96	−113.6	289.5	0.002	0.031	0.050	0.040	−505.4***	−490.3***
	(228.2)	(278.6)	(0.031)	(0.045)	(0.032)	(0.045)	(81.6)	(97.7)
	[–]	[–]	[–]	[–]	[–]	[–]	[> 50]	[> 50]
Birth Year 69-82	393.4**	443.2**	−0.016	−0.014	0.003	−0.008	−556.9***	−553.0***
	(157.0)	(182.9)	(0.021)	(0.028)	(0.021)	(0.028)	(56.1)	(62.5)
	[26]	[30]	[–]	[–]	[–]	[–]	[> 50]	[> 50]
Birth Year 55-68	621.2**	253.4	0.060**	0.078**	0.071**	0.051	−735.0***	−705.5***
	(286.4)	(351.9)	(0.028)	(0.033)	(0.034)	(0.038)	(74.1)	(81.5)
	[26]	[–]	[0.002]	[0.004]	[0.002]	[–]	[> 50]	[> 50]
Bootstrapped SE	✓	✓	✓	✓	✓	✓	✓	✓
Unit FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Coefficients represent ATET estimates from a control function approach with an ordered probit for deductible choice in the first stage and OLS or the de Chaisemartin and D'Haultfœuille (2024) staggered DID estimator in the second stage. All coefficients are reported after applying honest DID (Rambachan & Roth, 2023) with $M = 0$ (allows for linear deviations of parallel trends; $M > 0$ accounts for deviations from linearity; breakdown value is the highest M for which $p < 0.05$; (1) and (4) breakdown value search from 0 to 50 in intervals of 1; (2) and (3) breakdown value search from 0 to 0.05 in intervals of 0.001). Honest DID breakdown value in square brackets. Standard errors in parentheses (clustered at the household level and bootstrapped with 200 repetitions). Average effect considers periods $t = 2$ to $t = 4$ and excludes the anticipation period $t = 1$. The instrument for the control function approach is a binary variable indicating whether someone's gross premium in $t - 1$ is, within the subgroup of individuals with the same choice set and choices, above the 50th percentile, i.e. above the median. Due to data availability restrictions regarding out-of-pocket costs for 2018, period $t = 4$ cannot be computed. While the *did_multiplegt_dyn*-package in Stata (de Chaisemartin et al., 2025) considers this automatically, we manually exclude units first treated in 2020 for the out-of-pocket costs in the specification that does not account for staggered treatment timing, as we do not observe their outcome at baseline. The average effect on out-of-pocket costs considers periods $t = 2$ and $t = 3$.

incur healthcare expenditures upon receiving subsidies. Yet, these expenditures appear to be modest, as overall healthcare costs are not much affected by subsidy receipt. This is in contrast to the middle age group, for whom subsidies increase healthcare costs but do not significantly affect the likelihood of incurring any costs. Regarding out-of-pocket costs, the magnitude of the effect is greater in the male subsample relative to the female subsample, and it increases by age group.

5.3 Robustness checks

5.3.1 Income growth constraints

In our data preparation, we excluded consumers who experienced significant income shocks, as such shocks could lead to endogenous changes in subsidy eligibility. Therefore, we define a range of -5% to $+5\%$ and require past gross household income growth in the year used to determine subsidy eligibility to be within that range. Here, we present results for two narrower income growth ranges of -4% to $+4\%$ and -3% to $+3\%$. All in all, the observed patterns remain stable; however, the smaller sample size results in larger standard errors, and consequently, some differences in significance arise.

Table A2 in the Appendix presents the first-stage results for all ranges. Under common trends, the ATET estimates are similar; however, when imposing smoothness restrictions under honest DID, the estimates for the narrower income growth ranges are rendered insignificant. Nevertheless, examining the event study estimates in Figure A7 in the Appendix reveals that slightly lower estimates and larger standard errors, due to the restricted sample for the narrower ranges, lead to lower and insignificant average point estimates for the honest DID. The event study point estimates and general patterns, however, are similar over time for the three ranges.

The second-stage results for all income growth ranges are presented in Table A3 and Figure A8 in the Appendix. All results are based on the control function specification. Regarding healthcare costs, the patterns for the narrower income growth ranges are comparable to the wider range. We obtain significant pre-treatment estimates, which, upon relaxing the common trend assumption, lead to a positive average point estimate for the ATET. For the wider range, the estimate is CHF 288.6; for the narrower ranges, CHF 234.0 and CHF 197.7, respectively. However, the estimates for the narrower ranges are no longer significant, likely due to the smaller sample size. For the two binary indicators of reporting costs above CHF 0 and CHF 1,000, we do not find evidence that subsidies have an effect in all income growth ranges. Finally, concerning out-of-pocket costs, the narrower income growth range estimates are with CHF -581.9 and CHF -604.8 very similar to the estimate with a wider income growth range.

5.3.2 Cantonal premium subsidy system changes

Our sample-building steps aim to restrict endogenous treatment switches. More specifically, we attempt to capture only consumers switching into treatment due to exogenous changes in the cantonal premium subsidy systems. However, consumers could also move to the treated income range with small income changes within the predefined 5% range. To validate that these income changes do not entirely drive our results, we present results here separately for consumers who receive subsidies for the first time in a year in which their canton increased the income threshold for subsidy eligibility and for consumers whose canton used the same income threshold as in the previous year to determine subsidy eligibility in the consumer's year of first subsidy receipt.

Table A4 and Figure A9 in the Appendix present similar first-stage results for both groups; however, the standard errors are larger in the cantonal change subsample when applying honest DID. Table A5 and Figure A10 in the Appendix reveal that the positive second-stage estimate when relaxing the common trend assumption is entirely driven by consumers in cantons that increased the subsidy eligibility threshold in the year in which the consumers started to receive subsidies. While the estimates for the binary indicators of incurring positive healthcare costs or healthcare costs above CHF 1,000 are similar and insignificant for both subgroups, the estimates for out-of-pocket costs are significant for both, with a larger absolute coefficient for the no cantonal change subgroup.

5.3.3 (Non-)automatic payment of premium subsidies

Table A6 and Figure A11 in the Appendix present the first-stage results for two types of cantons: determining premium subsidies automatically or upon application. If individuals automatically receive premium subsidies and, hence, pay lower insurance premiums, the "trigger" effect of revising the insurance portfolio once eligible for premium subsidies may be weaker; i.e., a priori, we would expect a smaller effect of newly receiving subsidies on deductible choice. Although the results in the automatic sample have less power due to its smaller sample size, we indeed find that non-automatic cantons drive the effect of subsidies on the probability of choosing the lowest deductible, with around 2.5 to 2.7 pp.

Regarding healthcare costs, we expect the opposite pattern, at least for the direct effect. Since individuals in cantons with automatic subsidy payments do not have to actively take action to receive the subsidies, and thus pay lower premiums, the additional resources available may lead to a stronger income effect. Table A7 and Figure A12 in the Appendix show the second-stage results. The estimate for the ATET is indeed larger in magnitude than in non-automatic cantons, yet not statistically significant, which could again be related to the smaller sample size. Regarding the binary outcomes describing healthcare costs, neither the automatic nor the non-automatic subsample yields significant estimates. The estimate on out-of-pocket costs is negative and significant in both subsamples, but stronger in the automatic than in the non-automatic cantons, confirming our hypothesis.

6 Discussion and conclusion

Using rich administrative data from Switzerland, we analyze the effects of newly receiving health insurance premium subsidies on health plan choice and healthcare demand. Theoretically, premium subsidies can affect healthcare demand through two concurrent, non-mutually-exclusive channels: (i) a change in health insurance coverage, reflecting how individuals set their risk aversion and their beliefs about the distribution of future health shocks (Einav & Finkelstein, 2018), and (ii) an income effect, whereby a subsidy increases disposable income due to lower premium payments.

We find that consumers are more likely to increase health insurance coverage upon receiving subsidies, supporting our first hypothesis that subsidies induce consumers to choose more comprehensive health plans, reducing their financial risk of covering future health expenditures out-of-pocket. Thus, the subsidies at least partially relax consumers' liquidity constraints, allowing them to opt for more comprehensive health plans, which in turn lower the effective marginal price at the point of care. This finding aligns with evidence from Switzerland on in-kind versus cash transfers, which indicates that in-kind transfers increase the likelihood of choosing a low deductible (e.g., Kaufmann et al., 2017; Schmid et al., 2022; Vaidya, 2021). Furthermore, we confirm previous findings that insurance decisions are sensitive to premium changes (e.g., Finkelstein et al., 2012; Goldin et al., 2020; Lurie et al., 2021; Saltzman, 2019; Tebaldi, 2025), even in a setting with mandatory health insurance. Although the direction is clear and robust across different specifications, the magnitude of the effect on the probability of choosing the lowest deductible is modest, with approximately 1.5 pp in absolute terms or about 5% of the pre-treatment mean.

Furthermore, we investigate how premium subsidies affect healthcare demand, directly and indirectly, by applying a reduced form and a control function approach with an ordered probit in the first stage to account for the endogeneity of deductible choice. Consistent with previous evidence, choosing higher deductible levels leads to lower healthcare demand, as measured by the reported healthcare costs, the probability of reporting non-zero healthcare costs, and the probability of reporting healthcare costs greater than CHF 1,000 (e.g., Aron-Dine, Einav, & Finkelstein, 2013; Brot-Goldberg et al., 2017; Gerfin & Schellhorn, 2006; Manning, Newhouse, Duan, Keeler, & Leibowitz, 1987). Together with our initial findings on insurance coverage, these estimates suggest that premium subsidies indirectly affect demand through their impact on plan generosity. This pattern corroborates earlier evidence that higher insurance coverage increases healthcare utilization (Courtemanche et al., 2018; Dunn et al., 2021; Finkelstein et al., 2012; Taubman, Allen, Wright, Baicker, & Finkelstein, 2014), extending the conclusion to a mandatory-coverage context. In the Swiss context, prior work documents that selecting the lowest deductible facilitates demand inducement (Zabrodina et al., 2020), individuals with high deductibles use fewer healthcare services (Kaiser & Gerfin, 2017), a deductible reset at the start of the year reduces demand among consumers with a high deductible (Gerfin et al., 2015), and full insurance raises the likelihood of incurring costs (Boes & Gerfin, 2016).

Once incorporating the subsidy effect, our second-stage estimates indicate a direct impact on healthcare demand: Subsidies increase total healthcare costs by CHF 297.3, on average. Hence, the effect of premium subsidies on healthcare demand does not only operate through selection on plan choice. Relative to the upward trend in costs in the control group, treated units exhibited a declining trend prior to receiving subsidies. Relaxing the common trend assumption and extrapolating the treated group's pre-treatment trend, we provide evidence that the subsidies counteract the treatment group's negative trend in healthcare costs relative to the control group. These conclusions are conditional on the assumption that the pre-treatment trend would have continued in the same manner in the absence of the treatment.

While we do not find conclusive evidence that the probability of non-zero costs or costs above CHF 1,000 is affected by newly receiving subsidies, we estimate that subsidies have a significantly negative effect of CHF −582.2 on out-of-pocket costs. Considering only financial aspects, premium subsidies achieve their intended goal of reducing out-of-pocket spending. Yet, this last result naturally raises the question of whether subsidies might induce moral hazard, whereby individuals consume more healthcare when their effective price of care decreases (Einav & Finkelstein, 2018). To examine this possibility, we first demonstrate that subsidy receipt does not impact cost-sharing. Table A8 in the Appendix summarizes the average effect estimates of newly receiving subsidies on the amount of healthcare costs covered by the consumer through cost-sharing. The results suggest that the observed decline in out-of-pocket spending is entirely driven by lower net premiums, as we do not find evidence that cost-sharing is affected by receiving premium subsidies. It also follows that the additional costs from the direct effect of receiving subsidies on utilization, which occurs on the intensive margin, are largely insurer-covered. Furthermore, the estimated direct effect of subsidies on total healthcare costs exceeds the indirect effect operating through deductible choice.[31] At the same time, it should be noted that the average direct effect of health insurance premium subsidies on total healthcare costs is still small compared to the variability in costs observed for the treated units (around 1/15th of one standard deviation).

Receiving a subsidy could, in principle, also create an incentive to seek low-value or unnecessary care. While our data do not allow us to directly value healthcare demand, our second-stage results do not support evidence that subsidies induce excess utilization consistent with moral hazard. Although out-of-pocket costs decrease after receiving subsidies, consumers on average spend less than the entire subsidy amount (either in additional healthcare or for more expensive health plans). We further observe different pre-treatment trends between treatment and control groups (despite having a sample-building procedure designed to produce similar groups), which the subsidy appears to counteract. In settings without diverging pre-treatment patterns, treatment effects are generally insignificant, suggesting that the observed demand responses are unlikely to reflect moral hazard. A remaining channel for moral hazard could stem from the increased likelihood of choosing the lowest deductible in the first stage; however, the effect of deductible choice on healthcare spending is likely limited given the small effect size found in the first-stage estimates.

Some limitations concerning the data need to be mentioned. First, we do not directly observe health status, which prevents us from drawing conclusions on the outcomes and also prevents us from assessing the optimality of the consumers' decisions. Second, we observe the statistics on compulsory health

[31]With a back-of-the-envelope calculation using the coefficients on the deductible levels in Table 4 for the outcome healthcare costs and the estimated first-stage effect of receiving subsidies on the probability of choosing the lowest deductible (1.5 pp), the average indirect effect through the deductible choice can be approximated to be in the range of CHF 17 to CHF 45, only considering switches to the lowest deductible. Compared to the estimated average direct effect of CHF 288.6, the approximate average indirect effect is substantially lower.

insurance over five years. For data privacy reasons, these statistics cannot be obtained for longer time periods. The chosen design and the anticipation period leave us with two periods before and three periods after the treatment switch occurs, and even less for out-of-pocket costs. The validity of the results could be enhanced by extending the observation period, thereby capturing potential inertia in consumer behavior. The limited number of pre-treatment periods also restricts the ability to assess pre-trends and the validity of pre-trend extrapolations. Furthermore, the COVID-19 pandemic began during the observation period, which may raise bias concerns. The limited observation period does not allow for robustness checks concerning the pandemic, yet the staggered DID design may alleviate some of these concerns, as not all individuals are treated at the same time. We acknowledge, however, that the optimal solution would explicitly test potential effects of the pandemic. Third, we only observe gross household income and cannot calculate the significant household income that is used by the cantons to determine subsidy eligibility. Observing significant income would allow for more robust identification strategies, e.g., in cantons that use a step-wise subsidy schedule, a regression discontinuity design could be applied. Fourth, our measure of healthcare costs is restricted to those reported to the insurer. Especially, consumers with higher deductible levels could refrain from reporting costs incurred under the *tiers garant* system if they do not expect to reach the deductible threshold. We also cannot distinguish between different types of healthcare costs (e.g., outpatient vs. inpatient costs) and the number of cost-generating episodes. Finally, the reported costs are borne either by the insurer or the consumer and do not include shares borne by other parties, e.g., the cantonal shares for inpatient services[32]. Thus, the healthcare cost variable does not capture the entire impact on the healthcare system.

To conclude, this study provides new evidence on how low-income households adjust their healthcare behavior in response to changes in the affordability of insurance coverage. While institutional features of Switzerland, such as compulsory coverage and regional variation in subsidy design, shape the specific magnitude of our results, the underlying mechanisms are relevant to a broad range of regulated insurance markets. Our findings underscore the importance of considering both liquidity constraints and behavioral responses when designing income-based subsidies, while considering efficiency, equity, and moral hazard. Understanding these behavioral responses is essential for evaluating the welfare implications of premium support programs, both in systems with mandatory participation and in those relying on voluntary enrollment. Future research could extend these insights along several dimensions by comparing subsidy responsiveness across different institutional settings, exploiting richer administrative data on subsidy generosity, household health risk, and disaggregated healthcare consumption.

[32] Art. 49a KVG

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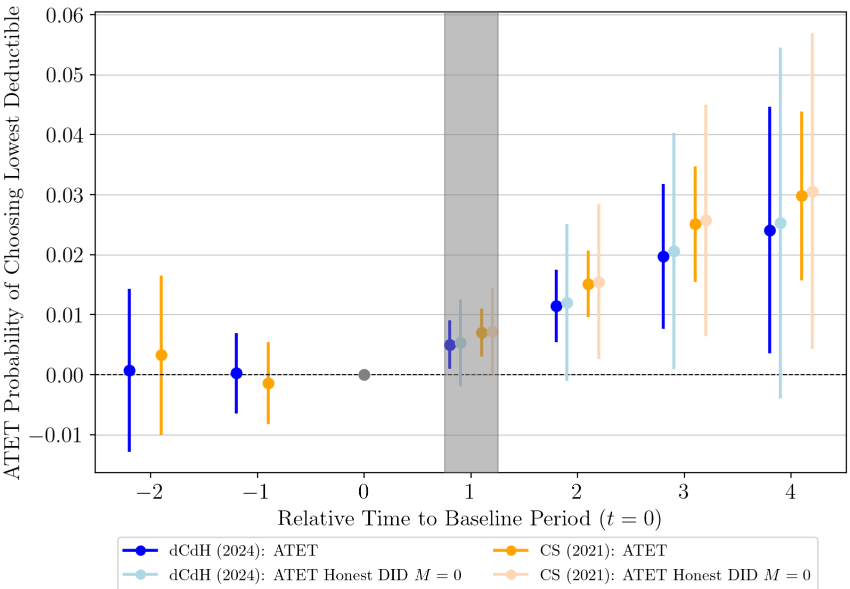
A Appendix

A.1 Comparison of staggered DID estimators

FIGURE A1: ATET OF RECEIVING SUBSIDIES ON PROBABILITY OF CHOOSING LOWEST DEDUCTIBLE BY PROBABILITY OF CHOOSING LOWEST DEDUCTIBLE, BY STAGGERED DID METHOD, EVENT STUDY



Notes: Visualization of $ATET_t$ estimates by de Chaisemartin and D'Haultfœuille (2024) and Callaway and Sant'Anna (2021) with 95% confidence intervals. Individual and year fixed effects are included. For the de Chaisemartin and D'Haultfœuille (2024) estimator, treated individuals are compared to untreated individuals within the same canton. Further controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends.

A.2 Control function with ordered probit reduced form

Wooldridge (2015) establishes a control function approach with a probit reduced form in a setting with

$$y_1 = \boldsymbol{z_1\delta_1} + \gamma_1 y_2 + u_1 \tag{2}$$

$$y_2 = 1[\boldsymbol{z\delta_2} + e_2 > 0], \tag{3}$$

where y_1 is the response variable and y_2 the endogenous explanatory variable. $\boldsymbol{z}$ is a vector of exogenous variables including unity, while $\boldsymbol{z_1}$ is a subvector of $\boldsymbol{z = (z_1, z_2)}$ and also contains unity. Then,

$$E(y_1|\boldsymbol{z}, y_2) = \boldsymbol{z_1\delta_1} + \gamma_1 y_2 + \eta_1[y_2\lambda(\boldsymbol{z\delta_2}) - (1 - y_2)\lambda(-\boldsymbol{z\delta_2})], \tag{4}$$

where $\lambda(\cdot) = \phi(\cdot)/\Phi(\cdot)$ denotes the inverse Mills ratio. The generalized error is

$$r(y_2, \boldsymbol{z\delta_2}) \equiv y_2\lambda(\boldsymbol{z\delta_2}) - (1 - y_2)\lambda(-\boldsymbol{z\delta_2}). \tag{5}$$

In the first stage, Wooldridge (2015) estimates the probit model

$$P(y_2 = 1|\boldsymbol{z}) = \Phi(\boldsymbol{z\delta_2}) \tag{6}$$

to get the generalized residuals

$$\hat{r}_{i2} \equiv y_{i2}\lambda(\boldsymbol{z_i\hat{\delta}_2}) - (1 - y_{i2})\lambda(-\boldsymbol{z_i\hat{\delta}_2}), \quad i = 1, \ldots, N. \tag{7}$$

In the second stage, Wooldridge (2015) consistently estimates $\boldsymbol{\delta_1}$, γ_1, and η_1 by running the OLS regression

$$y_{i1} \text{ on } \boldsymbol{z_{i1}}, y_{i2}, \hat{r}_{i2}, \quad i = 1, \ldots, N. \tag{8}$$

In our setting, y_2 corresponds to the deductible level, which is not binary but instead an ordered categorical variable with six levels. We replace the first-stage probit model in Equation 6 with an ordered probit. Using the notation in Wooldridge (2015), the ordered-probit selection rule as defined for example in Chiburis and Lokshin (2007) is

$$y_{i2}^* = \boldsymbol{z\delta_2} + e_2 \tag{9}$$

$$y_{i2} = \begin{cases} 0 & \text{if } -\infty < y_{i2}^* \leq \mu_1, \\ 1 & \text{if } \mu_1 < y_{i2}^* \leq \mu_2, \\ 2 & \text{if } \mu_2 < y_{i2}^* \leq \mu_3, \\ \vdots & \\ J & \text{if } \mu_J < y_{i2}^* < \infty, \end{cases} \tag{10}$$

where $\mu_1 < \mu_2 < \cdots < \mu_J$. Translating to the notation in Wooldridge (2015), the generalized residuals in an ordered probit according to Vella (1993) are

$$E(e_2|d_{ji}) = d_{ji}\hat{\pi}_{ji}\hat{\Pi}_{ji}^{-1}(1 - \hat{\Pi}_{ji})^{-1}(d_{ji} - \hat{\Pi}_{ji}), \tag{11}$$

where $d_{ji} = 1[y_{i2} = j]$, $\hat{\Pi}_{ji}$ is the estimated probability of i being in j, and $\hat{\pi}_{ji}$ the estimated density at that point. We use the ordered probit generalized residuals in the second-stage regression (Equation 8 and its corresponding adaption for the staggered DID estimator by de Chaisemartin and D'Haultfœuille (2024)).

A.3 Additional outcomes first stage

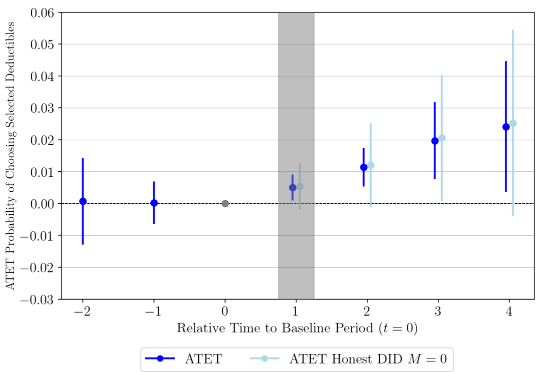
Table A1: ATET of Receiving Subsidies on Probability of Choosing Lowest Deductible and Selected Middle Deductibles

	(1)	(2)	(3)	(4)
Deductible Level(s)	300	300, 500	300, 500, 1,000	300, 500, 1,000, 1,500
ATET	0.015***	0.018***	0.018***	0.016***
	(0.004)	(0.004)	(0.004)	(0.005)
ATET Honest DID $M = 0$	0.016**	0.015*	0.014	0.010
	(0.008)	(0.009)	(0.009)	(0.009)
Honest DID Breakdown	< 0.001	–	–	–
Unit FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Individuals	210,160	210,160	210,160	210,160
Switchers	10,913	10,913	10,913	10,913

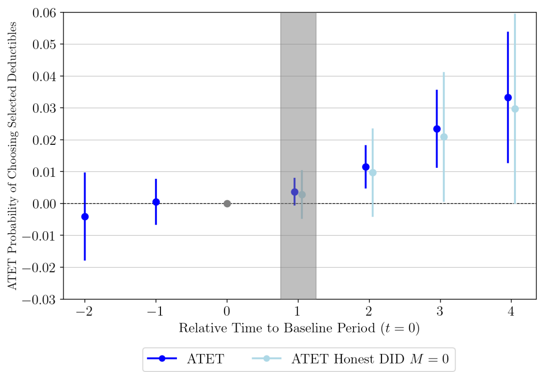
Notes: Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Coefficients represent $ATET_t$ estimates by de Chaisemartin and D'Haultfœuille (2024). Standard errors in parentheses (clustered at the household level). Individual and year fixed effects are included. Treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. Average effect considers periods $t = 2$ to $t = 4$ and excludes the anticipation period $t = 1$. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends; $M > 0$ accounts for deviations from linearity; breakdown value is the highest M for which $p < 0.05$; breakdown value search from 0 to 0.05 in intervals of 0.001.

Figure A2: ATET of Receiving Subsidies on Probability of Choosing Lowest Deductible and Selected Middle Deductibles, Event Studies

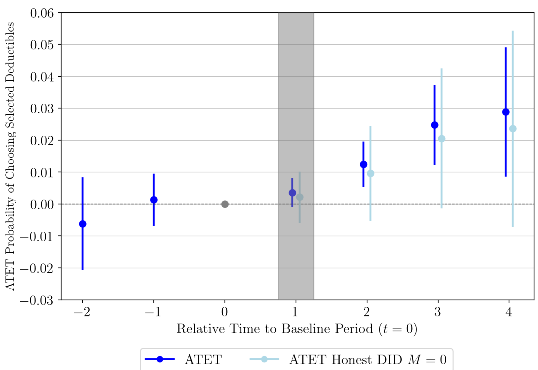
(a) 300



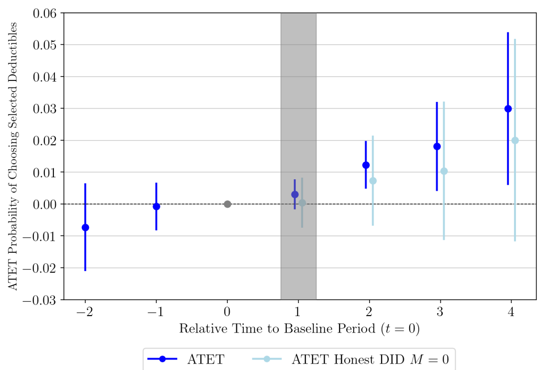
(b) 300, 500



(c) 300, 500, 1,000



(d) 300, 500, 1,000, 1,500

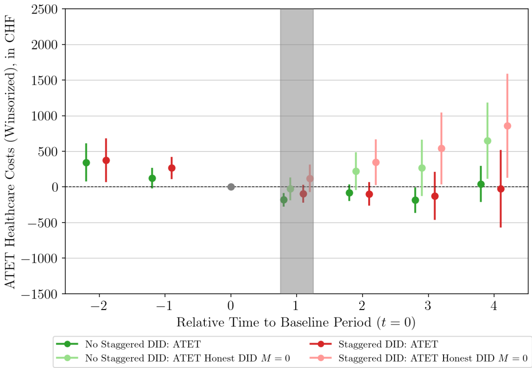


Notes: Visualization of $ATET_t$ estimates by de Chaisemartin and D'Haultfœuille (2024) with 95% confidence intervals. Individual and year fixed effects are included. Treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends.

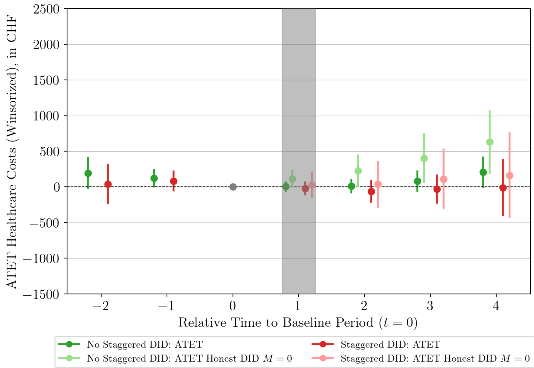
A.4 Subsamples

Figure A3: ATET of Receiving Subsidies on Healthcare Costs (Winsorized), in CHF, Control Function, Event Studies by Subsample

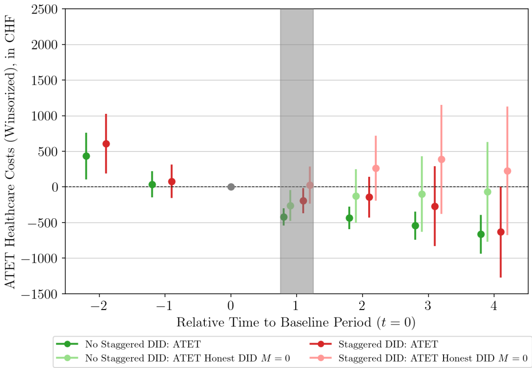
(a) Female



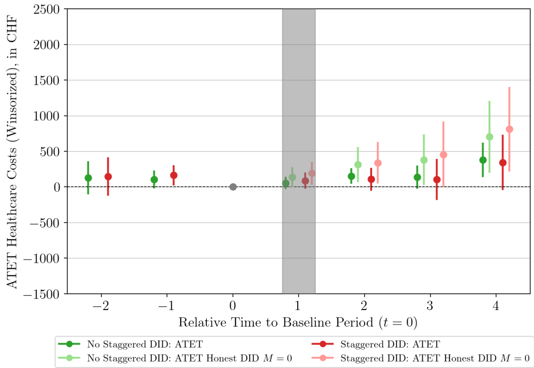
(b) Male



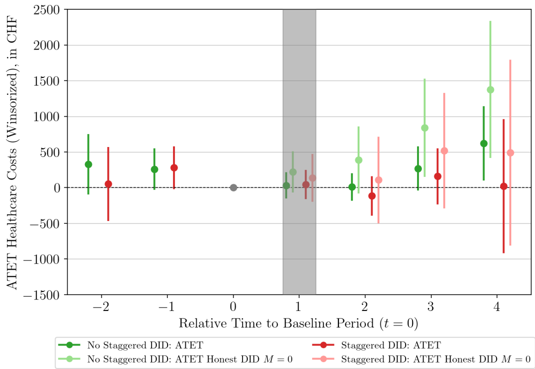
(c) Birth Year 83-96



(d) Birth Year 69-82

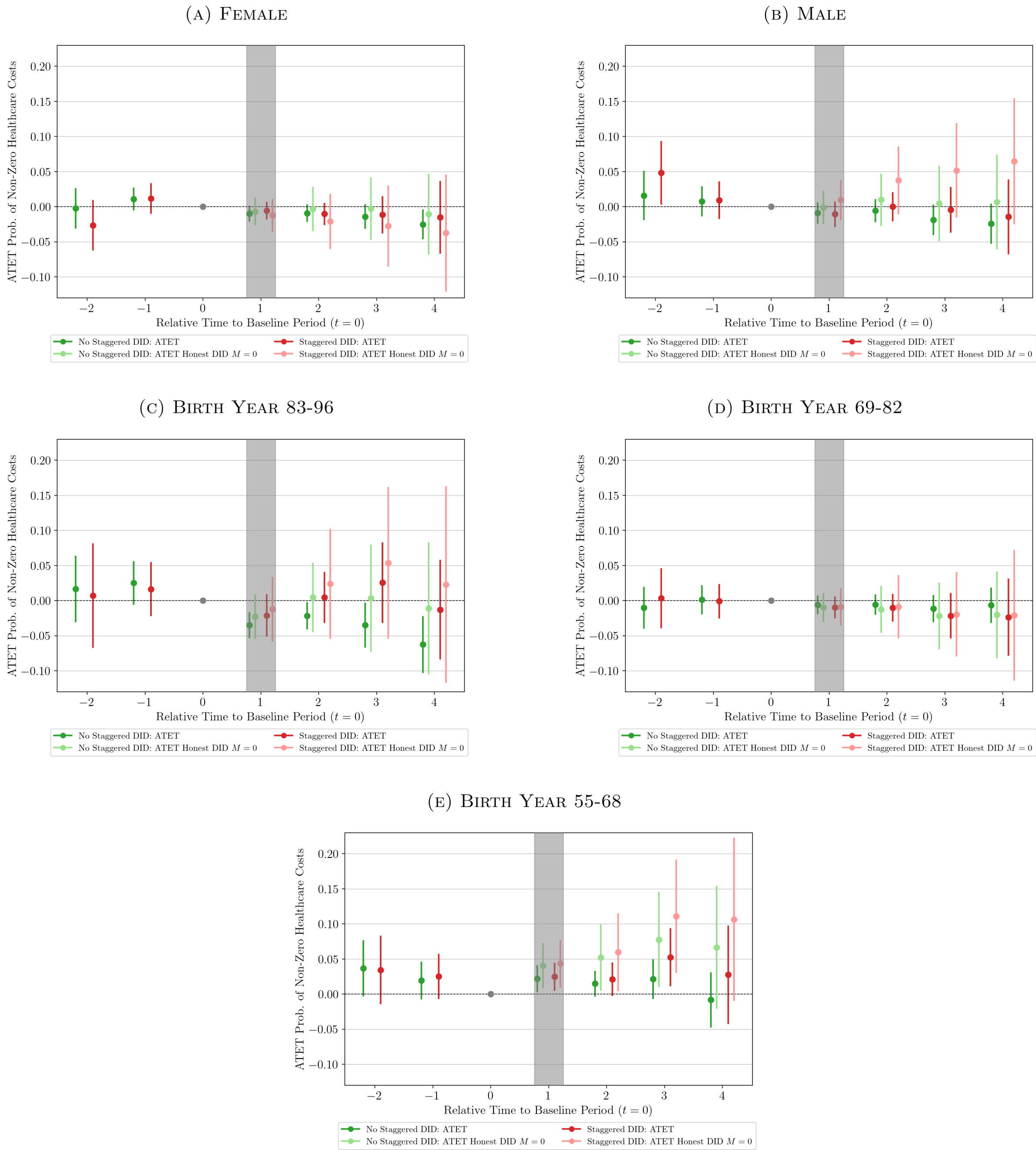


(e) Birth Year 55-68



Notes: Visualization of ATET estimates with 95% confidence intervals from a control function approach with an ordered probit for deductible choice in the first stage and OLS or the de Chaisemartin and D'Haultfœuille (2024) staggered DID estimator in the second stage. Standard errors are bootstrapped with 200 repetitions. Individual and year fixed effects are included. For the staggered DID, treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. The instrument for the control function approach is a binary variable indicating whether someone's gross premium in $t-1$ is, within the subgroup of individuals with the same choice set and choices, above the 50th percentile, i.e., above the median. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends.

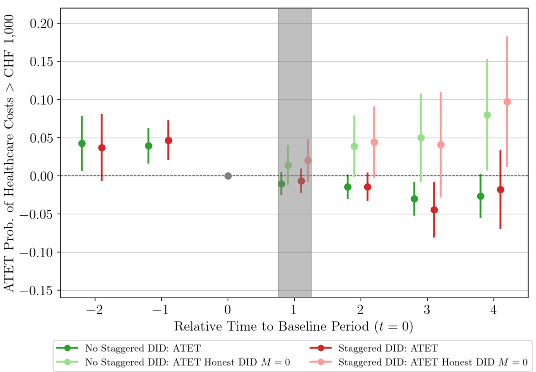
Figure A4: ATET of Receiving Subsidies on Non-Zero Healthcare Costs (Binary), Control Function, Event Studies by Subsample



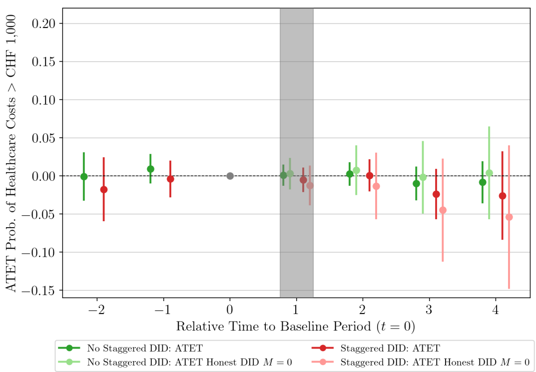
Notes: Visualization of ATET estimates with 95% confidence intervals from a control function approach with an ordered probit for deductible choice in the first stage and OLS or the de Chaisemartin and D'Haultfœuille (2024) staggered DID estimator in the second stage. Standard errors are bootstrapped with 200 repetitions. Individual and year fixed effects are included. For the staggered DID, treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. The instrument for the control function approach is a binary variable indicating whether someone's gross premium in $t-1$ is, within the subgroup of individuals with the same choice set and choices, above the 50th percentile, i.e., above the median. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends.

Figure A5: ATET of Receiving Subsidies on Healthcare Costs > CHF 1,000 (Binary), Control Function, Event Studies by Subsample

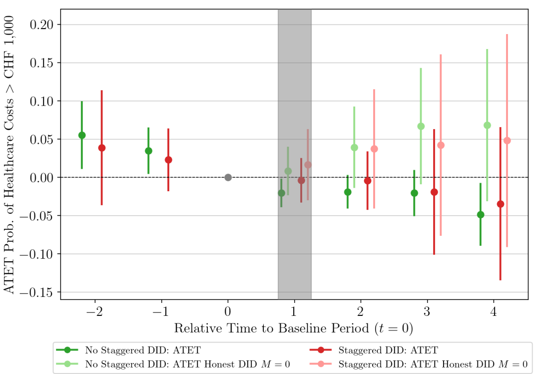
(a) Female



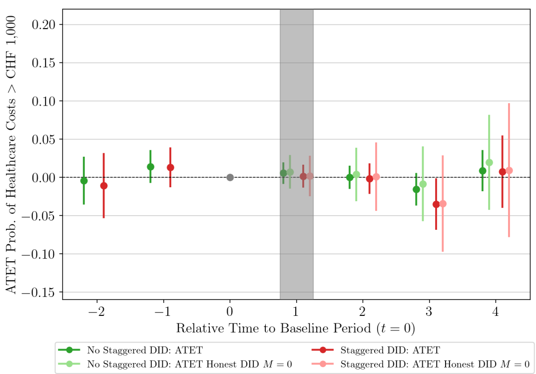
(b) Male



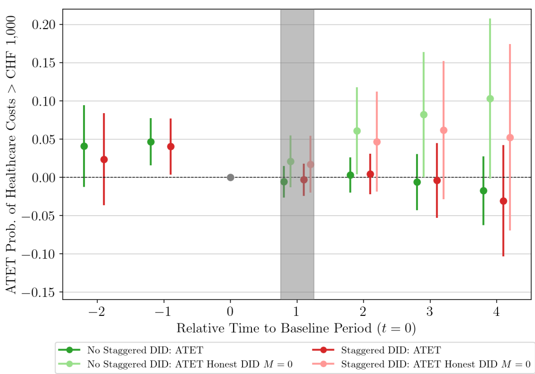
(c) Birth Year 83-96



(d) Birth Year 69-82



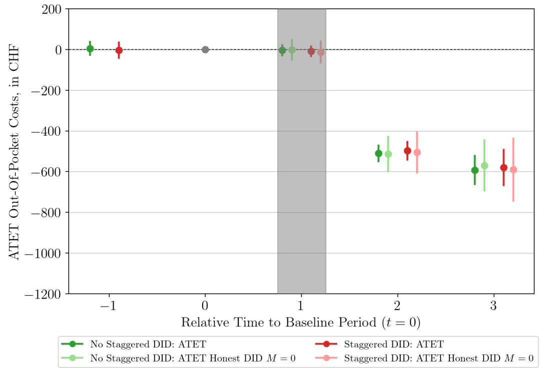
(e) Birth Year 55-68



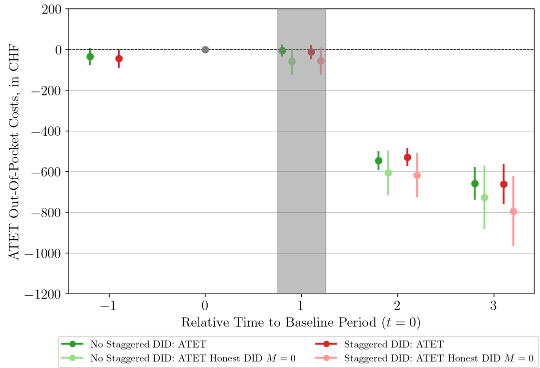
Notes: Visualization of ATET estimates with 95% confidence intervals from a control function approach with an ordered probit for deductible choice in the first stage and OLS or the de Chaisemartin and D'Haultfœuille (2024) staggered DID estimator in the second stage. Standard errors are bootstrapped with 200 repetitions. Individual and year fixed effects are included. For the staggered DID, treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. The instrument for the control function approach is a binary variable indicating whether someone's gross premium in $t-1$ is, within the subgroup of individuals with the same choice set and choices, above the 50th percentile, i.e., above the median. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends.

Figure A6: ATET of Receiving Subsidies on Out-Of-Pocket Costs, in CHF, Control Function, Event Studies by Subsample

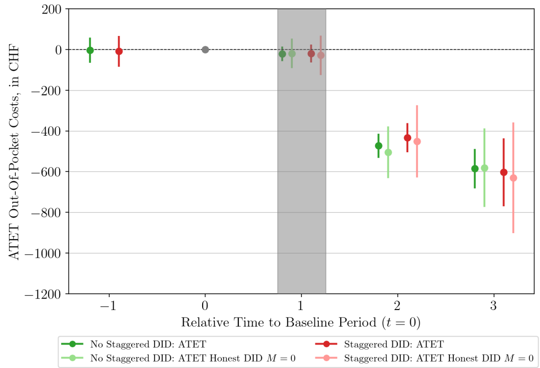
(a) Female



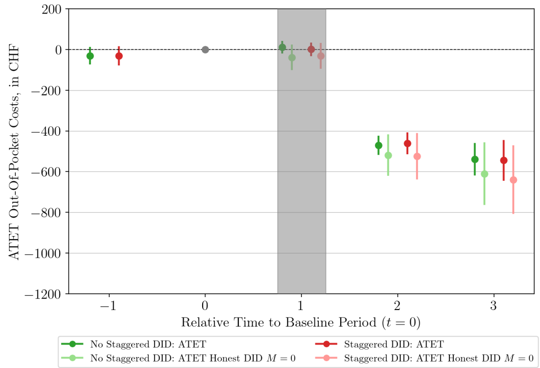
(b) Male



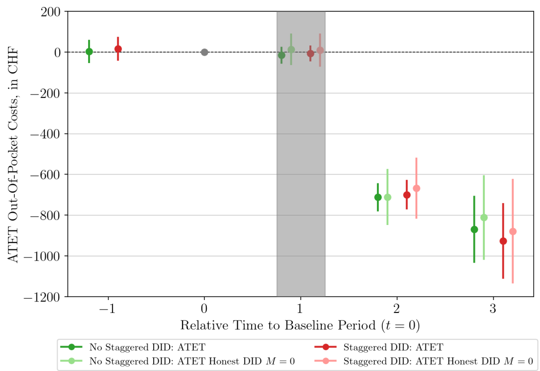
(c) Birth Year 83-96



(d) Birth Year 69-82



(e) Birth Year 55-68



Notes: Visualization of ATET estimates with 95% confidence intervals from a control function approach with an ordered probit for deductible choice in the first stage and OLS or the de Chaisemartin and D'Haultfœuille (2024) staggered DID estimator in the second stage. Standard errors are bootstrapped with 200 repetitions. Individual and year fixed effects are included. For the staggered DID, treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. The instrument for the control function approach is a binary variable indicating whether someone's gross premium in $t-1$ is, within the subgroup of individuals with the same choice set and choices, above the 50th percentile, i.e., above the median. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends. Due to data availability restrictions regarding out-of-pocket costs for 2018, periods $t = -2$ and $t = 4$ cannot be computed. While the *did_multiplegt_dyn*-package in Stata (de Chaisemartin et al., 2025) considers this automatically, we manually exclude units first treated in 2020 for the out-of-pocket costs in the specification that does not account for staggered treatment timing, as we do not observe their outcome at baseline.

A.5 Robustness checks

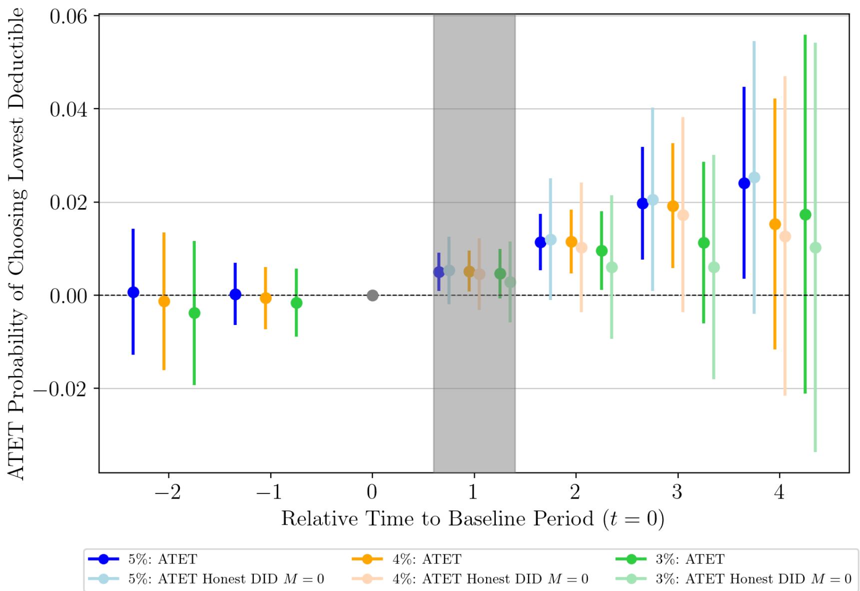
A.5.1 Income growth constraints

TABLE A2: ATET OF RECEIVING SUBSIDIES ON PROBABILITY OF CHOOSING LOWEST DEDUCTIBLE, DIFFERENT SIGNIFICANT INCOME GROWTH RANGES

	5%	4%	3%
ATET	0.015***	0.014***	0.011*
	(0.004)	(0.005)	(0.006)
ATET Honest DID $M = 0$	0.016**	0.012	0.007
	(0.008)	(0.009)	(0.010)
Honest DID Breakdown	< 0.001	–	–
Unit FE	✓	✓	✓
Year FE	✓	✓	✓
Individuals	210,160	175,480	132,190
Switchers	10,913	9,621	7,937

Notes: Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Coefficients represent $ATET_t$ estimates by de Chaisemartin and D'Haultfœuille (2024). Standard errors in parentheses (clustered at the household level). Individual and year fixed effects are included. Treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. Average effect considers periods $t = 2$ to $t = 4$ and excludes the anticipation period $t = 1$. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends; $M > 0$ accounts for deviations from linearity; breakdown value is the highest M for which $p < 0.05$; breakdown value search from 0 to 0.05 in intervals of 0.001.

Figure A7: ATET of Receiving Subsidies on Probability of Choosing Lowest Deductible, Different Significant Income Growth Ranges, Event Study



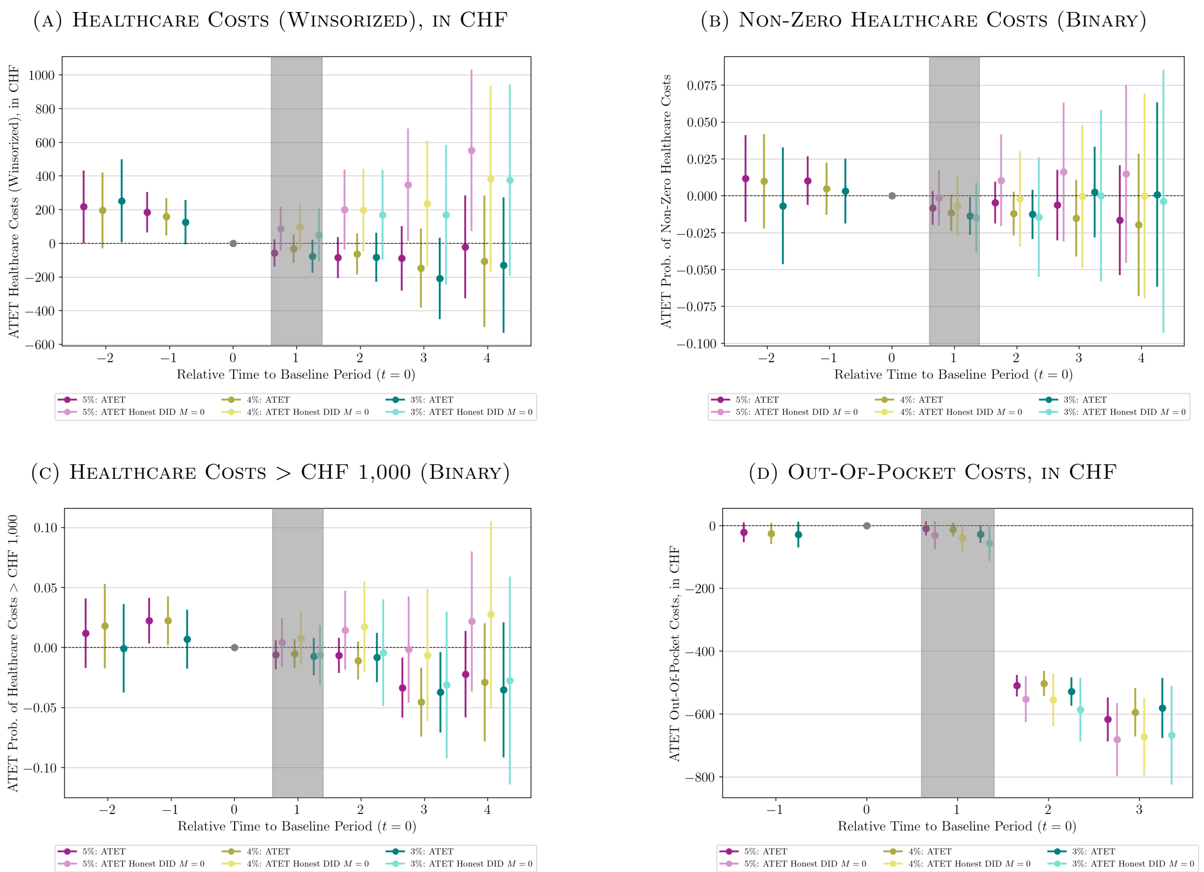
Notes: Visualization of $ATET_t$ estimates by de Chaisemartin and D'Haultfœuille (2024) with 95% confidence intervals. Individual and year fixed effects are included. Treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends.

Table A3: ATET of Receiving Subsidies on Healthcare Demand, Different Significant Income Growth Ranges

	(1) Healthcare Costs (Winsorized), in CHF			(2) Non-Zero Healthcare Costs (Binary)		
	5%	4%	3%	5%	4%	3%
ATET	−77.3	−92.0	−122.6	−0.007	−0.014	−0.007
	(66.9)	(76.3)	(84.6)	(0.008)	(0.009)	(0.010)
ATET Honest DID $M = 0$	288.6**	234.0	197.7	0.013	−0.001	−0.009
	(142.3)	(152.5)	(165.6)	(0.019)	(0.020)	(0.025)
Honest DID Breakdown	8	–	–	–	–	–
Bootstrapped SE	✓	✓	✓	✓	✓	✓
Unit FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Individuals	190,608	159,210	119,846	190,609	159,211	119,846
Switchers	10,045	8,854	7,297	10,045	8,854	7,297
	(3) Healthcare Costs > CHF 1,000 (Binary)			(4) Out-Of-Pocket Costs, in CHF		
	5%	4%	3%	5%	4%	3%
ATET	−0.016**	−0.023**	−0.020*	−534.3***	−524.2***	−540.6***
	(0.008)	(0.010)	(0.011)	(18.9)	(21.8)	(26.1)
ATET Honest DID $M = 0$	0.011	0.012	−0.015	−582.2***	−581.9***	−604.8***
	(0.019)	(0.023)	(0.026)	(41.0)	(46.3)	(56.7)
Honest DID Breakdown	–	–	–	> 50	> 50	> 50
Bootstrapped SE	✓	✓	✓	✓	✓	✓
Unit FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Individuals	190,608	159,210	119,846	96,652	78,668	57,653
Switchers	10,045	8,854	7,297	4,422	3,904	3,209

Notes: Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Coefficients represent ATET estimates from a control function approach with an ordered probit for deductible choice in the first stage and the de Chaisemartin and D'Haultfœuille (2024) staggered DID estimator in the second stage. Standard errors in parentheses (clustered at the household level and bootstrapped with 200 repetitions). Individual and year fixed effects are included. Treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. Average effect considers periods $t = 2$ to $t = 4$ and excludes the anticipation period $t = 1$. The instrument for the control function approach is a binary variable indicating whether someone's gross premium in $t - 1$ is, within the subgroup of individuals with the same choice set and choices, above the 50th percentile, i.e., above the median. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends; $M > 0$ accounts for deviations from linearity; breakdown value is the highest M for which $p < 0.05$; (1) and (4) breakdown value search from 0 to 50 in intervals of 1; (2) and (3) breakdown value search from 0 to 0.05 in intervals of 0.001. Due to data availability restrictions regarding out-of-pocket costs for 2018, periods $t = -2$ and $t = 4$ cannot be computed. The average effect on out-of-pocket costs considers periods $t = 2$ and $t = 3$.

Figure A8: ATET of Receiving Subsidies on Healthcare Demand, by Different Significant Income Growth Ranges, Event Studies



Notes: Visualization of ATET estimates with 95% confidence intervals from a control function approach with an ordered probit for deductible choice in the first stage and the de Chaisemartin and D'Haultfœuille (2024) staggered DID estimator in the second stage. Standard errors are bootstrapped with 200 repetitions. Individual and year fixed effects are included. Treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. The instrument for the control function approach is a binary variable indicating whether someone's gross premium in $t-1$ is, within the subgroup of individuals with the same choice set and choices, above the 50th percentile, i.e., above the median. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends. Due to data availability restrictions regarding out-of-pocket costs for 2018, periods $t = -2$ and $t = 4$ cannot be computed in panel (D).

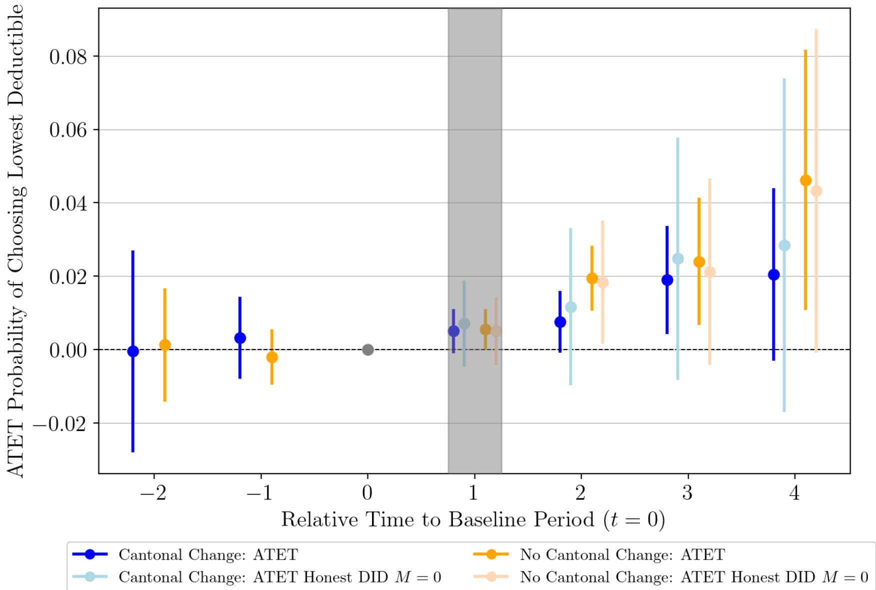
A.5.2 Cantonal premium subsidy system changes

Table A4: ATET of Receiving Subsidies on Probability of Choosing Lowest Deductible, by Cantonal Subsidy System Change in Year of Treatment Switch

	Cantonal Change	No Cantonal Change
ATET	0.013**	0.022***
	(0.005)	(0.005)
ATET Honest DID $M = 0$	0.018	0.021**
	(0.014)	(0.009)
Honest DID Breakdown	–	< 0.001
Unit FE	✓	✓
Year FE	✓	✓
Individuals	87,217	120,666
Switchers	7,082	3,826

Notes: Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Coefficients represent $ATET_t$ estimates by de Chaisemartin and D'Haultfœuille (2024). Standard errors in parentheses (clustered at the household level). Individual and year fixed effects are included. Treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. Average effect considers periods $t = 2$ to $t = 4$ and excludes the anticipation period $t = 1$. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends; $M > 0$ accounts for deviations from linearity; breakdown value is the highest M for which $p < 0.05$; breakdown value search from 0 to 0.05 in intervals of 0.001.

Figure A9: ATET of Receiving Subsidies on Probability of Choosing Lowest Deductible, by Cantonal Subsidy System Change in Year of Treatment Switch, Event Study



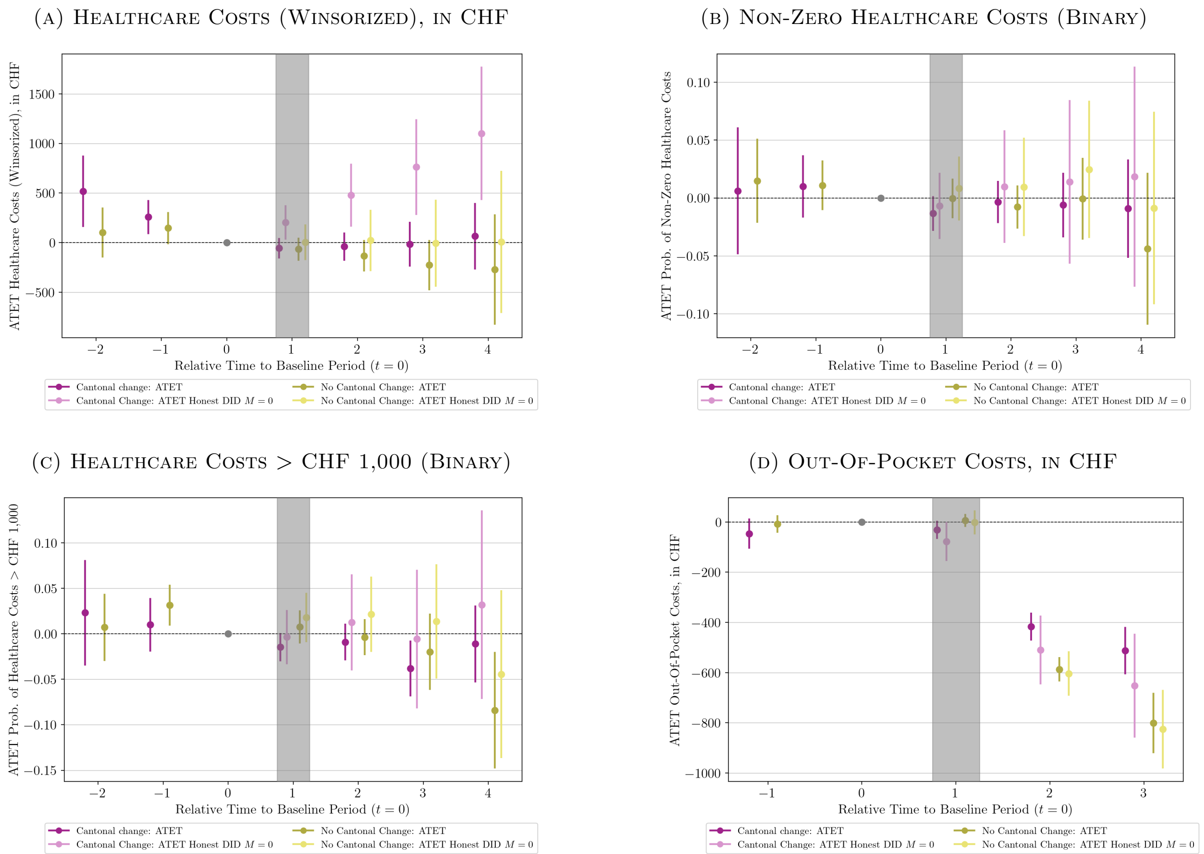
Notes: Visualization of $ATET_t$ estimates by de Chaisemartin and D'Haultfœuille (2024) with 95% confidence intervals. Individual and year fixed effects are included. Treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends.

Table A5: ATET of Receiving Subsidies on Healthcare Demand, by Cantonal Subsidy System Change in Year of Treatment Switch

	(1) Healthcare Costs (Winsorized), in CHF		(2) Non-Zero Healthcare Costs (Binary)		(3) Healthcare Costs > CHF 1,000 (Binary)		(4) Out-Of-Pocket Costs, in CHF	
	Cantonal Change	No Cantonal Change	Cantonal Change	No Cantonal Change	Cantonal Change	No Cantonal Change	Cantonal Change	No Cantonal Change
ATET	−14.6	−161.8*	−0.005	−0.009	−0.018*	−0.013	−445.3***	−621.9***
	(78.0)	(83.6)	(0.010)	(0.010)	(0.011)	(0.011)	(30.1)	(26.2)
ATET Honest DID $M = 0$	664.2***	15.9	0.012	0.011	0.011	0.015	−552.2***	−639.9***
	(206.7)	(174.2)	(0.031)	(0.023)	(0.034)	(0.024)	(79.0)	(47.9)
Honest DID Breakdown	> 50	–	–	–	–	–	> 50	> 50
Bootstrapped SE	✓	✓	✓	✓	✓	✓	✓	✓
Unit FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Individuals	79,370	109,203	79,370	109,204	79,370	109,203	32,459	63,470
Switchers	6,530	3,510	6,530	3,510	6,530	3,510	1,914	2,506

Notes: Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Coefficients represent ATET estimates from a control function approach with an ordered probit for deductible choice in the first stage and the de Chaisemartin and D'Haultfœuille (2024) staggered DID estimator in the second stage. Standard errors in parentheses (clustered at the household level and bootstrapped with 200 repetitions). Individual and year fixed effects are included. Treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. Average effect considers periods $t = 2$ to $t = 4$ and excludes the anticipation period $t = 1$. The instrument for the control function approach is a binary variable indicating whether someone's gross premium in $t - 1$ is, within the subgroup of individuals with the same choice set and choices, above the 50th percentile, i.e., above the median. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends; $M > 0$ accounts for deviations from linearity; breakdown value is the highest M for which $p < 0.05$; (1) and (4) breakdown value search from 0 to 50 in intervals of 1; (2) and (3) breakdown value search from 0 to 0.05 in intervals of 0.001. Due to data availability restrictions regarding out-of-pocket costs for 2018, periods $t = -2$ and $t = 4$ cannot be computed. The average effect on out-of-pocket costs considers periods $t = 2$ and $t = 3$.

Figure A10: ATET of Receiving Subsidies on Healthcare Demand, by Cantonal Subsidy System Change in Year of Treatment Switch, Event Studies



Notes: Visualization of ATET estimates with 95% confidence intervals from a control function approach with an ordered probit for deductible choice in the first stage and the de Chaisemartin and D'Haultfœuille (2024) staggered DID estimator in the second stage. Standard errors are bootstrapped with 200 repetitions. Individual and year fixed effects are included. Treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. The instrument for the control function approach is a binary variable indicating whether someone's gross premium in $t-1$ is, within the subgroup of individuals with the same choice set and choices, above the 50th percentile, i.e., above the median. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends. Due to data availability restrictions regarding out-of-pocket costs for 2018, periods $t = -2$ and $t = 4$ cannot be computed in panel (D).

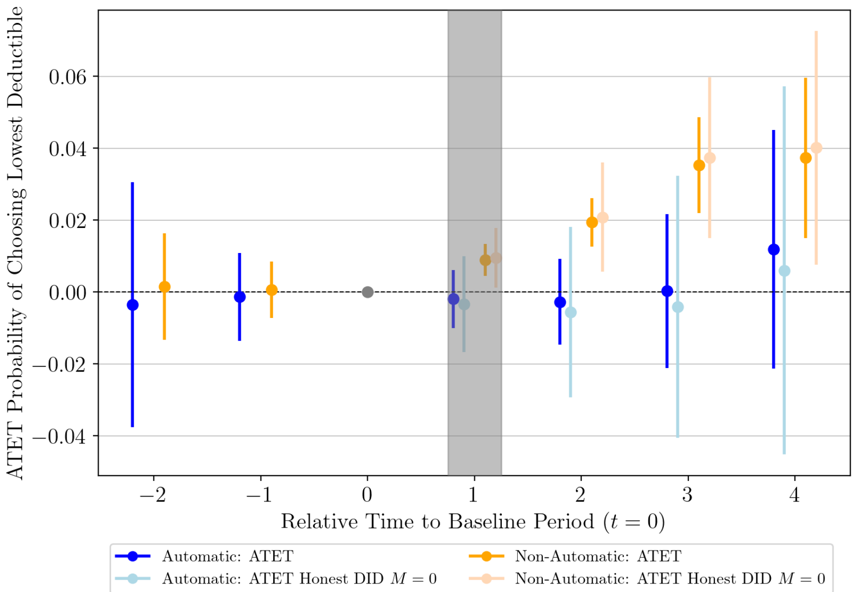
A.5.3 (Non-)automatic payment of premium subsidies

TABLE A6: ATET OF RECEIVING SUBSIDIES ON PROBABILITY OF CHOOSING LOWEST DEDUCTIBLE, BY AUTOMATIC AND NON-AUTOMATIC CANTONS

	Automatic	Non-Automatic
ATET	0.001	0.025***
	(0.008)	(0.004)
ATET Honest DID $M = 0$	−0.003	0.027***
	(0.016)	(0.009)
Honest DID Breakdown	–	0.002
Unit FE	✓	✓
Year FE	✓	✓
Individuals	66,345	143,815
Switchers	3,933	6,980

Notes: Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Coefficients represent $ATET_t$ estimates by de Chaisemartin and D'Haultfœuille (2024). Standard errors in parentheses (clustered at the household level). Individual and year fixed effects are included. Treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. Average effect considers periods $t = 2$ to $t = 4$ and excludes the anticipation period $t = 1$. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends; $M > 0$ accounts for deviations from linearity; breakdown value is the highest M for which $p < 0.05$; breakdown value search from 0 to 0.05 in intervals of 0.001.

Figure A11: ATET of Receiving Subsidies on Probability of Choosing Lowest Deductible, by Automatic and Non-Automatic Cantons, Event Study



Notes: Visualization of $ATET_t$ estimates by de Chaisemartin and D'Haultfœuille (2024) with 95% confidence intervals. Individual and year fixed effects are included. Treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends.

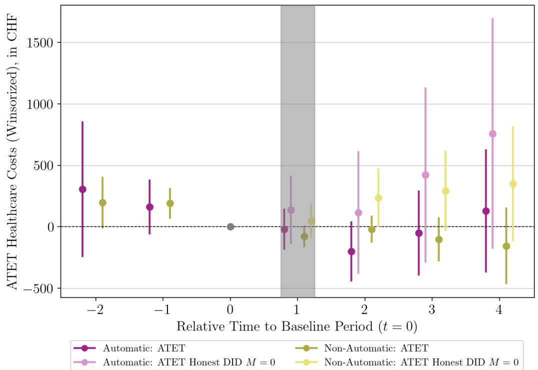
Table A7: ATET of Receiving Subsidies on Healthcare Demand, by Automatic and Non-Automatic Cantons

	(1) Healthcare Costs (Winsorized), in CHF		(2) Non-Zero Healthcare Costs (Binary)		(3) Healthcare Costs > CHF 1,000 (Binary)		(4) Out-Of-Pocket Costs, in CHF	
	Automatic	Non-Automatic	Automatic	Non-Automatic	Automatic	Non-Automatic	Automatic	Non-Automatic
ATET	−96.3	−55.1	−0.014	−0.001	−0.021	−0.012	-727.6^{***}	-473.7^{***}
	(125.1)	(61.8)	(0.014)	(0.007)	(0.017)	(0.008)	(43.6)	(24.2)
ATET Honest DID $M = 0$	323.1	261.6^{*}	−0.020	0.023	0.082^{*}	−0.002	-835.5^{***}	-509.1^{***}
	(310.0)	(138.5)	(0.043)	(0.017)	(0.049)	(0.023)	(100.7)	(50.1)
Honest DID Breakdown	–	–	–	–	–	–	> 50	> 50
Bootstrapped SE	✓	✓	✓	✓	✓	✓	✓	✓
Unit FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Individuals	59,910	130,698	59,910	130,699	59,910	130,698	31,563	65,089
Switchers	3,613	6,432	3,613	6,432	3,613	6,432	974	3,448

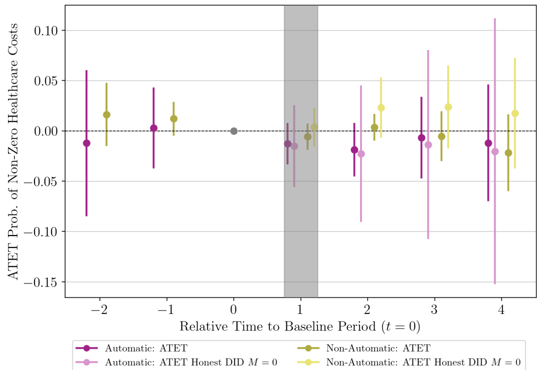
Notes: Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Coefficients represent ATET estimates from a control function approach with an ordered probit for deductible choice in the first stage and the de Chaisemartin and D'Haultfœuille (2024) staggered DID estimator in the second stage. Standard errors in parentheses (clustered at the household level and bootstrapped with 200 repetitions). Individual and year fixed effects are included. Treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. Average effect considers periods $t = 2$ to $t = 4$ and excludes the anticipation period $t = 1$. The instrument for the control function approach is a binary variable indicating whether someone's gross premium in $t - 1$ is, within the subgroup of individuals with the same choice set and choices, above the 50th percentile, i.e., above the median. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends; $M > 0$ accounts for deviations from linearity; breakdown value is the highest M for which $p < 0.05$; (1) and (4) breakdown value search from 0 to 50 in intervals of 1; (2) and (3) breakdown value search from 0 to 0.05 in intervals of 0.001. Due to data availability restrictions regarding out-of-pocket costs for 2018, periods $t = -2$ and $t = 4$ cannot be computed. The average effect on out-of-pocket costs considers periods $t = 2$ and $t = 3$.

Figure A12: ATET of Receiving Subsidies on Healthcare Demand, by Automatic and Non-Automatic Cantons, Event Studies

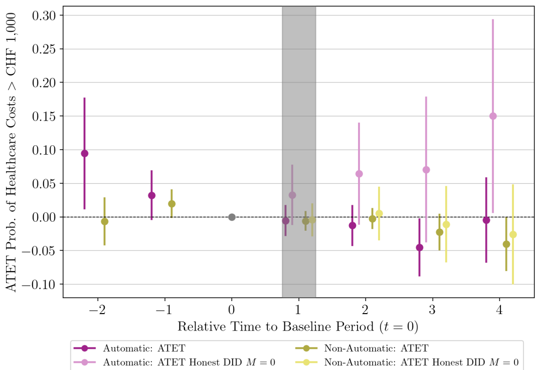
(a) Healthcare Costs (Winsorized), in CHF



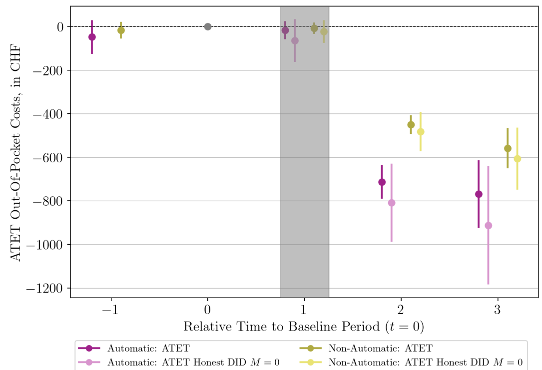
(b) Non-Zero Healthcare Costs (Binary)



(c) Healthcare Costs > CHF 1,000 (Binary)



(d) Out-Of-Pocket Costs, in CHF



Notes: Visualization of ATET estimates with 95% confidence intervals from a control function approach with an ordered probit for deductible choice in the first stage and the de Chaisemartin and D'Haultfœuille (2024) staggered DID estimator in the second stage. Standard errors are bootstrapped with 200 repetitions. Individual and year fixed effects are included. Treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. The instrument for the control function approach is a binary variable indicating whether someone's gross premium in $t-1$ is, within the subgroup of individuals with the same choice set and choices, above the 50th percentile, i.e., above the median. Honest DID (Rambachan & Roth, 2023): $M = 0$ allows for linear deviations of parallel trends. Due to data availability restrictions regarding out-of-pocket costs for 2018, periods $t = -2$ and $t = 4$ cannot be computed in panel (D).

A.6 Cost-sharing

Table A8: ATET of Receiving Subsidies on Cost-Sharing

Control Function	✗	✓	✓
Staggered DID	✓	✗	✓
ATET	5.735	−4.280	−2.612
	(9.410)	(7.635)	(10.734)
ATET Honest DID $M = 0$	−6.733	4.669	−24.369
	(21.998)	(20.853)	(23.421)
Honest DID Breakdown	–	–	–
Bootstrapped SE	✗	✓	✓
Unit FE	✓	✓	✓
Year FE	✓	✓	✓
Individuals	193,413	164,171	177,578
Switchers	10,286	11,107	9,546

Notes: Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Coefficients represent ATET estimates from the reduced form specification with the de Chaisemartin and D'Haultfœuille (2024) staggered DID estimator and from a control function approach with an ordered probit for deductible choice in the first stage and OLS or the de Chaisemartin and D'Haultfœuille (2024) staggered DID estimator in the second stage. Standard errors in parentheses (clustered at the household level and for control function specifications bootstrapped with 200 repetitions). Individual and year fixed effects are included. For estimates with the de Chaisemartin and D'Haultfœuille (2024) estimator, treated individuals are compared to untreated individuals within the same canton and with the same household type. Controls include inflation-adjusted gross household income in logs, Swiss citizenship, and squared age. Average effect considers periods $t = 2$ to $t = 4$ and excludes the anticipation period $t = 1$. The instrument for the control function approach is a binary variable indicating whether someone's gross premium in $t - 1$ is, within the subgroup of individuals with the same choice set and choices, above the 50th percentile, i.e., above the median. Honest DID Rambachan and Roth (2023): $M = 0$ allows for linear deviations of parallel trends; $M > 0$ accounts for deviations from linearity; breakdown value is the highest M for which $p < 0.05$; breakdown value search from 0 to 50 in intervals of 1.

B Online appendix

B.1 Income Types

TABLE B1: INCOME TYPES, ADDITIONS, AND DEDUCTIONS USED TO DETERMINE SUBSIDY ELIGIBILITY

Canton	Base	Additions and deductions
AG	taxable income ($t-3$)	+ 20% of taxable wealth + property maintenance + pillar 2 purchases + pillar 3a contributions + donations + political donations + losses of self-employed persons + additional social tax deduction
AI	taxable income ($t-2$)	+ 10% of taxable wealth + property maintenance and administration costs + pillar 2 purchases + pillar 3a contributions (2022) + income settled via federal law on combating illicit employment (BGSH) (2022) + income pursuant to Art. 22 and Art. 23 Abs. 1 StG is added up to 100%
AR	taxable income ($t-2$)	+ 15% of taxable wealth + pillar 2 purchases + pillar 3a contributions + property expenses + income settled via federal law on combating illicit employment (BGSH) + previous year's losses + membership fees and donations to political parties + voluntary payments to legal entities
BE	pure income ($t-2$)	+ 5% of pure wealth (− wealth deduction in 2021–2022) + pillar 2 purchases + pillar 3a contributions + two-earner deduction + non-taxable income + membership fees and donations + weekly stay away from home + property maintenance, if greater than 1% of the official value + negative net income from participations in communities of inheritance and co-ownership + excess losses from the previous period not taken into account − self-borne medical expenses − social deductions (married: CHF 13,000; single parent: CHF 6,500; for first child/young adult: CHF 15,000 (from 2020 onwards); for further children/young adults: CHF 10,000; single adults: CHF 2,200
BL	taxable income ($t-2$)	+ 20% of taxable wealth + net income of non-self-occupied properties − child support payments (with federal tax deduction) − CHF 5,000 for every child (with federal tax child deduction)
BS	net income ($t-2$)	+ 10% of taxable wealth (− wealth deduction)
FR	net income	+ 5% of taxable wealth

	($t-2$)	+ insurance premiums and contributions + interest liabilities in excess of CHF 30,000 + property maintenance costs in excess of CHF 15,000
GE	RDU ($t-2$)	
GL	gross income ($t-2$)	+ 10% of taxable wealth + additional income settled directly with OASI − imputed rental value − CHF 5,000 for every child or young adult in training − alimony deductions
GR	taxable income ($t-2$)	+ 10% of pure wealth + non-taxed income from significant investments + net income from properties, if value is negative + pillar 2 contributions and purchases + pillar 3a contributions + membership fees and donations to political parties
JU	taxable income ($t-2$)	+ 3% of taxable wealth − return on properties + excess property expenses + excess expenses for undistributed community of heirs and co-ownerships + interest liabilities + business losses + business losses carried forward + liquidation losses + fraction of real estate yield exceeding interest liabilities − CHF 5,000 for every married, widowed, divorced, or separated taxpayer; CHF 10,000 for household entitled to a child tax deduction; CHF 4,000 for the first two children; CHF 6,000 for every further child
LU	net income ($t-2$)	+ 10% of pure wealth + pillar 2 purchases + pillar 3a contributions + business losses from previous years + income taxed using the simplified settlement procedure (2021–2022) + deductions for maintenance and management costs for privately owned properties, insofar as they exceed 20 percent of the gross rental income or the taxable rental value of buildings − costs related to illness, accident, and disability − CHF 9,000 for every child or young adult in training
NE	gross income ($t-1$)	+ 30% of net wealth (− wealth deduction; max. CHF 10,000) − if not employed, contributions to AHV, IV, EO, and ALV − business expenses (max. CHF 10,000) − alimonies for divorced/separated spouses and/or for children
NW	pure income ($t-2$)	+ 20% of pure wealth (2021–2022) + income taxed using the simplified settlement procedure (2021–2022) + pillar 2 purchases (2021–2022) + deduction from the partial income method (2021–2022) + deduction for property maintenance minus 15% of income from private properties
OW	net income ($t-2$)	+ 10% of taxable wealth + property losses − work-related expenses

		− alimonies and permanent burdens (dauernde Lasten) − insurance deduction − costs related to illness, accident, and disability − childcare costs through third parties − debt interest (max. the amount of property income) − CHF 7,000 for married couples; CHF 7,000 for every child
SG	pure income ($t-2$)	+ 20% of taxable wealth + pillar 3a contributions + pillar 2 contributions + property expenses exceeding 20% of rental income + losses from previous years + 75% of income settled via federal law on combating illicit employment (BGSH) + (political) donations + deduction of 30% of the rental value of owner-occupied home (2022) + deduction of 30% of income from qualifying investments held as business assets (2022) + deduction of 30% of income from qualified investments in private assets − CHF 4,000 for every child for receiving a family allowance
SH	pure income ($t-2$)	+ 15% of taxable wealth + negative balances of income from real estate + pillar 3a contributions + donations to charitable organizations and political parties − CHF 16,000 for households with children; CHF 8,000 for households without children − relief deduction for very low incomes
SO	taxable income ($t-2$)	+ 50% of total wealth (before 2022: 20–50%) + payments from pension fund + business losses from previous years + pillar 3a contributions up to maximum deduction + deduction for real estate costs − lump-sum settlements for recurring benefits
SZ	federal pure income ($t-2$)	+ 10% of pure wealth (− wealth deduction) + extraordinary property maintenance + pillar 2 purchases
TG	simple tax ($t-1$)	
TI	net income ($t-3$)	+ $\frac{1}{15}$ of net wealth − average cantonal premium − mandatory social security contributions − alimony contributions − passive private and corporate interests (max. CHF 3,000) − work-related expenses (max. CHF 4,000)
UR	net income ($t-2$)	+ 15% of taxable wealth + rental value of own apartment/house + rental and leasing income + income from right of residence and usufruct − property maintenance costs − interest on debt − work-related expenses − job-oriented education and training costs − alimony and pension benefits

			− costs related to illness, accident, and disability
VD	net income		+ $\frac{1}{15}$ of taxable wealth (over household-type-specific thresholds)
	$(t-2)$		+ pillar 3a contributions
			+ property maintenance costs above threshold
		(2021–2022)	+ deduction for health insurance premiums above threshold
VS	net income		+ 5% of net revalued wealth
	$(t-2)$		+ negative real estate income
			+ pillar 3a contributions
		(2020–2022)	+ losses from self-employment that have not been offset
			− alimony payments
			− capital benefits received
ZG	pure income		+ 10% of pure wealth
	$(t-2)$	(2021–2022)	+ pillar 2 purchases
			+ pillar 3a contributions
		(2021–2022)	+ extraordinary property expenses
			− CHF 8,500 per child
ZH	taxable income		*all additions only from 2021–2022*
	$(t-3$ prov.;		+ 10% of taxable wealth (− wealth deductions)
	$t-0$ def.)		+ losses from the use of properties held as private assets
			+ pillar 2 purchases
			+ pillar 3a contributions
			+ donations to charitable organizations

Notes: Additions or deductions valid only for some years are marked. Any additions or deductions that are not marked are used from 2018–2022. Sources: Ecoplan (2022), GDK (2022). Furthermore, certain cantonal laws and regulations were used. AR: *Gesetz über die Einführung des Bundesgesetzes über die Krankenversicherung (EG zum KVG)*. JU: *Arrêté concernant la réduction des primes dans l'assurance-maladie*; 2018–2022. ZH: *Einführungsgesetz zum Krankenversicherungsgesetz (EG KVG)*.

B.2 Data preparation and sample strategy

B.2.1 Household types

We implement some general requirements to assign an observation to a household. First, we only include households with adults aged 26 to 64 and children aged 0 to 17. Second, the observations need to be in the relevant population, meaning that they belong to the permanent population at their main residence. Third, the ID of an observation can only be used once.

In addition to the above requirements, we use further variables for our household type assignment:

- `householdtyp18`: household type variable with children defined as younger than 18 years
- `hh_count`: number of people with the same household ID in a given year, own variable
- `compbasicofphhperm`: household composition of the relevant population
- `classofmaritalstatus`: marital status of the adults in the household

For these variables, the household types take on the following values:

- Single adults
  - `householdtyp18==10`: one-person household

  - `hh_count==1`
  - `compbasicofphhperm==101` or `102`: one male or female adult
  - `classofmaritalstatus!=2`: not married or in registered partnership

- Married couple with no children
  - `householdtyp18==20`: married couple with no children
    * We do not include consensual couples with no children as they are taxed individually.
    * We do not include same-sex couples with no children as we cannot distinguish between married/registered couples (jointly taxed) or consensual couples (taxed individually).
  - `hh_count==2`
  - `compbasicofphhperm==110`: two adults of different sex
  - `classofmaritalstatus==2`: married or in registered partnership

- Married couple with X children
  - `householdtyp18==30`: married couple with at least one child
    * We do not include consensual couples with children as they are taxed individually.
    * We do not include same-sex couples with children as we cannot distinguish between married/registered couples (jointly taxed) or consensual couples (taxed individually).
  - `hh_count==2+X`
  - `compbasicofphhperm==220`: two adults of different sex with minor(s)
  - `classofmaritalstatus==2`: married or in registered partnership

- Single parent with X children
  - `householdtyp18==40`: single parent household with at least one child
  - `hh_count==1+X`
  - `compbasicofphhperm==210` or `211`: one male or female adult with minor(s)
  - `classofmaritalstatus!=2`: not married or in registered partnership

B.2.2 Income

The dataset on individual earning accounts lists income (gross income relevant for calculating old-age and survivors' insurance contributions) from various sources separately. Therefore, we need to compute a total income measure by individual so that the income data can be merged to the other datasets. Furthermore, we want to identify individuals, which have earned at least part of their income from sources we do not want to include in our analyses. Such individuals are flagged, their summed income is set to missing, and their household (at least for the given year) is omitted in the analyses. Special income sources as flagged by the variable `naffilie` (billing number) are:

- Care credits (`naffilie=="11111111111"`): Individuals looking after relatives in need of care are entitled to care credits, which are no direct cash payments but are added to earned income, thereby possibly increasing old-age and survivors' insurance and disability insurance (OASI/DI) payments.[33] In the dataset, entries for care credits are listed with an income of 0, which is why we do not need to account for care credits.

- Loss of earnings (`naffilie=="77777777777"`): Services in the Swiss army, civil defence, Red Cross service, civilian service, J+S[34] leadership courses, or youth shooting instructor courses lead to a compensation for loss of earnings.[35] If such payments are listed in the individual earning accounts, this indicates that the compensations flow directly to the individuals and not to their employers. Therefore, we include payments from loss of earnings compensation.

- We flag all observations with at least one income entry with one of the following billing numbers and exclude these observations from the analysis:

  - Daily allowances from military insurance (`naffilie=="66666666666"`): If military insurance covers an accident or illness rendering someone unfit for work, this person is entitled to daily allowances.[36]
  - Loss of earnings due to Covid (`naffilie=="55555555555"`)
  - Disability insurance allowances (`naffilie=="88888888888"`)
  - Unemployment benefits (`naffilie=="999999XXXXX"`)

Furthermore, the type of contribution is categorized in the variable `cgcot`:

- Voluntarily insured persons and care credits (`cgcot=0`): Voluntary OASI/DI makes it possible to, under certain conditions, remain insured if the place of residence is abroad.[37] We do not want to keep the voluntarily insured in the sample as they most likely have their residence abroad. As `cgcot=0` also includes individuals receiving care credits, we identify the voluntarily insured persons with `cgcot=0` and `naffilie!="11111111111"`. We flag all voluntarily insured and exclude these observations from the analysis.

- Employed or unemployed, income of employees with a contributing employer and benefits subject to contributions (`cgcot=1`): We keep these observations, as the unemployed are dropped before.

[33] Art. 29$^{\text{septies}}$ AHVG (Federal Law on Old-Age and Survivors' Insurance, *Bundesgesetz über die Alters- und Hinterlassenenversicherung*)
[34] *Jugend + Sport* (Youth + Sport)
[35] Art. 1a EOG (Federal Law on Income Compensation, *Bundesgesetz über den Erwerbsersatz*)
[36] Art. 28 MVG (Federal Law on Military Insurance, *Bundesgesetz über die Militärversicherung*)
[37] Art. 2 AHVG

- Employed or unemployed, employer not liable to pay contributions (`cgcot=2`): We keep these observations, as the unemployed are dropped before.
- Income of self-employed persons, including capital gains, excluding self-employed farmers (`cgcot=3`): We keep these observations.
- Income of self-employed persons in agriculture, including capital gains (farmers) (`cgcot=9`): We keep these observations.
- We flag all observations with at least one income entry of the following types and exclude these observations from the analysis:
  - Income of non-employed persons (`cgcot=4`)
  - Contribution stamps, e.g., students (`cgcot=5`)
  - Collective contributions for persons without a social security number, in groups (`cgcot=6`)
  - Persons without pension-generating income (`cgcot=7`)
  - Split entries (`cgcot=8`): To calculate the OASI/DI pension of divorced individuals, income earned during marriage is split.[38]

We sum up gross income by individual and set it to missing if the individual has been flagged before merging the income data to the other datasets. Then, the following cases are possible.

(a) Income is not missing and observation is not flagged

(b) Income is missing and observation is flagged

(c) Income and flag is missing

Individuals in (a) are used as usual and individuals in (b) will be omitted at a later stage as their income is set to missing. For (c), the only reason the flag would be missing is that there was no observation for this individual in the individual accounts dataset. Therefore, we assume that these individuals did not earn anything and set their income to 0.

B.2.3 FOPH data

The FOPH dataset is structured in a similar way as the income data, as individuals could appear multiple times, e.g., if someone had multiple mandatory health insurance contracts in the same year. Therefore, we employ data preparation and combination steps here as well. We can only combine multiple entries when there is no variation in some variables. In other words, we require a set of variables to be the same for all entries of a given individual in the FOPH dataset. If this is not the case, this indicates that these variables change during the year, therefore, we do not merge any data from the FOPH dataset

[38] Art. $29^{\text{quinquies}}$ Abs. 3 AHVG

to this individual (the observation will be dropped later, most likely due to missing outcomes). These variables are listed below.

- Canton
- Risk class
- Hospitalization in the preceding year
- Tariff type
- Model type
  - For model type “bonus”: “bonus” premium level
- Tariff acronym
- Accident coverage
- Deductible
- Premium region
- Pharmaceutical cost group flag medication over CHF 5,000 (2018 and 2019)
- Pharmaceutical cost group flag yes/no (2020, 2021, and 2022)

Furthermore, we require the sum of coverage months to be exactly 12. Otherwise the individual was not covered for the entire year and we do not want to include such cases.

For individuals with no variation in the variables above, we can sum up the following variables to merge them to the other datasets.

- Gross premium
- Net premium
- Gross healthcare costs
- Cost-sharing
- Net healthcare costs

We calculate the health insurance premium subsidy by subtracting the summed up net premium from the summed up gross premium.

B.2.4 Sample strategy

In this subsection, we describe the strategy to generate the household-type-specific samples we use for the analyses. The steps in this subsection are the first ones where observations are dropped. In the preceding subsections, we flag and prepare them to be dropped later. We perform the following steps to create household-specific "clean" datasets.

First, we apply the significant income growth restriction by only considering households whose income growth is between -5% and $+5\%$ (as measured by log differences) in the "significant" year. For treated households, the relevant income growth measure is in the year, which was considered for determining subsidy eligibility in the year of the switch. For example, if a household switches treatment in 2020 and resides in a canton which considers income in year $t-2$ to determine subsidy eligibility, we compare the income growth measure in 2018 to the specified range. Conversely, for untreated household, there is no period of first treatment. Therefore, we simply consider the income growth measure from the period used to determine subsidy eligibility in the current year. In total, 21.8% of all adults in the selected household types survive the significant income growth restriction.

Second, we apply the remaining sample building steps listed below. Table B2 shows, by household type, the percentages of observations dropped at every step after having applied the significant income growth restriction.

1. We drop households with observations that were observed in a different household of the same type in a prior year (e.g., if someone is married from 2018 to 2019 in household A and then marries someone else in 2021, thereby forming household B, we only keep household A in our sample). This step is redundant for single adults.

2. We drop all households in the years in which we do not know the subsidy amount for at least one household member. The exception is 2018, where we do not have data on net premiums and, therefore, cannot compute the subsidy amount.

3. We drop all households in the years in which gross income is missing for at least one household member.

4. We drop all households in the years in which gross income is negative for at least one household member.

5. We drop all households in the years in which the subsidy is negative for at least one household member.

6. We drop all households in the years in which at least one member has moved cantons.

7. As we do not have data on net premiums in 2018, we need to assume the indicator of whether a household received subsidies or not for 2018.

(a) We compute household gross income in the significant year. A significant year here relates to the year relevant to calculating subsidy eligibility, which is two years prior for most cantons.

(b) We adjust for inflation and express household income in the significant year in 2019-CHF.

(c) We compute the log difference of household gross income from the significant year between 2019 and 2018 to estimate the growth rate.

(d) We assume that the indicator of whether a household received subsidies for 2018 is the same as in 2019 if...

  i. ...the log difference is between -0.2 and $+0.2$.

  ii. ...the log difference is lower than -0.2, and the household did not receive subsidies in 2019.

  iii. ...the log difference is greater than $+0.2$, and the household received subsidies in 2019.

(e) We drop any remaining households in 2018 for which we could not assume the indicator of whether they received subsidies in 2018.

8. For every household and year, we calculate the maximum possible subsidy in this canton during the given year with our premium subsidy simulation model. As soon as our calculated subsidy is higher than the maximum subsidy, we drop the household for this year and all future years, as this might indicate that the household benefits from social assistance other than health insurance premium subsidies only. We keep the household in the sample for the years before. In cantons with multiple premium regions, the maximum possible subsidy represents an average across premium regions, meaning that this average might be lower than the true maximum subsidy in some premium regions. However, this does not pose issues as we mainly want to identify households that located around the eligibility threshold and not those around the maximum subsidy level.

9. We drop all households, which we observe only in 2018 at this stage because the indicator of receiving a subsidy was assumed based on 2019. In these cases, the observation for 2019 was dropped. Therefore, we drop 2018 as well.

10. We drop all households once they reappear after the household was missing for one or more years. We keep the household in the sample for the years before.

11. We drop all households in the year they change their subsidy status for the second time and in all future years. We keep the household in the sample for the years before.

12. We drop all households that switch treatment, and we do not observe two years before the first treatment switch. This is required because of the anticipation period in our design.

13. We drop all households that switch treatment one period after the last period in which we still have them in the dataset. This is because they could anticipate their switch in the next period.

14. Again, we drop all households, which we observe only in 2018 at this stage because the indicator of receiving a subsidy was assumed based on 2019. In these cases, the observation for 2019 was dropped. Therefore, we drop 2018 as well.

15. Some households would be treated but due to previous steps all treated periods for the household have been dropped. We drop all years for such households in this step.

16. We drop all treated units with current income above CHF 250,000. These observations are very rare, but might interfere the canton-specific income ranges generated in the next step.

17. For every canton and household type, we define ranges for the current household gross income as described in section 3.3. We drop all households whose current household gross income is outside of the defined ranges at least once.

Table B2 shows the percentage of adults lost for each restriction we impose by household type. With 38.7% of adults lost, the biggest restriction (after the significant income growth restriction) is the current household gross income restriction (step 17), aiming to build comparable treatment and control groups.

Table B2: Steps of Building the Sample(s) by Household Type After Significant Income Growth Restriction

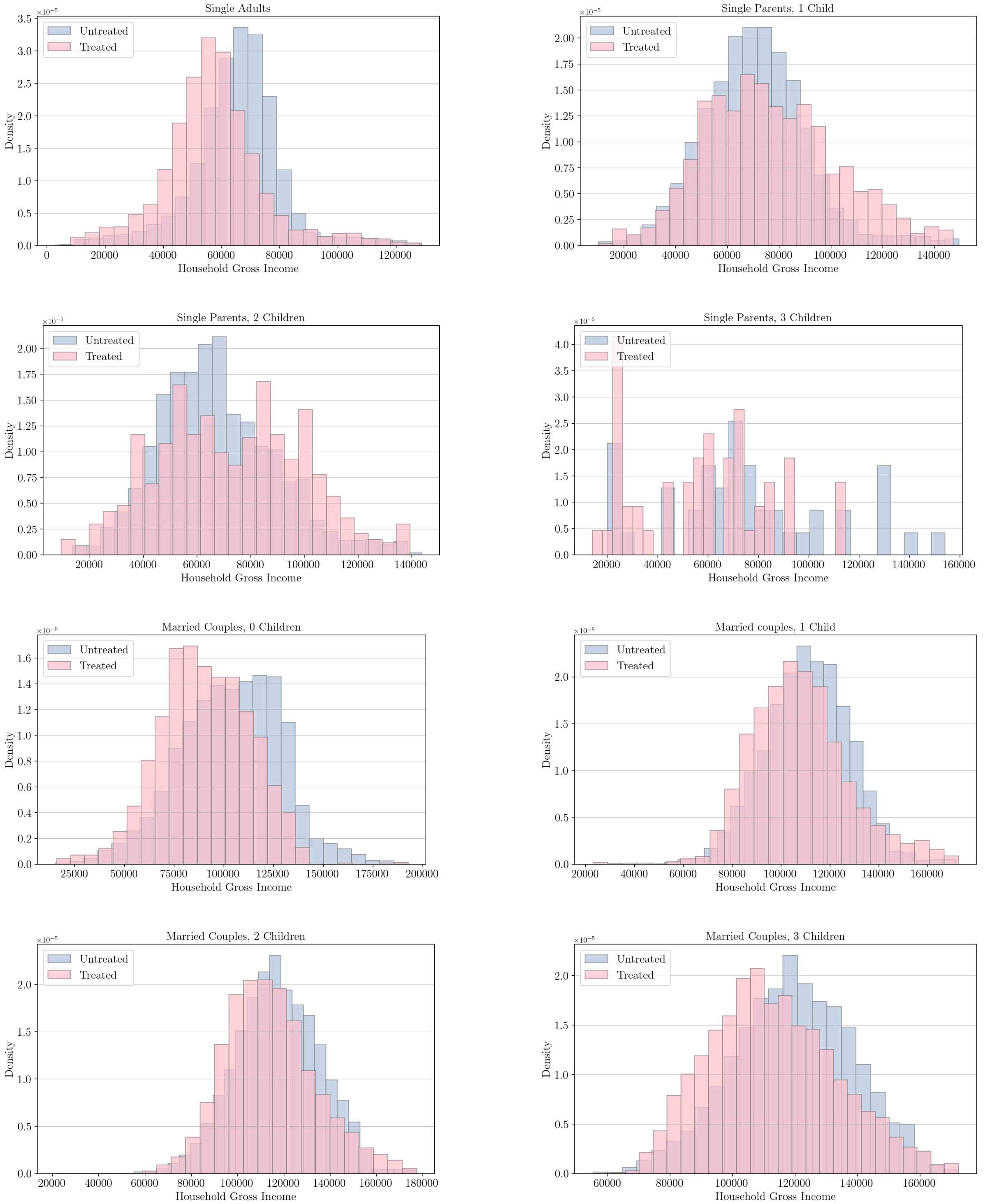
		Single Adults/Parents				Married Couples				Total
Step	Children	0	1	2	3	0	1	2	3	
Start		822,362	37,516	15,931	1,900	701,186	300,210	526,952	148,868	2,554,925
1		−0.0%	−0.3%	−0.2%	−0.3%	−0.0%	−0.0%	−0.0%	−0.1%	−0.0%
2		−5.6%	−9.7%	−8.9%	−11.5%	−7.0%	−20.4%	−13.2%	−16.2%	−11.3%
3		−5.4%	−5.3%	−4.4%	−5.3%	−6.0%	−4.8%	−3.6%	−3.1%	−4.6%
4		−0.0%	−0.0%	−0.0%	−0.0%	−0.0%	−0.0%	−0.0%	−0.0%	−0.0%
5		−0.0%	−0.0%	−0.0%	−0.0%	−0.0%	−0.0%	−0.0%	−0.0%	−0.0%
6		−0.6%	−0.4%	−0.2%	−0.5%	−0.3%	−0.4%	−0.2%	−0.2%	−0.3%
7		−11.7%	−14.1%	−15.3%	−17.5%	−14.0%	−17.1%	−17.0%	−17.3%	−15.2%
8		−0.7%	−0.5%	−0.5%	−0.4%	−0.1%	−0.1%	−0.1%	−0.3%	−0.3%
9		−0.2%	−0.1%	−0.1%	−0.1%	−0.0%	−0.0%	−0.0%	−0.1%	−0.1%
10		−1.2%	−1.4%	−1.3%	−0.7%	−1.4%	−1.1%	−1.7%	−1.6%	−1.4%
11		−0.4%	−1.0%	−0.8%	−0.7%	−0.2%	−0.7%	−1.1%	−1.0%	−0.7%
12		−0.8%	−3.5%	−3.8%	−3.5%	−0.8%	−1.5%	−1.5%	−1.5%	−1.3%
13		−1.2%	−2.6%	−2.3%	−2.1%	−0.6%	−1.6%	−1.9%	−1.9%	−1.4%
14		−0.4%	−0.7%	−0.7%	−0.6%	−0.2%	−0.5%	−0.7%	−0.7%	−0.5%
15		−0.5%	−1.2%	−1.1%	−0.5%	−0.3%	−0.8%	−1.2%	−1.2%	−0.8%
16		−0.0%	−0.0%	−0.0%	−0.0%	−0.0%	−0.0%	−0.0%	−0.1%	−0.0%
17		−43.8%	−24.5%	−25.9%	−43.8%	−50.1%	−32.5%	−34.7%	−28.9%	−38.7%
Remaining		27.4%	34.5%	34.5%	12.6%	19.0%	18.3%	23.0%	25.9%	23.3%
		225,678	12,941	5,504	239	133,314	55,086	121,292	38,556	592,610

Notes: The first column refers to the step number described in section B.2.4. Only adults are reported in this table. The number of observations is on an individual level and observations count as many times as they are observed in different years (i.e., a married couple who is observed over 5 years counts as 10 observations in this table).

B.3 Data

B.3.1 Income distribution

Figure B1: Gross Household Income by Household Type and Treatment Status



Notes: For every canton and household type, we define minimum and maximum thresholds for current household gross income and exclude households with an income outside the range at least once in their observation period. The ranges are set to the 5th and 95th percentiles of the treatment groups' income distribution for a given canton and household type. This figure shows the income distribution of the treatment and control groups by household type. For simplification purposes, these distributions are at the level of Switzerland and not at the cantonal level, at which the income ranges can vary.

B.3.2 Descriptive statistics

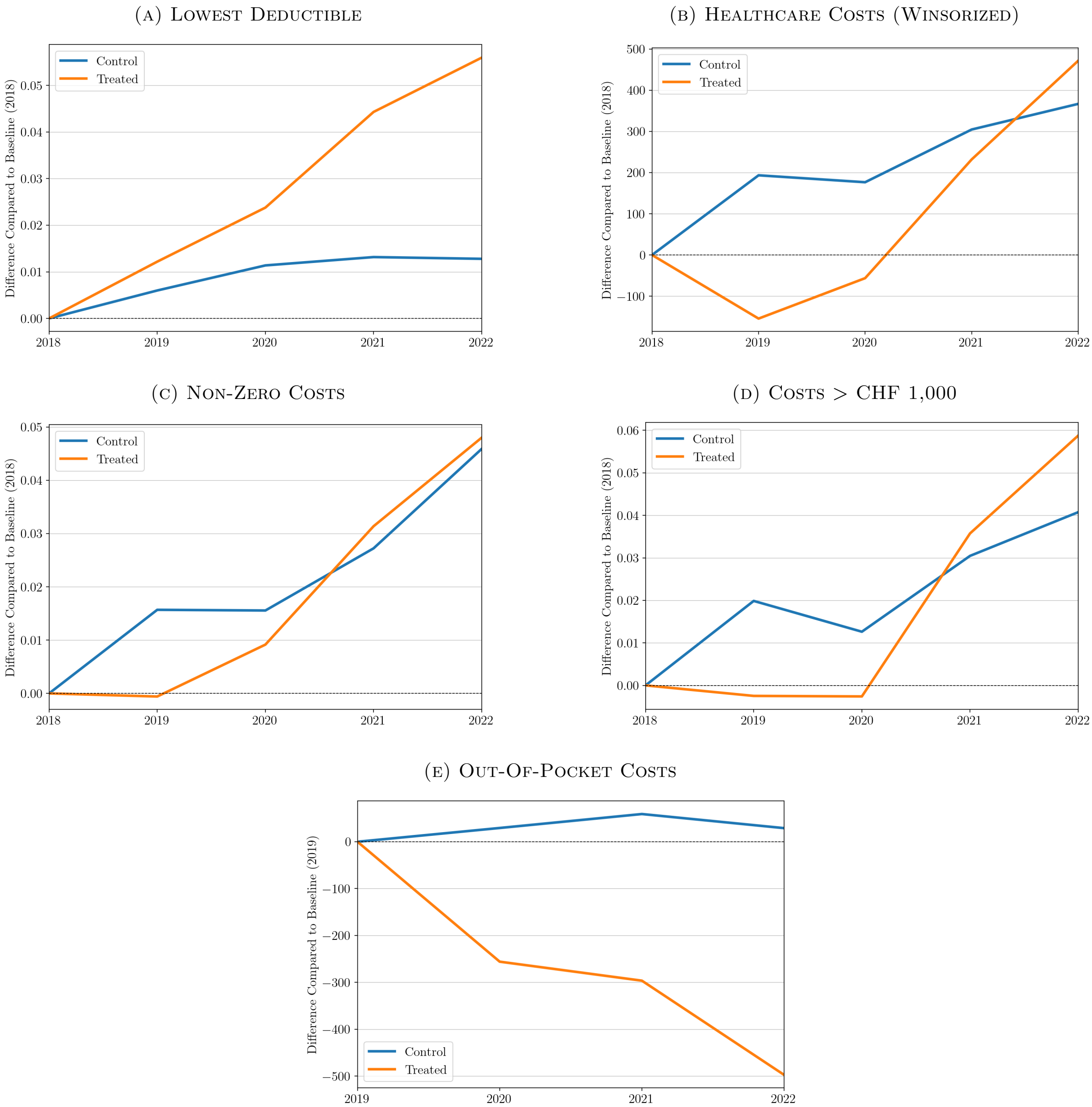
Table B3: Descriptive Statistics (Population and Sample)

	Total		Subsidies		No Subsidies		
	Mean	SD	Mean	SD	Mean	SD	p
Panel A. Population							
Individual Characteristics							
Age	44.56	10.49	42.96	10.10	46.09	10.32	< 0.001
Female	0.494	0.500	0.532	0.499	0.481	0.500	< 0.001
Swiss Nationality	0.699	0.459	0.611	0.487	0.751	0.432	< 0.001
Chronically Ill	0.117	0.321	0.164	0.370	0.106	0.308	< 0.001
HH Gross Income	131,245.4	183,074.6	75,451.5	43,382.3	145,562.8	203,558.5	< 0.001
HH Subsidy Yes/No	0.219	0.414					
HH Subsidy	604.7	1,844.6	4,165.7	2,934.0			
HH Gross Premium	7,670.2	3,072.8	8,512.1	3,201.8	7,467.3	3,012.9	< 0.001
Health Plan Choice							
Lowest Deductible	0.350	0.477	0.506	0.500	0.313	0.464	< 0.001
Healthcare Demand							
Healthcare Costs	3,143.6	8,583.1	4,319.4	10,710.1	2,757.5	8,145.8	< 0.001
Non-Zero Costs	0.769	0.422	0.810	0.392	0.760	0.427	< 0.001
Costs > CHF 1,000	0.455	0.498	0.527	0.499	0.433	0.496	< 0.001
Out-Of-Pocket Costs	4,298.6	1,441.5	3,067.1	1,639.5	4,649.9	1,193.6	< 0.001
Observations		11,744,634		1,704,718		6,075,733	
Panel B. Sample							
Individual Characteristics							
Age	46.20	10.30	42.97	8.64	47.21	10.57	< 0.001
Female	0.516	0.500	0.540	0.498	0.508	0.500	< 0.001
Swiss Nationality	0.777	0.416	0.707	0.455	0.799	0.400	< 0.001
Chronically Ill	0.118	0.323	0.091	0.287	0.127	0.333	< 0.001
HH Gross Income	88,751.3	29,613.7	88,836.0	28,624.8	88,724.9	29,916.1	0.219
HH Subsidy Yes/No	0.238	0.426					
HH Subsidy	626.7	1,422.3	2,601.8	1,805.4			
HH Gross Premium	6,575.8	2,461.0	7,304.4	2,319.1	6,348.3	2,459.9	< 0.001
Health Plan Choice							
Lowest Deductible	0.350	0.477	0.366	0.482	0.345	0.475	< 0.001
Healthcare Demand							
Healthcare Costs	2,531.0	7,451.4	2,233.2	6,528.1	2,624.0	7,715.0	< 0.001
Non-Zero Costs	0.742	0.437	0.753	0.431	0.739	0.439	< 0.001
Costs > CHF 1,000	0.417	0.493	0.404	0.491	0.421	0.494	< 0.001
Out-Of-Pocket Costs	4,453.8	1,265.5	3,808.6	1,345.1	4,660.7	1,165.6	< 0.001
Observations		592,610		141,114		451,496	

Notes: HH: Household. Monetary values are in CHF. Observations are pooled over the observation period (2018–2022). Panel A focuses on the entire population for this household type, panel B focuses on our used sample based on the inclusion criteria we define for identification. The p-value in the last column compares the mean of "Subsidies" to "No Subsidies" (t-test for non-binary variables, chi-squared test for binary variables). We assume subsidy receipt for 2018 in Panel B (see Chapter B.2), but not in Panel A, therefore, the sum of observations with and without subsidies is lower than the total number of observations in Panel A. The binary variable indicating chronic illness is constructed using data on pharmaceutical cost groups.

B.3.3 Mean trends

Figure B2: Trends By Control and Treatment Group



Notes: Visualization of differences between yearly means and the mean in the first observed year. Treated units can be treated for the first time in 2020, 2021, or 2022. In Panel B, the right tail of healthcare costs is winsorized at the 99th percentile (on a yearly basis) to reduce the impact of extreme values. In Panel E, the baseline is 2019 as no data is available for 2018.

B.4 Instrument

B.4.1 Exogeneity checks

Although exogeneity is untestable, we can verify whether exogeneity is at least plausible. In our specification, the deductible level is endogenous, and we use a binary indicator of whether the gross premium percentile is above the 50th percentile within the unit's subgroup as an instrument. As additional exogenous variables in our model, we include a binary subsidy indicator, inflation-adjusted household income, a binary indicator of Swiss nationality, and squared age. Table B4 shows the estimated coefficient of the instrument when regressing any of the above-mentioned variables on the instrument and the remaining exogenous variables, including unit and year fixed effects. For no specification do we find evidence that the instrument is significant. Likewise, in Table B5, we regress the instrument on all other exogenous variables and find no significant effects.

Table B4: Regressing Exogenous Variables on Instrument and Other Exogenous Variables

	Outcomes			
	(1) Subsidy	(2) Income	(3) Swiss	(4) Squared Age
Instrument	0.001 (0.002)	0.001 (0.001)	0.000 (0.000)	0.230 (0.293)
Individual FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Individuals	183,652	183,652	183,652	183,652

Notes: Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Coefficients represent estimates from an OLS regression of the selected variable on the instrument and the remaining variables. Individual and year fixed effects are included. Standard errors in parentheses (clustered at the household level). Income is inflation-adjusted and in logs.

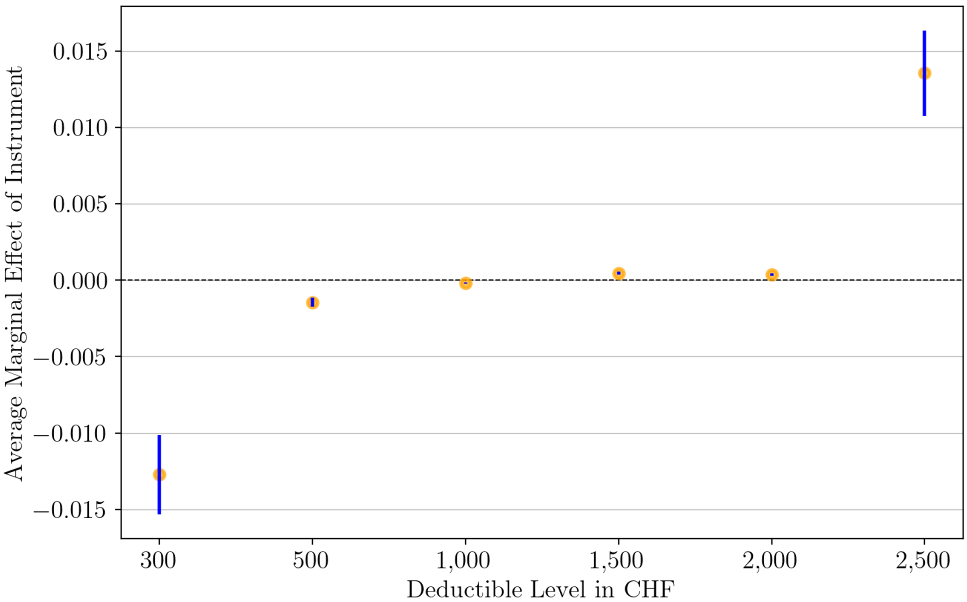
Table B5: Regressing Instrument on Other Exogenous Variables

	Outcome
	Instrument
Subsidy	0.000 (0.004)
Income	0.006 (0.006)
Swiss	0.001 (0.013)
Squared Age	0.000 (0.000)
Individual FE	✓
Year FE	✓
Individuals	183,652

Notes: Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Coefficients represent estimates from an OLS regression of the instrument on the variables listed in the table. Individual and year fixed effects are included. Standard errors in parentheses (clustered at the household level). Income is inflation-adjusted and in logs.

B.4.2 Average marginal effects

Figure B3: Average Marginal Effects of Instrument



Notes: Marginal effect of instrument on deductible levels in the ordered probit first stage, generated with the `margins` command in Stata. Confidence intervals are plotted in a different color to visualize the short intervals for intermediate deductible choices. All effects are statistically significant, although the effects for intermediate deductible choices are very small in magnitude. This is not surprising as most individuals choose the lowest or highest deductible level.