Transfer Payment Systems and Financial Distress: Insights from Health Insurance Premium Subsidies*

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Abstract

How should payment systems of means-tested benefits be designed to improve the financial situation of needy recipients most effectively? We study this question in the context of mandatory health insurance in Switzerland, where recipients initially received either a cash transfer or subsidized insurance premiums (a form of in-kind transfer). A federal reform in 2014 forced cantons (i.e. states) to universally switch to in-kind provision. We exploit this setting based on a difference-in-differences design, analyzing eight years of rich individual-level accounting data and applying a machine learning approach to identify cash recipients prior to the reform. We find that switching from cash to in-kind transfers persistently reduces the likelihood of late premium payments by about 20% and of government debt collection for long-term missed payments by approximately 12%. There is no evidence for a negative spillover effect on the timely payment of the non-subsidized co-pay bills for health services after the regime change.

Keywords: Health insurance, transfers, cash subsidies, in-kind transfers, financial distress, debt collection, machine learning, honest sample split

JEL classifications: D14, G52, H24, I13

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1 Introduction

Many countries maintain significant redistribution programs offering transfers to individuals with limited financial resources. Means-tested benefits are thereby targeted to help people buy necessary goods and services. It is understood that financial difficulties that arise due to too little available resources should be overcome. However, financial distress can also be the result of limited planning abilities and limited self-control. For given resources, people's well-being might be lower due to the misallocation of means and to the mental strain related to difficulties in keeping finances in order.[1] In this latter case, aspects of transfer systems other than generosity might turn out to be crucial. In particular, payment modalities offer means to simplify the spending decisions of poor individuals and households and to reduce their exposure to the temptations of short-term liquidity.[2] While concerns about such misuses of cash transfers are one of the most common rationales for implementing in-kind provision to thereby encourage recipients to consume a particular good or service (see, e.g, Currie and Gahvari, 2008, for a review), little empirical evidence exists whether cash payments indeed lead to adverse financial outcomes for individuals.

In this paper, we study the effect of transfers via cash versus the provision of subsidized services (a form of in-kind transfer) on the financial distress of recipients. We do this in the context of mandatory health insurance with community rated premiums, i.e., premium payments that mark a major expenditure for most households. Specifically, we test the hypothesis that the limited discretion when health insurance premiums are individually subsidized improves payment behavior for mandatory coverage when compared to a cash transfer of equal magnitude. We can test this hypothesis by exploiting an ideal setting in which health insurance is mandatory and the transfers are individually determined independently of the payment system. This allows us to identify the effect of an important property of the procedural design of means-tested benefits absent any confounding influence of benefit generosity and consumption choice, i.e., in our case, whether to buy health insurance or not.

The concrete institutional context is a federally imposed change from cash to in-kind transfers of premium subsidies for needy individuals within the mandatory health

[1] There is a rich literature in behavioral household finance documenting how financial illiteracy and cognitive as well as motivational limitations negatively affect people's financial and subjective well-being (see, e.g., Gerardi et al. 2013, Lusardi and Mitchell 2014, Ponce et al. 2017 or Beshears et al. 2018). Financial distress is thereby to some extent self-reinforcing as it undermines people's capacity for long-term planning and decision-making (see, e.g., Mullainathan and Shafir, 2013).

[2] If individuals face self-control problems, potentially magnified by poverty (Banerjee and Mullainathan, 2010; Haushofer and Fehr, 2014; Bernheim et al., 2015; Carvalho et al., 2016), a large (relative to household income) lump sum cash payment may cause adverse long-term outcomes for recipients compared to in-kind transfers. In such a context, provision in-kind might instead act as a hard commitment device (see, e.g., Bryan et al., 2010, for an overview), which can be optimal for recipients, especially if they are naive about their self-control problems (O'Donoghue and Rabin, 1999).

insurance system in Switzerland in 2014. The individual premium subsidies constitute the second biggest means-tested transfer program in the country (after social assistance). In all, CHF 4.7 billion were paid out in 2018, benefiting roughly 26% of the resident population. In 2014, the premium load after subsidies amounted to 12% of the disposable income of the average subsidized household (B,S,S., 2015).[3] As an example, for a family with two children and a gross household income of CHF 70,000 (about US-$ 75,000) (roughly at the 25% percentile of income distribution), the average yearly subsidy across all cantons is about CHF 3,800 (about US-$ 4,000) (thus about 5% of gross income). Net of subsidies, the remaining premiums for this household make up 14% of its disposable net income (Ecoplan, 2018).

The reform affected only a limited number of jurisdictions, providing us with a quasi-experimental setting. The primary motivation for the reform was reducing arrears with payments that in a significant number of cases led to personal bankruptcies. In 2018, health insurers initiated unsuccessful debt collection proceedings against approximately 200,000 individuals, and the resulting loss certificates for outstanding premiums amounted to about CHF 400 million (Federal Office of Public Health, 2020).[4] The cantons, i.e., the states of the Swiss Confederation, subsequently assume responsibility and cover by law 85% (with the respective health insurer covering the remaining 15%) of these loss certificates. A popular argument against cash subsidies was that people would divert them to short-term consumption so that they were no longer available for paying premium bills due later in the year, suggesting limited willpower and/or financial planning skills upon exposure to a tempting temporary positive liquidity shock. Up to 2014, Swiss cantons were free to organize the subsidy scheme, and the payment modalities in particular, within federal regulations. Prior to the reform, some cantons thus provided recipients with a yearly one-time lump sum cash payment, while others remitted it to the beneficiaries' health insurers. The health insurers in turn reduced the premium bills by the amount of the transfer.[5] In total, seven cantons changed their system to in-kind transfers in 2014. We can compare the changes in payment performance in these cantons with the changes in six other cantons, that have always employed premium reductions and thus serve as control cantons.[6] The setting thus offers the possibility of a difference-in-differences (DID)

[3]The federal guideline specifies that a household's mandatory health insurance premiums should not exceed 8% of its *taxable* income.

[4]As the exact figures on unpaid health insurance premiums are not available for all 26 cantons, the numbers given here represent extrapolated estimates based on the available data.

[5]This process is similar to that in the exchanges of the Affordable Care Act in the United States (although in the ACA, individuals can also elect to pay the full premium first and receive a tax rebate later).

[6]Both in the treated and control groups of cantons, three cantons made additional, albeit comparatively minor, adjustments to their cantonal health insurance subsidy schemes that coincided with the larger payout system reform in January 2014. While we pool all cantons in our main analyses we later also provide separate results using only the seven cantons in which either no aspect of the subsidy scheme changed or only the switch to in-kind occurred.

design.

Our data on individual payment performance are from one of the biggest health insurance providers in Switzerland, offering us access to detailed accounting data (about 22 million bills) regarding arrears in premium payments between 2012 and 2019. We focus on two outcomes related to an individual bill that indicate payment problems. First, whether the insurer sent a payment reminder to customers, which occurs roughly a month after the due date of the bill. Second, initiation of the debt collection process (typically after the bill remains unpaid for three months), which leads to an entry in a government registry and may have substantial long-term impact on future access to rental housing or credit.[7] However, Swiss law forbids health insurance companies to terminate contracts even with delinquent customers and requires health care providers to always provide at least emergency care. Difficulties in matching administrative and private enterprise accounting data have often limited evaluations of how government transfer schemes affect recipients, particularly in the case of direct cash payments. Likewise, in our application, health insurance providers did not know which of their customers received premium subsidies under the cash transfer scheme. We overcome this common obstacle by using the detailed individual-level data to adopt a machine learning approach in which we predict recipients of premium subsidies. Consequently, we can compare in a DID subsidized individuals both in treated and untreated cantons before and after the reform. The imputation of the subsidy status furthermore allows us to adopt another DID and to include only treated cantons, where non-recipients therein now act as the control group. Finally, we also combine the two strategies in a triple-differences design.

We find that the two different payment modalities lead to substantially different outcomes regarding individuals' financial distress. The change from cash to in-kind transfers results in a 19.5% reduction in the probability that a payment reminder is sent out for the premium bill of a subsidized individual.[8] In the case of debt collection, which indicates severe financial problems, the switch to in-kind payments decreases its likelihood among subsidy recipients by 12.1%. The improvements in payment behavior set in immediately following the reform. Moreover, the lower level of payment problems remained for both the relatively milder and the more severe late payments, i.e., neither an attenuation nor an amplification of the effect is observed. In a supplementary analysis, we do not find any negative (or positive) effects of the reform on the payment performance for co-pay bills

[7] See https://www.ch.ch/en/debts-what-effects for further information on the debt collection process in Switzerland.

[8] Since these results are based on an imputed recipient's status, we also repeat the analyses with all individuals, irrespective of whether they receive premium reductions. We find that across the entire population, the reduction in payment problems amounts to roughly a quarter to a third of the effect size we find when comparing only recipients between control and treated cantons. Given that around one fourth of the people in the overall sample receives premium subsidies (and were thus affected by the switch to in-kind), this effect size is very much in line with our findings based on the imputed status.

for medical services, i.e., expenditures not specifically targeted by the means-tested transfers. Finding no negative side effects of the reduced flexibility (as individuals no longer receive cash) in this domain could be taken as an indication that recipients likewise do not have more difficulty paying other regularly occurring household expenses than before the reform. This suggests that the provision of health insurance transfers in-kind reduces financial distress among recipients overall, and not only with regard to health insurance premiums, compared with equally sized cash transfers.

Our study complements existing evidence regarding the impact of payment modality (see, e.g., Cunha, 2014; Hidrobo et al., 2014, 2016), disbursement patterns (see, e.g., Foley, 2011; Carr and Packham, 2018) and labeling (see, e.g., Kooreman, 2000; Beatty et al., 2014) of means-tested benefits on diet, other household expenditures, health, domestic violence, and other forms of crime in subsidized households. We emphasize financial distress as an important additional outcome variable and suggest limited financial planning capacity as a key moderating factor in the effectiveness of alternative payment modalities. Interestingly, little seems to be known about whether beneficiaries themselves prefer cash over in-kind transfers. Information on the preferences of subsidized individuals is particularly absent for economically advanced countries and rather refers to developing countries, such as the study by Hirvonen and Hoddinott (2021). The latter authors provide empirical evidence from a food assistance program in Ethiopia showing that, given the choice, recipients tend to choose in-kind provision over equally sized cash transfers (potentially for the reason that the former can serve as a commitment device).

The present analysis furthermore contributes to the literature on the design of health insurance subsidy mechanisms (see, e.g., Decarolis, 2015, and Decarolis et al., 2020, for Medicare Part D, Curto et al., 2021, for Medicare Advantage, and Tebaldi, 2017, for the ACA exchanges). These mechanisms, in particular in the institutional context of the United States, are usually investigated with regard to the consequences for consumer demand and insurers' incentives. Specifically, if enrollment is voluntary, the demand for health insurance is expected to depend on the subsidy design and the outside options. For insurers, moreover, the design of the subsidy mechanism shapes the incentives to require high premiums and to raise the cost for the tax payer. In this research, the focus therefore usually lies on the equilibrium outcome on the health insurance market of the joint demand and supply responses. Accordingly, little is known so far on the effectiveness of the various subsidy mechanisms in reducing financial distress and making health plans affordable. This holds especially in choice-based health insurance markets with mandatory enrollment as investigated in the current analysis. Moreover, in the analyzed health insurance market here, the insurers have no way to raise the subsidy they can receive for an individual customer or group of consumers. As a result, we can focus on consumer behavior regarding financial distress.

Another stream of related research studies the consequences of subsidized health *services* in comparison with those of cash transfers. Lieber and Lockwood (2019), for example, show that within Medicare Home Care in the United States, in-kind transfers lead to over-consumption among beneficiaries, but improve targeting compared to cash-like transfer payments. Related to our study, Kaufmann et al. (2017) present evidence from the same premium subsidy scheme in Switzerland and find that recipients are slightly more likely to choose health insurance plans with a low deductible if they receive their benefits in-kind rather than in cash. However, the choice of lower deductibles has no effect on health care utilization in this context, i.e., the in-kind transfer (compared to in cash) does not lead to over-consumption among recipients. Nevertheless, we take the deductible choice into account in the control strategy of our analysis.

The remainder of the paper is structured as follows. In Section 2, we describe the health insurance system in Switzerland, the premium subsidy program and the harmonization of its payment scheme to in-kind in 2014. We explain our empirical strategy in Section 3, before discussing our data and the machine learning approach for predicting recipients of cash transfers in Section 4. Finally, we present the results of our empirical analyses in Section 5. Section 6 provides concluding remarks.

2 Institutional background

2.1 Health insurance in Switzerland

Similarly to the Netherlands, Germany, and the US marketplaces in the ACA, the Swiss health insurance system is organized according to principles of regulated competition.[9] Health plans and health providers compete on price and quality, while regulation ensures individual affordability of health plans and risk solidarity. Health insurance is compulsory, but consumers can freely choose among approximately 50 private insurers (open enrollment). The standard health plan includes free physician choice, an individual deductible of CHF 300 and a co-insurance rate of 10% up to the stop-loss amount of CHF 700.[10] To ensure risk solidarity, health plans have to charge community-rated premiums per region and age category (children aged $0-18$, young adults aged $19-25$, and adults). Consumers can choose a higher deductible and health plans with managed care features. The voluntary deductible ranges from CHF 500 to $2,500$; managed care options include telemedicine, preferred provider, and health maintenance organization health plans.[11] Choosing a vol-

[9] The following general description draws heavily on Schmid et al. (2018).

[10] For children, the individual deductible is zero and the stop-loss amount is CHF 350. If a family has more than two children, the childrens' cumulative stop-loss amount is CHF 700.

[11] The voluntary deductible for children ranges from CHF 100 to 600; children can also choose managed care health plans.

untary deductible and/or a managed care option leads to a lower premium. However, the premium discount is subject to strong regulations ensuring risk solidarity between individuals with low and high risk. Finally, it is important to note that each health plan has to offer the same coverage in terms of physician services, hospital services, prescription drugs and so on. Health insurers *must not* extend the coverage of the basic health insurance plans, but they are allowed to sell supplementary health insurance plans that cover services not included in the basic coverage (e.g., dental services).

2.2 Health insurance premium subsidies in Switzerland

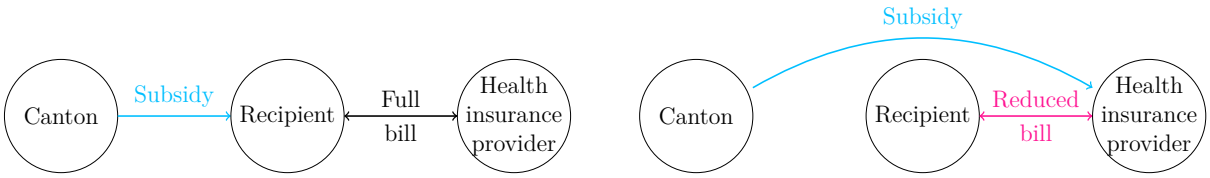
To ensure health plan affordability, low-income individuals and families receive means-tested premium subsidies from their canton of residence (Kaufmann et al., 2017). Refusing the purchase of health insurance is not possible as cantons assign those who refrain from buying the mandatory health coverage to a health insurance company. Health insurers, in turn, cannot prevent any individuals from signing up with them and cannot terminate contracts for any reason, such as continuously unpaid premiums. However, as long as an individual has outstanding premium bills, he or she cannot switch to another health insurance company. While the premium subsidies are based on federal law, partially financed by the federal government, and household income and family size (particularly the number of children) always constitute the most important factors for determining eligibility, cantons have considerable leeway regarding the design of the subsidies. This is reflected in different dimensions: First, there are differences in the income threshold determining subsidy eligibility, whereby the subsidy decreases either linearly or stepwise as income rises (see Gerritzen et al., 2014, for further details). Second, the family structure, in particular marital status and the number of children, is taken into account differently. While some cantons, for instance, shift the income threshold upwards to account for family size, other cantons directly set higher subsidies per child as family size increases. Third, there is considerable variation in the cantonal reference premiums due to large differences in healthcare expenditures across cantons. The reference premium corresponds to an average premium (of the standard health plan) and is used to determine the maximum subsidy amount. Altogether, these differences primarily affect the number of subsidy beneficiaries and the subsidy per beneficiary.

Payment modalities of health insurance premium subsidies

Up to 2014, cantons were allowed to decide on a further dimension of the premium subsidy payment system. They determined the *recipient* of the payment. The subsidy could be either paid into the beneficiary's bank account or to his or her insurer.[12] In the former

[12]The main reason for allowing cash transfers in 1996, when the scheme started, was the lack of an adequate digital infrastructure for the exchange of recipient information between cantons and health

case, the health insurance invoiced the full premium amount without even knowing which individuals received a subsidy. We refer to this payment modality as *cash transfer*. In the latter case, the health insurance provider deducted the subsidy amount from the premium, i.e., the provider invoiced a reduced premium bill. We refer to this payment system as *in-kind premium reduction*. Graphically, we show the two systems below in Figure 1.



(a) Cash transfer (b) In-kind premium reductions

Figure 1: Comparison of transfer payment systems

Payment harmonization of 2014

In the summer of 2007, the Social Security and Health Committees of the Swiss parliament passed a motion requesting the Federal Council to add an amendment to the next change in the national Health Insurance Law that would require cantons to pay out premium subsidies only in-kind. The committee members proposing the motion argued that cash payments tempted recipients to spend the benefits for purposes other than health insurance premiums.[13] However, essentially no empirical evidence existed to support (or refute) this (probably) most common justification of in-kind transfers in the political discourse. The harmonization to in-kind was backed by all parties across the political spectrum and passed unanimously in March 2010.

The amended Health Insurance Law went into effect on January 1, 2012 and the seven cantons still making use of cash transfers were given until January 1, 2014 to adapt their systems to transferring the subsidy directly to health insurance companies.[14] The deadline became the actual reform date in all the affected cantons. Further revisions to the Health Insurance Law had no separate impact on the cantons depending on their payment system but were effective nationwide. These other aspects of the new Health Insurance Law that went into effect as of 2012 primarily involved hospital financing and risk adjustment. They thus did not relate to the payment system of premium subsidies, and, moreover, they affected all cantons the same way. In fact, the harmonization to in-kind provision represented the only modification to premium subsidies in the revision of the Health Insurance Law.

insurance companies in many cantons at the time.

[13] https://www.parlament.ch/de/ratsbetrieb/amtliches-bulletin/amtliches-bulletin-die-verhandlungen?SubjectId=11790 (in German).

[14] Two other cantons deducted the subsidy from annual personal income taxes before the reform and are thus not suited for our analysis.

3 Empirical strategy

3.1 Treatment and control cantons

The simultaneous switch of some Swiss cantons from paying out subsidies in cash to subsidizing individuals' premiums in-kind on January 1, 2014, with other cantons maintaining the latter system, offers an ideal setting that can be studied as a quasi-experiment. It allows us to measure the causal effect of one over the other transfer system on the financial situation of recipients.[15]

Specifically, seven cantons switched from cash to in-kind provision in 2014 and thus qualify as treatment cantons for our analysis. The cantons included in the treatment group are displayed in Figure 2 in red. These are the cantons of Lucerne (LU), Uri (UR), Schwyz (SZ), Nidwalden (NW), Basel-Country (BL), Grisons (GR) and Thurgau (TG), with roughly 380,000, 35,000, 150,000, 40,000, 280,000, 190,000 and 260,000 inhabitants, respectively, at the time of the reform.

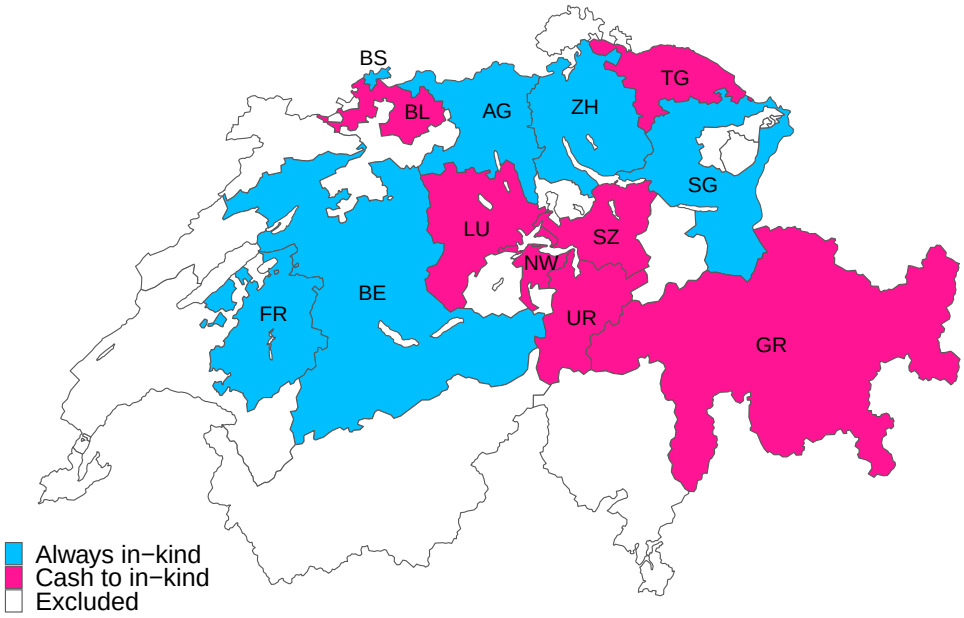


Figure 2: Overview of cantonal premium subsidy payout systems

Six cantons had already employed in-kind transfers for their premium subsidy schemes since their inception in 1996 and thus qualify for the control group. The remaining 13 cantons had to be excluded. First, since all cantons changing from cash to in-kind payment belong to the German-speaking part of Switzerland and payment behavior is quite different in the fully French- and Italian-speaking cantons (see, e.g., Federal Statistical Office,

[15] A further benefit of our setting lies in the mandatory purchase of basic health insurance in Switzerland. In other circumstances where the subsidized good or service does not need to be bought, a switch to in-kind could potentially change the pool of individuals that remain interested in claiming the benefit.

2020), we did not include these latter cantons in our control group. Second, the seven remaining German-speaking cantons had to be excluded because they employed unique systems that could neither be classified as cash nor in-kind prior to 2014 (but applied tax deductions), switched the apportionment of the deductions or had just adopted premium reduction systems of their own volition in the years preceding the federal mandate.

The six cantons marked blue in Figure 2 that employed a scheme with premium reductions since 1996 are Zurich (ZH), Berne (BE), Fribourg (FR), Basel-City (BS), St. Gall (SG) and Aargau (AG), with respective populations of about 1.4 million, 980,000, 290,000, 180,000, 480,000, and 630,000 at the time of the reform. Besides the payout system, cantons from time to time adjust other aspects of their cantonal premium subsidy schemes, such as the exact eligibility rules, the formulas to determine the subsidy amount, or the overall generosity. In three of the control cantons (i.e., ZH, BS, and AG) and four of the treated cantons (i.e., LU, SZ, BL, and GR), no tweaks to other aspects of the subsidy scheme coincided with the reform in 2014. However, in the other six cantons some institutional adjustments occurred. While these adjustments were no major changes, perhaps with the exception of those in Nidwalden[16] and Berne, they might nonetheless had some consequences on the generosity of subsidies and the fraction of people eligible for them. Any potential effect of the reform is thus not as "cleanly" picked up in this latter compared to the former group of cantons. While we consider all the thirteen cantons in our main analyses and treat them alike, we also perform the analyses separately for the two groups of cantons. This allows us to check whether what we consider minor adjustments affect the overall findings in a systematic way. Table B4 in the Appendix provides an overview of the average premium and the average subsidy in the cantons included in our sample before and after the reform. We observe no substantially different evolution of these variables between control and reform cantons.

3.2 Estimation approaches

With this setting, we can follow a DID approach to estimate the average treatment effect of the reform on the financial distress of the treated. In contrast to the classic two-by-two DID, we adopt a slightly more refined control strategy and include fixed effects for every month in our sample, δ_t, i.e., for example, February 2012 or April 2014 (and do not use a single dummy variable indicating whether an observation is from the post period). Accordingly, there is no main effect for the post period. Similarly, as we can rely on individual level panel data, we consider individual fixed-effects, α_i. As will be explained in Section 4, we exclude all individuals who changed their place of residence between

[16]In Nidwalden, following a successful popular initiative the share of the eligible population was cut from almost 50% in 2013 to only about 25% in 2014. Thus fewer people received (small and very small) subsidies but remaining recipients benefited from higher amounts.

cantons during the observation period. Accordingly, we cannot estimate a main effect for belonging to the treated group of cantons. The standard binary DID indicator of living in a treated canton thus only appears in the interaction term. Regarding the dependent variable, we are interested in outcome Y_{ijt}, which is whether a payment problem occurred for a given health insurance premium bill received by individual i in canton j at time t.

In order to estimate the treatment effect, we pursue two separate DID strategies as well as a combination of them. Our most straightforward DID is to compare recipients before and after the reform (with the post reform indicated by $Post_t$) in treated (or reform) cantons (RC_j) with recipients in control cantons before and after the reform. Time variant individual characteristics (introduced below) are captured by X_{it}. With this approach only considering subsidy recipients, we thus estimate the following equation (1).

$$Y_{ijt} = \alpha_i + \delta_t + \beta^{DID1} RC_j \times Post_t + X_{it} + \epsilon_{ijt} \tag{1}$$

Any potential Swiss-wide developments that affected the entire population of recipients in the two groups of cantons alike (for example, a general improvement in economic conditions for low-income individuals) is therefore canceled out. Other trends that occur only within the group of treated cantons, such as separate paths of economic growth, however, could still lead to spurious findings of a reform effect. To avoid our results being driven by such canton-specific factors, we employ a second DID strategy. We consider non-recipients for the control group as another category of individuals unaffected by the payment harmonization. Contrasting an outcome for payment problems for recipients (Rec) with that for non-recipients in treated cantons only takes into account any developments affecting the payment behavior in general within the group of cantons switching from cash to in-kind. As some individuals switch between being recipients and non-recipients, we can estimate the main effect of receiving a subsidy, even though individual fixed-effects are again included. The DID estimation approach limited to reform cantons thus takes the form expressed in equation (2).

$$Y_{ijt} = \alpha_i + \delta_t + \beta_1 Rec_{it} + \beta^{DID2} Rec_{it} \times Post_t + X_{it} + \epsilon_{ijt} \tag{2}$$

Next, we combine the two previous approaches to estimate difference-in-differences-in-differences, or triple differences (TD). This provides us with an extensive control strategy, as we account for both national trends among subsidy recipients, as well as broader cantonal trends in the group of treated cantons. Using TD also considers any changes before and after the reform between recipients and non-recipients in control cantons. Therefore, we also control for potential situations in which the development among recipients in these control cantons actually represents an improvement or deterioration in payment behavior relative to non-recipients in the same cantons. Equation (3) summarizes the estimation

approach.

$$Y_{ijt} = \alpha_i + \delta_t + \beta_1 Rec_{it} + \beta_2 Rec_{it} \times RC_j + \beta_3 Rec_{it} \times Post_t + \\ \beta_4 RC_j \times Post_t + \beta^{TD} Rec_{it} \times RC_j \times Post_t + X_{it} + \epsilon_{ijt} \quad (3)$$

Finally, as a check of whether the results for the subgroup of subsidy recipients correspond to the changes in payment behavior of the entire population, we also adopt a simple DID design in which we compare all individuals in treated cantons with all individuals in control cantons. As the coefficients of the OLS estimator for the entire population scale linearly with the share of the treated population and since about 25% of individuals receive premium subsidies, these estimates should be roughly a quarter of the size of the coefficients for recipients (if the outcomes of other groups indeed do not change over time). The estimation follows equation (1) with the sample not restricted to recipients.

We employ ordinary least squares regressions to estimate our coefficients of interest, i.e., $\beta^{DID1,2}$, and β^{TD}. Given our outcome variable measures whether there is a payment problem associated with a particular bill or not, this is in effect a linear probability model.

The most extensive specifications include individual and household level characteristics X_{it} that can vary over time (and are thus not picked up by the included individual fixed-effects). The corresponding variables for the insured person capture the purchase of accident coverage, the level of deductible, the payment rhythm, the means of payment, the bill issue date early or late in the month, the monthly supplemental health and dental insurance premiums, an estimate of individual income, and the estimated number of neighbors. At the household level, the variables approximated household size, approximated number of children in the household, estimate of total household income (adjusted to the size and the composition of the household), whether all members of the household are Swiss citizens, and whether no member of the household is a Swiss citizen are captured.

As noted above, we control for individual fixed-effects, i.e., only variation within individuals' payment history is exploited to identify the treatment effect. When we follow the same people over time around the reform year 2014, we exclude that the composition of the sample might be affected by the treatment and lead to spurious correlations due to sample selection.

Regarding the clustering of standard errors, we consider potential autocorrelation in two dimensions. First, we take into account that an individual's payment problems are likely correlated over time. Second, we allow for spatial autocorrelation within a canton as cantonal policies and conditions might lead to correlated consequences for people's payment problems. For example, insurance premiums are set on an annual basis for each canton separately, and thus individuals experience canton-specific premium increases.

Moreover, the recipient status introduced below is predicted separately for each canton and for each year. To take all these aspects into account, we thus adopt a two-way clustering of the standard errors at the individual and the canton-year level.

4 Data

4.1 Individual level information

We have access to the accounting records of the CSS Group, one of the largest Swiss health insurers, for the years 2012 to 2019 with an annual average of roughly 1.2 million individuals enrolled in compulsory health plans. Our initial data comprises all individuals who lived at least one day in one of the following thirteen cantons: Aargau (AG), Basel-Country (BL), Berne (BE), Fribourg (FR), Basel-City (BS), Grisons (GR), Lucerne (LU), Nidwalden (NW), Schwyz (SZ), St. Gall (SG), Thurgau (TG), Uri (UR) and Zurich (ZH). For each individual, we have information on the date of birth, gender, language, citizenship status, civil status, and approximate household size. Regarding individual health plans, we observe the insurer within the CSS Group (CSS, INTRAS, Arcosana, or Sanagate), the chosen deductible level and plan type in terms of managed care, accident coverage, and the premium. In addition, we have information on the preferred payment method (payment slip, direct debiting procedure, or electronic billing), payment periodicity (monthly, biannually, or annually), and we know whether bills are paid by the insured person or by a third party (e.g., his or her parents). We also have individual information on the number of supplementary health insurance plans and the total premium paid for these plans. Changes of health plan or personal details are observed on a daily basis. Finally, the data comprises a rough continuous income estimate ranging from very low income (-1) to very high income (1). Regarding the subsidized premiums, we observe for each recipient the monthly and annual amount, and the subsidy period. For the cash subsidies, however, we observe neither who the individual recipients are, nor the transfer amount in the relevant cantons before 2014. We therefore impute this recipient status (see Section 4.3 for further details) for each client in each calendar year.

For the individuals' payments, we have detailed information on each invoice. We observe the invoice type (e.g. health plan premium, co-payment bill), the billing amount, the billing date, and the receipt of payment. In other words, we can calculate the payment duration. We also know whether the individual received a reminder or a payment request and the final status of the bill (e.g., paid, debt collection, default, and so on).

4.2 Compilation of panel with monthly bills

We initially retrieve all mandatory health insurance premium bills for any of the 1.22 million CSS clients ever under contract between January 1, 2012 and December 31, 2019 in any of the thirteen cantons. As individual premium bills can have multiple entries (for example, partial payments, internal accounting offsets and so on), we aggregate the information for each unique bill ID. So we end up with the total amount due, the number of days elapsing until settlement, and the highest level of payment collection steps taken associated with each of these 63.9 million bills.

From this basis, we only retain individuals in our sample who had a mandatory health insurance contract with CSS for at least a full year before and after the reform in 2014. This holds for about 770,000 individuals. Furthermore, we remove all clients (about 80,000 individuals) who moved across cantons between 2012 and 2019 to prevent problems arising from their exposure to different premium subsidy systems. To this monthly client-bill panel, we merge the additional client and contractual information described above. As roughly 15% of individuals choose a billing interval other than monthly, we get, overall, an unbalanced panel structure, in which we observe monthly payers more frequently than other individuals.

Due to the lack of information on premium subsidy recipients under the cash transfer system, we choose to predict this status using a machine learning approach. To achieve strict separation between the observations we use to train the predictive models and those we use in our actual analysis, we put aside a random subset of 50% of year-client observations for learning purposes and subsequently consider only the remaining half for our analysis. These are roughly 22 million monthly observations of distinct health insurance premium bills from approximately 620,000 individual clients. Descriptive information (separated between before and after the reform as well as between control and reform cantons) on the number of bills and our two outcome measures for financial problems can be found in Table B1 in the Appendix.

Due to the Covid-19 pandemic, we cannot use the information about financial problems from both indicators over the entire time period. Specifically, the measure for debt collection is affected by the order of the federal government issued in spring 2020 to suspend all ongoing debt collection proceedings and to halt any new ones between March 19 and April 19, 2020.[17] As debt collection can be initiated approximately three to four months after the date a bill was due, debt collections for bills issued in late 2019 were generally affected by the Covid-19 pandemic and specifically by the corresponding government measures.[18] Accordingly, we have to exclude observations from the year 2019

[17] https://www.admin.ch/gov/de/start/dokumentation/medienmitteilungen/bundesrat.msg-id-78482.html (available only in German).

[18] In addition to the federal measures, some cantons (such as Berne) extended the halt of the debt

for the analysis on debt collection. For the analysis of payment reminders, however, we can use the observations from the year 2019 as any reminder for a bill issued in this year would have been sent out by February 2020 at the latest (and thus no yet affected by any disturbances caused by the looming pandemic).

Table B2 in the Appendix provides an overview of the composition of individual and household characteristics in our final sample across control and treated cantons before and after the reform. The descriptive statistics show that there have not been any substantial changes in the pool of individuals that would coincide with the reform we study.

4.3 Classification of subsidy recipient status

Cantonal governments paying out cash transfers prior to the harmonization in 2014 did not inform health insurance providers as to which of their customers receive premium subsidies. We are thus only able to directly identify the recipient status (and corresponding subsidy amounts) for all years in cantons belonging to the control group. For the cantons that switched to in-kind transfers in 2014, identification is only possible after the reform took place. Obviously, this presents a challenge to our empirical strategy, which relies on comparing recipients from treated and untreated cantons, as well as recipients with non-recipients within cantons. Therefore, the correct classification of the subsidy status of all individuals in our data over the entire time becomes a crucial step before we can begin our actual analysis of the effect of cash versus in-kind transfers on the financial distress of recipients.

Such a classification is possible, though, due to the large number of clients and the scope of information in our data. It allows us to recreate the cantonal premium subsidy eligibility rules closely enough to predict the status for every individual in each year.[19] Even though the formulas determining eligibility are public, we cannot directly apply them to our data, as even our detailed individual-level data do not offer sufficient information.[20] Given the highly non-linear eligibility formulas, simple logit regressions models

collection proceedings to the end of June 2020. Moreover, some health insurance companies turned more lenient. Overall, the turmoil resulted in an administrative backlog in debt collection proceedings.

[19]One alternative would be to simply hold the recipient status in 2014 (the first year we know it in all cantons) of each individual constant for every year. However, this approach is fraught with several issues that likely result in biased findings. First, recipient status varies quite substantially within individuals over the years we observe. By keeping the recipient status fixed in this way, only 73% of actual recipients would be correctly classified in 2012, just two years removed from the reform (see Figure D4 in the Appendix). Second, and more importantly, this approach leads by design to a 100% prediction accuracy in 2014. This is problematic, as it implies that prediction accuracy improves in a highly discontinuous manner that aligns exactly with the reform. We would thus have greater difficulty to separate how much of the outcome in our subsequent analysis can be attributed to the payment system and how much arose due to the distortions introduced by different pre- and post-reform levels in prediction accuracy than with a more dynamic prediction approach.

[20]For example, the indicator of income available to us is only an estimate and we do not know the true size and composition of every household in our data, as parents (who are clients of the health insurance

do not perform sufficiently well for estimating the propensity to receive subsidies. The corresponding predictions are too inexact[21], so that our results are biased significantly towards zero, most likely due to the high share of non-recipients (for whom the reform has no effect) being wrongly classified as recipients. For this reason, we opt to employ several machine learning algorithms that are better suited to dealing with the complex nonlinearities in eligibility determination.

4.4 Machine learning protocol

For our main analysis, we determine a predicted recipient status for *each* individual in the data set in *each* year, i.e. even when the actual status is known. We adopt this strategy in order to prevent any bias in the estimated treatment effect of the reform that would arise just because the status is fully accurate over the full duration of the sample in the control group but only after the reform in the treatment group. We proceed in four steps[22]:

First, we determine the "learning set". For this, we reduce the monthly panel described above to only one observation per individual and calendar year (as subsidy eligibility is determined on an annual basis). Then we separate the observations we use to train the prediction models from those on which we conduct our analysis afterwards. Our large data set enables us to randomly split our data in half, into a "learning set" and an "analysis set" and still have enough observations to conduct both tasks.[23]

Second, we define the exact training data sets. Specifically, we only use data from after the reform in 2014 for creating the prediction models for all cantons (i.e., also for those that always applied in-kind transfers) and all years. This ensures that information from the same years is used to tune the algorithms and to evaluate their performance for the cantons in the treatment and control groups. If treated otherwise, differences in the quality of the predictions at the reform date (with more accurate predictions for the cantons in the control group before the reform, observed over the whole period, than for those in the treatment group) might bias the results of the main analysis. In the end, we have roughly 2.44 million client-year observations left in our training data. Since each canton sets eligibility rules individually and income level thresholds differ substantially between cantons, we create cantonal subsets and train the models for each canton separately.

Third, we compute the necessary inputs for the prediction of the recipient status. As

company) might sign up their children at a different insurer.

[21]Figure D6 in the appendix provides an example of using the same prediction procedure as in our eventual machine learning approach, but instead relying on logistic regressions. It shows that only about 47% of the individuals predicted to be recipients in this way actually receive subsidies.

[22]Figure D1 in the Appendix provides an overview of the machine learning protocol in graphical form.

[23]This method resembles the *honest* approach proposed by Athey and Imbens (2016) for using machine learning to detect heterogeneous treatment effects. Since the observations used for later analyses had never been part of any prediction step, this procedure attenuates the problems associated with using generated explanatory variables.

the stacking of multiple models often performs better than a single, highly tuned algorithm (see, e.g., Mullainathan and Spiess, 2017), we use three different types of machine learning algorithms, *gradient boosted trees*, *random forests*, and *neural networks* for each of the thirteen cantonal training samples. We train the algorithms to detect premium subsidy recipients using five-fold cross validation. As this provides us with three different propensities to receive subsidies per individual-year observation, we then run a simple logistic regression of the predicted on the actual statuses to find the optimal weight for each of the three models (and do this again separately for each canton). Finally, we apply this weighted stack of models to the observations in the analysis set to predict the probability of receiving a subsidy.

Fourth, we determine the threshold likelihood above which an individual is classified as a subsidy recipient based on a data-driven approach. Specifically, we choose for each canton the value which maximizes the harmonic mean of precision and recall (also known as the F_1 score).[24] Once again, we only use observations made after all cantons had adopted in-kind transfers.

Ultimately, these four steps provide us with a binary prediction for the subsidy recipient status of each individual in every year we observe him or her in our data set used for the subsequent analyses.

4.5 Performance of the status classification

Out of sample (in the analysis set after 2014), we correctly classify 83% of individuals as recipients or non-recipients in each respective year (accuracy) with our machine learning approach. When individuals indeed receive premium subsidies in the respective year, we identify them as doing so in 73% of cases (sensitivity); for non-recipients this rate is 87% (specificity). Precision (how many individuals classified as recipients indeed receive subsidies) is 65%. This means that in the pool of predicted recipients, about one-third of individuals have not actually enjoyed reduced health insurance premiums in the year in question. Any estimated treatment effect is thus likely a lower bound.[25] Additional information on how well our predictions perform over time in treated and control cantons

[24] Alternatively, we could have chosen the cut-off point in such a way that the predicted share matches the share of actual recipients in our data in each year. However, as this share is not known for the cash transfer system, we would have had to extrapolate the share of recipients for each year, using official statistics for all insured individuals within a canton.

[25] Such an attenuation bias is also suggested when we apply the same classification process as in our machine learning approach, but rely on logistic regressions (including some simple interaction terms as explanatory variables that most more complex machine learning algorithms would be able to detect themselves). These simpler logit models perform substantially worse (73% accuracy, 69% sensitivity, 75% specificity, and 47% precision) in correctly predicting individuals' subsidy recipient status. Especially because the logistic regression model approach classifies many more non-recipients as recipients, the effects of the reform become much smaller if we use these less accurate predictions (see Tables D5 to D7 in the Appendix).

can be found in Figure D2 in the Appendix. We see some improvements in prediction quality after the reform relative to before in the cantons always applying in-kind transfers (for which it is possible to pursue this evaluation). Compared to the alternative approaches, as well as compared to the absolute levels, they are rather small in relative terms, however. Given that we estimate the propensity to receive subsidies in the same way for both groups of cantons, there is little reason for why any upticks we observe in the control cantons should be unique to this group. Any possible effects arising from different levels of prediction accuracy before and after the reform should therefore cancel out in our various difference-in-differences approaches.

5 Results

We discuss the results of our main analysis in three subsections. First, we provide graphical evidence depicting the mean outcomes of our dependent variables for financial distress on a monthly basis over time for recipients across reform and control cantons as well as for recipients and non-recipients within the group of reform cantons. Second, we present the results from the corresponding DID and TD estimates. Third, we explore the effect heterogeneity and robustness. And fourth, we show and discuss the possible side effects of the reform on the settlement of co-payment bills.

5.1 Graphical evidence for financial distress

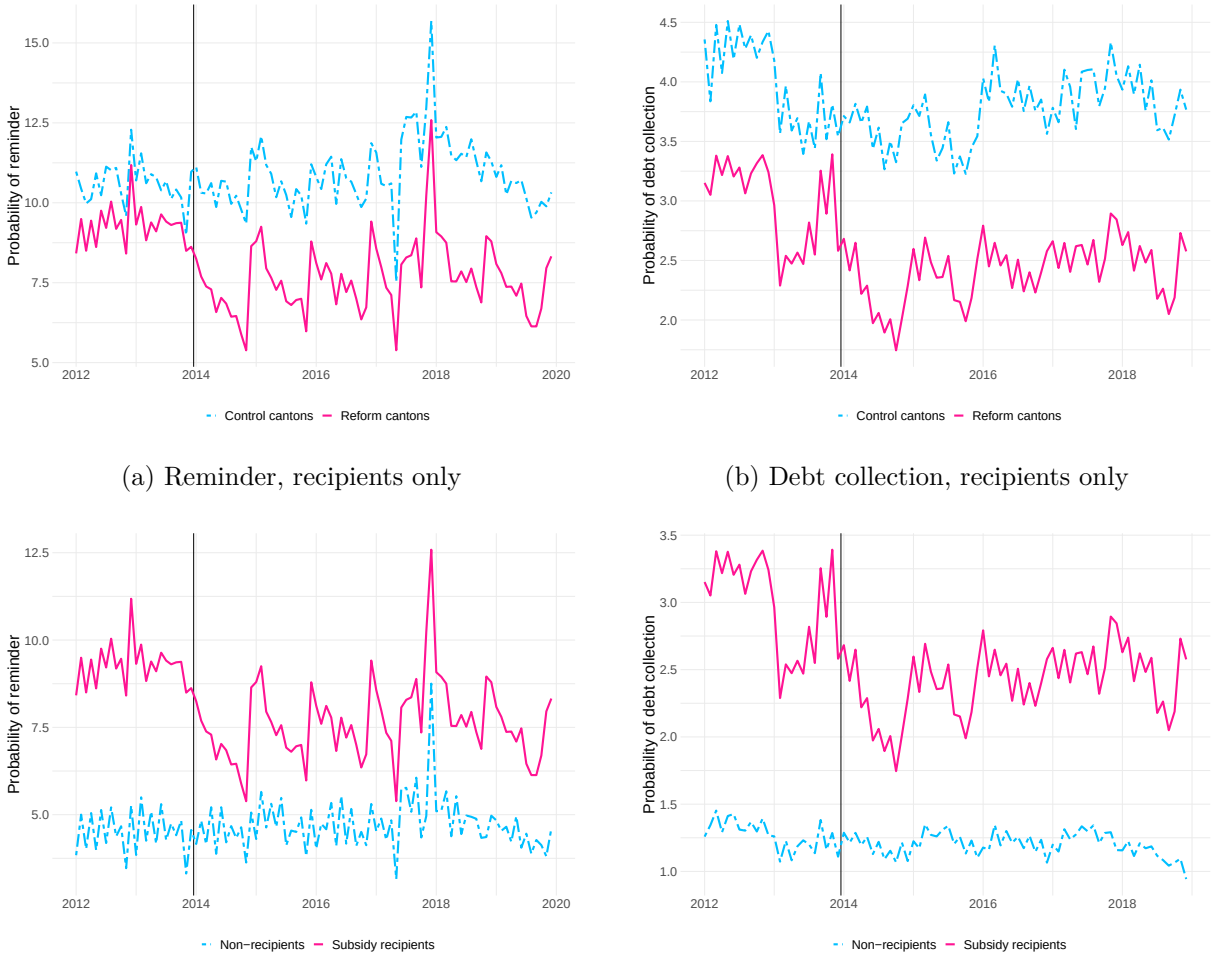
In Figure 3, we present the raw probability, aggregated over each month between January 2012 and December 2019 (December 2018 for debt collections), for a given bill that the recipient will receive at least a payment reminder or enter debt collection, respectively. The two panels on the left display the results for payment reminders, while the two on the right show results for debt collection. On the top row, we compare subsidy recipients[26] in the seven cantons that changed from cash to in-kind transfer on January 1, 2014 with recipients in the six cantons that always applied in-kind transfers. The bottom row only considers the cantons affected by the payment reform and compares subsidy recipients against non-recipients in these cantons.[27]

The graphical evidence in Figure 3 clearly indicates an effect of the reform on payment behavior for health insurance premiums.[28] Before the reform, the likelihood of cash

[26]In the following, the status of subsidy recipient (or non-recipient) always refers to the classification based on our machine learning procedure, unless otherwise noted.

[27]The same raw probabilities for all individuals in control and treated cantons are displayed in Figure C3 in the Appendix.

[28]Recipients of premium subsidies in cantons that always used in-kind provision generally had more problems with paying their premium bills than did recipients in cantons with cash transfers, both before and after the reform. While this might seem somewhat odd at first, it likely arises from the three control



(a) Reminder, recipients only

(b) Debt collection, recipients only

(c) Reminder, reform cantons only

(d) Debt collection, reform cantons only

Figure 3: Switch from cash transfers to in-kind premium subsidies and payment behavior

Notes: For all individuals, subsidy recipient status is determined based on the classification procedure outlined in Section 4.3.

transfer recipients receiving at least a payment reminder was, on average, 9.3% (see also Table B1). This likelihood immediately drops to around 7.6%, on average, once the same benefits are provided in-kind. Based on the averages for the two years before and after the reform, the difference of 1.7 percentage points represents a reduction in payment difficulties of roughly a fifth for the population affected by the reform. This effect appears to have been highly persistent (with ongoing regular seasonal patterns) over the six years observed following the reform.[29] It is noteworthy that pre-reform, the fractions

cantons having larger urban population centers, which are associated with higher levels of financial difficulties in general.

[29]In 2017, a significant downtick in May followed by a substantial uptick in December in the reminder probability can be observed. This pattern is due to a federal mandate requiring health insurance companies to issue bills before the start of a month with insurance coverage (rather than at the end of the respective month). Since the exact timing of this shift depended on the means of payment chosen by the clients and the number of bills per month, the composition of the affected pool of individuals varied

of payment reminders among recipients in control and treated cantons seem to follow a common trend, even unconditionally, which is a crucial requirement for our DID strategy later. Furthermore, and as expected, since there was no change for recipients in control cantons, which always applied in-kind payments, we observe little change in the probability of receiving at least a reminder. This provides some assurance that the substantial improvement regarding moderate financial problems in the treated cantons is not due to some nation-wide trend or a potential concurrent change in the billing process by the health insurer (which the company also assures us did not happen).

For severe financial distress, measured by the initiation of debt collection proceedings, we observe in panel (b) a pattern similar to that for payment reminders, albeit less stark. Debt collection proceedings (which start roughly three months after the due date of the premium bill) are substantially less likely to be initiated for subsidy recipients in treated cantons after the 2014 reform than before. No analogous reduction occurs for recipients in the control cantons. Under the cash regime, recipients had a probability of about 3%, on average, of each bill entering debt collection, while after the switch to in-kind transfers this mean probability decreases to roughly 2.4%. The corresponding 0.6 percentage-point decline suggests an improvement in severe financial problems of about 20%. However, this figure overestimates the effect of the reform on debt collection, as there was a similar decrease in its likelihood across all cantons from 2012 to 2013. Since the movement over time prior to the reform occurred in parallel in both the control and treated cantons, it will eventually be correctly captured in the DID approach below.

In panels (c) and (d), we also compare the development for recipients before and after the reform with that for non-recipients in cantons that changed payment modalities from cash to in-kind. This comparison should reveal whether there has been a general shift within the treated cantons that has led to the reduction in financial difficulties observed in panels (a) and (b). There seems to have been no general shift. Instead, and as expected, the likelihood for non-recipients of receiving a payment reminder and of experiencing debt collection proceedings does not change with the reform.[30] In contrast, for recipients, we

between the different months of 2017 leading to the observed patterns in reminders. Given the absence of a similarly extreme pattern in debt collections (as indicated in panel (b)), the abnormally high number of additional reminders in December 2017 is indicative of some confusion about the timing and the number of bills among some individuals, rather than a fundamental inability to pay the premium bills. As this mandated change applied to all cantons in the same way, we are not concerned that it affects our regression analyses that include year-month fixed effects.

[30]This observation can also serve as an indirect validation of our machine learning procedure. As we train our models for all the cantons only on the post-reform period, it would have been possible for a structural break in prediction accuracy and outcomes to occur that is synchronous with the reform. In fact, the alternative classification of holding recipient status constant at the status observed in 2014 for all years in our analysis would indeed introduce a substantial discontinuity in prediction accuracy. Tables D1 to D3 in the Appendix show that the corresponding results are much less stable over the different control groups. This becomes particularly pronounced in the design comparing recipients and non-recipients in treatment cantons only, as here there is no longer a group of other cantons where a similarly large shift in the quality of the classification occurred.

observe a substantially lower fraction of bills ending up with a reminder or the initiation of debt collection.

5.2 Estimation results for premium bills

We formally estimate the treatment effect of the switch from cash transfers to reduced insurance premiums on the financial distress of the treated subsidy recipients within DID and TD analyses. Figure 4 displays the corresponding coefficients of the most extensive specifications for the treatment effect for the three strategies using different control groups to compare subsidy recipients in treated cantons.[31] We present the 95%, 99%, and 99.9% confidence intervals of each coefficient as black, gray, and light gray lines.

The regression analyses applying multiple control strategies confirm the insights that we draw from the graphical evidence presented above. Compared to cash transfers, the in-kind provision through premium reductions leads to a substantial reduction in payment difficulties. The estimated effect sizes are only slightly smaller than the simple differences in means before and after reform in treated cantons discussed above. The results are thus robust to accounting for developments in the two relevant control groups. Overall, the estimated effects turn out rather stable across the different DID and TD strategies. Moreover, the estimated effects are statistically significant at the 99.9% level in the case of the probability of receiving a reminder, and at the 95% level in the case of the likelihood to enter debt collection. These levels are calculated based on standard errors adjusted for two-way, i.e., individual and canton-year clustering in all specifications.

Specifically, the first two rows show that the probability of each bill receiving at least a payment reminder is reduced by 2.25 percentage points when comparing recipients in treated cantons with those in the control cantons (see also Table C2 in the Appendix) and 1.81 percentage points when comparing recipients with non-recipients in treated cantons (see also Table C3 in the Appendix), respectively. These are the standard DID results with individual fixed-effects, year-month fixed-effects and various time-variant individual and household level controls. In row three, we combine the two approaches in a triple differences (TD) analysis. The development in payment behavior after the reform relative to before among recipients in treated cantons is compared to the development among recipients in control cantons and non-recipients in both treated and control cantons. With this comprehensive control strategy, a decrease of 1.81 percentage points in the probability of getting at least a payment reminder is estimated. As the likelihood of a payment reminder was 9.29% among recipients in the treated cantons before the reform, the coefficients for

[31] The full regression results for each estimation are presented in Tables C1 to C4 in the Appendix. Including year-month effects (instead of a single post-reform dummy as in standard DID) as well as numerous time-varying individual and household-level controls leads to virtually no change in the estimated coefficients of interest compared to the baseline with only individual fixed-effects.

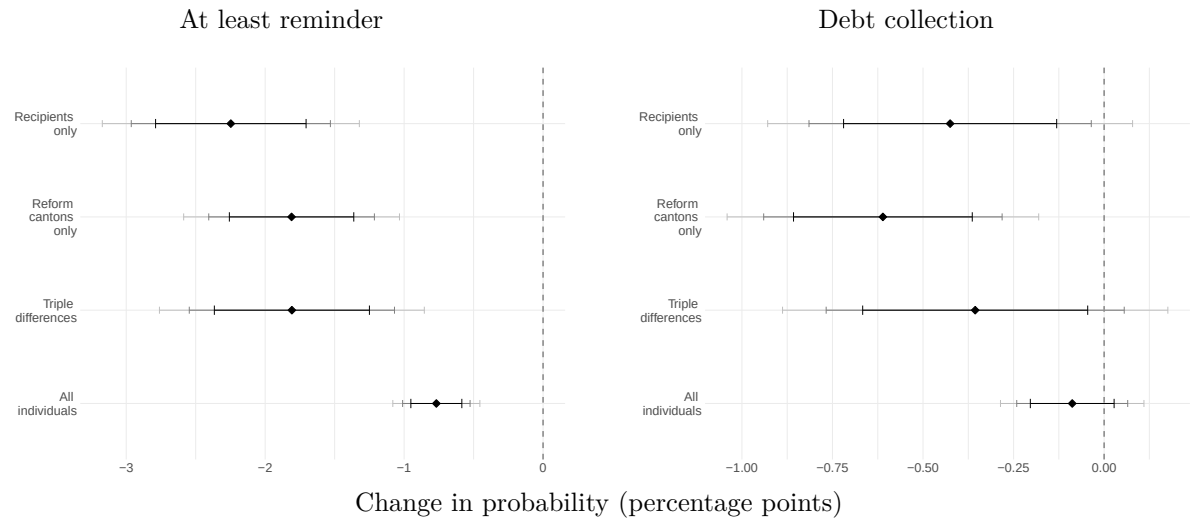


Figure 4: Treatment effect of payment reform on the payment problems of the treated subsidy recipients

Notes: Diamonds represent the beta coefficient of the relevant difference-in-differences (or triple differences, respectively) interaction terms. Among predicted subsidy recipients in reform cantons before the reform, the baseline probability for any given bill to receive at least a payment reminder is 9.29%, and 2.97% to enter into debt collection. All results include individual and specific year-month fixed-effects. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, timing of bill within month, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, household size and composition adjusted estimated income, all household members are Swiss citizens, no household member is a Swiss citizen. All standard errors are two-way clustered on the individual and canton-year levels. Black, gray and light gray lines correspond to the 95%, 99% and 99.9% confidence intervals, respectively. The complete results can be found in Tables C1 to C4 in the Appendix.

the estimated effects in the three specifications translate to relative reductions in response to the switch to in-kind of 24.2%, 19.5%, and 19.5%, respectively.

For severe financial problems, i.e, a bill entering debt collection, the estimated effect of the reform amounts to a reduction by 0.43 percentage points when comparing recipients in treated cantons with those in control cantons and by 0.61 percentage points when comparing recipients with non-recipients in treated cantons. Based on the TD, we estimate a reduction of 0.36 percentage points. Compared to the 2.97% probability of each bill entering debt collection among recipients in treated cantons before the reform, the relative reductions amount to 14.5%, 20.5% and 12.1%, respectively. The results for entering debt collection thus seem to be somewhat smaller and more sensitive to the chosen specification compared to the one for receiving a payment reminder. In the next section, we will discuss an adjustment to the subsidy scheme applying to the poorest individuals in one control canton as a potential driver of this sensitivity.

In the last row of the two panels in Figure 4, we compare the payment behavior for premium bills of all individuals (regardless of recipient status) in treated cantons with that of all individuals in control cantons. These estimations offer a cross-check of the results that rely on the classification of subsidy recipient status. We find that among all

individuals, the probability of receiving at least a reminder for a particular bill decreases by 0.77 percentage points relative to pre-reform and control cantons. Due to the properties of the OLS estimator, we can linearly scale the size of the coefficient to approximate the treatment effect on the treated. By multiplying the coefficient by four (as the share of recipients in treated cantons is, on average, about 25%), we get an extrapolated effect on the treated of about 3 percentage points. This effect size is larger than the ones estimated based on the three strategies that make use of the imputed recipient status. While a misclassification of the recipient status biases the effect towards zero (and might thus speak for the results that do not require this information), we still consider it more important to control for unobserved confounders (and accordingly prioritize the three specifications discussed before). For the likelihood of debt collection proceedings, quadrupling the estimated coefficient of -0.09 percentage points (not statistically different from zero at the 95% level) over all individuals yields an extrapolated treatment effect on those treated of about -0.36 percentage points. This effect size is the same as the one estimated based on the TD strategy and slightly smaller than the ones estimated based on the DID strategies. Overall, we interpret the estimates of the cross-checks as support for the general results.

5.3 Effect heterogeneity and robustness

In order to further illuminate the consequences of cash versus in-kind transfers, we analyze potential differences in the behavioral reactions depending on people's payment methods and the amount of subsidies they receive. As a kind of placebo test, we estimate the effect of the reform on predicted non-recipients. Finally, the sensitivity of the findings is studied with regard to two sub-samples of treated cantons and alternative strategies to classify individuals' recipient status.

5.3.1 Differential effect by payment method

The effects of the reform are expected to depend on people's payment method. In our context, individuals can either pay their premium bills by payment slip (which requires an active effort to settle each bill), direct debit (where the transaction is automated by the bank), and e-bill (which is done via online banking and payment can be executed either manually or automatically). The larger the latitude of a subsidy recipient in the timing of the payment of his or her premiums, the larger any potential effect of the regime switch is expected to be. Thereby, individuals aware of potential self-control problems under a cash transfer system might well opt to use direct debit as a commitment device. Relatedly, the group using the payment slip method might be particularly at risk as it includes those individuals who (mistakenly) do not consider it a risk to their ability to pay ongoing expenses later on when they experience a large influx of liquidity early in the year

due to the cash transfer. Figure C4 in the Appendix presents graphical evidence that the reduction in payment problems after the switch to in-kind was indeed most pronounced among those recipients who had a non-automated method of payment. In fact, we detect almost no impact on subsidized individuals who were not required to undertake an active effort in the timely payment of each premium bill. This latter finding and the absence of any similar development for payment slip users in the control cantons can also be seen as indirect evidence that the reduced number of financial problems is indeed driven by the reform.

5.3.2 Differential effect by the amount of premium subsidies

The regime change in the payout system is expected to matter more where higher transfers are involved. To test this idea about treatment intensity, we apply the same procedure as for predicting the recipient status to estimate the subsidy amount an individual receives (as a fraction of the average cantonal premium for his or her age group and region). We group the estimates in quintiles and show the raw probability of payment problems separately for each of these five groups in Figures C6 and C7 in the Appendix. For individuals who most likely received a large fraction of their premiums subsidized, the graphical evidence clearly shows that they are affected the most by the payout reform. As one would expect, no such development is observed for the most highly subsidized group of individuals in control cantons. For them nothing changed from 2013 to 2014 as they received their subsidies in the form of premium reductions throughout.

5.3.3 Effects for predicted non-recipients

If the regime change is not picking up some other institutional variation than the switch from cash transfers to subsidized insurance premiums, then no effect of the reform on non-recipients is to be expected. As we do not know the exact status, we cannot implement this kind of placebo test. Instead, we resort to compare *predicted* non-recipients in control and reform cantons. Indeed, as evident in Table C5 in the Appendix, the coefficients are much smaller than for predicted recipients across the two groups of cantons. However, they are not zero. This is most likely the case because we incorrectly classify about a quarter of actual recipients as non-recipients. These wrongly assigned non-recipients then in turn could cause the small improvement in payment behavior we detect in this subsample.

5.3.4 Effects for subsamples of treated cantons

The harmonization of the payout system by 2014 was the only change in the federal reform of the mandatory health insurance that pertained to the premium subsidies (and thus with

direct potential consequences for people's financial distress). However, in some cantons there were some other institutional changes as well. As mentioned when we detailed the selection of cantons for our analysis, there are several cantons among both the control and treated group that undertook some minor adjustments to their cantonal premium subsidy schemes at the same time as the harmonization was implemented. To check the sensitivity of our findings with regard to potentially confounding institutional changes, we repeat the same analyses as before, but now separately for the group of cantons that either changed nothing at all or only the payout system (LU, SZ, BL, GR, ZH, BS, AG), and the group of cantons were some tweaks either coincided with the reform or took place temporarally close to it (UR, NW, TG, BE, FR, SG).[32]

The results for the two sub-samples of cantons are displayed in Figures C8 and C9 in the Appendix. For the probability of receiving at least a payment reminder, the effects appear to be quite similar between the former and later group of cantons. If anything, the effects appear to be slightly more precisely estimated using only the group of cantons without any further changes around the time of the reform (potentially because the tweaks in the other cantons introduced some noise). Overall, the payout system seems to play such an important role for people's payment behavior that it dominates any potential systematic effects from other changes to the subsidy schemes.

For the probability of debt collection in the group of cantons where nothing else (other than the payout system) changed around the switch from cash to in-kind, we find a large and statistically significant reduction in severe financial difficulties across all the specifications. With a relative reduction of about 13.8% (based on the TD specification), the effect size becomes roughly one sixth larger than the estimate we obtain from the full sample. For the sub-sample of treated and control cantons where additional changes have taken place the results differ depending on the control strategy. When we rely on the variation between recipients and non-recipients in treated cantons, we find a statistically significant (at the 95% level) relative reduction of 13.6%. This is similar to the effect of the switch from cash to in-kind we found in the sub-sample of cantons with no other changes. However, when we also incorporate observations from the control cantons, the estimated TD coefficient is lower and amounts to -0.16 (a relative decrease of 5.6%), whereby this effect is not statistically significant. It turns out that the smaller negative effect in this specification seems to be driven by observations from the canton of Berne. Just prior to the payout system reform in 2014, the canton of Berne increased the premium subsidies

[32]With the exception of the changes in two cantons, i.e., the cantons of Nidwalden (see Footnote 16) and Berne (which we will discuss in more detail below), they were of a substantially less fundamental nature than the switch in the payout system and referred mostly to eligibility. Since the overall amount in premium subsidies is to a large extent fixed by federal law, any (mostly small) increases or decreases to the recipient share from year to year thus move the generosity per recipient in the respective opposite direction. Consequently, it is quite difficult to predict in which direction these changes are affecting overall financial distress (and potentially bias the estimated effects of the payout system reform).

to recipients of other means-tested programs (and thus to individuals most likely to have severe financial problems) (B,S,S., 2015). This would explain why we observe a substantial reduction in debt collections for health insurance premium bills in the canton of Berne, which serves as a control canton in our analysis (and thus depresses any effect due to the switch to in-kind subsidies in the treated cantons).[33]

5.3.5 Alternative strategies for the classification of subsidy recipients

Our empirical strategy relies on the classification of people's status as subsidy recipients. As the above discussion of the results for predicted non-recipients has shown, the estimated effect size likely depends on how well the classification into recipients and non-recipients works. However, as we have no benchmark with perfect information, we can only asses the sensitivity of the results vis-à-vis alternative classification strategies. One straightforward alternative simply considers and holds constant an individual's status in the first year after the reform. After this point in time the status is known across all cantons. While the prediction is now fully accurate in 2014, it is for sure not in the other years. The degree of inaccuracy is difficult to assess. Specifically, we have no possibility to assess whether the quality of the classification is better or worse in the treated cantons before the reform in comparison to the one achieved with the ML approach employed in the main analysis. If we get qualitatively similar results, the results in the main analysis are less likely a statistical artefact driven by the sophisticated classification algorithm. The findings of the corresponding analyses are reported in Tables D1 to D3 in the Appendix. They are indeed similar to the ones in the main analysis. If anything, slightly larger point estimates are observed. For example, in the TD specification the relative reduction after the switch from cash to in-kind amounts to 25.7% for payment reminders and 16.3% for debt collection.

A more standard approach than machine learning to classify subsidy recipients would rely on a logistic regression model to estimate the propensity of each individual to receive premium subsidies on a yearly basis. Since eligibility depends on many interacting factors, a simple logistic regression performed on the same data likely performs much worse though in this task than the more sophisticated ML algorithms. Indeed, whereas about 30% of the individuals predicted to receive subsidies based on the ML model did not in fact do so, this fraction rises to over 50% when applying a logistic regression (see also Figure D6 in the Appendix). This high degree of misclassification means that we attempt to measure the effect of a treatment (switching from cash to in-kind) with a sample of

[33] If we exclude observations from the canton of Berne, we find statistically highly significant effects for debt collection that are close in size to those of the cantons without any further adjustments. Specifically, the estimated coefficient in the TD specification jumps from -0.16 ($p < 0.5$) to -0.39 ($p < 0.05$) when we exclude Berne. As a side note, the exclusion of observations from the canton of Berne also stabilizes the estimates for debt collection across the different control strategies in our main pooled specifications.

predicted recipients including many individuals who never actually received the treatment. In the concrete application, the resulting coefficients for the treatment are thus likely substantially downward biased. This is consistent with the results in Tables D5 to D7 in the Appendix reporting substantially smaller effect sizes than observed in the main analysis. For TD, and relying on logistic regressions for the prediction of recipient status, we only estimate a 13.7% reduction for payment reminders and a decrease of 4.6% for debt collection. Even though a relative reduction of over one eight in late payments would seem quite large on its own, it is only half of the effect we find with the better classification strategy based on ML.

5.4 Financial distress in the payment of co-pay bills

In traditional economic reasoning, cash transfers are attractive due to the flexibility they offer recipients. In the case of an income or expenditure shock, means are optimally allocated so that the overall mental and financial costs of delaying the settlement of some bills but not others are minimal. Accordingly, the observed payment behavior for the particular category of insurance bills under the cash regime was optimal, and providing the in-kind subsidy in the form of a reduced insurance premium restricts optimization and may well lead to more financial distress, albeit in other areas. We address this argument that is relevant from a welfare perspective in two ways. First, we provide some arguments that put the importance of paying insurance bills and the specific indicators of financial distress in perspective. Second, we empirically assess potential side-effects and analyze payment behavior for largely unexpected expenditures.

According to law, insurers are not allowed to limit insurance coverage or terminate contracts for basic coverage if customers do not pay their premium bills on time. However, as long as outstanding premiums are not fully paid, individuals are not allowed to change their health insurance provider and thus cannot benefit from less expensive alternatives. Moreover, some cantons also limit individuals' access to medical care (and serve basic medical needs only) once health insurers initiate debt collection against delinquent clients. Independently of any implications for medical care, entering debt collection might have severe consequences more broadly and is thus not chosen lightly. In particular, landlords regularly ask for a current extract from the debt register to screen out financially distressed applicants. We thus consider entering debt collection to be an indicator of general financial problems and not just a reflection of domain-specific payment behavior or a consequence of short-term liquidity management.

We explore this interpretation based on an empirical test for potential unintended consequences in payment behavior in another spending category. Specifically, we can study the same people's payment behavior for co-pays for medical services. These include all expenses for hospital treatments, purchases at pharmacies, and in some cases doctor

visits (particularly in physician networks). The medical service provider first sends the invoice to the health insurance company, which in turn bills their clients for the full amount until the yearly deductible is reached. The median medical co-pay bill in our data amounts to 68.6 Swiss Francs. Bills for co-pays are always sent on a separate invoice to customers and, thus, there is no administrative connection between billing for premiums and co-pays. There is no subsidy scheme for medical co-pays, so payment system reform did not affect this area in any way. Importantly, health insurers are by law not allowed to use the premium subsidy to offset any co-pay bills. Otherwise, the billing and payment collection process is identical to the one for bills for health insurance premiums. For data quality reasons we had to exclude the year 2012 from our analysis of co-pay bills, i.e., we have to rely on one year of data prior to the reform in our analysis.[34]

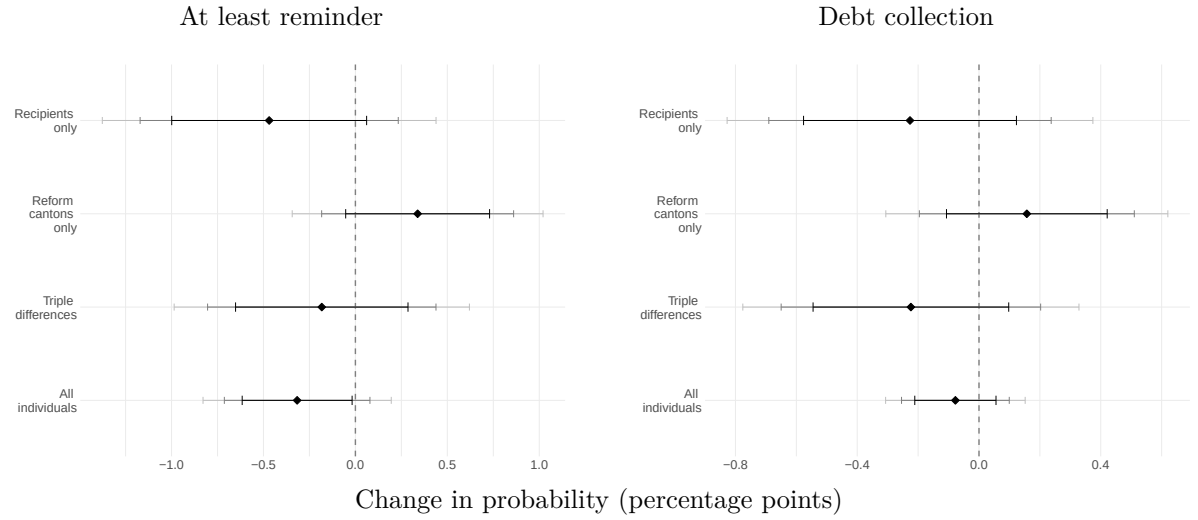


Figure 5: Treatment effect of the reform on payment problems for medical co-pays of the treated subsidy recipients

Notes: Diamonds represent the beta coefficient of the relevant difference-in-differences (or triple differences, respectively) interaction terms. Among predicted subsidy recipients in reform cantons before the reform, the baseline probability for any given bill to receive at least a payment reminder is 8.36%, and 2.27% to enter into debt collection. All results include individual and specific year-month fixed-effects. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, timing of bill within month, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, adjusted average estimated income in household, all household members are Swiss citizens, no household member is a Swiss citizen. All standard errors are two-way clustered on the individual and canton-year levels. Black, gray and light gray lines correspond to the 95%, 99% and 99.9% confidence intervals.

In Figure 5, we present the main DID and TD coefficients for the reform effect on payment behavior regarding co-pay bills of subsidy recipients. The full regression tables can be found in Tables C6 to C9 in the Appendix. In contrast to the effect on premium bill payment, we do not observe a systematic reaction in the settlement of medical co-

[34] As with the premium bills, we exclude the year 2019 for the estimations regarding debt collection due to the government measures against the Covid-19 pandemic in March 2020.

pays. Reminders and debt collection do not become more likely among subsidy recipients after the reform (the probability before the reform thereby amounts to 8.36% of at least a reminder and 2.27% of debt collection). Rather, we observe most point estimates to be negative; however, they come with relatively large standard errors. We interpret the evidence as a null effect and an indication that no unintended consequences occurred by switching from cash to in-kind subsidy payments in this additional domain of household expenditures.

6 Conclusion

Effective redistribution is determined by much more than the amount of transfers. Not only is it difficult to reach some of those who qualify for subsidies because they have no fixed address, no bank account or do not pay taxes, but also potential beneficiaries are required to apply. Disbursement and take-up are hands-on problems for welfare programs and indicate that the procedural aspects of means-tested transfers matter.

With our analysis, we want to underscore the importance played by procedural "details" in people's financial distress, in particular, if individuals' capacity to plan consumption and their motivation to stick to plans are limited. These individuals might best be helped if transfers simplified their financial planning and freed up mental capacities for other tasks. This aspect is inherently related to the decision as to providing subsidies in cash or in-kind.

Our empirical study exploits a quasi-natural experiment with health insurance premium subsidies in Switzerland to assess the relevance of payment modalities for recipients' payment problems. In a DID and TD setting, we estimate a substantial reduction in payment problems for treated subsidy recipients after the switch is made from cash transfers to reduced insurance premiums. Moreover, using the same setting and variation, we do not observe any unintended consequences regarding recipients' payment behavior on unsubsidized co-pays for medical services. This suggests an overall reduction in financial distress.

The discussion on a (unconditional) basic income replacing or complementing means-tested programs should consider that financial problems are not just a matter of means but also of procedures. Complementary research should further explore the potentially crucial role of these aspects for people's (economic) well-being. Transfers in cash versus in-kind are only one key aspect. Another aspect is whether the transfer comes anonymously or involves some social interaction, for example, with a social worker. If it were to play a role we should know it when we design the transfer programs for our future.

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Transfer Payment Systems and Financial Distress: Insights from Health Insurance Premium Subsidies

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December 23, 2021

Online Appendix

Table of Contents

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A The internal bill collection process

The insurer follows a clearly organized bill collection process that guarantees an equal treatment of all the customers. Figure A1 provides a graphical overview showing the different steps and minimal duration. However, it is important to note that the process can last up to four years. In what follows, we first explain for which bills this process applies. Afterwards, we describe the different steps of the process.

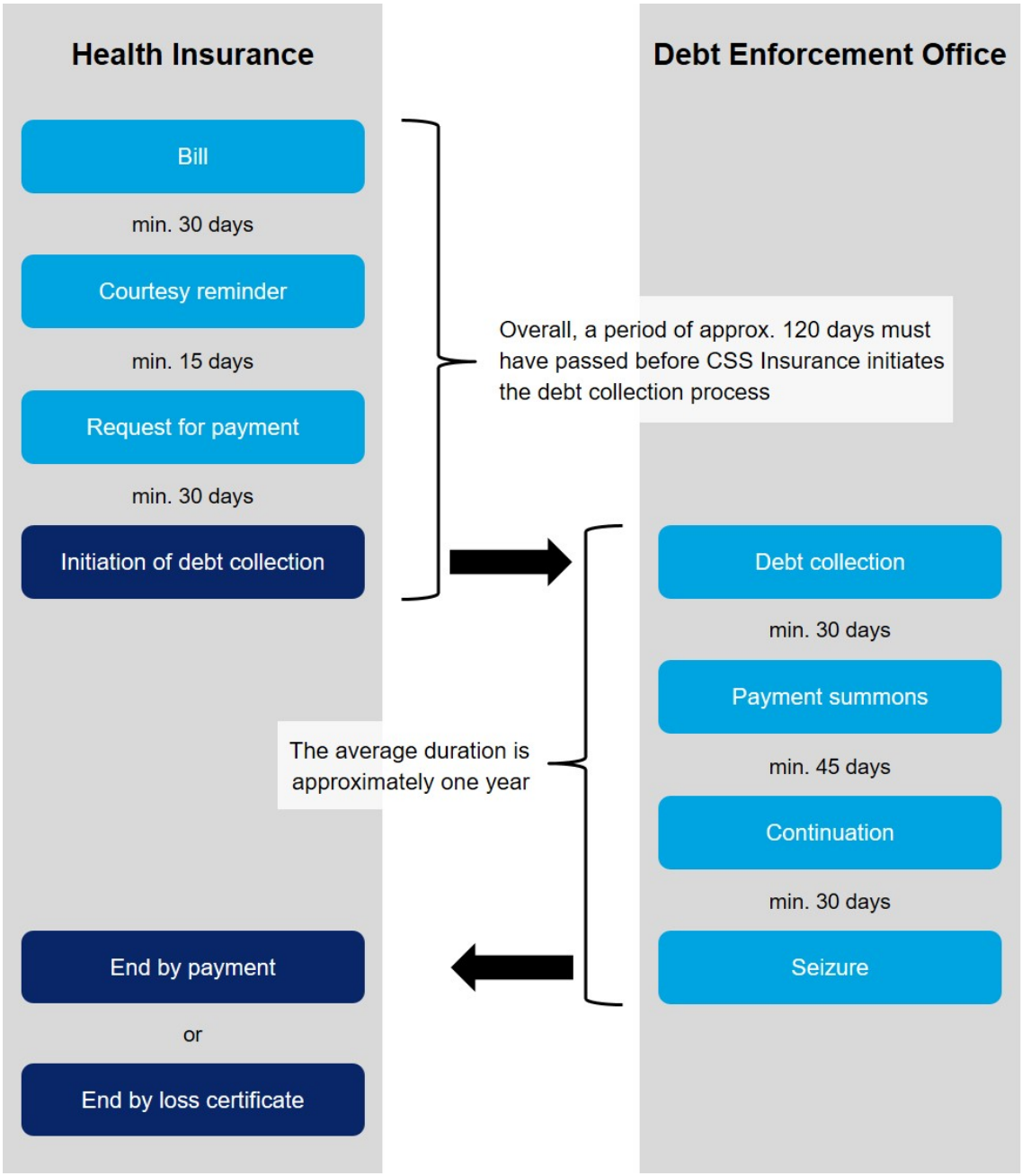


Figure A1: Overview of the bill collection process

Basically, there are two bill types that are relevant in our context: Health plan premium bills and co-payment bills. Regarding the latter, it is important to note that two health provider remuneration systems co-exist (see Schmid, 2017, for details). In the *payer system* ("tiers payant"), the health care providers send their bills to the health insurance, which reimburses the providers and charges the patients for their cost share. Here, the patients owe the insurance the co-payments, that is, the health insurance debt collection process also applies to co-payment bills under the payer system. Note that about 70% of all health services bills corresponding to about 80% of the health care costs are remunerated in the payer system. In the *guarantee system* ("tiers garant"), however, the health care providers send their bills to the patients, which (have to) pay the bills themselves and forward copies to their insurance. After receiving the copy, the insurance reimburses the patient within an average of 10 days (Federal Office of Public Health, 2015). Here, the billed amount is owed the health care providers, which have to initiate the debt collection process if the patient does not pay. Consequently, we consider health plan premium bills and the *payer system* co-payment bills. These bills undergo the following process:

(a) **Courtesy reminder:** The header of each bill shows the issue date and the due date.[1] The difference between these two dates is always 30 days or more. If no payment is recognized before or latest on the due date, CSS Insurance mails a courtesy reminder to the customer. The courtesy reminder shows again an issue date and the *new* due date. Essentially, the reminder gives the customer (at least) an additional fifteen days to pay the billed amount. There are no further consequences if the receipt of payment is before or on the new due date.

(b) **Request for payment:** If the bill remains unpaid after the courtesy reminder, CSS Insurance mails a request for payment to the customer. The customer has (at least) an additional 30 days to pay the billed amount. However, the payment request includes a penalty charge of CHF 20.00; in addition, it warns the customer of further negative consequences in case the billed amount remains unpaid (e.g. the initiation of debt collection; in case of supplementary insurance plans, CSS Insurance can suspend the contract). If there is still no receipt of payment, the billed amount is

[1] In case the printed due date is a Saturday, Sunday or holiday like Ascension Day, the effective due date is the following Monday

on hold for debt collection. However, a period of approximately 120 days must have passed before CSS Insurance finally initiates debt collection.

(c) Debt collection: CSS Insurance initiates debt collection by submitting a request for enforcement to the debt enforcement office.[2] The debt enforcement office is a public institution, which is organized in each canton separately. However, debt enforcement follows a strict process that is determined by federal law. It is important to note that the request for enforcement has to be sent to the office located where the customer lives (e.g. the office in the canton of residence); in addition, debt enforcement is not free and CSS Insurance has to pay the proceedings costs in advance (the costs vary with the amount of debt). After receiving the request, the debt enforcement office issues a payment summons to the customer.

(d) Payment summons: After receiving the payment summons, the customer has three options. First, the customer accepts that he or she has to pay the amount due. In this case, the customer has 20 days from receipt of the payment summons to pay the amount of debt *and* the debt enforcement costs, which were paid by CSS Insurance in advance $\rightarrow (g)$. Second, the customer can contest the payment summons if he or she does not agree with the amount demanded. In this case, the customer has to make an official objection within 10 days after receiving the payment summons. Note that the customer does *not* have to state any reason $\rightarrow (e)$. Third, the customer does neither contest the payment summons nor pay the amount due $\rightarrow (e)$. Overall, it takes a minimum of 30 days between the debt collection request and the delivery of the payment summons. However, if the customer is inaccessible, it can take much longer.

(e) Continuation: If the customer does neither contest the payment summons nor pay the amount due, CSS asks the debt enforcement office to continue the debt enforcement proceedings $\rightarrow (f)$. However, if the customer files an objection to the payment summons, the debt enforcement is on hold. To overrule the objection and to continue the debt enforcement proceedings, CSS Insurance now has to apply to the appropriate authority, which checks whether the debt is valid. The initial authority

[2]Note that CSS Insurance initiates debt collection only four times a year. Hence, the duration from the initial billing to the initiation of debt collection is typically between 120 and 180 days.

differs between basic and supplementary health insurance. In principle, if the debt stems from basic health insurance premiums or co-payments, the initial authority is the legal department of CSS Insurance; if the debt are related to supplementary health insurance plans, the initial authority is commonly a regional court.[3] In any case, the last resort is the Federal Supreme Court of Switzerland. Once the debt is legally decided to be valid, CSS asks the debt enforcement office to continue the debt enforcement proceedings $\rightarrow (f)$.

(f) Seizure: The continuation of the debt enforcement proceedings results in the seizure of customer's assets, which also includes garnishment of wages. However, asset seizure and wage garnishment is limited, that is, it is only possible if the customer's standard of living does not fall below a certain level. If some debts still remain unpaid, CSS Insurance receives a loss certificate that shows the amount of the loss incurred. The loss certificate allows CSS Insurance to collect the debts within a period of 20 years (of course, the financial situation of the customer has to improve first).

(g) End of process: The process ends either by payment of the debt, e.g. the seized assets offset the debt, or by receiving the loss certificate.[4] If CSS Insurance receives a loss certificate for debts related to basic health insurance, the customers' canton of residence covers 85% of the amount excluding the debt enforcement costs.

[3]Note that CSS Insurance also has the possibility to terminate or suspend supplementary health insurance plans if the corresponding premiums or co-payments are not paid. In other words, not paying these debts implies a loss in coverage.

[4]Note that the process can also end by writing off a bad debt. However, this is only possible for debts related to supplementary health insurance plans as the insurers are obliged to collect all debts related to mandatory health insurance plans.

B Additional descriptive statistics

Table B1: Descriptive statistics for premium bills

	Control cantons		Reform cantons	
	Before	After	Before	After
All individuals				
Number of unique bills (2012–2019)	3,891,029	10,958,760	1,870,653	5,279,206
Fraction of bills with payment reminder	7.24% (25.92)	7.61% (26.52)	5.81% (23.39)	5.49% (22.78)
Number of unique bills (2012–2018)	3,891,029	9,279,998	1,870,653	4,451,095
Fraction of bills entering debt collection	2.39% (15.26)	2.26% (14.88)	1.71% (12.96)	1.50% (12.15)
Predicted subsidy recipients				
Number of unique bills (2012–2019)	1,189,113	3,238,171	487,070	1,317,527
Fraction of bills with payment reminder	10.60% (30.79)	10.89% (31.15)	9.29% (29.04)	7.63% (26.54)
Number of unique bills (2012–2018)	1,189,113	2,767,854	487,070	1,123,847
Fraction of bills entering debt collection	3.98% (19.55)	3.74% (18.98)	2.97% (16.99)	2.40% (15.30)
Predicted non-recipients				
Number of unique bills (2012–2019)	2,701,916	7,720,589	1,383,583	3,961,679
Fraction of bills with payment reminder	5.76% (23.31)	6.24% (24.18)	4.58% (20.91)	4.78% (21.34)
Number of unique bills (2012–2018)	2,701,916	6,512,144	1,383,583	3,327,248
Fraction of bills entering debt collection	1.68% (12.87)	1.64% (12.69)	1.26% (11.17)	1.20% (10.87)

Notes: Standard deviation in brackets.

Table B2: Descriptive statistics for explanatory variables

	Control cantons		Reform cantons	
	Before	After	Before	After
Accident insurance	0.57 (0.5)	0.56 (0.5)	0.56 (0.5)	0.55 (0.5)
Monthly supplemental health insurance premiums	89.19 (155.4)	95.2 (161.84)	97.29 (150.81)	104.62 (159.49)
ln(Monthly dental insurance premiums)	0.45 (1.12)	0.52 (1.23)	0.49 (1.15)	0.57 (1.27)
Income (estimate, 0 to 2 range)	0.41 (0.23)	0.4 (0.22)	0.36 (0.2)	0.35 (0.2)
Number of neighbors (estimate)	500.47 (514.67)	504.63 (509.24)	600.27 (626.54)	611.56 (628.35)
Household equivalent income	0.24 (0.2)	0.24 (0.2)	0.21 (0.17)	0.21 (0.17)
Household size	2.45 (1.41)	2.35 (1.37)	2.51 (1.41)	2.38 (1.36)
Number of children in household	0.79 (1.08)	0.69 (1.03)	0.78 (1.08)	0.65 (1.02)
All household members Swiss citizens	0.67 (0.47)	0.67 (0.47)	0.73 (0.44)	0.73 (0.44)
No household member Swiss citizen	0.26 (0.44)	0.27 (0.44)	0.21 (0.41)	0.22 (0.41)
Lowest deductible	0.56 (0.5)	0.56 (0.5)	0.57 (0.5)	0.56 (0.5)
Monthly billing rhythm	0.87 (0.34)	0.88 (0.32)	0.88 (0.33)	0.89 (0.32)
Payment by direct debit	0.43 (0.5)	0.45 (0.5)	0.53 (0.5)	0.56 (0.5)
Bill issued early in month	0.62 (0.49)	0.63 (0.48)	0.48 (0.5)	0.51 (0.5)

Notes: Standard deviation in brackets. Based on all premium bill observations (21,999,648 in total) between 2012 and 2019 from 621,593 unique individuals.

Table B3: Descriptive statistics for co-pay bills

	Control cantons		Reform cantons	
	Before	After	Before	After
All individuals				
Number of unique bills (2013–2019)	437,639	3,598,017	191,003	1,634,852
Fraction of bills with payment reminder	5.96% (23.68)	7.63% (26.55)	5.30% (22.41)	6.67% (24.95)
Number of unique bills (2013–2018)	437,639	2,913,707	191,003	1,318,956
Fraction of bills entering debt collection	1.57% (12.43)	1.76% (13.14)	1.03% (10.09)	1.09% (10.39)
Predicted subsidy recipients				
Number of unique bills (2013–2019)	147,482	1,066,288	54,063	404,854
Fraction of bills with payment reminder	8.99% (28.61)	11.20% (31.54)	8.36% (27.68)	9.86% (29.81)
Number of unique bills (2013–2018)	147,482	878,385	54,063	333,050
Fraction of bills entering debt collection	2.90% (16.78)	3.58% (18.57)	2.27% (14.89)	2.50% (15.60)
Predicted non-recipients				
Number of unique bills (2013–2019)	290,157	2,531,729	136,940	1,229,998
Fraction of bills with payment reminder	4.42% (20.55)	6.13% (23.98)	4.10% (19.82)	5.62% (23.04)
Number of unique bills (2013–2018)	290,157	2,035,322	136,940	985,906
Fraction of bills entering debt collection	0.89% (9.41)	0.97% (9.82)	0.54% (7.33)	0.62% (7.84)

Notes: Standard deviation in brackets. Co-pay bills data is only available from 2013 on.

Table B4: Average cantonal premiums and subsidies

	Control cantons		Reform cantons	
	Before	After	Before	After
Average premium	3190.3	3574.1	2697.4	3095.0
Average subsidy per recipient	1796.4	2014.1	1416.0	1726.0
Average remaining premium load for recipients after subsidy	0.441	0.444	0.482	0.442

Notes: Data stems from the official statistics of the Federal Office of Public Health (FOPH) and represents the averages for all individuals in the cantons. All values in Swiss Francs. All cantons have equal weight in calculating the averages (i.e., the figures are not adjusted for population size).

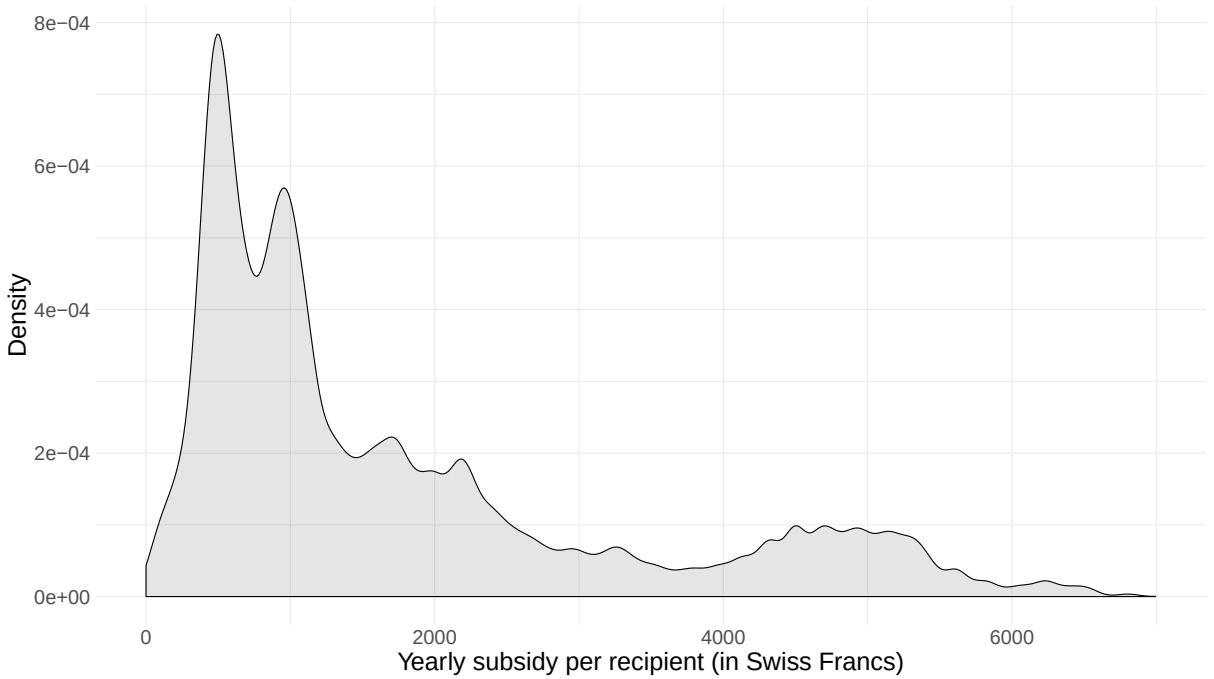


Figure B1: Distribution of yearly premium subsidy per recipient

Notes: Multiple persons in a household can receive subsidies. Here, we show the subsidy on a per person basis. The data is covering recipients from all the seven cantons in the years after the payment reform.

C Complete results and robustness checks

C.1 Premium bills

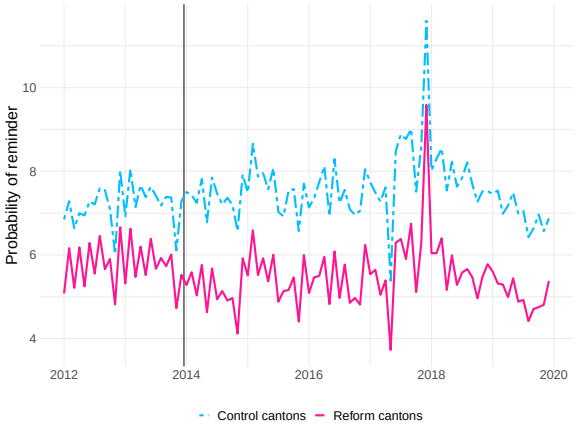


Figure C1: Reminder, all individuals

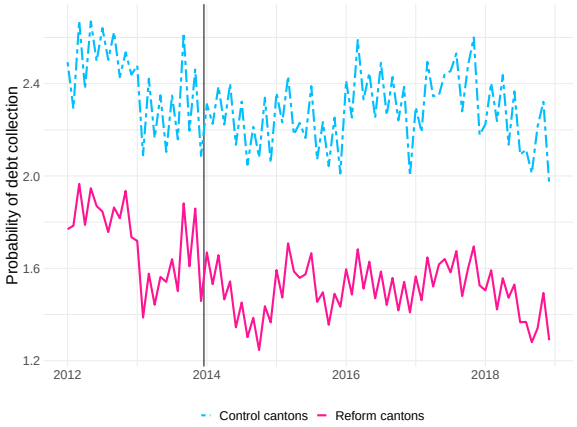


Figure C2: Debt collection, all individuals

Figure C3: Switch from cash transfers to in-kind premium subsides and payment behavior

Table C1: Effect of the payment reform on payment problems (all individuals)

	At least payment reminder			Entered debt collection		
	(1)	(2)	(3)	(4)	(5)	(6)
Post reform	0.37***			−0.12**		
	(0.10)			(0.06)		
Reform canton × post	−0.70***	−0.70***	−0.77***	−0.07	−0.07	−0.09
	(0.13)	(0.09)	(0.09)	(0.08)	(0.06)	(0.06)
Individual fixed-effects	✓	✓	✓	✓	✓	✓
Specific month fixed-effects		✓	✓		✓	✓
Ind. & hh level controls			✓			✓
Observations	22.0M	22.0M	22.0M	19.5M	19.5M	19.5M
Adjusted R^2	0.45	0.45	0.46	0.56	0.56	0.56

Notes: Among all individuals in reform cantons before the reform, the baseline probability for any given bill to receive at least a payment reminder is 5.81%, and 1.71% to enter into debt collection. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, timing of bill within month, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, adjusted average estimated income in household, all household members are Swiss citizens, no household member is a Swiss citizen. Standard errors (clustered on 621,593/618,194 individuals and 104/91 canton-years) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table C2: Effect of the payment reform on payment problems (predicted subsidy recipients only)

	At least payment reminder			Entered debt collection		
	(1)	(2)	(3)	(4)	(5)	(6)
Post reform	0.29			−0.31**		
	(0.22)			(0.13)		
Reform canton × post	−2.07***	−2.07***	−2.25***	−0.37*	−0.37**	−0.43***
	(0.32)	(0.28)	(0.27)	(0.19)	(0.15)	(0.15)
Individual fixed-effects	✓	✓	✓	✓	✓	✓
Specific month fixed-effects		✓	✓		✓	✓
Ind. & hh level controls			✓			✓
Observations	6.23M	6.23M	6.23M	5.57M	5.57M	5.57M
Adjusted R^2	0.46	0.46	0.46	0.53	0.53	0.53

Notes: Among predicted subsidy recipients in reform cantons before the reform, the baseline probability for any given bill to receive at least a payment reminder is 9.30%, and 2.97% to enter into debt collection. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, timing of bill within month, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, adjusted average estimated income in household, all household members are Swiss citizens, no household member is a Swiss citizen. Standard errors (clustered on 220,456/212,464 individuals and 104/91 canton-years) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table C3: Effect of the payment reform on payment problems (reform cantons only)

	At least payment reminder			Entered debt collection		
	(1)	(2)	(3)	(4)	(5)	(6)
Recipient (predicted)	0.37	0.36	0.45*	0.01	0.01	0.03
	(0.25)	(0.25)	(0.25)	(0.12)	(0.12)	(0.12)
Post reform	0.11			−0.04		
	(0.08)			(0.04)		
Recipient × post	−1.77***	−1.74***	−1.81***	−0.60***	−0.59***	−0.61***
	(0.23)	(0.23)	(0.22)	(0.12)	(0.12)	(0.12)
Individual fixed-effects	✓	✓	✓	✓	✓	✓
Specific month fixed-effects		✓	✓		✓	✓
Ind. & hh level controls			✓			✓
Observations	7.15M	7.15M	7.15M	6.32M	6.32M	6.32M
Adjusted R^2	0.45	0.45	0.45	0.54	0.54	0.54

Notes: Among predicted subsidy recipients in reform cantons before the reform, the baseline probability for any given bill to receive at least a payment reminder is 9.30%, and 2.97% to enter into debt collection. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, timing of bill within month, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, adjusted average estimated income in household, all household members are Swiss citizens, no household member is a Swiss citizen. Standard errors (clustered on 199,169/198,057 individuals and 56/49 canton-years) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table C4: Effect of the payment reform on payment problems (triple differences)

	At least payment reminder			Entered debt collection		
	(1)	(2)	(3)	(4)	(5)	(6)
Recipient (predicted)	−0.77***	−0.78***	−0.77***	−0.19**	−0.19**	−0.20**
	(0.14)	(0.13)	(0.14)	(0.08)	(0.08)	(0.08)
Post reform	0.38***			−0.04		
	(0.07)			(0.04)		
Recipient × reform canton	1.14***	1.15***	1.24***	0.21	0.21	0.24*
	(0.29)	(0.29)	(0.28)	(0.14)	(0.14)	(0.14)
Recipient × post	−0.06	−0.03	−0.02	−0.27***	−0.27***	−0.27***
	(0.17)	(0.16)	(0.17)	(0.10)	(0.10)	(0.10)
Reform canton × post	−0.27***	−0.27***	−0.31***	−0.003	−0.001	−0.01
	(0.10)	(0.07)	(0.08)	(0.05)	(0.04)	(0.04)
Recipient × reform canton × post	−1.71***	−1.71***	−1.81***	−0.33**	−0.32**	−0.36**
	(0.28)	(0.28)	(0.28)	(0.15)	(0.15)	(0.16)
Individual fixed-effects	✓	✓	✓	✓	✓	✓
Specific month fixed-effects		✓	✓		✓	✓
Ind. & hh level controls			✓			✓
Observations	22.0M	22.0M	22.0M	19.5M	19.5M	19.5M
Adjusted R^2	0.45	0.46	0.46	0.56	0.56	0.56

Notes: Among predicted subsidy recipients in reform cantons before the reform, the baseline probability for any given bill to receive at least a payment reminder is 9.30%, and 2.97% to enter into debt collection. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, timing of bill within month, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, adjusted average estimated income in household, all household members are Swiss citizens, no household member is a Swiss citizen. Standard errors (clustered on 621,593/618,194 individuals and 104/91 canton-years) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table C5: Effect of the payment reform on payment problems (predicted non-recipients only)

	At least payment reminder			Entered debt collection		
	(1)	(2)	(3)	(4)	(5)	(6)
Post reform	0.38***			−0.02		
	(0.07)			(0.04)		
Reform canton × post	−0.24**	−0.24***	−0.28***	−0.01	−0.01	−0.02
	(0.10)	(0.07)	(0.07)	(0.05)	(0.04)	(0.04)
Individual fixed-effects	✓	✓	✓	✓	✓	✓
Specific month fixed-effects		✓	✓		✓	✓
Ind. & hh level controls			✓			✓
Observations	15.8M	15.8M	15.8M	13.9M	13.9M	13.9M
Adjusted R^2	0.48	0.48	0.48	0.61	0.61	0.61

Notes: Among non-recipients (based on constant 2014 status) in reform cantons before the reform, the baseline probability for any given bill to receive at least a payment reminder is 4.58%, and 1.26% to enter into debt collection. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, timing of bill within month, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, adjusted average estimated income in household, all household members are Swiss citizens, no household member is a Swiss citizen. Standard errors (clustered on 512,320/503,339 individuals and 104/91 canton-years) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

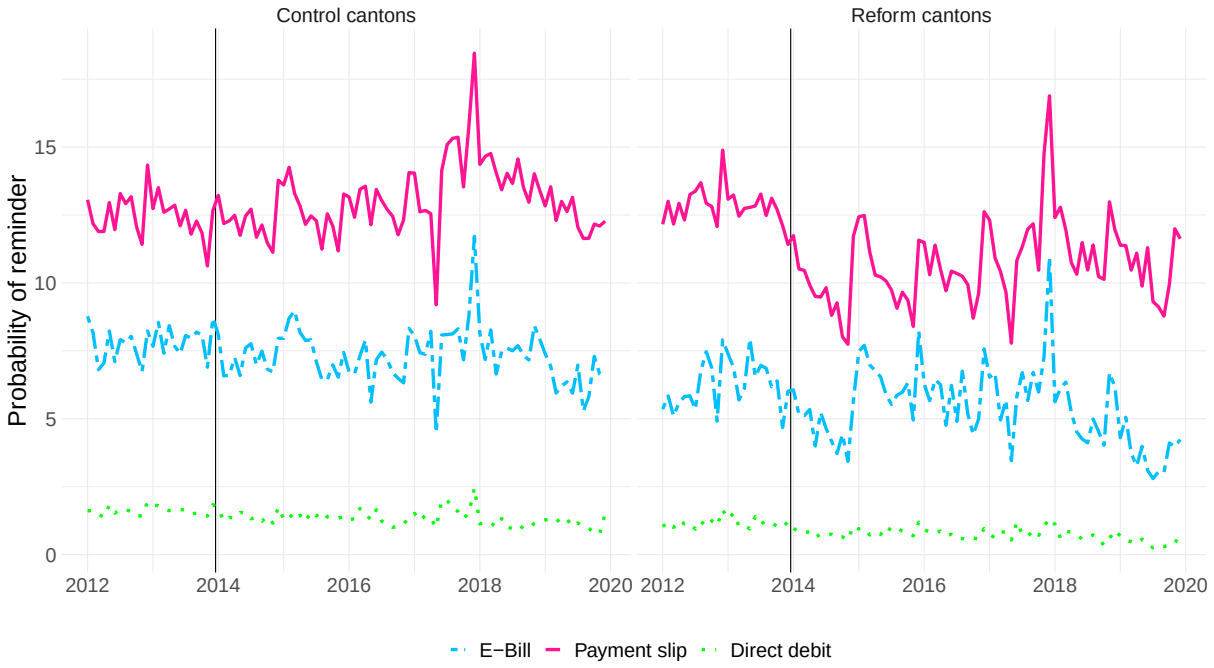


Figure C4: Probability of reminder among predicted subsidy recipients across payment methods

Notes:

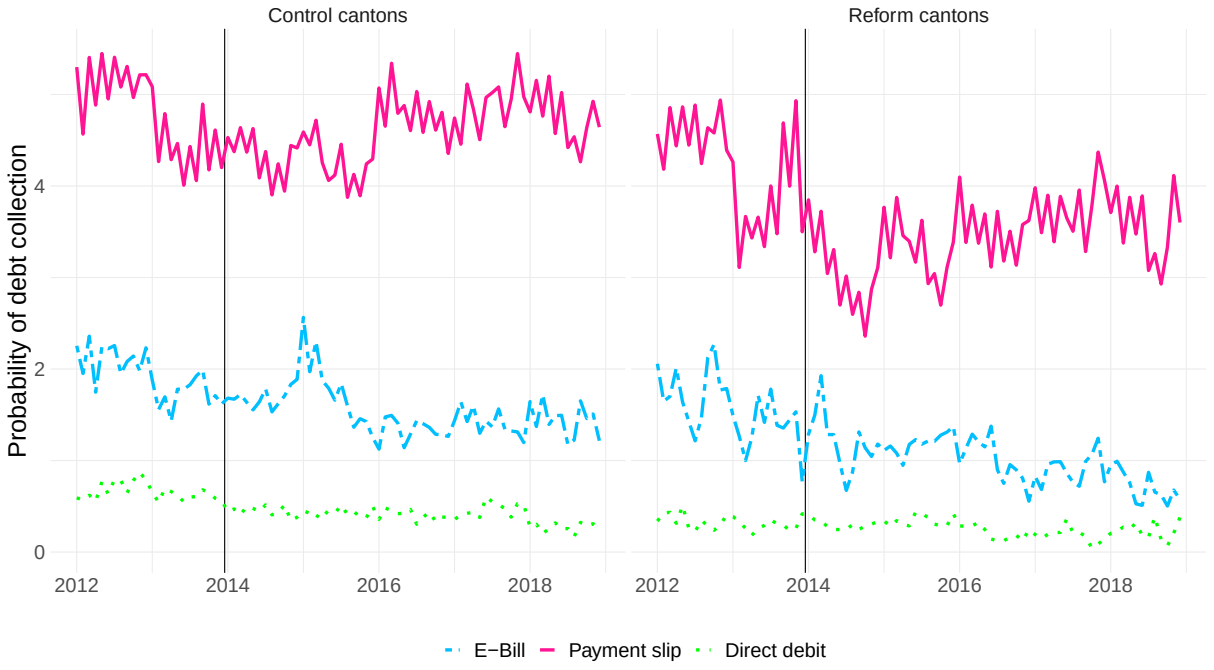


Figure C5: Probability of debt collection among predicted subsidy recipients across payment methods

Notes:

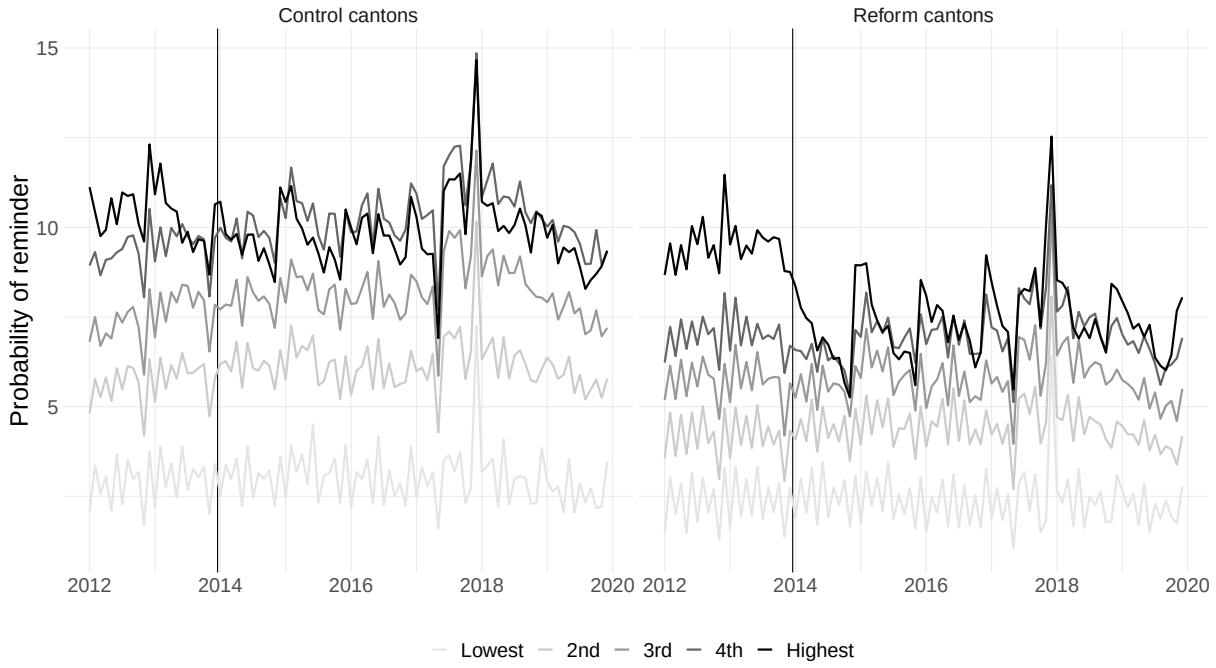


Figure C6: Probability of reminder across predicted subsidy amount as fraction of premium quintiles

Notes:

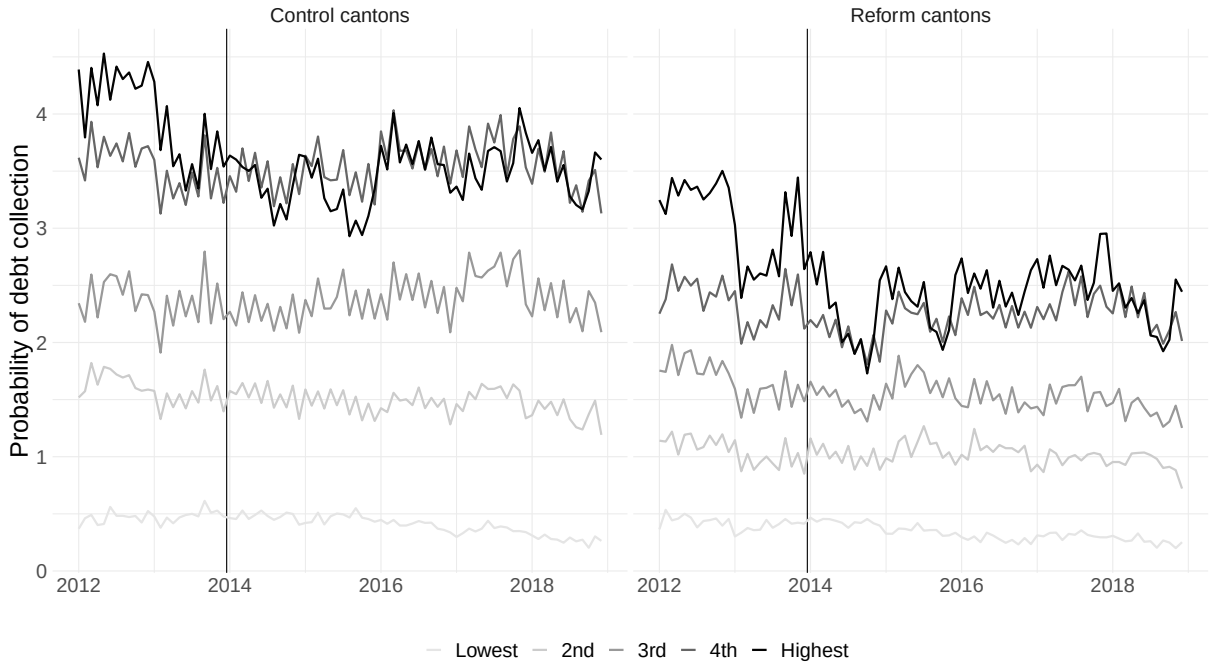


Figure C7: Probability of debt collection across predicted subsidy amount as fraction of premium quintiles

Notes:

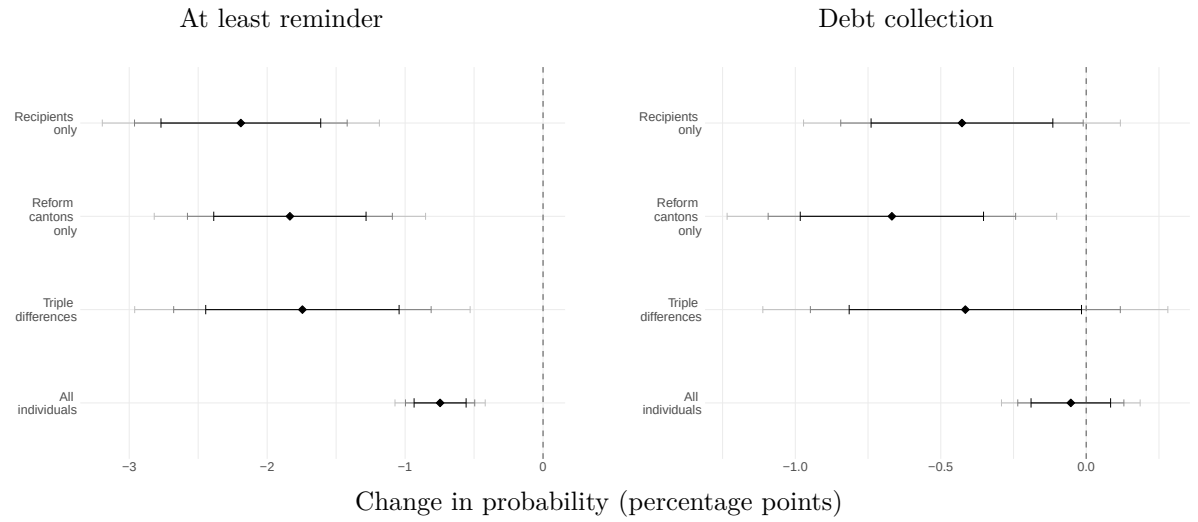


Figure C8: Treatment effect of payment reform on the payment problems of the treated subsidy recipients (cantons without any changes to their subsidy schemes only)

Notes: Diamonds represent the beta coefficient of the relevant difference-in-differences (or triple differences, respectively) interaction terms. Among predicted subsidy recipients in reform cantons before the reform, the baseline probability for any given bill to receive at least a payment reminder is 9.22%, and 3.01% to enter into debt collection. All results include individual and specific year-month fixed-effects. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, timing of bill within month, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, adjusted average estimated income in household, all household members are Swiss citizens, no household member is a Swiss citizen. All standard errors are two-way clustered on the individual and canton-year level. Black, gray and light gray lines correspond to the 95%, 99% and 99.9% confidence intervals.

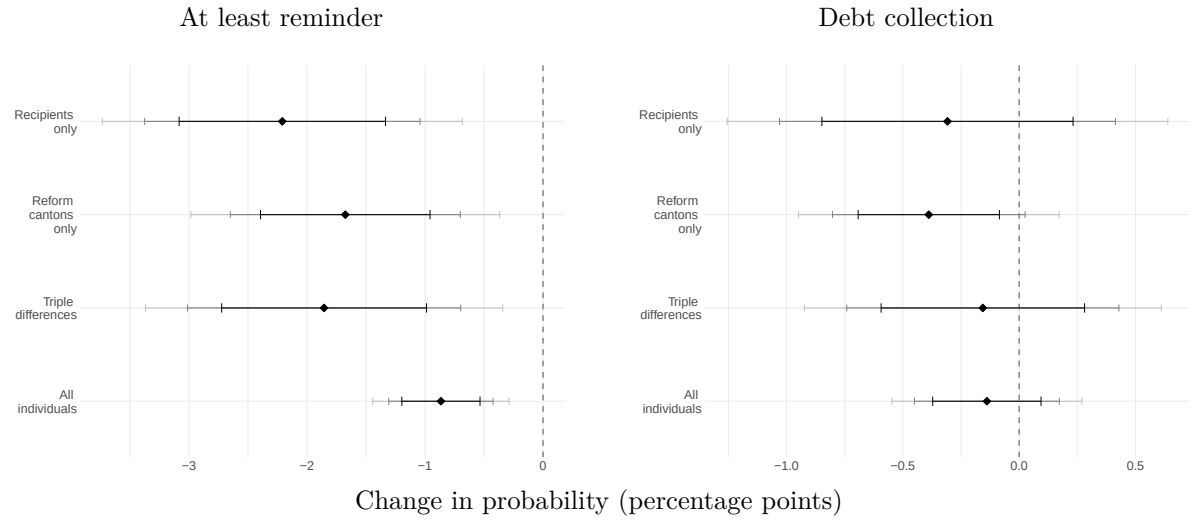


Figure C9: Treatment effect of payment reform on the payment problems of the treated subsidy recipients (cantons with further adjustments to their subsidy schemes only)

Notes: Diamonds represent the beta coefficient of the relevant difference-in-differences (or triple differences, respectively) interaction terms. Among predicted subsidy recipients in reform cantons before the reform, the baseline probability for any given bill to receive at least a payment reminder is 9.57%, and 2.85% to enter into debt collection. All results include individual and specific year-month fixed-effects. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, timing of bill within month, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, adjusted average estimated income in household, all household members are Swiss citizens, no household member is a Swiss citizen. All standard errors are two-way clustered on the individual and canton-year level. Black, gray and light gray lines correspond to the 95%, 99% and 99.9% confidence intervals.

C.2 Medical co-pays

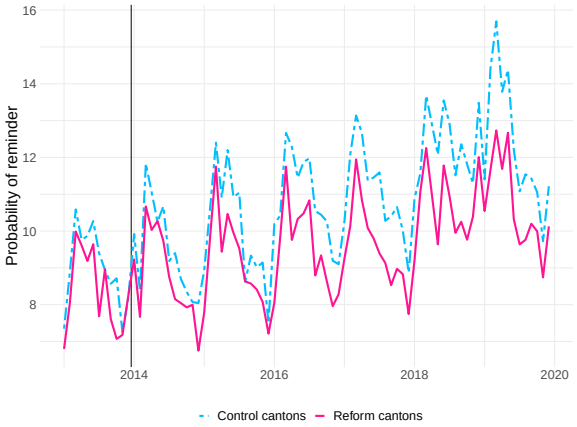


Figure C10: Reminder, recipients only

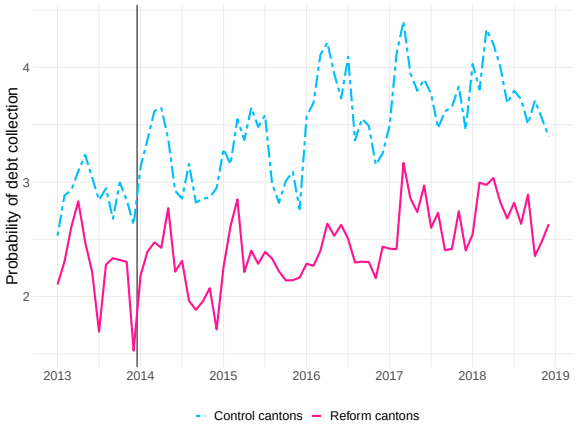


Figure C12: Debt collection, recipients only

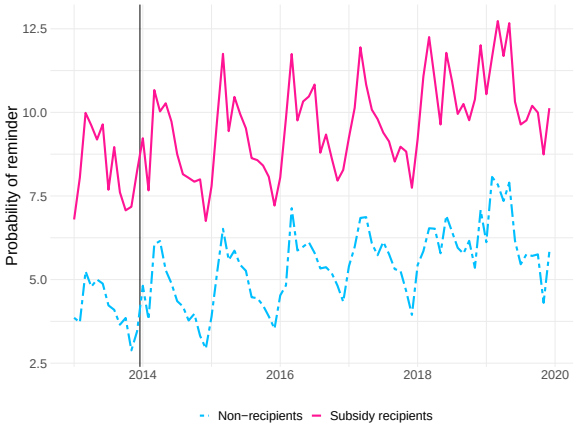


Figure C11: Reminder, reform cantons only

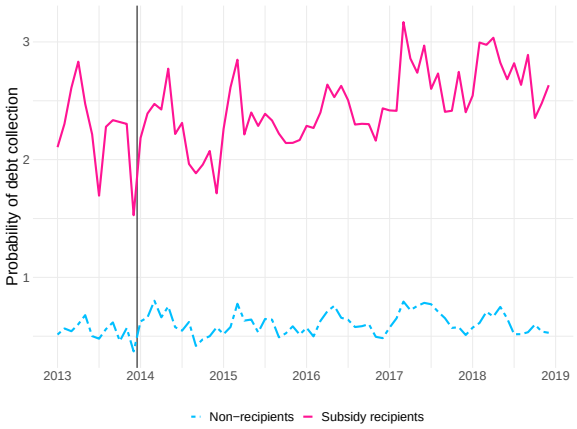


Figure C13: Debt collection, reform cantons only

Figure C14: Switch from cash transfers to in-kind premium subsidies and payment behavior for medical co-pays

Notes: For all individuals, subsidy recipient status is determined based on the classification procedure outlined in Section 4.3.

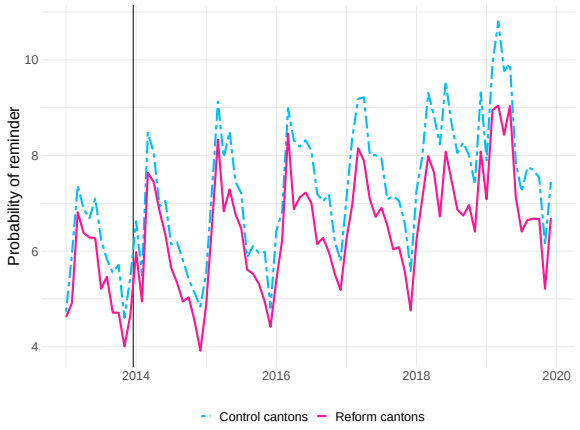


Figure C15: Reminder, all individuals

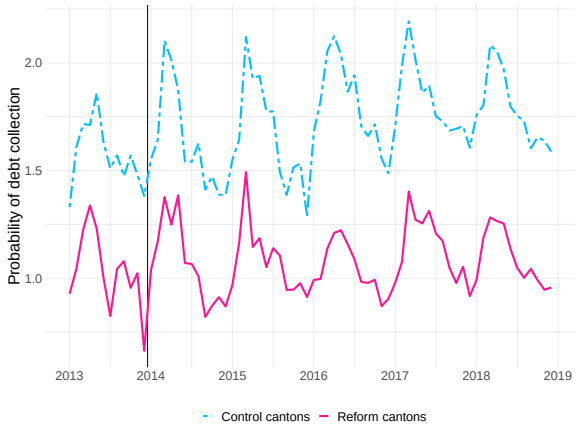


Figure C16: Debt collection, all individuals

Figure C17: Switch from cash transfers to in-kind premium subsides and payment behavior for medical co-pays

Table C6: Effect of the payment reform on payment problems for medical co-pays (all individuals)

	At least payment reminder			Entered debt collection		
	(1)	(2)	(3)	(4)	(5)	(6)
Post reform	1.57***			0.21***		
	(0.15)			(0.05)		
Reform canton × post	−0.31	−0.31**	−0.32**	−0.08	−0.08	−0.08
	(0.20)	(0.14)	(0.15)	(0.07)	(0.07)	(0.07)
Individual fixed-effects	✓	✓	✓	✓	✓	✓
Specific month fixed-effects		✓	✓		✓	✓
Ind. & hh level controls			✓			✓
Observations	5.86M	5.86M	5.86M	4.86M	4.86M	4.86M
Adjusted R^2	0.28	0.28	0.28	0.48	0.48	0.48

Notes: Among all individuals in reform cantons before the reform, the baseline probability for any given medical co-pay bill to receive at least a payment reminder is 5.30%, and 1.03% to enter into debt collection. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, adjusted average estimated income in household, all household members are Swiss citizens, no household member is a Swiss citizen. Standard errors (clustered on 501,012/480,955 individuals and 91/78 canton-years) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table C7: Effect of the payment reform on payment problems for medical co-pays (predicted subsidy recipients only)

	At least payment reminder			Entered debt collection		
	(1)	(2)	(3)	(4)	(5)	(6)
Post reform	1.94***			0.53***		
	(0.22)			(0.11)		
Reform canton × post	−0.47	−0.47*	−0.47*	−0.24	−0.24	−0.23
	(0.32)	(0.27)	(0.27)	(0.18)	(0.18)	(0.18)
Individual fixed-effects	✓	✓	✓	✓	✓	✓
Specific month fixed-effects		✓	✓		✓	✓
Ind. & hh level controls			✓			✓
Observations	1.67M	1.67M	1.67M	1.41M	1.41M	1.41M
Adjusted R^2	0.31	0.31	0.31	0.48	0.48	0.48

Notes: Among predicted subsidy recipients in reform cantons before the reform, the baseline probability for any given medical co-pay bill to receive at least a payment reminder is 8.36%, and 2.27% to enter into debt collection. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, adjusted average estimated income in household, all household members are Swiss citizens, no household member is a Swiss citizen. Standard errors (clustered on 170,446/160,229 individuals and 91/78 canton-years) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table C8: Effect of the payment reform on payment problems for medical co-pays (reform cantons only)

	At least payment reminder			Entered debt collection		
	(1)	(2)	(3)	(4)	(5)	(6)
Recipient (predicted)	−0.43*	−0.59**	−0.36	−0.21	−0.23*	−0.18
	(0.23)	(0.24)	(0.23)	(0.13)	(0.13)	(0.13)
Post reform	1.14***			0.09***		
	(0.11)			(0.03)		
Recipient × post	0.43**	0.48**	0.34*	0.17	0.18	0.16
	(0.19)	(0.20)	(0.19)	(0.13)	(0.13)	(0.13)
Individual fixed-effects	✓	✓	✓	✓	✓	✓
Specific month fixed-effects		✓	✓		✓	✓
Ind. & hh level controls			✓			✓
Observations	1.83M	1.83M	1.83M	1.51M	1.51M	1.51M
Adjusted R^2	0.26	0.26	0.26	0.44	0.44	0.44

Notes: Among predicted subsidy recipients in reform cantons before the reform, the baseline probability for any given medical co-pay bill to receive at least a payment reminder is 8.36%, and 2.27% to enter into debt collection. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, adjusted average estimated income in household, all household members are Swiss citizens, no household member is a Swiss citizen. Standard errors (clustered on 162,109/155,293 individuals and 49/42 canton-years) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table C9: Effect of the payment reform on payment problems for medical co-pays (triple differences)

	At least payment reminder			Entered debt collection		
	(1)	(2)	(3)	(4)	(5)	(6)
Recipient (predicted)	−0.53***	−0.69***	−0.47***	−0.36***	−0.38***	−0.34***
	(0.16)	(0.16)	(0.15)	(0.12)	(0.12)	(0.12)
Post reform	1.39***			0.08**		
	(0.13)			(0.03)		
Recipient × reform canton	0.09	0.08	0.11	0.15	0.15	0.15
	(0.28)	(0.28)	(0.27)	(0.17)	(0.17)	(0.17)
Recipient × post	0.58***	0.64***	0.51***	0.40***	0.41***	0.38***
	(0.15)	(0.15)	(0.14)	(0.10)	(0.10)	(0.10)
Reform canton × post	−0.24	−0.23*	−0.24*	0.01	0.01	0.005
	(0.17)	(0.12)	(0.13)	(0.04)	(0.05)	(0.05)
Recipient × reform canton × post	−0.15	−0.16	−0.18	−0.23	−0.23	−0.22
	(0.25)	(0.25)	(0.24)	(0.16)	(0.16)	(0.16)
Individual fixed-effects	✓	✓	✓	✓	✓	✓
Specific month fixed-effects		✓	✓		✓	✓
Ind. & hh level controls			✓			✓
Observations	5.86M	5.86M	5.86M	4.86M	4.86M	4.86M
Adjusted R^2	0.28	0.28	0.28	0.48	0.48	0.48

Notes: Among predicted subsidy recipients in reform cantons before the reform, the baseline probability for any given medical co-pay bill to receive at least a payment reminder is 8.36%, and 2.27% to enter into debt collection. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, adjusted average estimated income in household, all household members are Swiss citizens, no household member is a Swiss citizen. Standard errors ((clustered on 501,012/480,955 individuals and 91/78 canton-years) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table C10: Effect of the payment reform on payment problems for medical co-pays (predicted non-recipients only)

	At least payment reminder			Entered debt collection		
	(1)	(2)	(3)	(4)	(5)	(6)
Post reform	1.31***			0.06*		
	(0.12)			(0.03)		
Reform canton × post	−0.23	−0.23**	−0.23*	0.001	0.002	−0.001
	(0.16)	(0.11)	(0.12)	(0.04)	(0.04)	(0.04)
Individual fixed-effects	✓	✓	✓	✓	✓	✓
Specific month fixed-effects		✓	✓		✓	✓
Ind. & hh level controls			✓			✓
Observations	4.19M	4.19M	4.19M	3.45M	3.45M	3.45M
Adjusted R^2	0.26	0.27	0.27	0.51	0.51	0.51

Notes: Among predicted subsidy recipients in reform cantons before the reform, the baseline probability for any given medical co-pay bill to receive at least a payment reminder is 4.10%, and 0.54% to enter into debt collection. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, adjusted average estimated income in household, all household members are Swiss citizens, no household member is a Swiss citizen. Standard errors (clustered on 390,990/369,686 individuals and 91/78 canton-years) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

D Prediction of subsidy recipient status

D.1 Machine learning procedure

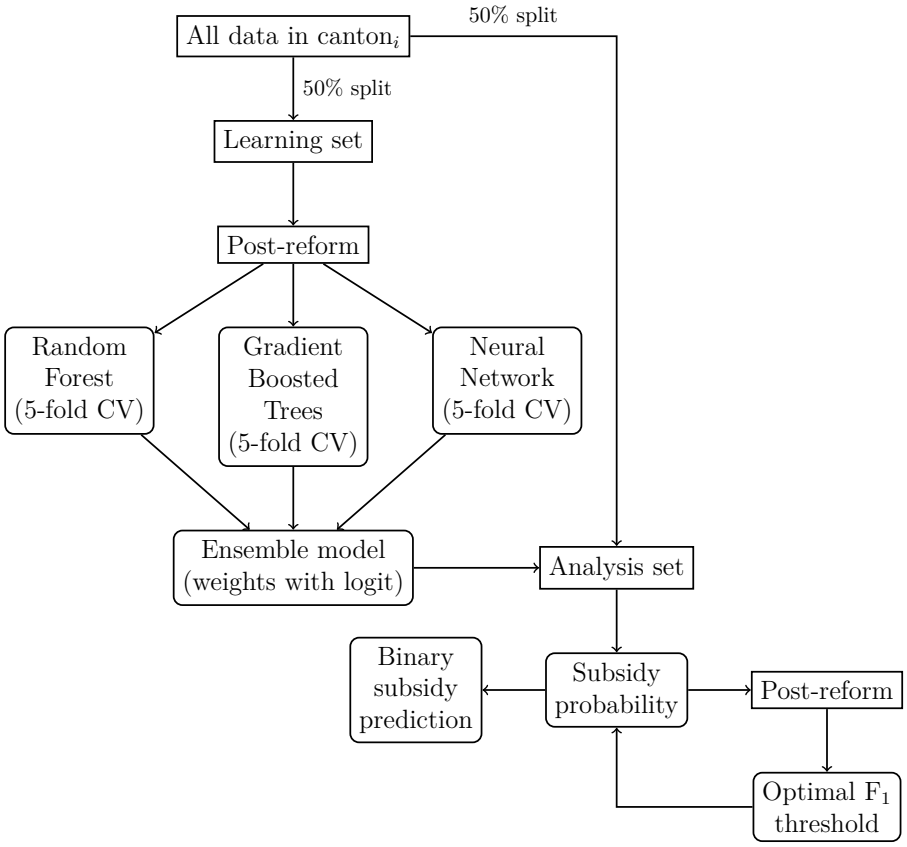
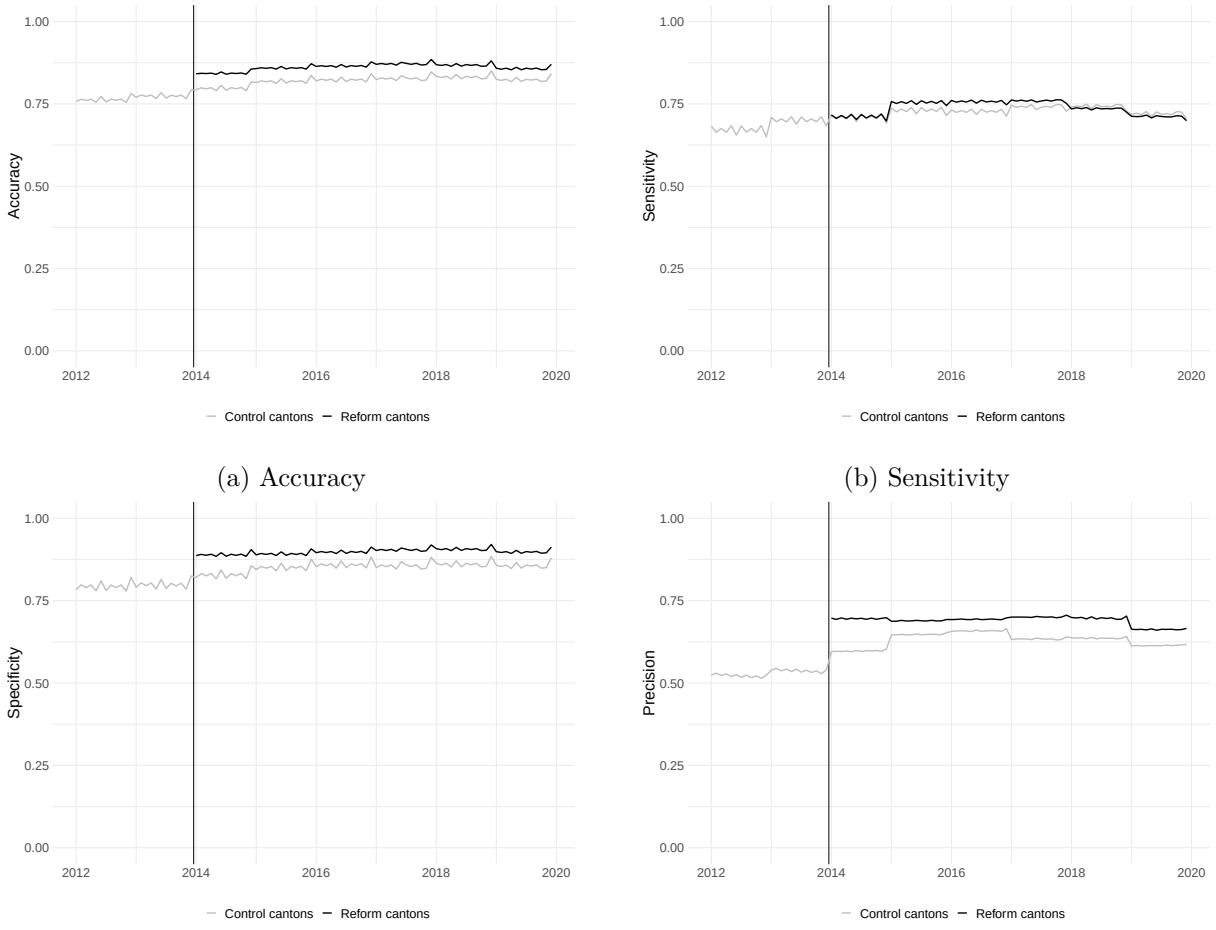


Figure D1: Process for classifying the premium subsidy status



(a) Accuracy

(b) Sensitivity

(c) Specificity

(d) Precision

Figure D2: Subsidy recipient status: Classification performance of machine learning approach

Notes: All measures refer to the out-of-sample classification performance among observations in the "analysis set" (which were never used for training the classification algorithms). Performance in reform cantons prior to the reform in 2014 cannot be evaluated, as we know the actual recipient status of any individuals living in these cantons only after the switch to in-kind provision. As explained in Sections 4.3-4.5 in the main text, the classification procedure is exactly the same for both groups of cantons. It is therefore unlikely that the relative classification performance between reform and control cantons is substantially different before the reform than after the reform.

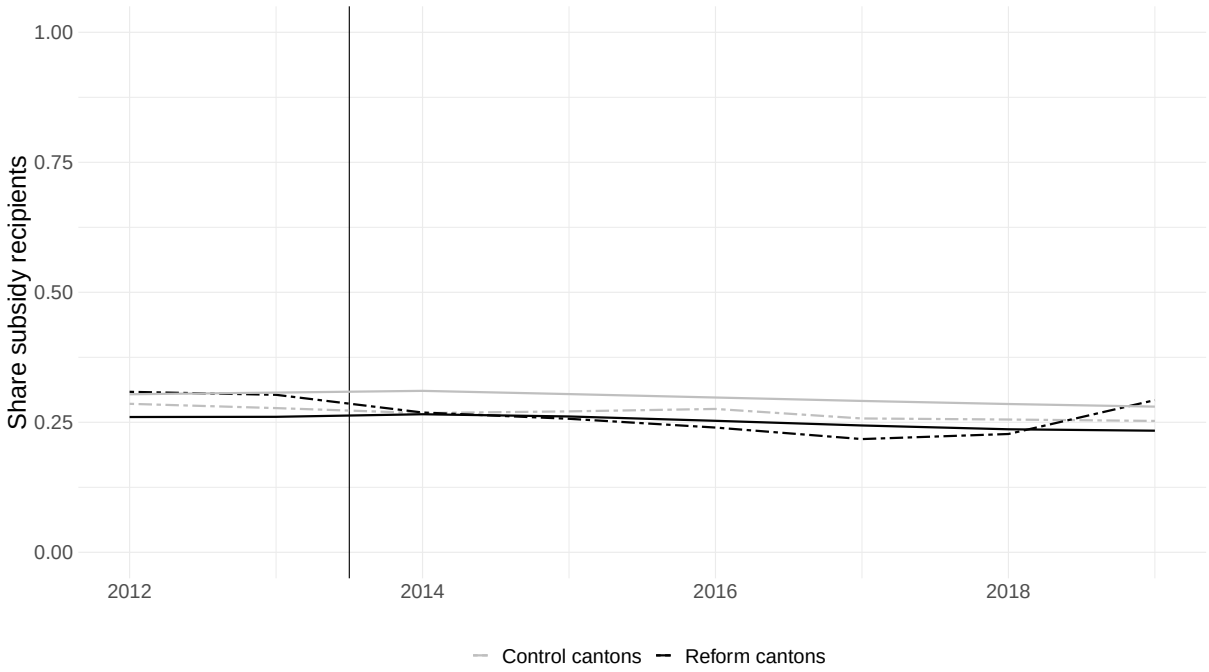
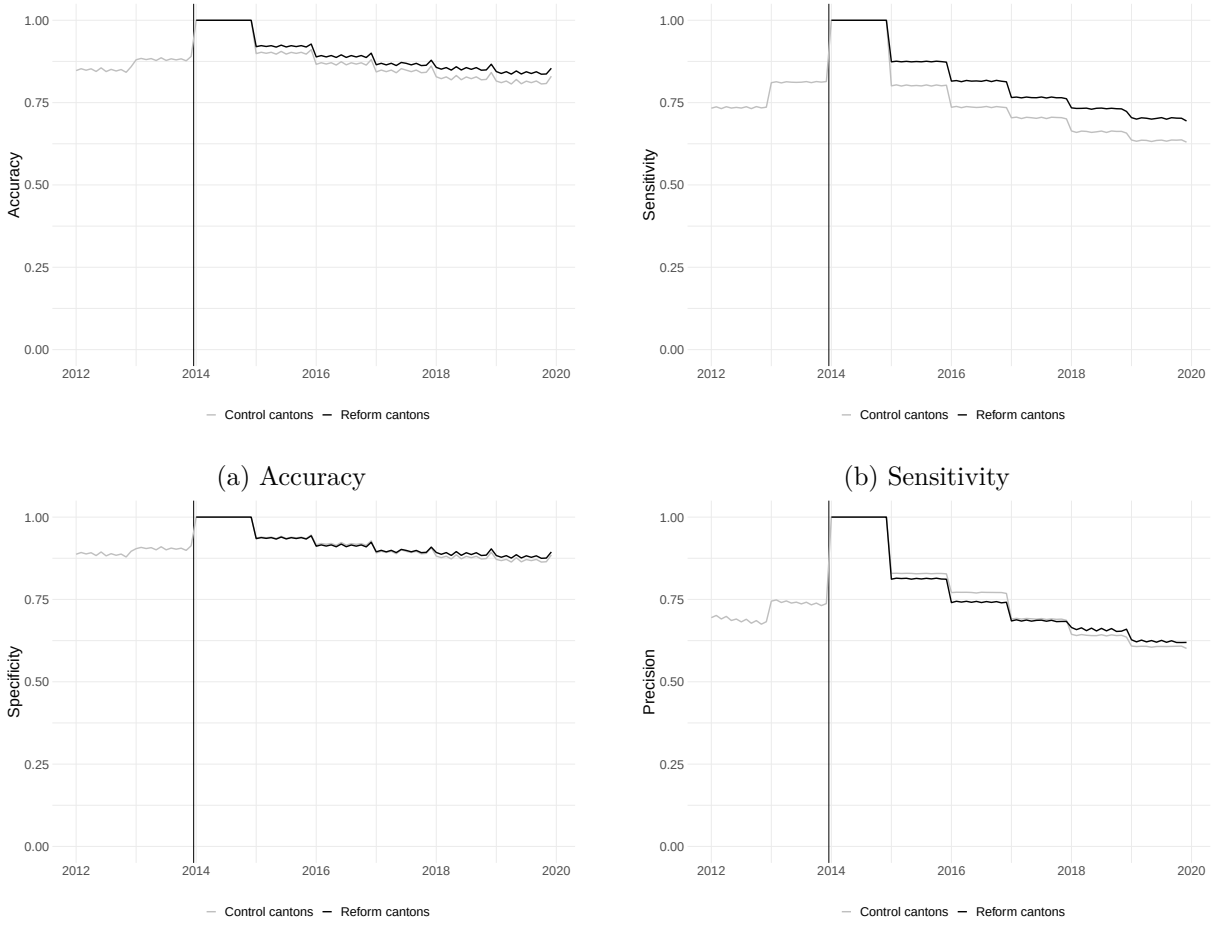


Figure D3: Comparison between real and predicted share of subsidy recipients (with machine learning approach) across control and reform cantons

Notes: Solid lines depict the share of recipients among all insured individuals in the health insurance company we use data of. Dashed lines depict the share of recipients among all insured individuals according to the official statistics of the Federal Office of Public Health (FOPH).

D.2 Alternative approaches for the classification of the subsidy recipient status

D.2.1 Recipient status in 2014 held constant in every year



(a) Accuracy

(b) Sensitivity

(c) Specificity

(d) Precision

Figure D4: Subsidy recipient status: Classification performance when holding recipient status in 2014 constant

Notes: All measures refer to the classification performance among observations in the "analysis set". Performance in reform cantons prior to the reform in 2014 cannot be evaluated, as we know the actual recipient status of any individuals living in these cantons only after the switch to in-kind provision. As explained in Sections 4.3-4.5 in the main text, the classification procedure is exactly the same for both groups of cantons. However, holding the recipient status in 2014 constant for all four years results in perfect classification in 2014 for all cantons. It is therefore likely that the relative classification performance between reform and control cantons is substantially different before the reform than after the reform.

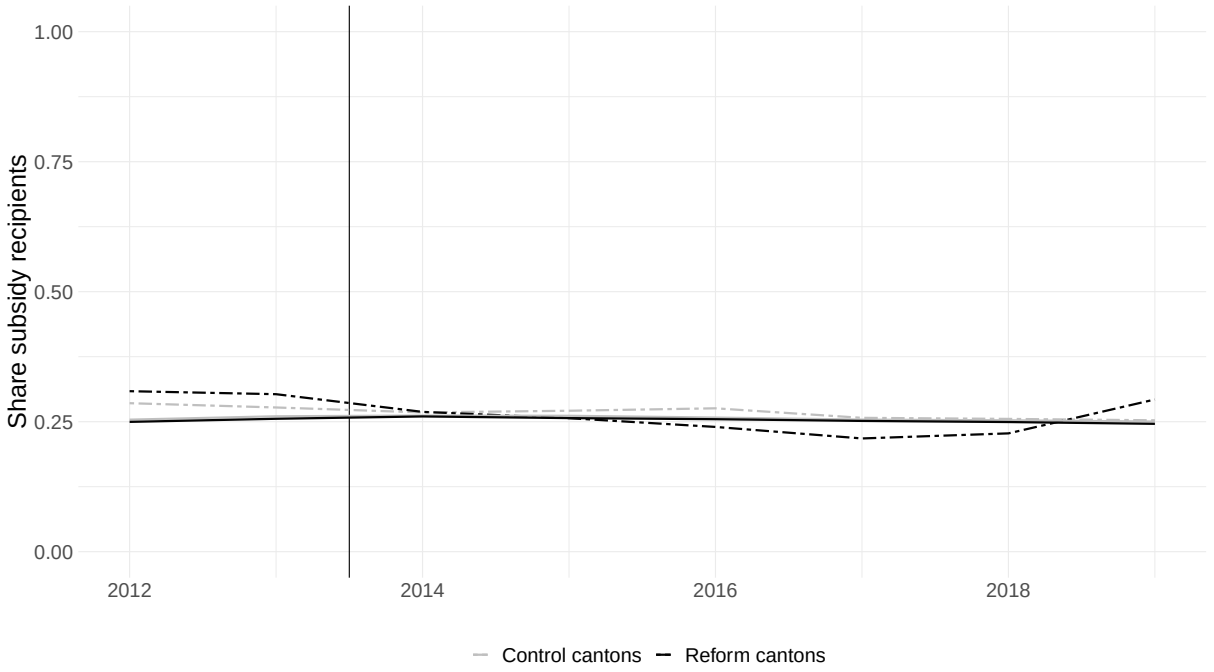


Figure D5: Comparison between real and share of subsidy recipients when holding recipient status in 2014 constant across control and reform cantons

Table D1: Effect of the payment reform on payment problems (subsidy recipients with 2014 status held constant only)

	At least payment reminder			Entered debt collection		
	(1)	(2)	(3)	(4)	(5)	(6)
Post reform	0.25			−0.24**		
	(0.23)			(0.12)		
Reform canton × post	−2.27***	−2.28***	−2.39***	−0.35*	−0.35***	−0.38***
	(0.34)	(0.24)	(0.24)	(0.19)	(0.12)	(0.12)
Individual fixed-effects	✓	✓	✓	✓	✓	✓
Specific month fixed-effects		✓	✓		✓	✓
Ind. & hh level controls			✓			✓
Observations	5.62M	5.62M	5.62M	5.00M	5.00M	5.00M
Adjusted R^2	0.40	0.40	0.41	0.44	0.44	0.44

Notes: Among predicted subsidy recipients in reform cantons before the reform, the baseline probability for any given bill to receive at least a payment reminder is 8.42%, and 2.39% to enter into debt collection. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, timing of bill within month, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, adjusted average estimated income in household, all household members are Swiss citizens, no household member is a Swiss citizen. Standard errors (clustered on 147,737/147,019 individuals and 104/91 canton-years) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table D2: Effect of the payment reform on payment problems (reform cantons with 2014 recipient status held constant only)

	At least payment reminder			Entered debt collection		
	(1)	(2)	(3)	(4)	(5)	(6)
Post reform	0.24***			−0.06*		
	(0.08)			(0.03)		
Recipient × post	−2.26***	−2.25***	−2.30***	−0.53***	−0.53***	−0.55***
	(0.26)	(0.26)	(0.26)	(0.14)	(0.14)	(0.14)
Individual fixed-effects	✓	✓	✓	✓	✓	✓
Specific month fixed-effects		✓	✓		✓	✓
Ind. & hh level controls			✓			✓
Observations	7.15M	7.15M	7.15M	6.32M	6.32M	6.32M
Adjusted R^2	0.45	0.45	0.45	0.54	0.54	0.54

Notes: Among subsidy recipients in treated cantons before the reform, the probability for any given bill to receive at least a payment reminder is 8.42%, and 2.39% to enter into debt collection. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, timing of bill within month, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, adjusted average estimated income in household, all household members are Swiss citizens, no household member is a Swiss citizen. Standard errors (clustered on 199,169/198,057 individuals and 56/49 canton-years) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table D3: Effect of payment reform on payment problems (triple differences with 2014 recipient status held constant)

	At least payment reminder			Entered debt collection		
	(1)	(2)	(3)	(4)	(5)	(6)
Post reform	0.41***			−0.08		
	(0.09)			(0.05)		
Recipient × post	−0.17	−0.15	−0.17	−0.16	−0.16	−0.17
	(0.23)	(0.23)	(0.24)	(0.10)	(0.10)	(0.10)
Reform canton × post	−0.17	−0.18*	−0.23**	0.02	0.02	0.01
	(0.11)	(0.10)	(0.10)	(0.06)	(0.06)	(0.06)
Recipient × reform canton × post	−2.10***	−2.10***	−2.16***	−0.37**	−0.37**	−0.39**
	(0.35)	(0.35)	(0.35)	(0.17)	(0.17)	(0.17)
Individual fixed-effects	✓	✓	✓	✓	✓	✓
Specific month fixed-effects		✓	✓		✓	✓
Ind. & hh level controls			✓			✓
Observations	22.0M	22.0M	22.0M	19.5M	19.5M	19.5M
Adjusted R^2	0.45	0.46	0.46	0.56	0.56	0.56

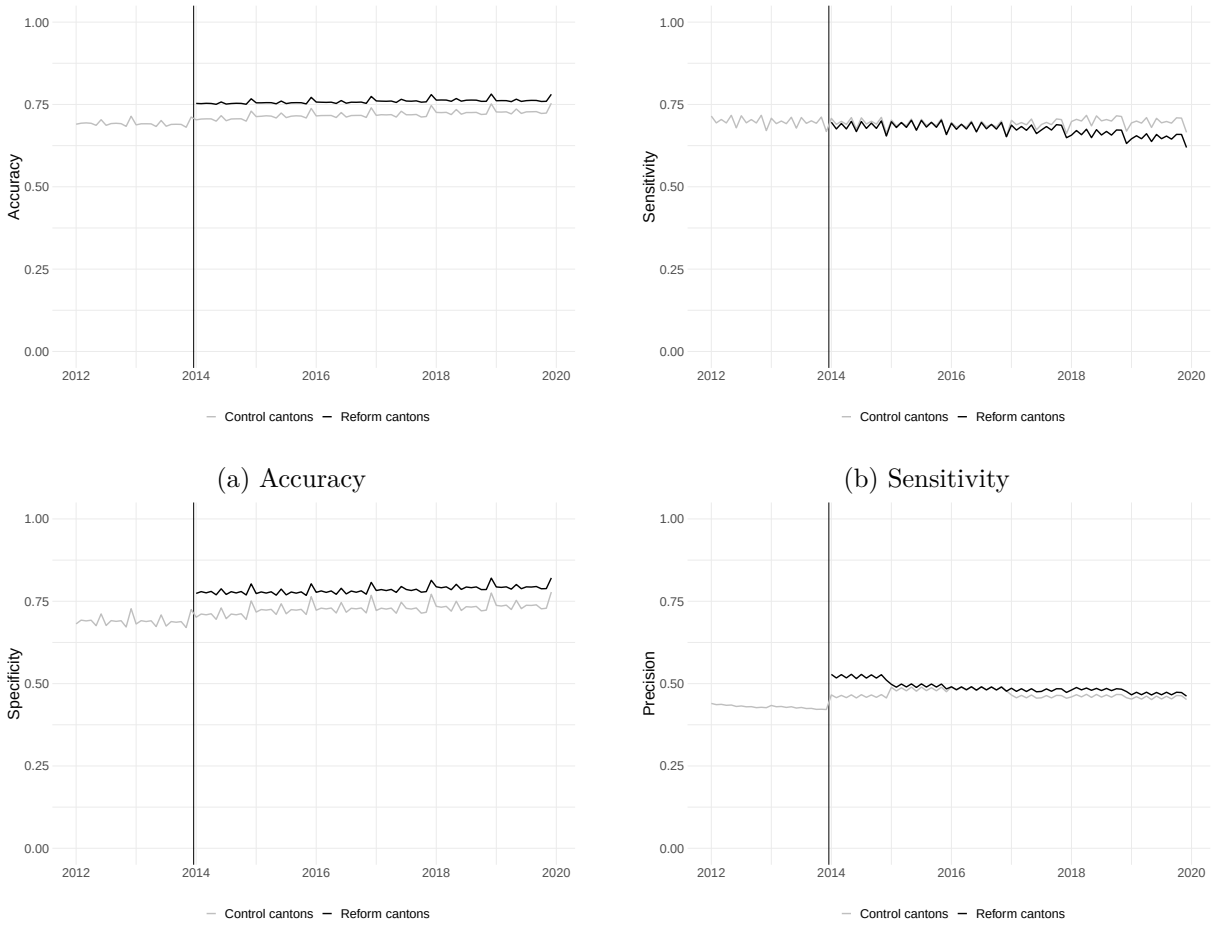
Notes: Among subsidy recipients in treated cantons before the reform, the probability for any given bill to receive at least a payment reminder is 8.42%, and 2.39% to enter into debt collection. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, timing of bill within month, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, adjusted average estimated income in household, all household members are Swiss citizens, no household member is a Swiss citizen. Standard errors clustered on 621,593/618,194 individuals and 104/91 canton-years) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table D4: Effect of the payment reform on payment problems (non-recipients with 2014 status held constant only)

	At least payment reminder			Entered debt collection		
	(1)	(2)	(3)	(4)	(5)	(6)
Post reform	0.41***			−0.08		
	(0.09)			(0.05)		
Reform canton × post	−0.17	−0.17**	−0.23**	0.02	0.02	0.01
	(0.11)	(0.09)	(0.09)	(0.06)	(0.05)	(0.05)
Individual fixed-effects	✓	✓	✓	✓	✓	✓
Specific month fixed-effects		✓	✓		✓	✓
Ind. & hh level controls			✓			✓
Observations	16.38M	16.38M	16.38M	14.49M	14.49M	14.49M
Adjusted R^2	0.48	0.48	0.48	0.60	0.60	0.60

Notes: Among non-recipients (based on constant 2014 status) in reform cantons before the reform, the baseline probability for any given bill to receive at least a payment reminder is 4.93%, and 1.48% to enter into debt collection. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, timing of bill within month, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, adjusted average estimated income in household, all household members are Swiss citizens, no household member is a Swiss citizen. Standard errors (clustered on 473,856/471,175 individuals and 104/91 canton-years) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

D.2.2 Logit model



(a) Accuracy

(b) Sensitivity

(c) Specificity

(d) Precision

Figure D6: Subsidy recipient status: Classification performance of logit model

Notes: All measures refer to the out-of-sample classification performance among observations in the "analysis set" (which were never used for the logit estimations). Performance in reform cantons prior to the reform in 2014 cannot be evaluated, as we know the actual recipient status of any individuals living in these cantons only after the switch to in-kind provision. As explained in Sections 4.3-4.5 in the main text, the classification procedure is exactly the same for both groups of cantons. It is therefore unlikely that the relative classification performance between reform and control cantons is substantially different before the reform than after the reform.

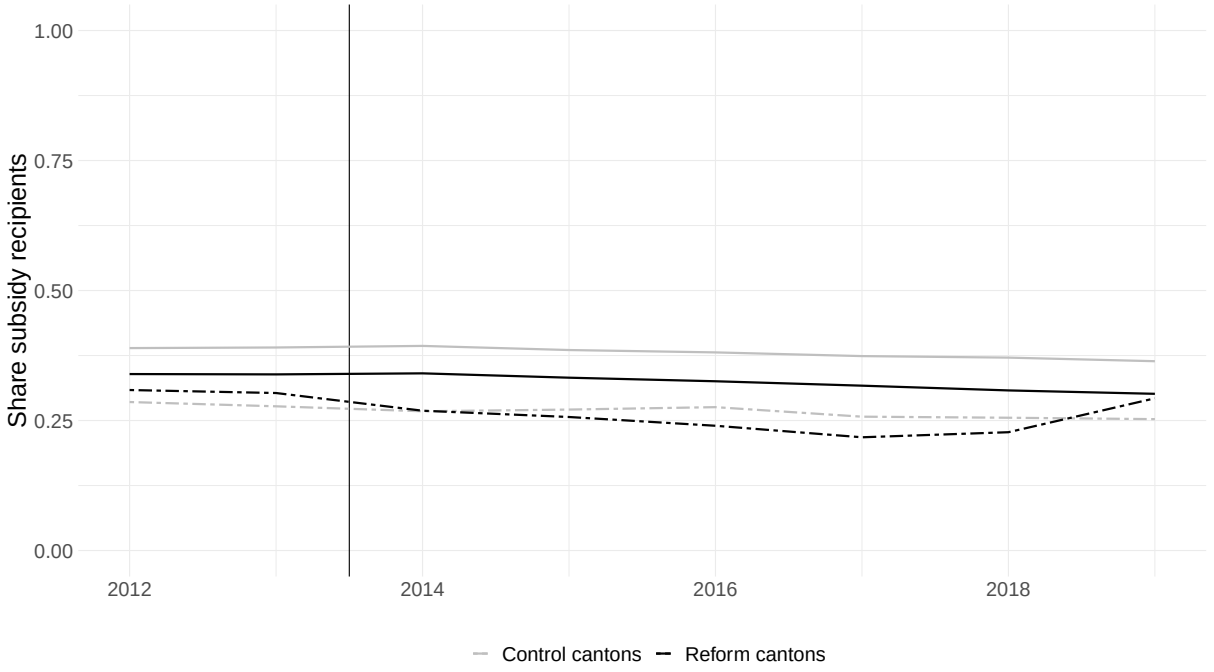


Figure D7: Comparison between real and predicted share of subsidy recipients (using a logit model) across control and reform cantons

Table D5: Effect of the payment reform on payment problems (predicted subsidy recipients using logit model only)

	At least payment reminder			Entered debt collection		
	(1)	(2)	(3)	(4)	(5)	(6)
Post reform	0.52***			−0.23**		
	(0.18)			(0.11)		
Reform canton × post	−1.41***	−1.41***	−1.61***	−0.13	−0.13	−0.19
	(0.25)	(0.22)	(0.21)	(0.15)	(0.12)	(0.12)
Individual fixed-effects	✓	✓	✓	✓	✓	✓
Specific month fixed-effects		✓	✓		✓	✓
Ind. & hh level controls			✓			✓
Observations	8.0M	8.0M	8.0M	7.1M	7.1M	7.1M
Adjusted R^2	0.48	0.48	0.48	0.57	0.57	0.57

Notes: Among predicted subsidy recipients in reform cantons before the reform, the baseline probability for any given bill to receive at least a payment reminder is 8.69%, and 2.81% to enter into debt collection. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, timing of bill within month, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, adjusted average estimated income in household, all household members are Swiss citizens, no household member is a Swiss citizen. Standard errors (clustered on 250,081/243,213 individuals and 104/91 canton-years) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table D6: Effect of the payment reform on payment problems (reform cantons with subsidy status prediction using logit model only)

	At least payment reminder			Entered debt collection		
	(1)	(2)	(3)	(4)	(5)	(6)
Recipient (predicted)	−0.34*	−0.33*	−0.21	−0.18*	−0.18*	−0.18*
	(0.18)	(0.18)	(0.16)	(0.09)	(0.09)	(0.10)
Post reform	−0.07			−0.10**		
	(0.07)			(0.04)		
Recipient × post	−0.84***	−0.82***	−0.91***	−0.29***	−0.28***	−0.31***
	(0.17)	(0.17)	(0.16)	(0.08)	(0.09)	(0.09)
Individual fixed-effects	✓	✓	✓	✓	✓	✓
Specific month fixed-effects		✓	✓		✓	✓
Ind. & hh level controls			✓			✓
Observations	7.15M	7.15M	7.15M	6.32M	6.32M	6.32M
Adjusted R^2	0.45	0.45	0.45	0.54	0.54	0.54

Notes: Among predicted subsidy recipients in reform cantons before the reform, the baseline probability for any given bill to receive at least a payment reminder is 8.69%, and 2.81% to enter into debt collection. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, timing of bill within month, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, adjusted average estimated income in household, all household members are Swiss citizens, no household member is a Swiss citizen. Standard errors (clustered on 199,169/198,057 individuals and 56/49 canton-years) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table D7: Effect of the payment reform on payment problems (triple differences with subsidy status prediction using logit model)

	At least payment reminder			Entered debt collection		
	(1)	(2)	(3)	(4)	(5)	(6)
Recipient (predicted)	−0.66***	−0.66***	−0.58***	0.004	0.003	0.001
	(0.13)	(0.13)	(0.13)	(0.07)	(0.07)	(0.07)
Post reform	0.28***			−0.05		
	(0.07)			(0.03)		
Recipient × reform canton	0.33	0.33	0.44**	−0.18	−0.18	−0.14
	(0.22)	(0.22)	(0.21)	(0.11)	(0.12)	(0.11)
Recipient × post	0.21	0.24	0.24	−0.19**	−0.19**	−0.20**
	(0.16)	(0.16)	(0.17)	(0.09)	(0.09)	(0.09)
Reform canton × post	−0.35***	−0.35***	−0.37***	−0.06	−0.05	−0.06
	(0.10)	(0.07)	(0.08)	(0.05)	(0.04)	(0.04)
Recipient × reform canton × post	−1.06***	−1.06***	−1.19***	−0.10	−0.09	−0.13
	(0.23)	(0.23)	(0.23)	(0.12)	(0.13)	(0.13)
Individual fixed-effects	✓	✓	✓	✓	✓	✓
Specific month fixed-effects		✓	✓		✓	✓
Ind. & hh level controls			✓			✓
Observations	22.0M	22.0M	22.0M	19.5M	19.5M	19.5M
Adjusted R^2	0.45	0.46	0.46	0.56	0.56	0.56

Notes: Among predicted subsidy recipients in reform cantons before the reform, the baseline probability for any given bill to receive at least a payment reminder is 8.69%, and 2.81% to enter into debt collection. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, timing of bill within month, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, adjusted average estimated income in household, all household members are Swiss citizens, no household member is a Swiss citizen. Standard errors (clustered on 621,593/618,194 individuals and 104/91 canton-years) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table D8: Effect of the payment reform on payment problems (predicted non-recipients using logit model only)

	At least payment reminder			Entered debt collection		
	(1)	(2)	(3)	(4)	(5)	(6)
Post reform	0.30***			−0.03		
	(0.06)			(0.03)		
Reform canton × post	−0.32***	−0.32***	−0.35***	−0.04	−0.04	−0.05
	(0.09)	(0.06)	(0.06)	(0.04)	(0.04)	(0.04)
Individual fixed-effects	✓	✓	✓	✓	✓	✓
Specific month fixed-effects		✓	✓		✓	✓
Ind. & hh level controls			✓			✓
Observations	14.0M	14.0M	14.0M	12.4M	12.4M	12.4M
Adjusted R^2	0.44	0.44	0.45	0.57	0.57	0.57

Notes: Among non-recipients (based on constant 2014 status) in reform cantons before the reform, the baseline probability for any given bill to receive at least a payment reminder is 4.33%, and 1.14% to enter into debt collection. Individual and household level controls include accident insurance, deductible, payment rhythm, means of payment, timing of bill within month, supplemental insurance premiums, estimated income, estimated number of neighbors, household size, number of children in household, adjusted average estimated income in household, all household members are Swiss citizens, no household member is a Swiss citizen. Standard errors (clustered on 460,684/451,653 individuals and 104/91 canton-years) in parentheses. * p<0.1, ** p<0.05, *** p<0.01.