The Effect of Health Insurance Subsidies on Mortality for Low Income Individuals

May 29, 2019

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Abstract

In regulated competitive health insurance markets, means-tested subsidies make health insurance affordable for low-income individuals. We examine the effect of changes in health insurance subsidies on mortality using administrative data on over 150,000 Swiss residents between 2002 and 2011. Subsidies are not significantly related to mortality in OLS regressions, but when we instrument for the subsidy using changes in the subsidy rule, we find that a permanent 100 Swiss franc per month decrease in the subsidy increases annual mortality by 0.16 percentage points per eleven months and this effect persists for up to 2 years. The rise in mortality in response to a reduction in subsidy appears to be due to: i) a reduction in disposable income; and ii) enrollment in higher deductible insurance policies by poorer individuals. Both mechanisms may reduce financial security and consumption of health care. Trends in aggregate mortality suggest subsidy reductions are associated with increases in mortality for causes more sensitive to the financial stress from sudden loss of income.

Keywords: Mortality, Health Insurance, Subsidies

JEL Codes: H31, I12, I13, I14

Declarations of interest: none.

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I. Introduction

Governments worldwide have prioritized provision of universal health insurance coverage for their citizens (WHO, 2010), with many countries adopting some form of managed competition in which consumers choose among competing private insurers (e.g. the United States, the Netherlands, and Switzerland). In order to make health insurance coverage affordable and ensure the sustainability of the insurance market by increasing enrollment, most managed competition systems include a government subsidy to reduce the cost of health insurance coverage and guarantee affordable and equitable access to health insurance (Einav et al., 2019). Since subsidies are expensive,[1] evaluation of the cost-effectiveness of the policy is necessary in terms of its impact on health insurance costs, quality of coverage (Frean et al., 2017) and above all, whether these subsidies are effective at improving population health (Pauly, 2010).

In this paper, we provide evidence on the effects of health insurance subsidies on mortality using a unique administrative dataset from the Swiss canton of Vaud. We select a sample of more than 180,000 residents for the period from 2002 to 2011. Our data cover the entire subsidized population of the canton and includes individuals in two different subsidy programs. We have information on subsidy levels, income/wealth, family demographic structure, and insurance choices. For individuals that are enrolled in the subsidy program in January, data are collected until the end of the year irrespective of whether they continue to receive the subsidy or not; therefore we can assess at least eleven month mortality risk (from January to December).

[1] In the United States, subsidies paid through the federal health insurance marketplaces to people under age 65 totaled 68.5 billion USD in 2018, according to the Congressional Budget Office (https://www.cbo.gov/system/files/2018-06/53826-healthinsurancecoverage.pdf). The comparable figure for Switzerland is 8.1 billion CHF in 2016 (https://www.bfs.admin.ch/bfs/fr/home/statistiques/sante/cout-financement.assetdetail.6386450.html), and for Canton Vaud, which we study, 0.5 billion CHF in 2018.

The majority of our analysis focuses on the “partial” subsidy program, in which the canton provides a means-tested subsidy towards the cost of health insurance, with the subsidy level varying over time, across income levels and household compositions, according to an allocation formula. Using annual variations in this subsidy rule, we construct instruments for an individual’s subsidy level as a function of these exogenous changes. In our preferred instrumental variables specification, we find that a 100 CHF (Swiss franc) reduction in an individual’s monthly subsidy (63% of the actual average subsidy) increases mortality over the subsequent eleven months by, on average, 0.16 (IV-Probit) to 0.2 (IV-LPM) percentage points (22-27%).

We also compare individuals in the partial subsidy program to those who receive a “complete” subsidy, which covers the entire cost of health insurance, in a difference-in-differences framework. In this analysis, we find that during a period of lower partial subsidy levels, mortality in the partial subsidy group rose by 0.23 percentage points, relative to the complete subsidy group, while the monthly subsidy relative to the completely covered population fell by 76.5 CHF, which implies that a 100 CHF reduction in the monthly subsidy is associated with a 0.3 percentage point increase in mortality. To put these figures into perspective, for the roughly 80,000 people in the partial subsidy group in 2006, our results imply that a 100 CHF reduction in the subsidy would have resulted in an additional 170 to 240 deaths.

We next turn to understanding how the subsidy affects mortality. Our data do not allow us to test whether the subsidy affects health care utilization but do allow us to test how the subsidy affects the choice of coverage. Subsidies are paid directly to the insurer and reduce the out of pocket premium paid by the individual. Irrespective of receiving the subsidy or not, one can reduce one’s out of pocket premium by choosing a higher deductible level (the amount an individual must pay before the insurance provider will pay expenses), restricting provider choice, or shopping around for cheaper insurers.

To study how subsidies affect mortality, we focus on two mechanisms that operate through the design of the insurance contract: 1) by changing the choice of health insurance coverage, the cost of accessing healthcare is affected (i.e. lower subsidies may induce individuals to choose higher deductibles or more restrictive provider networks); and 2) lower subsidies increase paid premiums and higher deductibles expose individuals to greater financial risk when ill, affecting disposable income and financial security. Both mechanisms may affect utilization of healthcare, while lower disposable income and reduced financial security may have direct effects on health. In a recent work, Finkelstein et al. (2019) find that amounts enrollees were willing to pay for health insurance is much lower than the expected cost, suggesting premiums have a large effect on individuals' decisions regarding insurance.

We demonstrate that a reduction in the subsidy increases deductibles for individuals who had purchased higher deductible plans the previous year, while individuals who previously chose the lowest deductible plans do not increase deductibles. This enables a comparative test of the effects on mortality rate of changes in the generosity of coverage and in disposable income. The change in mortality rates associated with a subsidy reduction is larger for lower income individuals who purchased higher deductible plans in the previous year and who increased their deductible levels in response to a subsidy reduction, relative to individuals who purchased the lowest deductible plan in the previous year. This result demonstrates that the generosity of health insurance coverage (lower deductibles) affects mortality for poorer people. Such individuals experienced an increase in disposable income (lower net premiums) when choosing a higher deductible, yet mortality increased anyway, possibly representing the effect of reduced health care utilization or lower financial protection against the costs of illness.

For the effects of disposable income, we find that a reduction in subsidy also increases mortality for individuals with the lowest deductible in the previous period who do not increase their deductibles. We interpret this finding as evidence of an effect of income on mortality, since reductions in the subsidy are associated with a corresponding increase in their net insurance premium, and therefore their decreased disposable income. Consistent with an income-driven effect of the subsidy on mortality, we find that the mortality rates for income-elastic causes of death increase in Canton Vaud, relative to two neighboring French regions, during periods with large subsidy cuts.

Our study concerns the effect of changes in subsidies on mortality rates, as mediated by insurance-related choices, and contributes to the literature on the health effects of insurance. While "the effect of health insurance on health…may seem intuitive" (Finkelstein et al., 2012) there is significant disagreement about the role of health insurance in improving health (see Goldman and Lakdawalla, 2010; Levy and Meltzer, 2008 and references therein, 2004). As for the specific effect of health insurance on mortality, evidence from randomized studies fails to find statistically significant effects of either health insurance coverage or differences in the generosity of insurance coverage on mortality rates (Baicker et al., 2013; Baicker and Finkelstein, 2011; Finkelstein et al., 2012; Manning et al., 1987; Newhouse, 1993). However, these studies are underpowered to detect effects on rare outcomes such as mortality (Black et al., 2019)[2].

[2] The Oregon Health Insurance Experiment, one of the largest randomized studies of health insurance coverage to date, failed to find a statistically significant effect, despite finding a 0.13 percentage point reduction in mortality (std. err.=0.27), and a 16% reduction in the probability of dying for newly insured individuals relative to controls.

Natural experiments provide larger sample sizes, but at the expense of less robust identification. Studies of expansions of public insurance (Miller, Pinto, & Vera-Hernández, 2013; Sommers, Baicker, & Epstein, 2012), of the Massachusetts health care reform (Sommers, Long, & Baicker, 2014), and of the effect of aging into Medicare (Andersen, 2018; Card et al., 2009; McWilliams et al., 2007, 2004) have found statistically significant effects on mortality. Medicaid expansion reduced mortality by 6% at a cost to society of between $327,000 and $867,000 (Sommers, 2017). However, a number of other studies using similar identification strategies have found no effect of insurance coverage on mortality, or have obtained results that are sensitive to specification (Fink et al., 2013; Finkelstein and McKnight, 2008; Goldman and Lakdawalla, 2010; McWilliams et al., 2010; Moreno-Serra and Smith, 2012; Polsky et al., 2010, 2009).

A review of the recent literature on health care and the health effects of insurance (Sommers et al., 2017) noted significant improvements in access to primary and preventive care as well as increased diagnosis, treatment, and prescription drug use for chronic conditions, in particular diabetes and circulatory diseases (Ghosh et al., 2019). Improvements in self-reported health and reduced prevalence of depression have been identified and observed reductions in mortality were larger for conditions amenable to healthcare.

Because the subsidy can be thought of as a form of income transfer for its recipients, our findings also contribute to the literature on the effects of income shocks and welfare payments on health, an area where there is also significant disagreement in regard to both the direction and magnitude of the effect. Recent evidence of sudden and significant increases in mortality, deteriorations in health status, and riskier health-related behaviors have been associated with austerity policies after the financial crisis in which government expenditure on health and social and welfare services was cut, notably in Spain and Greece. [3]

The remainder of the paper is organized as follows. Section II provides background on the Swiss health insurance system and the subsidy programs studied. Section III describes our data. Section IV explains our approaches to identifying and estimating the effect of subsidies on mortality. Sections V to VIII provide evidence that insurance subsidies reduce mortality. Section IX discusses potential mechanisms by which health insurance subsidies affect mortality and trends in mortality in Switzerland, Canton Vaud, and surrounding areas. In Section X, we present several robustness checks. Section XI discusses our findings and conclusions. An Appendix follows, in which additional results and robustness checks are presented.

II. Background

A. Institutional Setting and Subsidy Programs

Since 1996, the Swiss government has required residents to purchase basic health insurance from private insurers regulated by the Federal government, which mandates that insurers accept all applicants at actuarially fair premiums. The government regulates contract terms including premiums, deductibles, and cost-sharing. Premiums are based on modified community rating, and vary by age groups (0-18, 19-25, 26 and over), geographic area,[4]

[3] See Adda, Banks, & von Gaudecker, 2009; Apouey & Clark, 2014; Evans & Garthwaite, 2014; Frijters, Haisken-DeNew, & Shields, 2005; Gardner & Oswald, 2007; Gross & Tobacman, 2014; Herd, Schoeni, & House, 2008; Karanikolos et al., 2013; Lindahl, 2005; D. L. Miller, Page, Stevens, & Filipski, 2009; Milligan & Stabile, 2011; Ruhm, 2000; Stuckler, Reeves, Karanikolos, & McKee, 2015.

[4] Premium regions are based on local area average income; Vaud had three premium regions between 2003 and 2008 and two from 2009 on.

deductible levels,[5] and managed care.[6] Premiums are reviewed each year by the Federal Office of Public Health (OFSP) and announced in October, with individuals choosing their new plans for the coming calendar year during the open enrollment period in November (Kreier and Zweifel, 2010). Annual co-payments are limited to 700 CHF in addition to the deductible (Table S1 presents the deductible levels available each year).

Canton Vaud operates two in-kind subsidy programs for low-income individuals, the "complete" subsidy program and the "partial" subsidy program, that pay a contribution to the cost of health insurance. We will refer to the subsidy determined by the allocation rules as the maximum allowed subsidy, because it may differ from the actual subsidy paid directly to the insurer. The actual subsidy is the lesser of the premium for the chosen insurance plan or the maximum allowed subsidy, which is the same method used in the Affordable Care Act in the USA.

The complete program is financed federally, and primarily covers the elderly receiving income support, the very poor, and individuals who have exhausted their unemployment benefits. Individuals receive a subsidy that is tied to the average insurance premium for the most generous (i.e. lowest deductible without managed care) insurance plans in the individual's region of residence. Changes in the complete subsidy level reflect changes in the premiums set to cover the cost of medical care in the area.

The partial subsidy program bases eligibility and the generosity of the maximum allowed subsidy on the *revenue determinant*, which is a measure of financial capacity that combines

[5] Allowed deductibles are 230, 400, 600, 1200 and 1500 CHF per year in 2003; in 2004, the lowest deductible was increased to 300 CHF; from 2005 onwards, allowed deductibles are 300, 500, 1000, 1500, 2000, and 2500 CHF per year.

[6] Higher deductible contracts may reduce premiums by no more than 70% of the difference in deductibles subject to a premium floor. Managed care plans may offer a premium discount of up to 25%.

wealth and income and includes deductions for the number of children in the household. Below, we will refer to the *revenue determinant* as income, unless otherwise noted. We denote the maximum allowed subsidy in year *t* by $\zeta(y_{t-3}, R_t)$ where y_{t-3} is income three years prior and R_t are the subsidy allocation rules in effect in year t. $\zeta(y_{t-3}, R_t)$ is described in the Appendix and graphed in Figures III and S1.

For incomes lower than a stipulated threshold, the maximum allowed subsidy is constant and at its highest level. As income grows beyond the threshold, the maximum allowed subsidy gradually reduces until it reaches zero. The parameters defining the subsidy formula are set by the government (*Conseil d'Etat*) of the canton in October of each year, shortly before the open enrollment period begins and after insurers announce new premiums for the following year.[7]

Each year, the Office of Health Insurance in Vaud (OVAM, *Office Vaudois de l'Assurance Maladie*) calculates an individual's subsidy using data from the cantonal tax authorities, which provide income and wealth information from previous years' tax returns. From 2006 onwards, the canton utilizes data from the tax return from three years before the subsidy-year (see index *t-3* in (1) in the Appendix), so that the 2006 subsidy year is based on the 2003 tax return, and so forth. Before 2003, tax returns were completed on a biennial basis. Owing to the change in tax regulations, from biennial to annual, in 2004 the OVAM reviewed all previously filed cases (from personal communication with Philippe Spack, OVAM, 2014), which may have contributed to a decline in partial subsidy eligibility observed between 2004 and 2005. In our empirical analysis, we explore the robustness of our results to the drop-off in subsidy receipt that took effect in 2005.

[7] The *Conseil d'Etat* does not appear to respond to announced premiums when setting subsidy program parameters: regressing program parameters in year t on announced premiums for year t yields small and non-significant coefficients and the largest R^2 indicates that premium variation explains only 16% of the variation in the minimum subsidy and the coefficient of progressivity (results not shown, but available upon request).

B. Changes in the Partial Subsidy Program

The partial subsidy program is financed by the canton; as such, the level of the subsidy is sensitive to the local fiscal situation. There are revisions to one or more parameters of the subsidy rule in almost every year, with the highest subsidy level ($F_t$in (1) in Appendix) rising over time, except in 2005. The income level below which one receives the highest subsidy (C_t in (1) in Appendix) was reduced until 2007, but rose from 2008 onwards along with the subsidy income eligibility threshold A_t (Appendix Table S2 shows the list of the program parameters for the years 2002 through 2011 and Appendix Figure S1 graphs the partial subsidy rule for individuals 26 and older for various years).

The large reduction in subsidy levels in 2005 is the result of an expenditure reduction plan to reduce persistent budget deficits. The changes to the subsidy formula reduced both the highest allowed subsidy to 225 from 260 CHF per month and the income eligibility threshold to obtain the highest subsidy by 2000 CHF. Overall, 475 deficit reduction measures were planned for 2005 across different administrative departments (Service d'analyse et de gestion financières, 2008), including measures affecting public health and hygiene programs. The cuts in public health and hygiene affected a child wellbeing initiative and reduced reimbursement rates for palliative care and transfers to hospices, and therefore were not likely to impact mortality in the general population.

The effect of changes in subsidy eligibility and the level of subsidies are illustrated in panel A of Figure I, which plots the average subsidy received by individuals in the complete and partial subsidy programs, and participation in these programs. The complete subsidy group was unaffected by the budget cuts in 2005 as the subsidy is indexed to average premiums. The upward trend in the mean complete subsidy implies that the cost of health insurance coverage increased more rapidly than inflation during this period.

The partial subsidy program, however, was affected by the budget cuts, with a reduction in the mean subsidy in 2005, relative to 2004 and 2003 (see Figure 1). Subsequent increases in the level of the subsidy and widening of the eligibility thresholds after 2005 were the result of the newly incumbent socialist party, and an easing of the deficit reduction plan from 2008 onwards following a sustained budget surplus from 2007. The reduction in subsidies was also accompanied by lower rates of enrollment in the partial subsidy program. Participation continued to fall in subsequent years despite mean inflation-adjusted subsidies returning to 2003 levels by 2011 and health insurance premiums having risen faster than overall inflation.

III. Data

Our data come from the *Système d'enregistrement des subsides aux primes d'assurance maladie* (SESAM), a database maintained by the OVAM. These data record the amount and type of subsidy received by individuals; the insurer and deductible chosen; the monthly premium; demographic information; household composition; income; geographic location; and reason for and date of subsidy termination (including due to death). Individuals are continually added to the SESAM, but are removed at the end of the year if they are ineligible for a subsidy in the subsequent year. As a result, we "carried-back" data on individuals who joined the SESAM database later in the year to January in each year,[8] as Swiss law prevents individuals from altering insurance contract type during the year, barring exceptional circumstances (roughly 0.1% of the sample switches any contract terms between January and December). The fact that the SESAM removes ineligible individuals at the end of the year also implies that the longest follow-up period for mortality that we reliably observe is eleven months.

We inflate all monetary values to 2011 using the GDP deflator for Switzerland. In order

[8] We test for any bias resulting from the inclusion of these individuals in our specification checks.

to ensure that our sample is as consistent as possible over time, we restrict our main sample to individuals with incomes low enough to qualify for a subsidy in any year (30,864 CHF for people living alone, 46,231 CHF if living with other individuals).[9] Finally, we exclude anyone under the age of 26, because individuals who are 26 or older all face the same insurance premiums and subsidy rules, conditional on region and household composition, and mortality is a rare event for younger individuals.

Summary statistics for the partial and complete samples (see Table I) indicate that: 1) individuals enrolled in the partial subsidy program typically receive lower subsidies than individuals enrolled in the complete program, reflecting the fact that partial subsidies do not track the cost of health insurance in the canton; 2) individuals in the partially subsidized cohort choose plans with higher deductibles and lower insurance premiums than individuals in the complete subsidy, who are beneficiaries of more generous in-kind subsidies, and hence insurance coverage; 3) individuals with partial subsidies are generally younger and have higher income than individuals in the complete subsidy, reflecting the nature of eligibility for the complete subsidy.

IV. Estimation of the Effect of Subsidies on Mortality

In order to estimate the causal effect of health insurance subsidies on mortality, we must address three main threats to identification. First, the subsidies are likely to be endogenous, as the subsidy an individual receives depends on her choice of plan, in addition to her income, wealth, and household composition. Second, the subsidy rules may be correlated with other policies or events within the canton that influence mortality. Third, sample attrition may have been associated with changes in the basis for subsidy eligibility determination and allocation

[9] As a specification check, we also estimated our models based on the sample of individuals eligible for a subsidy based on the current year's rules, rather than the most restrictive income thresholds.

rules. Hence, changes in the rate of attrition could potentially bias our results if unobserved characteristics of individuals who dropped out from the sample are associated with mortality risk and changes in subsidy.

We use two different estimation approaches, an instrumental variables strategy (IV) and a difference-in-differences strategy (DID), each addressing the first two issues. To handle sample attrition, we exploit the panel structure and long follow-up period to minimize data loss and assess the robustness of our results to excluding attritters. In the Appendix, we discuss our approach to inference, as mortality shocks may be correlated across different individuals with similar income levels.

A. Instrumental Variable Estimates

For our IV analysis, the sample includes individuals who are enrolled in the partial subsidy program in the first observed month of a given year. We measure the effect of the subsidy on mortality using the regression:

(1) $\Pr(Mortality_{it} = 1 | X, Subsidy_{it}, Year_t) = f(\beta_0 + \beta_1 Subsidy_{it} + \beta_2 X_{it} + \beta_3 Year_t)$.

In (1), *i* indexes the individual, *t* is the year, $Subsidy_{it}$ $Subsidy_{it}$ is the actual subsidy received by the individual in the first month of the year, $t-1$ X_{it} is a vector of controls to be discussed below, $Year_t$ $Year_t$ is a vector of year dummies, $Mortality_{it}$ is an indicator for dying in year *t*, and f is either the identity (the linear probability model) or the standard normal cumulative distribution function (Probit).

As the subsidy is a function of *revenue determinant*, simply estimating the relationship between the actual observed level of subsidy received in a given year and mortality would not enable us to identify the effect of the subsidy as it would be highly correlated with plan choice, income, wealth and other household characteristics known to be associated with mortality. Therefore, we include controls for income/wealth and household characteristics in the set of controls X_{it}.

OLS and Probit estimates of β_1 in equation (1) will be biased upwards if the actual subsidy received and risk of death were positively correlated with lower income. Therefore, we use a set of plausibly exogenous instrumental variables derived from the subsidy rule to estimate (1). We start with the difference between the maximum allowed subsidy for an individual given her current characteristics and current period's subsidy rule, and her maximum allowed subsidy according to the previous period's subsidy rule, while maintaining her current characteristics (i.e. $\zeta(y_{t-3}, R_t) - \zeta(y_{t-3}, R_{t-1})$). This instrumental variable is highly correlated with the actual subsidy received in the current period and provides additional variation in subsidies independent of the observed income and household characteristics, which we condition on in both stages of the 2SLS IV regression.

However, the effect of this instrument on the actual subsidy is not monotonic: when we plot the subsidy against the change in the subsidy rule we observe that an increase in the instrument is associated with higher subsidies in some ranges, and lower subsidies in other ranges. Therefore, we also use the lagged subsidy rule $\zeta(y_{t-3}, R_{t-1})$ as a second instrument. Conditional on the lagged subsidy rule, the relationship between the change in the subsidy rule and the actual subsidy received is linear (hence monotonic), which ensures that positive changes in the annual subsidy rule will always be associated with a higher subsidy level. Since we do not believe that the lagged subsidy rule belongs in the second stage equation, we use the lagged subsidy rule as an additional source of instrumental variables. We use linear and quadratic terms in both the change in the subsidy and the lagged subsidy rule as our instruments.

Our identification strategy identifies β_1 if the instruments are: 1) correlated with the actual subsidy received; and 2) only affect mortality through their effect on the subsidy conditional on observable factors. The validity of the first assumption can be assessed from the first stage regression results, which we present in summary form in Table II, and the entire results (excluding commune fixed effects) in Appendix Table S2.

For the second threat to identification—that the subsidy rule change is correlated with other policies or events (unobserved variables) that affect mortality—we note that, in order for another event to bias our results, the event must be correlated with mortality and either the change in the subsidy rule or the lagged subsidy rule as well as income for only a subset of years.[10]

The subsidy rules are a function of three-year lagged revenue determinant, which would preclude simultaneity bias from contemporaneous health shocks that influence income, subsidy allocation and mortality. We control for year fixed effects at the canton level to capture general contemporaneous trends or shocks in economic, health, or demographic factors influencing mortality that may have been correlated with the rule changes. Any policy correlated with income for all years will be absorbed by our income controls. By including additional year-by-income interactions, we further control for unobserved heterogeneity in macro policy changes that allow for differential shocks across income groups (the main source of variation in our

[10] For example, an outbreak of disease in the canton in a single year will yield an upward (downward) bias to our estimates only if: i) the disease causes a non-trivial number of deaths; ii) the disease causes greater mortality in those years with negative (positive) subsidy shocks; and iii) disease-specific mortality is negatively correlated with income.

subsidy rule). We control for other covariates that are correlated with mortality (a cubic polynomial in age, gender, and their interactions) and the determinants of the subsidy (a quadratic polynomial in income, subsidy group membership, an interaction between linear income and subsidy group membership, and number of children in the household) along with premium region fixed effects and commune of residence. In subsequent analyses, we also condition on previous period deductible choices, which is a measure of health risk. This provides further evidence that we have been able to overcome potential bias from unobserved health risks that might have affected past income and persist over time.

B. Difference-in-Differences (DID) Estimates

Our second identification strategy uses a difference-in-differences model to deal with the correlation between subsidy levels and health by comparing the change in average mortality for the partially subsidized group relative to a comparison group, consisting of individuals who received complete insurance subsidies. We use the significant drop in partial subsidies following the implementation of the budget deficit reduction plan in 2005, followed by its reversal as a natural experiment, to test the effect of subsidy changes on mortality.

We use completely subsidized individuals as a comparison group as their subsidy was indexed to the trend in average insurance premiums that corresponded to changes in the cost of insurance for the most comprehensive insurance plan. On average, their net premiums would remain constant over time, which ensures that their disposable income after health insurance contributions and generosity of insurance would remain unaffected by subsidy policies, whereas average subsidy levels in the partial subsidy (treatment) group fluctuated over time due to political decisions, and not just as a result of changes in individual circumstances. As there are significant differences in crude mortality rates and observable characteristics between our treatment and comparison groups, we use coarsened exact matching (Iacus et al., 2012) on deciles of age and income and exact matching on year, household type, and gender to weight individuals in the complete subsidy comparison group in order to produce comparable samples in each time period.

This approach deals with the possibility of confounding from other policy changes that are correlated with income, as the matched DID estimation uses a comparison group of similar individuals in the same canton exposed to the same unobserved confounding factors that may vary over time, such as reductions in spending on public services. In this case the identifying assumption is that, after matching, unobservable differences between individuals in each group are fixed over time (absorbed in the group indicator) and that the trend in mortality observed in the complete subsidy group reflects that of the partial subsidy group had their relative subsidy levels remained unchanged (Lee and Kang, 2006).

Using the same notation as in (1), we estimate the following equation using a Probit functional form:

(2) $$\Pr\left(Mortality_{igt} = 1 \middle| X_{igt}, PartialSubsidy_{igt}, Year_{gt}\right) =$$

$$f\left(\delta_0 + \delta_1 PartialSubsidy_g + \delta_2 Year_t + \delta_3 Year_t \times PartialSubsidy_g + \delta_4 X_{igt} + \epsilon_{it}^2\right)$$

where g is the group (=1 for the partial subsidy group) and $PartialSubsidy_g$ $PartialSubsidy_g$ is a dummy variable for belonging to the partial subsidy group. δ_2 β_2 is a vector of year coefficients, which captures baseline trends in mortality in the complete subsidy group, while δ_3 β_3 β_3 is the vector of coefficients that we present in the figures as the year-specific difference that arises from belonging to the partial, rather than complete, subsidy group. We also present results from a more restricted specification, which groups years into the time periods 2002-2004;

2005-2007; and 2008-2011 (2002-2004 is the omitted reference group) based on significant changes in the partial subsidy program rules.

In the matched sample regression analysis, we control for age, gender, household income, an interaction between gender and age and an interaction between subsidy group and income, the age-gender-year matched mortality probability in two neighboring French regions, premium rating region, and year fixed effects.

The descriptive analysis in Table I shows that mortality rates are significantly higher among completely subsidized individuals, indicating that they are sicker, even after matching. However, the individuals who are not matched in the complete cohort are at even greater mortality risk than the average matched individual, implying that matching produces more comparable populations. The purpose of the matching in the difference-in-differences model is to increase the likelihood of the common trends assumption holding.

V. Results

A. Instrumental Variables Results

Figure IV plots "binned" residuals from a series of regressions on our control variables in equation (1), where the bins are defined as centiles of the x-axis variable. While OLS estimates show a positive association between the subsidy and the probability of dying in the following eleven months (see Panel A), the plot of mortality against the fitted subsidy from our first stage regression indicates that individuals who receive higher subsidies due to the rule changes and/or lagged subsidy rule are less likely to die than are individuals who receive lower subsidies (Panel B). The remaining panels of Figure IV demonstrate that our instruments are strongly correlated with the actual subsidy an individual receives (the controls for panels C and D are augmented with the omitted set of instruments), with the first stage F-statistic exceeding 10,000, indicating the instruments are “strong” (Angrist and Pischke, 2008; Staiger and Stock, 1997).[11]

Estimates of equation (1) reported in Table II support and quantify the results in the graphs of Figure III. Columns (1) and (2) demonstrate that our instruments are correlated with both mortality and subsidy levels, conditional on our controls. Columns (3) and (5) demonstrate that actual subsidy levels in year *t* are positively, albeit non-significantly, associated with death over the next 11 months.[12] Columns (4) and (6) report results from our instrumented regressions and demonstrate that higher subsidies reduce mortality over the subsequent 11 months, with a 100 CHF reduction in the monthly subsidy (which is a scale that we observe in our data) increasing the probability of dying over the subsequent 11 months by 0.16 to 0.20 percentage points (or 22% to 27%) for the IV-Probit and IV estimates respectively (see columns 4 and 6 of Table II). These results are statistically significant using the conventional cluster-robust covariance matrix (standard errors in round brackets), in which clusters are income bins. When we estimate the covariance matrix allowing for correlation between income bins, the IV results are no longer statistically significant at the 5% level, but remain significant at the 10% level with a p-value of 0.055 (standard errors in square brackets). The implied elasticity of mortality with respect to the monthly subsidy is between -0.34 to -0.43. Complete results from these models are presented in Appendix Table S3.

The instrumental variable estimates provide the local average treatment effect for a specific subgroup of the population whose level of subsidy would be affected by the changes in

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[11] In addition, it is not the case that our instruments are simply “adding up” to the subsidy since the first stage R-squared value is much smaller than 1.
[12] This result is somewhat sensitive to specification; when we include a broader sample of individuals the OLS coefficient on the subsidy decreases to 0.00155 (SE=0.00196) using all individuals who are income eligible for a subsidy under the rules in effect for that year; when we also include individuals with data from 2002 (for whom we do not have lagged subsidy rules), the point estimate increases to 0.00243 (SE=0.00178).

the subsidy rule. Although we cannot identify individuals affected by the instrument, we can characterize these individuals statistically.[13] Appendix Table S5 presents the population mean, likelihood of complying, and the implied complier mean for both the change in subsidy rule instrument and the lagged subsidy rule instrument. Broadly speaking, compliers for both instruments have higher matched mortality probabilities, pay higher net insurance premiums for coverage, and are older. In addition, compliers to the change in subsidy rule instrument are also more likely to have a low deductible insurance plan in the current (and previous) period and to purchase insurance with a lower gross insurance premium, while compliers to the lagged subsidy rule instrument purchase more generous insurance coverage, based on the gross insurance premium. This suggests that the population impacted by changes in the subsidy rule were a more vulnerable subgroup of the subsidized population with higher health risks, income constraints and insurance costs.

Finally, columns (7) and (8) of Table II presents results using the age-gender-year matched mortality rate from the neighboring French regions of Franche-Comté and Rhône-Alpes as a measure of mortality risk. By regressing a measure of mortality risk, correlated but not directly affected by the subsidy's changes, on the instrumented subsidy, we are testing whether compliers to the instrument are at systematically higher mortality risk, in which case our

[13] For a binary instrument and binary characteristic, the probability that an individual has the binary characteristic x given that she is a complier—that is, that their subsidy increases because of the instrument—can be written as:

$$\Pr(x=1|S_1>S_0)=\frac{\Pr(x=1,S_1>S_0)}{\Pr(S_1>S_0)}=\frac{\Pr(S_1>S_0|x=1)}{\Pr(S_1>S_0)}\Pr(x=1)=\frac{\mathrm{E}(S_1>S_0|x=1)}{\mathrm{E}(S_1>S_0)}\Pr(x=1)$$

Where $S_1>S_0$ denotes individuals whose *actual* subsidy increases because of receiving treatment and x is a binary characteristic. The first and second equalities follow from an application of Bayes' Rule, while the third equality reflects the idea that for a binary treatment the probability of complying equals the expectation of complying, which is the first-stage regression coefficient. Therefore, the final term is the ratio of the first stage regression coefficient for people with $x=1$ to the sample average first stage regression coefficient multiplied by the probability of the characteristic. We estimate this model using binary instruments and treatments defined as being above or below the median value of the change in or lagged maximum allowed subsidy (instruments) and the median subsidy. All estimates also partial out our control variables before estimating the first stage regressions. See Angrist, Lavy, and Schlosser (2005) and Angrist and Pischke (2008) for additional details.

observed reduction in the probability of death becomes smaller, relative to the individual's risk of dying. On the other hand, if our IV estimates are due to changes in the overall burden of disease in the area, then the coefficient on the subsidy should be comparable in size and magnitude between the French mortality rate and actual mortality regressions. We find that not only do the point estimates differ in size by an order of magnitude, but the Franche-Comté and Rhône-Alpes estimates are of the opposite sign. These results provide further indication that compliers for our instruments are individuals who are at higher risk of dying than the average person in our sample. In results that are not shown here, we also estimated our mortality risk models including age-by-year, age-by-gender, and year-by-gender fixed effects, so that the only variation in the dependent variable is the idiosyncratic change in mortality risk for age-gender matched individuals in that year. Including these additional fixed effects, we find a very small effect of the subsidy on the matched French mortality rate, while our IV results for eleven month mortality are unaffected.

B. Difference-in-Differences (DID) Estimates

Figure V presents the difference-in-differences results graphically. There exists significant variability over time in the difference in the subsidy received by the partial subsidy group relative to the complete subsidy group, even after matching (see panels A and B). Complete subsidies increase between 2002 and 2011, reflecting the rise in health insurance costs above indexed price inflation in the canton (the dip in 2008 reflects stable insurance premiums in nominal terms). Relative to the complete subsidy group in 2002, the average monthly partial subsidy is 100 CHF lower between 2005-7, which is a 77 CHF reduction compared to the difference in 2002-4. By 2008-11, the difference in subsidies between the partial and complete subsidy groups narrows so that average subsidies are 35 CHF lower per month compared to 2002-4.

The mortality trend over time for the complete subsidy group (matched by gender, income and year) closely follows that of the general, unsubsidized population in Canton Vaud (panel C of Figure V), supporting the assumption that changes in mortality in the partial subsidy group are due to changes in the subsidy, and not other unobserved economic, political or environmental changes. Relative to the complete subsidy group in 2002, individuals who received partial subsidies were more likely to die in years with significant subsidy cuts (panel D), with mortality rates increased by around 0.005 (38%).

Table III provides the point estimates corresponding to the pooled analyses in panels B, D, E, and F in Figure V. Between 2005-7 and 2002-4 mortality in the partially subsidized group increases by an economically and statistically significant, 0.23 percentage points (24.6% increase) relative to individuals in the complete subsidy group, and remains 0.23 percentage points higher between 2008-11 compared to 2002-3. Unmatched estimates are slightly larger and also statistically significant. Using the interaction between partial subsidy enrollment and year dummies as instruments implies that a 100 CHF reduction in the monthly subsidy increases mortality by 0.365 percentage points (30.3% reduction), which is slightly larger than the IV estimates using the change in the subsidy due to the rule change as an instrument for the partial subsidy group only.

VI. Sample Attrition

The third threat to identification comes from individuals switching between the two subsidy programs and attrition from the partial subsidy sample. Relatively few people switch between the programs in any given year (see Appendix Table S3 for rates of annual switching).

If differential selection into the partial or complete subsidy programs over time is associated with changes in the rules, then our estimates may be biased. If relatively sicker individuals switch from the partial to complete subsidy, as indicated by the higher observed mortality rate in the complete program, then there would be an upward bias (less negative estimates) of the subsidy effect in our difference-in-differences analysis. To account for within-year switching we adopt an intention to treat framework (ITT), with individuals assigned to their original subsidy program at the start of the year. We also test the robustness of our results to between-year switching by explicitly modeling selection into the complete or partial subsidy programs in each year using a Heckman sample selection correction model.

Some people also move out of the sample group because their income increases above the income threshold that we imposed for inclusion in our baseline sample (see Appendix Table S3). On average 15% of partially subsidized individuals are ineligible for inclusion in the subsequent year as a result of income growth. The most significant threat to the validity of our results arises from attrition due to an unusually large fraction, 25%, of people who became ineligible between 2004 and 2005, which coincided with the largest reduction in partial subsidies. This increased rate of attrition from the sample could bias our results if unobserved characteristics of individuals who dropped out from the sample were associated with mortality risk.

Individuals who exited the sample because they became ineligible were systematically healthier than those who remained in the sample, as indicated by their lower mortality risk. However, compared to the mortality rates of ineligible individuals in other years, the attrition at the end of 2004 was actually associated with a somewhat higher mortality risk for ineligible individuals (see Appendix Table S3). As a result, our estimates may be biased towards zero due to the excess attrition in 2004-5. We test the robustness of our results to including or excluding these individuals (see Section VIII and Appendix for results). The entry of newly subsidized individuals into the sample who are systematically healthier than the continuing sample, but not as healthy as ineligible individuals (based on mortality rates), also partially offsets attrition.

In addition to individuals exiting the sample due to financial ineligibility or by switching to the complete subsidy, across all years there are 11,000 observations in total that are missing (around 2% per year). Of these 11,000 observations, we are able to infer that almost 3,000 of them are alive because they subsequently re-enter our sample. For these observations, however, we do not know their income when they were not subsidized, which is required to construct an instrument for the subsidy. To resolve this problem, we impute income either using income from another year that used the same tax return or by carrying forward the previous inflation-adjusted income (we refer to this sample as the "lagged income sample"). We also estimate models using the first observed income level for all individuals in the SESAM data to construct our instrument (referred to as the "first observed sample" below) (results are presented in Appendix, Section S3).

Finally, deaths, by themselves, may bias our estimates, an effect known as "harvesting" or "mortality displacement". Frail individuals who have a higher probability of dying will die sooner so that, over time, the cohort of individuals who were in our data from the beginning becomes systematically healthier. The bias from harvesting can be either positive or negative, depending on the relationship between subsidy levels over time. Since subsidy levels are initially decreasing and then increasing, harvesting would downwardly bias our estimate of the effect of subsidies on mortality. As a crude check for harvesting effects, we split the sample at 2005, since in the early period subsidies are falling, while in the latter period subsidies are increasing. The harvesting effect would then predict that the pre-2005 subsidy effect would be larger than the post 2005 subsidy effect. We find a larger (in magnitude) effect of the subsidy on mortality prior to 2005 than in the later period (see Table VII as well as section VIII, and Appendix for results). This suggests individuals in the pre-2005 years were more frail and likely to die in response to the subsidy cuts in comparison to the post-2005 years, where we observe a slightly smaller negative effect, when there were subsidy increases and possibly individuals at lower risk of dying. This pattern may also reflect an asymmetric effect with the initial cuts having a larger impact than some of the later subsidy increases. Our DID estimates would be less impacted by harvesting as it would also apply to our control group.

VII. Mechanisms

A. Evidence from Micro Data

We analyze two mechanisms by which subsidies impact mortality: i) by affecting the choice of insurance contract and the expected year-end price of medical care; and ii) by affecting the net insurance premium, changing disposable income.

The individual decision making process we have in mind is in the spirit of the two-stage model of Cardon and Hendel (2001). In the first stage, the consumer chooses the insurance policy that yields the highest expected utility over a composite good and health. In the second stage, after the uncertain health state is realized, the individual chooses consumption of health care and all other goods. The choice of a policy in the first stage is essentially the choice of the budget set for the second stage.

The subsidy program influences the choice of policy in the first stage by changing the prices of policies. Given preferences and risk type, a subsidy increase (decrease) could be used to buy a lower deductible contract (could induce a switch to a higher deductible), which would reduce (increase) the out of pocket cost of healthcare and increase (reduce) financial protection in case of a health shock, or to switch to a more expensive (cheaper) and higher (lower) quality insurer.[14] If insurance generosity affects mortality, then the subsidy can impact mortality by affecting the choice of insurance contract, and in consequence the expected year-end price of medical care (Aron-Dine et al., 2013; Keeler et al., 1977).

The subsidy can also affect mortality by changing individuals' disposable income: an increase (decrease) in the subsidy can be used to further reduce the net premium (may increase premiums if individuals do not switch to cheaper plans) and hence increase (decrease) disposable income, with consequences on overall consumption decisions in the second stage. An increase in disposable income may improve health outcomes and reduce mortality rates if the extra income is spent on health-improving goods and services (e.g. healthier food, household energy, leisure activities etc.), or health care services. It could also reduce financial stress which is associated with poor health outcomes (Lovallo, 2015).

In Table IV, we show that subsidies affect insurance contract characteristics such as the deductible, gross and net insurance premiums. A decrease in the subsidy results in a decrease in the gross insurance premium, but the net premium paid by the individual still increases, which reduces disposable income. In the difference-in-differences analysis, we find that the 2005-2007 and 2008-2011 periods show significantly higher deductibles, lower gross insurance premiums, and higher net insurance premiums. Hence despite partially subsidized individuals choosing relatively cheaper contracts in response to the subsidy cuts, they continue to pay higher net premiums. This finding is compatible with an effect of lower subsidies increasing mortality rates through both the choice of less generous contracts, which increase the price of healthcare and

[14] Standard health insurance plans are available at different gross premiums depending on the insurer. Cheaper plans may result in a lower quality of service, such as delays in reimbursement as well as greater restrictions on treatment possibilities. Finally, individuals could default on their premium payments, which could leave them liable for costs or prevent timely access to care.

expose individuals to less financial risk protection in the case of a health shock, and reductions of disposable income. Interestingly, when subsidy levels rose again in 2008-11 relative to 2005-7, average deductibles did not change, remaining significantly above 2002-4 levels. This asymmetric response to the subsidy rise suggests individuals prefer to reduce premiums with certainty rather than lower deductibles, which reduces an uncertain out of pocket risk. It also confirms the importance of the deductible in explaining part of the mortality increase, which remained significantly higher in 2008-11 relative to 2002-4. Hence, we explore the link between subsidy, choice of deductible, net premiums and mortality in Table V and Table VI, allowing for heterogeneity in effects across income groups and health state. We do not have a direct measure of health risk type, so, motivated by evidence of adverse selection in the literature predicting that lower deductible plans are purchased by sicker individuals, (Akerlof, 1970; Cardon and Hendel, 2001; Rothschild and Stiglitz, 1976; Trottmann et al., 2012), we use lagged deductibles as a proxy for health state,[15] and test whether the effect of subsidies on mortality differs by prior deductible choice (see Table V). Individuals who chose higher deductibles in the previous year are, on average, less likely to die in the current year, all else equal (column 1 of Table V).[16] After controlling for prior deductible levels our IV estimates of the subsidy are unchanged, which supports our identification assumption that annual changes in the subsidy rule are uncorrelated with health risk. Interacting the subsidy with the lagged deductible (column 2 of Table V) indicates that there is significant heterogeneity in the effect of the subsidy on mortality (joint p-

[15] OLS regressions for the deductible or net insurance premium indicate that insurance contracts are adversely selected (not shown), which is consistent with individuals making plan choices based on their expected health outcomes and with evidence that individuals who choose lower deductible insurance plans are in worse health (Trottmann et al., 2012).

[16] The sample size is reduced since individuals must have been included in the SESAM for at least one month in the prior year. We group the lagged deductible into ranges of 230-300CHF, 400-600 CHF, 1000-1500 CHF, and 2000-2500 CHF, based on the number of people with each deductible level and rules governing how deductibles were reassigned in 2004 and 2005 following a reform of the set of allowed deductibles in Switzerland.

value <0.05).We elaborate on this finding by stratifying the sample of individuals eligible for the partial subsidy around median lagged income. For both groups the lagged deductible fixed effects exhibit the expected negative relationship with mortality, so that individuals who chose higher deductibles in the previous year are less likely to die in the current year (columns (3) and (5) of Table V). When we introduce interactions between the lagged deductible and the instrumented subsidy (columns (4) and (6) of Table V), we find evidence of heterogeneous effects for the low-income groups, but not the high-income groups (assessing statistical significance using the cluster-robust p-value).

Lower income individuals who chose higher deductibles in the previous year experience a significantly greater increase in mortality risk following a subsidy reduction relative to individuals who purchase the lowest allowed deductible (a 100 CHF reduction in the subsidy increases the probability of dying over the next eleven months by an additional 0.0020 percentage points, or a 71% risk increase). For higher income individuals, the coefficient on the interaction between the subsidy and lagged highest deductible is positive and non-significant, and is statistically significantly different from the comparable estimate for lower income individuals. For higher income individuals who chose the lower deductible group (400-600 CHF) we observe a statistically significantly lower mortality risk than higher income individuals who purchased the lowest deductible (their mortality risk is 0.0015 percentage points lower, or a 28% risk reduction).

Last, we investigate how these differences might arise by analyzing the effect of the subsidy on the deductible and net premium of the chosen insurance contract taking into account heterogeneous effects. Lower subsidies increase deductibles on average (column 1 of Table VI), but this effect is primarily attributable to the behavior of lower income individuals (for whom a

100 CHF reduction in the subsidy increases the deductible by 10.5 CHF), and there is no effect for higher income individuals on average. Allowing for heterogeneous effects of the subsidy by previous deductible choice indicates that individuals choosing lower deductible contracts in the previous period (for which we group 400CHF to 600CHF) buy a lower deductible if possible, while those choosing the two highest deductible groups in the previous period respond to subsidy cuts by increasing their deductible. The change in net premiums is consistent with these findings (see column 4, 5, 6 of Table VI). Both poorer and richer individuals with deductibles above 1000 CHF experience a small, but statistically significant reduction in their net insurance premium in response to subsidy cuts, reflecting the increase in their deductible levels as well as switching to less expensive insurance contracts. Nevertheless, the effect of a subsidy reduction is to increase net insurance premiums on average (columns 4, 5, 6), despite increasing deductibles.

The results of Tables V and VI are compatible with the following potential mechanisms. First, individuals choosing the lowest deductible plan in the previous year do not switch to higher deductible plans following a reduction in subsidies, which suggests that the relationship between reduced subsidy and increased mortality for these individuals is caused directly by a reduction in their disposable income and is not related to a deterioration in their insurance coverage, although the reduction in income may result in lower health care consumption. As a result, we would expect to see an increase in income-elastic causes of death during periods of subsidy reductions. Likewise, in response to a subsidy increase, individuals who have already selected the lowest deductible cannot benefit from the additional subsidy by reducing their deductible further, instead the subsidy contributes entirely to reducing premiums.

Second, poorer individuals choosing higher deductible plans are at greater mortality risk from a subsidy reduction than poorer individuals in the lowest deductible plan, but this is not the case for wealthier individuals, which suggests that the generosity of insurance coverage affects mortality. In terms of a subsidy increase, poorer individuals who chose the highest deductible and whose insurance premiums are fully subsidized, can only benefit from an in-kind subsidy by choosing a lower deductible insurance plan, which reduces their mortality risk. For higher income individuals who chose the lower deductible group (400-600 CHF), a subsidy reduction has less of an impact on their mortality risk because they switch to the lowest deductible, benefiting from lower out-of-pocket costs without incurring a large increase in their net premiums.

B. Trends in Population Mortality

To study differences in causes of death over time, we compare all-cause and cause-specific mortality rates in the population between Vaud and the two bordering French regions of Franche-Comté and Rhône Alpes (for more details, see panel B of Figure 1 and Appendix, , Figure S2). We observe that at the time of the large subsidy cut between 2005 and 2006 the gap between the French and Vaud mortality rates, which had been converging prior to 2005, widens substantially. Between 2005 and 2006 relative mortality risk for all causes increases by 6.1 percentage points to a relative mortality risk of 10.1%, corresponding to 330 additional deaths in total. In 2007 relative mortality risk was 8.2% higher in Canton Vaud compared to France, but by the end of the decade there was no difference in mortality risk between France and Vaud, possibly reflecting the expansion of the subsidy program, with increases in both the numbers eligible for the subsidy (extensive margin) as well as the level and the numbers eligible for for the maximum subsidy (intensive margin). The mortality increases are for causes responsive to short term income shocks (so called ‘deaths of despair’): suicides, cardiac and digestive causes of death (for instance alcoholic or drug induced liver failure and ulcers), and maternal and infant mortality, all of which increased markedly in the 2005-2007 period relative to the rates in France and compared to the prior trend.

VIII. Robustness Checks

We assess the robustness of our results to attrition bias in Table VII. First, we exclude (from all years) anyone who moved out of the sample group in 2005, since that was a year with particularly high attrition as the canton changed how it used tax return data to calculate subsidies. The results for this sample are comparable to our base results (column 2). We also excluded anyone who ever attrited from the sample (column 5), which yields somewhat larger estimates of the effect of subsidy on mortaily than our main analysis.

Lastly, we also estimated a model with the subsidy interacted with year (Figure V), which demonstrates that the effect of the subsidy on mortality appears in all years in our sample and does not depend only on variation in 2005—which is the year with significant attrition. The consistency of the cross-sectional IV estimates over time and the fact that omitting individuals who leave the sample group does not change our results suggest that changes in the composition of the partial subsidy group over time is unlikely to be influencing our findings. We present additional discussion of how we attempted to test whether attrition could explain our results in Section S3 in the Appendix. For the most part, we are able to replicate our main results using a variety of methods to demonstrate the robustness of results obtained using the attrited population.

A large number of additional robustness checks are also summarized in the Appendix. Our results are robust to including a variety of additional controls, many of which test whether the subsidy rule (and changes in the subsidy rule) is correlated with other temporal influences on mortality. We also vary both the duration of follow-up (e.g. mortality within 23 months, rather than 11 months) and restrict the sample to individuals in their first one, two, or three years in the

SESAM database, with no substantial changes in our overall results (although some of these results are less precise than our main results). We also consider alternative identification strategies including using the two sets of instruments individually. In this analysis, only the lagged subsidy instruments in the IV-Probit achieved statistical significance at the ten percent level, but all of the point estimates were reasonable consistent with our estimates using both sets of instruments together. We also constructed two other types of instruments that relied on either a counterfactual assumption or simulation. In the first approach we trended forward income and used trended income to calculate the subsidy, rather than actual income (Gruber and Saez, 2002), which yielded comparable results to our main analysis. In the second approach, we constructed simulated instruments (Currie and Gruber, 1996; Cutler and Gruber, 1996) based on the demographic characteristics that we observed in our data. The estimates produced were implausibly large in the LPM model, but plausible and consistent with our main analysis in the IV-Probit model. These results are presented and discussed in Appendix Section S2.

IX. Discussion and Concluding Remarks

We provide new evidence on the health effects of subsidies for the purchase of mandatory health insurance. We find that reductions in subsidies for lower income individuals increase mortality in the next eleven months, by conducting statistical analyses using two approaches—an instrumental variables approach and a difference-in-differences approach—which rely on different assumptions. The effect of insurance subsidies on mortality is non-trivial: a 100 CHF per month increase in the subsidy reduces mortality by 0.16 to 0.20 percentage points over the next eleven months in our instrumental variable analyses, and a comparable 75CHF reduction in subsidy levels was associated with a 0.23 percentage point increase in mortality in our difference-in-differences analysis. Expressed as a cost-effectiveness ratio, Canton Vaud is spending 550,000 CHF, approximately $760,000 PPP 2012, to save at least one statistical life year, which is close to the $867,000 societal cost for saving a life estimated by Sommers (2017) in relation to Medicaid expansion in the US. However, the welfare benefit of insurance subsidies is likely to be larger than implied by these cost-effectiveness ratios since these insurance subsidies also reduce financial risk exposure, which represents a significant part of the welfare benefit of other health insurance programs such as Medicare (Finkelstein and McKnight, 2008).

The magnitude of our estimates is plausible and consistent with mortality trends in Canton Vaud during the period with the largest cut in subsidies. Mortality was 0.16% higher for subsidized individuals during this time period due to the subsidy cuts and 18% of the population received partial subsidies. Therefore, our results imply an increase in mortality of between 2.5 and 3.6 additional deaths per 10,000, which accounts for 50 to 75% of the incremental mortality rate in Vaud in 2006 relative to 2004. Contemporaneous population-level evidence indicates that mortality rates during the period of the largest subsidy reductions were elevated for causes of death more responsive to short term income shocks, such as suicide, cardiac death, and maternal/infant mortality, while other causes such as cancer, that are less likely to be affected by short-run income fluctuations, were unchanged relative to neighboring provinces in France.

The reduction in premium subsidies caused individuals to choose higher deductibles and cheaper insurance plans, while still paying higher net premiums for their insurance. Kauffman et al. (2017) support these findings for Switzerland, where individuals receiving in-kind premium subsidies had higher probabilities of choosing lower deductibles than individuals receiving a cash transfer. Their results suggest that it is legitimate to expect changes in behavior, in our case the choice of deductible levels, following changes in subsidy levels.

We find that mortality risk from reductions in the subsidy is increased for individuals who have already enrolled in the lowest possible deductible plans. These individuals remain enrolled in the lowest plan even after reductions in the subsidy, and hence experience a significant increase in their net premium, providing evidence that negative income shocks can increase mortality rates. The incidence of the subsidy changes impacted a particularly vulnerable 'compliant' population: the most elderly, living alone, with the lowest deductibles who are paying the highest net premiums and are already at a higher risk of mortality, for whom a significant financial shock has the potential to lead to a rapid decline in health status (Hugonnier et al., 2018).

For individuals with lower income who have previously selected higher deductible insurance plans, it appears that subsidy reductions further increase mortality risk. We conjecture that poorer individuals who have previously chosen the highest deductible plans are at greater risk of illness than their less poor counterparts, but have enrolled in high deductible plans due to a tighter budget or liquidity constraint. Hence these individuals' mortality risk may be more affected by reduced health insurance coverage, which results in a higher cost of health care (reduced financial protection), and forgone health care utilization (Brot-Goldberg et al., 2017). We also observe persistence in elevated mortality risk for these individuals in our difference-in-differences analysis and in IV estimates on longer term mortality, which is consistent with a significant increase in deductible levels over time.

Our study has some limitations. The instrumental variables analysis relies on the assumption that the change in the subsidy rules only affects mortality through its effect on the subsidy. Individual income or other circumstances would be unlikely to change rapidly in response to unannounced policy changes, and we also control for income and a secular time trend to capture changes in macro-economic circumstances. Other sets of instruments that are based either on lagged income growth or a simulated instruments strategy yield similar, but noisier, estimates, suggesting that our identification strategy is valid. The difference-in-differences analysis relies on a parallel-trends assumption for comparability between our subsidized populations over time. By comparing matched groups, we improve the plausibility of these assumptions, but DID estimates may be more sensitive to attrition related to unobserved factors affecting mortality risk. However, the DID results are buttressed by trends in overall mortality in Canton Vaud, which would not be affected by attrition. We lack direct measures of health status, health care utilization, and cause of death at the individual level that would have enabled us to better control for risk selection and to further explore how subsidy changes are associated with mortality.

We interpret our findings as evidence of an effect of insurance generosity on mortality, especially evident for lower income individuals in high deductible plans and for those choosing low deductible plans. This study raises a number of policy concerns for the financing of health care and health insurance subsidies, particularly as the mechanism of financing health insurance and utilization of health care has direct implications on population health. Overall, our analysis stresses the importance of allocating adequate health insurance subsidies so that households' financial protection and health are not adversely affected by rising insurance premiums. Improved targeting of subsidies to more vulnerable groups such as the elderly living alone and indexing subsidies to rising premiums are possible solutions. More fundamentally, addressing factors that contribute to the regressive financing of health insurance and health care (Crivelli and Salari, 2014) may improve health outcomes for the financially disadvantaged.

Acknowledgements

Simona Grassi receives support from the Service des Assurances Sociales et de l'Hébergement (SASH) of the canton Vaud. Simona Grassi and Mark Dusheiko received support from the Cour de Comptes of the canton Vaud (Switzerland). Excellent research assistance was provided by Tristan Equey, Adrien Magnenat and Stephanos Vlachos. Part of this research was carried out while Simona Grassi was a Harkness Fellow at the Department of Health Care Policy at the Harvard Medical School. We thank Ken Snowden, Kosali Simon, and participants at the 2014 American Society of Health Economists Biennial, the 25th Annual Health Economic Conference, and seminar participants at Yale School of Public Health, Triangle Health Economics Workshop, and UNC Greensboro for helpful comments.

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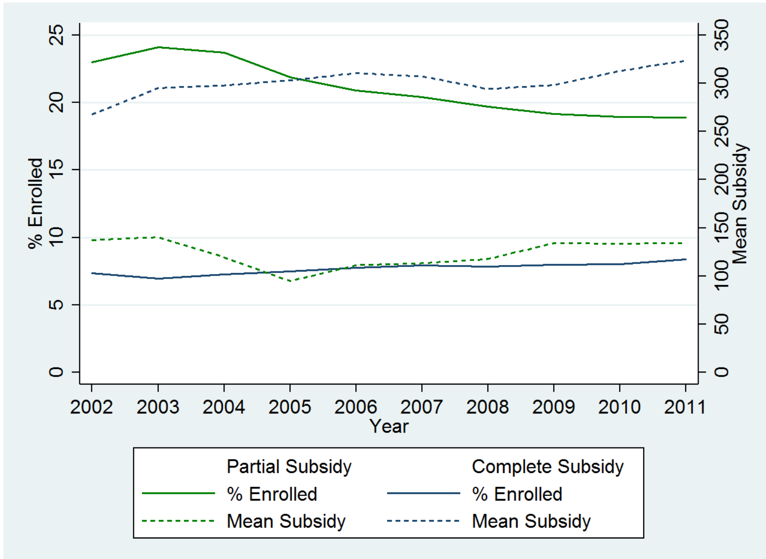
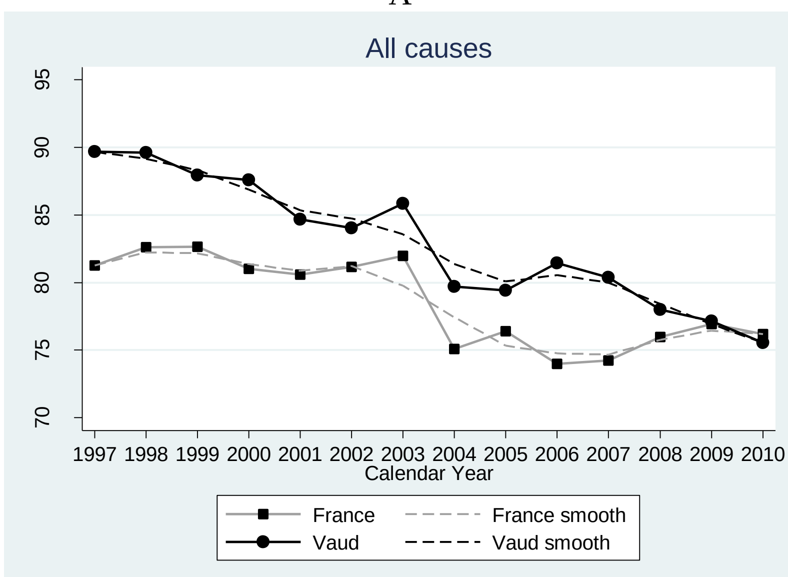
Figures



A



B

Figure I: Subsidy program participation rates and mean monthly subsidies received (A) and all-cause mortality per 10,000 for Vaud and two French regions (Franche-Comté and Rhône-Alpes) (B).

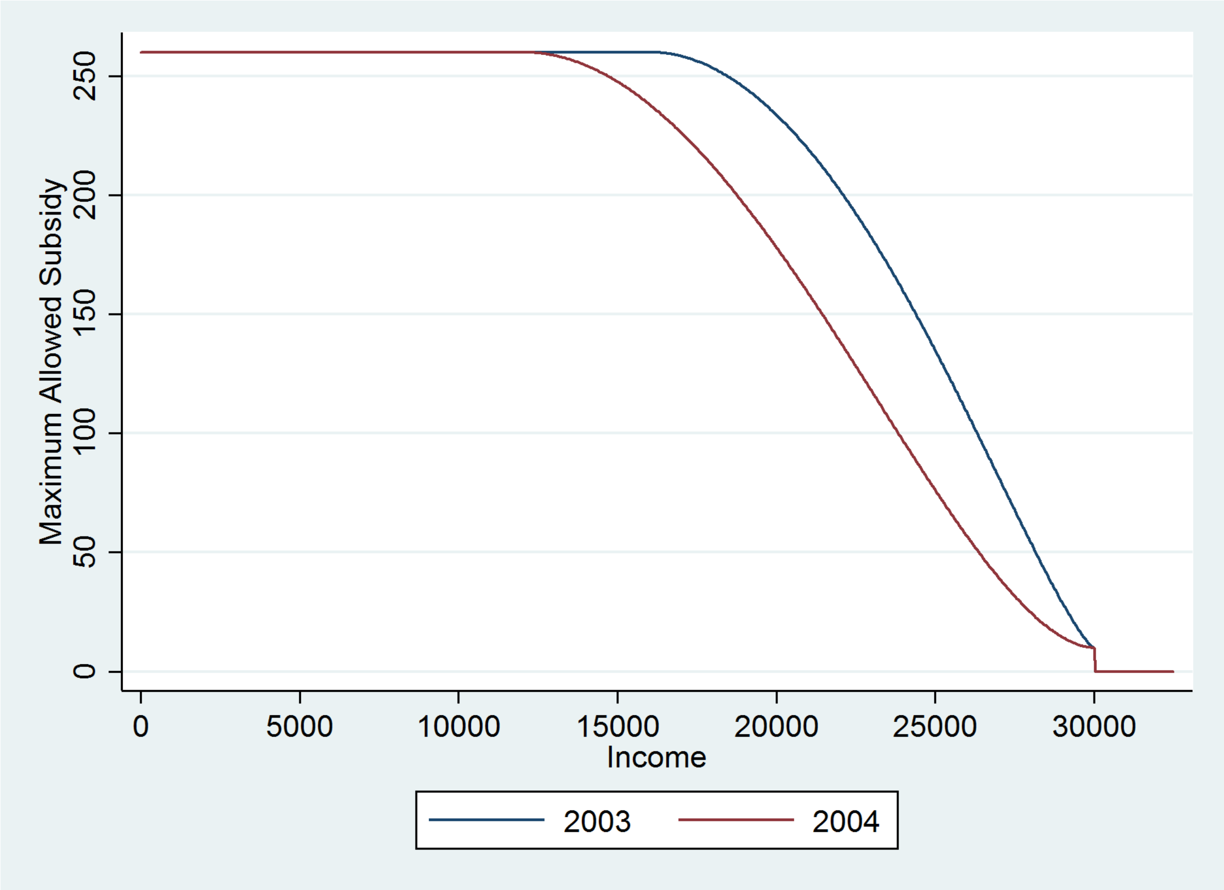


Figure II: Maximum allowed subsidy in 2003 and 2004.
Figure plots the maximum allowed subsidy for single individuals in 2003 and 2004. Income is the *revenue determinant*, which is an adjusted measure taking into account number of children and asset holdings.

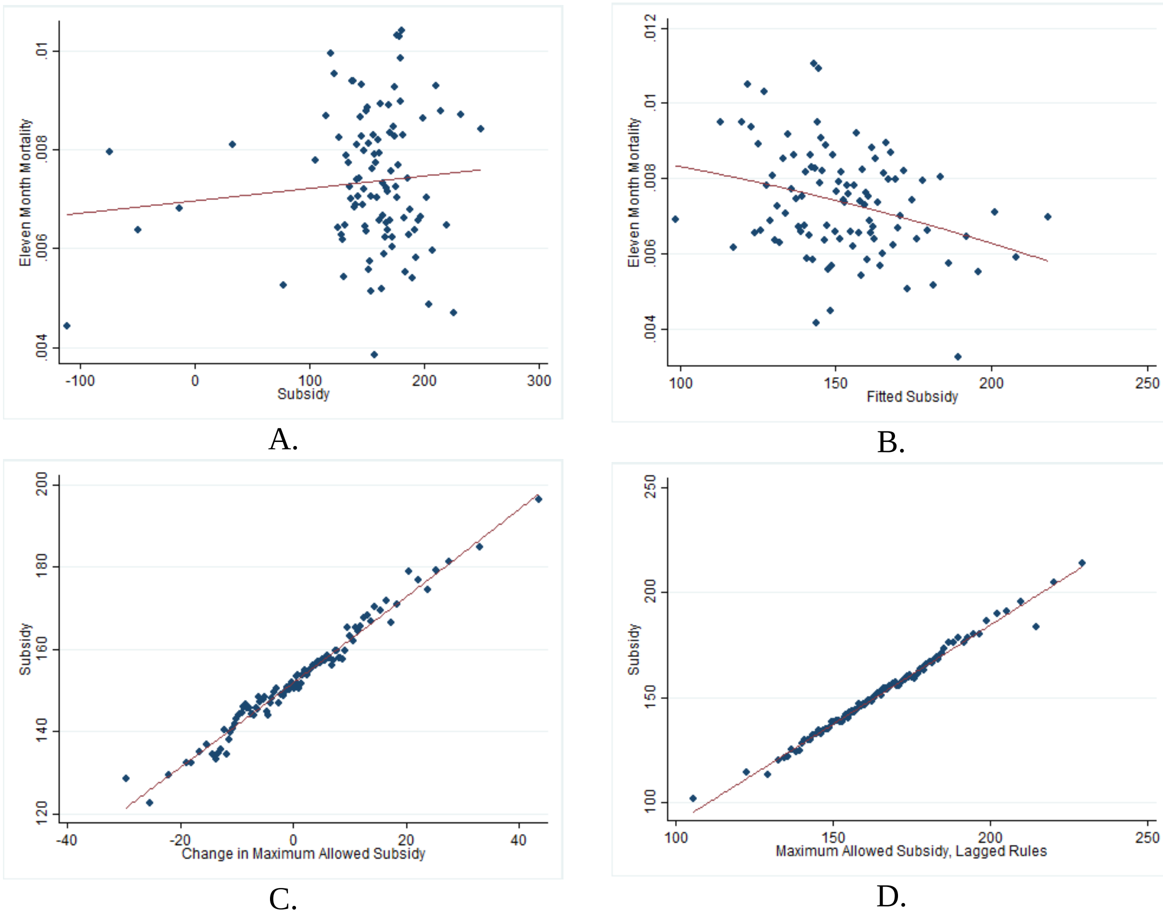


Figure III: OLS and reduced form estimates of the effect of insurance subsidies on mortality in panels A and B, and relationships between subsidy levels and instrumental variables in panels C and D. Panels A and B plot adjusted residuals from regression of mortality on either the monthly insurance subsidy or the instrumented subsidy level, year fixed effects and the full set of controls used in Table II. Panels C and D plot adjusted residuals from regressions of the subsidy on the instruments, controlling for year fixed effects and the full set of controls used in Table II. Dots are located at the means of the x and y-axis variables for bins defined by centiles of the x-axis variable.

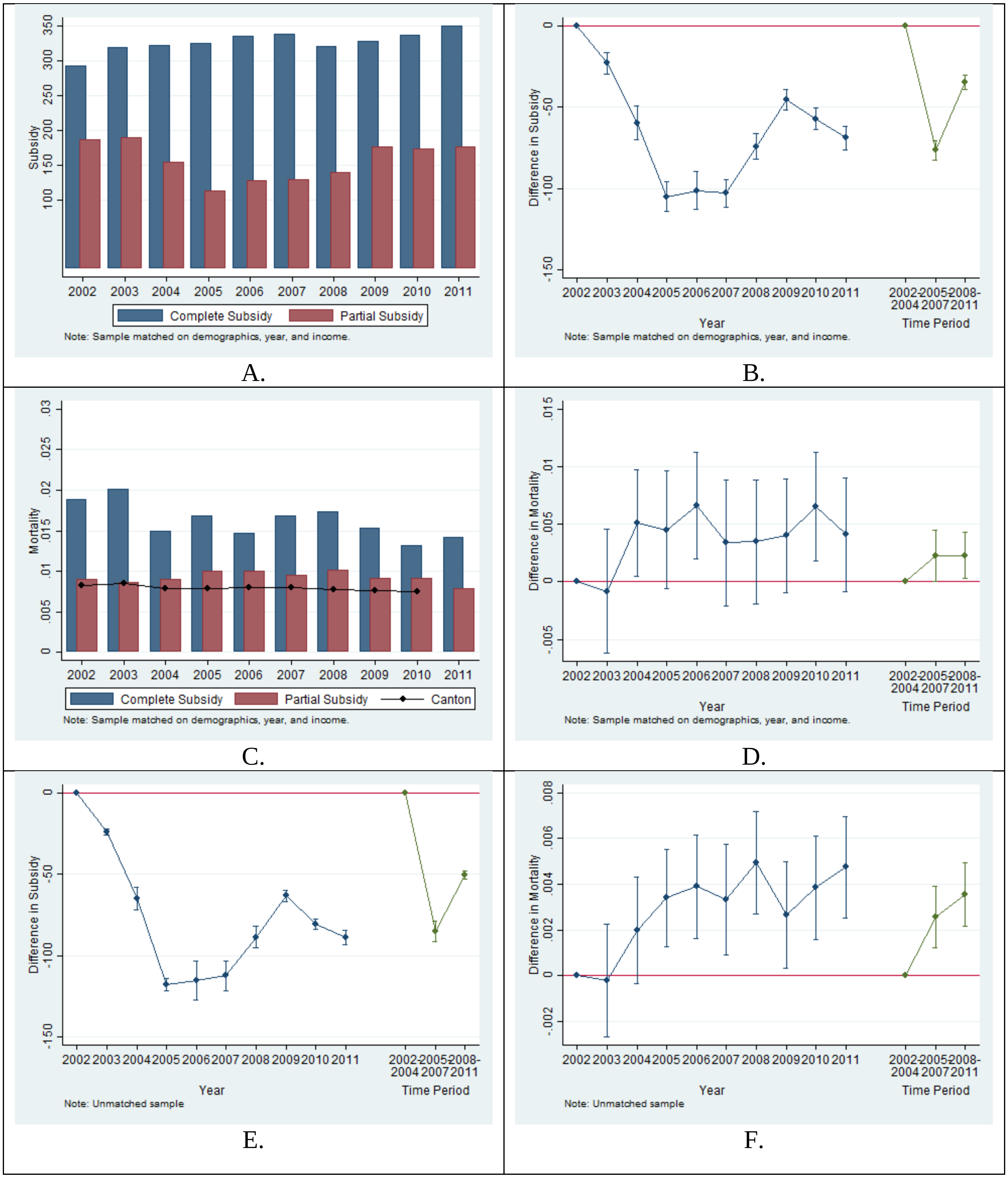


Figure IV: Difference-in-Differences effect of the subsidy program on subsidies and mortality. Panel A is the mean monthly subsidy by year and subsidy program (complete and partial subsidies). Panel B plots difference-in-differences coefficients on interaction terms obtained from a linear regression of the subsidy on year fixed effects, an indicator for being in the partial subsidy program, and their interaction, in addition to the covariates listed in Table I. Panel C plots the trends in mortality rates for complete and partial subsidy programs and the unsubsidized population in Canton Vaud. Panel D provides estimates for mortality from a Probit difference-in-differences analysis. Panels E and F plot difference-in-differences estimates for the subsidy and mortality using the unmatched, unweighted sample.

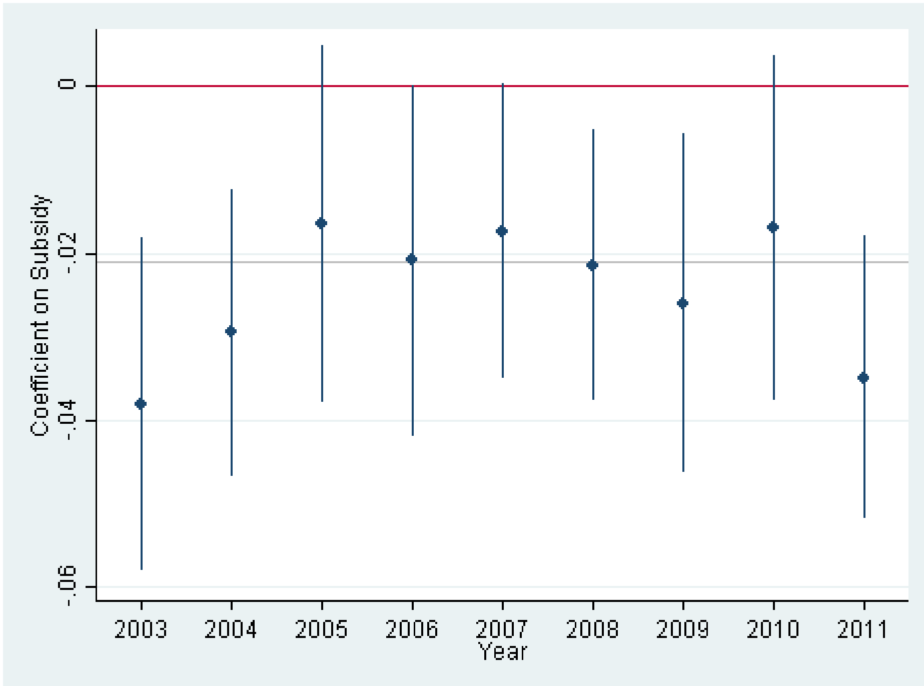


Figure V: Effect of subsidy on mortality by year. Point estimates are the coefficients on the year-by-subsidy interaction from the IV regression. Lines are 95% confidence intervals; gray line is the pooled subsidy coefficient.

Tables

Table I: Summary statistics

	Partial subsidy		Complete subsidy	
	All	Matched	All	Matched
Monthly Subsidy	156.2±96.4	161.7±98.2	343.4±78.6	336.7±74.1
Change in Max. Allowed Subsidy	0.6±26.2			
Lagged Max. Allowed Subsidy	165.9±92.9			
Dies	0.007±0.086	0.013±0.115	0.037±0.188	0.023±0.150
Deductible	670.5±603.1	587.9±553.0	383.6±325.2	438.3±409.5
Premium	321.6±58.4	340.2±57.6	365.2±45.4	356.5±49.4
Net premium	154.4±105.3	168.4±110.4	8.9±19.3	10.9±28.3
Income[1]	27403±11857	25127±12947	13985±12543	24951±12901
Age	46.3±16.2	52.5±19.0	60.1±19.5	52.7±19.1
% Female	0.57±0.50	0.65±0.48	0.61±0.49	0.65±0.48
% Adult, living with family	0.74±0.44	0.67±0.47	0.37±0.48	0.67±0.47
# Children age ≤18	1.02±1.14	0.80±1.11	0.32±0.80	0.69±1.03
Person-years	523321	131560	366441	168461
Individuals	135490	51609	79685	49724
Deaths	3864	1755	13457	6786

Means and standard deviations of study variables for individuals in January of each calendar year. Person-years refer to the total number of January observations in the data. The variable 'Individuals' counts the number of unique individuals in each group, and Deaths is the number of deaths in each group. Matched column indicates sample of individuals who could be coarsely matched between the two samples.

[1] Income is the *revenue determinant*, which is an adjusted measure taking into account number of children and asset holdings.

Table II: Effect of the partial subsidy program on mortality

Dependent var.	(1) Death	(2) Subsidy	(3) Death	(4)	(5)	(6)	(7) French Mortality Prob.	(8)
	Reduced form	First stage	Probit	IV-Probit	OLS	IV	OLS	IV
Change in rule	-0.0294***	1.054***						
	(0.0109)	(0.0282)						
	[0.00926]	[0.0306]						
Change in rule^2 ($\times 100\times 100$)	0.0188	0.100***						
	(0.0217)	(0.0249)						
	[0.00520]	[0.0355]						
Lagged rule	-0.0155	0.885***						
	(0.0132)	(0.0443)						
	[0.0177]	[0.0638]						
Lagged rule^2 ($\times 100\times 100$)	-0.00104	0.0183*						
	(0.00268)	(0.0108)						
	[0.00277]	[0.0156]						
Subsidy			0.00488	-0.0158**	0.00421	-0.0211**	0.000218	0.00500***
			(0.00378)	(0.00625)	(0.00620)	(0.00825)	(0.000438)	(0.000816)
			[0.00445]	[0.00657]	[0.00704]	[0.0110]	[0.000578]	[0.00142]
R-squared	0.065	0.744			0.065	0.065	0.895	0.895
Hansen's J						3.1		62.4
P-value						0.369		0.000

* p<0.1, ** p<0.05, *** p<0.01

Sample size is 460,738 in each column; the dependent variable in columns (1) and (3)-(6) is eleven month mortality (mean 0.00735), representing 3,387 deaths; the dependent variable in column (2) is the monthly subsidy in the first stage regression; the dependent variable in columns (7) and (8) is the average age-gender-year matched mortality rate from Franche-Comté and Rhône-Alpes (mean 0.0095). Coefficients are point estimates for linear models, average marginal effects for Probit and IV-Probit models; standard errors clustered on income bins in parentheses and standard errors allowing for correlation among individuals within 3,000 CHF by income in square brackets. All estimates except for the first stage regression in column (1) have been multiplied by 1,000 for clarity (in addition to the multiplication by 100 for the squared instruments).

Table III: Difference-in-Differences results

	Weighted		Unweighted	
	(1)	(2)	(3)	(4)
	Subsidy	11 Month Mortality	Subsidy	11 Month Mortality
Partial subsidy by				
2005-2007	-76.5***	0.00228**	-85.3***	0.00256***
	(3.1)	(0.00114)	(3.1)	(0.000686)
2008-2011	[8.2]	[0.000967]	[7.8]	[0.000610]
	-34.9***	0.00230**	-50.7***	0.00357***
	(2.2)	(0.00101)	(1.2)	(0.000705)
	[7.9]	[0.00187]	[5.9]	[0.000663]
R-squared	0.636	0.280	0.716	0.265
Partial subsidy mean	158.5	0.00928	156.2	0.00739
Complete subsidy mean	325.4	0.0165	343.4	0.0367
P-values from F-test				
Cluster Robust	0.000	0.045	0.000	0.000
Income Correlated	0.000	0.019	0.000	0.000
IV estimation with difference-in-differences instruments				
Subsidy		-0.0365**		-0.0479***
		(0.0167)		(0.0130)
		[0.0129]		[0.0118]
R-squared		0.079		0.086

* p<0.1, ** p<0.05, *** p<0.01

There are 393355 observations in the matched partial subsidy group, 336765 in the matched complete subsidy group, 523321 in the unmatched partial subsidy group, and 366,441 in the unmatched complete subsidy group. Standard errors, clustered on income bin, in parentheses; standard errors allowing for cross-income bin correlations out to 3,000 CHF in square brackets.

Table IV: Effect of partial subsidies on characteristics of insurance contracts

	(1) Deductible	(2) Gross Premium	(3) Net Premium
Effect of Subsidy (N=460738)			
OLS	-1.648***	0.157***	-0.230***
	(0.142)	(0.00981)	(0.0709)
	[0.180]	[0.0128]	[0.0826]
R-squared	0.156	0.235	0.716
IV	-0.141**	0.0301***	-0.990
	(0.0593)	(0.00432)	(0.00635)
	[0.122]	[0.00895]	[0.0149]
R-squared	0.139	0.221	0.569
Weighted Difference-in-Differences (N=490151)			
Partial subsidy by			
2005-2007	52.78***	-4.887***	78.35***
	(8.923)	(1.003)	(5.525)
	[9.637]	[1.795]	[9.947]
2008-2011	58.90***	-9.441***	26.27***
	(12.92)	(1.256)	(3.071)
	[14.22]	[1.977]	[7.609]
R-squared	0.159	0.284	0.687
Mean	503.9	349.3	80.0

* p<0.1, ** p<0.05, *** p<0.01

Standard errors clustered on income bins in parentheses; standard errors allowing for cross-income bin correlations up to 3,000 CHF in square brackets. Null hypothesis for the net premium for the effect of the subsidy is -1, rather than 0.

Table V: Heterogeneous effects of changes in partial subsidy rules on mortality by lagged deductible

	All		Low Income		High Income	
	(1)	(2)	(3)	(4)	(5)	(6)
Subsidy	-0.0243***	-0.0204**	-0.0338***	-0.0279***	-0.0518***	-0.0534***
	(0.00759)	(0.00946)	(0.00901)	(0.0100)	(0.0135)	(0.0130)
	[0.0107]	[0.0129]	[0.0171]	[0.0174]	[0.0112]	[0.0118]
-by-400-600CHF		0.00369		0.00106		0.0149**
Lagged Deductible		(0.00382)		(0.00531)		(0.00664)
		[0.00374]		[0.00488]		[0.00499]
-by-1000-1500CHF		-0.00852		-0.00607		0.0000191
Lagged Deductible		(0.00882)		(0.00882)		(0.00772)
		[0.0107]		[0.01000]		[0.0106]
-by-2000-2500CHF		-0.0153		-0.0198*		0.00207
Lagged Deductible		(0.0104)		(0.0108)		(0.0107)
		[0.0124]		[0.0131]		[0.0117]
400-600CHF	-0.391	-0.959	0.164	-0.0604	-0.713	-2.274**
Lagged Deductible	(0.318)	(0.847)	(0.301)	(1.273)	(0.538)	(0.925)
	[0.316]	[0.780]	[0.382]	[1.231]	[0.398]	[0.788]
1000-1500CHF	-1.327***	0.0530	-1.161***	0.192	-1.760***	-1.764*
Lagged Deductible	(0.258)	(1.515)	(0.370)	(1.841)	(0.419)	(0.971)
	[0.283]	[1.892]	[0.295]	[2.123]	[0.423]	[1.388]
2000-2500CHF	-2.008***	0.371	-2.678***	1.503	-2.063***	-2.268*
Lagged Deductible	(0.330)	(1.786)	(0.406)	(2.270)	(0.549)	(1.217)
	[0.357]	[2.149]	[0.636]	[2.796]	[0.440]	[1.377]
R-squared	0.064	0.064	0.087	0.087	0.056	0.056
Joint p-value of subsidy interactions						
Cluster-Robust		0.045		0.002		0.131
Income-correlated		0.007		0.000		0.000
Joint p-value of lagged deductibles						
Cluster-Robust	0.000	0.410	0.000	0.363	0.000	0.070
Income-correlated	0.000	0.075	0.000	0.012	0.000	0.008

* p<0.1, ** p<0.05, *** p<0.01

Income groups are split around median income for each subsidy group. Median income equals 22,222.22 for individuals living alone and 31,717.2 for individuals living with a family. Sample in columns (1) and (2) includes 418,391 observations, of which 0.007% die; columns (3) and (4) include 209,306 observations, of which 0.005% die; and columns (5) and (6) includes 209,085 observations, of which 0.010% die. Standard errors clustered on income bins in parentheses; standard errors, allowing for cross-income bin correlations up to 3,000 CHF in square brackets.

Table VI: Heterogeneous effects of partial subsidy on deductibles and net insurance premiums by lagged deductible

	Deductible			Net Insurance Premium		
	(1) All	(2) Low Income	(3) High Income	(4) All	(5) Low Income	(6) High Income
Panel A: No Subsidy-by-Deductible Interactions						
Subsidy	-0.0713***	-0.105***	-0.0284	-0.977***	-0.976***	-0.981***
	(0.0272)	(0.0382)	(0.0464)	(0.00332)	(0.00551)	(0.00519)
	[0.0423]	[0.0343]	[0.0626]	[0.00504]	[0.00558]	[0.00475]
R-squared	0.739	0.730	0.748	0.856	0.766	0.827
Panel B: With Subsidy-by-Deductible Interactions						
Subsidy	0.0246	-0.0286	0.0587	-0.984***	-0.986***	-0.985***
	(0.0251)	(0.0390)	(0.0442)	(0.00330)	(0.00534)	(0.00534)
	[0.0469]	[0.0517]	[0.0629]	[0.00549]	[0.00481]	[0.00551]
-by-400-600CHF	0.0376***	0.0840***	0.0521***	-0.00858***	0.00188	-0.0106***
Lagged Deductible	(0.0113)	(0.0254)	(0.0193)	(0.00243)	(0.00418)	(0.00316)
	[0.0111]	[0.0133]	[0.0211]	[0.00428]	[0.00413]	[0.00601]
-by-1000-1500CHF	-0.270***	-0.184***	-0.317***	0.0216***	0.0185***	0.00946**
Lagged Deductible	(0.0275)	(0.0483)	(0.0402)	(0.00245)	(0.00471)	(0.00399)
	[0.0372]	[0.0877]	[0.0520]	[0.00530]	[0.00842]	[0.00701]
-by-2000-2500CHF	-0.544***	-0.272**	-0.490***	0.0460***	0.0375***	0.0350***
Lagged Deductible	(0.0541)	(0.134)	(0.0829)	(0.00494)	(0.0107)	(0.00734)
	[0.0775]	[0.192]	[0.115]	[0.00745]	[0.0190]	[0.00746]
R-squared	0.740	0.731	0.748	0.857	0.767	0.827
Mean	672.9	649.3	696.5	163.5	108.1	219.0
Joint p-value of subsidy interactions						
Cluster-Robust	0.000	0.000	0.000	0.000	0.000	0.000
Income-correlated	0.000	0.000	0.000	0.000	0.071	0.000

* p<0.1, ** p<0.05, *** p<0.01

Dependent variable in columns (1) through (3) is the current year deductible; in columns (4) through (6), the current year net insurance premium. All models include lagged deductibles and all controls from Table II. Sample in columns (1) and (4) corresponds to sample from columns (1) and (2) of Table V; (2) and (5) correspond to columns (3) and (4) of Table V; and (3) and (6) correspond to columns (5) and (6) of Table V. Standard errors clustered on income bins in parentheses; standard errors allowing for cross-income bin correlations up to 3,000 CHF in square brackets.

Table VII: Robustness checks for sensitivity of partial subsidy effects to attrition bias

	(1) Base	(2) No attrit, 2005	(3) Before 2005	(4) 2005 or later	(5) Never attrit
Effect of Subsidy					
IV-Probit	-0.0158**	-0.0172***	-0.0343**	-0.0157***	-0.0174
	(0.00625)	(0.00652)	(0.0154)	(0.00599)	(0.0113)
	[0.00657]	[0.00625]	[0.00926]	[0.00618]	[0.0112]
IV	-0.0211**	-0.0222***	-0.0355**	-0.0234***	-0.0330**
	(0.00825)	(0.00797)	(0.0156)	(0.00795)	(0.0149)
	[0.0110]	[0.00978]	[0.0143]	[0.00914]	[0.0184]
R-squared	0.065	0.067	0.060	0.068	0.097
N	460738	419521	122979	337759	248117
Mean	0.007	0.008	0.007	0.007	0.013
# Deaths	3387	3252	871	2516	3195
Weighted Difference-in-Differences					
Partial subsidy by					
2005-2007	0.00201*	0.00212			0.00206
	(0.00164)	(0.00176)			(0.00206)
	[0.00122]	[0.00122]			[0.00142]
2008-2011	0.00414**	0.00453**			0.00400
	(0.00128)	(0.00141)			(0.00166)
	[0.000936]	[0.000994]			[0.000938]
R-squared	0.265	0.258			0.250
Partial subsidy group mean	0.0134	0.0139			0.0180
Complete subsidy group mean	0.0231	0.0252			0.0279
P-value from F-test	0.000	0.000			0.002

* p<0.1, ** p<0.05, *** p<0.01

Standard errors clustered on income bin in parentheses; standard errors allowing for cross-income bin correlations out to 3,000 CHF in square brackets. IV-Probit point estimates and standard errors are for average marginal effects. Point estimates and standard errors for IV and IV-Probit are multiplied by 1,000.

Appendix

S1. Functional Form, Sample Selection, and Duration of Follow-Up
S.2 Alternative Identification Strategies
S.3 Attrition
S.4 Trends in Population Mortality
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Table S1: Allowed deductibles by year
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Table S3: Regression and IV estimates of the relationship between mortality (per 1,000) and study covariates, and first stage of instrumental variables model
Table S4: Entry into and attrition from partial subsidy program each year
Table S5: Characteristics of compliers
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Table S8: Robustness of IV estimates to subsets of instruments
Table S9: Sensitivity of the effect of the partial subsidy program on mortality to alternative samples

Formula for the Maximum Allowed Subsidy

The formula for the Maximum Allowed Subsidy in the partial subsidy group is:

$$(1) \qquad \zeta(y_{t-3}, R_t) = \begin{cases} F_t & \text{if} \quad y_{t-3} \le C_t \\ E_t + \left\{ (F_t - E_t)[1 - (\frac{y_{t-3} - C_t}{A_t - C_t})^2]^{P_t} \right\} & \text{if} \quad C_t < y_{t-3} \le A_t \\ 0 & \text{if} \quad y_{t-3} > A_t \end{cases}$$

Where:

- y_{t-3} is income (measured three years previously),
- R_t is the vector of subsidy parameters (A_t, C_t, E_t, F_t, and P_t) for year *t* that depend on household and individual characteristics (e.g. there is a different R_t for students who live alone under age 26 than for individuals living with a family and aged 26 or older),
- F_t is the highest subsidy potentially received by all those with income lower than the level C_t,
- E_t is the lowest subsidy,
- A_t is the income threshold above which no subsidy is allowed,
- C_t is the income threshold above which the subsidy starts to decrease until the level at which E_t applies, and
- P_t is the coefficient of progressivity, which governs the rate at which the subsidy decreases with increases in income.

Inference

The subsidy policy that we study depends on individual characteristics including income, which is a strong correlate of mortality. As a result, we are concerned that individuals with similar incomes are likely to have similar unobserved mortality shocks, while individuals whose incomes are far apart are likely to have uncorrelated mortality shocks. Our first solution to this problem is to estimate cluster-robust covariance matrices, following Bertrand, Duflo, and Mullanaithan (2004), in which our clusters are 100 CHF wide income bins (excluding 0, which is treated as its own cluster).

The cluster-robust covariance matrix assumes that any correlation in mortality rapidly fades with differences in income (i.e. there is no correlation with someone whose income differs from yours by 101 CHF). As an alternative, we also estimate covariance matrices that allow for correlations between income bins. The approach is drawn from spatial econometrics and is a one-dimensional analogue to Conley's method for estimating spatial covariance matrices (Conley, 1999). Specifically, consider the conventional cluster-robust covariance matrix:

$$V = A^{-1}(\sum_{c \in C} X_c' \mu_c \mu_c{}' X_c) A^{-1},$$

where the matrix *A* is either the Hessian matrix for maximum likelihood models or the appropriate cross-product for linear models, *c* is the index of the cluster (indexed by income), X_c is the matrix of covariates, and μ_c is the matrix of residuals or scores. We replace the expression in parentheses with the expression c ∈ Cj|abs(j-c) ≤ wcj|abs(j-c) ≤ wcj|abs(j-c) ≤ w

$X_c' \mu_c{}' \mu_j X_j X_c' \mu_c{}' \mu_j X_j X_c' \mu_c \mu_c' X_c \sum_c \sum_{j|abs(j\text{-}c) \leq w} X_c' \mu_c{}' \mu_j X_j$, where *w* is a bandwidth parameter. For example, the contribution to the covariance matrix from individuals in the 2,000 to 2,100 CHF bin will include both the within-bin correlation and also the correlation between that bin and neighboring bins, up to w away. In our application we use a bandwidth of 3,000 CHF, since this value tends to yield the most conservative standard errors.

Robustness checks

S1. Functional Form, Sample Selection, and Duration of Follow-Up

We report results for a number of alternative model specifications, which include IV models with non-linear controls for age, income and interactions with gender and subsidy groups. These additional controls have no effects on our point estimates, but reduce precision (Table S6). Estimates are robust to including commune-by-year fixed effects, as well as year-by-income interactions, suggesting that changes in the subsidy rule are independent of other temporal changes influencing mortality holds. Accounting for selection into the partial rather than complete subsidy group using Heckman sample selection correction provides identical results (Table S7). Our results persist up to 23 months, but are non-significant after 35 months, even though the marginal effect of the subsidy remains around -.02 in all cases (columns (11) to (13) of Table S6).

The duration for which individuals are observed in the sample may be associated with their state of health and changes in the subsidy rule. Hence, in columns (14) to (16) of Table S6 we restrict the sample to individuals who have been in the database for one, two, or three years only. In all cases, our main results persist, although some are no longer statistically significant.

S.2 Alternative Identification Strategies

We explored the sensitivity of our IV estimates to choice of identifying instruments (Table S8). Using just the change in the subsidy rule, we find little change in the reduced form relationship between our instruments and mortality (Panel A, column (2)). However, there is significant attenuation of the first-stage relationship (Panel A, column (1)) between our instruments and the subsidy, which is consistent with a failure of the monotonicity assumption since the same level of the instrument now corresponds to both very high and very low subsidy levels. As a result, our IV estimates (Panel B, columns (1) and (2)) are 50% larger than in our base specification. Using just the lagged subsidy instrument yields a non-significant reduced form relationship (Panel A, column (4)) and similar first-stage regression coefficients as in our base specification. As a result, we find non-significant effects of the subsidy on mortality that are 20-25% smaller than our base specification.

We also considered two alternative identification strategies. First, following Gruber and Saez (2002), we constructed a counterfactual income estimate for each individual based on trending forward lagged income at the rate of inflation and using this alternative income estimate to construct our instruments. When we estimated models using these instruments, we used the counterfactual, rather than actual, income among our control variables. The resulting point estimates are similar to our base specification (Panels A and B, columns (5) and (6)), but are estimated less precisely, with the consequence that that only the IV-Probit marginal effect can be distinguished from zero at the ten percent level. However, the marginal effect from the IV-Probit specification implies that a 100 CHF reduction in the monthly subsidy increases the probability of dying during the year by 0.14 percentage points, versus 0.16 percentage points using our base specification. We also constructed simulated instruments (Currie and Gruber, 1996; Cutler and Gruber, 1996) in which we computed an individual's subsidy in each year and then computed year-by-age group-by-gender-by-family status means for the change in the subsidy rule, lagged subsidy rule and income, which we used in place of actual income in our estimates (Panels A and B, columns (7) and (8)). The IV-LPM results using simulated instruments are implausibly large, implying that a sustained 100 CHF increase in the monthly subsidy reduces the probability of dying over the subsequent eleven months by 1%, while the IV-Probit results are roughly half the size of the results in Table II. In no cases do the alternative identification strategies yield a positive effect of the subsidy on mortality, suggesting that our estimates reflect a real effect of subsidies on reducing the risk of death.

S.3 Attrition

Attrition bias could explain our results if attrition is negatively correlated with subsidy levels and individuals who leave the sample group are significantly healthier even after conditioning on observable characteristics. Estimates produced by including all individuals above our income eligibility threshold (i.e. eligible given current year rules) are still negative and significant, but smaller than for the more restricted sample, which is consistent with the fact that ineligible individuals are wealthier and healthier (Table S6 column 2).

We discuss the main variations on our IV and DID estimates obtained using different subsamples to address the potential confounding effect of attrition on our estimates in the main text, with reference to Table VII and Figure V.

As previously explained in Section VIII, we deal with missing incomes for individuals who attrited, but were found alive in the SESAM ("filled-in" samples), using two methods denoted as the "carry-forward" sample and "first-observed" sample. Table S9 reports results using the "carry-forward" sample, in which we assign missing income data using the last observed income, and the "first-observed" sample, in which we use the first observation for each individual to construct our subsidy instrument. With one exception, all analyses using these two alternative samples are comparable in size and statistical significance to our main specifications. The exception arises when using the first observed income, for which the IV-LPM and IV-Probit models yield smaller and non-significant estimates of a 0.09 and 0.12 percentage point increase in mortality following a 100 CHF reduction in the subsidy, respectively.

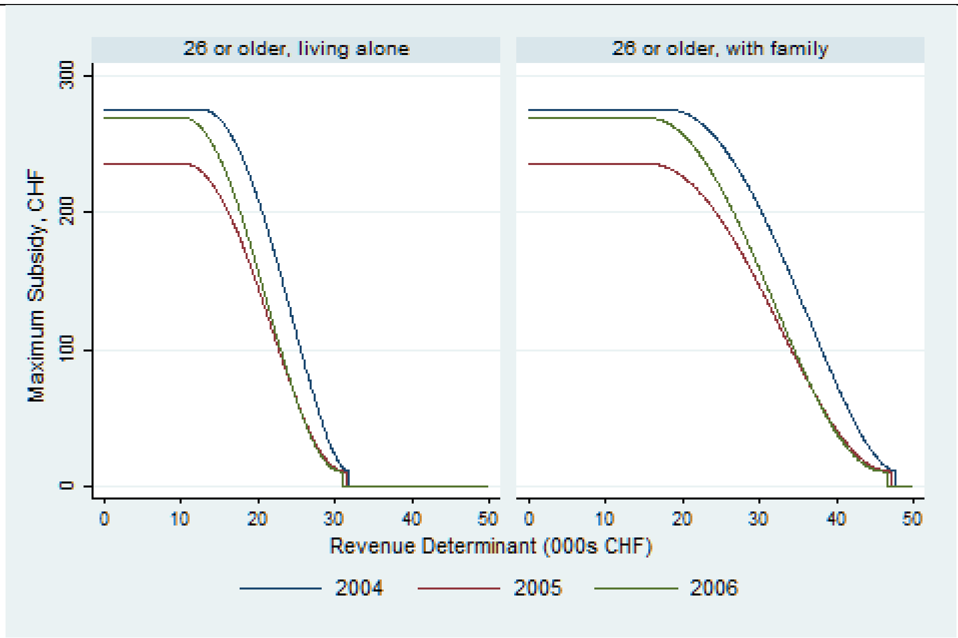
S.4 Trends in Population Mortality

To study differences in causes of death over time, we compare all-cause and cause-specific mortality rates in the population between Vaud and the two bordering French regions of Franche-Comté and Rhône Alpes. France currently and over the period covered by our analysis relies on a stable and comprehensive social health insurance system paid for through taxation. Coverage is universal and compulsory, is provided to all residents by noncompetitive statutory health insurance funds, and experienced no major policy changes between 1997 and 2011 (Mossialos et al., 2015). Mortality data were obtained from the Swiss Office of Federal Statistics for Switzerland and from Eurostat for France.

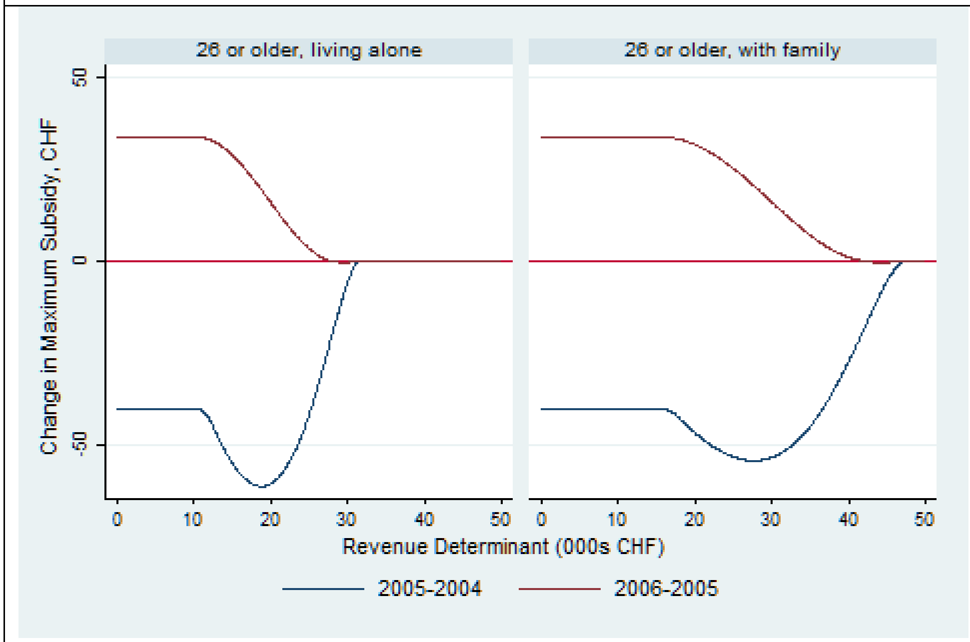
As reported in Section VII.B of the main text, mortality trends (all causes of death) between the two regions moved broadly in parallel in the late 1990s and early 2000s, with Vaud converging towards the lower French mortality rate, which suggests the two regions were subject to similar influences on mortality (Figure I panel B, main text). Relative mortality risk was 10.3% [95% CI: 7.3 - 13.4%] higher in Canton Vaud in 1997, but this declined significantly to 3.9% [95% CI: 1 - 6.9%] by 2005. At the same time as the large subsidy cut between 2005 and 2006 and the decline in participation in the partial subsidy program, the gap between the French and Vaud mortality rates widened substantially. Between 2005 and 2006 relative mortality risk for all causes increased by 6.1 percentage points [95% CI: 1.8 - 10.4] to a relative mortality risk of 10.1% [95% CI: 7.9 – 13.2%], corresponding to 330 additional deaths in total. In 2007 relative mortality risk was 8.2% [95% CI: 5.2 – 11.3%] higher in Canton Vaud compared to the French regions, but from 2008 there were increases in the eligibility thresholds for the subsidy (extensive margin) as well as the maximum subsidy threshold and level (intensive margin). By the end of the decade there was no significant difference in mortality risk between France and Canton Vaud, possibly reflecting the expansion of the subsidy program.

Looking at specific causes of death, Figure S2 plots the ratio of relative risks in mortality between Vaud and the French regions over time, indexed to baseline risk levels in the year 1997. These data indicate that the mortality increases observed following the period of subsidy reduction arose for causes of death that are likely to be responsive in the short term to income shocks (so called deaths of despair, see Case and Deaton, 2017): suicides, cardiac and digestive causes of death (for instance alcoholic or drug induced liver failure and ulcers), and maternal and infant mortality all increased markedly in the 2005-2007 period, both relative to the rates in France and compared to the prior trend. This kind of effect on suicides, mental and maternal health is a consistent finding in the literature on the impact of economic hardship on health, and indebtedness has been associated with higher mortality, worse physical health and health behaviors such as alcohol and substance abuse as well as poor nutrition (Evans and Garthwaite, 2014, demonstrate that positive income shocks improve maternal health; see also Stuckler et al., 2015; Turunen and Hiilamo, 2014). Increased stress induced by financial hardship is also associated with heightened risk of myocardial infarction (e.g. Vaccarino et al., 2014).

Figures



A.



B.

Figure S1: Maximum allowed subsidy and change in maximum allowed subsidy in Canton Vaud from 2004 to 2006 (partial subsidy group).

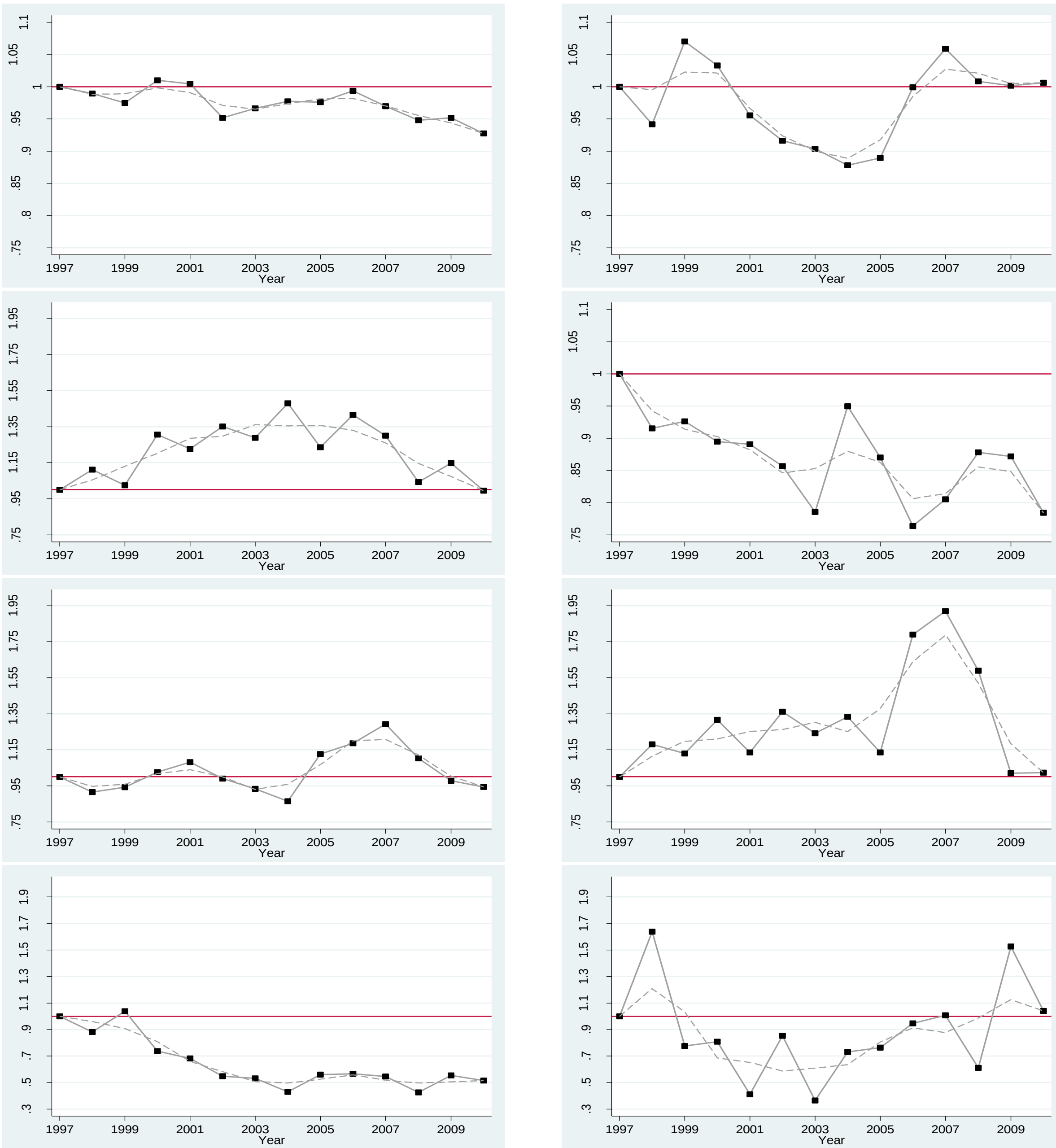


Figure S2: Relative risk of death by cause in Canton Vaud compared to neighboring French regions (Franche-Comté and Rhône-Alpes).

Canton Vaud relative to French regions is depicted by the solid line, while the dashed line is Epanechnikov kernel local mean plot. Mean mortality rate per 10,000 over all periods in Canton Vaud by cause of death is: cancer 20.98; cardiac 19.86; respiratory 6.35; cerebrovascular 6.16; digestive 2.46; suicide 1.68; infectious disease 0.89; and maternal/infant 0.26.

Tables

Table S1: Allowed deductibles by year

Deductible (in CHF)	2002-2003	2004	2005-2011
230	X		
300		X	X
400	X	X	
500			X
600	X	X	
1000			X
1200	X	X	
1500	X	X	X
2000			X
2500			X

Table S2: Changes in the partial subsidy rule from 2002 to 2011 (age >26) in CHF (refere to formula in (1))

	Year	Living alone	Living with family
Lowest subsidy (*E*)	2002	17	17
	2003	10	10
	2004	10	10
	2005	10	10
	2006	10	10
	2007	10	10
	2008	10	10
	2009	10	10
	2010	10	10
	2011	10	20
Highest subsidy (*F*)	2002	245	245
	2003	260	260
	2004	260	260
	2005	225	225
	2006	260	260
	2007	280	280
	2008	290	290
	2009	290	290
	2010	290	290
	2011	290	290
Revenue threshold below which subsidy is at the highest level (*C*)	2002	16000	22000
	2003	16000	22000
	2004	12000	17000
	2005	10000	15000
	2006	10000	15000
	2007	10000	15000
	2008	12000	17000
	2009	17000	19000
	2010	17000	19000
	2011	17000	19000
Revenue threshold above which no subsidy is allocated (*A*)	2002	30000	45000
	2003	30000	45000
	2004	30000	45000
	2005	30000	45000
	2006	30000	45'000
	2007	30000	45000
	2008	32000	46000
	2009	32000	50000
	2010	32500	51000
	2011	32500	65000
Coefficient of progressivity (*P*)	2002	1.2	1.2
	2003	1.3	1.3
	2004	1.8	1.8
	2005	2.2	2.2
	2006	2.3	2.3
	2007	2.3	2.3
	2008	2.3	2.3
	2009	2.3	2.3
	2010	2.3	2.3
	2011	2.3	2.3

Table S3: Regression and IV estimates of the relationship between mortality (per 1,000) and study covariates, and first stage of instrumental variables model

	(1)	(2)	(3)	(4)	(5)
	Probit	IV-Probit	OLS	IV	First-stage
Monthly Subsidy	0.000223	-0.00100***	0.00251	-0.0202***	
	(0.000142)	(0.000331)	(0.00218)	(0.00688)	
Change in Subsidy					1.051***
					(0.00683)
Change in Subsidy^2					0.000997***
					(0.000122)
Allowed Subsidy,					0.882***
Lagged Rules					(0.00741)
Allowed Subsidy^2,					0.000180***
Lagged Rules					(0.0000154)
Income	-0.0000237***	-0.0000207***	-0.000613***	-0.000573***	0.00297***
	(0.00000215)	(0.00000229)	(0.0000465)	(0.0000468)	(0.0000474)
Income^2	2.94e-10***	-7.38e-11	2.90e-09***	-3.46e-09*	-5.13e-08***
	(7.73e-11)	(1.18e-10)	(9.04e-10)	(1.96e-09)	(1.63e-09)
Live with Others	-0.245***	-0.323***	-10.25***	-11.50***	-15.64***
	(0.0521)	(0.0569)	(0.901)	(0.975)	(1.124)
Live with Others-	0.0000064***	0.0000148***	0.000415***	0.000561***	0.000399***
by-Income	(0.00000231)	(0.00000310)	(0.0000440)	(0.0000608)	(0.0000583)
Female	-0.0564	-0.0752	32.08**	32.07**	-1.486
	(0.661)	(0.660)	(13.94)	(13.94)	(5.292)
Age	0.0639***	0.0659***	12.30***	12.33***	1.104***
	(0.0240)	(0.0240)	(0.791)	(0.791)	(0.252)
Female-by-Age	-0.00521	-0.00352	-2.307**	-2.295**	0.534*
	(0.0333)	(0.0332)	(0.931)	(0.930)	(0.303)
Age^2	-0.000746*	-0.000756*	-0.271***	-0.271***	-0.00393
	(0.000393)	(0.000393)	(0.0165)	(0.0165)	(0.00461)
Female-by-Age^2	0.0000441	0.00000882	0.0553***	0.0550***	-0.0133**
	(0.000533)	(0.000531)	(0.0194)	(0.0194)	(0.00550)
Age^3	0.0000055***	0.0000054***	0.00194***	0.00193***	-0.0000296
	(0.00000206)	(0.00000206)	(0.000107)	(0.000107)	(0.0000268)
Female-by-Age^3	-0.000000299	-8.94e-08	-0.000449***	-0.000447***	0.0000837***
	(0.00000273)	(0.00000272)	(0.000126)	(0.000126)	(0.0000318)
Year					
2003	0.0673*	0.0915**	0.700	1.076**	-2.252***
	(0.0364)	(0.0367)	(0.514)	(0.521)	(0.375)
2004	0.0506	0.0253	0.497	0.0408	1.369***
	(0.0381)	(0.0385)	(0.526)	(0.543)	(0.528)
2005	0.0669*	-0.00370	0.663	-0.683	2.315***
	(0.0403)	(0.0437)	(0.567)	(0.692)	(0.478)
2006	0.0909**	0.0313	1.215**	0.142	-1.468***
	(0.0393)	(0.0418)	(0.562)	(0.641)	(0.436)
2007	0.0505	-0.00517	0.703	-0.266	-1.903***
	(0.0397)	(0.0419)	(0.563)	(0.626)	(0.438)
2008	0.0847**	0.0404	1.164**	0.346	-7.519***
	(0.0398)	(0.0413)	(0.583)	(0.633)	(0.507)
2009	0.0530	0.0496	0.888*	0.791	-6.780***
	(0.0351)	(0.0351)	(0.526)	(0.527)	(0.503)
2010	0.0486	0.0425	0.859	0.739	-4.396***
	(0.0357)	(0.0357)	(0.528)	(0.530)	(0.352)
Region (2003-2008)					
1(ref)					

2	0.0257	0.0246	0.401	0.393	-0.382*
	(0.0223)	(0.0222)	(0.328)	(0.328)	(0.212)
3	0.0698***	0.0675***	1.055***	1.037**	-0.411*
	(0.0238)	(0.0238)	(0.406)	(0.406)	(0.229)
Region (2009-2011)					
1(ref)					
2	0.0425	0.0426	0.428	0.416	-0.845***
	(0.0291)	(0.0290)	(0.489)	(0.489)	(0.306)
# Children	-0.0840***	-0.0769***	-0.704***	-0.590***	5.174***
	(0.0184)	(0.0185)	(0.111)	(0.117)	(0.0965)
Constant	-4.428***	-4.165***	-166.7***	-161.6***	-76.57***
	(0.465)	(0.466)	(11.77)	(11.85)	(4.595)

* p<0.1, ** p<0.05, *** p<0.01

Standard errors clustered on income bins in parentheses. Full results for model reported in Table II columns 3-6.

Table S4: Entry into and attrition from partial subsidy program each year

	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	Total	Excl. 2011
New entrants this period	62583	13592	10320	9182	9922	10294	8473	10839	9126	12081	156412	144331
	(1.00)	(0.22)	(0.17)	(0.18)	(0.19)	(0.21)	(0.18)	(0.23)	(0.20)	(0.26)	(0.30)	(0.30)
	{0.008}	{0.008}	{0.005}	{0.005}	{0.008}	{0.007}	{0.005}	{0.006}	{0.005}			{0.007}
Continue to next period	47121	51946	41998	43083	39278	37706	36056	35588	34133	45920	412829	366909
	(0.75)	(0.86)	(0.67)	(0.84)	(0.74)	(0.76)	(0.78)	(0.76)	(0.76)	(0.99)	(0.79)	(0.77)
	{0.008}	{0.007}	{0.008}	{0.008}	{0.008}	{0.008}	{0.008}	{0.008}	{0.007}			{0.008}
Attrit, not seen again	832	948	1204	1099	991	971	792	691	743		8271	8271
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)		(0.02)	(0.02)
Attrit, found alive	308	458	496	514	297	267	188	187	79		2794	2794
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)		(0.01)	(0.01)
Die	477	438	433	373	408	375	379	356	331	294	3864	3570
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Switch to complete, eligible	2129	2595	2590	2363	2441	2279	1956	2312	2136		20801	20801
	(0.03)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)		(0.04)	(0.04)
	{0.040}	{0.036}	{0.021}	{0.027}	{0.027}	{0.030}	{0.035}	{0.030}	{0.031}			{0.031}
Switch to complete, ineligible	296	233	225	261	299	228	212	261	245		2260	2260
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)		(0.00)	(0.00)
	{0.037}	{0.034}	{0.031}	{0.015}	{0.033}	{0.022}	{0.042}	{0.023}	{0.020}			{0.029}
Ineligible next period	11420	4095	15320	3487	9291	7746	6596	7500	7047		72502	72502
	(0.18)	(0.07)	(0.25)	(0.07)	(0.18)	(0.16)	(0.14)	(0.16)	(0.16)		(0.14)	(0.15)
	{0.002}	{0.001}	{0.003}	{0.001}	{0.001}	{0.001}	{0.001}	{0.002}	{0.002}			{0.002}
Total	62583	60713	62266	51180	53005	49572	46179	46895	44714	46214	523321	477107

Numbers in parentheses are the column percentages for the row categories; numbers in curly brackets are the mortality rates in the next year. The last column is the total for all years except 2011.

Table S5: Characteristics of compliers

	Population mean	Change in Subsidy Rules: Likelihood of complying	Change in Subsidy Rules: Complier mean	Lagged Subsidy Rules: Likelihood of complying	Lagged Subsidy Rules: Complier mean
% Female	0.568	0.998	0.567	1.000	0.568
% Live with family	0.743	0.716***	0.532	1.017	0.756
% Age>Median	0.484	1.036	0.501	1.036**	0.501
% Income>Median	0.500	0.779	0.389	0.981	0.490
% Mortality Risk>Median	0.497	1.187***	0.590	1.031*	0.513
% Deductible 500 CHF or Over	0.502	0.628***	0.315	1.000	0.502
Gross Premium>Median	0.494	0.879*	0.434	1.039***	0.513
Net Premium>Median	0.500	1.334***	0.667	1.146***	0.573
Age					
26-30	0.135	1.080	0.146	0.914***	0.123
31-35	0.149	0.921	0.137	0.958**	0.143
36-40	0.167	0.976	0.163	0.978	0.164
41-45	0.152	0.721***	0.109	1.023	0.155
46-50	0.103	0.491***	0.051	1.026	0.106
51-55	0.063	0.782*	0.049	1.001	0.063
56-60	0.046	1.086	0.050	0.998	0.046
61-65	0.038	1.283	0.049	1.036	0.040
66-70	0.029	1.555***	0.046	1.090*	0.032
70-79	0.062	1.741***	0.109	1.077*	0.067
80 or over	0.060	1.673***	0.101	1.071	0.065
Lagged Deductible					
230/300 CHF	0.447	1.112**	0.497	0.993	0.444
400-600 CHF	0.334	1.382***	0.462	1.004	0.336
1000-1500 CHF	0.165	0.430***	0.071	1.022	0.169
2000-2500 CHF	0.054	-0.396***	-0.021	0.963	0.052

* p<0.1, ** p<0.05, *** p<0.01

Likelihood is ratio of the first stage coefficient on the instrument conditional on the row variable to the unconditional first stage coefficient. Subsidy and instruments are dummies for having a subsidy or instrument greater than the median for one's subsidy group and year. All controls from Table II were partialled out of the subsidy and instrument dummies using the full sample. Complier mean is the product of likelihood and the population mean.

Table S6: Robustness checks for model specification (IV)

	(1)	(2)	(3)	(4)	(5)	(6)
	Base	Income eligible, current year	(1) + household size-by-year	(3) + income-by-year	(4) + household size-by-income squared	(5) + income squared-by-year
Subsidy	-0.0211**	-0.00949**	-0.0231***	-0.0324***	-0.0175*	-0.0228**
	(0.00825)	(0.00457)	(0.00818)	(0.00830)	(0.0102)	(0.0104)
	[0.0110]	[0.00706]	[0.0108]	[0.0110]	[0.0107]	[0.0102]
R-squared	0.065	0.063	0.065	0.065	0.065	0.065
N	460738	488366	460738	460738	460738	460738
Mean	0.007	0.007	0.007	0.007	0.007	0.007
# Deaths	3387	3577	3387	3387	3387	3387

	(7)	(8)	(9)	(10)	(11)	(12)
	(6) + income-by-household size-by-year	(1) + person fixed effects	Exclude "in-year" fills	Commune-by-year FE	23 month mortality	35 month mortality
Subsidy	-0.0197*	-0.0275***	-0.0264***	-0.0211***	-0.0222**	-0.0243
	(0.0104)	(0.00705)	(0.00754)	(0.00593)	(0.0107)	(0.0150)
	[0.00900]		[0.0109]		[0.0149]	[0.0189]
R-squared	0.065	0.035	0.064	0.064	0.134	0.194
N	460738	460738	431821	460738	407409	344376
Mean	0.007	0.007	0.008	0.007	0.016	0.026
# Deaths	3387	3387	3245	3387	4359	4807

	(13)	(14)	(15)	(16)
	47 month mortality	12 months w/ subsidy	24 months w/ subsidy	36 months w/ subsidy
Subsidy	-0.0140	-0.0183	-0.0232*	-0.0201
	(0.0206)	(0.0181)	(0.0138)	(0.0124)
	[0.0277]	[0.0178]	[0.0143]	[0.0141]
R-squared	0.249	0.076	0.073	0.070
N	273248	138435	213702	269098
Mean	0.038	0.006	0.007	0.007
# Deaths	4972	884	1397	1799

* p<0.1, ** p<0.05, *** p<0.01

Standard errors in parentheses, clustered on income bin, except for columns (8) and (10) which are clustered on person (8) or commune-year (10); standard errors, allowing for cross-income bin correlations up to 3,000 CHF, in square brackets. The dependent variable is eleven month mortality, except in columns (11) to (13), which instead use the indicated durations for mortality. All point estimates and standard errors are multiplied by 1,000.

Table S7: Heckman-selection corrected estimates of effect of partial subsidies on mortality

	Mortality				Falsification			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	IV	Heckman-OLS	Heckman-IV	OLS	IV	Heckman-OLS	Heckman-IV
Subsidy	0.003	-0.020***	0.003	-0.020***	0.001***	0.005***	0.001***	0.005***
	(0.002)	(0.007)	(0.002)	(0.007)	(0.000)	(0.001)	(0.000)	(0.001)

* p<0.1, ** p<0.05, *** p<0.01

Standard errors clustered on income bins in parentheses; coefficients have all been multiplied by 1,000 for clarity.

Table S8: Robustness of IV estimates to subsets of instruments

	Change in subsidy instruments only		Lagged subsidy instruments only		Lagged income instruments		Simulated instruments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	First stage	Reduced form	First stage	Reduced form	First stage	Reduced form	First stage	Reduced form
Panel A: First-stage and reduced-form regressions								
Change in rule	0.411***	-0.0161*			0.912***	-0.0130	0.747***	0.0257
	(0.0139)	(0.00923)			(0.0270)	(0.0129)	(0.0953)	(0.106)
	[0.0531]	[0.00608]			[0.120]	[0.0111]	[0.305]	[0.103]
Change in rule^2	0.00228***	0.000189			-0.000277	0.000354	0.00176	0.00197
	(0.000385)	(0.000183)			(0.000309)	(0.000265)	(0.00118)	(0.00152)
	[0.00181]	[0.000181]			[0.00141]	[0.000141]	[0.00186]	[0.00138]
Lagged rule			0.708***	-0.00489	0.707***	-0.00535	1.032***	-0.152**
			(0.0310)	(0.0110)	(0.0231)	(0.0126)	(0.0957)	(0.0756)
			[0.0782]	[0.0177]	[0.104]	[0.0109]	[0.416]	[0.0765]
Lagged rule^2			-0.0000196	-0.0000189	0.0000203	-0.00000883	-0.00134***	0.000423*
			(0.0000519)	(0.0000223)	(0.000102)	(0.0000317)	(0.000460)	(0.000252)
			[0.000207]	[0.0000322]	[0.000423]	[0.0000207]	[0.000997]	[0.000267]
Panel B: Second-stage regressions								
	IV-LPM	IV-probit	IV-LPM	IV-probit	IV-LPM	IV-probit	IV-LPM	IV-probit
Subsidy	-0.035	-0.022	-0.016	-0.015*	-0.011	-0.014*	-0.104	-0.011
	(0.023)	(0.020)	(0.010)	(0.008)	(0.009)	(0.007)	(0.064)	(0.038)
	[0.019]	[0.018]	[0.014]	[0.007]	[0.009]	[0.007]	[0.044]	[0.032]
N	460738	456026	460738	456026	399661	394925	460738	432231
Mean	0.007	0.007	0.007	0.007	0.007	0.007	0.007	0.008
# Deaths	3387	3387	3387	3387	2961	2961	3387	3387

* p<0.05, ** p<0.01, *** p<0.001

The dependent variable in odd-numbered columns of panel A is the monthly subsidy, and in even-numbered columns of panel A and all columns of panel B is eleven month mortality. Lagged income instruments are constructed by trending forward lagged income at the inflation rate. Simulated instruments are constructed using the age-gender-subsidy-group specific distribution of income applied to the subsidy rules for each year. Lagged income instruments models include lagged income, rather than current income, as controls. Simulated instruments models included average income in the age-gender-subsidy-group cell, rather than current income, as controls and add fixed effects for each age-gender-subsidy-group combination used in constructing the simulated instrument. Point estimates where the dependent variable is eleven month mortality have been multiplied by 1,000 for clarity. Standard errors clustered on income bins are in parentheses, standard errors allowing for correlation between income bins are in square brackets (see text for details).

Table S9: Sensitivity of the effect of the partial subsidy program on mortality to alternative samples

					Falsification		
	(1) Probit	(2) IV-Probit	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) DID
Base Sample							
Subsidy	0.00488	-0.0158**	0.00421	-0.0211**	0.000218	0.00500***	
	(0.00378)	(0.00625)	(0.00620)	(0.00825)	(0.000438)	(0.000816)	
Partial subsidy by							
2005-2007							0.00228**
							(0.00114)
2008-2011							0.00230**
							(0.00101)
R-squared			0.065	0.065	0.895	0.895	
N	456026	457235	460738	460738	460738	460738	727946
Mean	0.007	0.007	0.007	0.007	0.009	0.009	0.013
P-value from F-test							0.045
Last Observed Income							
Subsidy	0.0146***	-0.0205***	0.0155*	-0.0193***	0.000434*	0.00423***	
	(0.00478)	(0.00584)	(0.00814)	(0.00702)	(0.000232)	(0.000792)	
Partial subsidy by							
2005-2007							0.00177
							(0.00111)
2008-2011							0.00194**
							(0.000922)
R-squared			0.064	0.063	0.895	0.895	
N	486416	487709	491501	491501	491501	491501	761588
Mean	0.007	0.007	0.007	0.007	0.009	0.009	0.012
P-value from F-test							0.091
First Income							
Subsidy	0.0113***	-0.0177	0.0110***	-0.00888	0.0000750	0.00199	
	(0.00198)	(0.0121)	(0.00287)	(0.0153)	(0.000146)	(0.00209)	
Partial subsidy by							
2005-2007							0.000586
							(0.00111)
2008-2011							0.00195**
							(0.000868)
R-squared			0.060	0.059	0.895	0.895	
N	518412	519828	523473	523473	523473	523473	806812
Mean	0.006	0.006	0.006	0.006	0.009	0.009	0.010
P-value from F-test							0.055

* p<0.1, ** p<0.05, *** p<0.01

The dependent variable is eleven month mortality in columns (1) to (4) and (7); matched French mortality rate in columns (5) and (6). Panel A repeats baseline results. Panel B uses a sample that carries the last observed income forward both as income and to construct instruments. The subsidy is assumed to be 0 for observations that were inferred based on an individual being in the

SESAM database before and after that year. Panel C uses income from the first observed year as the income measure and in constructing the instruments. All models include the same controls as in Table II, although the relevant income measure differs across the three panels, as indicated by the panel title. Models in columns (1), (2), and (7) are Probit or IV-Probit models; remaining models are OLS and TSLS. Point estimates are average marginal effects in columns (1) and (2), OLS and IV coefficients in columns (3) to (6), and differences in the average marginal effect of belonging to the partial subsidy group, relative to the 2002-2004 reference period, in column (7). Point estimates have been multiplied by 1,000 for clarity in columns (1) through (6). Standard errors clustered on income bins are in parentheses.