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Policy induced increases in private healthcare financing provide short-term relief of total healthcare expenditure growth: Evidence from OECD countries

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ABSTRACT

Healthcare reforms have long been advocated as a cure to the increasing healthcare expenditures in advanced economies. Nevertheless, it has not been established whether a market solution via private financing, rather than public financing, curb aggregate healthcare expenditures. To our knowledge, this paper is the first that quantifies the impact of reforms that significantly increases (decreases) the private (public) share of healthcare financing on total healthcare expenditures relative to income in 20 OECD countries. Our reform measure is based on structural break testing of the private share of total expenditures, and verification using evidence of policy reforms. To quantify the effect of these reforms we apply Propensity Score Matching and Inverse Probability Weighted regression analysis. Over a 5-year evaluation period the reforms lead to an accumulated cost saving 0.45 percentage points of GDP. The yearly effects of the reforms are largest in the first years in the post-reform period and decreases in size as a function of time since the reform. Our findings suggest that the investigated healthcare reforms have a relatively short-lived effect on aggregate health spending relative to GDP. The findings are robust to various sensitivity tests.

1. Introduction

For decades most developed economies have experienced a rapid increase in total Health Care Expenditures (HCE) relative to income. At the same time, the private share of HCE decreased (Fan and Savedoff, 2014, Fig. 1). In light of this ‘Health Financing Transition’ academics and policy-makers worried that the healthcare systems would become unsustainable (OECD, 1987; Oxley and MacFarlan, 1995; Chernichovsky, 1995). In an attempt to increase efficiency and curb public expenditures, countries introduced healthcare reforms. Till date, it has not been established quantitatively in a multi-country sample of comparable reforms whether significant policy reforms that shift healthcare financing from public to private entities curbs total healthcare expenditures relative to GDP. We aim to fill this gap in the literature, by quantitatively analysing the effect of Health Care Financing (HCF) reforms¹ on total costs relative to GDP in developed economies in the short to medium run.²

To detect significant policy induced reforms we employ a methodology designed to identify structural reforms (Wiese, 2014). First,

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¹ We use the words reforms and privatisations interchangeably. In our framework a privatisations can purely be the result of downsizing public healthcare financing relatively to private financing.

² It is important to stress that the objective of the analysed reforms were to curb health-spending growth relative to income growth, see for example Busse and Reisberg (2004) and Glenngård et al. (2005). Privatisation could also be an objective, we take account of this in our analysis.

Fig. 1. Total healthcare expenditures and the private share of total healthcare expenditures over time, and analysed reforms. Note: The vertical lines indicate policy induced upward structural breaks in the private share of healthcare spending.

structural break tests are applied to the private share of HCE to identify ‘potential reforms’. Secondly, to qualify as a HCF reform the potential reform must be corroborated by evidence of an actual policy change. This ensures that the 23 analysed reforms are policy induced and makes a statistically significant positive (negative) impact on the privately (publicly) financed share of HCE. That way, we avoid including reforms in our sample that did not fundamentally alter the institutional setup of the health care financing system.

We estimate the effect of the reforms shown in Fig. 1 on the change in total HCE as % of GDP in the following 5-years. Following a reform we observe a stagnating or a decreasing development of total HCE relative to income in the medium run for several countries, for example in France and Spain. It is very likely that the countries that undergo HCF reforms are the ones where there is a potential for cost savings. This implies selection into treatment, which will bias any standard OLS estimate of the effect of reforms on total HCE. Ideally we would like to know what would have happened to total HCE in the absence of a reform. As can be seen in Fig. 1 we have multiple observations in the sample of countries where no reform took place. Therefore, both Propensity Score Matching (PSM) and Inverse Probability Weighted (IPW) regression analysis are applied. This allows identification of appropriate reform counterfactuals mitigating potential selection bias. The estimated effect of the reforms is of the magnitude 0.45 percentage points of GDP saved over the five following years. Additionally we show that the estimated cost savings in the post-reform period are large in the first year(s) and almost continually decreasing over time, approaching a zero-effect in the 5th year. We wish to bring to the reader's attention that the analysed reforms may have adverse effects on total expenditures in the longer run and that the analysed reform may result in a net increase in total HCE in the longer run.³

Section 2 reviews the relevant literature. Section 3 discusses the identification of HCF reforms and gives a brief characteristic of the reforms. Section 4 presents the estimation approach along with the data. Section 5 gives the main results, while section 6 investigates the robustness of the results. Section 7 discuss the findings and concludes.

2. Literature review

Most reforms with an expenditure-curbing objective can be categorized into the 2nd or the 3rd reform wave (Cutler, 2002). These waves of healthcare reforms were introduced while maintaining the objective of universal coverage and equal access obtained in the 1st

³ Perhaps through decreases in healthcare equality and population health as some authors suggest (Cutler, 2002; Woodward and Kawachi, 2000). Or, if the development of a private system to supplement the public system takes time and therefore migration to the private system happens with delay.

reform wave in the 1960's and 1970's. The 2nd wave in the 1980's-1990's focused on the supply side by introducing cost controls, rationing and expenditure caps with the objective to limit or decrease public spending. However, such policy instruments only works, if the substitution effect to private financing is limited. That is, such initiatives will only be successful in lowering total expenditures if health consumers do not fully supplement the rationed publicly financed services with private substitutes. Also, decentralised management schemes were meant to incentivise local management to reduce over-utilisation whereby total HCE should decrease (Cutler, 2002).

The 3rd wave in the 1990's-2000's focused on the demand side through incentives and competition. Reforms mainly introduced/increased co-payments, like patients' share of drug costs and user fees. Such reforms were mainly aimed at re-introducing the link between consumption and the individuals' marginal cost of healthcare. With moral hazard present, these policies should reduce over-utilisation and hence reduce total costs (Zweifel and Manning, 2000; Fan and Savedoff, 2014). That is, incentivise individuals to behave prudently health-wise and to use the system only when necessary. Conversely, other authors argue that total HCE relative to income increases as a result of private financing. For example, private insurance to cover user fees and co-payments brings new money into the system. Apart from bringing an element of competition to health financing, private insurers have less ability to apply the cost control measures that worked containing public expenditures, like spending caps and global budgeting. As a result, total HCE may increase (Colombo and Tapay, 2004).⁴

Theoretically reforms that increase the private share of total HCF, either by limiting public expenditures or increasing private expenditures, have the potential to curb total expenditures, at least in the short to medium run. We analyse whether this effect is present following reforms belonging in the 2nd and 3rd reform wave.

Many case studies have provided estimates of the expenditure-containing effect of reforms, at least in sub-sectors of the healthcare system (e.g. hospital care, general practitioner), including privatisation-type reforms, usually without quantifying the reductions in total/national health expenditures relative to GDP (e.g. Cutler, 2002; Kamke, 1998; Saltman and Figueras, 1998; Tuohy and Flood, 2004; Wörz and Busse, 2005). From a policy and societal perspective, it is important to know the extent to which a key goal of HCF reforms was achieved.

Recent studies have gone some way in quantifying the effect on expenditures relative to GDP of reforms similar in type to the ones analysed in this paper. At the individual country level, hospital-financing reform is not found to have an effect on total HCE in Switzerland (Braendle and Colombier, 2016). Likewise, variation in co-payments in Sweden has no effect on the number physician visits (Jakobsen and Svenson, 2016). Colombo and Tapay (2004) conclude that increased opportunity to take out private health insurance generally increases total HCE relative to GDP.

In the literature on the determinants of HCE some studies analyse the effect of the private share level.⁵ These studies find limited (Leu, 1986), or no effect (Hitiris and Posnett, 1992). Xu et al. (2011) find no effect of whether healthcare is financed through taxes or out-of-pocket payments on total HCE relative to GDP. In addition Basu et al. (2012) provides a comprehensive systematic literature review of empirical studies on private and public healthcare systems in middle- and low-income countries. Their findings do not support the hypothesis that the private sector is more efficient, accountable, or medically effective than the public sector. In sum, the quantitative empirical literature suggests a remarkably limited, if any, cost curbing effect of increases in the private share of HCF on total expenditures relative to income. In our view this warrants research as countries still pursue such reforms with the aims of containing expenditures and increasing efficiency.

3. Identifying HCF privatisations⁶

3.1. Structural breaks

We measure to what extent public and private funds finance healthcare. The ratio y_{it} , the private share of HCE relative to total HCE (public + private) in country i at time t is used. Using data provided by the OECD, this ratio is calculated as: $y_{it} = \frac{\text{private expenditure}_{it}}{\text{private expenditure}_{it} + \text{public expenditure}_{it}}$. It can be interpreted as the percentage of private financing of total spending, as percentage of GDP, see Fig. 1. Table A1 in the appendix gives the summary statistics of the data used to identify potential reforms.

Structural break testing is applied to identify significant shifts in the ratio. A structural break is a fundamental change in the Data Generating Process (DGP), for example due to an economic reform (Hansen, 2001). We apply the Bai & Perron (B&P) -filter to identify structural breaks (Bai and Perron, 1998, 2003). In order to define potential reforms in the context of the B&P-filter, consider a model with m possible structural breaks in an OLS framework that takes the form:

$$y_t = \delta_j + u_t \quad (t = 1, \dots, T, j = 1, \dots, m + 1)$$

where y_t is the dependent variable, in this case the time series of private share of total HCE for each country considered. δ_j is a vector of

⁴ Related to the discussion of whether healthcare-financing privatisations can be used to curb expenditure growth is the discussion of whether healthcare is a luxury good or a necessity good (e.g. McGuire et al., 1987; Bhat and Jain, 2006). If it is a luxury good, perhaps restrictions on publicly financed healthcare are better justifiable.

⁵ A crucial difference between these studies and our approach is that we look at changes in the private share. That allows us to isolate a treatment effect.

⁶ This section draws on Wiese (2014).

Table 1
HCF reforms.

Country	Sample period	Potential reforms B&P-tests, 5% significance level	Verified reforms
Australia	1971–2011		
Austria	1960–2012	1967, 1989	1989
Canada	1970–2012	1986, 1993	1986, 1993
Denmark	1971–2012	1984, 1990	1984, 1990
Finland	1960–2013	1993	1993
France	1990–2012	2003	2003
Germany	1970–2013	1983, 1998, 2004	1983, 1998, 2004
Greece*	1987–2011	(1994)	(1994)
Iceland	1960–2013	1993	1993
Ireland	1960–2012	1985	
Italy	1988–2013	1994	1994
Japan	1960–2012		
Netherlands	1972–2013	1996	1996
New Zealand	1970–2011	1990	1990
Norway	1960–2013	1980, 1988	1988
Portugal	1970–2011	1982, 2006	1982, 2006
Spain	1960–2012	1995	1995
Sweden	1970–2012	1985, 1992, 2001	1985, 1992, 2001
Switzerland	1985–2012		
UK	1960–2012	1985, 1997	1985, 1997
USA	1960–2012		
Total		26	23

Belgium was excluded because the time series is too short to run the B&P-filter. * The reform in Greece is excluded from the analysis due to missing observations on covariates in the PSM model.

estimated coefficients (constants) of which there are $m+1$, so δ_j is the mean at the different segments of the time series y_t . u_t is the error term. The segments generate a stepwise linear route through the times series y_t and give m structural breaks or alternatively $m+1$ regimes.⁷ An upward regime shift (structural break) is detected as a potential privatisation/cost-containment reform for which validation is required. A shift to a new regime is unlikely to happen by chance, dependent on the test-size employed. We employ a 5% significance level. Thus, a regime shift implies that the underlying DGP has been altered, generating a structural break.

The minimum distance between breaks is restricted by the trimming parameter h , expressed in percentage of the sample size.⁸ Here a trimming of $h = 0.15$ or $h = 0.2$ is chosen (smaller samples call for larger trimming), because it generates the best fit with *de jure* evidence while still being econometrically sound. The trimming parameter implies that no potential reform can be identified at the beginning and end of each series. The appropriate observations are excluded in the estimations that follow to avoid identification error. A heteroskedasticity and autocorrelation consistent covariance matrix is used (Antoshin et al., 2008).

3 general test procedures are possible when applying the filter, see Bai and Perron (1998, 2003). We apply all three, see appendix A2 for details. If at least two of them indicate an upward structural break in a given year it is taken as evidence of a potential reform. In cases where the timing of the break differs slightly the decision is based on graphical analysis. See the outcomes of the 3 procedures in Table A2 in the appendix and our final set of potential reforms and sample periods in Table 1.

3.2. Healthcare reforms

Structural breaks can be caused by factors other than policy-induced shift in the private share, for example exogenous shifts in consumer preferences, or relative price movements. Thus, the detected structural breaks need to be verified. Column 4 in Table 1 below shows the reforms that can be verified, see Table A3 in the appendix for details.

To perform the verification the WHO's and European Observatory on Health Systems and Policies "Healthcare Systems in Transition" country reports are employed. These reports are available for each country covering the sample period and contain descriptions of health policy reforms introduced over time. When a report describes a reform that could have had the objective to either reduce the public share of HCF, increase the private share of HCF, or both, it is taken as evidence of a *de jure* reform. A time lag is often present between the *de jure* reforms and their outcomes (Acemoglu et al., 2006). In most cases the length of this lag is one year (see Table A3 in the appendix). If more than two years passed between a policy change and a detected structural break, the potential reform is not coded as a verified reform.

The verified reforms can be characterized as policy-driven HCF privatisation/cost-containment reforms. These reforms either target the private share of HCF from the supply side, the demand side, or both. Decentralization of financial authority with the objective to

⁷ The idea underlying the B&P-filter is straightforward. It generates the segmented route through the time series that yields the significantly lowest Sum of Squared Residuals (SSR) compared to a baseline SSR. The segments can be thought of as regimes where y_t fluctuates around a constant mean δ_j .

⁸ h is determined by the researcher prior to the analysis.

make local managers responsible for public spending and productivity are examples of supply side reforms (e.g. Italy, 1994; New Zealand, 1990; UK, 1997). Likewise, global budgeting schemes and spending caps (e.g. Sweden, 1985; Denmark, 1982) were supply side initiatives. Examples of demand side reforms are consumer cost-sharing by introduction of co-payments (e.g. Germany, 1998), or increases in patients' share of drug costs (e.g. Sweden, 2001; UK, 1985). In many cases the validated reforms are a combination of demand- and supply-side changes (e.g. Italy, 1994; Portugal, 2006; Sweden, 1992). See Table A3 in the appendix for specific information about each individual policy reform in the sample. As we are interested in whether healthcare reforms curb total expenditures, only the verified reforms are used in the following estimations. 23 of the 26 detected reforms can be validated.

A risk of the methodology is that the outcome of a policy reform can be hidden in the data by unrelated economic changes, such as exogenous shifts in consumer preferences or relative price movements. The opposite can also happen, that a policy change has no significant impact on the data, but unrelated economic changes lead us to conclude that it had. Either way, the sequential procedure is less prone to identification error than identification using policy input data or economic outcome data alone.

Additionally we only analyse the impact of reforms that are large enough to significantly shift the public (private) share of HCE. One could argue that our approach to identify reforms leads us to overestimate the effect on total HCE relative to GDP. On the contrary, the policy reports document that many reforms were introduced, but some were too small to cause a structural break. Nonetheless they may still affect total HCE. By design these reforms are placed in the control group. One could argue that this causes a downward bias in our estimates. However, as Easterly (2006) suggests, many *de jure* 'reforms' are so-called "stroke-of-the-pen" policies. That is, policies with limited planned economic impact. That makes it difficult to judge the intention of a reform by reading policy documents and hence policy documents should be paired with an objective statistical measure, see also Wiese (2014) for a discussion of this issue.

4. Estimation approach

4.1. Empirical strategy

In a randomised control study treatment is assigned randomly. As a consequence, there is no selection into treatment. Therefore, an unbiased estimate of the treatment effect can be computed directly from such data. In our setting, the assignment to treatment is not random (i.e. the decision to conduct a HCF reform), and we can therefore only observe one of the potential outcomes for a country. That is, an observation is either in the treatment or the control group, never both. When randomization is not feasible, PSM constitutes a proper alternative. It has become a standard tool to assess the effects of treatments like (policy) interventions by identifying suitable counterfactuals in the absence of randomised experiments, thereby reducing selection bias (Imbens and Wooldridge, 2009; Imai et al., 2010; Heckman et al., 1997; Aidi and Franck, 2015; Nolan and Layte, 2017). The idea behind matching is to compare treated observations to non-treated observations that are similar on observable characteristics. After the matching is performed a straight comparison of means is possible.⁹

Consider our sample of countries of which some experience a HCF reform in certain years. We are interested in whether the non-random assignment of this treatment affects total HCE. The hypothesis is that it has negative effects (declining HCE). The outcome variable is defined as the '(average) change in total HCE as a % of GDP' over 1-, (3-) and (5-years) following a treatment (i.e. a HCF reform). Using 1-, 3- and 5-years after a HCF reform, enables us to look at the short to medium run effects of a HCF reform.

We drop the 4 observations before and the 4 observations after a treatment from the control group. Otherwise a treated observation could be matched with a non-treated observation that contains part of the outcome from a treated observation. Neglecting this would lead to biased estimates. In the example presented in Fig. 2, observations from 1986 till 1989 are dropped, as well as observations from 1991 till 1994. This gives a total 5 changes before and 5 changes after a treatment being dropped. Remember that the first change being dropped is the change between 1985 and 1986, and the last change is from 1994 to 1995. Dropping these observations/changes is done irrespectively of the outcome variable (whether it is 1 year or, 3- or 5-year average change in the total HCE as a % of GDP).¹⁰

The PSM method consists of two steps. First a logit model is used to estimate the propensity scores, i.e. the probabilities of receiving a treatment. Second, matching techniques are used to match each country-year observation that received a treatment with different observations from the control group, that are similar on observable characteristics, see next subsection. A treated observation can be matched with non-treated observations from the same country. However, this is not a problem if this is the best counterfactual based on observable characteristics. After matching the Average Treatment effect on the Treated (ATT) can be calculated as the average difference between the outcomes in treated countries and the matched counterfactuals.

The key assumption behind PSM is unconfoundedness, introduced by Rosenbaum and Rubin (1983). The implication of unconfoundedness, is that beyond the included covariates, there are no (unobserved) characteristics of the individual observation which is associated both with the outcome and the treatment (Wooldridge, 2005). That is, we have a sufficient set of predictors for HCF reforms in our set of covariates such that adjusting for differences in these covariates would provide valid estimates of causal effects.¹¹

The second assumption behind PSM is the overlap or common support assumption. This condition assumes that for each value of

⁹ Here we only briefly review the method, see Rubin (1974) and Rubin (1977) for more detail.

¹⁰ If these observations are kept in the sample for e.g. the 1 year effect, we might be matching with observations that contain part of the effect.

¹¹ Although this setting is closely related to one of standard linear regression analysis with a large set of control variables, the literature has gradually moved away from this method. The main reason for this shift is that, while local linearity of the regression functions may be a valid assumption, this may not be the case globally. This can lead to severe bias when estimating average treatment effects with OLS if the linear approximation is not precise (Imbens and Wooldridge, 2009).

Fig. 2. Dropping observations when constructing outcome variables. Notes: This example shows which variables are dropped when constructing the outcome variable (average change in HCE as a % of GDP over 1-, 3- and 5-years following a treatment) for the HCF reform in New Zealand (1990). The year in which the HCF reform happens (i.e. $T = 1$) is also used for the construction of the outcome variable, and thus only ‘4 years’ before and after are dropped. The year for which $T = 1$ is not dropped for obvious reasons.

observed characteristics, there is a positive probability of being either treatment or control (Heckman and Smith, 1999). This ensures that there is a sufficient overlap in the characteristics of treated and control observations such that an adequate match can be found.¹² The ATT effect that we estimate must therefore be defined conditionally on the region of overlap.

4.2. Determinants of HCF reforms and matching techniques

Due to the ‘unconfoundedness assumption’ a central part of PSM is selection of an appropriate set of covariates to estimate the propensity scores. We rely on literature on economic, political and health-sector specific factors that simultaneously are believed to cause total HCE and HCF reforms. We strive for a parsimonious model, see Table A4 for descriptive statistics of the covariates used for matching.

Macroeconomic crises are perhaps the most common factor that trigger economic reforms in general (Drazen and Grilli, 1993). The empirical evidence is robust; crises trigger reforms (Agnello et al., 2015; Drazen and Easterly, 2001; Pitlik and Wirth, 2003; Waelti, 2015), also HCF reforms (Wiese, 2014). Therefore, several measures that capture different economic crises are included: the growth rate of GDP (growth crisis), the unemployment rate (job crisis), the general government budget balance (government budget crisis) and the severity of government indebtedness (sovereign debt crisis). In times of economic crises reforms become more likely. Simultaneously, both governments and consumers are likely to cut HCE. The Health Economics literature on the determinants of HCE has consistently found that GDP determines it (Di Matteo and Di Matteo, 1998; Hatwig and Sturm, 2014).

Furthermore, the *inflation rate* is included because governments can use their power to issue money to finance fiscal expenditures. Also, a high inflation rate signals economic crisis, so the probability of reform increases, unless moderate inflation is used as source of finance and hence postpone reforms.

Special interest politics may drive reforms. Political parties promote policies that favour their constituencies to promote (re)election (Hibbs, 1977). Left-wing governments prefer a public tax-financed system and right-wing market-oriented governments favour privatisation to avoid re-distributional effects. Therefore, the type of reform considered is less likely during left-wing rule. In a similar manner, left-wing governments favour higher public spending on healthcare compared to right-wing governments (Herwartz and Theilen, 2014; Mou, 2013). Therefore, the Potrafke-index (Potrafke, 2009) of the ideological orientation of governments, in terms of economic policy, is included.

Scholars argue that a ‘window of opportunity’ opens after elections where newly elected governments can impose reforms at lower political costs (Haggard and Webb, 1993). Thus, reforms are more likely in the period following elections (Lora and Olivera, 2004). Also, the literature on the political determinants of HCE has found that public HCE are higher in election years (Potrafke, 2010). Therefore, a dummy variable capturing election years is included.

Two variables are included that capture cost developments likely to impact the probability of reform, while also signifying the potential for cost savings through reforms of financing. First, the share of the population older than 65 years; a larger fraction of elderly implies higher costs and declining tax revenues to finance the costs (Oxley and MacFarlan, 1995; Hatwig and Sturm, 2014). Second, the average of total HCE as percentage of GDP over the 5 years before the reform is included to capture the medium-term trend in healthcare costs directly. If costs are rapidly increasing it may call for policy action, such as HCF reform. This variable captures both demand and supply driven costs increases in the medium term, such as technological advances.

Lastly, if present, duration dependence in panel models with a binary dependent variable leads to wrong inference (Beck et al., 1998). The applied estimator relies on the assumption that the probability of reform within countries is independent over time. Previous similar reforms may impact the probability of further reforms; at the same time reforms impact total HCE. To correct for duration dependence, we apply a simple but well-performing method (Carter and Signorino, 2010). It is based on the inclusion of a variable counting the length of the spell of no reform at every observation, counting from the last year of reform, and its squared and cubed term. These 3 variables are included in the propensity score model.

After building the PSM model the treated observations are matched with the suitable counterfactuals. To improve the quality of our matching, we apply multiple matching techniques. Specifically, we apply 5 different matching algorithms with the objective to use methods that are dissimilar:

¹² If there are regions where the support of the observed characteristics does not overlap, then matching is only justified when performed over the region of common support (Caliendo and Kopeinig, 2008).

1. Radius matching. It is a variation of nearest neighbour matching that attempts to avoid ‘bad’ matches by imposing a tolerance distance
2. 5 nearest neighbours with replacement. Aside from performing the matching with replacement, it matches the treated observation with the five closest observations from the control group in terms of their propensity score.
3. Local linear regression imposing a calliper distance. Similar to kernel matching but includes a linear term in the weighting function. The caliper distance makes sure that pairs of treated and control observations are formed such that the difference in propensity scores between matched subjects differs at most by 0.5% in probability.
4. Kernel bootstrap. It constructs a match for each treated observation using a weighted average over multiple observations in the control group. Kernel estimators such as this one, can be viewed as a matching estimator where all observations within a certain bandwidth receive a weight.
5. Kernel bootstrap with trimming = 5. In contrast to the fourth approach it imposes common support by trimming 5 percentages of the treatment observations at which the propensity score density of the control observations is the lowest.

As an alternative to matching, we also apply the doubly robust estimator developed in [Robins and Rotnitzky \(1995\)](#), [Robins et al. \(1995\)](#), and [Van der Laan and Robins \(2003\)](#). The idea behind this estimator is to combine regression analysis with propensity score weighting, rather than conducting a standard regression analysis including a rich set of controls. That is, we estimate an IPW least square regression where the covariates enter twice; through the weights, and through inclusion in the regression equation.¹³ Specifically, treated observations get a weight by the inverse of the propensity score, whereas the control observations get a weight of the inverse of one minus the propensity score.¹⁴

5. Results

5.1. Propensity scores

The estimates from the logit model, which we used to calculate the propensity score is given in [Table A5](#) in the appendix. Running this model on our sample of 21 countries and 565 available observations results in a model that correctly classifies the outcomes 83% of the time, see [fig. A1](#) in the appendix. The *debt crisis indicator*, the *unemployment rate*, the *inflation rate*, and the *population over 65 years* are significant at the 5% level or less, with the expected signs. The duration dependence variables are jointly significant at the 5% level; thus, they belong in the model ([Beck et al., 1998](#)). Furthermore, in [Fig. 3](#) we observe that the common support condition is satisfied, as the support given by the treatment group completely overlaps the support by the control group.

[Fig. 3](#) shows the predicted probability of reform, i.e. propensity scores, for all observations in the sample. 145 control observations are dropped due to lack of common support.

5.2. The effect of healthcare financing reforms on total health care expenditures

The results indicate that HCF reforms lead to a significant HCE saving in the short and medium run. [Table 2](#) shows the ATT for the HCF reforms for different matching algorithms, and the doubly robust estimation, over 1-, 3- and 5-years.

The effect of HCF reforms for the 1st year, and 3- and 5-year average health care expenditure as percentage of GDP suggest a decreasing effect over time, across matching methods. The results indicate a cost saving in the first year after a reform in the range 0.08–0.21 percentage points of GDP. The average cost saving over a 3-year period is between 0.10 and 0.14 percentage points of GDP each year, while the average cost saving over the 5-year period is between 0.08 and 0.11 percentage points of GDP each year. Accumulated this means that approximately 0.45 percentage points of GDP are saved over the five years. This is a substantial economic effect. All 5-year average treatment effects are estimated statistically significant. This implies, all else being equal, that it is cost-efficient to privatise healthcare financing in the reformed countries. However, an assumption of all else being equal is not very realistic. The analysed reforms are likely to affect healthcare consumers’ choices in ways that affect quality and equity of healthcare. This is discussed in more depth in section 7.

To illustrate the dynamic effect of the reforms, [Fig. 4](#) displays the estimated yearly cost savings for each of the five years in the post reform period. The dynamic effects we observe are similar across the estimators we have applied, revealing a pattern showing that the largest cost saving is realized in the 1st year(s), and the effect is diminishing in the following years. Extrapolating this trend, the effect becomes positive after additional years. The statistical significance of the yearly effects displayed in [Fig. 4](#) are strongest in the first 3 years and then disappears for most methods in the 4th and/or 5th year. This supports the view that the cost-reducing effect of these reforms is strongest early in the post-reform period. Hence the analysed reforms can be used to provide short-term relief of healthcare expenditure growth, but it is debateable if the long-term effect is efficient as a cost containment tool, the findings of [Basu et al. \(2012\)](#) support this view.

It would be interesting to evaluate the effect over a longer time horizon. However, this is not possible since many of the analysed

¹³ It is said to be doubly robust, as it only requires one of the following two conditions to hold: The conditional mean model is correctly specified, or the PSM model is correctly specified. Weighting can be interpreted as removing the correlation between the covariates and the treatment indicator, and regression removes the direct effect of the covariates.

¹⁴ See [Robins and Rotnitzky \(1995\)](#) for further explanation.

Fig. 3. Common support distribution for the baseline propensity scores.

Table 2

Average treatment effect of the treated (ATT); PSM and IPW regression analysis.

Estimation method	(1)	(2)	(3)	(4)	(5)	(6)
	Radius	5-Nearest Neighbours with replacement	Local Linear Regression, calliper = 0.005 ^a bootstrap se	Kernel bootstrap se	Kernel with trimming bootstrap se ^b	Doubly robust
Variables	(t-stat)	(t-stat)	(p-value)	(p-value)	(p-value)	(t-stat)
Change in total HCE as % of GPD - 1 year	−0.083 (−1.05)	−0.144** (−2.05)	−0.175** (0.019)	−0.134** (0.036)	−0.097 (0.156)	−0.209*** (−2.87)
Change in total HCE as % of GPD - 3 year average	−0.098 (−1.63)	−0.124*** (−2.65)	−0.130** (0.013)	−0.118** (0.014)	−0.100* (0.051)	−0.139*** (−3.06)
Change in total HCE as % of GPD - 5 year average	−0.078* (−1.81)	−0.110*** (−3.22)	−0.098** (0.010)	−0.095*** (0.005)	−0.082** (0.036)	−0.100*** (−3.23)
Observations	393	393	393	393	393	393

Notes: Following [Stuart \(2010\)](#), we conducted balancing tests for the covariates after matching. Despite a number of differences in the covariates before matching, there are no significant differences between the treatment and control group after matching. The results of the balancing tests are available on request.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. For the bootstrap we use 1000 replications.

^a Value for maximum distance to controls.

^b Drops 5% of the treatment observations at which the propensity score density of the control observations is the lowest.

reforms are followed by additional privatisations 6–8 years after the first reform, see [Table 1](#). This feature of the data makes it impossible to disentangle which reform causes the effect after more than 5 years.

To obtain the estimates in [Table 2](#), add the yearly effects and divide by the appropriate number of years.

6. Robustness analysis

The inclusion of the duration dependence variables may be a cause of concern. Therefore, the analysis in [Section 5](#) is redone without their inclusion. The results are similar, see [Table A7](#) in the appendix. Likewise, it is important to establish whether individual countries included in the sample are driving the results. We therefore re-estimate the results excluding each individual country, one at the time. No exclusion of any specific country causes large changes in the results. Thus, the results are not driven by the inclusion of any specific country. See appendix [Tables A8–A10](#).

The PSM method does not allow the possibility of controlling for fixed-effects in the outcomes. Slow changing institutional differences between countries may be important drivers for the effect of reforms. Therefore as an additional robustness test we follow [Freedman and Berk \(2008\)](#) and estimate a weighted-LSDV model. When using this approach, the observations are weighted using the

Fig. 4. Yearly ATT estimates.

estimated propensity scores as with the double robust estimator. The fixed-effects estimation is then conducted without including further control variables (in contrast to the double robust estimator). The results of the estimated effects of HCF reforms are in line with our analysis above, showing the same decreasing pattern, although with a smaller magnitude. The results of the LSDV model can be found in Table A11 in the appendix.

The main assumption underlying our PSM analysis is un-confoundedness. Rosenbaum (1995) developed a sensitivity analysis, which focus is on what effect the unobserved covariates could have on the p-value for the test of no effect of treatment. Using this sensitivity test we check how much the odds of participation would have to be different in order to substantially change the p-value. Represented by a Γ , we test by what factor an unobserved covariate has to change the odds of participation in order to increase the p-value to above 0.10.¹⁵ The results of the sensitivity analysis are presented in Table A12 in the appendix. The analysis reveals that our results for the 5 years average are more robust compared to our 1-year results, with the 3 years average results landing in between. Specially, for the 5 years average results, it requires an unobserved covariate to change the odds of participation with more than factor 2.5 to increase the p-value to above 0.10.

7. Conclusion and discussion

Healthcare financing reforms that significantly increase the private share of funding reduce total healthcare expenditures in the short and medium run in the analysed countries. The results suggest an annual average cost saving over the 5-year period of 0.09 percentage points of GDP per year. Accumulated this means that approximately 0.45 percentage points of GDP are saved over 5 years. Our results also show that savings in total HCE are large in the beginning of the post reform period, but decreases continually approaching a zero effect after five years.

Equality and efficiency are also important parameters on which HCF reforms should be evaluated. Yet such an extensive analysis is outside the scope of this paper, below we discuss potential negative effects on health equality and efficiency resulting from the analysed reforms and the effects that may have on total HCE in the long run.

The found cost-containing effect may be due to adverse effects rather than efficiency increases, particularly concerning demand side reforms that rely on increased patient cost sharing. Specifically, low-income groups are more likely to be excluded due to budget constraints. If healthcare consumption falls more in low-income groups as Skriabikova et al. (2010) suggest, it will increase health inequalities. Woodward and Kawachi (2000) conclude that a society that tolerates large socioeconomic incline in health outcomes inequality will experience a drag on improvements in life expectancy, and pay the cost via (postponed) excess health care utilisation. Private payment may also cause consumers to choose options of lower quality, potentially leading to deteriorating overall health status of the population.

Supply-side reforms, such as spending caps and rationing, might lead to postponed excess health care utilisation and lead to dissatisfaction, for example due to longer waiting lines (Cutler, 2002). In sum, both supply- and demand-side reforms may induce negative effects on healthcare equality and efficiency, and thereby lead to increasing costs in the longer run. Given data and space

¹⁵ For more details, see Rosenbaum (1995) or Imbens and Wooldridge (2009).

restrictions we do not assess whether the analysed reforms are causing such adverse effects. These issues are interesting areas for future research.

Moreover, our measurement of the cost effect following a reform is isolated to costs accruing to the ‘Ministry of Healthcare’. A shift to increased private financing may lead to cost increases in other government departments. For example, less government money for psychiatric care may lead to increases in crime statistics and other forms of social expenditure. Some types of health inequalities have obvious spill-over effects on society, e.g. the spread of infectious diseases, the consequences of alcohol and drug abuse, or the occurrence of violence and crime (Woodward and Kawachi, 2000). Therefore, the isolated short to medium term cost saving may ultimately result in a net cost.

Another cause behind the decreasing effect may be a delayed substitution to private financing of healthcare that may result from the analysed reforms. For example, if no private insurance market exists such a market may take time to develop. Therefore the cost savings may decrease over time as a result of options to take out private insurance (Colombo and Tapay, 2004). This may be the case if increased private insurance does not work as a substitute to public expenditure, but rather functions as a complement. Additionally private healthcare systems, such as the US system carries much grater administration costs, compared to a public system such as the Canadian system (Woolhandler et al., 2003). Therefore increased private insurance may lead to higher total HCE in the longer run. Also, one could speculate that new governments with different political priorities, compared to the government that implemented the reform may implement somewhat more expansionary healthcare policies.

In sum, the literature suggests that it can be counter productive to privatise healthcare financing with the purpose of reducing total HCE, as the short run cost saving can result in increasing expenditures in the longer run. We believe that more research should be done to consider the long run effect of healthcare financing reforms.

Conflicts of interest

This paper expresses the opinion of the authors. The authors have no conflicts of interest to declare.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.ejpoleco.2019.02.001>.

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