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IS INCENTIVIZING BY SUBSIDIZING A BETTER WAY OF MANAGING CHRONIC HEALTH CONDITIONS?

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Is incentivizing by subsidizing a better way of managing chronic health conditions?

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ABSTRACT

The recognition that chronic care delivery is sub-optimal has led many health authorities around the world to its redesign. In Norway, the Department of Health implemented the Coordination Reform in January 2012 with the granting of subsidies to municipalities establishing emergency bed capacity (EBC) within their primary care facilities, with the explicit aim of reducing unnecessary admissions to hospitals. We examine the impact of this EBC policy on changes in emergency hospital admissions. Municipalities took advantage of these subsidies at different points of time, which means that there are differences in the local implementation of EBC, enabling us to use an identifying restriction to define the *treatment* and *control* groups. Using five different sources of register data and a quasi-experimental framework (the *difference-in-differences* regression approach), we estimate the causal effect of the changes in EBC on aggregate emergency admissions for eight ambulatory care sensitive conditions (ACSCs). We also estimate the impact on each condition separately. The results show that EBC exerted a significant and negative effect on the changes in emergency admissions. The effects are largely consistent with alternative model specifications but we find mixed results for the different ACSCs, in that EBC negatively affected emergency hospital admissions for angina and chronic obstructive pulmonary disease, but not congestive heart failure and diabetes. The main implication of the study is that EBC within primary care is a sensible way of redesigning chronic care as it leads to a meaningful reduction in hospital emergency admissions.

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1. Introduction

The recognition that chronic care delivery is sub-optimal has led many health authorities the world over to redesign their care of people with chronic conditions. Disease management programmes (DMP) (or integrated care programmes) have been implemented with the aim of providing better follow-up of existing conditions and reducing the risk of serious complications, ultimately improving patient health prospects and reducing expected future health services expenditures. DMPs were first developed in the US, where the experience of health authorities in identifying chronic conditions and providing care according to need has subsequently encouraged health authorities in Europe and elsewhere to experiment with various forms of integrated care.

In an effort to accomplish a reorientation of care, and with the explicit aim of reducing the number of unnecessary admissions to hospital (White Paper, 2008), health authorities in Norway implemented the Coordination Reform (CR) in January 2012. The CR introduced three novel economic incentives. These were: (i) forcing municipalities to internalize some costs of hospital admission by paying 20 percent of the national average cost for specific diagnoses-related groups (DRGs) (medical but not surgical), (ii) penalizing municipalities on a daily basis if patients with a “ready for discharge” status in need of primary care follow-up were hospitalized beyond the discharge date, and (iii) subsidies to municipalities establishing 24/7 emergency bed capacity (EBC) within their primary care facilities.

A change in government in 2013 saw the abolishing of the municipal co-payment scheme (i) in early 2015, the main arguments being that the co-payment scheme did not work as envisaged and that it placed too much risk in the hands of the municipalities. Askildsen et al. (2016) conclude that the use of specialist somatic health care services has not changed as a result of the municipal co-payment system, using data from the same time period as here (2010-2013).

However, the penalty scheme (ii) remains in place and municipalities are obliged to reimburse hospitals NOK 4,000 per day (About 474 USD or 434 EUR) in excess of the discharge date (as determined by the hospital). This scheme has contributed to a swifter takeover of patients in need of post-discharge follow-up by their respective home municipalities. Concerning changes to the subsidy scheme (iii), our interest here, by 1 January 2016 all municipalities had by law the obligation to provide such services, either alone or in cooperation with other municipalities.

Before the introduction of the subsidy scheme in Norway, municipalities had only weak incentives to avoid hospital admissions in general, because hospital admissions were (and still are) free of charge from the perspective of primary care services. Hospitals in Norway are state-owned, of which there are approximately 60 across the country, and so hospitalization shifts the costs from municipalities to the central government. Hospitals are organized and run by four regional health enterprises, being the extended arms of central health authorities. Obviously, hospitals vary in scope and size across the country but are reimbursed through a "mixed" prospective payment system: prospective fixed budgets (block grants) in combination with prospective variable DRG-based remuneration. The mix of grants and DRG-based remuneration varies, determined by the national parliament on a yearly basis. At present, the mix is 50:50 grants and DRG-based remuneration.

Municipalities, of which there are approximately 430 across Norway, provide primary care services, including home care services, (short- and long-term) nursing home services, and general practitioner (GP) services. The two major forms of home care services are assistance in daily activities and services provided by nurses and auxiliary nurses. People with chronic conditions are in principle followed up by home care services and GPs under contract with the municipalities. Thus, to the extent to which the municipalities use hospitals as a "buffer" in terms of bed capacity and/or "buffers" in relation to staff deficiencies regarding home services and/or short- and long-term institutional care, the management of chronic disease is likely neither to be cost-effective nor quality enhancing.

In this study, we analyse the effect of an arguably old-fashioned means of incentivizing economic actors, namely, the use of subsidies. Subsidies, i.e., incentives aimed at neither specific (groups of) staff members nor tied to specific performance measures, come across as somewhat dated compared to more sophisticated schemes such as pay for performance (P4P). In the literature on DMPs, we find several examples of such schemes, i.e., DMPs organized within a P4P scheme. Such DMPs have been introduced in Australia (Scott et al., 2009), England (Harrison et al., 2014; Dusheiko et al., 2011a), Italy (Bruni et al., 2009), Taiwan (Lee et al., 2010), the US (Lester et al., 2010), and the UK (Roland, 2004; Doran et al., 2006). However, the economic impact of sophisticated DMPs is not as clear-cut as one could be led to believe.

In two recent studies from England, a country with a (somewhat) similar national health care system as in Norway, there is only some evidence justifying their use, whereby reduced hospital costs are only identified in the case of stroke care. Any cost savings are mainly due to reductions in emergency admissions and outpatient visits, rather than to lower costs for patients treated in hospital or to reductions in elective admissions, and the primary care management of nine other chronic diseases are not associated with reduced hospital costs (Dusheiko et al., 2011b). Similarly, Harrison et al. (2014) estimate the impact of the national primary care pay for performance scheme or the Quality and Outcomes Framework, on emergency hospital admissions for ambulatory care sensitive conditions (ACSCs). ACSCs are customarily grouped in the categories of chronic, acute and vaccine-preventable (see, for example, Tian, 2012 and Blunt, 2013). See Purdy et al., 2009 for a discussion of different definitions of ACSCs and the associated disease codes. The scheme incentivizes some ACSCs but not others. The main finding is that the scheme is associated with a decrease in emergency admissions for incentivized ACSCs compared with conditions that are not incentivized.

Our analysis differs from the abovementioned DMP studies. As in Harrison's et al. (2014) reduced-form analysis, we examine emergency admissions at hospital for ACSCs as an increasingly accepted indicator of the access to and quality of primary health care. The reason is that these conditions develop over relatively long periods, and so timely and effective self-care, primary care or outpatient care can largely avoid the risk of crisis leading to emergency admission (Sanderson and Dixon, 2000). In this respect, unplanned hospitalizations for ACSCs are preventable, and in several studies, hospitalization rates for ACSCs serve as an outcome indicator to evaluate the access and quality of primary care. Studies in this area commonly examine the association between race, ethnicity, socioeconomic status and hospitalization for ACSCs (Oster et al., 2003; Laditka et al., 2003; Roos et al., 2005; Magán et al., 2011; Johnsen et al., 2012). The overall finding is that there is an inverse relation between socioeconomic status and emergency admissions for ACSCs. In addition, the US studies show that African Americans and Hispanics have significantly higher rates of hospitalization for ACSCs than do Whites.

Other studies examine trends, geographic variations and costs associated with ACSCs (Blunt et al., 2010; Lui et al., 2011; Gill et al., 2013; Bardsley et al., 2013; Weeks et al., 2016). For instance, the principal results from the UK studies are that admission rates are increasing over time but with notable variations by age group and individual condition, and that admission for ACSCs represents a large and increasing proportion of healthcare costs. Weeks et al. (2016) , comparing France with several other European countries along with Singapore, Australia, Canada, the US, and Brazil, conclude that France has higher admission rates than most other countries, with the possible exception of the US, Australia, and Brazil.

Of studies focusing on the institutional aspects of care delivery, Rizza et al. (2007), in an Italian study, add to the evidence on the need to develop and implement effective interventions to improve the delivery of healthcare at the community level. This is because they find a negative relationship between the use of community services and satisfaction with primary care, and the likelihood of experiencing ACSC-related hospitalization. In the US, Bindman et al. (2005) identify lower rates of ACSCs-related admissions among Medicare enrolees in managed care programs versus fee-for-service arrangements, while Probst et al. (2009) observe lower rates of admissions in areas with community health centres and rural health clinics.

The purpose of this paper is to examine whether subsidies in Norway to municipalities establishing EBC within primary care affect the changes in aggregate emergency admissions for eight ACSCs. Our analysis draws on five different sources of register data over the period 2010 to 2013. We exploit the fact that municipalities have taken advantage of the state subsidies at different points in time, which subsequently resulted in the different timing of the availability of emergency beds locally. We then use these differences as an identifying restriction and use a difference-in-differences (DID) regression approach to estimate the causal effect of changes in EBC in primary care on hospital emergency admissions for eight chronic conditions: namely, asthma, angina, chronic obstructive pulmonary disease (COPD), diabetes (not complicated), congestive heart failure, atrial fibrillation, epilepsy, and ulcers. Studies of DMPs aimed at those with diabetes, depression, heart failure and COPD show that cost savings are possible, but not necessarily so (de Bruin et al., 2011). In fact, in half of the studies reviewed, there are no cost savings, while in those where there are cost savings, the savings relate to the reduction in hospital admissions and specialist visits. Our main finding is that changes in EBC have a significant and negative effect on hospital emergency admissions. While these effects are consistent across alternative specifications, we obtain mixed results for different chronic conditions. In particular, EBC affects emergency hospital admissions for angina and COPD, but not those for congestive heart failure and diabetes.

The remainder of the paper is organized as follows. Section 2 presents the quasi-experimental framework in the form of the DID regression methodology. Section 3 details the data sources and variables used in the analysis. Section 4 provides the empirical results and Section 5 concludes the paper.

2. Estimation strategy

We aim to assess whether subsidies to municipalities for establishing EBC within primary care, an incentive provided under the CR in Norway, impacts ACSCs-related hospital admissions. In response to the reform, the municipalities took advantage of the subsidies at different points in time, which subsequently provide differences in the local timing of the introduction of EBC (see Figure 1). We use these differences as an identifying restriction by assuming that the expected change in outcomes for the control group would be the same as it would have been for the treatment group in the absence of treatment. Using a quasi-experimental framework (the DID approach), we then estimate the causal effect of the changes in EBC in primary care on ACSCs-related hospital admissions.

The quasi-natural experiment design in our context is one where we observe outcomes (i.e. ACSCs-related hospital admissions) for two groups over two periods. One of the two groups (the treatment group) is exposed to a treatment in the second period (after the reform) but not in the first period (before the reform), whereas the second group (the control group) is not exposed to the treatment during either period. We observe the same units within a group in each period and then subtract the expected gain in the control group from the average gain in the treatment group. As discussed, CR began in January 2012, so we consider 2010 and 2011 as being before reform, and 2012 and 2013 as being after the reform period.

Accordingly, we define the municipalities that established EBC within primary care by 31 December 2012 as the treatment group. As shown in Figure 1, it appears that 76 municipalities took advantage of the subsidies by the end of 2012. The remaining 294 municipalities constitute the control group. We omitted 10 municipalities because of ambiguity regarding the timing of the implementation of EBC. Few municipalities appear to have introduced EBC before the reform year 2012 (See Figure 1).

Formally, we estimate the following equation:

$$Em\_ACSC_{ijt} = \beta_1 R_t + \beta_2 T_j + \beta_3 R_t * T_j + \delta G_{ijt} + \gamma S_{ijt} + \eta_d + e_{ijt} \qquad (1)$$

where the dependent variable Em_ACSC_{ijt} indicates aggregate (for all eight diagnoses) ACSCs-related hospital emergency admissions by patients living in municipality *j* divided by the aggregate number of patients on the *i*th GP's lists in municipality *j* in year *t*, multiplied by 100. R_t and T_j are dummy variables identifying reform (a dummy variable if post treatment equals one) and treatment municipalities (a dummy variable if the observation is in the treatment group), respectively. The coefficient for $\hat{\beta}_3$ describes the *DID* estimate or impact of the reform.

To control for observable differences, we include both GP and patient level attributes. In Equation (1), the vector *G* includes all GP-level attributes and vector *S* comprises the average socioeconomic characteristics of patients for each GP. Given the ACSCs include pooled data across eight different diagnoses, and to control for diagnosis-specific fixed effects in Equation (1), we include diagnoses-determined dummy variables, η_d.

Suppose the introduction of EBC is not random but systematic, i.e. takes place in municipalities with high or low average emergency admissions for ACSCs or in periods with different average ACSCs. To capture municipality differences that are constant over time, in Equation (1), we include municipality fixed effects (ν_j), and to capture differences over time that are common to all municipalities we include yearly fixed effect (μ_t) and estimate the following equation:

$$Em\_ACSC_{ijt} = \beta_1' R_t + \beta_2' T_i + \beta_3' R_t * T_j + \delta' G_{ijt} + \gamma' S_{ijt} + \eta_d + \nu_j + \mu_t + e_{ijt} \quad (2)$$

To confirm the robustness of our estimates, we estimate four alternative models using Equation (2). The models differ in (i) the definition of the post-reform period, and (ii) the grouping of the treatment municipalities based on when municipalities introduced EBC locally. The first two models use the same definition of the treatment municipalities but with a different characterization of the post-reform period. Our base model (Model B1) specifies post-reform (i.e. *R* = 1) with 2012 and 2013 and pre-reform (i.e. *R* = 0) with 2010 and 2011. In this model, the treatment municipalities include (i.e. *T* = 1) municipalities that introduced EBC by December 31, 2012. A lag effect of the reform on ACSC admissions is viable, and this alternative characterization of the period after reform may elicit such an effect, if any. To check this, our second model (Model B2) uses the same definition for a treatment municipality and the pre-reform period, but with a different construction of the post-reform variable, such that the post-reform dummy lagged one period (i.e. *R1* =1) includes only observations for 2013.

The third model (Model S1) uses the same definition for the post- and pre-reform periods as Model B1, but with a different classification for the treatment municipalities. Model S1 specifies treatment municipalities as those that implemented EBC by 30 June 30, 2012 (*Treat_June* = 1). The fourth model (Model S2) use an identical definition of the treatment municipalities, but with a different definition of the post-reform period, where *R1* = 1 includes only (lagged) observations for 2013 (as in Model B2). We cluster standard errors to allow for arbitrary within-group correlations at the municipality level and to test the reform effect hypotheses against their one-sided alternatives (see, e.g. Beatty and Shimshack, 2011).

In other words, we hypothesize that the impact of EBC on ASCSs admissions will be negative, so the appropriate alternative hypothesis is a non-negative coefficient.

3. Data and variables

We merge five different sources of register data. From the Norwegian Patient Registry (NPR), we extract information on emergency hospital admissions, patient age, gender, and diagnoses for the period 2010 to 2013. Along with this, the KHUR is a public register administrated by the Norwegian Health Administration (HELFO), which is a subordinate of the Directorate of Health, and used for settling fee-for-service payments to GPs from the National Insurance Scheme.

From this register, we obtain information on the services provided by GPs. Specifically, these include the register records for every GP service that generates a fee, and thus enables us to observe the number of patient visits, patients and their diagnosis (i.e., ICPC-code), and the mix of services provided to each patient, such as medical/diagnostic tests and whether there have been prolonged consultations, etc. Most importantly, these data include patient and GP identifiers, which allow us to merge information on services provided by GPs to the individual patient and GP characteristics.

The GP characteristics, including the GP's age, gender, country of birth, whether specialized or not, list length, are from the GP database, while individual-level socioeconomic conditions such as education, income, living alone and disability status are from the Statistics Norway's database. Finally, we collect data on whether and when emergency beds are available in different municipalities from the Norwegian Directorate of Health.

3.1 Dependent variables

We construct nine dependent variables using the same approach. We create the main dependent variable Em_ASCS by aggregating emergency admissions across the eight ACSCs for each GP in a given year in a given municipality. Dividing by the total number of patients on a GP's list, we interpret the variable as the share of the emergency admission usage of the list patients (multiplying by 100, the interpretation is the percentage of the emergency admission usage of the list patients):

$$Em\_ASCS = \frac{\text{Number of emergency admissions at hospital due to ASCSs by each GP}}{\text{GP's list length}} \times 100.$$

Acknowledging that more than one emergency admission in a given year is possible for the same patient, the variable remains a relevant policy measure as it measures emergency episodes relative to the number of patients on a list.

Policymakers wish to reduce unnecessary admissions to hospitals and we interpret the development of ACSC-related emergency admissions as an indicator of whether the subsidy scheme works. Thus, a higher percentage of emergency admissions per patient in a period of time is "bad", a lower percentage is "good". Furthermore, if the shares of emergency admissions are significantly lower in municipalities establishing EBC compared to those without such bed capacity, the subsidy scheme is working as envisaged.

The ACSCs include the diagnoses of angina, asthma, atrial fibrillation, congestive heart failure, COPD, diabetes (not complicated), epilepsy, and ulcers. The respective dependent variables are:

i) Percentage of emergency admissions at hospital because of angina (*Em_Angina*);

ii) Percentage of emergency admissions at hospital because of asthma (*Em_Asthma*);

iii) Percentage of emergency admissions at hospital because of atrial fibrillation (*Em_Atri*);

iv) Percentage of emergency admissions at hospital because of congestive heart failure (*Em_Heart failure*);

v) Percentage of emergency admissions at hospital because of COPD (*Em_ COPD*)

vi) Percentage of emergency admissions at hospital because of diabetes (*Em_ Diabetes*);

vii) Percentage of emergency admissions at hospital because of epilepsy (*Em_ Epilepsy*);

viii) Percentage of emergency admissions at hospital because of ulcers (*Em_ Ulcer*);

Table 1 details the variable names and their definitions.

<<<<Table 1 about here>>>>

3.2 Control variables

3.2.1 GP attributes

GP characteristics may influence the level of referral to hospital care and, in turn, the percentage of hospital emergency admissions. Therefore, it is important to control for GP characteristics. The GP attributes we consider to be control variables in the analyses include country of birth, whether the GP is from Norway, gender, age, list size (workload), specialization, the number of consultations, the share of long consultations, the share of patient per GP with registered medical or diagnostic tests, and the number of patient visits in a calendar year.

3.2.2 Patient attributes averaged by GP

Patient socio-demographic characteristics also relate to the demand for health care and emergency admissions. The patient attributes we include as control variables in the analyses are patient age and gender, patient socioeconomic characteristics, including education, living conditions (live alone or not), disability status, and wage income. As all analyses are at the GP level, we average these patient attributes for each GP list.

4. Results

4.1 Descriptive statistics

From 2010 onwards, there has been a downward trend in the rate of emergency admissions in aggregate terms (*Em_ASCS*), from 0.96 percentage points in 2010 to 0.90 percentage points in 2013 (Table 2), a reduction of approximately half a percentage point overall. A downward trend also holds for some of the disease-specific rates. However, the COPD rate increases over time (from 0.165 to 0.176 percent), while for ulcers and atrial fibrillation, the rates are stable. The independent variables also appear to be rather stable over time, as shown in Table 2.

<<<<Table 2 about here>>>>

Figures 2–7 depict the aggregate trends for the control group of municipalities and the treatment group, and the same for the disease-specific trends. The trend for *Em_ASCS* is downward for both groups of municipalities. There are some differences in the disease-specific trends, notably for angina, asthma and COPD. For these conditions, the reduction in admission rates is greater in municipalities that have established EBC compared to those which have not.

<<<<Table 3 about here>>>>

Table 3 provides the aggregate averages of *EM_ACSC* for both the control group of municipalities and the treatment group, and for the disease-specific cases. With few exceptions, the average level pre-reform is highest for the control group.

More specifically, the yearly average of *EM_ACSC* for the control group in the pre-reform period (2010–2011) is equal (at the second decimal place, 0.915 to 0.917) to the post-reform period (2012–2013). The treatment group of municipalities experience a reduction in *Em_ASCS* (from 0.874 percentage points to 0.839 percentage points) of approximately half a percent.

There are some notable differences in the disease-specific averages. In the case of COPD (*Em_COPD*), the average emergency admissions percentage is *higher* post-reform compared to the pre-reform average (0.168 versus 0.179, an increase of more than half a percentage point). For the treatment group, the result is opposite: a reduction of 0.01 percentage point (from an average of 0.167 to an average of 0.157). For diabetes (*Em_Diabetes*), the pre-reform average is highest for the treatment group and does not quite catch up to post-reform (0.34 versus 0.35) although the reduction measured in percentage points is clearly in favour of the treatment group (0.002 versus 0.008).

4.2 Difference-in-Differences (DID) estimates

Table 4 reports the results from the DID estimates of the effects of EBC on aggregate emergency ACSCs admissions for alternative models. As discussed in Section 2, to confirm the sensitivity of our results we construct four alternative models conditioned on (i) the definition of the post-reform period and (ii) grouping of the treatment municipalities depending on the date when municipalities introduced EBC locally. To be specific, Models B1 and B2 define treatment municipality on the basis that EBC began before 31 December 2012. In contrast, Models S1 and S2 define treatment municipality as municipalities that began EBC before 30 June 2012. For the post-reform period, the first two models (i.e. Models B1 and S1) both specify the years 2012 and 2013, while Models B2 and S2, include only the year 2013 in the post-reform period.

<<<<Table 4 about here>>>>

As shown in Table 4, regardless of how we define the post-reform period or a treatment municipality, the interaction term *Reform×Treat* (i.e. the estimated coefficient for $\hat{\beta}_3$ in Equation 2) is negative and statistically significant in three of the four models. However, the clustered standard errors are larger for the second two models (S1 and S2) and this could be because these models include comparatively fewer treatment municipalities than in Models B1 and B2. Nonetheless, the absolute magnitudes of the coefficients are close. These results suggest that the CR had a negative effect on the emergency admissions relating to ACSCs. Even if the effects look rather weak, the negative effect is consistent regardless of the alternative definitions of the post-reform period and/or a treatment municipality, i.e. regardless of the previously mentioned combinations of (i) and (ii).

Overall, our DID estimates suggest that the reform reduced the percentage of ACSCs in aggregate by 0.03 percentage points (Table 4). In other words, ACSCs fell by about 0.38 per GP (with an average of 1,223 listed patients per GP). Based on the overall sample average of ACSCs before the reform (0.906), introducing EBC reduces emergency admissions related to ACSCs by around 3.4% ($\text{i.e.} \frac{0.0314}{0.906} \times 100$).

Table 4 shows that most of the control variables in the models are significant. All other things being equal, we find a positive effect on emergency ACSCs admissions for GP characteristics including being a native Norwegian, a male and older GP and negative effects for specialized GP and those GPs providing a larger number of consultations. Patient characteristics (averaged at the GP level) also exhibit a significant relation with emergency admissions for ACSCs with their hypothesized signs. In particular, higher patient age and a larger share of male and less educated patients, and those living alone on a GP's list tend to increase emergency admissions. Conversely, larger average wage income per listed patient tends to decrease emergency admissions.

One question is whether the negative effect of the reform on aggregate ACSCs emergency admission also hold for each ACSC separately. To respond, we re-estimate Equation 2 for each of the ASCS admissions. Table 5 illustrates the results of the eight different diagnoses with the alternate post-reform definitions. As shown in Table 5, save heart failure and ulcers, the CR has a negative impact on emergency admissions for the six remaining diagnoses. However, across all model specifications, we only observe a significant negative impact for two of the diagnoses: angina and COPD. The relatively small number of observations for some of the other diagnoses may be one reason for the statistical insignificance effect of the reform.

<<<<Table 5 about here>>>>

4. 2.1 Robustness check

We perform several tests for robustness. A key assumption of the DID is that of a common or parallel trend. This states that in the absence of treatment, the average outcomes of the treatment group and the control group would follow parallel paths over time. Unfortunately, it is difficult to test this assumption given it is impossible to observe the data after the introduction of the reform. However, pre-reform data may indicate that the trends are identical (Angrist and Pischke, 2009). Within this, we could confirm that the trend in the outcome variable (e.g. ACSCs) for both the treatment and control groups during the pre-reform period are similar. Following common practice, we graphically examine the average

ACSCs emergency admission from 2010 to 2013 to see whether the common trend assumption is satisfied in the years before implementation of the reform in January 2012.

Figure 2 depicts ACSCs hospital admissions over time for both control and treatment municipalities. As shown in Figure 2, average ACSCs admissions decreased during 2010 and 2011 in both control and treatment municipalities and the trends are generally parallel to each other. Nonetheless, to some extent we have already tested the parallel trend assumption when we define the pre-reform year as 2010 and the post-reform year as 2011 and estimate the DID regression using Equation 2. This is one year before the introduction of the actual CR in 2012, and we know of no other particular events at the period that could have systematically affected ACSCs emergency admissions. Thus, if this intervention has any effect, it leads us to suspect that the effects revealed in Tables 4 are spurious, as indicated by the second column in Table 6 and the possible effect of a placebo intervention in the DID setup. There the interaction coefficient (i.e. the DID estimate) is insignificant, which implies that there is no placebo reform effect on ACSC-related admissions during 2010 and 2011.

<<<<**Table 6 about here**>>>>

In our DID analyses, we also identify treatment municipalities as those that introduced EBC by 31 December 2012 and control municipalities as those that did not. We use these differences as an identifying restriction. As a further robustness check, we created treatment municipalities arbitrarily using Norwegian county numbers (see the map in Appendix A). For example, municipalities belonging to the first three counties (1–3) are considered to be in the treatment municipality and the next three (counties 4–6) are considered as control municipalities. Similarly, for counties 7–9 and so forth. Using this placebo process for the treatment and control municipalities, we re-estimate the reform effect using Equation 2. If this placebo treatment municipality has a significantly different effect of actual reform, we could doubt that the introduction of EBC has a causal impact on ASCSs admissions.

As shown in the third column in Table 6, the interaction coefficient (i.e. DID estimate) for the placebo treatment municipality is insignificant. Furthermore, corresponding to Models B2 and S2 (Table 4) we re-estimate the model using the alternative post-reform year specification (i.e. R =1 if year = 2013) for these placebo treatment municipalities. As detailed in the fourth column in Table 6, for this alternative post-reform definition, the interaction coefficient (i.e. DID estimate) of the placebo treatment municipality is also insignificant. These alternative test results also suggest that our DID estimates in Table 4 are robust.

5. Concluding remarks

By implementing the CR in 2012, health authorities in Norway have taken a markedly different strategy than many others in terms of attempting to reduce the number of unnecessary admissions to hospital. It is then of interest to evaluate whether this somewhat old-fashioned way of incentivizing hospitals through subsidization still has some merit. Needless to say, the fundamental motivation for the subsidy schemes resembles that for other jurisdictions outside Norway and their quest for better (and cheaper) disease management. The Norwegian health authorities concede that the current coordination between primary care providers and hospitals is sub-optimal, particularly in relation to the needs of chronically ill patients, for whom primary care can serve as a substitute for hospital care (White Paper, 2008). Thus, our study is of relevance to other health authorities also seeking new ways to promote sound DMP, notably because our study is a causal one. In addition, our chosen instrument, the establishment of an EBC, represents a direct mechanism or explicit tool through which municipalities can work in that it represents bed capacity available at no explicit cost for nursing homes, GPs and others. Given low occupancy rates, the utilization of

EBC capacity seems to have been off to a slow start. However, our analytical results suggest a promising direction to future emergency admissions rates at hospitals for ACSCs. Our identification strategy may raise some concerns but tests concerning the robustness of our estimates (placebo reform and placebo treatment) presented in table 6, suggest that our estimates are robust and that the effect is causal.

Many definitions of DMPs exist reflecting different approaches to disease management across jurisdictions (Tsiachristas et al., 2011), but DMP is basically about preventing the onset of chronic disease and providing primary care that can reduce the likelihood of hospitalization for people that already suffer a chronic condition. To put things bluntly, this is not a trivial issue because, in a global context, chronic diseases are the largest cause of death (Yach et al., 2004; Abegunde et al., 2007). From a narrower viewpoint, for the US alone, chronic health conditions are the leading cause of death and disability and represent the largest component of health care costs (Centre for Diseases Control and Prevention, 2016). In Europe, the significance of chronic diseases in terms of deaths and healthcare costs are also well established (Reinhardt, 2010). Thus, the prevention of chronic disease and the reorientation of the provision of care to support people with chronic conditions can bring about reductions in premature deaths, increase their quality of life, and reduce the growth rate in costs associated with hospital care.

The use of subsidies to incentivize municipalities in establishing emergency bed capacity within primary care, a novelty in the primary care sector in Norway, at first appears as a somewhat dated strategy. Compared to P4P schemes introduced elsewhere, subsidies are not very sophisticated as they imply only de facto additional bed capacity. P4P schemes, on the other hand, introduce more or less elaborate quality indexes aimed at rewarding changes in those indexes complementary to the goals of health authorities. Research thus far show only limited effects, if any, on hospital admissions, following the implementation of P4P schemes.

The use of P4P has been growing worldwide in the last decade. However, there is no consensus in the economic literature concerning the efficacy, applicability and optimal implementation of P4P schemes. Obviously, detailed theoretical and empirical studies are required to make a justified assessment of such schemes. However, our analysis shows that authorities have other tools in the policy toolbox readily available. Consequently, the reduction in emergency admissions is within reach, given a comparable institutional context, without relying on what are admittedly more sophisticated yet indirect incentive schemes. Acknowledging that we have not performed a complete welfare analysis by explicitly calculating the cost and benefits of the subsidy scheme, emergency admissions at hospitals are typically high-cost activities while EBC locally typically costs less. Thus, our findings are necessary but not sufficient conditions for the net positive welfare effect of the subsidy scheme.

Interestingly, rates of emergency department admissions at hospitals for so-called ACSCs are sometimes indicators of the quality of primary–specialist care coordination. Our results indicate that emergency bed capacity locally leads to a reduction in aggregate emergency department admissions at hospital for a set of ACSCs. The disease-specific analyses show that the introduction of an EBC in primary care significantly reduces the rates of emergency department admissions for angina, COPD and asthma. In addition, emergency department admissions at hospitals are likely to be more costly in terms of both direct costs (wages) and indirect costs (draw resources away from other patients, longer waiting lists) compared to EBC in primary care. In this lies a potential welfare gain for the reform.

To the extent that patients with ACSCs are at least as well off concerning their health condition after treatment (no higher readmissions or death rates, etc.) in the primary care setting rather than at hospital, the welfare gain is even more likely to be positive. This could be a topic for future research. Our ambition is to examine the welfare effects in detail in a future paper that brings together both the level of subsidies and the running expenses of EBC locally into the analysis.

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Table 1: Definition of the Variable

Variable name	Definition of the Variable
Dependent Variables	
Em_ASCS	$= \frac{\text{Number of emergency admissions at hospital due to ASCS by each GP}}{\text{GP's list length}} * 100$
Em_Angina	$= \frac{\text{Number of emergency admissions at hospital due to Anginaby each GP}}{\text{GP's list length}} * 100$
Em_Asthma	$= \frac{\text{Number of emergency admissions at hospital due to Asthma by each GP}}{\text{GP's list length}} * 100$
Em_Diabetes	$= \frac{\text{Number of emergency admissions at hospital due to Diabetes without complications by each GP}}{\text{GP's list length}} * 100$
Em_COPD	$= \frac{\text{Number of emergency admissions at hospital due to COPD by each GP}}{\text{GP's list length}} * 100.$
Em_Heart failure	$= \frac{\text{Number of emergency admissions at hospital due to heart failure by each GP}}{\text{GP's list length}} * 100$
Em_Atrial	$= \frac{\text{Number of emergency admissions at hospital due to Artial fibrillation by each GP}}{\text{GP's list length}} * 100$
Em_Epilepsy	$= \frac{\text{Number of emergency admissions at hospital due to Epilepsy by each GP}}{\text{GP's list length}} * 100.$
Em_Ulcer	$= \frac{\text{Number of emergency admissions at hospital due toUlcer by each GP}}{\text{GP's list length}} * 100$
Independent Variables	
Norw_GP	Whether GP comes from Norway=1, otherwise=0
Male_GP	Whether GP is a male=1, female=0
Specialist	Whether GP is a specialist=1; otherwise=0
Age_GP	GP's age in year
Consult_GP	Number of GP's consultation
Share_LC_GP	Share of patient per GP with long consultation
Share_Test_GP	Share of patient per GP done with medical/diagnostic test
Visist_GP	Number of patient visit per GP
Pat_Age	Patient average age per GP
Pat_Mal	Share of male patient per GP
Pat_Edu1	Share of patient per GP with elementary level of education
Pat_Alone	Share of patient per GP with live alone
Pat_Disable	Share of patient per GP with disability
Pat_Wage	Average early wage income per GP divided by 1000
Angina	if Angina=1
Asthma	if Asthma=1
Diabetes	if Diabetes without complications=1
COPD	if COPD=1
Heart	if Heart failure =1
Atrial fibrillation	if Atrial fibrillation=1
Epilepsy	if Epilepsy=1
Ulcer	If Ulcer==1

Table 2: Descriptive statistics for the variables used in the analyses: 2010-2013

Variable	**2010** (N=28 312)		**2011** (N=29 064)		**2012** (N=29 460)		**2013** (N=29 936)	
	Mean (%)	Std. Dev. (%)	Mean (%)	Std. Dev. (%)	Mean (%)	Std. Dev. (%)	Mean (%)	Std. Dev. (%)
Em_ASCS	0.936	0.525	0.882	0.575	0.908	0.590	0.901	0.592
Em_Angina	0.175	0.187	0.156	0.182	0.152	0.197	0.148	0.192
Em_Astma	0.063	0.101	0.056	0.094	0.049	0.101	0.047	0.086
Em_COPD	0.165	0.223	0.171	0.253	0.174	0.234	0.176	0.245
Em_Heart failure	0.174	0.188	0.158	0.190	0.165	0.193	0.162	0.176
Em_Diabetic	0.040	0.068	0.035	0.063	0.035	0.071	0.035	0.069
Em_Ulcer	0.006	0.024	0.005	0.022	0.006	0.025	0.006	0.024
Em_Atrial	0.231	0.209	0.220	0.229	0.242	0.232	0.248	0.257
Em_Epilepsy	0.083	0.143	0.081	0.136	0.085	0.139	0.079	0.124
Norw_GP	0.710	0.454	0.702	0.457	0.695	0.460	0.692	0.462
Male_GP	0.665	0.472	0.657	0.475	0.646	0.478	0.638	0.481
Specialist	0.652	0.476	0.653	0.476	0.662	0.473	0.658	0.474
Age_GP	49.70	10.06	49.72	10.30	49.78	10.45	49.70	10.61
Consult_GP	3.409	1.290	3.352	1.297	3.355	1.156	3.326	1.127
Share_LC_GP	0.354	0.186	0.350	0.182	0.363	0.183	0.378	0.185
Share_Test_GP	0.574	0.145	0.584	0.142	0.589	0.139	0.586	0.140
Visist_GP	9.057	10.12	8.890	10.14	8.705	9.896	8.451	9.401
Pat_Age	51.23	24.92	51.23	25.20	51.54	24.72	52.39	24.84
Pat_Mal	1.451	0.310	1.447	0.316	1.447	0.318	1.448	0.320
Pat_Edu1	0.340	0.114	0.329	0.109	0.325	0.108	0.317	0.107
Pat_Alone	0.308	0.093	0.304	0.087	0.305	0.087	0.307	0.088
Pat_Disable	0.158	0.071	0.153	0.068	0.150	0.067	0.158	0.071
Pat_Wage	145.8	55.67	153.79	55.39	162.63	61.48	158.65	61.19
Angina	0.124	0.330	0.125	0.330	0.125	0.331	0.125	0.330
Astma	0.125	0.331	0.124	0.330	0.125	0.331	0.126	0.331
Kols	0.125	0.330	0.125	0.331	0.124	0.330	0.125	0.331
KrHjSvikt	0.125	0.331	0.126	0.332	0.124	0.330	0.125	0.330
Diabets_NC	0.125	0.331	0.124	0.330	0.125	0.331	0.126	0.332
Ulcer	0.126	0.332	0.125	0.331	0.125	0.331	0.125	0.330
Atrial_F	0.125	0.331	0.126	0.331	0.125	0.330	0.125	0.331
Epilepsi	0.125	0.331	0.125	0.330	0.127	0.332	0.125	0.330

Table 3: Descriptive statistics of the dependent variables for Control and Treatment municipalities: Before and After Reform (with alternative definitions)

Variable	***Before Reform***		***After Reform***			
	Year 2010 & 2011		***Year* 2012 & 2013**		**Year 2013**	
	Control (n=47 612)	Treatment (n=9 320)	Control (n=49 220)	Treatment (n=9 772)	Control (n=24 720)	Treatment (n=4 980)
	Mean (Std. Dev.)	**Mean** (Std. Dev.)	**Mean** (Std. Dev.)	**Mean** (Std. Dev.)	**Mean** (Std. Dev.)	**Mean** (Std. Dev.)
Em_ASCS	0.915 (0.561)	0.874 (0.498)	0.917 (0.604)	0.839 (0.517)	0.913 (0.609)	0.839 (0.493)
Em_Angina	0.167 (0.188)	0.156 (0.166)	0.153 (0.199)	0.133 (0.169)	0.151 (0.199)	0.130 (0.152)
Em_Astma	0.059 (0.098)	0.061 (0.097)	0.049 (0.095)	0.044 (0.087)	0.048 (0.085)	0.043 (0.092)
Em_COPD	0.168 (0.243)	0.167 (0.218)	0.179 (0.244)	0.157 (0.215)	0.180 (0.253)	0.158 (0.197)
Em_Heart failure	0.168 (0.193)	0.157 (0.167)	0.164 (0.184)	0.159 (0.190)	0.164 (0.176)	0.153 (0.173)
Em_Diabetic	0.036 (0.065)	0.043 (0.070)	0.034 (0.071)	0.039 (0.065)	0.033 (0.069)	0.043 (0.067)
Em_Ulcer	0.006 (0.023)	0.005 (0.022)	0.006 (0.024)	0.005 (0.027)	0.006 (0.023)	0.005 (0.027)
Em_Atrial	0.229 (0.223)	0.207 (0.200)	0.248 (0.251)	0.227 (0.212)	0.251 (0.264)	0.234 (0.219)
Em_Epilepsy	0.083 (0.142)	0.078 (0.128)	0.083 (0.135)	0.075 (0.109)	0.081 (0.127)	0.073 (0.108)

Table 4: Effect of the emergency bed capacity within primary care on ACSC Hospital Admission: Difference-in-differences estimates with *alternative combinations of before and after reform years (cluster standard errors in municipalities are in the parentheses)*

	Model B1	**Model B2**	**Model S1**	**Model S2**
	Reform=1 (year = 2012 & 2013) Reform=0 (year =2010 & 2011)	*Reform*=1 (year = 2013) Reform=0 (year 2010 & 2011)	*Reform*=1 (year 2012-2013) Reform=0 (year =2010 & 2011)	*Reform*=1 (year = 2013) Reform=0 (year 2010 & 2011)
Reform	0.0024	-0.0073	0.0016	-0.0112
	(0.0044)	(0.0118)	(0.0104)	(0.0111)
Treat$^{\Phi}$	0.7419***	-0.1363***	0.1612***	0.1392***
	(0.0211)	(0.0240)	(0.0203)	(0.0209)
Reform*Treat	**-0.0314****	**-0.0281***	**-0.0331**	**-0.0281***
	(0.0190)	**(0.0199)**	**(0.0330)**	**(0.0212)**
Control variables				
Norw_GP	0.0402***	0.0415***	0.0401***	0.0415***
	(0.0126)	(0.0153)	(0.0126)	(0.0153)
Male_GP	0.1432***	0.1557***	0.1432***	0.1557***
	(0.0118)	(0.0117)	(0.0118)	(0.0117)
Specialist	-0.0297**	-0.0232*	-0.0296**	-0.0232*
	(0.0136)	(0.0139)	(0.0136)	(0.0139)
Age_GP	0.0033***	0.0030***	0.0033***	0.0030***
	(0.0006)	(0.0007)	(0.0006)	(0.0007)
Consult_GP	-0.0034	-0.0024	-0.0034**	-0.0023
	(0.0041)	(0.0045)	(0.0041)	(0.0045)
Share_LC_GP	-0.0090	0.0164	-0.0090	0.0164
	(0.0306)	(0.0298)	(0.0306)	(0.0298)
Share_Test_GP	0.1414***	0.1285***	0.1418***	0.1289***
	(0.0391)	(0.0426)	(0.0390)	(0.0426)
Visist_GP	0.0084***	0.0084***	0.0084***	0.0084***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Pat_Age	0.0017***	0.0017***	0.0017***	0.0017***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Pat_Male	0.0131**	0.0104	0.0130**	0.0103
	(0.0067)	(0.0067)	(0.0066)	(0.0067)
Pat_Edu1	0.0679	0.0701	0.0681	0.0707
	(0.0848)	(0.0836)	(0.0848)	(0.0836)
Pat_Alone	0.0723	0.0606	0.0729	0.0611
	(0.0802)	(0.0762)	(0.0802)	(0.0762)
Pat_Disable	-0.0917	-0.0710	-0.0927***	-0.0716
	(0.0977)	(0.1010)	(0.0299)	(0.1010)
Pat_Wage	-0.0001***	-0.0014***	-0.0014***	-0.0014***
	(0.0000)	(0.0001)	(0.0001)	(0.0001)
Number of observation	98 976	74 015	98 976	74 015
R-squared	0.36	0.36	0.36	0.36
Year fixed-effects	Yes	Yes	Yes	Yes
Municipality fixed-effects	Yes	Yes	Yes	Yes

Note: $^{\Phi}$ In Models B1 and B2: *Treat*=1 if municipality implemented emergency bed by December 31, 2012.

In Models S1 and S2: *Treat*=1 if municipality implemented emergency bed by June 30 2012.

DID coefficients (i.e. Reform*Treat) tested against one-sided alternatives.

'*', '**' and '***'represents significance level at the 10%, 5% and 1% level respectively.

Table 5: Effect of the establishing emergency bed capacity within primary care on ACSCs Hospital Admission: *Difference-in-differences* estimates for specific diagnoses with alternative before and after reform years *(cluster standard errors in municipalities are in the parentheses)*

Variable	Angina	Asthma	Diabetes	COPD	Heart Failure	Atrial_F	Epilepsi	Ulcer
Reform	-0.005	-0.004*	-0.003**	0.022***	-0.011**	0.004	0.009**	0.000
	(0.005)	(0.002)	(0.002)	(0.005)	(0.005)	(0.007)	(0.004)	(0.003)
Treat	-0.158***	0.153***	0.003	0.226***	0.370**	0.012*	0.014	0.068***
	(0.015)	(0.008)	(0.003)	(0.013)	(0.017)	(0.009)	(0.006)	(0.011)
Reform*Treat¥	**-0.012***	**-0.006**	**-0.002**	**-0.016***	**0.009**	**-0.004**	**-0.005**	**0.003**
	(0.009)	**(0.005)**	**(0.003)**	**(0.012)**	**(0.008)**	**(0.010)**	**(0.006)**	**(0.006)**
Number of observation	13 110	13 935	14 334	13 721	12 954	14 336	13 871	2 715
R-squared	0.45	0.19	0.12	0.26	0.27	0.31	0.21	0.17
Reform1	-0.004	-0.004*	-0.005***	0.027***	-0.012***	0.011**	0.004	-0.001
	(0.005)	(0.003)	(0.002)	(0.006)	(0.005)	(0.006)	(0.004)	(0.003)
Treat	0.162***	0.155***	0.024	-0.209***	0.025*	-0.298***	-0.047***	0.005
	(0.016)	(0.008)	(0.003)	(0.035)	(0.015)	(0.029)	(0.010)	(0.015)
Reform1*Treat±	**-0.009**	**-0.006**	**0.004**	**-0.019***	**0.002**	**-0.001**	**-0.005**	**0.005**
	(0.011)	**(0.007)**	**(0.004)**	**(0.014)**	**(0.008)**	**(0.010)**	**(0.006)**	**(0.009)**
Number of observation	9 816	10 427	10 724	10 263	6 442	10 731	10 340	20 42
R-squared	0.47	0.17	0.17	0.28	0.31	0.29	0.22	0.19

Note: ¥*Reform*=1 if after Reform includes year 2012-2013; Reform=0 if before Reform includes year 2010-2011.

Treat=1 if municipality implemented emergency bed by December 31, 2012.

±*Reform1*=1 if after Reform includes year 2013; Reform13=0 if before Reform includes year 2010-2011

All the models are also control for the variables included in table 4.

DID coefficients (i.e. Reform*Treat/ Reform1*Treat) tested against one-sided alternatives.

'*', '**' and '***'represents significance level at the 10%, 5% and 1% level respectively.

Table 6: *Difference-in-differences* estimates for “placebo” reform effect on ACSCs Hospital Admission: construct artificial/placebo treatment municipalities and placebo reform year*(cluster standard errors in municipalities are in the parentheses)*

Variable	Placebo reform effect¥	Artificial/placebo treatment municipalities ᶲ*Reform*=1 (year = 2012 & 2013) Reform=0 (year =2010 & 2011)	Artificial/placebo treatment municipalities ᶲ*Reform*=1 (year = 2013) Reform=0 (year =2010 & 2011)
Placebo_Reform	-0.0438*** (0.110)	---	---
Treat	-2.743*** (0.295)	---	---
Placebo_Reform*Treat	**-0.0072 (0.0247)**	---	---
Reform	---	-0.0008 (0.0121)	---
Placebo_Treat	---	0.7305*** (0.0195)	---
Reform* Placebo_Treat	---	**-0.0040 (0.0154)**	---
Reform1±	---	---	-0.0137 (0.0127)
Placebo_Treat	---	---	-1.044*** (0.0547)
Reform1* Placebo_Treat	---	---	**-0.0037 (0.0175)**
Number of observation	48 652	98 976	74 015
R-squared	0.34	0.36	0.36
Year fixed-effects	Yes	Yes	Yes
Municipality fixed-effects	Yes	Yes	Yes

Note: ¥Placebo reform=1, if Year=2011 and Placebo reform=0, if year=2010

ᶲ the *Placebo treatment* municipalities are created arbitrarily/randomly using Norwegian county numbers (see the map in Appendix A). For example, municipalities belongs to the first three counties (1-3) are considered to be in the treatment municipality and the next 3 (4-6) considered as control municipality, and doing the same procedure for the rest of the counties.

±*Reform1*=1 if after Reform includes year 2013; Reform13=0 if before Reform includes year 2010-2011

All the models are also control for the variables included in table 4.

DID coefficients (i.e. Reform*Treat /Reform* Placebo_Treat/ Reform1* Placebo_Treat) tested against one-sided alternatives.

‘*’, ‘**’ and ‘***’represents significance level at the 10%, 5% and 1% level respectively.

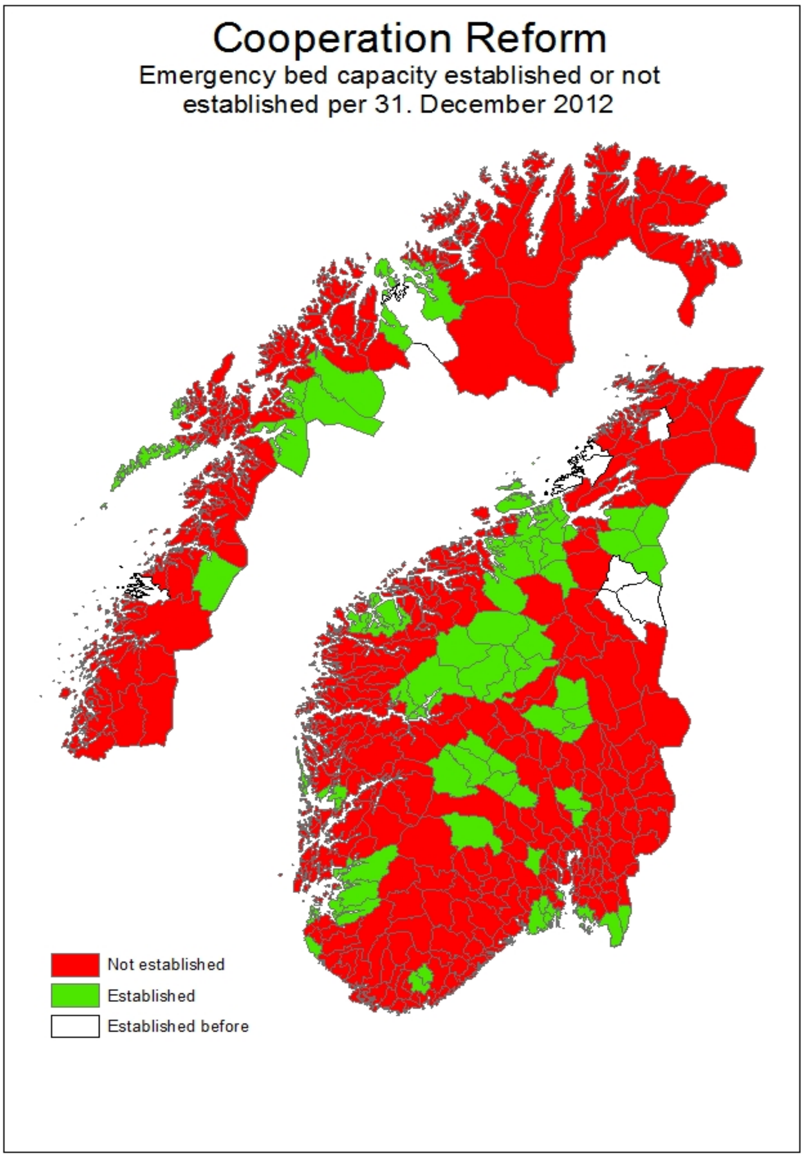

Figure 1: Status of the municipalities in respect to EBC established or not.

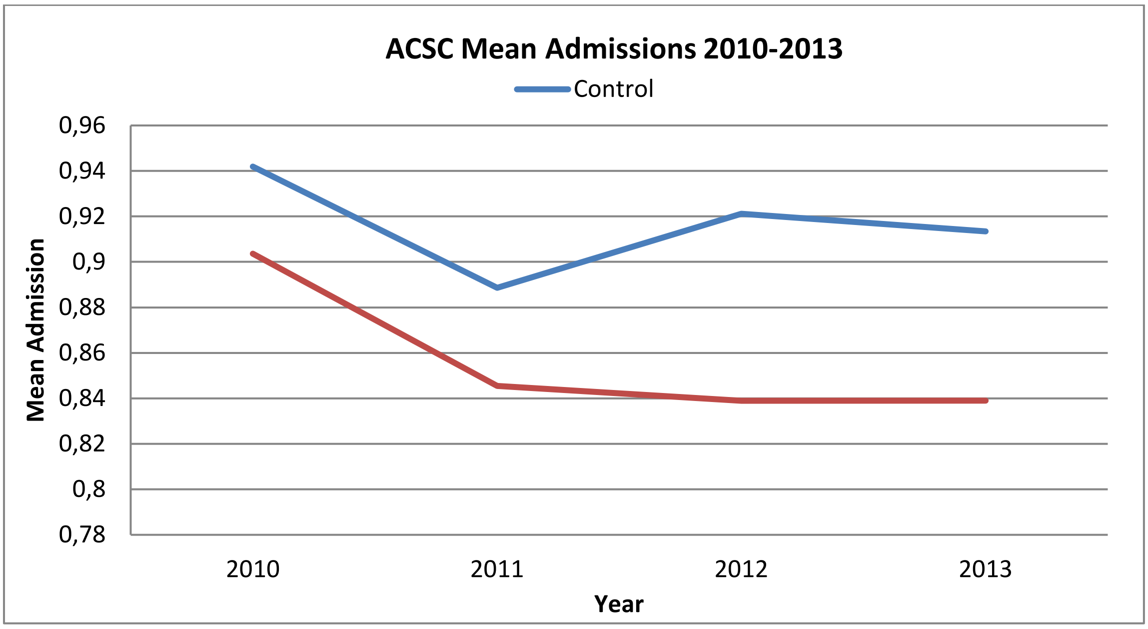

Figure 2: ACSCs hospital admissions over the year for control and treatment municipality.

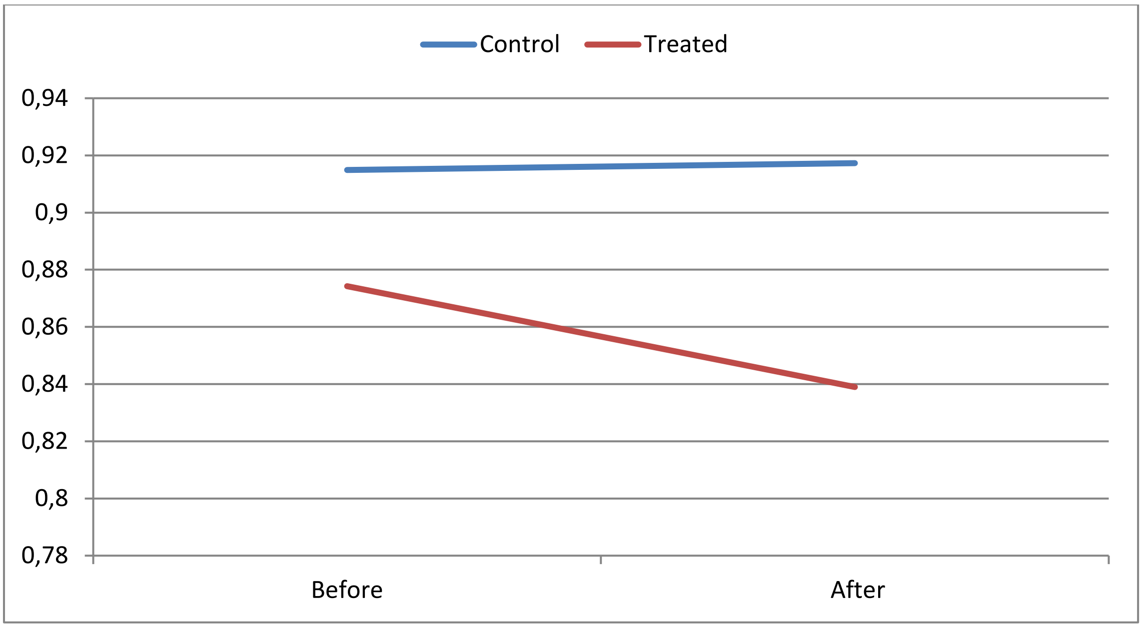

Figure 3: ACSCs hospital admissions before and after reform in control and treatment municipality.

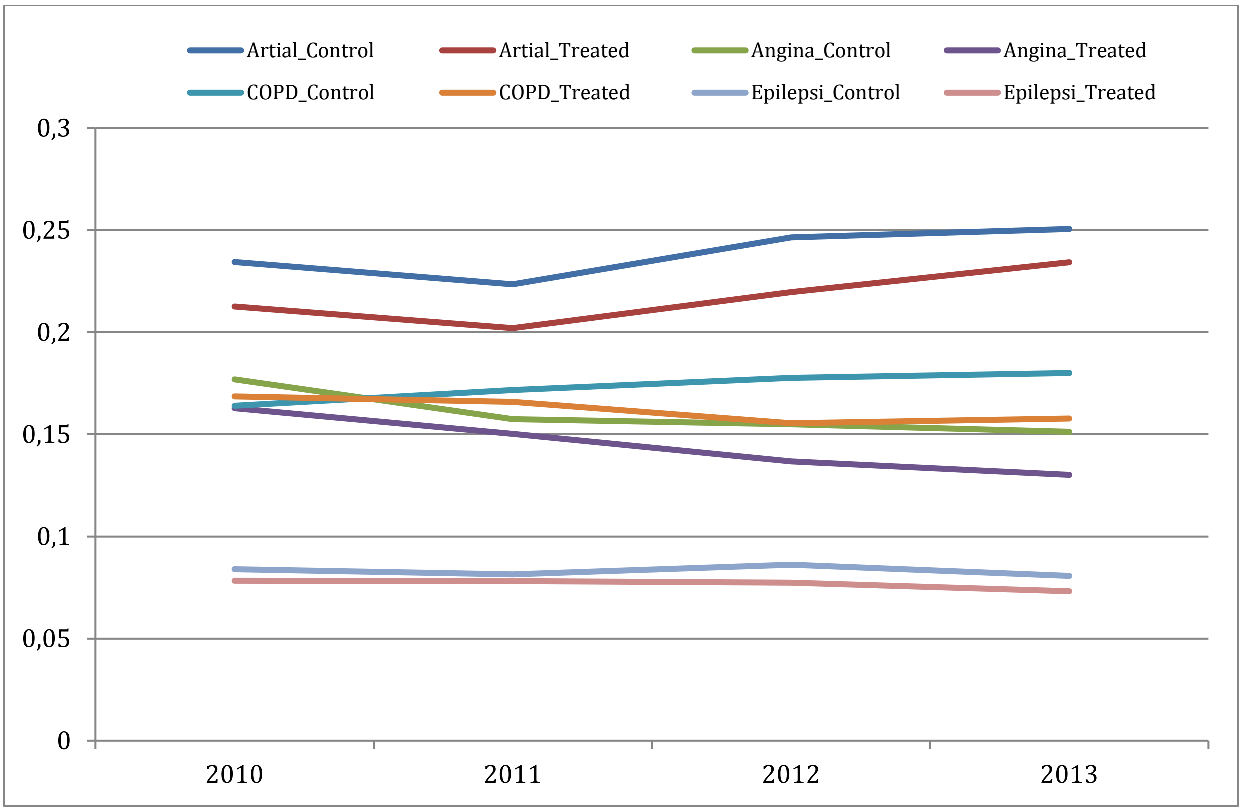

Figure 4: Atrial F, Angina, COPD and Epilepsy emergency hospital admissions over the year for control municipality (control) and treatment municipality.

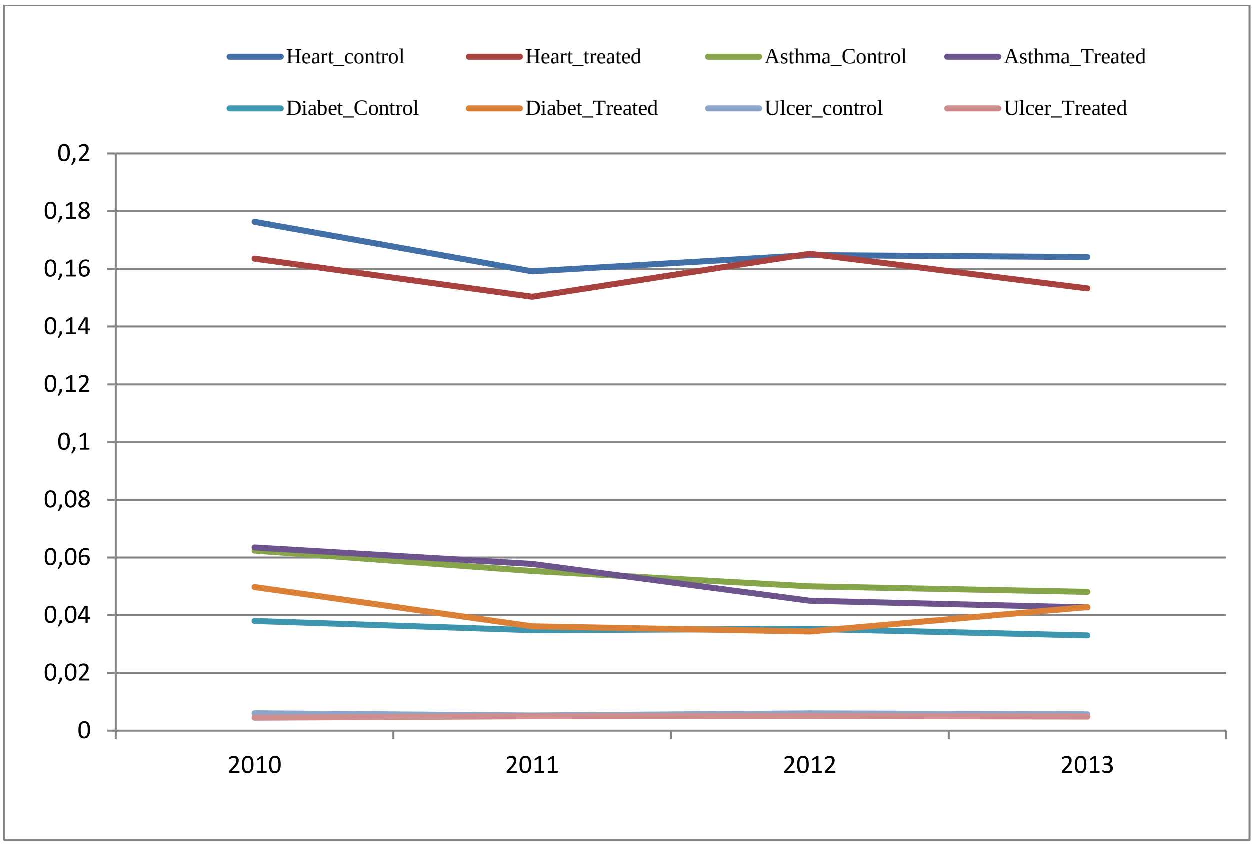

Figure 5: Heart failure, Asthma, Diabetes and Ulcer emergency hospital admissions over the year for control municipality (control) and treatment municipality.

Appendix A

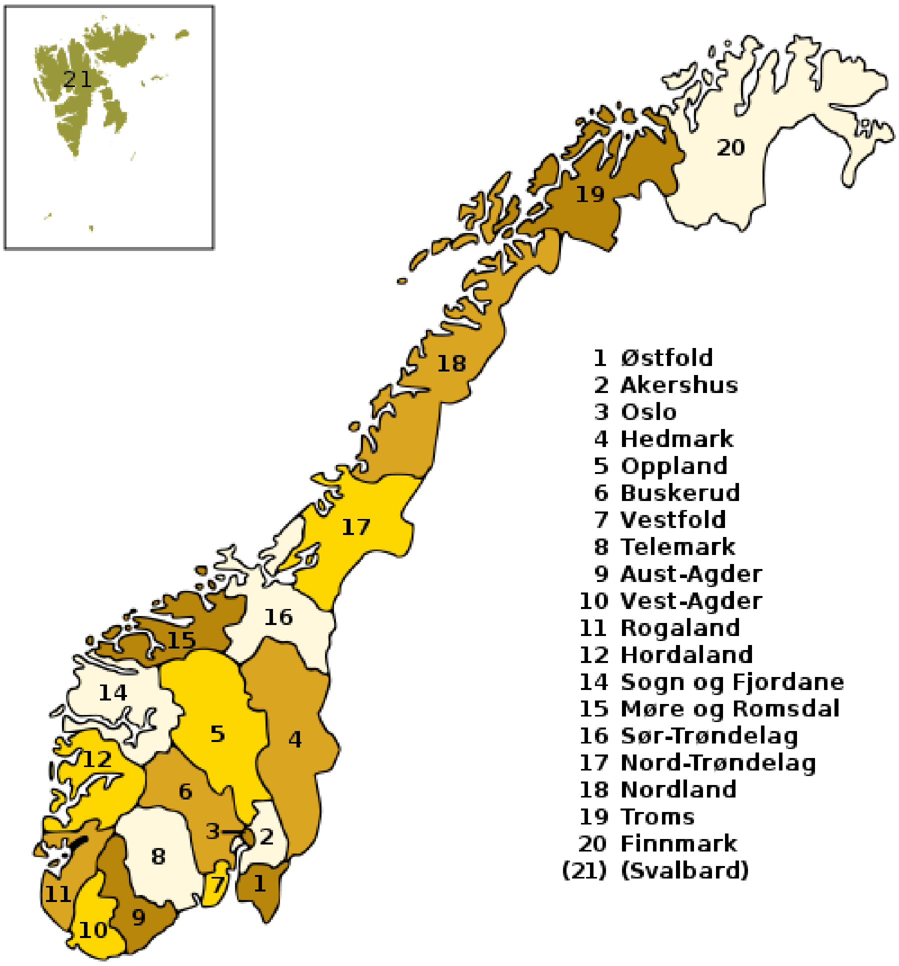

Figure A1: Counties of Norway (source: https://en.wikipedia.org/wiki/Counties_of_Norway)

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