Competition, capitation and coding: do public primary care providers respond to increased competition?

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Competition, capitation and coding: do public primary care providers respond to increased competition?

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ABSTRACT

The case for competition in healthcare markets rests on economic models in which providers seek to maximize profits. However, little is known regarding how public healthcare providers, who might not have a profit motive, react to increased competition from private providers. This study considers the heterogeneous effects of a primary healthcare reform in a Swedish region that considerably loosened entry restrictions and increased patients’ freedom of choice, thus enabling increased competition. Our difference-in-differences analysis contrasts local markets that were affected by both entry and choice with local monopoly markets, which were unaffected by the reforms. Using detailed administrative data on all visits to public health centers in 2008-2011, we find that providers in markets with increasing competition registered more diagnoses in an administrative database, thus increasing their reimbursement per patient. Although the economic significance of the effect is small, the result suggests that public providers are indeed sensitive to competition.

1. Introduction

Dissatisfaction with the performance of public healthcare services has led to political interest in policies that promote competition in many countries (Gaynor et al., 2015). The case for competition relies on economic models in which healthcare providers are assumed to be maximizing profits, possibly with some regard to patients’ wellbeing (Brekke et al., 2014; Gaynor et al., 2015). Whether the predictions of these models carry over to agencies whose main objective is not profit maximization – such as public healthcare providers – is not certain (Duggan, 2000). Given the prevalence of public providers in healthcare, it is of key policy interest to examine how such providers react to competition.
This study exploits plausibly exogenous variation due to a policy reform in a Swedish region (Skåne) to examine if greater exposure to competition affects public primary care providers’ strategic decisions. The reform, which established free entry for private providers and free choice of provider for patients, implied a surge in the number of private providers in areas where there were already several providers to choose from, but had hardly any effect on monopoly markets. Thus, we contrast public providers operating in these two kinds of markets in a difference-in-differences (DID) analysis.

In our study setting, providers cannot respond to competition by lowering patient fees, which are regulated by the healthcare authority. However, providers do have some degrees of freedom to influence the size of the public reimbursement per registered patient, the capitation, which is positively affected by the number of diagnoses physicians register for their patients in an administrative database. The idea behind the system is to compensate providers for the expected care need of their patients. However, for providers pressed by competition, the system clearly implies an incentive to register more diagnoses than otherwise. Registering diagnoses entails no direct costs for the provider, and does not compromise medical ethics as the administrative database is not linked to the patients’ medical records. Using a unique dataset containing all visits to public group practices (henceforth denoted “health centers”) between 2008 and 2011 by working age adults, we examine if the number of registered diagnoses per visit increased more for patients visiting health centers subject to increased competition due to the reform.

Whether to expect any response to competition among public providers is not obvious. In an empirical analysis of for-profit, not-for-profit, and public healthcare hospitals in the United States, Duggan (2000) suggested that the main dividing line is that public hospitals have soft budget constraints and thus are insensitive to financial incentives.¹ Arguably, the public health centers in Skåne also operate in a soft budget constraint environment. Their owner – the regional government – has the authority to raise taxes and take up loans that may be viewed as implicitly guaranteed by the national government (von Hagen and Dahlberg, 2004); moreover, the region can cross-subsidize its own providers (Konkurrensverket, 2014). Further, physicians as well as managers are salaried and thus

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¹ Although profit maximizing behavior is not a priori incompatible with a soft budget environment, it is not obvious why it would persist in such circumstances. Brekke et al. (2015) develop a model of hospital competition under a soft budget constraint, assuming hospitals are profit-maximizers. However, they do not consider whether the profit-maximization assumption is tenable.
have no claims on any residual income. The political importance of showing a decent financial performance is probably the prime reason why public health centers would react to competition in our study setting.  

After reviewing related literature in section 2, we give an institutional background of the primary care sector in Skåne, the choice and entry reform, the incentives for diagnosis registrations and the structure of the primary care market before and after the reform in section 3. Our empirical strategy and data are described in sections 4 and 5 and the results in section 6. Section 7 provides a concluding discussion.

2. Related literature

The literature on competition in primary care is very small, and our study is the first to focus on public providers’ responses to competition. There are two previous causal studies of the Swedish choice and entry reforms, both of which have used similar identification strategies as the present study (Dietrichson et al., 2016; Fogelberg, 2014). Fogelberg found an increase in antibiotics prescriptions in more competitive areas, albeit only in county councils where providers had no financial incentives to restrict prescriptions. Dietrichson et al. detected small positive effects of competition on overall patient satisfaction, but no effects on the avoidable hospitalization rate or on patient satisfaction with waiting times. While these results suggest that Swedish primary care providers sometimes do react to competition, it is possible that private providers drive the effects.

Outside Sweden, primary care is typically provided by self-employed general practitioners (GPs), for whom profit-maximizing behavior is a plausible model. Theoretically, the effects of competition depend on whether prices are regulated or not. In the former case, healthcare providers compete on care quality, broadly defined, which is thus expected to improve following increased competition. When prices are not regulated, the effect on quality is ambiguous as there is also another dimension, price, on which providers may compete (Gaynor et al., 2015). In Norway, GPs subject to increasing competition have been shown to react by making more referrals to specialists, increasing the length of consultations, prescribing more addictive drugs and issuing more sickness

\footnote{At least anecdotally, the budget deficit is a central measure of health center managers’ performance.}
absence certifications (Brekke et al., 2017; Iversen and Lurås, 2000; Iversen and Ma, 2011; Kann et al., 2010; Markussen and Røed, 2016). These are all outcomes that patients may be interested in, and thus indicative of GPs’ efforts to please their patients in a setting where prices are regulated. In England, where prices are fixed too, greater competition (more rival GPs nearby) is associated with better process quality, fewer avoidable hospitalizations and higher patient ratings, though the magnitudes of the effects are small (Gravelle et al., 2018). Studies from the United States (US) and Australia, where GPs compete not only on quality but also on price, indicate that competition is associated with lower prices (Gravelle et al., 2016; Schneider et al., 2008) and that competition may lead to lower quality (measured by certain procedure rates and consultation length) in such a setting (Gravelle et al., 2016; Johar et al., 2014).

Outside the primary care sector, studies from the US indicate that greater competition among hospitals is associated with lower prices. Studies from the US and England have confirmed the theoretical distinction between settings with and without regulated prices in terms of the competition effect on quality, measured by mortality (Gaynor et al., 2013; Gaynor and Town, 2011; Moscelli et al., 2018b). However, recent studies from England have found negative effects of competition on other quality measures (readmissions and patient-reported outcomes), suggesting that the effect may vary across outcomes (Moscelli et al., 2018a; Skellern, 2017).

Our study is also related to the literature on coding practices in activity-based hospital payment systems, e.g., payment based on Diagnosis Related Groups (DRG). Such systems embody similar incentives for healthcare providers to upcode patients, i.e., to classify patients as belonging to a more severe disease category than what is medically warranted in order to receive higher compensation. Empirical studies from the US indicate that for-profit hospitals are especially likely to upcode (Silverman and Skinner, 2004; Steinbusch et al., 2007). Notably though, empirical studies of public hospitals in Nordic countries have documented associations between DRG prices or payment and coding practices (Anthun et al., 2017; Serdén et al., 2003).

3. Institutional background

Region Skåne is one of Sweden’s 21 independent and locally elected county councils, which organize the public healthcare system. In Skåne, as well as in the rest of the country,
health care is mainly financed by a proportional income tax, complemented by user fees (subject to a low annual cap) which are set by the healthcare authority.

The lion’s share of primary care is provided by multi-professional health centers. The health centers are staffed with a handful of salaried GPs, nurses, and – to varying extent - other professional categories such as behavioral therapists and physiotherapists. Today, the region owns and operates roughly half of all health centers.

In 2007-2009, a set of Swedish county councils – including Skåne – voluntarily implemented reforms that established free entry for private providers and enabled patients to choose freely among primary care providers. The motivation behind the reforms was to improve access to primary care and responsiveness to patients (Anell, 2015).3 From the reform date (May 1, 2009 in Skåne) onwards, any care provider that fulfilled the conditions for accreditation specified by the county council would be allowed to enter the market, providers would no longer be allowed to reject patients who wished to enroll, and patients would be allowed to choose among all health centers in the county. Switching costs were also lowered by the reform, in the sense that choice forms were made available at all health centers and on the health authorities’ joint website (www.1177.se), where it also became possible to switch providers online. Over time, the amount of publicly available information about providers also increased as the website was augmented with patient satisfaction ratings and information about special competencies (Anell et al., 2017; Dietrichson et al., 2016). Already before the reform, there was a limited form of patient choice in Skåne, but providers were allowed to reject patients and had responsibility only for patients living in their administratively determined uptake area. Furthermore, there was very little information about providers available to patients. Potential entrants had to negotiate with the region, which could veto new establishments.

Another prominent part of the reform in Skåne was the redesign of the reimbursement system. Prior to the reform, approximately 80% of the reimbursement was prospective capitation based on the number of inhabitants in each center’s uptake area, and 20% was retrospective fee-for-service (FFS). Since the reform, there is no longer any FFS (except

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3 These local reforms later inspired the national government to mandate similar policies in all counties by 2010.
for visits made by patients listed at other centers) and the capitation is tied to the actually enrolled patients. To account for variations in expected costs, the capitation is higher for patients with many diagnoses or low socioeconomic status. Patients’ morbidity weight is calculated using the Johns Hopkins Adjusted Clinical Groups (ACG) system,\(^4\) based on all diagnoses registered during the past 18 months in the region’s administrative care database. Providers thus have a stronger incentive to register diagnoses than before the reform. Acknowledging this incentive, the region conducted two medical audits of registration practices in 2012-13. Two private health centers were warned for inappropriate registrations. No public units were warned; in fact, the audit concluded that some failed to register diagnoses even when it was warranted (Glenngård, 2015).

In Skåne (as well as in the rest of Sweden), patient fees are regulated and thus providers cannot compete on price. Patient fees account for only 4.5% of total revenues and are discounted from the capitation.

**Health centers in Skåne**

Figure 1a shows that there were approximately 125 health centers before the reform, and that 20 new centers opened up on May 1 2009.\(^5\) Figure 1b shows the number of public health centers. Three public health centers were closed shortly before the reform, but immediately replaced by private centers. Two new public centers were established at the reform date (replacing a previous nurse-staffed center, *Sw. distriktssköterskemottagning*), but no new public centers have opened since.

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\(^4\) [www.hopkinsacg.org](http://www.hopkinsacg.org)

\(^5\) The figure does not include subsidiary units (10 units) or private solo practices on separate contracts from the time before the patient choice system (21 units). Neither type of the excluded units has listed patients. Private solo practices are reimbursed by FFS.
a) All health centers (public and private)  
b) Publicly owned health centers

**Figure 1.** Number of health centers in Skåne by month. The vertical dashed line indicates the reform month (May 2009). Subsidiary units and private solo-practices are excluded. Source: own data collection from 1177.se, onvard.se and Hsi Hälso & Sjukvårdsinformation, see Dietrichson et al. (2016).

Figure 2 shows the locations of health centers in Skåne in January 2010. Most new centers (colored black in the figure) chose to locate in the southwestern part of the county, which is most densely populated. Although, there were examples of entries also in quite a few of the less dense areas.

**Figure 2.** Locations of health centers in Skåne. Circles= public, triangles = private. Hollow = already opened before reform, black filled= opened after the reform. Source: own data collection (see Fig. 1).
On the reform date, residents who had not already enrolled at a center were assigned to
the health center at the shortest straight-line distance from their homes. In May 2009, the
number of listed patients at public health centers ranged from 3,343 to 16,926. By late
2015, about 40% of the population residing in the region had switched care centers at
least once since the reform; around 10% of the population switches providers in a year
(Anell et al., 2017).

Overall, the propensity to visit primary care in Skåne increased after the reform (Beckman
and Anell, 2013). Total expenditure on primary care increased immediately following the
reform (Anell, 2016), but the increase was absorbed by private providers: total
expenditure on publicly provided primary care was constant at around 3 billion SEK until
it increased to 3.3 billion SEK in 2011 according to the county’s annual financial reports
for 2008-11.

4. **Empirical strategy**

4.1 **Identification**

Although the reform applied to the whole region, we argue – in the same spirit as Gaynor
et al. (2013) and Dietrichson et al. (2016) – that its impact was bound to be small in some
local markets. Specifically, lower entry barriers should not affect the supply side of
markets where no one wishes to enter, while lower switching costs and increased access
to information about providers have little bearing on the sensitivity of demand in markets
where patients’ choice sets are small.

In line with this reasoning, we adopt a difference-in-differences strategy comparing
health centers that were monopolies right before the reform (i.e., in April 2009) to the
other health centers. For simplicity, we define the relevant market for each health center
as a circle with 3 km radius around the health center’s location. When the choice set only
includes one health center, demand sensitivity should not be affected by the reform – it
does not matter that it has become easier to switch providers, and information about

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6 Almost 70% of the population is registered at a health center within 3 km distance from their home (Anell
et al., 2018), which suggests that our chosen market definition is large enough to include relevant
competitors. We also perform sensitivity tests with larger radii. Notably, all public health centers faced
competition within a 20 km radius pre-reform.
providers outside one’s choice set is irrelevant. Moreover, as seen from Fig. 3, there was almost no entry after the reform in monopoly markets. Thus, all channels through which the reform might have an impact were virtually absent in the monopoly group.

Intuitively, one might have thought that post-reform entry would be at least as common in monopoly markets, as the reform implied that entry restrictions were removed. However, as Figure 3 shows, post-reform entries were concentrated to the already competitive areas. According to our 3 km market definition, 37 public health centers were pre-reform monopolies (see Table 1); only three of these were exposed to entry after the reform. One of these three would have been classified as a pre-reform non-monopoly if we had used a larger market definition of 10 km, whereas the two others would have been classified as pre-reform monopolies also with a market definition of 15 km. Thus, including these in the control group implies that our estimate is conservative.

Table 1. The number of public health centers classified as monopolies with different market definitions.

|                     | Market definition: | 1 km | 3 km | 5 km | 10 km |
|---------------------|--------------------|------|------|------|-------|
| Monopolies          |                    | 68   | 37   | 30   | 17    |
| Non-monopolies      |                    | 19   | 50   | 57   | 70    |

Note: Each column shows the number of public health centers according to their treatment status with different definitions of the market. Monopolies (non-monopolies) had no (had at least one) competitor within a 1(3)5(10) km straight-line distance in April, 2009. Competitors may be public or private.
In summary, it does not seem as though the entry restrictions were the reason why these health centers were monopolies before the reform, but rather that they were located in unattractive markets. Obviously, this means that the monopoly and non-monopoly markets are very different. However, the attractiveness of the location can reasonably be viewed as constant over our short sample period and is thus accounted for in our DID estimation strategy.

### 4.2 Estimation

We estimate versions of the following model:

\[ y_{ijht} = \alpha + \beta_1 \cdot Post_t + \beta_2 \cdot CompetitionPre_h \cdot Post_t + \gamma \cdot X_{iy} + \mu_h + \lambda_y + \theta_q + M_m + \epsilon_{ijht} \]

where \( y_{ijht} \) is the number of diagnoses registered for individual \( i \) during her \( j \)th visit at provider \( h \) on date \( t \) \((y_{ijt} \in [0, \infty))\). \( Post_t \) is a dummy variable indicating visits that took place after the reform. \( CompetitionPre_h \) indicates health centers \( h \) that were non-monopolies pre-reform. \( \beta_2 \), the interaction parameter on \( CompetitionPre_h \cdot Post_t \), is the parameter of interest. A rejection of the null hypothesis that \( \beta_2 \) equals zero indicates that public health centers responded to increased competition by changing their diagnosis registration practices.

\( X_{iy} \) is a vector of patient-and-year-level covariates. Most of the covariates enter as categorical variables; continuous covariates are included as third-degree polynomials. Health center fixed effects, \( \mu_h \), pick up the impact of unobserved features that do not vary over the sample period; with a sample period of only 4 years, many center-specific features can plausibly be viewed as constant. To further explore channels behind the effect in our baseline model, we include patient and physician fixed effects in later stages of the estimation.

\( \lambda_y, \theta_q \) and \( M_m \) are calendar year, quarter-of-year and month fixed effects, and \( \epsilon_{ijht} \) is an idiosyncratic error term. Standard errors are clustered at the health center level.
5. Data

Our information about the primary care market derives from a register with information on the names, geographical coordinates, opening and closing dates, and ownership status (public/private) of all primary care providers in 2005-13 (Dietrichson et al., 2016). To this data, we add information on the number of patients and capitation reimbursement in May 2009 from the region’s own administrative databases.7

The health center-level data is linked to a microdataset including information about all visits to public health centers made by patients aged 18-65 in 2008-2011. For each visit, we have information about the date, the name of the health center, if the patient was enrolled (“listed”) at that health center, a pseudonymized identifier for the GP the patient was in contact with, and 3-digit ICD10 codes for registered diagnoses. In our analysis, we use the number of GP visits in 2008, VisitPre, as a proxy for the individual’s initial health status. We also include a dummy indicating whether the visit took place at the health center where the individual was listed.

The microdata also includes patient background characteristics from Statistics Sweden’s register. We have annual information about age, gender (dummy for being a woman), civil status (dummy for being single), number of children, country of birth (dummy foreign background indicates individuals born in a foreign country or by two foreign-born parents), highest attained education level and disposable income in 100s of SEK.

Our study sample includes the universe of visits to public health centers made by working-age adults in 2008-2011, excluding i) visits at the two public health centers opened on the reform date and ii) visits with non-physician staff (nurses, physiotherapists etc). Table 2 presents descriptive statistics for patient variables in 2008 and 2011, separating between visits at non-monopoly and monopoly health centers (HCs) respectively. Non-monopolies register fewer diagnoses per visit compared to monopolies but both groups experience an increase in the diagnosis registration over the study period. The cross-sectional differences observed between the groups reflect that many non-monopolies are located in the more urban areas, where the populations are slightly younger and include more people with foreign background. The considerably lower

7 We have no information about the number of patients belonging to the administratively determined uptake area of health centers before the reform. This means that we cannot, e.g., study the effect of competition on the number of listed patients.
income in the non-monopoly group suggests that high-income individuals sort into private providers when such providers are available. It is further notable that compared to 2008, public health centers catered to more young patients, immigrants and highly educated patients in 2011 in both non-monopoly and monopoly markets.
| Variable                  | Non-monopoly HCs 2008 (n=267,603) | Monopoly HCs 2008 (n=169,497) | t-test between HC groups 2008 | Non-monopoly HCs 2011 (n=298,123) | Monopoly HCs 2011 (n=184,083) | t-test between HC groups 2011 | t-test within Non-monopoly HCs 2008 and 2011 | t-test within Monopoly HCs 2008 and 2011 |
|--------------------------|----------------------------------|--------------------------------|--------------------------------|----------------------------------|--------------------------------|---------------------------------|---------------------------------|---------------------------------|
| Number of diagnoses      | 1.201                            | 1.290                          | 0.734                          | 1.433                            | 1.471                          | 0.967                          | 0.976                           | 0.976                           |
| VisitPre                 | 3.123                            | 2.979                          | 2.328                          | 1.329                            | 1.335                          | 1.950                          | 0.284                          | 0.551                           |
| Visit at listed HC       | 0.980                            | 0.977                          | 0.150                          | 0.976                            | 0.976                          | 0.153                          | 0.551                          | 0.119                           |
| Woman                    | 0.608                            | 0.602                          | 0.489                          | 0.602                            | 0.593                          | 0.491                          | 0.000                          | 0.000                           |
| Children                 | 0.657                            | 0.728                          | 1.033                          | 0.654                            | 0.722                          | 1.034                          | 0.000                          | 0.307                           |
| Age                      | 42.561                           | 44.83                          | 13.383                         | 42.042                           | 44.024                         | 13.476                         | 0.000                          | 0.000                           |
| Foreign background       | 0.340                            | 0.153                          | 0.360                          | 0.364                            | 0.167                          | 0.373                          | 0.000                          | 0.000                           |
| Disposable income (hundreds of SEK) | 3228                            | 4015                           | 3525                           | 3482                            | 4350                           | 3802                           | 0.000                          | 0.000                           |
| Primary education        | 0.207                            | 0.203                          | 0.402                          | 0.196                            | 0.190                          | 0.392                          | 0.000                          | 0.000                           |
| Secondary education      | 0.451                            | 0.507                          | 0.5                            | 0.444                            | 0.508                          | 0.500                          | 0.000                          | 0.000                           |
| Post-secondary education | 0.324                            | 0.28                           | 0.449                          | 0.342                            | 0.292                          | 0.455                          | 0.000                          | 0.000                           |

Note: Statistics for all visitors at public health centers (HCs) in 2008 and 2011, presented by HC type: Non-monopoly HCs had a competitor within 3km radius as of April, 2009 while Monopoly HCs had no competitor within a 3 km radius in April, 2009. Two-sample two-sided t-tests with unequal variances. VisitPre is the number of GP visits made in 2008.
6. Results

6.1 Main results

Figure 4 illustrates that the number of registered diagnoses per visits has increased after the reform. The figure further indicates a parallel development in monopolies and non-monopolies prior to the reform, perhaps with a slightly larger increase in the non-monopoly group the months just before the reform. This may be interpreted as an anticipation effect: as providers knew already in early 2009 that they would be subject to the ACG system, those who feared a loss of patients due to increased competition had incentives to intensify their registration efforts already by then.\(^8\) Notably though, the divergence should not be over-interpreted as it does not appear in more formal test (see Appendix A).

\[\text{Figure 4} \quad \text{Average number of diagnoses per visit. Monthly averages by pre-reform monopoly status.}\]

According to Figure 4, the monopolies and non-monopolies have more similar registration patterns after the reform. Table 3 shows our DID estimates of the reform effect. The results suggest that since the reform, health centers in competitive markets have increased the number of registered diagnoses by almost 0.05 more than health centers in non-competitive markets. In relation to the mean number of diagnoses

\(^8\) It is unlikely that details about the payment system were known before then, because the details were not spelled out until the budget decision was taken in late 2008 (Regionstyrelsen, Beslutsförslag, Dnr 0800875).
registered during a visit, the effect amounts to about 3 percent. To get a sense of the
economic magnitude of our result, we aggregate our data at the health center and month-
level and add information about the capitation reimbursement and number of enrolled
patients of each health center in May 2009. Using this dataset, we first run the aggregate
analogue of our main specification (i.e., a DID analysis of the mean number of diagnoses
per visit) and confirm that we get a similar estimate as in the visit-level analysis. We then
calculate the correlation between the average number of diagnoses per visit of a health
center and its average capitation. Combining these two estimates, we calculate that the
expected increase in the capitation per patient due to the competition effect on diagnosis
registrations would amount to 0.29 SEK (in 2009 prices).9 For a health center of average
size in May 2009, i.e., with 9,925 listed patients, each of whom brought an average
capitation of 190 SEK, the increase in revenue following the increase in registrations
would cover the loss of 15 listed patients.10 Thus, the economic significance of the effect
is negligible.

In Appendix B, we show that the estimates are very similar, but slightly more precise,
when defining the post-treatment period to begin in January 2009, i.e., acknowledging the
incentive for strategically minded providers to increase diagnosis registrations already
before the reform. When we allow for anticipation effects already from July 2008
onwards, the treatment effect is slightly attenuated (though still significant) – which
makes sense, given that the treatment period then includes as many as 10 pre-reform
months. The effect is further robust to increasing the radius of the circles defining the
relevant market to up to 10 km. When extending the border to 15 km, the result becomes
insignificant and switches sign; however, with markets this large, there are only 5 health
centers that are classified as pre-reform monopolies, two of which actually experienced
nearby entry post reform. Thus, the comparison group is too small to make a meaningful
comparison. The effect further disappears when the radius is decreased to 1 km. One
plausible interpretation of this result is that health centers with a competitor 2-10 km
away indeed do react to the competition so that there is no contrast to health centers with

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9 In a regression of the average capitation on the average number of diagnoses per visit, the estimated
correlation is 7.3 SEK. Thus if the average number of diagnoses per visit increases by 0.04 (our DID estimate
in the aggregate analysis, as compared to 0.05 in the visit-level specification), the average capitation
increases by 0.04*7.3 =0.29 SEK.
10 15=(0.29*9,925)/190
competitors at an even shorter distance. It can finally be noted that the baseline estimate is similar when excluding the two largest cities from the estimations (results not shown).
|                          | Coefficient | Standard Error |
|--------------------------|-------------|----------------|
| Post                     | -0.0535***  | (0.0173)       |
|                          | -0.0414**   | (0.0174)       |
| PostXCompetitionPre      | 0.0468*     | (0.0270)       |
|                          | 0.0476*     | (0.0270)       |
| Woman                    | 0.00310     | (0.00311)      |
| Children                 | -0.00880*** | (0.00161)      |
| Single                   | 0.00509*    | (0.00272)      |
| Age                      | -0.0174***  | (0.00343)      |
| Age^2                    | 0.000502*** | (9.04e-05)     |
| Age^3                    | -2.84e-06***| (7.29e-07)     |
| Foreign                  | 0.0495***   | (0.00374)      |
| Disposable Income        | -7.97e-06***| (6.00e-07)     |
| Disposable Income^2      | 0***        | (0)            |
| Disposable Income^3      | -0***       | (0)            |
| Secondary Education      | -0.0451***  | (0.00301)      |
| Post-Secondary Education | -0.0758***  | (0.00383)      |
| VisitPre                 | 0.0535***   | (0.00223)      |
| VisitPre^2               | -0.00495*** | (0.000234)     |
| VisitPre^3               | 0.000110*** | (5.97e-06)     |
| Visit at listed HC       | 0.112***    | (0.00716)      |
| 2008                     | -0.239***   | (0.0137)       |
|                          | -0.280***   | (0.0146)       |
| 2009                     | -0.158***   | (0.0123)       |
|                          | -0.163***   | (0.0125)       |
| 2010                     | -0.0895***  | (0.0160)       |
|                          | -0.0957***  | (0.0161)       |

Note: The dependent variable is the number of registered diagnoses per visit. Post = 1 in all time periods from May 2009 onwards. CompetitionPre = 1 for visits at health centers that had a competitor within a 3 km radius before the reform. PostXCompetitionPre is the coefficient of interest. All specifications include health center fixed effects and calendar year (ref=2011), quarter-of-year and month-of-year fixed effects (suppressed in table). Standard errors clustered by health center in parentheses: *** p<0.01, ** p<0.05, * p<0.1.
6.2 Channels

Although the magnitude of the effect is small, we would like to understand why the average number of registered diagnoses increased more post reform in areas more exposed to competition. Is it due to conscious behavioral changes in registration practices, as outlined in the introduction of the paper, or to something else?

Given that the reform entailed more freedom of choice for patients, one natural concern is that the patient mix facing public health centers in competitive areas has changed and thus that the effect does not reflect changes in physician behavior. For instance, if healthy individuals are more likely to switch providers, then the average health status among visitors at public providers in competitive areas is worse after the reform, thus warranting more diagnoses for medical reasons. Several robustness checks indicate that changes in the patient case-mix do not completely explain our main result (Appendix Table C1). Looking first at two proxies for bad health, age and foreign background, we find that the average age of visitors has not increased more in competitive areas, although the share of visitors with foreign background has increased more in competitive areas post reform.\(^\text{11}\) As patients with foreign background on average have more diagnoses (see Table 3), this case mix change may partly account for the main effect. However, it does not fully explain the effect, as further estimations indicate that the same patient is treated differently after the reform (with relatively more diagnoses in competitive areas). Specifically, our main result still holds up when we restrict the scope for case-mix changes by re-estimating the main specification on a sample including only patients that visited public health centers both before and after the reform, as well as on a sample excluding visits after September 2010. Perhaps most convincing, the main result remains when we include patient fixed effects in the model. I.e., a given individual who visited a health

\(^{11}\) A third proxy for health status is the number of visits each patient makes in a year. Using a dataset with one observation for each patient and year, we find no significant DID with regard to this variable (results not shown). We thank Ansgar Wübker for suggesting this test.
center both before and after the reform was likely to get more diagnoses during the post-reform visit if the health center was located in a competitive area than otherwise.  

Another possible explanation for the effect is that the GP mix might have changed after the reform. For instance, the new private health centers established in (some) non-monopoly markets might have recruited GPs from existing public centers. If GPs differ in their propensity for registrations, relocations of GPs may be another reason why registration patterns at public health centers change differently in more or less competitive areas. However, we get similar results when estimating the model on a sample including only GPs who remained employed after the reform, and when we add GP fixed effects to the model. The results hold up even when we include both GP fixed effects and time-varying observable patient characteristics and/or time-invariant unobservable patient characteristics (patient fixed effects). These results strongly suggest that the experience of being in a competitive environment makes a given GP behave differently in comparable consultations.

7. Conclusions

Our main result is that public primary care providers do react to increased competition from private providers. In a setting where profits can be increased by registering more diagnoses for patients, we find that patients received more diagnoses in areas that were affected by intensified competition compared to other areas. Although the result to some extent may be due to changes in the patient case mix, we show that such changes do not explain the whole effect. Neither is the effect due to changes in the mix of physicians employed at the health centers – the same physician acts differently in comparable patient consultations depending on the degree of competition.

Notably, the increased diagnosis registrations in competitive areas do not necessarily mean that patients are receiving inappropriate diagnoses (upcoding). In their own audits, administrators in the studied region have found examples of under- as well as over-reporting of diagnoses in primary care (Glenngård, 2015). Nonetheless, from a governance or management perspective, it would be preferable if all health centers had a

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12 Additionally, previous research suggests that people who switch providers in Skåne tend to be sicker than stayers (in the sense that they have consumed more healthcare (Anell et al., 2017)). This suggests that if anything, we are underestimating the effect of competition on diagnosis registrations.
standardized approach to registrations – in particular, there is no argument why registration behavior should depend on the degree of competition.

Our result is broadly in line with previous evidence of competition in primary care. Notably, our results indicate that the effects of competition found in previous studies of the Swedish choice and entry reforms need not solely be due to responses among private providers. This finding has important implications for policy in a setting where private providers constitute a minority: as public providers are not insulated from the forces of competition, competition-enhancing policies apparently has a bite also on these providers. Although, one should keep in mind that the magnitudes of the effects detected in this and other studies are small. In our case, the research design implies that we only study the effect of increased competition on providers already subject to some competition. It is possible that the effect would have been greater for challenged monopolies. However, we note that Dietrichson et al. (2016) did not find an effect of competition on avoidable hospitalizations regardless of the degree of pre-reform competition.

Our finding is also relevant with respect to the discussion of upcoding in hospital payment system and its association with for-profit status (Anthun et al., 2017; Serdén et al., 2003; Silverman and Skinner, 2004). Our result corroborate the findings in earlier studies of Nordic public hospitals, suggesting that public healthcare providers are indeed concerned with their financial performance.

Future studies of competition in primary care should address desirable outcomes such as increased access or decreased use of (unnecessary) secondary care, but also undesirable outcomes such as dumping of expensive patients or skimping on quality. More generally, more research, theoretical as well as empirical, on the incentives of public providers is warranted.

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Appendix

Appendix A Pre-reform trends

To see if the treatment and control groups follow the same trend before the reform, we run regressions on pre-reform data. In the table below, the sample period in columns 1-2 and 5 covers Jan 1, 2008 to April 30, 2009; the sample period in columns 3-4 spans Jan 1, 2008 to Dec 31, 2008. The first columns show estimates of the following specification:

\[ y_{ijht} = \alpha + \beta_1 \times TT + \beta_2 \times TT \times CompetitionPre_h + \gamma' X_{ijt} + \mu_h + \lambda_y + \theta_a + M_m + \epsilon_{ijht} \]

TT is a linear time trend variable, which we allow to differ between previous non-monopolies and monopolies. Based on these estimates, we do not reject that monopolies and non-monopolies followed the same linear trend pre-reform. In the final column, we include an interaction term between TT, CompetitionPre and a dummy for the first four months in 2009 (i.e. the final months before the reform), Post08. The tendency for divergence seen in Figure 4 evidently fades after the inclusion of health center and time fixed effects.

Table A1. Pre-reform trends.

|              | (1)       | (2)       | (3)       | (4)       | (5)       |
|--------------|-----------|-----------|-----------|-----------|-----------|
| TT           | 0.00266   | 0.00620   | 0.00372   | 0.00674   | 0.00713   |
|              | (0.00932) | (0.00922) | (0.00944) | (0.00935) | (0.00935) |
| TTxCompetitionPre | 0.00114   | 0.00104   | -0.000199 | -0.000351 | -0.000447 |
|              | (0.00154) | (0.00154) | (0.00207) | (0.00207) | (0.00199) |
| TTxCompetitionPre*Post08 | 0.000787 | (0.000728) | (0.000728) | (0.000728) | (0.000728) |
| Constant     | 1.218***  | 1.074***  | 1.182***  | 1.102***  | 1.051***  |
|              | (0.227)   | (0.227)   | (0.118)   | (0.125)   | (0.123)   |
| Observations | 592,720   | 592,720   | 437,100   | 437,100   | 592,720   |
| R2           | 0.082     | 0.104     | 0.089     | 0.108     | 0.104     |
| Clusters     | 87        | 87        | 87        | 87        | 87        |
| Mean of y    | 1.335     | 1.335     | 1.335     | 1.335     | 1.335     |
| Patient covariates | No | Yes | No | Yes | Yes |
| Time period  | 01/2008-04/2009 | 01/2008-04/2009 | 01/2008-12/2008 | 01/2008-12/2008 | 01/2008-04/2009 |

The dependent variable is the number of registered diagnoses per visit. TT is a linear time trend. CompetitionPre = 1 for visits at health centers that had a competitor within a 3 km radius before the reform. All specifications include health center fixed effects, calendar year effects, quarter-of-year and
Appendix B Robustness checks

To explore the possibility that providers adjusted their registration behavior already before the reform took place, we replace the *Post* dummy by another dummy, *Post08*, that takes on the value one already from January 2009 onwards, and include a *Post08* *CompetitionPre* interaction (columns 1-2 of Table B1). The larger coefficient on the post-reform variable in this case indicates that efforts to increase registrations were widespread throughout the region (not only in the competitive areas). Columns 3-4 show the effect when allowing for anticipation effects already from July 2008 onwards.

Table B1. Main specification, allowing for anticipation effects.

|                | (1)       | (2)       | (3)       | (4)       |
|----------------|-----------|-----------|-----------|-----------|
| Post08         | 0.186***  | 0.247***  |           |           |
|                | (0.0161)  | (0.0172)  |           |           |
| Post08XCompetitionPre | 0.0468*   | 0.0474*   |           |           |
|                | (0.0243)  | (0.0243)  |           |           |
| PostJuly08     |           |           | 0.00498   | -0.00436  |
|                |           |           | (0.0172)  | (0.0170)  |
| PostJuly08XCompetitionPre | 0.0397*   | 0.0397*   |           |           |
|                |           |           | (0.0210)  | (0.0212)  |
| Constant       | 1.218***  | 1.077***  | 1.406***  | 1.335***  |
|                | (0.0133)  | (0.0402)  | (0.0143)  | (0.0424)  |
| Observations   | 1,891,671 | 1,891,671 | 1,891,671 | 1,891,671 |
| R²             | 0.053     | 0.086     | 0.053     | 0.086     |
| Clusters       | 87        | 87        | 87        | 87        |
| Mean of y      | 1.335     | 1.335     | 1.335     | 1.335     |
| Patient covariates | No | Yes | No | Yes |

The dependent variable is the number of registered diagnoses per visit. Post08 = 1 in all time periods from Jan 2009 onwards. CompetitionPre = 1 for visits at health centers that had a competitor within a 3 km radius before the reform. All specifications include health center fixed effects, calendar year, quarter-of-year and month-of-year fixed effects. Standard errors clustered by health center in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Table B2 shows robustness checks using different market definitions (1 km, 5 km, 10 km).

Table B2. Main specification, market definitions.

|                  | 1 km | 2 km | 3 km | 4 km | 5 km | 6 km |
|------------------|------|------|------|------|------|------|
|                  | (1)  | (2)  | (3)  | (4)  | (5)  | (6)  |
| Post             | -0.0302** (0.0133) | -0.0182 (0.0129) | -0.0586*** (0.0212) | -0.0460** (0.0213) | -0.0774** (0.0327) | -0.0650* (0.0328) |
| PostXCompetitionPre | 0.0246 (0.0442) | 0.0267 (0.0445) | 0.0485* (0.0282) | 0.0485* (0.0283) | 0.0637* (0.0366) | 0.0639* (0.0367) |
| Constant         | 1.452*** (0.0136) | 1.363*** (0.0403) | 1.452*** (0.0134) | 1.363*** (0.0403) | 1.452*** (0.0134) | 1.362*** (0.0401) |
| Observations     | 1,891,671 | 1,891,671 | 1,891,671 | 1,891,671 | 1,891,671 | 1,891,671 |
| R²               | 0.053 | 0.086 | 0.053 | 0.086 | 0.053 | 0.086 |
| Clusters         | 87    | 87    | 87    | 87    | 87    | 87    |
| Mean of y        | 1.335 | 1.335 | 1.335 | 1.335 | 1.335 | 1.335 |
| Patient covariates | No | Yes | No | Yes | No | Yes |

The dependent variable is the number of registered diagnoses per visit. Post = 1 in all time periods from May 2009 onwards. CompetitionPre = 1 for visits at health centers that had a competitor within a 1 km radius (columns 1 and 2), 5 km radius (columns 3 and 4) and 10 km (columns 5 and 6), respectively, before the reform. All specifications include health center fixed effects, calendar year, quarter-of-year and month-of-year fixed effects. Standard errors clustered by health center in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Appendix C: Channels

To study patient case mix changes (Table C1), we first replace the main dependent variable by age and foreign background, respectively (columns 1-2). As a second approach to the case mix issue, we estimate the main specification on (i) a sample that only includes individuals who visited public health centers both before and after the reform (column 3) and (ii) a sample that excludes all visits from September 2010 onwards (column 4). In both cases, the idea is to keep the case mix as constant as possible. Finally, we augment our main model with patient fixed effects (columns 5-8); in this way, we are essentially asking whether the same patient is treated differently depending on whether she or he visits a health center in a more or less competitive area. In the final two columns, the patient FE model is estimated on the subsample of patients who were observed both before and after the reform (i.e., a subset of the sample in column 3), to rule out that identification only comes from new-coming patients.
Table C1. Patient case mix analysis.

|                  | Age (1)  | Foreign Stayers (2) | Before Sep 2010 (3) | Patient FE (4) | Patient FE, Stayers (5) | Patient FE, Stayers (6) | Patient FE, Stayers (7) | Patient FE, Stayers (8) |
|------------------|----------|---------------------|---------------------|----------------|--------------------------|--------------------------|--------------------------|--------------------------|
| Post             | -0.614***| -0.00566*           | -0.0308*            | -0.0380*       | -0.0416***               | -0.0485***               | -0.0486***               | -0.0535***               |
|                  | (0.0800) | (0.00297)           | (0.0183)            | (0.0193)       | (0.00382)                | (0.00382)                | (0.00408)                | (0.00408)                |
| PostXCompetitionPre | 0.0905   | 0.0116**           | 0.0568*            | 0.0497         | 0.0523***               | 0.0611***               | 0.0526***               | 0.0616***               |
|                  | (0.122)  | (0.00457)           | (0.0289)            | (0.0301)       | (0.00343)                | (0.00342)                | (0.00345)                | (0.00345)                |
| Constant         | 43.99*** | 0.288***           | 1.489***            | 1.336***       |                          |                          |                          |                          |
|                  | (0.0821) | (0.00258)           | (0.0145)            | (0.0124)       |                          |                          |                          |                          |
| Observations     | 1,891,671| 1,891,671          | 1,307,946           | 1,286,621      | 1,760,365                | 1,760,365                | 1,306,912                | 1,306,912                |
| R²               | 0.764    | 0.166              | 0.054               | 0.047          | 0.000                    | 0.002                    | 0.000                    | 0.003                    |
| Clusters         | 87       | 87                 | 87                  | 87             | 334665                   | 334665                   | 200592                   | 200592                   |
| Mean of y        | 42.95    | 0.281              | 1.368               | 1.286          | 1.335                    | 1.335                    | 1.335                    | 1.335                    |
| Patient covariates | No       | No                | No                  | No             | Yes                      | No                       | Yes                      | Yes                      |

The dependent variable in column 1 is the patient’s age, in column 2 a dummy for patients with foreign background, in columns 3 - 8 the number of registered diagnoses per visit. Column 3 shows the estimates for a sample containing only individuals who visited public health centers both before and after the reform (Stayers). Column 4 shows estimates for a sample that excludes all visits from September 2010 onwards. Column 5-6 present the estimates including patient fixed effects. Column 7-8 show the estimates for the sample of stayers including patient fixed effects. All specifications include health center fixed effects, calendar year, quarter-of-year and month-of-year fixed effects. Standard errors clustered by health center in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
To examine the importance of changes in the GP case mix, we first estimate the main specification on a sample that only includes visits at GPs who have been observed working at the public HCs both before and after the reform (Table C2, columns 1-2). As a second approach, we augment the model with GP fixed effects; first on the baseline estimation sample (column 3-4), and second on the restricted sample of GPs observed both before and after the reform (columns 5-6). Finally, we include both GP and patient fixed effects at once (columns 7-8).
## Table C2. GP mix analysis

|                      | GP Stayers (1) | GP FE (2) | GP FE, GP Stayers (3) | GP FE, GP Stayers (4) | GP FE and patient FE (5) | GP FE and patient FE (6) | GP FE and patient FE (7) | GP FE and patient FE (8) |
|----------------------|----------------|-----------|-----------------------|-----------------------|-------------------------|-------------------------|-------------------------|-------------------------|
| Post                 | -0.0368**      | -0.0498***| -0.0432***            | -0.0483***            | -0.0397**               | -0.0463***              |                        |                         |
|                      | (0.0165)       | (0.0147)  | (0.0145)              | (0.0149)              | (0.0172)                | (0.0172)                |                        |                         |
| PostXCompetitionPre  | 0.0502*        | 0.0526**  | 0.0524**              | 0.0574**              | 0.0650**                |                        |                        |                         |
|                      | (0.0254)       | (0.0228)  | (0.0231)              | (0.0273)              | (0.0277)                |                        |                        |                         |
| Observations         | 1,551,001      | 1,551,001 | 1,853,347             | 1,853,347             | 1,551,001               | 1,551,001               | 1,723,094               | 1,723,094               |
| R²                   | 0.055          | 0.090     | 0.132                 | 0.098                 | 0.130                   | 0.364                   | 0.365                   |                         |
| Patient covariates   | No             | Yes       | No                    | Yes                   | No                      | Yes                     | No                      | Yes                     |
| Clusters             | 87             | 87        | 87                    | 87                    | 87                      | 87                      | 87                      | 87                      |
| Mean of y            | 1.349          | 1.349     | 1.340                 | 1.340                 | 1.349                   | 1.349                   | 1.340                   | 1.340                   |

The dependent variable is the number of registered diagnoses per visit. Columns 1-2 show estimates of the main specification on a sample including only visits at GPs who have been observed working at public HCs both before and after the reform (=GP stayers). Columns 3-4 show the estimates of our main specification with GP fixed effects. Columns 5-6 show estimates of a specification with GP fixed effects on the sample of GP stayers. Columns 7-8 present the estimates of the main specification augmented with GP and patient fixed effects. All specifications include health center fixed effects, calendar year, quarter-of-year and month-of-year fixed effects. Standard errors clustered by health center in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
