Received: 18 October 2019 | Revised: 15 January 2020 | Accepted: 24 February 2020

DOI: 10.1002/hec.4015

Check for updates

RESEARCH ARTICLE

WILEY Health Economics

Patient choice, entry, and the quality of primary care: Evidence from Swedish reforms

Jens Dietrichson[1] | **Lina Maria Ellegård**[2] | **Gustav Kjellsson**[3]

[1]VIVE—The Danish Center for Social Science Research, Copenhagen, Denmark

[2]Department of Economics, Lund University, Lund, Sweden

[3]Department Economics and Centre for Health Economics (CHEGU), University of Gothenburg, Gothenburg, Sweden

Correspondence
Lina Maria Ellegård, Department of Economics, Lund University, Tycho Brahes väg 1, Lund SE-220 07, Sweden.
Email: lina_maria.ellegard@nek.lu.se

Funding information
Crafoordska Stiftelsen, Grant/Award Number: 2014/0664; Forskningsrådet om Hälsa, Arbetsliv och Välfärd, Grant/Award Number: 2014/0861; Konkurrensverket (Swedish Competition Authority), Grant/Award Number: 316/2013

Abstract

Policies aiming to spur quality competition among health care providers are ubiquitous, but their impact on quality is *ex ante* ambiguous, and credible empirical evidence is lacking in many contexts. This study contributes to the sparse literature on competition and primary care quality by examining recent competition enhancing reforms in Sweden. The reforms aimed to stimulate patient choice and entry of private providers across the country but affected markets differently depending on the initial market structure. We exploit the heterogeneous impact of the reforms in a difference-in-differences strategy, contrasting more and less exposed markets over the period 2005–2013. Although the reforms led to substantially more entry of new providers in more exposed markets, the effects on primary care quality were modest: We find small improvements of patients' overall satisfaction with care, but no consistently significant effects on avoidable hospitalisation rates or satisfaction with access to care. We find no evidence of economically meaningful quality reductions on any outcome measure.

KEYWORDS

competition, patient choice, primary health care, quality

JEL CLASSIFICATION

I11; I18; H75; D04

1 | INTRODUCTION

Policies promoting competition in the health care sector are ubiquitous. Standard health economic models predict that providers compete on quality when prices are regulated (Gaynor & Town, 2011; McGuire, 2011). However, if patients have limited abilities to infer quality, the link between competition and quality weakens (e.g., Arrow, 1963). Even if patients are well-informed, providers may react to increased demand elasticity by reducing the quality offered to unprofitable patients (skimping) and increasing the quality offered to profitable patients (cream skimming) (Brekke, Siciliani, & Straume, 2014).

This is an open access article under the terms of the Creative Commons Attribution License, which permits use, distribution and reproduction in any medium, provided the original work is properly cited.
© 2020 The Authors. Health Economics published by John Wiley & Sons Ltd

A literature on quality competition in the hospital sector under regulated prices has emerged in recent years,[1] but evidence from the primary care sector—that is, general practitioners' (GPs) services—is scarce (Gaynor, Ho, & Town, 2015; Propper, 2012). Primary care is the patient's first point of contact with health care, provides preventive services, and treats patients with chronic conditions (e.g., Bailey & Goodman-Bacon, 2015; Kringos, Boerma, Hutchinson, & Saltman, 2015; Rosano et al., 2013; Starfield, Shi, & Macinko, 2005). As the provided service is multifaceted and the patient group is diverse, competition may have different effects than in hospital markets.

The small empirical literature on quality effects of patient choice and competition in primary care settings with regulated prices suffers from a lack of exogenous variation (e.g., Berlin, Busato, Rosemann, Djalali, & Maessen, 2014; Jürges & Pohl, 2012; Pike, 2010; Rosano et al., 2013; Stroka-Wetsch, Talmann, & Linder, 2016). In the most convincing study (Gravelle, Liu, Propper, & Santos, 2019), the main specification relies on GP practice fixed effects (FEs) and thus accounts for unobserved (geographical or demographic) common determinants of choice, competition, and quality but not for the endogeneity of entries and exits. The study finds small increases in patient satisfaction but no strong indications that competition improves objective quality measures (e.g., avoidable hospitalisation rates).[2] A related literature, which examines measures of opportunistic behavior rather than quality, indicates that competition makes GPs issue more sick leave certificates (Brekke, Holmås, Monstad, & Straume, 2019; ISF, 2014; Markussen & Røed, 2016), provide more (intense) treatment (Iversen & Lurås, 2000; Iversen & Ma, 2011), prescribe more drugs (Fogelberg, 2014; Kann, Biørn, & Lurås, 2010; Schaumans, 2015), and try to affect the risk-adjustment factor to increase their capitation (Dackehag & Ellegård, 2019).

We provide causal evidence of how primary care quality is affected by competition enhancing policies—that is, increased patient choice and reduced barriers to entry. We use plausibly exogenous variation induced by a set of reforms implemented in Swedish primary care 2007–2010. Since the reforms, there is free entry for providers that meet basic requirements, there is more publicly available information about providers, and it has become easier (and less costly) to search for and switch providers. These three components have the potential to increase patient mobility and increase the elasticity of demand with respect to quality. The number of providers has increased by around 20% since the reforms, solely due to entry of private providers.

Our identification strategy exploits heterogeneity in the impact of the reforms across markets. Similar to Propper, Burgess, and Gossage (2008), Cooper et al. (2011), and Gaynor et al. (2013), who study competition among hospitals in the English NHS, we exploit prereform variation in market conditions in a difference-in-differences (DID) model. We start from the plausible assumption that markets in which there was room for new entrants before the reforms were on average more affected by all the main components of the reforms. Accordingly, we define a treatment group as consisting of municipalities where, 6 months before a reform, the average number of patients per care provider would exceed a median-sized private provider if one additional provider entered the market.

Our set of outcome variables includes both objective and subjective indicators of care quality. The objective quality measure is the rate of hospitalisations for so-called ambulatory care sensitive conditions (ACSC). The ACSC hospitalisation rate is closely linked to primary care quality, as ACSCs are medical conditions for which appropriate primary care would be expected to prevent hospitalisations (Rosano et al., 2013). The subjective quality measures originate from three waves of a large patient satisfaction survey, from which we have prereform and postreform data for a subset of regions. The survey data allow us to develop four measures of subjective care quality (phone accessibility, waiting times, overall impression, and willingness to recommend the practice) and to examine indications of skimping and cream skimming.

2 | PRIMARY CARE AND CHOICE REFORMS IN SWEDEN

Swedish government has three layers: the national government, 21 regions, and 290 municipalities. Health care services are mainly the responsibility of the regions. Primary care accounts for almost 20% of total health care expenditures. Fees for visits and prescription drugs are regulated and capped.

[1]Among studies in regulated-price settings, several investigations of the English English National Health Service (NHS) and US Medicare indicate improvements for cardiac patients (e.g., Bloom, Propper, Seiler, & Van Reenen, 2015; Colla, Bynum, Austin, & Skinner, 2016; Cooper, Gibbons, Jones, & McGuire, 2011; 2012; Gaynor, Moreno-Serra, & Propper, 2013; Gaynor, Propper, & Seiler, 2016; Kessler & McClellan, 2000; Kessler & Geppert, 2005). There are also examples of null findings and negative effects, in cardiac care and other areas (Colla et al., 2016; Gowrisankaran & Town, 2003; Mukamel, Zwanziger, & Tomaszewski, 2001; Moscelli, Gravelle, & Siciliani, 2016; Moscelli, Gravelle, Siciliani, & Santos, 2018; Moscelli, Gravelle, & Siciliani, 2018; Skellern, 2018).

[2]In a market price context, FE studies of Australian primary care find weak or zero association between competition and primary care quality (Gravelle, Scott, Sivey, & Yong, 2016; Johar, Jones, & Savage, 2014).

TABLE 1 Timing of patient choice reforms

Year	Date	Reforming regions
2007	Jan 1	Halland
2008	Jan 1	Stockholm, Västmanland
	Jan 1	Uppsala
	Mar 1	Kronoberg
2009	May 1	Skåne
	Sep 1	Östergötland
	Oct 1	Västra Götaland
	Jan 1	Blekinge, Dalarna, Gävleborg, Jämtland, Kalmar, Norrbotten, Södermanland, Västernorrland, Västerbotten, Örebro
2010	Mar 23	Gotland
	May 3	Värmland
	Jun 1	Jönköping

Note. Source: Swedish Competition Authority (2012).

Primary care is provided via group practices, primary care centers (PCCs). The payment system for PCCs varies, but 70%–80% is typically capitation based.[3] Fee-for-service makes up most of the residual, while pay-for-performance is a small share of payments (Ellegård, Dietrichson, & Anell, 2018; Anell, Dackehag, & Dietrichson, 2018). The PCC staff are nearly always salaried and are thus not affected directly by the design of the payment system (Anell, Glenngaard, & Merkur, 2012). A PCC typically employs four to six 6 GPs plus nurses, but the staff may also include physiotherapists, occupational therapists, social workers, and cognitive therapists (Anell, 2015). The median number of registered patients was 7,300 in 2011, though private PCCs were usually smaller (median = 5,700). The regions own and operate most PCCs, although private providers' share of total expenditure increased from 15% in 2005 to 27% in 2013. The share of private providers varies considerably across regions: In 2013, the min–max range was 14%–50%. Most private PCCs are for-profit, limited liability companies.

Long waiting times and low responsiveness to patients are long-standing concerns in Swedish primary care. In 2007–2009, the center-right local governments of eight regions tried to address these problems by implementing reforms that removed restrictions on patients' choice of PCC and instituted free entry for PCCs (Anell, 2015). Inspired by these local reforms, the national center-right wing government enacted the Act of Free Choice (SFS, 2008:962, 2008:962), which obliged all regions to enact similar reforms by 2010. Table 1 shows the reform date of each region.

All reforms shared three components. First, barriers to entry were reduced: The region may not block the establishment of new care centers that fulfill prespecified accreditation criteria (differing across regions), for example, with respect to the set of services, a PCC must offer.[4] Second, switching costs were reduced. Providers must accept all patients, and patients may switch whenever they like. Although many regions formally allowed patients to switch providers before the reforms, they did not promote it actively. Third, search costs have decreased as the regions have substantially increased the amount of publicly available information about providers. Since April 2010, the regions run a joint website, 1177.se, at which patients can find information about all PCCs. During our study period, the website included information about, for example, opening and telephone hours, professions and expertise available at each PCC, and patient satisfaction ratings.

3 | EMPIRICAL STRATEGY

3.1 | Identification

Identifying the effects of increased patient choice and reduced barriers to entry on quality is challenging, as unobserved determinants of quality and reverse causality may bias the estimates. We exploit plausibly exogenous variation induced by the reforms in a DID analysis. Because all reforms were rolled out in a short time span, we cannot simply use late-adopting regions as a control group.[5] Similar to Cooper et al. (2011) and Gaynor et al. (2013), our DID instead exploits prereform spatial variation in local market conditions. By letting municipalities define local markets, we obtain variation in treat-

[3]One exception is that the capitation share in Region Stockholm was only 40% during our study period. Our results are robust to excluding Stockholm (results available on request).
[4]Anell et al. (2018) examine how requirements and regulations affect entry by private providers and find few stable associations.
[5]A few months is too short a follow-up period for our objective outcome measure, which pick up downstream effects of primary care. For the subjective measures, no regions remain untreated during two survey waves.

ment status within regions and thus mitigate confounding issues related to other region-wide changes. Using local markets as the unit of analysis also has advantages relative to a provider-level analysis, which is susceptible to selection effects (e.g., treatment effects may be due to selection in the types of patients flowing between providers rather than to behavioral responses by providers). The municipality is a conservative market definition as the relevant market is smaller for most patients. However, studying markets has disadvantages when it comes to teasing out the mechanisms behind the effects (see Section 6).

Our treatment group consists of local markets where the reforms plausibly had stronger impact. We posit that areas with many patients per PCC before the reform would be more attractive for potential entrants, as it ought to be easier to attract a sufficiently large patient stock to make a profit in these areas. We consider the market structure 6 months before the reform and calculate the average number of patients per care center if another center would enter the municipality.[6] If the number is greater than 5,500, the municipality is classified as treated, that is, if

$$\frac{Population}{N.o.Centers + 1} > 5,500. \tag{1}$$

The 5,500 threshold is slightly below the median patient stock of private PCCs and roughly corresponds to the minimum patient stock required to break even estimated by an initiated expert in the Region Skåne (personal communication with Magnus Kåregård). In Online Appendix C, we show that our results are robust to higher and lower cutoffs and to a continuous treatment definition based on the left-hand side of Equation (1).[7]

To build confidence in our estimation strategy, we show in Section 5.1 that postreform entry was much more frequent in our treatment group. We cannot test directly whether the treatment group was also at least as affected by the reduced search and switching costs. However, these costs ought to be irrelevant in monopoly markets and of greater importance in markets with several providers. Indeed, the treatment group had more providers on average, fewer monopolies, more entries, and more examples of prereform monopolies that were broken up after the reforms. Survey data reported in Online Appendix B indicates that residents in the treatment municipalities feel more informed about primary care providers than residents in the comparison group, although the difference is not statistically significant.

3.2 | Estimation

We use variations of the following DID model on municipality-level data to examine the plausibility of our treatment definition and to estimate effects on our objective quality measure:

$$y_{mt} = \alpha + \beta T_{mt} + \mu_m + \theta_t + \delta_q + \lambda_y + \varepsilon_{mt}, \tag{2}$$

where y_{mt} is the outcome variable, the treatment indicator T_{mt} is the interaction between a dummy for treated municipalities and a postreform dummy ($T_{mt} = treated_m * post_t$), μ_m are municipality FEs, ε_{mt} is an error term, and θ_t, δ_q, and λ_y are month-compared-to-reform, quarter-of-the-year, and year FEs. t indexes time relative to the reform; thus, all reforms are implemented at $t = 0$. We use a balanced[8] sample from 90 months pre- to 42 months postreform.[9] Because the municipalities differ widely in terms of population size, the regressions are weighted by the annual population size using Stata's *aweight* command (Solon, Haider, & Wooldridge, 2015; StataCorp, 2013).

To establish that our treatment in Equation (1) is sensible, we first estimate Equation (2) with the number of PCCs in the municipality as the outcome variable. We also estimate cross-sectional regressions in which the outcome variable is a dummy indicating whether the number of centers (in a given period) has increased relative to the situation 12 months before the reform and the treatment group dummy is the only explanatory variable.

To examine objective quality, we estimate several versions of Equation (2). We first use a flexible version that allows the reforms to have a differential effect in each of the years relative to the reform. That is, we replace βT_{mt} by interactions between a dummy for being a treated municipality and a set of year-to-reform dummies (12-month periods centered

[6]The treatment group is identical if we look at the situation 24 months prereform.
[7]As entry-deterring measures are scarce in primary care, it seems unlikely that the incentives to invest in quality are nonmonotone in our proxy for profitability (Dafny, 2005; Ellison & Ellison, 2011).
[8]1 (2) month(s) of data is missing for three (one) small municipalities. One newly created municipality is not observed for the initial 18 preperiod months.
[9]Balancing the sample in this way avoids the potential bias in DID/two-way FE models discussed, for example, by Athey and Imbens (2018), de Chaisemartin and D'Haultfœuille (2018), Goodman-Bacon (2018), and Strezhnev (2018).

TABLE 2 Descriptive statistics prereform

	Treatment				Comparison			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variable	**Mean**	**SD**	**Min**	**Max**	**Mean**	**SD**	**Min**	**Max**
ACSC rate (cases/10,000 residents)	15.5	4.1	4.9	36.7	18.2	6.9	1.9	54.5
Population size (number of residents)	51,454	81,245	11,126	795,163	11,154	7,253	2,500	62,388
Pop density (residents per km^2)	226.8	590.7	1.2	4,228.2	31.6	105.1	0.2	1161.4
Mean *income* (1,000 SEK)	239.2	27.4	203.0	370.7	216.7	24.0	184.1	442.6
Share of *children* (<10 years, %)	11.2	1.6	8.6	17.3	9.8	1.4	7.1	15.8
Share of *elderly* (≥65 years, %)	18.4	3.3	10.3	25.0	22.3	3.2	11.8	30.4
Share with ≤*primary* education (%)	23.9	4.1	12.6	36.7	28.1	3.7	11.2	37.4
Municipalities	147				142			

Note. Time averages, calculated over the period 18 to 7 months prereform for each Swedish municipality. *ACSC rate* is based on monthly municipality-level data. All other variables are based on yearly municipality-level data. Sources: National Board of Health and Welfare (Socialstyrelsen), Statistics Sweden.

around the implementation date), $\beta_{\tilde{t}}(treated_m * reformyear_{\tilde{t}})$. Secondly, we let the treatment indicator βT_{mt} take the value of 1 from the reform month and onward. Starting from a baseline model without covariates, we sequentially add municipality-specific linear trends $\kappa_m t$ and municipality-year-level covariates X_{mt}.[10] Finally, we allow for anticipation effects and a sluggish response to the reforms by not using the potentially noisy period around the implementation date in the DID comparison. In this "donut" specification, we let treatment start 6 months after a reform implementation (rather than at $t = 0$) and include an additional treatment dummy for the 12 months around a reform.

For our four subjective quality measures, we estimate the following DID equation on patient-level survey data:

$$y_{imt} = \alpha + \beta T_{mt} + \mu_m + \lambda_t + \varepsilon_{imt}, \tag{3}$$

where y_{mt} is one of four dependent variables, T_{mt} is a treatment indicator, μ_m are municipality FEs, λ_t are survey wave FEs, and ε_{imt} is an error term. We use a linear probability model (LPM), as all variables are binary. As the probability of an individual being included in the surveys differs across municipalities, we weight each observation by the inverse of the selection probability (the number of surveys sent out divided by the municipality's population size).

Standard errors are clustered at the region level whenever computationally possible and at the municipality level otherwise. The results are qualitatively similar when we use the wild cluster bootstrap, which may be more appropriate given the small number of clusters when we cluster at the region level (Cameron, Gelbach, & Miller, 2008; Cameron & Miller, 2015, see Online Appendix).

4 | DATA

We use a unique register of all PCCs in Sweden 2005–2013, including starting and closing dates, geographic coordinates, and ownership details, to identify the number of centers per municipality. We combine this with official population statistics to arrive at the treatment definition in Equation (1).

Our objective quality measure derives from the Swedish national inpatient register, which includes dates and ICD10 codes for all inpatient episodes 1999–2013. The primary objective quality measure *ACSC rate* is the number of ACSC hospitalisations, that is, inpatient episodes that well-functioning primary care could prevent (e.g., Kringos, Boerma, Van Der Zee, & Groenewegen, 2013; Starfield et al., 2005), per 10,000 residents, by municipality and month. We use the Swedish definition of ACSC,[11] but other countries have similar definitions (AHRQ, 2001; NHS Group, Department of Health, 2014; Purdy, Griffin, Salisbury, & Sharp, 2009).

The ACSC estimations include a set of municipality and year-level covariates (and their squares): population size (*Population size*), population density (*Pop density*, residents per km^2), mean income level in thousands of SEK (*income*), share of 16–74 year olds with at most primary education (*primary*), share of children <10 years (*children*), and share of residents

[10]The variables in Table 2 and their squares.

[11]These chronic diagnoses are included in the measure: anemia, asthma, diabetes, heart failure, high blood pressure, chronic obstructive pulmonary disease, and ischaemic heart disease. The measure also includes acute diagnoses: bleeding ulcers, diarrhea, epileptic cramps, pelvis tract infections, pyelitis, and ear and respiratory tract infections (National Board of Health and Welfare, 2014).

TABLE 3 Definitions of subjective quality measures and descriptive statistics

Panel A: Dependent variables			
Variable	Description	Variable = 0	Variable = 1
Phone access	Phone accessibility	Poor/OK/Good	Very good/Excellent
Waiting times	Waiting time for appointment	More than 2 days	0–2 days
Overall impression	Overall rating of care/treatment	Poor/OK/Good	Very good/Excellent
Recommendation	Would you recommend the center?	No/Partly	Yes, completely

Panel B: Individual background characteristics	
Variable	Description
Stable contact	= 1 if respondent always sees the same physician, 0 otherwise
Visit0	= 1 if no previous visits at this primary care center, 0 otherwise
Visit1	= 1 if 1 previous visit at this primary care center, 0 otherwise
Visit23	= 1 if 2–3 previous visits at this primary care center, 0 otherwise
Visit4	= 1 if 4 previous visits at this primary care center, 0 otherwise
Poor health	= 1 if respondent's self-rated health = poor, 0 otherwise
OK health	= 1 if respondent's self-rated health = OK, 0 otherwise
Good health	= 1 if respondent's self-rated health = good, 0 otherwise
Very good health	= 1 if respondent's self-rated health = very good, 0 otherwise
Excellent health	= 1 if respondent's self-rated health = excellent, 0 otherwise

Panel C: Descriptive statistics						
		Treatment			Comparison	
	(1)	(2)	(3)	(4)	(5)	(6)
Variable	Mean	SD	Obs	Mean	SD	Obs
Phone access	0.488	0.500	18,461	0.500	0.500	12,696
Waiting times	0.562	0.496	17,945	0.563	0.496	12,256
Overall Impression	0.626	0.484	22,983	0.643	0.479	16,434
Recommendation	0.704	0.456	22,847	0.707	0.455	16,323
Stable contact	0.567	0.496	23,292	0.556	0.497	16,635
Stable missing	0.012	0.107	23,292	0.012	0.111	16,635
Visit0	0.164	0.370	23,292	0.160	0.366	16,635
Visit1	0.194	0.396	23,292	0.192	0.394	16,635
Visit23	0.351	0.477	23,292	0.358	0.479	16,635
Visit4	0.276	0.447	23,292	0.278	0.448	16,635
Visit missing	0.015	0.120	23,292	0.012	0.111	16,635
Poor health	0.064	0.246	23,292	0.070	0.254	16,635
OK health	0.289	0.453	23,292	0.303	0.459	16,635
Good health	0.322	0.467	23,292	0.330	0.470	16,635
Very good health	0.221	0.415	23,292	0.204	0.403	16,635
Excellent health	0.090	0.286	23,292	0.082	0.275	16,635
Health missing	0.014	0.117	23,292	0.011	0.107	16,635
No. municipalities	48				75	

Note. Panel C displays individual-level data from the 2009 wave of the national patient satisfaction survey conducted by the Swedish Association of Local Authorities and Regions. Means and standard deviations are estimated using weights accounting for the selection probability. Included regions: Blekinge, Dalarna, Gotland, Gävleborg, Jämtland, Jönköping, Kalmar, Södermanland, Värmland, Västerbotten, Västernorrland, and Örebro. Norrbotten implemented the reform in 2010, but as the region did not participate in the 2009 survey, it is not included in the estimations.

≥65 years of age (*elderly*). The covariates capture potential differences in patient casemix development and may increase the precision of the estimates. It is unlikely that these covariates were directly affected by the reforms. Table 2 shows prereform descriptive statistics for a period from 18 to 7 months prereform. The average population size and density are higher in the treatment group. The medians are considerably lower than the mean (31,349 and 60.3, respectively), indicating that the averages are affected by a few very large municipalities. That is, the population size of the municipalities in the treatment group vary, and many are rather small.

Our subjective quality measures derive from the 2009, 2011, and 2013 waves of the national patient satisfaction survey. All respondents had recently made a visit to the PCC they were asked to rate. The survey covers all PCCs in participating regions. As the first wave was conducted in the fall of 2009, there is no prereform data for regions that had already implemented their reforms by then (see Table 1). Thus, the analysis of subjective quality is restricted to the 12 regions

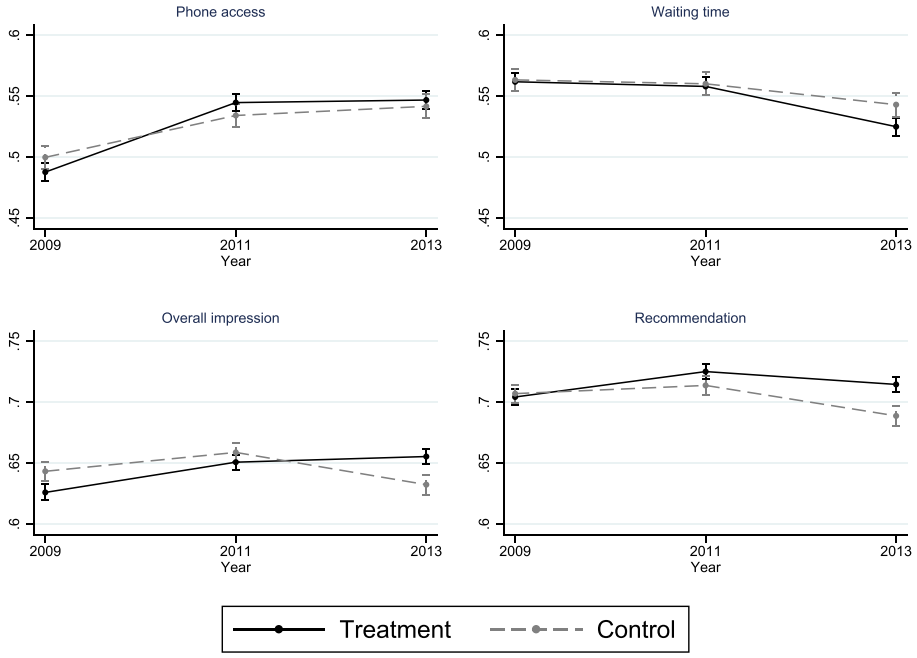


FIGURE 1 Average patient satisfaction ratings by survey wave and group. The graphs display, for each of the four dummy outcome variables, the share of respondents whose answers are coded as 1 (phone access ≥ very good, waiting time ≤ 2 days, overall impression ≥ very good, would completely recommend the PCC to others, respectively) with 95% confidence intervals. Means and confidence intervals are estimated using weights accounting for the selection probability. Treatment (black line) and comparison group (gray line). Only respondents from the 12 regions that implemented the reform in 2010 and were part of the survey in 2009 are included (i.e., Norrbotten is not included) [Colour figure can be viewed at wileyonlinelibrary.com]

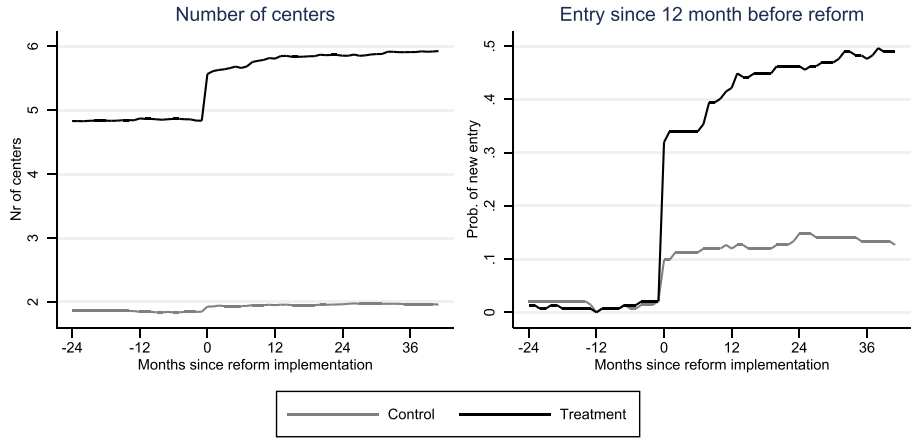


FIGURE 2 New entry. The two graphs show the average number of providers and share of municipalities with more providers after a reform, by treatment status. The lines display the coefficients from regressing (i) (right graph) the number of PCCs in the municipality and (ii) (left graph) an indicator variable equal to 1 if there are more PCCs in a certain month than 12 months before a reform on the treatment indicator [Colour figure can be viewed at wileyonlinelibrary.com]

(123 municipalities) that implemented their reforms in 2010 and participated in the 2009 survey. With this restriction, we have a yearly sample of 30,000–40,000 respondents. None of the three largest urban areas are included.

The average municipality-level response rate is 56% (stable across survey waves), though the response rates differ between survey questions. We construct four dummy variables capturing subjective quality: *Phone access*, *Waiting times*, *Overall impression*, and *Recommendation* (see the upper part of Table 3 for definitions). For all dummy variables, the value 1 indicates better quality. Figure 1 displays, by treatment status and survey wave, the share of observations coded as 1 for each of the four outcome variables. The difference in levels in the prereform survey in 2009 is very small for all variables. The shares are consistently quite far below 1, suggesting that ceiling effects are not a concern.

We also have individual-level data on respondents' self-rated health and previous experiences with their PCC (due to secrecy agreements, we were not provided access to other background variables).[12] Table 3 contains variable definitions and descriptive statistics for the prereform survey wave.

[12]The dataset does not include any personal data, that is, information relating to an identifiable person who can be directly or indirectly identified in particular by reference to an identifier. Therefore, the study does not require ethical review (SFS, 2003:460, 2003:460).

TABLE 4 Estimations on market structure

	All municipalities			Municipalities with subj. measure data		
	(1) *Centers* DID	(2) *Early entry* LPM	(3) *Lasting entry* LPM	(4) *Centers* DID	(5) *Early entry* LPM	(6) *Lasting entry* LPM
Treatment	0.848***	0.242***	0.363***	0.529***	0.170**	0.331***
	(0.225)	(0.0546)	(0.0622)	(0.147)	(0.0705)	(0.0806)
Constant		0.0986***	0.127***		0.0800**	0.107***
		(0.0282)	(0.0256)		(0.0316)	(0.0359)
Municipality FE	Yes	-	-	Yes	-	-
Year FE	Yes	-	-	Yes	-	-
Quarter FE	Yes	-	-	Yes	-	-
Month to reform FE	Yes	-	-	Yes	-	-
Observations	19,074	289	289	8,118	123	123
Municipalities	289	289	289	123	123	123
Regions	21	21	21	12	12	12

Note. Estimations of new entry in all regions (Columns 1–3) and in the 12 regions for which we have information about subjective measures (Columns 4–6). The unit of observation is the municipality. Columns 1 and 4 show DID estimates of a specification where *Treatment* is the interaction between a treatment group dummy and region-specific dummies for postreform months. The dependent variable is the number of PCCs (*Centers*). Columns 2–3 and 5–6 show estimates from cross-sectional regressions with a treatment group dummy as the only explanatory variable. The dependent variables are dummies for having more PCCs than 12 months before the reform, in the month following the reform implementation (*Early entry*) and after 42 months (*Lasting entry*). Standard errors (in parentheses) are clustered by region in Columns 1–3 and by municipality in Columns 4–6. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5 | RESULTS

5.1 | Market structure

In December 2005—1 year before the first reform—there were 958 PCCs in Sweden, that is, 0.93 PCCs per capita in the treatment group (147 municipalities) and 1.7 PCCs per capita in the comparison group (142 municipalities); 7 years later, after all regions had implemented their reforms, the number of centers had increased by 20% to 1,159.

The increase primarily occurred in the municipalities defined as treated. The leftmost graph in Figure 2, which depicts the average number of PCCs by treatment status and month (in event time), illustrates that the average number of PCCs increased rather quickly postreform in both groups, but to a much larger extent in the treatment group. While the increase in the average number of PCCs was close to one in the treatment group, it was considerably smaller in the comparison group. The rightmost graph in Figure 2 shows the share of municipalities that had more PCCs than 12 months before reform (by period). The graph confirms that entry took place rather quickly postreform and indicates that entry was not confined to only a few municipalities. While the prereform differences between treatment and comparison groups are never statistically significant, all postreform differences are strongly significant.

Table 4 further illustrates the difference between the treatment and comparison groups. Column 1 presents the estimate from a DID model as specified in Equation (2) using the number of PCCs as the outcome variable. The treatment effect amounts to 0.85 centers and is statistically significant. Columns 2 and 3 show estimates from LPMs in which the outcome variables equal to one if a municipality had more PCCs in the 1st month (column 2), and in the 42nd month (column 3), postreform compared to 12 months before the reform. The coefficients on the treatment dummy are highly significant and large (0.242 and 0.363, to be compared with the comparison group mean of 0.099 and 0.127). Columns 4–6 show that the entry patterns differ between control and treatment groups in the same way in the subset of municipalities for which we may study the subjective quality measures (12 regions; 48 treatment and 75 comparison municipalities). The smaller increase in the number of centers is due to the exclusion of the largest municipalities.

The higher entry in the treatment group is not only explained by the higher prevalence of large urban municipalities in that group: the smaller, less densely populated, and more rural municipalities in the treatment group also contribute to the difference relative to the comparison group. For instance, 6 months prereform, there were 43 treatment and 77 comparison municipalities with only one PCC; 19 (44%) of these small municipalities experienced entry after the reform in the treatment group, but only 6 (8%) in the comparison group.

An increased number of PCCs may not only increase competition but also access to care, but the reforms left key measures of access relatively unaffected: In Online Appendix A, we show that a very large share of new care centers were

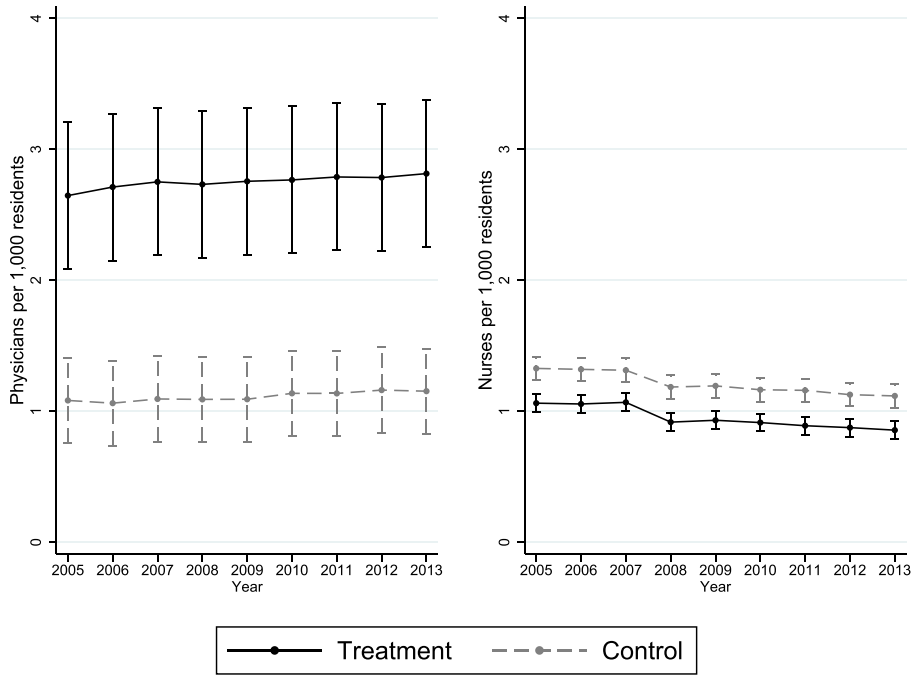


FIGURE 3 Development of physician and nurse densities by group. The lines display the means and confidence intervals for the physician and nurse densities (employed physicians/district nurses per 1,000 residents) by treatment and control group [Colour figure can be viewed at wileyonlinelibrary.com]

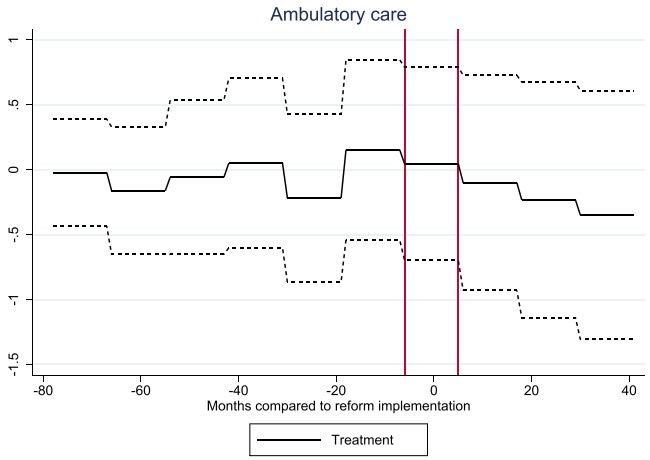


FIGURE 4 Yearly treatment effects on the ACSC hospitalisation rate. The lines display the $\beta_{\tilde{t}}$ coefficients (solid) and 95% confidence intervals (dashed) from a flexible version of Equation (2) in which the time-invariant treatment effect specification βT_{mt} is replaced by interactions between a dummy for being a treated municipality and a set of year-to-reform dummies (indicating 12-month periods centered around the implementation date), $\beta_{\tilde{t}}(treated_m * reformyear_{\tilde{t}})$. The first 12-month period is used as reference period and is therefore excluded from the figure. The model includes covariates. Standard errors used to calculate the confidence intervals are clustered by municipality. The prereform mean (standard deviation) of *ACSC rate* is 17.1 (6.1) [Colour figure can be viewed at wileyonlinelibrary.com]

established in the proximity of existing centers. Accordingly, a report from the Swedish Competition Authority (Swedish Competition Authority, 2010) concluded that the travel time to the second closest care center changed little for most residents between 2009 and 2010. Furthermore, Figure 3 shows that the physician and nurse densities developed similarly over time in the treatment and comparison groups. Although the occupation statistics do not discriminate between GPs and physicians with other medical specialties, it seems unlikely that physicians would switch from secondary to primary care to staff the new PCCs. Instead, it seems like the existing primary care workforce was spread out over more units in the treatment municipalities.

We conclude that our treatment group experienced entry of new centers to a substantially larger degree than the comparison group. Entry took place relatively fast after the reforms and lasted over time. Although the number of providers increased, there is little to indicate that access to primary care—travel distance and staff density—improved, leaving competition as a plausible mechanism for reform effects on care.

5.2 | Objective and subjective quality measures

Figure 4 shows the differences between the *ACSC rate* of the treatment and comparison groups obtained by a flexible version of Equation (2) allowing for time-varying treatment effects and differential prereform trends. The red vertical lines mark the implementation year. The figure does not indicate systematic pretrend differences between the treatment and comparison groups: There are positive as well as negative "placebo" estimates, and all of them are small and statisti-

TABLE 5 DID models of avoidable hospitalisations (*ACSC rate*)

	(1) Baseline	(2) Linear trends	(3) Covariates	(4) Donut
Treatment	–0.358	–0.338	–0.169	–0.0288
	(0.330)	(0.267)	(0.286)	(0.378)
Linear trends	No	Yes	Yes	Yes
Covariates	No	No	Yes	Yes
Observations	38,125	38,125	38,125	38,125
Municipalities	289	289	289	289
Regions	21	21	21	21

Note. Dependent variable: *ACSC rate*. *Treatment* = 1 if there were more than 5,500 patients per PCC + 1 6 months prereform. All specifications include municipality, year, quarter, and month-to-reform FEs. Columns 2–4 include municipality-specific linear trends. Columns 3–4 include municipality-and-year level covariates. Column 4 includes a dummy for the 6 months prereform and postreform (a "donut hole"). The estimation sample includes all 289 municipalities and covers the period from 90 months pre- to 42 months postreform. Standard errors clustered by region in parentheses. ***$p <$ 0.01, **$p < 0.05$, *$p<0.1$.

cally insignificant. The confidence intervals (dashed lines) are calculated using standard errors clustered by municipality instead of region, as the covariance matrix is otherwise not of full rank. This choice likely implies that the interval is too narrow, but as the figure clearly show, the coefficients are far from significant anyway. The largest pretrend difference is –0.22, which is less than 0.04 of the prereform overall standard deviation of 6.1. All postreform treatment effects are negative, and increasingly so over time, indicating quality improvements. However, all estimates are statistically insignificant and small. For example, the largest (i.e., the last) estimate corresponds to 0.06 of a standard deviation.

Table 5 presents estimates of Equation (2) using a constant treatment effect. The estimates without covariates in Columns 1–2 are negative but statistically insignificant, amounting to around 0.06 of a standard deviation. The magnitude of the effect decreases when including covariates (Column 3, 0.03 standard deviations) and is extremely close to zero when using a donut specification (Column 4, 0.0003 standard deviations).[13] If there are unobserved determinants related to both the treatment variable and the *ACSC rate*, for example, changes in the patient casemix, then these unobserved determinants are likely associated to the treatment and outcome in a similar way as the observed covariates, suggesting that our estimates, if anything, are biased away from zero (e.g., Oster, 2019).

Table 6 presents estimates for the four subjective quality measures. For *Phone access*, *Overall impression*, and *Recommendation*, the estimated treatment effects are positive and amount to 3%–4% of the prereform mean. However, only *Overall impression* is statistically significant. The estimate on *Waiting time* is negative but statistically insignificant and small in relation to the mean. Individual level covariates only make the coefficients somewhat more precisely estimated.

5.3 | Sensitivity analyses

In the Online Appendix, we show that the results are robust to a wide range of sensitivity tests such as using other treatment cutoff levels or a continuous treatment definition.

For the *ACSC rate*, the effects are still insignificant when using the same sample as in the estimations for the subjective measures, and we find no effects on another objective quality measure (unplanned hospitalisations). For the subjective measures, the results are robust to excluding individuals not responding to all of the four survey questions from the estimation sample.

Excluding population/survey weights yields statistically insignificant coefficients for all measures, indicating that larger municipalities experienced larger quality improvements. Notably though, the average effects do not hide substantial effects of opposing sign for large and small municipalities: When excluding the 5% largest and smallest municipalities from the sample, we obtain estimates that are close to zero and insignificant for the *ACSC rate*, and the estimates for the subjective measures are similar to the baseline (though less precise).

In Section 5.1, we found little to indicate that the new PCCs improved access in a substantial sense.[14] To further guard against the possibility that our estimates capture the net effect of increased access and increased competition, we con-

[13]In a DID contrasting only the last observed postreform year to the prereform period, the estimated treatment effect is of the same magnitude as the last estimate in the flexible model presented in Figure 4. The estimate is not statistically significant at the 10% level.

[14]The new care centers were mainly located in the close proximity of existing providers and the physician and nurse density developed similarly in the treatment and comparison group. Further evidence in this regard is that the subjective measure most directly linked to access to care, *Waiting times*, did not improve more in the treatment group.

TABLE 6 Subjective measures of primary care quality

Panel A: No covariates				
	(1)	(2)	(3)	(4)
Dependent variable:	*Phone access*	*Waiting times*	*Overall impression*	*Recommendation*
Treatment	0.0208	–0.00787	0.0264**	0.0230
	(0.0171)	(0.0115)	(0.0126)	(0.0141)
Observations	92,194	89,744	118,649	117,920
Municipalities	123	123	123	123
Regions	12	12	12	12
Mean of dependent	0.492	0.563	0.631	0.705
Panel B: Individual level covariates				
	(1)	(2)	(3)	(4)
Dependent variable:	*Phone access*	*Waiting times*	*Overall impression*	*Recommendation*
Treatment	0.0198	–0.00715	0.0248**	0.0212*
	(0.0158)	(0.0117)	(0.0101)	(0.0115)
Stable contact	0.133***	–0.0864***	0.223***	0.227***
	(0.00564)	(0.00520)	(0.00460)	(0.00481)
Stable missing	0.0489***	–0.0377**	0.0555***	0.120***
	(0.0160)	(0.0184)	(0.0148)	(0.0140)
OK health	0.0607***	0.0535***	0.0989***	0.112***
	(0.00633)	(0.00821)	(0.00691)	(0.00678)
Good health	0.0955***	0.105***	0.178***	0.168***
	(0.00757)	(0.00856)	(0.00746)	(0.00667)
Very good health	0.180***	0.170***	0.300***	0.217***
	(0.00847)	(0.00835)	(0.00850)	(0.00680)
Excellent health	0.201***	0.247***	0.340***	0.250***
	(0.00996)	(0.00877)	(0.00954)	(0.00820)
Health missing	0.113***	0.104***	0.158***	0.158***
	(0.0177)	(0.0196)	(0.0171)	(0.0165)
Visit1	–0.00641	0.0448***	–0.00936*	–0.0197***
	(0.00598)	(0.00488)	(0.00512)	(0.00504)
Visit23	0.00369	0.0889***	–0.0104**	–0.0367***
	(0.00537)	(0.00515)	(0.00420)	(0.00495)
Visit4	0.0112*	0.138***	0.0146***	–0.0435***
	(0.00589)	(0.00570)	(0.00493)	(0.00497)
Visit missing	–0.0143	0.0833***	–0.0420***	–0.0169
	(0.0185)	(0.0172)	(0.0157)	(0.0179)
Observations	92,194	89,744	118,649	117,920
Municipalities	123	123	123	123
Regions	12	12	12	12

Note. The sample covers three survey waves (2009, 2011, and 2013). The differences in observations across columns reflect differences in response rates to the four survey questions. All models include municipality and survey FEs. Standard errors clustered by municipality in parentheses. ***$p<0.01$, **$p<0.05$, *$p<0.1$.

struct a sample including only municipalities where the number of PCCs did not increase (thus, access certainly did not improve). Contrasting municipalities with at least two PCCs (nonmonopolies) to municipalities where there was only one PCC (monopolies), we find that objective quality improved at least as much, and subjective quality significantly more, in the former group. Notably, the reforms potentially increased the competitive pressure on nonmonopolies, as more information about PCCs became available and switching costs were reduced. The monopolies, on the other hand, were not affected, as their patients had no alternative provider to switch to. Although this sample is selective, these estimates suggest that the reduced entry barriers was not the only important component of the reform, and that increased competition, rather than increased access, explains the modest improvements of the subjective quality measures in our baseline estimations. On a related note, the effects are similar in markets where reform-driven entry broke up a local monopoly as in markets with multiple PCCs.

In specifications using respondents' self-reported health and previous primary care experiences as dependent variables, we find no indications that the patient casemix diverged in treatment and comparison municipalities. Thus, the significant

increase in the overall impression of care quality is *not* driven by treatment group PCCs catering more to the demands of healthier patients (one type of cream-skimming).

For the subjective measures, a concern may be that center-right voters were in favor of the patient choice policy and rated their PCC higher postreform to avoid cognitive dissonance. However, the results are similar regardless of the municipality's share of center-right voters.

6 | LIMITATIONS

Some limitations of our study are noteworthy. First, our estimates may be biased if there are unobserved changes in the patient casemix. Such trends are difficult to completely rule out, but given that we find only small pretrend differences and that our covariate sets include either the population size, density and age structure (ACSC models), or the individuals' self-reported health (subjective measures models), we believe that the scope of this bias is limited.

Second, our treatment definition might understate the effects of the reforms if some of the comparison municipalities were more affected than some of the municipalities in the treatment group. We do however find similar results with a continuous measure, which should be less sensitive to this type of measurement error. Noteworthy, the coefficient for phone access is statistically significant at the 10% level with the continuous specification.

Third, our conclusions are based on a follow-up period of approximately 3 years. This period may be too short to capture changes in the outcome measures. In particular, there might be a longer lag between changes in the treatment of individuals with chronic conditions and changes in these individuals' need for inpatient care. The follow-up period of our study nonetheless compares favorably to most studies of competition in the health care sector, including those using similar outcome measures.

Fourth, our paper is not a comprehensive evaluation of the welfare effects of the competition enhancing components of the reforms. For instance, taking fixed entry costs into account, there might have been excess entry (e.g., Mankiw & Whinston, 1986). Our data do not allow us to establish whether the number of entries was optimal. Furthermore, we cannot establish whether the improvements of patients' impressions are driven by improvements at existing practices or by new entrants offering higher quality services. With increased patient choice, providers (incumbent and new) may also find it worthwhile to engage in horizontal differentiation, in which case the improved patient satisfaction may be due to improved patient-provider matching. As we find that increased patient choice by itself is associated with higher satisfaction (comparing monopoly and non-monopoly markets with no entry), our main results are likely not exclusively due to new entrants offering higher quality or inducing horizontal differentiation.

7 | CONCLUDING REMARKS

We find modest improvements of patients' overall impressions of care quality, but no significant improvements of the avoidable hospitalisation rate or (patient satisfaction with) waiting times in primary care markets more affected by reforms that increased patient choice and reduced barriers to entry. We present several pieces of evidence suggesting that the reforms had limited effect on patients' access to care, leaving increased competition as the most likely mechanism behind the results. Our results are in line with the small literature examining the causal effects of competition on primary care quality in markets with regulated prices (Gravelle et al., 2019).

Frictions on both the demand and the supply side may limit the potential for quality competition in the Swedish primary care market. Survey evidence suggests that patients rarely use publicly reported information to evaluate providers (Glenngård, Anell, & Beckman, 2011; Swedish Agency for Health and Care Services Analysis, 2013), and randomised field experiments indicate that patients are more likely to switch when switching costs are reduced and they are provided with comparative information about nearby PCCs by postal mail (Anell, Dietrichson, Ellegård, & Kjellsson, 2017).

On the supply side, qualitative research indicates that the fixed costs of acquiring a new facility limits the incentives to expand beyond current capacity constraints (Vengberg, Fredriksson, & Winblad, 2019). Further, the factors leading to popularity may not be scalable, as the human capital of individual physicians and nurses cannot simply be duplicated. In fact, recruiting is a challenge according to a majority of primary care managers (Petterson & Jaktlund, 2013). Finally, in a mainly capitation-based reimbursement system, the incentive to compete on quality to keep the current patient stock is counteracted by the incentive to limit service provision. The relative importance of these factors is an interesting topic for future research.

ACKNOWLEDGEMENT

We are grateful to Hugh Gravelle, Rita Santos, Andrew Wilcock, and participants at ASHEcon, Health Economics Study Group workshop, the SFI–Lund workshop in Health Economics, and the Third Swedish National Conference in Health Economics for helpful comments. We thank Mahan Nikpour (Swedish Association of Local Authorities and Regions), Stefan Jönsson (Swedish Competition Authority), and Håkan Lennhoff (Swedish Agency for Health and Care Services Analysis) for providing data, and Magnus Kåregård (Region Skåne) for information about Swedish patient choice systems. This work was supported by the Swedish Competition Authority (grant 316/2013), FORTE (grant 2014/0861), and the Crafoord foundation (grant number 2014/0664). No funding sources were involved in the design, data collection, analysis, or interpretation of data for this article.

CONFLICT OF INTEREST

The authors have no conflict of interest. This work has previously appeared in the working papers series of the Department of Economics, Lund University (WP no. 2016:36).

ORCID

Jens Dietrichson https://orcid.org/0000-0003-4178-7682
Lina Maria Ellegård https://orcid.org/0000-0003-1311-9927
Gustav Kjellsson https://orcid.org/0000-0002-0843-1038

REFERENCES

AHRQ (2001). Guide to prevention quality indicators: Hospital admission for ambulatory care sensitive conditions. URL: http://www.qualityindicators.ahrq.gov/Downloads/Modules/PQI/V31/pqi_guide_v31.pdf, Services Agency for Healthcare Research and Quality, Department of Health and Human Services Website accessed 09 December 2016.

Anell, A. (2015). The public-private pendulum—Patient choice and equity in Sweden. *New England Journal of Medicine*, *372*(1), 1–4.

Anell, A., Dackehag, M., & Dietrichson, J. (2018). Does risk-adjusted payment influence primary care providers' decision on where to set up practice? *BMC Health Services Research*, *18*, 179.

Anell, A., Dietrichson, J., Ellegård, L. M., & Kjellsson, G. (2017). Information, switching cost, and consumer choice: Evidence from two randomized field experiments in Swedish primary care. (*2017:7*): Working Paper, Department of Economics, Lund University.

Anell, A., Glenngaard, A. H., & Merkur, S. M. (2012). Sweden: Health system review. *Health systems in transition*, *14*(5), 1–159.

Arrow, K. J. (1963). Uncertainty and the welfare economics of medical care. *American Economic Review*, *53*(5), 941–973.

Athey, S., & Imbens, G. W. (2018). Design-based analysis in difference-in-differences settings with staggered adoption. (*w24963*): National Bureau of Economic Research.

Bailey, M. J., & Goodman-Bacon, A. (2015). The war on poverty's experiment in public medicine: Community health centers and the mortality of older americans. *American Economic Review*, *105*(3), 1067–1104.

Berlin, C., Busato, A., Rosemann, T., Djalali, S., & Maessen, M. (2014). Avoidable hospitalizations in Switzerland: A small area analysis on regional variation, density of physicians, hospital supply and rurality. *BMC Health Services Research*, *14*, 289.

Bloom, N., Propper, C., Seiler, S., & Van Reenen, J. (2015). The impact of competition on management quality: Evidence from public hospitals. *Review of Economic Studies*, *82*(2), 457–489.

Brekke, K. R., Holmås, T. H., Monstad, K., & Straume, O. R. (2019). Competition and physician behaviour: Does the competitive environment affect the propensity to issue sickness certificates? *Journal of Health Economics*, *66*, 117–135.

Brekke, K. R., Siciliani, L., & Straume, O. R. (2014). Can competition reduce quality? (*Working Paper 4629*): CESifo.

Cameron, A. C., Gelbach, J. B., & Miller, D. L. (2008). Bootstrap-based improvements for inference with clustered errors. *Review of Economics and Statistics*, *90*(3), 414–427.

Cameron, A. C., & Miller, D. L. (2015). A practitioner's guide to cluster-robust inference. *Journal of Human Resources*, *50*(2), 317–372.

Colla, C., Bynum, J., Austin, A., & Skinner, J. (2016). Hospital competition, quality, and expenditures in the U.S. medicare population. (*Working Paper 22826*): NBER.

Cooper, Z., Gibbons, S., Jones, S., & McGuire, A. (2011). Does hospital competition save lives? Evidence from the English NHS patient choice reforms. *Economic Journal*, *121*(554), 228–260.

Cooper, Z., Gibbons, S., Jones, S., & McGuire, A. (2012). Does competition improve public hospitals' efficiency? Evidence from a quasi-experiment in the English national health service. (*CEP Discussion Paper 1125*): London School of Economics and Political Science, LSE Library.

Dackehag, M., & Ellegård, L. M. (2019). Competition, capitation and coding: Do public primary care providers respond to increased competition? *CESifo Economic Studies*, *65*, 402–423.

Dafny, L. S. (2005). Games hospitals play: Entry deterrence in hospital procedure markets. *Journal of Economics and Management Strategy*, *14*(3), 513–542.

de Chaisemartin, C., & D'Haultfœuille, X. (2018). Two-way fixed effects estimators with heterogeneous treatment effects. Unpublished manuscript.

Ellegård, L. M., Dietrichson, J., & Anell, A. (2018). Can pay-for-performance reimbursement stimulate the appropriate use of antibiotics? *Health Economics*, *27*(1), e39–e54.

Ellison, G., & Ellison, S. F. (2011). Strategic entry deterrence and the behavior of pharmaceutical incumbents prior to patent expiration. *American Economic Journal: Microeconomics*, *3*(1), 1–36.

Fogelberg, S. (2014). Effects of competition between healthcare providers on prescription of antibiotics. Unpublished manuscript, Stockholm University.

Gaynor, M., Ho, K., & Town, R. J. (2015). The industrial organization of health care markets. *Journal of Economic Literature*, *53*(2), 235–284.

Gaynor, M., Moreno-Serra, R., & Propper, C. (2013). Death by market power: Reform, competition and patient outcomes in the British National Health Service. *American Economic Journal: Economic Policy*, *5*(4), 134–166.

Gaynor, M., Propper, C., & Seiler, S. (2016). Free to choose? Reform, choice, and consideration sets in the English National Health Service. *American Economic Review*, *106*(11), 3521–3557.

Gaynor, M., & Town, R. J. (2011). Competition in health care markets. In Pauly, M. V., Mcguire, T. G., & Barros, P. P. (Eds.), *Handbook of Health Economics, Volume 2*. Amsterdam: Elsevier, pp. 499–637.

Glenngård, A. H., Anell, A., & Beckman, A. (2011). Choice of primary care provider: Results from a population survey in three Swedish counties. *Health Policy*, *103*, 31–37.

Goodman-Bacon, A. (2018). Difference-in-differences with variation in treatment timing. (*w25018*): National Bureau of Economic Research.

Gowrisankaran, G., & Town, R. J. (2003). Competition, payers, and hospital quality. *Health Services Research*, *38*(6), 1403–1421.

Gravelle, H., Liu, D., Propper, C., & Santos, R. (2019). Spatial competition and quality: Evidence from the english family doctor market. *Journal of Health Economics*, *68*, 102249.

Gravelle, H., Scott, A., Sivey, P., & Yong, J. (2016). Competition, prices and quality in the market for physician consultations. *The Journal of Industrial Economics*, *64*(1), 135–169.

ISF (2014). Vårdvalets effekter på sjukskrivningarna. (*2014:17*): Rapport från Inspektionen får socialfårsåkringen.

Iversen, T., & Lurås, H. (2000). Economic motives and professional norms: The case of general medical practice. *Journal of Economic Behavior and Organization*, *43*(4), 447–470.

Iversen, T., & Ma, C.-T. A. (2011). Market conditions and general practitioners' referrals. *International Journal of Health Care Finance and Economics*, *11*(4), 245–265.

Johar, M., Jones, G., & Savage, E. (2014). What explains the quality and price of GP services? An investigation using linked survey and administrative data. *Health Economics*, *23*(9), 1115–1133.

Jürges, H., & Pohl, V. (2012). Medical guidelines, physician density, and quality of care: Evidence from German SHARE data. *European Journal of Health Economics*, *13*(5), 635–649.

Kann, I. C., Biørn, E., & Lurås, H. (2010). Competition in general practice: Prescriptions to the elderly in a list patient system. *Journal of Health Economics*, *29*(5), 751–764.

Kessler, D. P., & Geppert, J. J. (2005). The effects of competition on variation in the quality and cost of medical care. *Journal of Economics and Management Strategy*, *14*(3), 575–589.

Kessler, D. P., & McClellan, M. B. (2000). Is hospital competition socially wasteful? *Quarterly Journal of Economics*, *115*(2), 577–615.

Kringos, D. S., Boerma, W. G., Hutchinson, A., & Saltman, R. B. (2015). *Building primary care in a changing europe*: WHO Regional Office for Europe.

Kringos, D. S., Boerma, W., Van Der Zee, J., & Groenewegen, P. (2013). Europe's strong primary care systems are linked to better population health but also to higher health spending. *Health Affairs*, *32*(4), 686–694.

Mankiw, N. G., & Whinston, M. D. (1986). Free entry and social inefficiency. *The RAND Journal of Economics*, *17*, 48–58.

Markussen, S., & Røed, K. (2016). The market for paid sick leave. (*No. 9825*): IZA Discussion Paper.

McGuire, T. G. (2011). Physician agency and payment for primary medical care. In Glied, S., & Smith, P. C. (Eds.), *The Oxford Handbook of Health Economics* (pp. 602–623).Oxford: Oxford University Press.

Moscelli, G., Gravelle, H., & Siciliani, L. (2016). Market structure, patient choice and hospital quality for elective patients. (*CHE Research Paper 139*): Centre for Health Economics, University of York.

Moscelli, G., Gravelle, H., & Siciliani, L. (2018). Effects of market structure and patient choice on hospital quality for planned patients. (*No. 1118*): School of Economics, University of Surrey.

Moscelli, G., Gravelle, H., Siciliani, L., & Santos, R. (2018). Heterogeneous effects of patient choice and hospital competition on mortality. *Social Science and Medicine*, *216*, 50–58.

Mukamel, D. B., Zwanziger, J., & Tomaszewski, K. J. (2001). HMO penetration, competition, and risk-adjusted hospital mortality. *Health Services Research*, *36*(6), 1019–1035.

NHS Group, Department of Health (2014). The NHS outcomes framework 2015/2016. Technical Report December 2014.

National Board of Health and Welfare (2014). Utveckling av indikatorerna undvikbar slutenvård och oplanerade återinskivningar. Socialstyrelsen, (*Report 2014-2-12*).

Oster, E. (2019). Unobservable Selection and Coefficient Stability: Theory and Evidence. *Journal of Business & Economic Statistics*, *37*(2):187–204. https://doi.org/10.1080/07350015.2016.1227711

Petterson, S., & Jaktlund, Å. (2013). Primärvårdens läkarbemanning—Öppna jämförelser mellan landsting och driftsformer av primärvårdens försörjning av specialistläkare 2012: Sveriges läkarförbund.

Pike, C. (2010). An empirical analysis of the effects of GP competition. (*MPRA Paper 27613*). Germany: University Library of Munich.

Propper, C. (2012). Competition, incentives and the English NHS. *Health Economics*, *21*, 33–40.

Propper, C., Burgess, S., & Gossage, D. (2008). Competition and quality: Evidence from the NHS internal market 1991–9. *The Economic Journal*, *118*(525), 138–170.

Purdy, S., Griffin, T., Salisbury, C., & Sharp, D. (2009). Ambulatory care sensitive conditions: Terminology and disease coding need to be more specific to aid policy makers and clinicians. *Public Health*, *123*(2), 169–173.

Rosano, A., Loha, C. A., Falvo, R., van der Zee, J., Ricciardi, W., Guasticchi, G., & de Belvis, A. G. (2013). The relationship between avoidable hospitalization and accessibility to primary care: A systematic review. *European Journal of Public Health*, *23*(3), 356–360.

Schaumans, C. (2015). Prescribing behavior of General Practitioners: Competition matters. *Health Policy*, *119*(4), 456–463.

SFS (2003:460). Lag om etikprövning av forskning som avser människor [Act on ethical review of research concerning human beings].

SFS (2008:962). Lag om valfrihetssystem [Act on free choice systems].

Skellern, M. (2018). The effect of hospital competition on value-added indicators of elective surgery quality.

Solon, G., Haider, S. J., & Wooldridge, J. M. (2015). What are we weighting for? *Journal of Human Resources*, *50*(2), 301–316.

Starfield, B., Shi, L., & Macinko, J. (2005). Contribution of primary care to health systems and health. *Milbank Quarterly*, *83*(3), 457–502.

StataCorp, L. (2013). Stata: Release 13-statistical software. TX: College Station.

Strezhnev, A. (2018). Semiparametric weighting estimators for multi-period difference-in-differences designs.

Stroka-Wetsch, M., Talmann, A., & Linder, R. (2016). Does competition in the out-patient sector improve quality of medical care? Evidence from administrative data. (*638*): Ruhr Economic Papers.

Swedish Agency for Health and Care Services Analysis (2013). Vad vill patienten veta för att välja? Vårdanalys utvärdering av vårdvalsinformation. Vårdanalys (*Report 2013:4*).

Swedish Competition Authority (2010). Uppföljning av vårdval i primärvården – valfrihet, mångfald och etableringsförutsättningar. Konkurrensverkets rapportserie 2010.

Swedish Competition Authority (2012). Val av vårdcentral - Förutsättningar för kvalitetskonkurrens i primärvården. Konkurrensverket, (*Report 2012:2*).

Vengberg, S., Fredriksson, M., & Winblad, U. (2019). Patient choice and provider competition–quality enhancing drivers in primary care? *Social Science and Medicine*, *226*, 217–224.

SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of the article.

How to cite this article: Dietrichson J, Ellegård LM, Kjellsson G. Patient choice, entry, and the quality of primary care: Evidence from swedish reforms. *Health Economics*. 2020;29:716–730. https://doi.org/10.1002/hec.4015