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# Effects of Increased Competition on Quality of Primary Care in Sweden

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**Abstract**

In the last decades, many health systems have implemented policies to make care providers engage in quality competition. But care quality is a multi-dimensional concept, and competition may have different impacts on different dimensions of quality. The empirical evidence on competition and care quality is scarce, in particular regarding primary care. This paper adds evidence from recent reforms of Swedish primary care that affected competition in municipal markets differently depending on the pre-reform market structure. Using a difference-in-differences strategy, we demonstrate that the reforms led to substantially more entry of private care providers in municipalities where there were many patients per provider before the reforms. The effects on primary care quality in these municipalities are modest: we find small improvements in subjective measures of overall care quality, but no significant effects on the rate of avoidable hospitalizations or patients' satisfaction with access to care. We find no indications of quality reductions.

Keywords: Competition, Patient choice, Primary health care, Quality
JEL Classification: I11; I18; H75; D04

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# 1 Introduction

High-quality primary care can free up resources in the more expensive secondary care sector, by preventing unnecessary hospitalizations (Starfield et al., 2005; Rosano et al., 2013). Finding policies that improve the quality of primary care is therefore of great interest. In accordance with standard health economic models, which propose that competition will improve quality when prices are regulated (McGuire, 2011; Gaynor and Town, 2011), policy-makers are keen to stimulate health care competition. For example, the last couple of decades have witnessed a wave of patient choice reforms aimed to increase demand responsiveness to quality.[1]

The relationship between competition and quality is however less straightforward than the standard models suggest. One reason is that many dimensions of care quality are unobservable for patients (Arrow, 1963). Therefore, competition-enhancing reforms do not necessarily increase patients' responsiveness to quality. In fact, the relationship between competition and quality is ambiguous even with well-informed patients: semi-altruistic physicians may respond to increased responsiveness of demand by reducing the quality offered to unprofitable patients (skimping), while increasing the quality offered to profitable patients (cream-skimming) (Brekke et al., 2014). Competition may also increase the importance of other opportunistic behaviors such as physician-induced demand, or leniency to perform patient-initiated procedures (Iversen and Lurås, 2000).

A variety of impacts of competition can be expected in the primary care sector, which deals with a diverse set of health problems and a heterogeneous patient group. The empirical evidence on the effects of competition on primary care quality is however scarce (Propper, 2012; Gaynor et al., 2015). The literature studying markets with regulated prices generally lacks the exogenous variation needed for causal inference, or uses outcome measures that either have an ambiguous relation to quality or directly measure opportunistic behavior. Studies looking at broader quality indicators are, with one notable exception (Gravelle et al., 2016a), based on either cross-sectional associations (e.g., Pike, 2010; Stroka-Wetsch et al., 2016), or longitudinal data with area fixed effects (Iversen and Lurås, 2000; Hanspers, 2013). The fixed effects approach may account for unobserved (geographical or demographic) common determinants of competition and quality, but still fails to address the endogeneity of entries and exits in the market.

In this paper, we provide evidence on causal effects of competition on broad measures of care quality. We use exogenous variation induced by a set of choice reforms implemented in Swedish primary care during 2007-2010. Since the reforms, there is free entry for providers meeting basic requirements, there is more publicly available information about providers, and it has become easier (and less costly) to search for and switch between providers. These three features opened up for more patient mobility in general, and potentially for higher demand responsiveness to quality. The number of providers has increased by around 20 percent since the reforms, solely due to entry of private providers.

Our identification strategy exploits that the impact of the reforms on competition was bound to differ between local markets. In resemblance to Propper et al. (2008), Cooper et al. (2011) and Gaynor et al. (2013), who study competition between hospitals in the English NHS, we exploit pre-reform variation in market conditions in a difference-in-differences (DID) model. We start from the plausible assumption that competition was intensified more in markets where there was room for new entrants before the reform. Put differently, we assume that potential entrants would find it more attractive to enter in municipalities where they could expect to attract a patient stock of reasonable size. Accordingly, we define a 'treatment group' consisting of municipalities where – six

[1] Examples include Australia, Belgium, Denmark, Israel, the Netherlands, the United Kingdom (UK), and the Medicare prescription drug coverage system in the United States (US; Cooper et al., 2011; Ikkersheim and Koolman, 2012; Santos et al., 2015).

months before a reform – the average number of patients per care provider would exceed 5,500, given the entry of one additional provider. This number roughly corresponds to the median patient list size of private primary care providers (the results are robust to varying the exact threshold).

In support of our identification strategy, we show that post-reform entry was indeed much more common in our treatment group than in the comparison group. The other competition-enhancing components of the reform – increased access to information, and reduced search and switching costs – were likely at least as important in the treatment group as in the comparison group. Information and switching costs should be of larger importance if there are more providers in the market, and there were more providers, as well as more entry, in our treatment group. Residents in these municipalities further perceived themselves to be informed about available providers to a similar (if not larger) degree than residents in the comparison municipalities. Notably, these dimensions of increased competition would not be captured by alternative estimation strategies using direct measures of competition (such as the number of providers in an area, or market concentration measures), as such measures react ambiguously to changes in entry threats, availability of information, and switching costs.

Our outcome variables include objective as well as subjective indicators of care quality. The objective quality measure is the rate of hospitalizations for Ambulatory Care Sensitive Conditions (ACSC), for which we have access to a monthly time-series of hospital records covering the whole country during the period 1999-2013. The ACSC rate is closely linked to primary care quality, as these are conditions for which appropriate primary care should prevent hospitalizations (Rosano et al., 2013).

The subjective quality measures originate from three waves of a large patient satisfaction survey, from which we have pre- and post-reform data for a subset of county councils. The survey data allows 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. These measures are closely linked to the stated objectives behind the reform: to increase accessibility and responsiveness to patients (Anell, 2015).

The only consistently significant effect we find is that a larger share of patients had a very good or excellent impression of their care provider in more competitive areas. The difference is about 2.5 percentage points, which amounts to 4 percent of the pre-reform mean. There is no impact of competition on the ACSC rate or patients' satisfaction with waiting times. Our conclusions are robust to a set of sensitivity tests, such as varying the treatment definition and using an instrumental variables (IV) approach to study the direct effect of new entry. Further, we find no indications that the improvement of patients' overall impression is driven by cream-skimming.

These results are in line with theories emphasizing observability and heterogeneity of patients' responsiveness to quality. Whereas the ACSC rate captures a dimension of quality that may be hard to observe for patients, the subjective experience is observed by definition. From the providers' point of view, the overall impression is of concern to a much larger set of patients than the ACSC patient group. Further, adopting a more service-minded attitude ought to be easier than improving the treatment of ACSC patients, and less costly than shortening waiting times.

Our results also fit well with the previous literature. Among studies of primary care in a regulated-price setting, cross-sectional analyses generally show positive associations between competition (proxied by number of competitors or density of general practitioners, GPs) and care quality (Pike, 2010; Rosano et al., 2013; Berlin et al., 2014; Stroka-Wetsch et al., 2016).[2] However, Gravelle et al. (2016a) show that such associations are unlikely causal, as the positive associations

[2] As an exception, Jürges and Pohl (2012) find no associations with process measures of care quality for elderly in German data.

in their data disappear once they control for GP fixed effects. Among longitudinal studies, the one most related to ours is Hanspers (2013), which uses Swedish data aggregated at a higher level (county council and year). She finds that the share of private providers is associated with more GP visits, but unrelated to waiting times, subjective quality and ACSC rates. Hanspers (2013) further attempts to exploit the Swedish choice reforms by studying the early adopters (with similar results). However, as the reforms came to affect the whole country within a few years time, the variation available to identify a treatment effect by such a strategy is at best limited.[3]

The study closest to ours in terms of causal ambitions is Gravelle et al. (2016a). They exploit variation from two natural experiments in the NHS and rely on similar identifying assumptions to provide causal evidence. Their results are also similar to ours: few strong indications that competition has positive effects on objective quality measures (e.g. the ACSC rate), and small increases in patient satisfaction.

Our findings are also aligned with studies showing correlations between increased competition and more leniency towards patients and (suspected) supplier-induced demand. This strand of literature has shown that GPs facing higher competition are more lenient when issuing sick leave certificates (ISF, 2014; Markussen and Røed, 2016), provide more (intense) treatment (Iversen and Lurås, 2000; Iversen and Ma, 2011), and prescribe more drugs (Kann et al., 2010; Schaumans, 2015). There is also evidence of directly detrimental opportunistic responses to increased competition. Exploiting the Swedish choice reforms by comparing more and less urban areas in a DID model, Fogelberg (2014) finds a short-term increase in antibiotics prescriptions in urban areas.

Beyond the context of primary care with regulated prices, fixed effects studies of primary care settings where market prices prevail find similar results to ours, i.e. weak or zero associations between competition and primary care quality (Johar et al., 2014; Gravelle et al., 2016b). There is a larger literature on competition in the hospital sector. Among studies in regulated-price settings,[4] several studies of the English NHS and US Medicare indicate improvements for cardiac patients (e.g. Kessler and McClellan, 2000; Kessler and Geppert, 2005; Cooper et al., 2011, 2012; Gaynor et al., 2013; Bloom et al., 2015; Colla et al., 2016; Gaynor et al., 2016). However, there are also examples of null findings and negative effects, in cardiac care as well as in other areas (Mukamel et al., 2001; Gowrisankaran and Town, 2003; Gravelle et al., 2014; Colla et al., 2016).

The paper proceeds as follows: Section 2 gives a background to the Swedish primary health care system. Section 3 describes the reforms and their relationship to increased competition. Section 4 presents our identification strategy, which is backed up by data in Section 5. Section 6 and 7 show our estimates for objective and subjective quality measures. Section 8 provides a discussion and concluding remarks.

## 2 Primary care in Sweden

Swedish government is divided into three layers: the national government, 21 county councils, and 290 municipalities. Health care services are mainly the responsibility of the county councils.[5] All

[3] Among the eight county councils that implemented the reform before 2010, only three were treated for more than one year (and the majority were treated less than 8 months) before the whole control group had implemented their reforms. Data aggregated at the year and county council-level thus provides little variation, and interpretations are further complicated by the potential sluggishness of quality changes and anticipation effects of the reforms.

[4] For evidence of the effects of competition in hospital markets with market prices, see e.g. Dranove and White (1994), Gaynor and Haas-Wilson (1999), Gaynor and Town (2011), Propper (2012), and Gaynor et al. (2015) for reviews.

[5] The municipalities share responsibility for certain population groups, i.e. they provide some services for school-age children, but more importantly, municipalities are responsible for long-term elderly care.

residents of a given municipality belong to the same county council. Thus, when a county council changes its health care policy, all residents of municipalities within that county council are affected by the policy change.

Health care accounts for 90 percent of the county councils' expenditures. The main revenue sources are a proportional income tax (71 percent), central government grants (20 percent), and patient fees (4 percent).[6] Patients pay a regulated fee for visits at care facilities and part of the cost for prescribed drugs, up to an annual cap. The payment system to primary care providers varies between counties, but capitation is the main reimbursement type (about 70-80 percent of revenues; 40 percent in Stockholm county during our study period[7]). Fee-for-service make up most of the residual. Over the past decade, it has become more common to risk-adjust the capitation rate, to make withdrawals from the base reimbursement when listed patients visit other providers, and to use pay-for-performance as a complementary reimbursement ($<5$ percent of payment).[8]

Primary care is provided in group practices denoted *care centers*. The main staff categories are GPs and nurses, but may also include other professions such as physiotherapists, occupational therapists, social workers, and cognitive therapists (Anell, 2015). Care centers typically employ four to six GPs. The median number of registered patients is 7,400 (similar to British GP practices, Santos et al., 2015), though private care centers are usually smaller than public care centers.

A distinguishing feature of Swedish primary care is that the county councils own and operate most care centers, but private provision has become increasingly common: in 2005, private providers accounted for 15 percent of primary care expenditures, to be compared with 27 in 2013. The private share varies considerably between counties; the min-max range in 2013 was 14-50 percent. The vast majority of private care centers are for-profit, limited liability companies.

In an international perspective, primary care plays a relatively small role in Swedish health care. The proportion of GPs (in relation to all physicians) is lower than in most comparable high-income countries. In comparison with other OECD countries, Swedes make fewer primary care visits. As opposed to for example Norwegian or British GPs, Swedish GPs have no formal gatekeeper function (Anell, 2015).

# 3 Patient choice, competition, and quality of care

## 3.1 Patient choice and entry reforms

Between 2007 and 2010, all county councils implemented reforms that removed all restrictions on patients' choice of care center and instituted free entry. Table 1 shows the date of implementation by county. The reforms implemented before 2010 were initiated by the counties themselves; in 2010, the Act of Free Choice (SFS, 2008:962) came into force, obliging all counties to enact reforms. The main objectives of the reform(s) were to increase access to primary care, improve its responsiveness to patients, and strengthen the role of primary care relative to secondary care (Anell, 2015).

All counties' reforms had three important components in common. First, patients may choose to enroll with any care centers in the county, and providers are not allowed to refuse patients that wish to enroll.[9] Patients may switch care center as often as they like. Second, from April

[6] The figures are for 2013, see `http://skl.se/ekonomijuridikstatistik/ekonomi/sektornisiffror.1821.html`.

[7] Notably, as the reimbursement system may affect the response to the market structure, our results are robust to excluding the municipalities in Stockholm county. Results are available upon request.

[8] Anell et al. (2016a) finds that the introduction of risk-adjustment affects private entrants' location decision within counties.

[9] However, should a center not wish to expand, they can probably try to deter patients by e.g. stating very long waiting times.

Table 1: Timing of patient choice reforms

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

Source: Swedish Competition Authority (2012).

2010 onwards, the councils have a website in common, `1177.se`. At the website, patients can find information about all care centers (for example contact details and patient satisfaction ratings), and also switch care center online. Third, there is free entry for new care centers that fulfil certain pre-specified requirements. The requirements are determined by each county council, and differ for example regarding the set of services that has to be supplied at care centers. Also, around the time of reform, the regulations of how patients would be assigned to newly opened care centers differed between counties.[10]

### 3.2 Reform impacts on competition

The main components of the reforms were the establishment of free entry, increased access to information, and reduced switching costs. The establishment of free entry for private primary care providers was perhaps the most revolutionizing part of the reforms. The number of care centers has increased by about 20 percent after the reforms. The transparent rules for entry plausibly also increased entry threats in many areas. Actual as well as potential entry increases competitive pressure on providers that want to retain, or attract new, patients. Also, recognizing that almost all new entrants were private (the number of public care centers has actually decreased), their stronger profit motives suggest that competition became more fierce.

Before the reforms, there was very little information publicly available about primary care center characteristics. Many counties formally allowed patients to switch provider, but they did not promote this actively, and care centers were allowed to refuse patients to enroll. After the reform, the amount of publicly available information about the set of providers and their characteristics has expanded substantially. Counties informed their residents about the right to choose provider at the time of the reform, and the reforms were widely covered in media. The website 1177.se, the most comprehensive source of information about care centers, describes features such as opening and telephone hours, professional categories and expertise available at the care center, and quality ratings from a national patient survey. Apart from expanding the set of available information,

[10] Anell et al. (2016a) examine how requirements and regulations affect entry by private providers. They find few stable associations, except that entry is less likely in counties that require care centers to provide a relatively broad set of services.

the time cost of searching for alternative providers has also decreased due to the accessibility of information online.

Switching costs have been reduced in several ways. Most importantly, there are no longer any restrictions on patients' choice sets and providers may not refuse patients. The switching process has also become more smooth; for example, choice forms are available at all care centers and at 1177.se, where it is furthermore possible to switch electronically.

The small literature on patients' choice of primary care provider suggest that quality is a determinant (Kolstad and Chernew, 2009; Dixon et al., 2010; Iversen and Lurås, 2011; Santos et al., 2015). As the risk of losing patients increases when patients become more mobile and informed about different providers, the increased access to information and reduced switching costs due to the reforms may have served to increase the degree of competition in Swedish primary care (see e.g. Klemperer, 1995, for a discussion of switching costs in a range of markets).[11]

# 4 Identification strategy

Identifying the effects of competition on primary care quality is challenging, as unobserved determinants of quality may be related to the level of, and changes in, competition. Furthermore, past quality levels may indicate where entry is likely to be successful in terms of attracting patients. Thus, both omitted variables and reverse causality are major concerns.

To circumvent these problems, we exploit the plausibly exogenous increase in competition induced by the reforms. As the reforms affected the whole country within a short time period, we construct a "treatment" group consisting of municipalities that plausibly were more strongly affected by the competition-increasing features of the reform. To define this treatment group, we consider the market structure six months before the reform, and calculate the (average) number of patients per care center if another care center would enter the municipality. If this number is greater than 5,500, we classify the municipality as treated; that is, if

$$\frac{Population}{NoOfCenters + 1} > 5,500. \tag{1}$$

The idea is that areas with many patients per care center were more attractive to potential entrants, as it should be easier to attract a sufficient patient stock to make a normal profit in such areas. 5,500 patients is slightly below the median patient stock of private care centers,[12] and roughly corresponds to the minimum patient stock required to break even asserted by an initiated expert in the county of Skåne (personal communication with Magnus Kåregård). In Appendix A.2, we show that our results are robust to using higher and lower cut-offs.[13]

To build confidence in our estimation strategy, we show in Section 5 that post-reform entry was much more frequent in our treatment group. We cannot directly test if these municipalities were also at least as affected by increased access to information and reduced switching costs due to the reforms, although these features ought to be more important if new providers enter, or if there are more care centers to choose from. The average number of care centers is greater in our treatment

[11] Public reporting of performance may also improve quality through peer comparisons by intrinsically motivated health care personnel (Kolstad, 2013; Godager et al., 2016). Such effects seem unlikely in Swedish primary care, as information is reported at the care center level, making direct peer comparisons difficult.

[12] According to enrollment data available for 2011 and 2013; the medians were then 5,731 and 6,135 respectively.

[13] The exact timing of when we measure the cut-off is not important for our definition; e.g., the treatment group is identical if we instead look at 24 months before a reform.

group, as it includes more populous municipalities.[14] Also, survey data reported in Appendix A.1 indicate that treatment municipality residents feel more informed about primary care providers than residents in the comparison group, although the difference is not significant.

Our DID approach, in which reform-driven increases in competition are identified using a treatment group defined by the pre-reform market structure, has important advantages over (fixed-effects) approaches that use direct measures to capture changes in the degree of competition. For instance, direct measures based on the number of providers in an area or the Herfindahl-Hirschman Index (HHI) react ambiguously to, or fail to register, impacts on competition from changes in entry threats,[15] information levels, or switching costs.[16] Direct measures are also plagued by reverse causality; for example, providers that invest in quality may push others out of the market. A specific drawback of HHI measures that use patient flows to define markets is that they ignore quality as a determinant of substitutability between providers, or assume that the relevant market is independent of provider quality. If patients are willing to travel longer to see high-quality providers, such providers would appear to have more competitors and less market power, despite that the opposite is true(Tay, 2003; Kessler and Geppert, 2005).[17] Though we also assume that the market is independent of provider quality, the consequences are less severe with a market definition that is fixed over time, as exogenous quality changes are not confounded with changes in the degree of competition.

Previous Swedish studies on the reforms have used either the early adopters (Hanspers, 2013) or urban municipalities to define areas affected by increased competition (Fogelberg, 2014). The former strategy is severely limited by the short post-reform period available for the early adopters, and also risks confounding the reform impact with other county-level policy changes. The second strategy disregards that competition may be fierce even if there are only a few care centers. For example, there was post-reform entry in many municipalities that were classified as untreated with the definition used in Fogelberg (2014).

## 5 Market structure

To establish that our treatment definition in Equation (1) is sensible, we examine the post-reform entry patterns in the treatment and comparison group. Due to the decentralized organization of Swedish health care, there is a dearth of publicly available primary care data (Anell, 2015). By combining two previous registers, and by contacting a large number of authorities and firms as well as individual care centers, we have built a unique register over all care centers in Sweden during 2005-2013. We have information on starting and closing dates, coordinates, and ownership details.

[14] As equity in access to primary care is an important concern for Swedish health care policy, there is at least one care center in every municipality. This implies that populous municipalities often have relatively many patients per care center (pre- as well as post-reform).

[15] Industrial organization models of competition indicate that potential entry contributes to the level of competition faced by incumbents (e.g. Salop, 1979; Dixit, 1979), but more intense competition from potential entrants would not be registered by measures based on the number of providers. If we in addition assume that one provider disproportionally increases its share of patients, then market concentration would increase, and a HHI would indicate a *decrease* in competition.

[16] To exemplify, imagine a market with two competing providers, whose patients are uninformed about some quality dimension that is known by the providers. When patients receive information about the quality dimension, the providers' incentives to improve this dimension of quality increases, as patients can react to the information and switch provider if the other one is superior. Such an increase of competition would not be registered by competition measures based on the number of providers, and the reaction of a HHI could again be ambiguous. However, it would be picked up by our DID approach, assuming that the treatment group definition is valid. Similar examples can be constructed with reduced search and switching costs.

[17] Measures based on predicted patient flows, pioneered by Kessler and McClellan (2000), avoid this problem.

Table 2: Pre-reform number of care centers per capita

| | Treatment | | | | Comparison | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | Mean | SD | Min | Max | Mean | SD | Min | Max |
| *Centers per capita* | 0.93 | 0.27 | 0.38 | 1.6 | 1.7 | 0.71 | 0.91 | 4.3 |
| *Municipalities* | 147 | | | | 142 | | | |

*Note*: Pre-reform distribution of care centers per capita. Time averages calculated over the period 18 to 7 months pre-reform for each municipality.

Table 2 shows, by treatment status, the pre-reform mean number of care centers per 10,000 residents (*Centers per capita*). As should be expected, given our definition of treatment, the mean is lower in the treatment group. In December 2005, one year before the first reform, there were 958 care centers in total in Sweden.[18] Seven years later, after all county councils had implemented reforms, the number had increased by 20 percent to 1,159.[19] Entry was not evenly spread though, as we will now show.

The leftmost part of Figure 1 plots, by treatment status and month (in event time), the share of municipalities that had more care centers compared to 12 months before reform (upper part) and the average number of care centers (lower part). Entry took place rather quickly after reform in both groups, but was much more common in the treatment group. The average number of care centers increased by 1 in the treatment group, but by considerably less in the comparison group. The pre-reform differences between treatment and comparison groups are never statistically significant, while all post-reform differences are strongly significant.

As patients are free to visit any care center in their county council, one may be concerned about using the municipalities to define the relevant market. It is therefore notable that our treatment group definition identifies high-entry markets also for other market definitions. The graphs in the center and rightmost parts of Figure 1 illustrate entries within markets defined as circles (with radii of 5/15 km) around each care center that existed 6 months pre-reform. Also with markets defined in this way, we see that markets located in treatment group municipalities were more exposed to entry than markets located in comparison group municipalities.[20]

Table 3 repeats the main message from Figure 1 in table form. Column (1) shows the results from a linear probability model in which the outcome variable indicates whether, during at least one month in the post-reform period, the number of care centers in a municipality was larger than 12 months pre-reform. The coefficient on the treatment dummy is large (0.389 compared to the comparison group mean of 0.162) and highly significant. Column (2) uses a dependent variable equal to one if there were more care centers 42 months post reform, an indicator of lasting entry. The coefficient is almost as large as in column (1), and highly significant.

As a final way of illustrating the differential reform impact on the treatment and comparison groups, column (3) shows DID estimates indicating that the average number of care centers increased by almost one center more in the treatment municipalities than in the comparison group.

[18] This number excludes a small number of private practices operating in a separate contracting system put in place by the central government during the 1990's. These practices are not participating in the patient choice system. Subsidiary units are also excluded, as their patients are enrolled at the parent care center.

[19] The GP density, i.e. number of specialists in general medicine per 10,000 residents, increased from 59 to 64 between 2006 and 2013 (unweighted county average). The density decreased in only a few counties.

[20] For the radius-based market definitions, the treatment group shows a slight increase in the probability of entry from around half a year before a reform. As the reforms were known in advance, it is not surprising that there was some entry before the exact reform date.

Table 3: Estimations on market structure

| | All municipalities | | | Municipalities with subj. measure data | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | *Any entry* | *Lasting entry* | *Centers* | *Any entry* | *Lasting entry* | *Centers* |
| | LPM | LPM | DID | LPM | LPM | DID |
| *Treatment* | 0.389*** | 0.363*** | 0.848*** | 0.353*** | 0.331*** | 0.529** |
| | (0.0557) | (0.0622) | (0.225) | (0.0938) | (0.0915) | (0.251) |
| *Constant* | 0.162*** | 0.127*** | | 0.147*** | 0.107*** | |
| | (0.0293) | (0.0256) | | (0.0386) | (0.0312) | |
| Municipality FE | - | - | Yes | - | - | Yes |
| Year FE | - | - | Yes | - | - | Yes |
| Quarter FE | - | - | Yes | - | - | Yes |
| Month to reform FE | - | - | Yes | - | - | Yes |
| Observations | 289 | 289 | 19,074 | 123 | 123 | 8,118 |
| Municipalities | 289 | 289 | 289 | 123 | 123 | 123 |
| County councils | 21 | 21 | 21 | 12 | 12 | 12 |

*Note:* Estimations of new entry in all county councils (columns 1-3), and in the 12 county councils for which we have information about subjective measures (columns 4-6). The unit of observation is the municipality. Columns 1-2 and 4-5 show estimates from cross sectional regressions with a treatment group dummy as the only explanatory variable. The dependent variables are dummies for having a larger number of municipalities, compared to 12 month before the reform, in *any period* after the reform (*Any entry*), and after 42 months (*Lasting entry*). Columns 3 and 6 show DID estimates of a specification where *Treatment* is the interaction between a treatment group dummy and county-specific dummies for post-reform months. The dependent variable is the number of care centers (*Centers*). Standard errors (in parentheses) are clustered by county council in columns 1-3, and by municipality in columns 4-6. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

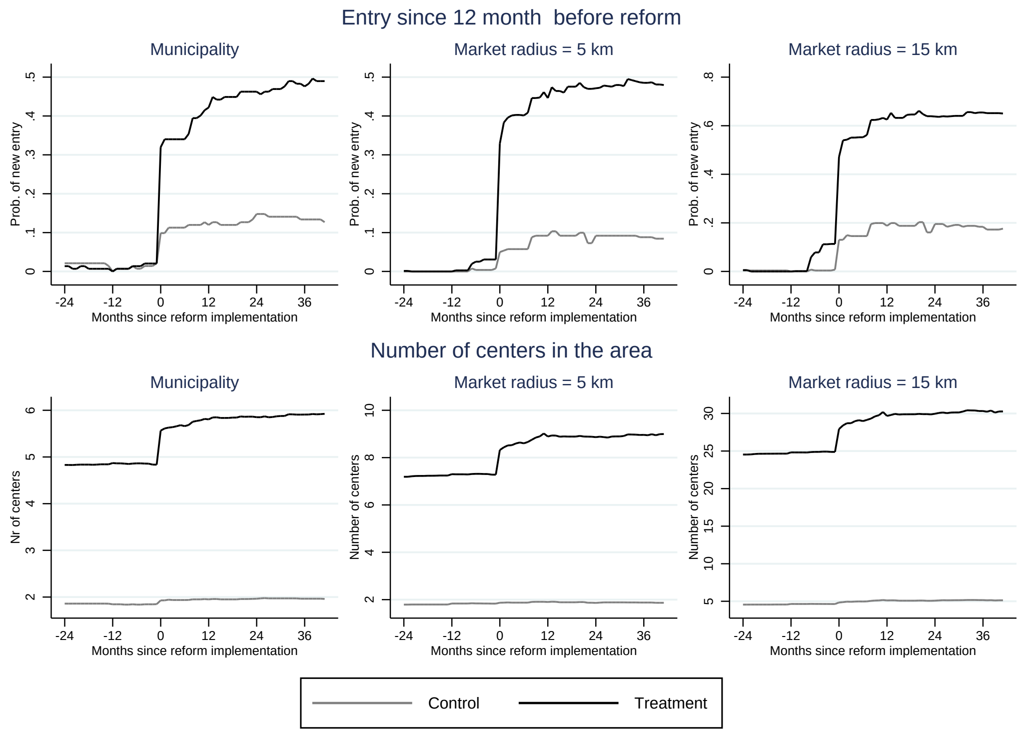


Figure 1: The lines display the coefficients from regressing treatment and comparison group dummies on i) (upper panel) an indicator variable equal to 1 if there are more care centers in a certain month compared to 12 months before a reform, ii) (lower panel) the number of care centers in a market. The sample period is 24 months pre- to 42 months post-reform. The markets are either defined by municipal borders or by a radius of 5 km/15 km around each care center that existed 6 months before its county reformed.

The DID model is specified as follows:

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

where $center_{mt}$ is the number of care centers in municipality $m$ in month $t$, $T_{mt}$ is a treatment indicator, taking the value 1 from the month when a treatment group municipality is first affected by reform and onwards. $\mu_m$ are municipality fixed effects, $\theta_t, \delta_q$, and $\lambda_y$ are month-compared-to-reform, quarter-of-the-year, and year fixed effects,[21] and $\varepsilon_{mt}$ is an error term. All reforms are implemented in month zero, and the balanced sample includes observations from 24 months pre- to 42 months post-reform. The regression is weighted with the square root of the population size, and standard errors are clustered at the county council level.[22]

Although our treatment definition appears sensible for the whole set of municipalities, it does not guarantee that it is suitable when considering only the 12 counties for which we have information about subjective quality measures (see Section 7.1). Columns (4)-(6) repeat the estimations in columns (1)-(3) on a sample restricted to municipalities located in these 12 counties.[23] The results are similar to those for the full set of counties.

[21] We use the Stata command *xtivreg2* to partial out the time effects in our estimations.

[22] Due to the small number of clusters, we have checked robustness using the wild cluster bootstrap (Cameron et al., 2008; Cameron and Miller, 2015). We use version 2.0.0 of *cgmwildboot* for Stata, developed by Judson Caskey. The results in Table 3 remain significant.

[23] Here, due to the small number of counties, we cluster standard errors at the municipality level.

The higher prevalence of large urban municipalities in the treatment group partly explains why this group experienced more entry, but smaller and more rural municipalities in the treatment group contribute to the difference too. 43 of the 147 treated municipalities had only one care center 6 months pre-reform; hardly a sign of a large population. 19 of these 43 experienced entry after the reform. In the comparison group, only 6 of the 77 pre-reform monopolies experienced entry. This corresponds to relative frequencies of 44 percent (treatment) and 8 percent (comparison), respectively. It is also notable that the smaller set of counties for which we have data on subjective quality measures does not include any of the largest municipalities; the largest municipality in this subset has a population of 134,000, to be compared with the full sample maximum of 795,000.

# 6 Competition effects on objective quality: ACSC hospitalizations

## 6.1 Data

Avoidable hospitalizations are identified by hospitalizations with certain diagnoses (*Ambulatory Care Sensitive Conditions*), for which well-functioning primary care would prevent inpatient episodes. ACSC hospitalizations is a commonly used concept in studies of primary care quality (e.g. Starfield et al., 2005; Kringos et al., 2013). We use the Swedish definition of ACSC;[24] other countries have similar definitions (AHRQ, 2001; Purdy et al., 2009; NHS Group, Department of Health, 2014).

We use data from the Swedish national inpatient register, which covers all inpatient episodes, for 1999-2013, as the classification of diseases changed in 1998 when ICD-10 was implemented. *ACSC rate* is the municipality-and-monthly number of avoidable hospitalizations per 10,000 residents. The data is aggregated at the municipality level, based on patients' municipality of residence, as we do not have access to less aggregated data. Using data aggregated to the market-level mitigates problems due to sorting of patients between care centers. The municipality level represents a conservative choice, as the relevant primary care market is smaller for most patients. [25]

Figure 2 shows the development of the *ACSC rate* for the treatment (black line) and comparison group (gray line) in event time. The balanced sample runs from 90 months pre- to 42 months post reform. The lines reveal that the level is higher and more volatile in the comparison group, but the trends are similar and there is no sharp break around the reform date.

Our estimations also include a set of municipality-and-year level covariates: population size (*Population size*), population density (*Pop density*, i.e. residents per km$^2$), mean income level in thousands of SEK (*Income*), percentage share of 16-74 year-olds with at most primary education (*Primary*), percentage share of children <10 years (*Children*), and the percentage share of residents 65 years of age or older (*Elderly*). We also include the squared values of these covariates in the estimations. The covariates capture potential differences in case mix development, and may increase precision. It is unlikely that these covariates were directly affected by the reforms.

Table 4 shows pre-reform descriptive statistics for a period running from 18 to 7 months before reform. The average population size and density is higher in the treatment group. The considerably lower medians of these variables (31,349 and 60.3) indicate that the averages are pulled up by a few very large municipalities; that is, many treatment municipalities are rather small and dispersed in

[24]The following chronic diagnoses are included in the measure: anaemia, asthma, diabetes, heart failure, high blood pressure, COPD, and ischemic heart disease. The measure also includes the following acute diagnoses: bleeding ulcers, diarrhoea, epileptic cramps, pelvis tract infections, pyelitis, and ear and respiratory tract infections (National Board of Health and Welfare, 2014).

[25]In one of the most densely populated regions, Skåne, 90 percent of patients are enrolled at a care center in their municipality of residence (Anell et al., 2016b). This suggests that the risk of bias due to patient sorting across municipality borders is low also for less dense regions.

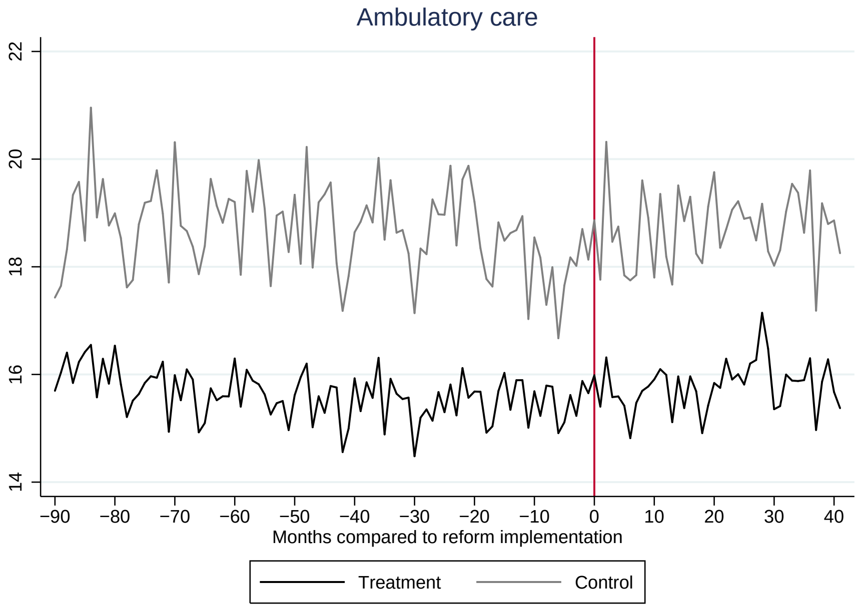


Figure 2: The lines display the monthly average of *ACSC rate* in the treatment (black) and comparison (gray) municipalities. The vertical red line marks the reform month.

terms of population. But some size difference between treatment and comparison is inevitable, given our treatment definition in combination with the "at least one care center per municipality"-policy seemingly used by the counties.

## 6.2 Estimation

Our baseline DID model for the *ACSC rate* can be be expressed as follows:

$$ACSC\ rate_{mt} = \alpha + \beta_{\bar{t}} T_{mt} + \gamma X_{mt} + \mu_m + \kappa_m t + \theta_t + \delta_q + \lambda_y + \varepsilon_{mt} \quad (3)$$

where $T_{mt}$ is a treatment indicator, $X_{mt}$ is a vector including the municipality-and-year covariates (in levels and as squares), $\mu_m$ are municipality fixed effects, $\kappa_m$ are municipality-specific coefficients on the linear pre-reform trend variable $t$, $\varepsilon_{mt}$ is an error term, $\theta_t, \delta_q$, and $\lambda_y$ are month-compared-to-reform, quarter-of-the-year, and year fixed effects, respectively.[26]

The estimation of Eq. 3 follows event time rather than calendar time, and all reforms are implemented at *t=0*. The balanced sample runs from 90 months pre- to 42 months post reform. Because the municipalities differ widely in terms of population size, the regressions are weighted by the square root of the population each year. Standard errors are clustered at the county council level (whenever computational possible), and at the municipality level otherwise. The results with county-level clusters are similar when using the wild cluster bootstrap (which may be more appropriate given the small number of clusters (Cameron et al., 2008; Cameron and Miller, 2015); results not shown).

We estimate several variants of Eq. 3. Initially, we estimate a flexible but low-powered specification, in which we allow the reforms to have a differential effect over time and also check for

[26] Note that this combination of time effects subsumes the reform indicator, which is why it is not included in the equation. We use the Stata command *xtivreg2* to partial out the time effects in our estimations.

Table 4: Descriptive statistics pre-reform

| | Treatment | | | | Comparison | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Variable | Mean | SD | Min | Max | Mean | SD | Min | Max |
| *ACSC rate* | 15.5 | 4.1 | 4.9 | 36.7 | 18.2 | 6.9 | 1.9 | 54.5 |
| *Population size* | 51,454 | 81,245 | 11,126 | 795,163 | 11,154 | 7,253 | 2,500 | 62,388 |
| *Pop density* | 226.8 | 590.7 | 1.2 | 4,228.2 | 31.6 | 105.1 | 0.2 | 1161.4 |
| *Income* | 239.2 | 27.4 | 203.0 | 370.7 | 216.7 | 24.0 | 184.1 | 442.6 |
| *Primary* | 23.9 | 4.1 | 12.6 | 36.7 | 28.1 | 3.7 | 11.2 | 37.4 |
| *Children* | 11.2 | 1.6 | 8.6 | 17.3 | 9.8 | 1.4 | 7.1 | 15.8 |
| *Elderly* | 18.4 | 3.3 | 10.3 | 25.0 | 22.3 | 3.2 | 11.8 | 30.4 |
| Municipalities | 147 | | | | 142 | | | |

*Note*: Time averages, calculated over the period 18 to 7 months pre-reform for each municipality. *ACSC rate* is based on monthly municipality-level data. All other variables are based on yearly municipality-level data.

pre-reform (linear and non-linear) trend differences between treatment and comparison group (Laporte and Windmeijer, 2005). In this specification, the treatment coefficient is a time-varying vector $\beta_{\bar{t}}$, with each treatment effect representing a yearly average. To check for pre-reform trend differences, the treatment indicator $T_{mt}$ switches on after the first 12-month period. We also include the covariates in Table 4 along with their squares. For ease of exposition, we present the estimates from this model in a figure.

Thereafter, we restrict the treatment effect to be constant – i.e. $\beta$ becomes a time-invariant scalar. As the initial estimation reveals very small pre-trend differences, we let the treatment indicator take the value 1 from the reform month and onwards. To a baseline model without covariates, we sequentially add municipality-specific linear trends $\kappa_m t$, and then covariates $X_{mt}$. Finally, we allow for a sluggish response to the reforms by estimating a "'donut" specification, in which treatment starts six months after reform implementation (rather than at t=0), and the pre-period ends six month before reform. This specification, which in practice adds a dummy variable for the 12 months period around $t = 0$, recognizes that the effect of competition may be underestimated if there is a lag between the increase in competition and the primary care outcomes.

### 6.3 Results

Figure 3 shows the differences between the treatment and comparison groups according to our most flexible specification of Eq. (3), i.e. with time-varying treatment effects and allowing for pre-trend differences.[27] The red lines mark the implementation year.

The figure does not indicate systematic pre-trend differences between the treatment and comparison group: there are positive as well as negative "placebo" estimates, and they are all small and statistically insignificant. The largest pre-trend difference is -0.22 ($p = 0.51$), which is less than 0.04 of the (joint treatment and comparison) pre-reform standard deviation of 6.1. All post-reform treatment effects are negative, and increasingly so over time, indicating quality improvements. However, the estimates are all statistically insignificant and small; to illustrate, the largest (i.e. the last) estimate corresponds to 0.06 of a standard deviation ($p = 0.48$).

[27]The confidence interval (dashed lines) use standard errors clustered by municipality instead of county council. The reason is that 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 coefficient are far from significant anyway.

Table 5: DID models of avoidable hospitalizations (*ACSC rate*)

| | Panel A: Population-weighted | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | Baseline | Linear trends | Covariates | Donut |
| *Treatment* | -0.358 | -0.338 | -0.169 | -0.134 |
| | (0.330) | (0.267) | (0.286) | (0.238) |

| | Panel B: Unweighted | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | Baseline | Linear trends | Covariates | Donut |
| *Treatment* | 0.149 | 0.0282 | 0.0470 | 0.0193 |
| | (0.365) | (0.236) | (0.193) | (0.180) |
| 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 |
| County councils | 21 | 21 | 21 | 21 |

*Note*: Dependent variable: *ACSC rate*. *Treatment* = 1 if there were more than 5,500 patients per care center + 1 six months pre-reform. All specifications include municipality, year, quarter, and month-to-reform fixed effects. Columns 2-4 include municipality-specific linear trends. Columns 3-4 include municipality-and-year level covariates. Column 4 include a dummy for the six months periods pre- and post reform (a "donut hole"). The estimation sample includes all 289 municipalities and covers the period starting 90 months before reform implementation to 42 months after reform. Standard errors clustered by county council in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

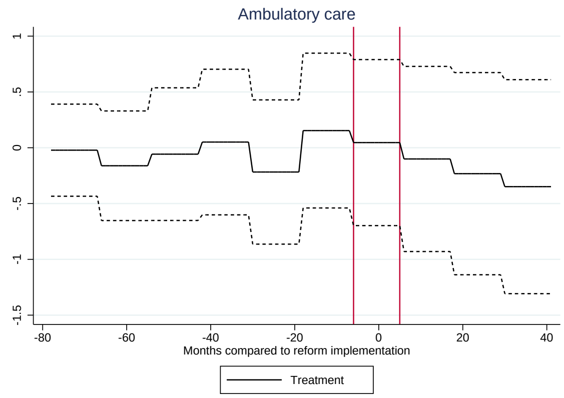


Figure 3: Differences in avoidable hospitalization rates (*ACSC rate*) of treatment and comparison groups. The lines display the coefficients (solid) and confidence intervals (dashed) from Eq. (3) including covariates. The first 12 months are used as reference period, thus excluded from the figure. Standard errors used to calculate the confidence interval are clustered by municipality. Pre-reform mean (standard deviation) of *ACSC rate* is 17.1 (6.1).

Panel A of Table 5 presents estimates of the specifications with a constant treatment effect. The estimates without covariates in columns 1-2 are negative but statistically insignificant, amounting to 0.06 of a standard deviation. The estimate is further attenuated when including covariates (column 3, 0.03 standard deviations) and using a donut specification (column 4, 0.02 standard deviations). In Appendix A.2.1, we show that the results are similar for other cut-off levels of the treatment definition.

Panel B shows corresponding estimates from regressions without population weights. All treatment coefficients are still very small but positive, indicating quality reductions. The qualitative difference between the results with and without population weights indicates that larger municipalities – which are more influential in the weighted estimations – experienced larger quality improvements. But it is not the case that the average effect hides two very large effects of opposing sign: also when excluding the largest and smallest municipalities from the estimation sample, we obtain estimates that are very close to zero and statistically insignificant. These results are available in Appendix A.4. In Appendix A.5 we further shows that the average effect does not hide a stronger quality response in former monopolies than in markets with several care centers.

Our identification strategy acknowledges that the reforms may have increased competition through several channels, including higher potential for entry, more available information and lower switching costs. However, if actual entry is really what matters for quality, our approach underestimates the effect, as we assign some municipalities with no entry to the treatment group (and vice versa). In Appendix A.6, we contrast municipalities with entry to municipalities without in a similar DID analysis as above. We also use our main treatment definition as an instrumental variable for entry. Both strategies yield estimates of similar order of magnitude as in Table 5.

Possibly, the increase in information and lower switching costs were more important aspects of the reforms. To tentatively explore this possibility, Appendix A.7 considers the subset of municipalities that experienced no entry throughout the period. We contrast two groups: those that were monopolies throughout the period, and those with at least two care centers. We find no post-reform differences between these two groups either.

In sum, the null effect on avoidable hospitalizations is a robust result in all our specifications.

We therefore conclude that there us no discernible impact on this outcome measure.[28]

# 7 Subjective measures of care center quality

## 7.1 Data

The source of our subjective quality data is three waves of a biannual national patient satisfaction survey carried out by the Swedish Association of Local Authorities and Regions. All respondents had recently made a visit to the care center they were asked to rate. The survey covers all care centers, except in 2009 when the counties of Stockholm and Norrbotten did not participate. Notably, as the first wave was conducted in the fall of 2009, there is no pre-reform data for counties that had already implemented their entry and choice reforms by that time (see Table 3.1). Thus, the analysis of subjective quality is restricted to the 12 county councils (123 municipalities) that participated in the 2009 survey, and for which this year belongs to the pre-reform period. With this restriction. we have a yearly sample of 30,000-40,000 respondents. Notably, the largest municipality in the sample with subjective data had approximately 134,000 residents; that is, the most urban areas are not in the sample.

We continue to define our treatment group at the municipality level; that is, a respondent belongs to the treatment group if there was at least 5,500 residents per care center in the respondent's municipality of residence six months pre-reform. This yields 48 (75) municipalities in the treatment (comparison) group. The average municipality-level response rate is 56 percent (stable between 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* (upper part of Table 6).[29] For all dummy variables, the value 1 indicates better quality. Figure 4 displays, by treatment status and survey wave, the average share of the four outcome variables. The difference in levels in the pre-reform survey in 2009 is very small for all variables, and the development over time also reveals relatively small changes. Of note, the average shares are always quite far from the theoretical max of 1, suggesting that ceiling effects are not a concern.

We also have individual level data on respondents' self-rated health and previous experiences with the care center (see lower part of Table 6 for definitions).[30] Table 7 displays descriptive statistics for the pre-reform survey wave.

## 7.2 Estimation

We estimate the following DID equation for the four subjective quality measures:

$$y_{imt} = \alpha + \beta T_{mt} + \gamma X_{imt} + \mu_m + \lambda_t + \varepsilon_{imt} \tag{4}$$

where $y_{mt}$ is one of the four dependent variables, $T_{mt}$ is a treatment indicator, $X_{imt}$ is a vector of individual background characteristics, $\mu_m$ are municipality fixed effects, $\lambda_t$ are year (survey wave)

[28]In Appendix A.3, we repeat this analysis for another outcome variable, namely the rate of unplanned inpatient care episodes. The results are in line with those for the ACSC rate.

[29]The original Swedish wording of the questions are "Hur upplever du mottagningens tillgänglighet per telefon?"; "Hur länge fick du vänta på ditt besök?"; "Hur värderar du som helhet den vård/behandling du fick?"; and "Skulle du rekommendera den här mottagningen till andra?".

[30]Due to secrecy agreements, we were not allowed to get access to other background variables, such as age and gender.

Table 6: Definitions of subjective quality measures

**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, wholly |

**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 care center, 0 otherwise |
| *Visit1* | = 1 if 1 previous visit at this care center, 0 otherwise |
| *Visit23* | = 1 if 2-3 previous visits at this care center, 0 otherwise |
| *Visit4* | = 1 if 4 previous visits at this 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 |

*Note: Variables from 2009-13 waves of national patient satisfactions survey.*

Table 7: Pre-reform descriptive statistics for patient satisfaction survey

| | Treatment | | | Comparison | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Variables | Mean | SD | Obs | Mean | SD | Obs |
| *Phone access* | 0.497 | 0.500 | 18,461 | 0.500 | 0.500 | 12,696 |
| *Waiting times* | 0.563 | 0.496 | 17,945 | 0.564 | 0.496 | 12,256 |
| *Overall impression* | 0.634 | 0.482 | 22,983 | 0.651 | 0.477 | 16,434 |
| *Recommendation* | 0.712 | 0.453 | 22,847 | 0.715 | 0.451 | 16,323 |
| *Stable contact* | 0.586 | 0.493 | 23,067 | 0.564 | 0.496 | 16,474 |
| *Visit0* | 0.167 | 0.373 | 22,966 | 0.160 | 0.367 | 16,422 |
| *Visit1* | 0.197 | 0.398 | 22,966 | 0.195 | 0.396 | 16,422 |
| *Visit23* | 0.357 | 0.479 | 22,966 | 0.361 | 0.480 | 16,422 |
| *Visit4* | 0.278 | 0.448 | 22,966 | 0.284 | 0.451 | 16,422 |
| *Poor health* | 0.066 | 0.248 | 22,983 | 0.068 | 0.253 | 16,439 |
| *Tolerable health* | 0.292 | 0.454 | 22,983 | 0.309 | 0.462 | 16,439 |
| *Good health* | 0.327 | 0.469 | 22,983 | 0.331 | 0.471 | 16,439 |
| *Very good health* | 0.224 | 0.417 | 22,983 | 0.207 | 0.405 | 16,439 |
| *Excellent health* | 0.091 | 0.288 | 22,983 | 0.084 | 0.278 | 16,439 |
| *N.o. municipalities* | 48 | | | 75 | | |

*Note*: Individual-level data from 2009 survey wave.

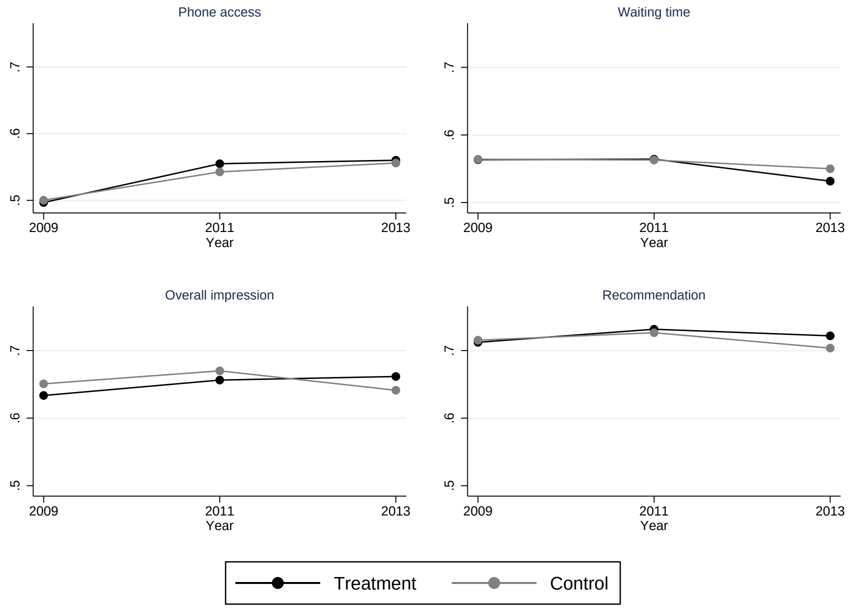


Figure 4: The graphs display, for each of the four dummy outcome variables, the share of respondents whose answers equal one. Treatment (black line) and comparison group (gray line). Note that only respondents from 12 counties are included.

fixed effects, and $\varepsilon_{imt}$ is an error term. As the model only includes binary variables, we estimate the equation by a linear probability model (LPM).

The probability of an individual being included in the surveys differ, as different number of individuals where sampled in different municipalities/care centers. As our treatment is assigned at the municipality level, we weight each observation by the inverse of the selection probability (the number of sent out surveys divided by the municipality's population size). Given the small number of county councils in these estimations, we cluster standard errors at the municipality level (using the county council levels yields smaller standard errors in all cases). The results are similar when using the wild cluster bootstrap at the county council-level (not shown).

### 7.3 Results

Table 8 presents estimates for the four subjective quality measures. Panel A shows the results of specifications excluding individual-level covariates, Panel B shows specifications including covariates,[31] and Panel C shows regressions without the survey weights (including covariates). For *Phone access*, *Overall impression* and *Recommendation*, the estimated treatment effects amount to 3-4 percent of the pre-reform mean; however, only *Overall impression* is consistently significant. The estimate on *Waiting time* is negative but statistically insignificant and small in relation to the mean. The estimates are hardly affected by regression weights or covariates, though the latter improve precision somewhat.[32] The results are robust to varying the cut-off of the treatment definition (see Appendix A.2.2).

Notably, Panel B shows that all subjective quality measures are strongly positively correlated

[31]The reference category is an individual who did not have a stable physician contact, had poor self-reported health, and had not visited the care center before.

[32]The wild cluster bootstrap does not change our inference (results not shown).

Table 8: Subjective measures of primary care quality

Panel A: No covariates

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | *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 |
| County councils | 12 | 12 | 12 | 12 |
| $R^2$ | 0.043 | 0.021 | 0.016 | 0.026 |

Panel B: Individual level covariates

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | *Phone access* | *Waiting times* | *Overall impression* | *Recommendation* |
| *Treatment* | 0.0196 | -0.00658 | 0.0252** | 0.0224* |
| | (0.0160) | (0.0118) | (0.0101) | (0.0115) |
| *Stable contact* | 0.133*** | -0.0869*** | 0.224*** | 0.228*** |
| | (0.00576) | (0.00526) | (0.00462) | (0.00484) |
| *Tolerable health* | 0.0606*** | 0.0530*** | 0.0973*** | 0.110*** |
| | (0.00661) | (0.00817) | (0.00697) | (0.00673) |
| *Good health* | 0.0947*** | 0.105*** | 0.177*** | 0.166*** |
| | (0.00797) | (0.00857) | (0.00770) | (0.00680) |
| *Very good health* | 0.179*** | 0.169*** | 0.299*** | 0.217*** |
| | (0.00865) | (0.00820) | (0.00880) | (0.00689) |
| *Excellent health* | 0.200*** | 0.246*** | 0.339*** | 0.250*** |
| | (0.0103) | (0.00892) | (0.00975) | (0.00838) |
| *Visits1* | -0.00546 | 0.0445*** | -0.00983* | -0.0204*** |
| | (0.00599) | (0.00488) | (0.00519) | (0.00515) |
| *Visits23* | 0.00437 | 0.0889*** | -0.0102** | -0.0365*** |
| | (0.00532) | (0.00521) | (0.00420) | (0.00502) |
| *Visits4* | 0.0127** | 0.138*** | 0.0147*** | -0.0435*** |
| | (0.00584) | (0.00565) | (0.00497) | (0.00504) |
| Observations | 89,300 | 87,024 | 115,553 | 115,059 |
| Municipalities | 123 | 123 | 123 | 123 |
| County councils | 12 | 12 | 12 | 12 |
| $R^2$ | 0.067 | 0.045 | 0.089 | 0.093 |

Panel C: Unweighted estimates with individual level covariates

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | *Phone access* | *Waiting times* | *Overall impression* | *Recommendation* |
| *Treatment* | 0.00936 | -0.00809 | 0.0198** | 0.0158 |
| | (0.0170) | (0.0121) | (0.00981) | (0.0104) |
| Observations | 89,300 | 87,024 | 115,553 | 115,059 |
| Municipalities | 123 | 123 | 123 | 123 |
| County councils | 12 | 12 | 12 | 12 |
| $R^2$ | 0.066 | 0.047 | 0.089 | 0.093 |

*Note*: Standard errors clustered by municipality in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Municipality and survey fixed effects are included in all estimations. The sample covers the three years 2009, 2011, and 2013. The (joint treatment and comparison) means of the dependent variables in 2009 are: *Phone access*: 0.500; *Waiting times*: 0.564; *Overall impression*: 0.641; *Recommendation*: 0.714. The differences in total observations reflect differential response rates to the underlying survey questions. Panel A excludes individual-level covariates, Panel B and C includes covariates, Panel C do not use sample weights.

with self-rated health. This raises some concerns relating to the interpretation of our results. If the patient mix has developed differently in our treatment and comparison groups (e.g. due to cream-skimming in the group more strongly affected by competition), the higher subjective ratings in the treatment group may reflect a composition effect rather than substantial quality improvements. However, we find no indications of differential patient mix changes when estimating Eq. (4) using respondents' self-reported health and previous primary care experiences as dependent variables (see Appendix A.8). That is, our results are not driven by treatment group care centers primarily catering to the demands of healthier patients (one particular instance of cream-skimming).[33]

The reported estimations contrast very satisfied respondents to respondents who are just satisfied or even dissatisfied with their care center. If we instead use the dissatisfied group as reference category (i.e., by transferring respondents rating phone access or overall impression as 'good' to the high-quality category, or transferring respondents 'partly' willing to recommend their care center to the high-quality category),[34] the estimates are almost half as large (results not shown). The effects are still marginally significant ($p < 0.1$) for *Overall impression* and *Recommendation*; notably, this definition removes a lot of variation, as few patients rate their care center as 'OK/poor' or do not want to recommend it at all.

Compared to the *ACSC rate* estimations, we are less concerned about potential heterogeneity over population size for the subjective measures, as the largest municipalities did not participate in the patient satisfaction survey. Indeed, we obtain similar estimates when excluding the largest and smallest municipalities from the sample (Appendix A.4). The precision is lower in these estimations, but this is likely linked to the loss of almost 20,000 observations.[35]

Appendix A.6 presents DID and IV estimates for subjective measures using exposure to entry as an alternative definition of treatment. The estimates are larger than the baseline results in Table 8. But in Appendix A.7, we find that among the municipalities *not* experiencing entry, patient satisfaction increased more in municipalities where there was some competition to begin with (i.e. at least two care centers). Although this difference is not necessarily causal, it suggests that increased entry was not the only important channel for the reform effects. For this reason, and because of the potential endogeneity of entry in the DID estimates, we are reluctant to view the IV and DID estimates using the alternative treatment definition as causal.

# 8 Discussion

Our findings are better aligned with the theoretical literature that emphasizes limited observability of care quality and conflicting incentives for semi-altruistic health care personnel (e.g. Arrow, 1963; Brekke et al., 2014) than with the standard health economic model of competition under regulated prices. Primary health care is multifaceted, and the impact of competition may differ for different dimensions of quality. Providers in more competitive markets face incentives to primarily target quality dimensions that are easily observable and are relevant to a large (or more profitable) share of the population.

In line with these incentives, we find no significant impacts of competition on the rate of avoidable hospitalizations. This measure captures a dimension of quality that is difficult for patients to observe, and it is likely an irrelevant choice parameter for a large share of the general population

[33] As the survey only targeted patients that had actually visited the care center, we cannot rule out cream-skimming with regards to the set of registered patients; e.g. we cannot rule out that treated care centers managed to enrol very healthy patients, whose probability of making a visit was close to zero.

[34] There is no way to similarly redefine *Waiting times*.

[35] As the treatment group in the patient satisfaction sample contains very few pre-reform monopolies, we have limited power to detect heterogeneity between initial monopolies and initial non-monopolies.

that does not suffer from the included ambulatory care sensitive conditions. There is a lack of available objective indicators of how good care centers are at preventing adverse health outcomes. In addition, it is unclear how well the information that currently is made publicly available (mainly subjective patient satisfaction measures) relates to objective health outcomes like the ACSC rate. For these reasons, improving quality of care for ACSC patients may not be highly prioritized by (possibly semi-altruistic) care centers in more competitive environments. Instead, they are more likely to cater to the larger set of patients that are interested in other quality dimensions.

The few significant improvements we detect regard patients' overall impression, which is important to most patients and reflects quality dimensions that are readily observable. By contrast, we find no improvements of another observable quality dimension, *Waiting times*. A plausible explanation relates to the costs of shortening waiting times, which may require recruitment of more staff or tedious rescheduling. In comparison, the costs associated with adopting a more service-minded (or lenient) attitude during appointments – thus affecting patients' overall impressions – are negligible.

There are reasons to believe that frictions on the demand side of the primary care market limit the potential for quality improvements. Survey evidence suggests that Swedish patients rarely use publicly reported information to evaluate providers (Glenngård et al., 2011; Swedish Agency for Health and Care Services Analysis, 2013). Randomized field experiments moreover indicate that patients are more likely to switch care center if they are provided comparative information about nearby care centers (Anell et al., 2016b). Further facilitating patients' access to information may therefore be one way for county councils to increase patient mobility, and to improve demand responsiveness and care quality.

It is in order to discuss some limitations of our study. For primarily two reasons, our estimates may overstate the beneficial effects of competition. The first is unobserved changes in the patient mix, for example due to urbanization among healthier population groups. Given that we find little pre-trend 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.

The second issue is changes in access to care that correlate with our treatment variable. A larger supply of providers simultaneously intensifies the competitive pressure and increases patients' access to care (notably, this problem affects all studies that rely on changes in the number of providers to estimate competition effects). Access may further be affected by changes in the distribution of GPs. For instance, it is possible that the number of GPs at care centers in our comparison group decreased, if physicians migrated into markets with higher competition (due to higher salaries or locations in attractive areas). Greater access may by itself reduce the need for hospitalizations and affect patient satisfaction. It is, thus, possible that a beneficial accessibility effect on the ASCS rate balances adverse effects due to increased competition, thus producing a negligible net effect. Recalling that patients in our treatment group did not become more satisfied with access to care (i.e. waiting times), such an interpretation is less likely.

If the degree of competition between care centers is higher in some municipalities in the control group than in some of municipalities in the treatment group, our results may underestimate the magnitude of competition effects. Of note, such measurement error bias is a concern to all studies that do not perfectly measure the degree of competition experienced by providers (but instead relies on proxies based on market shares, number of firms, distances etc). As a final caveat, we want to highlight that our conclusions are based on a follow-up period of approximately three years. This may be too short a period for capturing 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 favourably to most studies of competition in the health care sector, including those using similar outcomes.

### 8.1 Concluding remarks

Our results indicate that increasing competition in primary care markets by promoting entry, increasing the information about providers, and reducing search and switching costs yields modest improvements of patients' overall impressions, but no significant effects on avoidable hospitalizations and (patients' satisfaction with) access to care. The results are in line with those of earlier panel data studies on primary care in markets with regulated prices, which find zero or trivial effects on objective measures (Gravelle et al., 2016a), and some indications of increased leniency towards patients (e.g. Kann et al., 2010; Iversen and Ma, 2011; Fogelberg, 2014; Markussen and Røed, 2016). The association between competition and quality appears weak or non-existent also in primary care markets with market prices (Johar et al., 2014; Gravelle et al., 2016b).

The evidence from primary care can be contrasted to the hospital competition literature, which more often finds stronger associations between competition and quality. Comparing hospital and primary care markets, a salient difference is that the choice of hospital is more likely to be guided by health care personnel. Also, switching GP implies giving up a long-run relationship, which is less often the case for the more specialized interventions in hospitals. An interesting question for future research is to relate differential effects of competition – between and within markets – to potential frictions on the demand and supply side, in order to identify institutional settings that produce beneficial effects of choice and competition.

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# A Appendix

## A.1 Perceptions of being informed

In Table A.1, we report results from LPM models examining differences in how informed about the choice of care center citizens perceive themselves. The dependent variable is an indicator equal to 1 if the respondent answered "yes'" to a question of whether the respondent felt he/she had been sufficiently informed to make an active choice of care center.[36] The measure is taken from a nationwide online survey made by the Swedish Competition Authority in October 2011, which included in total 2,029 respondents and aimed to be representative of the population 18-75 years in all county councils (Swedish Competition Authority, 2012). Column (1) includes only the treatment indicator and county council fixed effects for the full sample. Column (2) adds respondent covariates from the survey (indicators for gender, age group, and number of visits). Column (3) and (4) mirrors (1) and (2), respectively, but uses the restricted sample of municipalities that we use in the analysis of the patient satisfaction surveys.

The coefficients indicate that patients in our treatment group tend to report that they are sufficiently informed more often than the comparison group, but no coefficients are significant. The size of the coefficients ranges from 1.1 to 3.4 percentage points, which can be compared to the comparison group mean frequencies of 58 and 56 percent in the full and restricted sample. Importantly, we find no signs of the treatment group being *less* informed. Such a finding would have been a concern for our identification strategy, as it would indicate that the reforms affected competition in the treatment group more than the comparison group in some respects, and less in others.

## A.2 Sensitivity to treatment cut-off

### A.2.1 Ambulatory care

Our treatment group is constituted by municipalities where the pre-reform number of residents per care center was higher than 5,500. We here check sensitivity to raising the cut-off to 6,000 and lowering it to 5,000. With the higher cut-off, 21 municipalities formerly in the treatment group switch to the comparison group. With the lower cut-off, 28 municipalities formerly in the comparison group switch to the treatment group.

Figure A.1, which is analogous to Figure 3, indicates no substantial or significant effects on the ACSC rate for either a higher (upper part of figure) or a lower cutoff. There is a slight negative pre-trend with the lower definition, i.e. when some formerly comparison municipalities switch to the treatment group, though no pre-trend estimate are significant. On the other hand, the pre-reform trends for the higher treatment cut-off looks even better than with our baseline definition. Indeed, when scrutinizing the data, we find that most of the market structure impacts shown in Section 5 derive from municipalities that remain in the treatment group even with the higher cut-off.

Table A.2 shows estimates analogous to those in Table 5. Though the point estimates are somewhat different, they are still very small in relation to the mean as well as the standard deviation of the dependent variable, and none is statistically significant. Thus, also with other cut-offs, a null effect is the most reasonable interpretation.

[36] Original Swedish wording: "Tycker du att du har fått tillräckligt med information för att kunna göra ett aktivt val av vårdcentral?".

Table A.1: Perceptions of being informed

| | (1)<br>LPM | (2)<br>LPM | (3)<br>LPM | (4)<br>LPM |
|---|---|---|---|---|
| *Treatment* | 0.0106 | 0.0217 | 0.0225 | 0.0344 |
| | (0.0408) | (0.0408) | (0.0376) | (0.0374) |
| *Male* | | -0.0471** | | -0.0489* |
| | | (0.0186) | | (0.0286) |
| $Age\ 25-34$ | | 0.152** | | 0.134** |
| | | (0.0681) | | (0.0640) |
| $Age\ 35-44$ | | 0.186** | | 0.254*** |
| | | (0.0659) | | (0.0597) |
| $Age\ 45-54$ | | 0.211*** | | 0.232*** |
| | | (0.0595) | | (0.0592) |
| $Age\ 55-64$ | | 0.311*** | | 0.357*** |
| | | (0.0577) | | (0.0581) |
| $Age\ 65+$ | | 0.321*** | | 0.325*** |
| | | (0.0563) | | (0.0590) |
| $Visits\ 1$ | | 0.108** | | 0.103** |
| | | (0.0434) | | (0.0448) |
| $Visits\ 2$ | | 0.104** | | 0.104** |
| | | (0.0453) | | (0.0462) |
| $Visits\ 3$ | | 0.133*** | | 0.103** |
| | | (0.0378) | | (0.0482) |
| $Visits\ 4-10$ | | 0.116*** | | 0.133*** |
| | | (0.0398) | | (0.0470) |
| $Visits\ 10+$ | | 0.171** | | 0.223** |
| | | (0.0666) | | (0.0939) |
| Observations | 1,939 | 1,938 | 1,105 | 1,105 |
| $R^2$ | 0.059 | 0.105 | 0.056 | 0.110 |

*Note*: The table reports coefficients and standard errors from LPM models where the dependent variable is an indicator equal to 1 if the respondent answered "yes'" to a question of whether the respondent felt he/she had been sufficiently informed to make an active choice of care center. All specifications include county council fixed effects. Standard errors clustered by county council in parentheses in column (1) and (2). *** p<0.01, ** p<0.05, * p<0.1.

Table A.2: Objective quality: sensitivity to treatment cutoff

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Baseline | Linear trends | Covariates | Donut |
| **Panel A: Higher cutoff** | | | | |
| *Treatment* | -0.189 | -0.190 | 0.0263 | 0.0182 |
| | (0.281) | (0.230) | (0.267) | (0.239) |
| **Panel B: Lower cutoff** | | | | |
| *Treatment* | -0.0475 | 0.156 | 0.378 | 0.353 |
| | (0.307) | (0.312) | (0.276) | (0.280) |
| 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 |
| County councils | 21 | 21 | 21 | 21 |

*Note*: The table shows coefficients from estimations contrasting the groups with average low and high number of patients per care center with *ACSC rate* as dependent variable. Cutoff is less than one care center per 6,000 residents in the upper panel (A), and 5,000 residents in the lower panel (B). All specifications include municipality, year, quarter, and month-to-reform fixed effects. The sample covers the period starting 42 months before reform implementation to 42 months after and uses *ACSC rate* as dependent variable. Standard errors clustered by county council in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

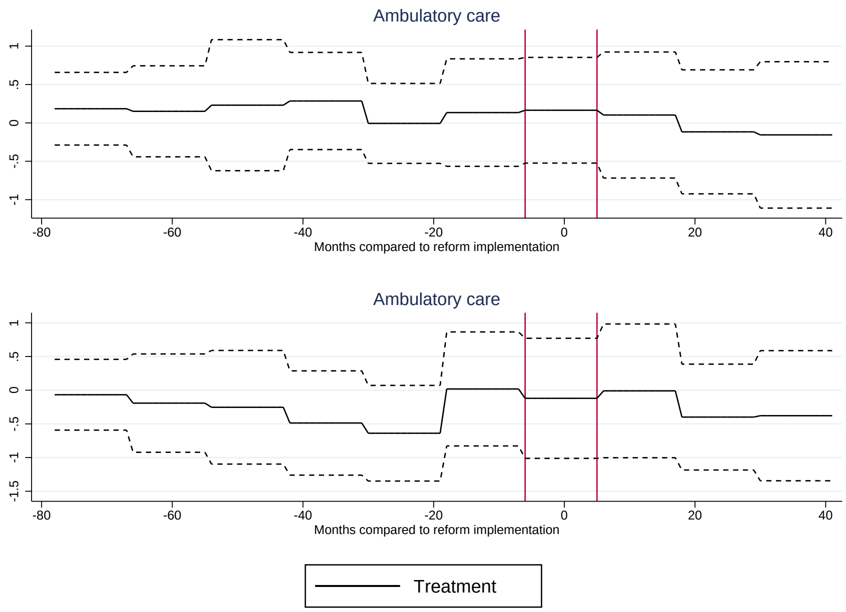


Figure A.1: The lines display the coefficients (solid) and confidence intervals (dashed) from Eq. (3) excluding covariates and municipality-specific linear trends. The first 12 months are used as reference period, thus excluded from the figure. Standard errors are clustered by county council. Pre-reform mean (standard deviation) of *ACSC rate* is 17.1 (6.1). **Upper panel:** higher treatment cut-off ($>$ 6,000 residents per care center). **Lower panel:** lower treatment cut-off($>$ 5,000 residents per care center).

### A.2.2 Subjective measures

When we raise the cut-off for being classified as treated to at least 6,000 patients per care center, 10 municipalities switch from treatment to comparison in the subjective measure sample. When we lower the cut-off to at least 5,000 patients per care center, 17 change from comparison to treatment. As shown in Table A.3 neither of these changes have much impact on the results. The only notable difference to Table 8 is that the effect for *Recommendation* becomes insignificant when we use the lower cut-off.

## A.3 Unplanned inpatient care

The frequency of *ACSC rate* is low, and the variable is relatively noisy. We therefore use another measure, *Unplanned inpatient care*, to check robustness. This variable measures the number of acute inpatient care episodes per 10,000 residents (see Table A.4 for descriptives). The better the quality of preventive and primary care, the fewer acute inpatient care episodes should occur. The measure has two main drawbacks: First, it is a less clear cut measure of primary care quality, as many acute inpatient care episodes cannot be prevented by primary care. Second, the data quality for this variable is not as good as for ambulatory care. In particular, the reporting from one county council deteriorates during approximately six months during our period.

Figure A.2 indicates an increasing trend in the treatment group from 90 to about 20 months before reform, after which the trend declines (a declining trend indicates an improvement in quality). This decline is slight though, and it starts well before the implementation of reforms. It therefore seems highly tenuous to attribute the improvements to the reforms. None of the yearly treatment

Table A.3: Subjective measures of primary care quality with higher and lower cutoff for treatment

| | Panel A: Higher cutoff | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | *Phone access* | *Waiting times* | *Overall impression* | *Recommendation* |
| *Treatment* | 0.0190 | -0.0177 | 0.0257** | 0.0184* |
| | (0.0158) | (0.0117) | (0.0105) | (0.0111) |
| Observations | 89,300 | 87,024 | 115,553 | 115,059 |
| Municipalities | 123 | 123 | 123 | 123 |
| County councils | 12 | 12 | 12 | 12 |
| $R^2$ | 0.067 | 0.045 | 0.089 | 0.093 |

| | Panel B: Lower cutoff | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | *Phone access* | *Waiting times* | *Overall impression* | *Recommendation* |
| *Treatment* | 0.0217 | 0.00295 | 0.0257** | 0.0188 |
| | (0.0178) | (0.0141) | (0.0119) | (0.0133) |
| Observations | 89,300 | 87,024 | 115,553 | 115,059 |
| Municipalities | 123 | 123 | 123 | 123 |
| County councils | 12 | 12 | 12 | 12 |
| $R^2$ | 0.067 | 0.045 | 0.089 | 0.093 |

*Note*: Standard errors clustered by municipality in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The table shows estimates using higher and lower cut-offs for treatment; in Panel A, the cut-off is at least 6,000 residents per care center, in Panel B, the cut-off is at least 5,000 patients per center. Municipality and survey wave fixed effects and individual covariates are included in all estimations. The (joint treatment and comparison) means of the dependent variables in 2009 are: *Phone access*: 0.500; *Waiting times*: 0.564; *Overall impression*: 0.641; *Recommendation*: 0.714. The differences in total observations reflect differential response rates to the underlying survey questions.

Table A.4: Unplanned inpatient care pre-reform

| | Treatment | | | | comparison | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) Mean | (2) SD | (3) Min | (4) Max | (5) Mean | (6) SD | (7) Min | (8) Max |
| *Unplanned inpatient care* | 99.5 | 19.0 | 39.6 | 159.3 | 111.9 | 23.8 | 41.0 | 218.0 |
| *N.o. municipalities* | 147 | | | | 142 | | | |

*Note*: Pre-reform = the period 18 to 7 months before a reform. Monthly municipality-level data.

or placebo effects are significantly different from zero, and most are small.[37]

Table A.5 displays similar estimations as the baseline estimations for ACSC (Table 5) in the main text. The estimates are negative in all four specifications, and significant in columns (1) and (2). When we add covariates in columns (3) and (4), the estimates become smaller and insignificant. The effect is larger than for *ACSC rate* (the covariate specification estimate is 0.08 of the joint pre-reform standard deviation of 23.3, as compared to 0.03 for the ACSC rate). But the estimate is still relatively modest, indicating little effect on objective quality measured by unplanned hospital admissions.

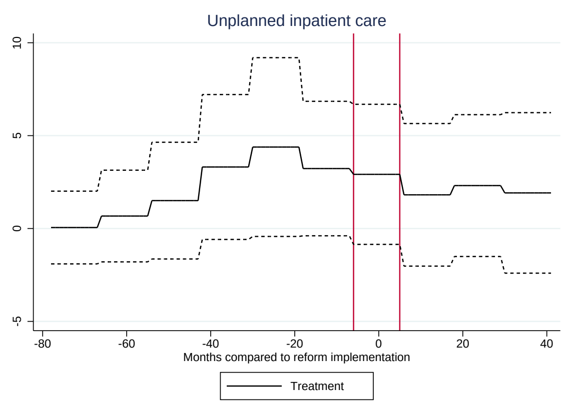


Figure A.2: The lines display the coefficients from regressions using a variant of equation 2 on a period starting 90 months before a reform is implemented and 42 months after. The first 12 months is the reference category for all groups, and the lines are all flat and equal to the comparison group by definition during this period. There are six placebo effects before the reforms are implemented (Months compared to reform = 0). Treatment starts 6 months before a reform is implemented. The dependent variable is *Unplanned inpatient care*. The estimation displayed includes covariates. The joint treatment and comparison group mean and standard deviation in the pre-reform period is 101.8 and 23.3. Standard errors used to construct confidence intervals are clustered on municipality.

## A.4 Sensitivity to population size

The differences in the results from the weighted and unweighted regressions calls for examination of the influence of very large and small municipalities. In order to check the robustness of our results, we re-estimate our estimations excluding the largest and smallest 5 percent of the mu-

[37] The confidence interval (dashed lines) use standard errors clustered by municipality instead of county council. The reason is that 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 coefficient are far from significant anyway.

Table A.5: DID models using *Unplanned inpatient care* as dependent variable

| | (1) Baseline | (2) Linear trends | (3) Covariates | (4) Donut |
|---|---|---|---|---|
| *Treatment* | -3.165*** | -4.681** | -1.835 | -1.793 |
| | (1.081) | (2.080) | (2.086) | (1.762) |
| Linear trends | No | Yes | Yes | Yes |
| Covariates | No | No | Yes | No |
| Observations | 38,125 | 38,125 | 38,125 | 38,125 |
| Municipalities | 289 | 289 | 289 | 289 |
| County councils | 21 | 21 | 21 | 21 |

*Note*: The table shows coefficients from estimations contrasting the groups with high and low pre-reform number of patients per care center with *Unplanned inpatient care* as the dependent variable. All specifications include municipality, year, quarter, and month-to-reform fixed effects. The sample covers the period starting 90 months before reform implementation to 42 months after. The joint treatment and comparison group mean and standard deviation in the pre-reform period is 101.8 and 23.3 Standard errors clustered by county council in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

nicipalities from the estimation samples. For the objective measure (the ACSC rate), this means that we exclude 30 of the 289 municipalites in the original sample. The sample restriction has a substantial effect on the min-max range of population size; looking at the reform month, the min-max range changes from 2,460-810,120 residents in the original sample to 4,931-95,732 residents in the restricted sample. Despite removing all major cities, the DID estimations on the ACSC rate yields overall similar results as in the full sample: Figure A.3 and Table A.6 show no indications of increased competition having substantial impact on the ACSC rate.

Moving to the subjective measures, the exclusion of the largest and smallest 5 percent implies that 14 out of the 123 municipalities in the original sample are excluded. The min-max range of population size in 2009 thereby narrows down from 2,500-134,006 to 4,361-84,736. Though this is a notable change, it is not as striking as the change for the full sample of municipalities. The explanation is that for the subjective sample, the largest municipalities are already left out of the sample because their counties did not participate in the patient survey before the reforms. For this reason, we expect the scope for heterogeneity revealed by the restricted sample to be limited.

Indeed, the estimates for the subjective measures, presented in Table A.7, are similar to the baseline results. Only one estimate is statistically significant, but given the similarity of estimates we believe that this is more likely due to the loss of precision than to heterogeneity in terms of population size.

The treatment group in the patient satisfaction sample contains comparably few pre-reform monopolies (16). We therefore have little power to detect heterogeneity by initial monopoly status. However, it can be noted that in models including covariates (not shown), there is a tendency of heterogeneity with respect to the effect on waiting times, which is negative (though insignificant) for non-monopolies (marginal effect=-0.0131, p=0.334) but positive (though insignificant) for monopolies (marginal effect = -0.0131+0.0296 =0.0165). Notably, the interaction term is also far from significant (p=0.375).

Table A.6: Heterogeneity over large and small municipalities, and initial market structure

Panel A: Excluding the largest and smallest municipalities

| | (1) Baseline | (2) Linear trends | (3) Covariates | (4) Donut |
|---|---|---|---|---|
| *Treatment* | -0.247 | -0.111 | -0.0972 | -0.0442 |
| | (0.304) | (0.294) | (0.305) | (0.253) |
| Linear trends | No | Yes | Yes | Yes |
| Covariates | No | No | Yes | Yes |
| Observations | 34,169 | 34,169 | 34,169 | 34,169 |
| Municipalities | 259 | 259 | 259 | 259 |
| County councils | 21 | 21 | 21 | 21 |

Panel B: Heterogeneity over initial market structure

| | (1) Linear trends | (2) Covariates | (3) Linear trends | (4) Covariates |
|---|---|---|---|---|
| *Treatment* | -0.403 | -0.350 | -0.156 | -0.144 |
| | (0.327) | (0.279) | (0.297) | (0.250) |
| *Treatment* $\times$ *OneCenter* | 0.436 | 0.114 | -0.0872 | 0.0668 |
| | (0.351) | (0.358) | (0.326) | (0.315) |
| 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 |
| County councils | 21 | 21 | 21 | 21 |

*Note*: All specifications use *ACSC rate* as dependent variable, and include municipality, year, quarter, and month-to-reform fixed effects. The sample covers the period starting 90 months before reform implementation to 42 months after. Panel A shows coefficients from estimations contrasting the treatment and comparison groups where we have excluded the 5 percent largest and smallest municipalities. Panel B shows results from estimations on the full sample of municipalities; in these estimations, we estimate a separate effect for municipalities in which there was only one care center before the reform. Standard errors clustered by county council in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.7: Subjective measures of primary care quality, excluding largest and smallest municipalities

Panel A: No covariates

| | (1) *Phone access* | (2) *Waiting times* | (3) *Overall impression* | (4) *Recommendation* |
|---|---|---|---|---|
| *Treatment* | 0.0196 | -0.00566 | 0.0200 | 0.0244 |
| | (0.0192) | (0.0128) | (0.0144) | (0.0162) |
| Observations | 71,077 | 68,969 | 91,381 | 90,847 |
| Municipalities | 109 | 109 | 109 | 109 |
| County councils | 12 | 12 | 12 | 12 |
| $R^2$ | 0.043 | 0.026 | 0.018 | 0.031 |

Panel B: Individual level covariates

| | (1) *Phone access* | (2) *Waiting times* | (3) *Overall impression* | (4) *Recommendation* |
|---|---|---|---|---|
| *Treatment* | 0.0183 | -0.00195 | 0.0179 | 0.0224* |
| | (0.0183) | (0.0132) | (0.0116) | (0.0135) |
| *Stable contact* | 0.132*** | -0.0942*** | 0.225*** | 0.226*** |
| | (0.00598) | (0.00629) | (0.00426) | (0.00497) |
| *Tolerable health* | 0.0640*** | 0.0602*** | 0.0998*** | 0.114*** |
| | (0.00817) | (0.00854) | (0.00783) | (0.00781) |
| *Good health* | 0.0979*** | 0.114*** | 0.176*** | 0.165*** |
| | (0.00919) | (0.00890) | (0.00872) | (0.00848) |
| *Very good health* | 0.180*** | 0.177*** | 0.297*** | 0.211*** |
| | (0.00944) | (0.00837) | (0.00968) | (0.00879) |
| *Excellent health* | 0.198*** | 0.252*** | 0.331*** | 0.244*** |
| | (0.0107) | (0.0116) | (0.0105) | (0.0101) |
| *Visits1* | -0.00646 | 0.0422*** | -0.0119** | -0.0203*** |
| | (0.00701) | (0.00576) | (0.00586) | (0.00472) |
| *Visits23* | 0.00544 | 0.0879*** | -0.0110** | -0.0393*** |
| | (0.00649) | (0.00657) | (0.00502) | (0.00496) |
| *Visits4* | 0.0154** | 0.138*** | 0.0174*** | -0.0469*** |
| | (0.00591) | (0.00705) | (0.00570) | (0.00516) |
| Observations | 68,876 | 66,899 | 89,019 | 88,665 |
| Municipalities | 109 | 109 | 109 | 109 |
| County councils | 12 | 12 | 12 | 12 |
| $R^2$ | 0.066 | 0.052 | 0.089 | 0.096 |

*Note*: The largest and smallest five percent of municipalities are excluded. Municipality and survey fixed effects are included in all estimations. The sample covers the three years 2009, 2011, and 2013. The joint treatment and comparison group means of the dependent variables in 2009 are: *Phone access*: 0.500; *Waiting times*: 0.564; *Overall impression*: 0.641; *Recommendation*: 0.714. The differences in total observations reflect differential response rates to the underlying survey questions. Standard errors clustered by municipality in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

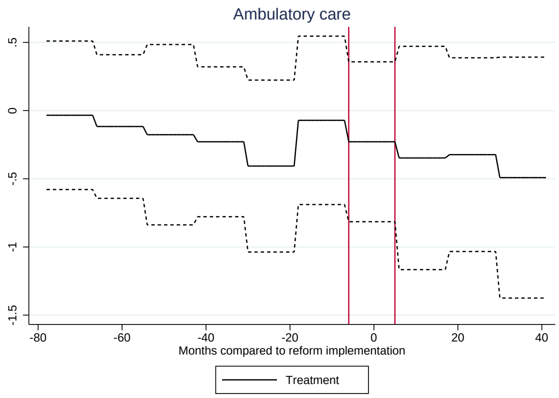


Figure A.3: The lines display the coefficients (solid line) and confidence intervals (dashed lines) from regressions where we have excluded the 5 percent largest and smallest municipalities. We use a variant of equation 2 on a period starting 90 months before a reform is implemented and 42 months after. The first 12 months is the reference category for all groups, and is excluded from the figure. There are 6 placebo effects before the reforms are implemented (Month compared to reform = 0). Dependent variable is *ACSC rate*. The joint mean and standard deviation over the pre-reform period is 16.8 and 5.5 respectively. The confidence interval is based standard errors clustered on county councils.

## A.5 Heterogeneity by initial monopoly status

The pre-reform market structure is another dimension over which there may be heterogeneity in the effect of competition. For instance, the effect of breaking up a monopoly may be qualitatively different from the effect of adding one more care center to an already competitive market. To test this, we estimate a triple interaction model for the ACSC rate, using an interaction between the treatment indicator and a dummy for pre-reform monopoly municipalities. Panel B of Table A.6 presents the results. The interaction term is statistically insignificant in all specifications, though it can be noted that the treatment effect is qualitatively different in pre-reform monopolies as opposed to non-monopolies: Within the set of pre-reform *non*-monopolies, the ACSC rate decreases slightly more in treated municipalities (estimate *Treatment*); the small magnitudes are similar to our baseline results, and the trend specification estimate is even statistically significance at the 10 percent level. Within the group of pre-reform monopolies, the treatment effect estimates are very close to zero ($Treatment + Treatment \times OneCenter$) but of positive sign.

## A.6 Importance of actual entry

An advantage of our identification strategy relative to strategies relying on direct measures of the current number of players in the market (and market shares) is that it has the potential to pick up other consequences of the reform than increased entry, for example reduced switching costs and increased access to comparative information. But if actual entry is really what matters for quality, our approach underestimates the effect, as the resulting treatment group includes many municipalities where there was no entry. In this section, we therefore instead concentrate on the importance of actual entry. *New entry* is a dummy variable taking the value one in the post-reform period for municipalities where there was at least one new care center established during the period 6 months pre- to 42 months post-reform.

Defining treatment in this way implies that there are 105 municipalities in the treatment group and 184 municipalities in the comparison group in the estimations for ACSC. Figure A.4 is analogous to Figure 3, though with treatment defined by *New entry*. Compared to Figure 2, the *New entry* treatment definition displays more of a downward sloping trend, but all treatment and placebo effects are small and insignificant. Column (1) of Table A.8 confirms that the relationship between new entry and avoidable hospitalizations is small and statistically insignificant.

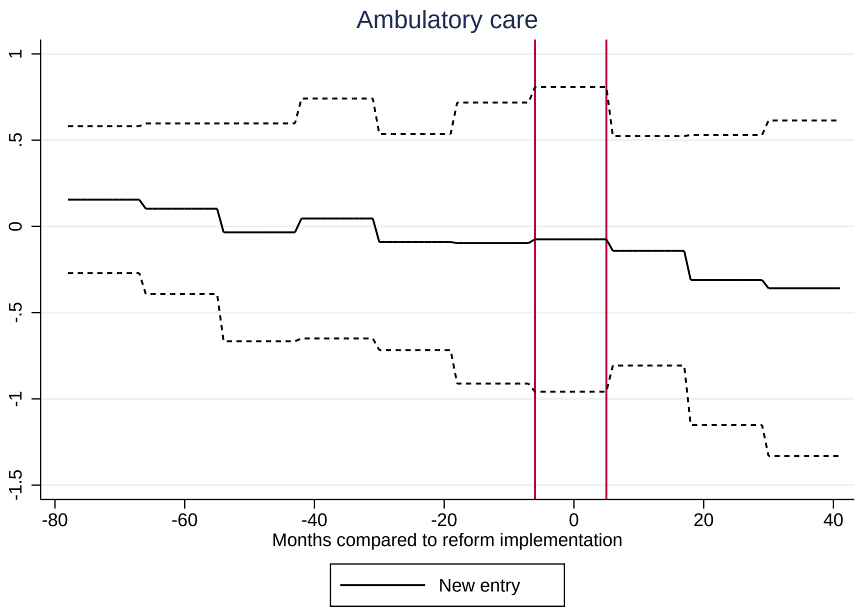


Figure A.4: The lines display the coefficients (solid line) and confidence intervals (dashed lines) from regressions using a variant of equation 2 on a period starting 90 months before a reform is implemented and 42 months after. The first 12 months is the reference category for all groups, and are excluded from the figure. There are 6 placebo effects before the reforms are implemented (Month compared to reform = 0). Dependent variable is *ACSC rate*. Mean and standard deviation over the pre-reform period is 17.1 and 6.1 respectively. Confidence intervals use standard errors clustered on county councils. Treatment is defined by having new entry in the period from 6 months before a reform to 42 months after.

By 'cherry-picking' municipalities in which at least one new care center entered the market, we clearly introduce endogeneity; that is, it is possible that potential profits from entry correlate with the potential for quality changes. Therefore, we next use our previous treatment definition (Eq. 1) as an instrumental variable for *New entry*. Column (2) of Table A.8 contains the reduced form estimates and column (3) shows the first stage, which indicates a strong positive effect of the instrument on new entry ($F = 35.29$). Column (4) shows the IV estimate of the effect of new entry on avoidable hospitalizations. The estimate is negative and of larger magnitude than the reduced form, but far from significant.

Moving to the subjective measures, 35 of the 123 municipalities in this sample experienced new entry. Panels A-D of Table A.9 presents the DID and IV results for the four subjective measures. The first stage estimates for the IV (column 2) are about 0.4 and highly significant in all four cases. The slightly different coefficients, and $F$-values, in these regressions is caused by the differential response rates for the four questions. Both the DID and IV results indicate larger quality improvements in municipalities that experienced actual entry after the reforms. In the DID, it is also notable that phone access is significantly higher in the treatment group (Panel A) and that the association with *Waiting times* (Panel B) is positive (though insignificant and much smaller in size compared to the other measures). Comparing coefficients with the baseline model shows that

Table A.8: Results for *ACSC rate* defining treatment by new entry post-reform.

| | (1) DID | (2) Reduced form | (3) First stage | (4) IV |
|---|---|---|---|---|
| *New entry* | 0.149 | | | -0.400 |
| | (0.184) | | | (0.708) |
| *Treatment* | | -0.169 | 0.421*** | |
| | | (0.295) | (0.0715) | |
| Observations | 38,125 | 38,125 | 38,125 | 38,125 |
| Municipalities | 289 | 289 | 289 | 289 |
| County councils | 21 | 21 | 21 | 21 |
| First-stage $F$-statistic | | | 34.68 | |

*Note*: Column (1) is a DID regression using *New entry* instead of the previous treatment definition. Columns (2)-(4): IV-estimations using the previous treatment definition (the *Treatment* variable in Table 5) as an instrumental variable for *New entry*. All specifications include covariates, municipality linear trends and municipality-, year-, quarter-, and month-to-reform fixed effects. The sample period ranges from 90 months pre- to 42 months post-reform. Standard errors clustered by county council in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

subjective quality improvements are even larger when defining treatment by actual entry.

While the results in this section are similar (or stronger) than the baseline results, it should be stressed that the entry-based specifications rely on stronger assumptions than our baseline model. As already mentioned, the DID based on actual entry runs into the risk of selecting a treatment group of municipalities that would have had a favorable quality development anyway. The slight negative (though insignificant) pre-trend in Figure A.4 indicates that this concern may be warranted. Also, when checking for patient mix differences according to *New entry*, we find that the share of patients with a stable physician contact has increased significantly more in the new entry group (results not shown). This variable has a strong positive relation to high quality ratings according to Table 8. While it does not prove that there are differences in the patient mix (for instance, the likelihood of stable contacts may indeed have increased, simply because of competition), we can likewise not rule out that it reflects a changing patient mix. The IV strategy in turn requires that the reforms affected quality only via new entry. That is a strong assumption, given the increased availability of information, lower switching costs, and not least stronger potential threats of entry coupling the reforms. The next section indicates that other channels than entry indeed may be important.

## A.7 Importance of choice

This section contains an exploratory analysis of the importance of other consequences of the reform than new entry. To remove the influence of new entry, we concentrate on the set of municipalities where there was no entry from six months before a reform and throughout the post-reform period. Within this set, we contrast municipalities where there was only one center throughout the period with the municipalities where there was always at least two care centers. The always-monopoly municipalities were arguably less affected by other features of the reform: increased access to information and lower switching costs have few implications if there is only one primary care center, and the threat of potential entry ought to remain small in this group including many small municipalites.

In the estimations using the ACSC rate as dependent variable, the set of municipalities not

Table A.9: Instrumental variables estimation, subjective measures

Panel A: *Phone access* as dependent variable

| | (1) DID | (2) Reduced form | (3) First stage | (4) IV |
|---|---|---|---|---|
| *New entry* | 0.0492*** | | | 0.0505 |
| | (0.0169) | | | (0.041) |
| *Treatment* | | 0.0196 | 0.387*** | |
| | | (0.0160) | (0.114) | |
| Observations | 89,300 | 89,300 | 89,300 | 89,300 |
| Municipalities | 123 | 123 | 123 | 123 |
| County councils | 12 | 12 | 12 | 12 |
| $F$-statistic, excluded instrument | | | 11.64 | |

Panel B: *Waiting times* as dependent variable

| | (1) DID | (2) Reduced form | (3) First stage | (4) IV |
|---|---|---|---|---|
| *New entry* | 0.0127 | | | -0.0169 |
| | (0.0117) | | | (0.0313) |
| *Treatment* | | -0.00658 | 0.389*** | |
| | | (0.0118) | (0.113) | |
| Observations | 87,024 | 87,024 | 87,024 | 87,024 |
| Municipalities | 123 | 123 | 123 | 123 |
| County councils | 12 | 12 | 12 | 12 |
| $F$-value, excluded instrument | | | 11.79 | |

Panel C: *Overall impression* as dependent variable

| | (1) DID | (2) Reduced form | (3) First stage | (4) IV |
|---|---|---|---|---|
| *New entry* | 0.0403*** | | | 0.0627** |
| | (0.0101) | | | (0.0259) |
| *Treatment* | | 0.0252** | 0.402*** | |
| | | (0.0101) | (0.110) | |
| Observations | 115,553 | 115,553 | 115,553 | 115,553 |
| Municipalities | 123 | 123 | 123 | 123 |
| County councils | 12 | 12 | 12 | 12 |
| $F$-value, excluded instrument | | | 13.34 | |

Panel D: *Recommendation* as dependent variable

| | (1) DID | (2) Reduced form | (3) First stage | (4) IV |
|---|---|---|---|---|
| *New entry* | 0.0348*** | | | 0.0557* |
| | (0.0105) | | | (0.0284) |
| *Treatment* | | 0.0224* | 0.402*** | |
| | | (0.0115) | (0.110) | |
| Observations | 115,059 | 115,059 | 115,059 | 115,059 |
| Municipalities | 123 | 123 | 123 | 123 |
| County councils | 12 | 12 | 12 | 12 |
| $F$-value, excluded instrument | | | 13.27 | |

*Note*: Standard errors clustered by municipality in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All estimations include individual level covariates, municipality and survey fixed effects. The sample covers the three years 2009, 2011, and 2013, and all observations are weighted by the inverse municipality level probability of being in the survey a certain year. The joint treatment and comparison group means of the dependent variables in 2009 are: *Phone access*: 0.500; *Waiting times*: 0.564; *Overall impression*: 0.641; *Recommendation*: 0.714. The differences in total observations reflect differential response rates to the underlying survey questions. The differential response rate also explains the slightly different $F$-values in the first stage regressions.

experiencing entry includes 95 municipalities with at most one care center (*NoEntryComp*=0) and 89 municipalities with at least two care centers (*NoEntryComp*=1). Figure A.5 and Table A.10 show that the latter group improved slightly more in terms of the ACSC rate, but the point estimates are insignificant, relatively small, and the decline in ACSC episodes starts well before the reforms are implemented.

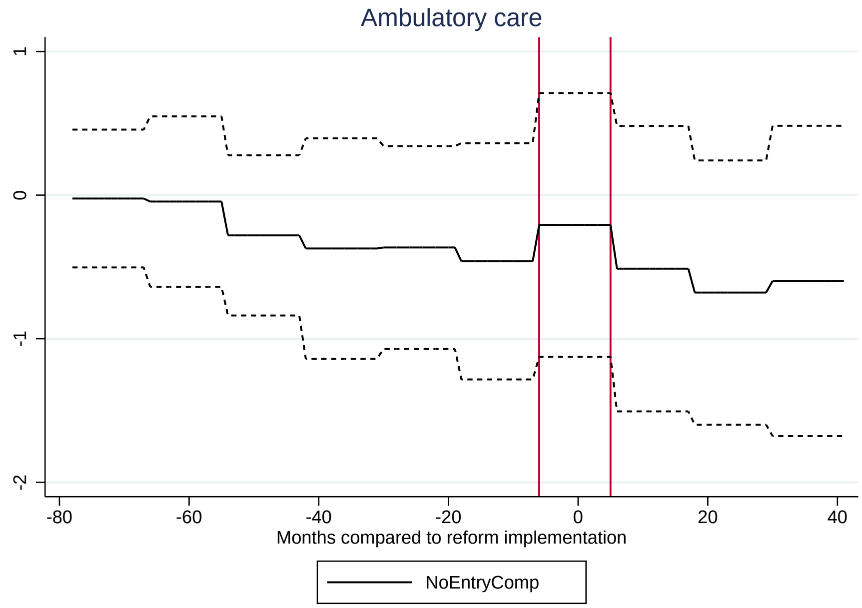


Figure A.5: The lines display the coefficients (solid line) and confidence intervals (dashed lines) from regressions using a variant of equation 2 on a period starting 90 months before a reform is implemented and 42 months after. The first 12 months is the reference category, excluded from the figure. There are 6 placebo effects before the reforms are implemented (Month compared to reform = 0). Dependent variable is *ACSC rate*. The sample includes only municipalities that did not experience entry during the period of 6 months before a reform to 42 months after. *NoEntryComp* includes municipalities where there was two or more care centers; the comparison group had only one care center. Confidence intervals use standard errors clustered by county council.

Table A.11 contains corresponding results for the subjective outcome measures. In this sample, the set of municipalities not experiencing entry includes 47 monopolies and 41 municipalities with at least two care centers. With the exception of *Waiting times*, the estimates on *NoEntryComp* are positive and significant. The estimates are larger than our baseline estimations, around 4 percentage points for all three dependent variables (6-8 percent of the means).

These estimations rely on a heavily selected sample and should not be viewed as causal. They nevertheless suggest that there may be other channels for the reform over and above the entry channel. Notably, if entry is not the only channel, the IV estimation strategy of the previous section is invalid. The results in this section therefore reinforce our belief in the main identification strategy.

## A.8 Patient composition checks of survey data

In Table A.12, we present the outcomes of six regressions testing the hypothesis that the composition of patients changed differentially between the three survey waves in the treatment and comparison

Table A.10: Importance of choice, ACSC rate

| | (1) Baseline | (2) Trends | (3) Covariates | (4) Donut |
|---|---|---|---|---|
| *NoEntryComp* | -0.320 | 0.0808 | 0.0215 | -0.0384 |
| | (0.332) | (0.256) | (0.239) | (0.202) |
| Linear trends | No | Yes | Yes | Yes |
| Covariates | No | No | Yes | Yes |
| Observations | 24,283 | 24,283 | 24,283 | 24,283 |
| Municipalities | 184 | 184 | 184 | 184 |
| County councils | 21 | 21 | 21 | 21 |

*Note*: The estimation sample only includes municipalities that did not experience entry during the sample period. The coefficients contrast the group of 89 municipalities that had at least two care centers (*NoEntryComp*=1) with the 95 municipalities in which there was only one care center (*NoEntryComp*=0). All specifications use *ACSC rate* as dependent variable, and include municipality, year, quarter, and month-to-reform fixed effects. The sample covers the period starting 90 months before reform implementation to 42 months after. Standard errors clustered by county council in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

groups. Each column uses a different outcome variable, taking the value 1 if: the patient has a stable physician contact at the care center (column 1); has visited the care center at least once before (2), at least twice before (3), or four or more times before (4); reports the self-rated health as "*good*", "*very good*", or "*excellent*" (5), or "*very good*", or "*excellent*" (6). The results show neither substantial nor statistically significant differences. The results are similar if we remove the sampling weights (not shown).

Table A.11: Importance of choice, subjective measures

| | Panel A: No covariates | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | *Phone access* | *Waiting times* | *Overall impression* | *Recommendation* |
| *NoEntryComp* | 0.0400* | 0.00205 | 0.0370** | 0.0422** |
| | (0.0202) | (0.0167) | (0.0166) | (0.0183) |
| Covariates | No | No | No | No |
| Observations | 44,663 | 43,380 | 58,308 | 57,920 |
| Municipalities | 88 | 88 | 88 | 88 |
| County councils | 12 | 12 | 12 | 12 |
| $R^2$ | 0.050 | 0.031 | 0.019 | 0.038 |
| | Panel B: Individual-level covariates | | | |
| | (1) | (2) | (3) | (4) |
| | *Phone access* | *Waiting times* | *Overall impression* | *Recommendation* |
| *NoEntryComp* | 0.0404** | 0.00291 | 0.0383*** | 0.0406** |
| | (0.0200) | (0.0165) | (0.0140) | (0.0155) |
| Covariates | Yes | Yes | Yes | Yes |
| Observations | 43,284 | 42,094 | 56,820 | 56,545 |
| Municipalities | 88 | 88 | 88 | 88 |
| County councils | 12 | 12 | 12 | 12 |
| $R^2$ | 0.074 | 0.056 | 0.091 | 0.102 |

*Note*: The estimation sample only includes municipalities that did not experience entry during the sample period. The coefficients contrast the group of 41 municipalities that had at least two care centers (*NoEntryComp*=1) with the 47 municipalities in which there was only one care center (*NoEntryComp*=0). Municipality and survey fixed effects are included in all estimations. The sample covers the three years 2009, 2011, and 2013. The differences in total observations reflect differential response rates to the underlying survey questions. The (joint treatment and comparison) means of the dependent variables in 2009 are: *Phone access*: 0.49; *Waiting times*: 0.56; *Overall impression*: 0.65; *Recommendation*: 0.71. Standard errors clustered by municipality in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.12: Composition checks

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | *Stable contact* | *Visit* $\geq 1$ | *Visit* $\geq 2$ | *Visit* $\geq 4$ | *Health* $\succeq$ *good* | *Health* $\succeq$ *very good* |
| *Treatment* | 0.0141 | 0.00510 | 0.00730 | 0.00463 | -0.000971 | -0.00487 |
| | (0.0164) | (0.00654) | (0.00914) | (0.00900) | (0.00814) | (0.00856) |
| Observations | 118,841 | 118,530 | 118,530 | 118,530 | 118,572 | 118,572 |
| Municipalities | 123 | 123 | 123 | 123 | 123 | 123 |
| County councils | 12 | 12 | 12 | 12 | 12 | 12 |
| $R^2$ | 0.057 | 0.002 | 0.002 | 0.002 | 0.005 | 0.005 |

*Note*: The table shows coefficients from LPM regressions of six dummy variables capturing patient survey respondent characteristics. Each dummy is regressed on the treatment variable and municipality and year (survey) fixed effects. Observations are weighted by the square root of the inverse ratio of the number of sent out surveys to population size. Standard errors clustered by municipality in parentheses. *** p<0.01, ** p<0.05, * p<0.1