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Abstract

Policies aimed to spur quality competition among health care providers are ubiquitous, but their impact on quality is *ex ante* ambiguous. This study contributes to the sparse empirical literature on primary care quality by examining the heterogeneous impact of recent competition enhancing reforms in Sweden. The reforms led to substantially more entry of new providers in more exposed markets, but the effects on primary care quality in these markets were modest: we find small improvements of patients' overall satisfaction with care, but no consistently significant effects on avoidable hospitalization rates or satisfaction with access to care.

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

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1 Introduction

Since the late 1980's, policies promoting competition in the health care sector have been ubiqui-tous.[1] The hope is that market discipline will strengthen providers' incentives to respond to pa-tients' preferences in a cost-effective manner. In settings with regulated prices, standard health eco-nomic models predict that providers compete for patients by offering high-quality services (McGuire, 2011; Gaynor and Town, 2011). However, these stylized models abstract from patients' limited ability to infer quality, which weakens the link between competition and quality (e.g., Arrow, 1963). Even if patients are well-informed, the effects of competition on quality may differ across patient groups. Providers may react to increased responsiveness of demand by reducing the qual-ity offered to unprofitable patients (skimping), while increasing the quality offered to profitable patients (cream-skimming) (Brekke et al., 2014).

The ambiguous theoretical predictions call for empirical studies of the effects of competition enhancing health care policies. Credible empirical evidence is still scarce, especially in settings with regulated prices.[2] Although a literature on patient choice in the hospital sector has emerged in recent years (e.g., Cooper et al., 2011; Gaynor et al., 2013; Gravelle et al., 2014; Colla et al., 2016; Moscelli et al., 2016; Gaynor et al., 2016), there is a lack of studies that credibly identify the effects of competition on quality in the market for primary care services (Propper, 2012; Gaynor et al., 2015). Primary care – i.e., general practitioners' services – is the patient's first point of contact with health care, and plays a key role in an efficient health care system, promoting health via preventive services and treatment of patients with chronic diseases (e.g., Starfield et al., 2005; Rosano et al., 2013; Kringos et al., 2015; Bailey and Goodman-Bacon, 2015). As the provided services are much more multifaceted and the patient group more diverse, the impacts of competition in primary care are potentially very different compared to more specialized health care markets. Further, primary

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[1]Examples include patient choice policies in 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., 2017).

[2]Regulated prices is a common feature in countries with national health systems, but also in e.g., Medicare. The context is also of increasing interest due to the American discussion of enhancing quality competition by regulating prices (Glied and Altman, 2017).

care patients are likely less informed consumers compared to patients in hospital markets, who's decisions often are supported by their GPs expertise.

In this article, we provide evidence of the causal effects of increasing patient choice and reducing barriers to entry (i.e., competition enhancing measures) on broad measures of primary care quality. We use plausibly exogenous variation induced by a set of reforms implemented in Swedish primary care 2007–2010. Since the reforms, there is free entry for providers that meet basic requirements, there is more publicly available information about providers, and it has become easier (and less costly) to search for and switch providers. These three components have 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 depended on the initial market structure. Similar to Propper et al. (2008), Cooper et al. (2011) and Gaynor et al. (2013), who study competition among hospitals in the English National Health Service (NHS), we exploit prereform variation in market conditions in a difference-in-differences (DID) model. We start from the plausible assumption that markets in which there was room for new entrants before the reforms were on average more affected by all the main components of the reforms. Potential entrants ought to find it more attractive to enter where they could expect to attract a patient stock of reasonable size. Accordingly, we define a treatment group as consisting of municipalities where, six months before a reform, the average number of patients per care provider would exceed a median-sized private provider if one additional provider entered the market. Our results are of similar magnitude and sign when we vary the threshold value, and when we use a continuous treatment definition.

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 reforms – increased access to information about providers, 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 greater importance if there are more

providers in the market, and the treatment group had more providers on average, more entries, and more examples of pre-reform monopolies that were broken up after the reforms. Further, residents in these municipalities perceived themselves to be informed about available providers to a similar (if not larger) degree as residents in the comparison municipalities. Notably, these dimensions of increased competition would not be captured by alternative estimation strategies using direct measures of competition (e.g., 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 both objective and subjective indicators of care quality. The objective quality measure is the rate of hospitalizations for so-called ambulatory care sensitive conditions (ACSC). The ACSC hospitalization rate is closely linked to primary care quality, as ACSCs are medical conditions for which appropriate primary care would be expected to 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.

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 areas that were more affected by the reforms. The difference is about 2.5 percentage points, which amounts to 4 percent of the pre-reform mean. The impacts on the ACSC rate, patients' satisfaction with phone accessibility, and their willingness to recommend the practice to others indicate improvements, but the effects are small and not consistently significant. The estimate for waiting times is negative, but very small and never significant. We find no indications that the improvement of patients' overall impression is driven by cream-skimming or skimping.

Our 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 difficult for patients to observe, the subjective experience is observed by the patient per definition. Moreover, only the subjective measures are publicly available. 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.

One challenge to our interpretation of the results is that entries and exits may not only affect competition among providers, but also access to care. Entry, for instance, reduces the travel time to relevant providers for at least some patients, and may attract staff from other markets. Disentangling the causal effects of competition and access is challenging – in fact, we are not aware of any study that has been able, or even tried, to address the issue. However, we claim that the theoretical effects on access were of limited importance in our case. The physician and nurse density did not increase more in the treatment group, and travel times were reduced for only a small share of patients. Moreover, our most direct measure of access to care – i.e., waiting times – did not improve more in the treatment group. Further, in an exploratory fixed effects analysis restricted to municipalities where the number of providers did not increase, we find similar results when comparing monopoly to non-monopoly markets. The subjective quality measures increased more in non-monopoly markets, which were more affected by the reform components that increased information and reduced search and switching costs.

The article proceeds as follows: Section 2 discusses the previous literature. Section 3 gives a background to the Swedish primary health care system. Section 4 describes the reforms and their relationship to increased competition. Section 5 outlines our empirical strategy, which is backed by the results in Section 6, in which we examine changes to the market structure in the treatment and comparison group. Sections 7 and 8 show our estimates for objective and subjective quality measures. Section 9 explores mechanisms and discusses limitations, and Section 10 provides concluding remarks.

2 Related literature

The existing empirical literature on patient choice and competition in primary care settings with regulated prices suffers from a lack of exogenous variation, or uses outcome measures that either have an ambiguous relation to quality or rather measure opportunistic behaviour.[3] Across countries an settings, cross-sectional analyses on competition (proxied by number of competitors or density of general practitioners, GPs) and care quality generally show positive associations (e.g., Pike, 2010; Rosano et al., 2013; Berlin et al., 2014; Stroka-Wetsch et al., 2016, though see Jürges and Pohl (2012) for an exception), but may capture other unobservable factors rather than the effect of competition.

The study closest to ours in terms of causal ambitions is Gravelle et al. (2018) who study English GP practices. Their main identification strategy relies on GP practice fixed effects which account for the unobserved (geographical or demographic) common determinants of choice, competition, and quality that cross-sectional analysis fails to capture. The GP fixed effects however fails to address the endogeneity of entries and exits, as well as endogenous changes in the providers' patient-mix. As a robustness test they also exploit variation from a competition enhancing policy within the NHS, relying on similar identifying assumptions as our study to provide causal evidence. Overall, their results are similar to ours: There are no indications of competition having negative impact on quality, but the magnitude of the positive effects are small and are only robust for measures of patient satisfaction and not for objective quality measures. In all their analysis, Gravelle et al. (2018) use GP practices instead of local markets as the unit of analysis, which creates problems of ambiguous sample selections that our study avoid.

Moving from the regulated-price to a market price context, fixed effects studies of Australian primary care find results similar to ours, i.e., weak or zero association between competition and primary care quality (Johar et al., 2014; Gravelle et al., 2016).

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[3]This strand of literature indicates that GPs facing higher competition are more lenient when issuing sick leave certificates (ISF, 2014; Markussen and Røed, 2016; Brekke et al., 2017), provide more (intense) treatment (Iversen and Lurås, 2000; Iversen and Ma, 2011), and prescribe more drugs (Kann et al., 2010; Fogelberg, 2014; Schaumans, 2015).

The literature on competition in the hospital sector is less scarce, but shows mixed results. Among studies in regulated-price settings,[4] several investigations of the English NHS and U.S. 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; Moscelli et al., 2016).

3   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, but they share responsibility with municipalities for certain population groups (i.e., school-age children and elderly). As all residents of a given municipality belong to the same county, a change in a county council's health care policy affects all residents of the municipalities within that county.

Health care accounts for 90 percent of the county councils' expenditures. In 2013, counties' main revenue sources were a proportional income tax, central government grants, and patient fees (71, 20, and 4 percent; SALAR, 2017). Patients pay a regulated fee for visits at care facilities and part of the cost for prescribed drugs, up to an annual cap.

Primary care accounts for slightly less than 20 percent of total health care expenditures. The payment system for primary care providers varies across counties, but capitation makes up around 70-80 percent of payment in most counties. The exception is Stockholm county that had a capitation share of only 40 percent during our study period (our results are robust to excluding the municipalities in Stockholm county, results available on request). Fee-for-service makes up most of the residual. Over the past decade, it has become more common to risk-adjust the capitation

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[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.

rate, to make withdrawals from the base reimbursement when listed patients visit other providers, and to use pay-for-performance as a complementary reimbursement (Anell et al., 2018). The latter is less than 5 percent of total payments in all county councils during our study period (Ellegård et al., 2018).

Primary care is provided via group practices denoted *primary care centers*. The main staff categories are GPs and nurses, but the staff may also include other professions such as physiotherapists, occupational therapists, social workers, and cognitive therapists (Anell, 2015). Care centers typically employ 4–6 GPs. The median number of registered patients was 7,300 in 2011 (similar to British GP practices, Santos et al., 2017), though private primary care centers were usually smaller than public ones during our study period (the median private care center enrolled around 5,700 patients in 2011).

A distinguishing feature of Swedish primary care is that the county councils own and operate most primary care centers, although private provision has become increasingly common: in 2005, private providers accounted for 15 percent of total primary care expenditures, compared with 27 percent in 2013. The share of private providers varies considerably across counties: in 2013 the min-max range was 14–50 percent. The vast majority of private primary care centers are for-profit, limited liability companies. Notably, the staff employed at primary care practices are nearly always salaried, also in private practices, and are thus not affected directly by the design of the reimbursement system (small practices with self-employed GP's can be an exception, but there are few of these).

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 only an informal gatekeeper function (Anell, 2015).

4 Patient choice, reduced barriers to entry, and the quality of primary care

4.1 The Swedish patient choice reforms

Long waiting times and low responsiveness to patients are long-standing problems in Swedish primary care. There is also widespread political desire to strengthen the role of primary care relative to secondary care. Against this background, eight county councils decided to implement reforms in 2007-2009 that removed all restrictions on patients' choice of primary care center and instituted free entry for primary care centers (Anell, 2015). The reason for the emphasis on free choice and entry were likely related to the ideological preferences of the center-right local governments in these counties. Inspired by the local reforms, the national center-right wing government enacted the Act of Free Choice (SFS, 2008:962), which obliged all counties to enact similar reforms by 2010. Table 1 shows the date of reform for each county.

[Table 1 about here.]

All county reforms shared two important components. First, the reforms reduced barriers to entry, in the sense that the councils have committed to not block the establishment of new care centers that fulfil pre-specified accreditation criteria. The criteria, which are updated annually, differ across county councils, for example with respect to the set of services care centers must offer. Anell et al. (2018) 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 primary care centers to provide a relatively broad set of services. Second, patients may enrol with any primary care center in the county – providers may no longer close their list for new patients – and switch whenever they like. Although many counties formally allowed patients to switch providers even before the reform, they did not promote it actively. At the time of the reforms, county councils informed their residents about their right to choose provider and the reforms were widely covered in media. To support patients' decisions, the county councils have substantially increased the

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amount of publicly available information about providers. Such information was scarce before the reform. Since April 2010, the county councils run a joint website, `1177.se`, at which patients can find information about all primary care centers. This website is the most comprehensive source of information about primary care centers, including opening times and telephone hours, professional categories and expertise available at the primary care center, and quality ratings from a national patient survey. Notably, the provision of information *online* has by itself reduced search costs.

Switching costs have also been reduced. The switching process has become easier: choice forms are available at `1177.se`, where it is also possible to switch care center electronically, and at all primary care centers. Because providers can no longer reject patients, the switching costs facing patients that wish to enrol at providers with a previously closed list have gone from infinity to zero.

4.2   Theoretical consequences of the choice reforms

The reforms may theoretically have intensified the degree of competition facing primary care providers via three mechanisms: reduced barriers to entry, increased access to information, and reduced search and switching costs.

The reduced entry barriers had a non-negligible effect on actual entry: nationally, the number of primary care centers increased by about 20 percent after the reform. The lowered entry barriers plausibly also increased entry threats in many areas. Both actual and potential entry increases the competitive pressure on providers that want to attract new or retain existing patients. As almost all new providers were private, the increased importance of profit maximization is another reason why competition might have become more fierce.

The reforms also lead to an increasing amount of publicly available information, both about the freedom of choice and about providers. Reduced switching costs and increased access to information may increase patients' demand elasticity, thus leading to intensified competition (see, e.g., Klemperer, 1995, for a discussion on switching costs in various markets).[5]   Although it is suffi-

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[5]Public reporting of performance may also improve quality through peer comparisons by intrinsically motivated

cient that providers think that patient demand has become more elastic for competition to become more fierce, existing evidence suggest that patients do take quality information into account when choosing a provider (Kolstad and Chernew, 2009; Dixon et al., 2010; Iversen and Lurås, 2011; Santos et al., 2017; Anell et al., 2017).

The increased entry induced by the reforms may have led to better access to primary care, which may improve quality. Entry always increases access in the sense of reducing the travel time for at least some patients. However, we show in Appendix A.1 that a very large share of new care centers located themselves nearby existing centers. These indications are also in line with a report from the Swedish Competition Authority (Swedish Competition Authority, 2010) on travel time changes to the second closest care center between 2009 and 2010 (which corresponds to the reform implementation of the late adopters). Improved travel time to providers is therefore likely of limited importance for primary care quality in our context. A potentially more important aspect of access arises if newly opened providers attract staff from other markets, thus increasing staff density. In Section 6, where we compare changes to the market structure in the treatment and comparison group, we show that there was no differential change after the reforms in the density of the two most important staff categories, physicians and nurses. Together with further pieces of evidence, presented in Section 9, these results indicate that the access channel was of limited importance. In what proceeds, we therefore emphasize the competition channel.

5   Empirical strategy

Identifying the effects of increased patient choice and reduced barriers to entry on care quality is difficult, as both unobserved determinants of quality and reversed causality may bias the estimates. To tackle these issues, we exploit plausibly exogenous variation induced by the Swedish reforms in a DID analysis. The quick roll-out of the reforms prevents us from using the late-adopters

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health care personnel (Kolstad, 2013; Godager et al., 2016). Such effects seem unlikely in Swedish primary care, as information is reported at the primary care center level, making direct peer comparisons difficult.

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as a control group.[6] Instead, we construct a treatment group consisting of local markets where the reforms plausibly had stronger impact. Notably, by allowing for variation in treatment status within county councils, this strategy has the advantage of mitigating confounding related to county-council specific changes (e.g., of the reimbursement system).

To define the treatment group, we consider the market structure 6 months before the reform and calculate the average number of patients per care center if another center would enter the municipality.[7] If the number is greater than 5,500, the municipality is treated; that is, if

$$\frac{Population}{No.ofCenters + 1} > 5,500. \tag{1}$$

The idea is that areas with many patients per primary care center were more attractive to potential entrants, because it would be possible to attract a sufficient patient stock to make a normal profit in these areas. The threshold of 5,500 patients is slightly below the median patient stock of private primary care centers 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.3, we show that our results are robust to using higher and lower cutoffs, and to using a continuous treatment definition based on the variable used to construct the cutoff.

To build confidence in our estimation strategy, we show in Section 6 that post-reform entry was much more frequent in our treatment group. We cannot test directly whether the treatment group was at least as affected by increased access to information and reduced switching costs; however, these features ought to be more important if new providers enter. Further, information and switching costs are irrelevant if there is only one provider in the market, which was the case for almost half of our comparison group, but only for a third of the treatment group. Finally, survey data reported in Appendix A.2 indicate that residents in the treatment municipalities feel more informed about primary care providers than residents in the comparison group municipalities,

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[6]As our objective outcome measure, avoidable hospitalizations, pick up downstream effects of preventive care and treatment of chronic illnesses, a few months is too short a follow-up period. For the subjective measures, no county council remains untreated after the first survey wave, thus it is not possible to use a DID strategy at the county level.

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

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although the difference is not significant.

Our DID approach has important advantages over (fixed-effects) approaches that use direct measures to capture changes in 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 of changes in entry threats, information levels, or switching costs. Industrial organisation models of competition indicate that potential entry contributes to the level of competition faced by incumbents (e.g. Salop, 1979; Dixit, 1979), and more information and lower switching costs should make patients more responsive to quality differences. But more intense competition from such sources is not registered by measures based on the number of providers. Market concentration may either decrease or increase by elevated quality competition. In the latter case, a HHI would indicate that competition has decreased, despite that the cause of the change in market concentration is more competition. 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 (Tay, 2003; Kessler and Geppert, 2005).[8] Though we, too, 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.

6   Market structure

To establish that our treatment definition in Eq. (1) is sensible, we examine the post-reform entry patterns in the treatment and comparison groups. To do this, we use unique data over all primary care centers in Sweden 2005-2013, including starting and closing dates, geographic coordinates, and ownership details.

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

In December 2005, one year before the first reform, Sweden had a total of 958 care centers.[9] The pre-reform mean number of primary care centers per 10,000 residents (*Centers per capita*), was 0.93 in the 147 municipalities in the treatment group, and 1.7 in the 142 municipalities in the comparison group. Seven years later, after all county councils had implemented reforms, the number of centers had increased by 20 percent to 1,159. The entries were 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 primary care centers than 12 months before reform (upper part) and the average number of primary care centers (lower part). Entry took place rather quickly post reform in both groups, but was much more common in the treatment group. The average number of primary care centers increased by about one 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.

Notably, our treatment definition produces similar results 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 primary care center that existed 6 months pre-reform. Markets located in treatment group municipalities were clearly more exposed to entry than markets located in comparison group municipalities. 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 were some entries before the reform date.

[Figure 1 about here.]

[Table 2 about here.]

Table 2 has a similar message as Figure 1. Column (1) shows the results from a linear probability model in which the outcome variable indicates whether, during at least one month in the

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

post-reform period, the number of primary care centers in a municipality was larger than 12 months pre-reform. The coefficient on the treatment dummy is large (0.389 compared with the comparison group mean of 0.162) and highly significant. Column (2) uses a dependent variable equal to 1 if there were more primary care centers 42 months post reform, an indicator of lasting entry. The coefficient is almost as large as in column (1) and highly significant. Column (3) shows DID estimates indicating that the average number of primary care centers increased by almost one in the treatment municipalities than in the comparison group. 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 primary 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 a reform. μ_m are municipality fixed effects, θ_t, δ_q, and λ_y are month-compared-to-reform, quarter-of-the-year, and year fixed effects, and ε_{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 county level.[10]

Because we have information about subjective quality only for 12 of the 21 counties (see Section 8.1), it is important to check that the treatment definition makes sense also for this restricted sample. Columns 4–6 repeat the estimations in columns 1–3 on a sample restricted to municipalities located in these 12 counties. The results are similar to those for the full set of counties.

The higher prevalence of large urban municipalities in the treatment group partly explains why this group experienced more entry. Because equity in access to primary care is an important concern in Swedish health care policy, every municipality has at least one primary care center. This implies that populous municipalities often have relatively many patients per primary care center. Yet, smaller and more rural municipalities in the treatment group also contribute to the difference.

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[10]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 2 remain significant.

Forty-three treated municipalities had only one primary care center six months pre-reform; nineteen of these small municipalities experienced entry after the reform. In the comparison group, only six of the 77 pre-reform monopolies experienced entry. This corresponds to relative frequencies of 44 percent (treatment) and 8 percent (comparison).

The increased entry in the treatment group did not imply increased access to medical staff. Figure 2 shows that the physician and nurse densities developed similarly over time in both groups. Although the occupation statistics does not discriminate between GPs and physicians with other medical specialities, it seems very unlikely that physicians would switch from secondary to primary care to staff the new primary care centers. That is, the existing core primary care workforce was spread out over more units in the treatment group. Together with the evidence that new care centers to large a extent located themselves close to existing centers (discussed in Section 4.2), this development makes changes in access to care an unlikely explanation of differences between the treatment and comparison groups.

[Figure 2 about here.]

7 Competition effects on objective quality: ACSC hospitalizations

7.1 Data

Avoidable hospitalizations are defined as hospitalizations with certain diagnoses (*ambulatory care sensitive conditions* – ACSC), for which well-functioning primary care could 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,[11] but

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[11]The following chronic diagnoses are included in the measure: anaemia, asthma, diabetes, heart failure, high blood pressure, COPD, and ischaemic 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).

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 1999–2013, as the classification of diseases changed in 1998 when ICD-10 was implemented.

ACSC rate is a municipality's monthly number of avoidable hospitalizations per 10,000 residents. The data is aggregated at municipality level, based on patients' municipality of residence, as we do not have access to less aggregated data. Using data aggregated at market level, in this case the municipality level, mitigates problems due to sorting of patients between primary care centers. The municipality level represents a conservative choice, as the relevant primary care market is smaller for most patients.[12]

[Table 3 about here.]

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 thousand SEK (*income*), percentage share of 16–74 year-olds with at most primary education (*primary*), percent of children <10 years (*children*), and percent of residents ≥ 65 years of age (*elderly*). We also include the squared values of these covariates in the estimations. The covariates capture potential differences in patient casemix development, and may increase the precision of the estimates. It is unlikely that these covariates were directly affected by the reforms.

Table 3 shows pre-reform descriptive statistics for a period from 18 to 7 months pre-reform. The average population size and density are higher in the treatment group. The considerably lower medians of these variables (31,349 and 60.3, respectively) indicate that the averages are affected by a few very large municipalities; that is, many treatment municipalities are rather small and dispersed in terms of population. However, some size difference between treatment and comparison is inevitable, given our treatment definition in combination with the "at least one primary care center per municipality" policy seemingly used by the counties.

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[12]In one of the most densely populated counties, Skåne, 90 percent of patients are enrolled at a primary care center in their municipality of residence (Anell et al., 2017). This suggests that the risk of bias due to patient sorting across municipality borders is low also for less dense regions.

7.2   Estimation

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

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

where T_{mt} is a treatment indicator, X_{mt} is a vector including the municipality-and-year covariates (in levels and as squares), μ_m are municipality fixed effects, κ_m are municipality-specific coefficients on the linear pre-reform trend variable t, ε_{mt} is an error term, and θ_t, δ_q, and λ_y are month-compared-to-reform, quarter-of-the-year, and year fixed effects, respectively.[13]

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 county level whenever computationally possible, and at 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 available on request).

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 pre-reform (linear and non-linear) trend differences between the treatment and comparison groups (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 3 along with their squares. For ease of exposition, we present the estimates from this model graphically (Fig. 3).

Thereafter, we restrict the treatment effect to be constant – i.e., β becomes a time-invariant

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[13]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.

scalar. Because the initial estimation reveals very small pre-trend differences, we let the treatment indicator take the value of 1 from the reform month and onward. We sequentially add municipality-specific linear trends $\kappa_m t$ and covariates X_{mt} to a baseline model without covariates. Finally, we allow for a sluggish response to the reforms by estimating a "'donut'" specification, in which treatment starts 6 months post reform implementation (rather than at $t = 0$) and the pre-reform period ends 6 months pre-reform. This specification, which in practice adds a dummy variable for the 12-month 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.

7.3 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. The red vertical lines mark the implementation year.

[Figure 3 about here.]

The figure does not indicate systematic pre-trend differences between the treatment and comparison groups: there are positive as well as negative "placebo" estimates, and all of them are small and statistically insignificant. The confidence interval (dashed lines) use standard errors clustered by municipality instead of county, as the covariance matrix is otherwise not of full rank. This choice may imply that the interval is too narrow, but as the figure clearly shows, the coefficients are far from significant anyway. 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, all estimates are statistically insignificant and small. For example, the largest (i.e., the last) estimate corresponds to 0.06 of a standard deviation ($p = 0.48$).

[Table 4 about here.]

Panel A of Table 4 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.3.1, we show that the results are similar for other cutoff levels of the treatment definition, and when we use a continuous treatment definition.

Panel B shows corresponding estimates from regressions without population weights. All treatment coefficients are very small but positive, indicating quality reductions. The qualitative difference between the results with and without population weights for the *ACSC rate* indicates that larger municipalities, which are more influential in the weighted estimations, experienced larger quality improvements. However, it is not the case that the average effect hides two very large effects of opposing sign: we obtain estimates that are very close to zero and statistically insignificant when excluding the 5 percent largest and smallest municipalities from the estimation sample. These results are available in Appendix A.5.

Our linear specification may hide significant non-linear effects. In particular, one might suspect that the effect of the reforms on competition would be greatest in municipalities where reform-driven entry break up a local monopoly. In Appendix A.6 we show that this does not seem to be case for our objective measure. The average effect on the *ACSC rate* does not hide a stronger quality response in monopolies than in markets with multiple primary care centers.

In sum, the small and statistically insignificant effect on avoidable hospitalizations is a robust result in all our specifications. We therefore conclude that there is no discernible impact on this outcome measure. In Appendix A.4, we repeat the baseline analysis for a related outcome variable, the rate of unplanned inpatient care episodes. The results are in line with those for the ACSC rate.

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8 Subjective measures of primary care center quality

8.1 Data

The source of our subjective quality data consists of 2009, 2011, and 2013 waves of a national patient satisfaction survey carried out by the Swedish Association of Local Authorities and Regions. All respondents had recently made a visit to the primary care center they were asked to rate. The survey covers all care centers in participating counties. 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 4.1). Thus, the analysis of subjective quality is restricted to the 12 counties (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. The largest municipality in the sample had approximately 134,000 residents, which means that the largest urban areas are not included.

We define the treatment group in the same way as in the previous analyses. This yields 48 (75) municipalities in the treatment (comparison) group. The average municipality-level response rate is 56 percent (stable across survey waves), though the response rates differ between survey questions.

We construct four dummy variables capturing subjective quality : *Phone access*, *Waiting times*, *Overall impression*, and *Recommendation* (see the upper part of Table 5 for definitions).[14] For all dummy variables, the value 1 indicates better quality. Figure 4 displays, by treatment status and survey wave, the average share of observations coded as 1 for each 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. Notably, the average shares are consistently quite far from the theoretical max of 1, suggesting that ceiling effects are not a concern.

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[14]The original Swedish wording of the questions are (authors' translation in parentheses) "Hur upplever du mottagningens tillgänglighet per telefon?" (In your experience, how accessible is the practice by phone?), "Hur länge fick du vänta på ditt besök?" (How long did you wait for your visit?), "Hur värderar du som helhet den vård/behandling du fick?" (What is your overall assessment of the treatment you received?), and "Skulle du rekommendera den här mottagningen till andra?" (Would you recommend this practice to others?).

We also have individual-level data on respondents' self-rated health and previous experiences with the primary care center (due to secrecy agreements, we were not provided access to other background variables, such as age and gender). Table 5 contains variable definitions and descriptive statistics for the pre-reform survey wave.

[Table 5 about here.]

[Figure 4 about here.]

8.2 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, μ_m are municipality fixed effects, λ_t are year (survey wave) fixed effects, and ε_{imt} is an error term. As the model only includes binary variables, we estimate the equation by using a linear probability model (LPM).

The probability of an individual being included in the surveys differs, as different numbers of individuals were sampled in different municipalities/primary 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 surveys sent out divided by the municipality's population size). Given the small number of counties in these estimations, we cluster standard errors at the municipality level (using county level yields smaller standard errors in all cases). The results are similar when using the wild cluster bootstrap at the county level (not shown).

8.3 Results

Table 6 presents estimates for the four subjective quality measures. Panel A shows the results of specifications excluding individual-level covariates, Panel B shows specifications including co-

variates,[15] 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.

The results are robust to varying the cutoff of the treatment definition, and have the same sign when we use a continuous treatment definition. The latter specification increases precision, and the estimates for phone access, overall impression, and recommendation are significant ($p < 0.1$, $p < 0.01$, and $p < 0.05$ respectively; see Appendix A.3.2). The size of the effect is similar though, moving a municipality from the comparison group mean to the treatment mean yields a percentage point increase that is very close to the magnitudes in our baseline specification. The increased precision may therefore stem from imposing more structure on the relationship between treatment and the outcome variables, and we prefer the baseline estimates for this reason.

[Table 6 about here.]

Notably, Panel B shows that all subjective quality measures are strongly positively correlated with self-rated health. This raises some concerns relating to the interpretation of our results. If the patient casemix 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 differences in the patient casemix changes when estimating Eq. (4) using respondents' self-reported health and previous primary care experiences as dependent variables (see Appendix A.7). That is, our results are not driven by treatment group primary care centers primarily catering to the demands of healthier patients (one type of cream-skimming).[16]

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

[16]As the survey only targeted patients who had actually visited the primary care center, we cannot rule out cream-

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The reported estimations contrast very satisfied respondents with respondents who are just satisfied or dissatisfied with their primary care center. If we instead use the dissatisfied group as reference category (this cannot be done for *Waiting times*); that is, by transferring respondents who rate phone access or overall impression as "good", as well as respondents "partly" willing to recommend their care center, to the high-quality category, the estimates are almost half as large (results not shown). The effects are still marginally significant ($p < 0.1$) for *Overall impression* and *Recommendation*. It should be noted that this definition removes a lot of variation, as few patients rate their primary care center as "OK/poor" or do not want to recommend it at all.

Compared to the *ACSC rate* estimations, potential heterogeneous effects depending on population size seem less likely 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 that sample (Appendix A.5). The precision is lower in these estimations, but this is likely linked to the loss of almost 20,000 observations. The treatment group in the patient satisfaction sample contains few pre-reform monopolies, and therefore we have limited power to detect heterogeneity between initial monopolies and initial non-monopolies.

9 Mechanisms and limitations

This section discusses explanations and interpretations of our results, as well as some limitations of our study.

9.1 Mechanisms

As noted in Section 4.2, entry of new providers may both affect the degree of competition and patients' access to primary care. Given the prevalence of entry in our treatment group, both mechanisms may in principle lie behind our estimated effects. Disentangling the causal effects of competition and access to care is not trivial; in fact, we are not aware of any study that separates the

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skimming with regards to the set of registered patients; e.g., we cannot rule out that treated primary care centers managed to enroll very healthy patients, whose probability of making a visit was close to zero.

effects of competition and access on health care quality. However, some key results lead us to believe that the access mechanism was of limited importance in our case.

First, it is worth to elaborate on how entry may affect access to care. Entry always increases access in the trivial sense that there are more care centers than before. However, if the supply of medical professionals in the market remains constant, then so does patients' access to medical professionals. As we showed in Section 6, the per capita number of physician and nurses developed similarly in the treatment and comparison group. Thus, increased access in this sense does not explain differences between the two groups.

Another way in which entry may increase access is through reduced travel time to providers. Two questions arise: *i)* is the travel time to care centers related to primary care quality, as measured in our study?; *ii)* has the establishment of new care centers reduced travel times for a sufficient number of patients to affect our quality measures? For our subjective quality measures, the theoretical link with travel time is tenuous: the patient survey questions concern patients' experiences during their actual contacts with the care center, not their experience of travelling to the center. Although we cannot rule out that patients' experiences are coloured by the travel time to the center, it seems like a stretch. We further note that the measure that is most directly linked to access to care, *Waiting times*, did not improve at all.

Travel time is more plausibly linked to our objective quality measure: long travel times may induce ACSC patients to avoid seeking preventive care, or skimp on visits related to treatment of chronic diseases. However, it appears unlikely that the new entries reduced travel times sufficiently to have a measurable impact on the ACSC rate. As discussed in Section 4.2, the new care centers were mainly located in the proximity of existing providers.

Additional analyses, reported in Appendix A.8 give further support for our assertion that the access channel is of limited importance. In these analyses, we exclude all municipalities where the number of care centers increased after the reforms. Within the remaining subset of municipalities, we contrast municipalities in which there was only one care center (monopolies) with municipalities with at least two primary care centers (non-monopolies). Whereas the competitive pressure on

non-monopolies potentially increased – as more information and lower switching costs increased the elasticity of demand – the monopolies were not affected, as their patients had no alternative provider to switch to. Importantly, quality differences between non-monopolies and monopolies are not attributable to increased access to care: by definition, the travel time to nearest care center did not decrease, and we show that development of the staff density was very similar in monopolies and non-monopolies.

These estimations give very similar results as our baseline models for both objective and subjective measures. Although this sample is a selected one and the association is not necessarily causal, the results suggest that reduced barriers to entry was not the only important component of the reform. They further indicate that the baseline estimates are *not* the net result of a negative competition effect and a positive access effect.[17] Rather, the results suggest that increased competition explains the modest improvements of the subjective quality measures in our baseline estimations, and that competition does not affect the ACSC rate.

The previous specification begs the question whether municipalities that were affected by both the choice and entry components of the reform experienced larger effects on quality. In this case, our baseline model underestimates the reform effect, as it assigns some municipalities with no entry to the treatment group (and vice versa). In Appendix A.9, we contrast municipalities with and without entry in a DID analysis, and use our baseline treatment definition (Eq. 1) as an instrumental variable for entry. Each of these strategies does yield somewhat larger estimates compared to our baseline specifications (for both objective and subjective measures). The magnitudes of the effects are still modest though, and they are not consistently significant. Because these estimations rely on stronger assumptions, we prefer our baseline model.

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[17]If competition from entry affects care centers differently compared to competition from non-entry related sources, this might still be the case. We are not aware of any theoretical model predicting such differential effects.

9.2 Limitations

Some remaining limitations of our study are noteworthy. First, our estimates may be biased if there are unobserved and heterogeneous changes in the patient casemix of the treatment and comparison group. For example, if healthier population groups tend to migrate to the, on average, larger and more urban municipalities in our treatment group, then the beneficial effects of the reforms might be overstated. Such trends are difficult to completely rule out, but given that we find only small 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.

Second, our treatment definition might understate the effects of the reforms if some comparison municipalities were more affected than some of municipalities in the treatment group. We do however find similar results with a continuous measure, which should be less sensitive to this type of measurement error (see Sections 7.3 and 8.3). This measurement error bias is a concern in all studies that do not perfectly measure the degree of competition experienced by providers (but instead rely on proxies based on market shares, number of firms, distances, etc.).

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

10 Concluding remarks

We find that municipalities that were more affected by reforms increasing patient choice and reducing barriers to entry experienced modest improvements of patients' overall impressions of care quality, but no significant improvements of the avoidable hospitalization rate or (patient satisfac-

tion with) waiting times. We present several pieces of evidence suggesting that the reforms had limited effect on patients' access to care, leaving increased competition as the most likely mechanism behind the results.

Our results are in line with the small literature examining the causal effects of competition on primary care quality in markets with regulated prices (Gravelle et al., 2018). Overall, these 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 target quality dimensions that are easily observable and that are relevant to a large (or more profitable) share of the population. In line with these incentives, we find no significant impacts on the *ACSC rate*, which is likely an irrelevant choice parameter for the large share of the general population that does not suffer from the included ambulatory care sensitive conditions. It is further difficult for patients to observe. There is a general lack of available objective indicators of how good Swedish primary 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) primary care centers in more competitive environments. Instead, they are more likely to cater to the larger set of patients who are interested in other quality dimensions.

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

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ligible. Our results are therefore in line with studies finding that more intense competition tends to increase opportunistic behaviour (Iversen and Lurås, 2000; Kann et al., 2010; Iversen and Ma, 2011; Fogelberg, 2014; ISF, 2014; Schaumans, 2015; Markussen and Røed, 2016; Brekke et al., 2017).

There is reason 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 primary care center if they are provided comparative information about nearby primary care centers by postal mail (Anell et al., 2017). 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.

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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 Götaland
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).

Table 2: 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 counties (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. We use the Stata command *xtivreg2* to partial out the time effects in our estimations. 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 primary care centers than 12 months 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 primary care centers (*Centers*). Standard errors (in parentheses) are clustered by county council in columns 1–3, and by municipality in columns 4–6. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: 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 (cases/10,000 residents)	15.5	4.1	4.9	36.7	18.2	6.9	1.9	54.5
Population size (number of residents)	51,454	81,245	11,126	795,163	11,154	7,253	2,500	62,388
Pop density (residents per km^2)	226.8	590.7	1.2	4,228.2	31.6	105.1	0.2	1161.4
Mean *income* (1,000 SEK)	239.2	27.4	203.0	370.7	216.7	24.0	184.1	442.6
Proportion with *primary* education (%)	23.9	4.1	12.6	36.7	28.1	3.7	11.2	37.4
Proportion of *children* ($<$ 10 years, %)	11.2	1.6	8.6	17.3	9.8	1.4	7.1	15.8
Proportion of *elderly* ($\geq$ 65 years, %)	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 Swedish municipality. *ACSC rate* is based on monthly municipality-level data. All other variables are based on yearly municipality-level data. Sources: National Board of Health and Welfare (Socialstyrelsen), Statistics Sweden.

Table 4: 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
Counties	21	21	21	21

Note: Dependent variable: *ACSC rate*. *Treatment* = 1 if there were more than 5,500 patients per primary 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 includes a dummy for the 6 months pre and post reform (a "donut hole"). The estimation sample includes all 289 municipalities and covers the period from 90 months pre to 42 months post reform. Standard errors clustered by county in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Definitions of subjective quality measures and descriptive statistics

Panel A: Dependent variables

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

Panel B: Individual background characteristics

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

Panel C: Descriptive statistics

	Treatment			Comparison		
	(1)	(2)	(3)	(4)	(5)	(6)
Variable	Mean	SD	Obs	Mean	SD	Obs
Phone access	0.497	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: Panel C displays individual-level data from the 2009 wave of the national patient satisfaction survey conducted by the Swedish Association of Local Authorities and Regions. Included counties: Blekinge, Dalarna, Gotland, Gävleborg, Jämtland, Jönköping, Kalmar, SÖdermanland, Värmland, Västerbotten, Västernorrland, Örebro.

Table 6: 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
Counties	12	12	12	12
	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
Counties	12	12	12	12
	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
Counties	12	12	12	12

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, i.e., 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; and *Recommendation*: 0.714. The differences in total observations reflect differences in response rates across survey questions. Panel A excludes individual-level covariates, Panels B and C include covariates, Panel C does not use sample weights.
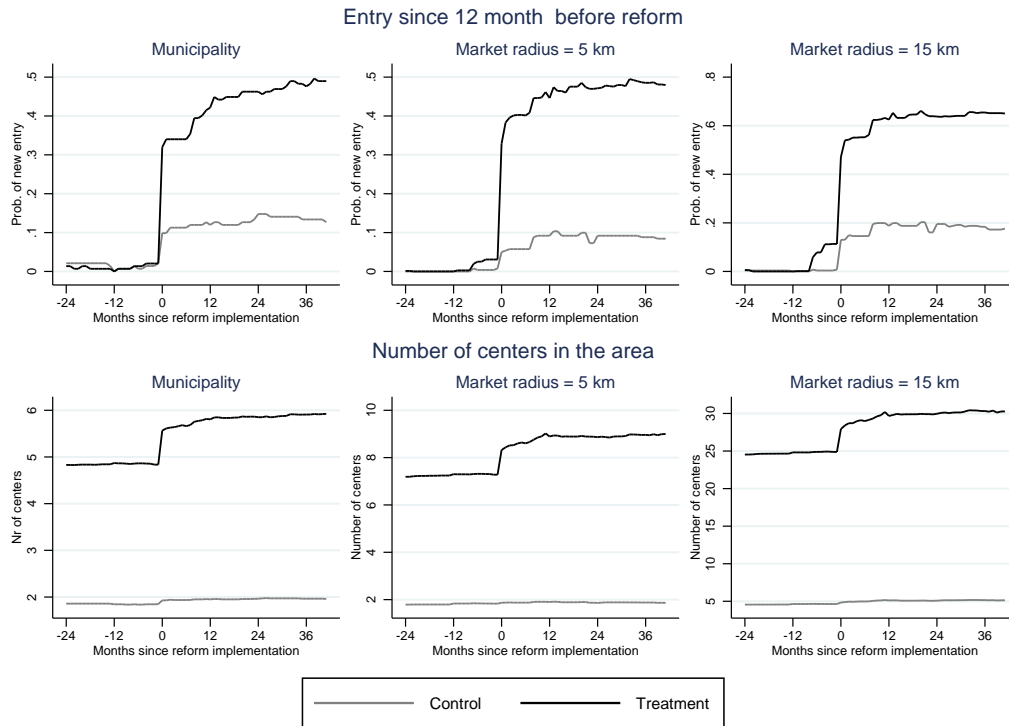
Figure 1: The lines display the coefficients from regressing treatment and comparison group dummies on i) (upper panel) an indicator variable equal to 1 if there are more primary care centers in a certain month than 12 months before a reform and ii) (lower panel) the number of primary care centers in 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 primary care center that existed 6 months before the respective county launched the reform.
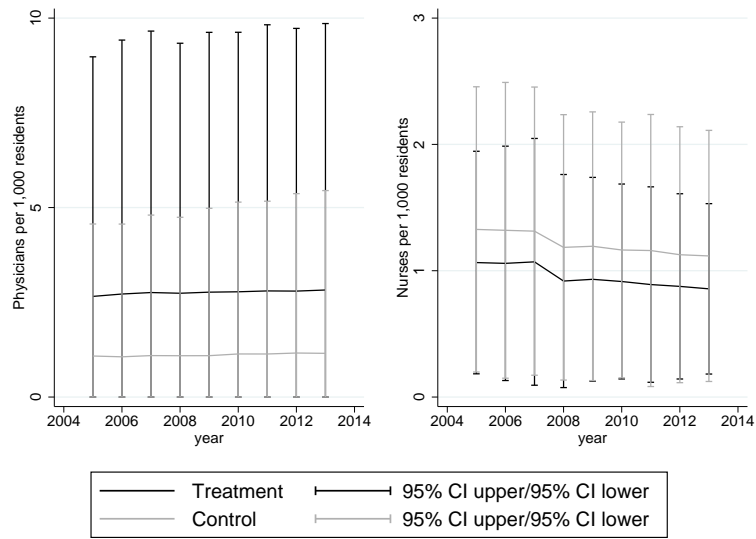
Figure 2: The lines display the means and confidence intervals for the physician and nurse densities (employed physicians/district nurses per 1,000 residents) by treatment and control group.
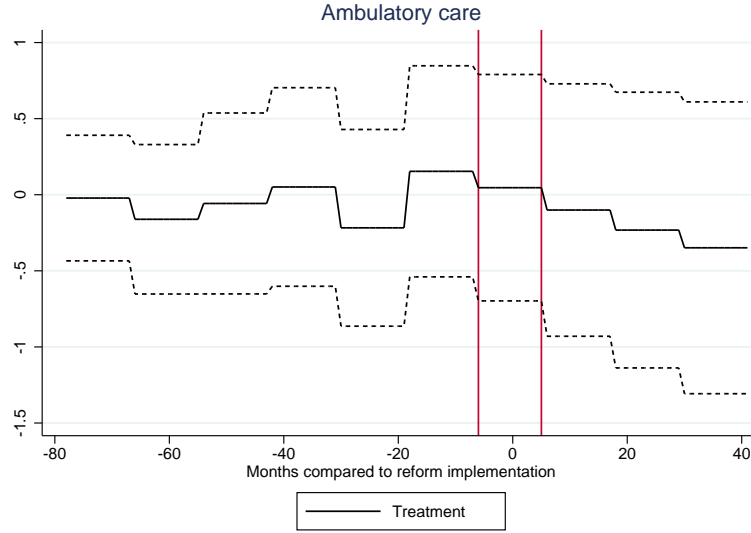
Figure 3: Differences in avoidable hospitalization rates (*ACSC rate*) of the treatment and comparison groups. The lines display the $\beta_{\bar{t}}$ coefficients (solid) and confidence intervals (dashed) from Eq. (3) including co-variates. The first 12-month period is used as reference period and is therefore excluded from the figure. Standard errors used to calculate the confidence interval are clustered by municipality. The pre-reform mean (standard deviation) of *ACSC rate* is 17.1 (6.1).

Figure 4: The graphs display, for each of the four dummy outcome variables, the share of respondents whose answers are coded as 1 (phone access ≥ very g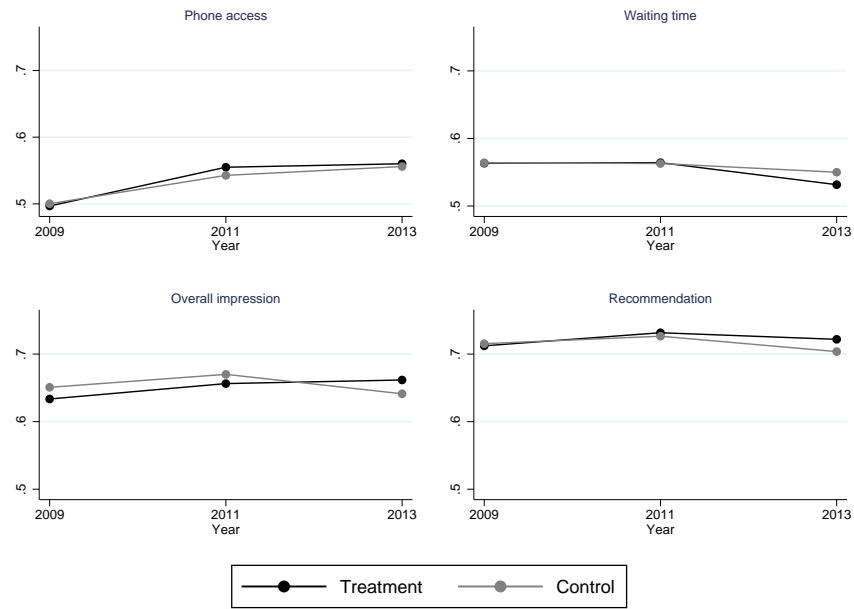ood, waiting time ≤ 2 days, overall impression ≥ very good, would completely recommend the primary care center to others, respectively). Treatment (black line) and comparison group (gray line). Note that only respondents from 12 counties are included.

A   Appendix

A.1   Location of primary care centers

This section examines the location of new entrants, and in particular whether they are located close to existing care centers. Figure A.1 display the location of care centers established before the reform (the circles) and of care centers established after the reforms (the triangles) at the end of 2013. Note that some new entrants are located almost exactly at the site of the older centers, and that the circles and triangles are overlapping in these cases.

The map reveals a clear pattern: new entrants overwhelmingly located themselves in areas with existing care centers. There is also a concentration of entry in markets in and around the three largest cities (Stockholm, Göteborg, and Malmö), and in other urban areas. Due to the resolution of the map, it is however difficult to make out more precisely how close to existing centers new entrants are located. In Figures A.2–A.4, we therefore take a closer look at the three most populous counties: Stockholm, Västra Götaland (where Göteborg is located), and Skåne (where Malmö is located). There are only 1-3 examples of new entrants being located around 10 km from an already existing center in each of these counties. As people in these areas can be expected to own cars, the potential reduction in travel times, as well as the share of patients affected, seem very small.

This evidence for 2013 is in line with the small travel time reductions reported by the Swedish Competition Authority in 2010 (Swedish Competition Authority, 2010). We therefore believe that this dimension of access to care was not greatly affected by the reforms. In turn, it seems like an unlikely explanation of our results.

Figure A.1: Care centers in Sweden on December 31, 2013. Care centers established after the reform date in their county council are represented by triangles, and care centers established pre-reform are represented by circles.

A.1
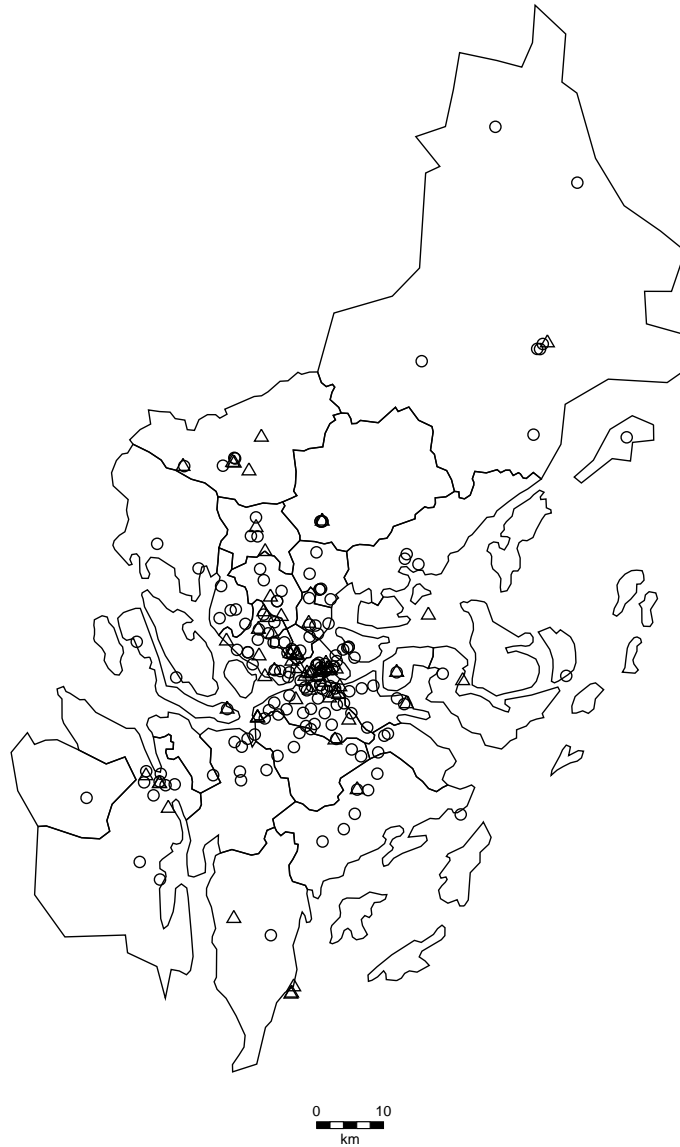
Figure A.2: Care centers in Stockholm County on December 31, 2013. Care centers established after the reform date in their county council are represented by triangles, and care centers established pre-reform are represented by circles.

Figure A.3: Care centers in Västra Götaland County on December 31, 2013. Care centers established after the reform date in their county council are represented by triangles, and care centers established pre-reform are represented by circles.
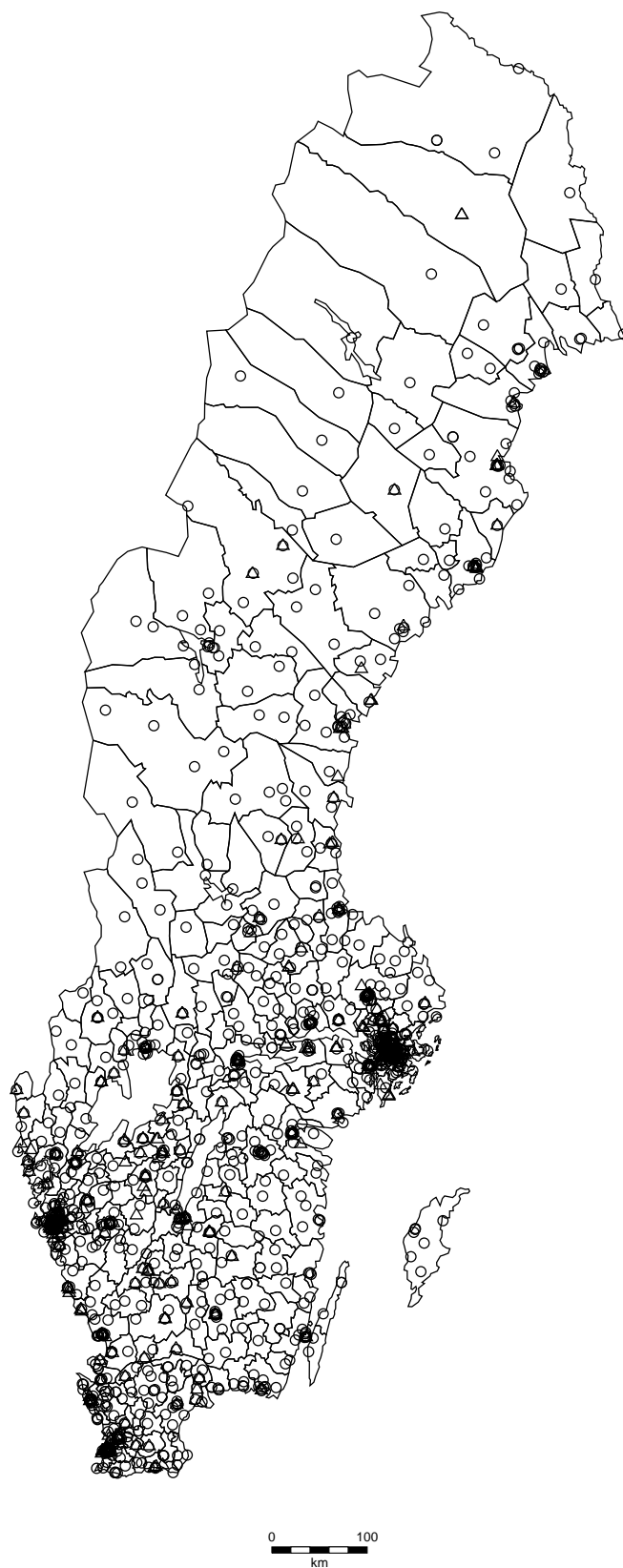
Figure A.4: Care centers in Skåne County on December 31, 2013. Care centers established after the reform date in their county council are represented by black triangles, and care centers established pre-reform are represented by circles.
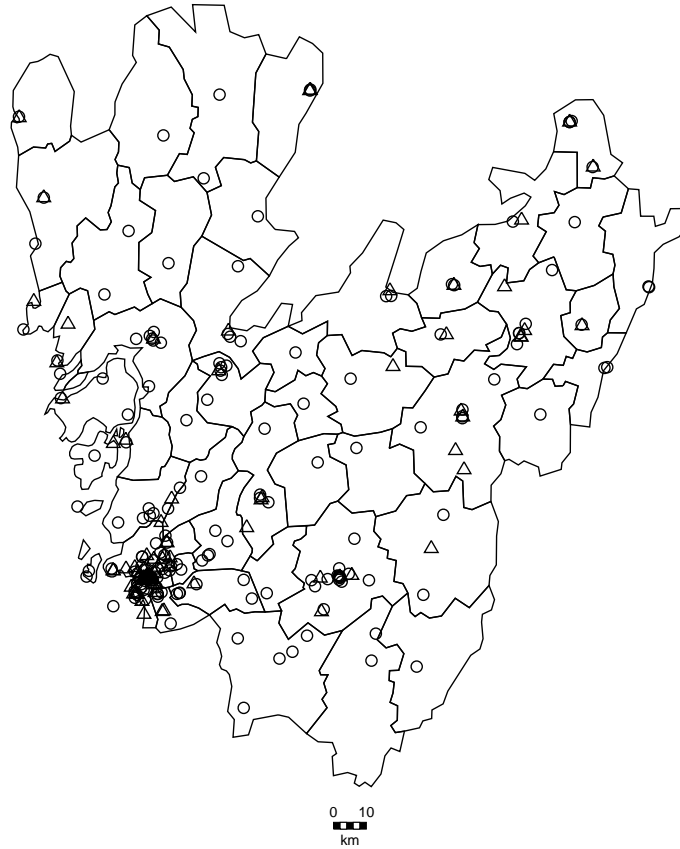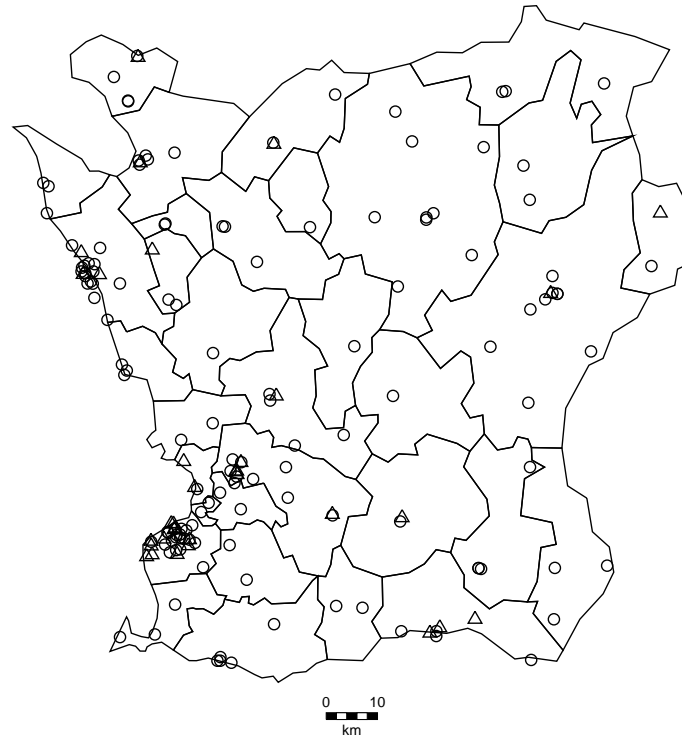
A.2 Perceptions of being informed

Table A.1 reports results from LPM models examining differences in the degree to which residents feel informed about the choice of care center. The dependent variable is an indicator equal to 1 if the respondent answered "yes'" when asked about whether he/she felt he/she had been sufficiently informed to actively choose a care center. (The original Swedish wording was: "Tycker du att du har fått tillräckligt med information för att kunna göra ett aktivt val av vårdcentral?"). The measure is taken from a nationwide online survey carried out by the Swedish Competition Authority in October 2011, which included 2,029 respondents and aimed to be representative of the population 18–75 years of age in all counties (Swedish Competition Authority, 2012). Column (1) includes only the treatment indicator and county fixed effects for the full sample. Column (2) adds respondent covariates from the survey (indicators for gender, age group, and number of visits). Columns (3) and (4) mirror columns (1) and (2), respectively, but use 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 patients in 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 with the comparison group's mean frequencies of 58 and 56 percent in the full and restricted sample, respectively. 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 more in the treatment group than in the comparison group in some respects, and less in others.

A.3 Sensitivity to treatment cutoff

A.3.1 Ambulatory care

Our treatment group consists of municipalities with a pre-reform number of residents per care center (plus 1) higher than 5,500. Below we first check whether our results are sensitive to raising

Table A.1: Perceptions of being informed

	(1) LPM	(2) LPM	(3) LPM	(4) 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

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 primary care center. All specifications include county fixed effects. Standard errors clustered by county in parentheses in column (1) and (2). *** p<0.01, ** p<0.05, * p<0.1.

the cutoff to 6,000 and lowering it to 5,000, and then to using a continuous treatment definition. With the higher cutoff, 21 municipalities formerly in the treatment group switch to the comparison group. With the lower cutoff, 28 municipalities formerly in the comparison group switch to the treatment group.

Figure A.5, which is analogous to Figure 3, indicates no substantial or significant effects of either a higher (upper part of figure) or a lower cutoff on the ACSC rate. There is a slight negative pre-trend with the lower cutoff, i.e., when some former comparison municipalities switch to the treatment group, though no pre-trend estimates are significant. As for the higher treatment cutoff, the pre-reform trends look even better than with our baseline definition. Indeed, when scrutinising the data, we find that most of the market structure impacts shown in Section 6 derive from municipalities that remain in the treatment group even with the higher cutoff.

Table A.2 shows estimates analogous to those in Table 4. 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 no coefficient is statistically significant. Thus, also with other cutoffs, a null effect is the most reasonable interpretation.

Table A.3 displays the results when we instead of defining a treatmnet and control group use a continuous treatment definition. The treatment variables is defined in a corresponding way as the earlier treatment indicators: the municipal population in thousands of inhabitants divided by the number of primary care centers in the municipality plus one. The four specifications in Table A.3 correspond otherwise to the ones used in our baseline specifications (e.g., in Table 4).

The signs of the coefficients are the same as in our baseline results; that is, the negative coefficients indicate improvements of the *ACSC rate*. The treatment-coefficient is significant in column (2), insignificant in column (1) and (3), and marginally significant in column (4). To compare the magnitudes to the baseline coefficients, we consider the thought experiment of moving an individual from the weighted comparison group mean of the treatment variable (3.84) to the weighted treatment group mean (8.08). Using the estimate of the treatment effect in column (3) would result in a reduction of the *ACSC rate* of 0.160, which is 0.026 of the joint pre-reform standard devia-

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

	Panel A: Higher cutoff			
	(1)	(2)	(3)	(4)
	Baseline	Linear trends	Covariates	Donut
Treatment	-0.189	-0.190	0.0263	0.0182
	(0.281)	(0.230)	(0.267)	(0.239)

	Panel B: Lower cutoff			
	(1)	(2)	(3)	(4)
	Baseline	Linear trends	Covariates	Donut
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
Counties	21	21	21	21

Note: The table shows coefficients from estimations contrasting the groups with average low and high number of patients per primary care center with *ACSC rate* as dependent variable. The cutoffs are less than one primary 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 in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.
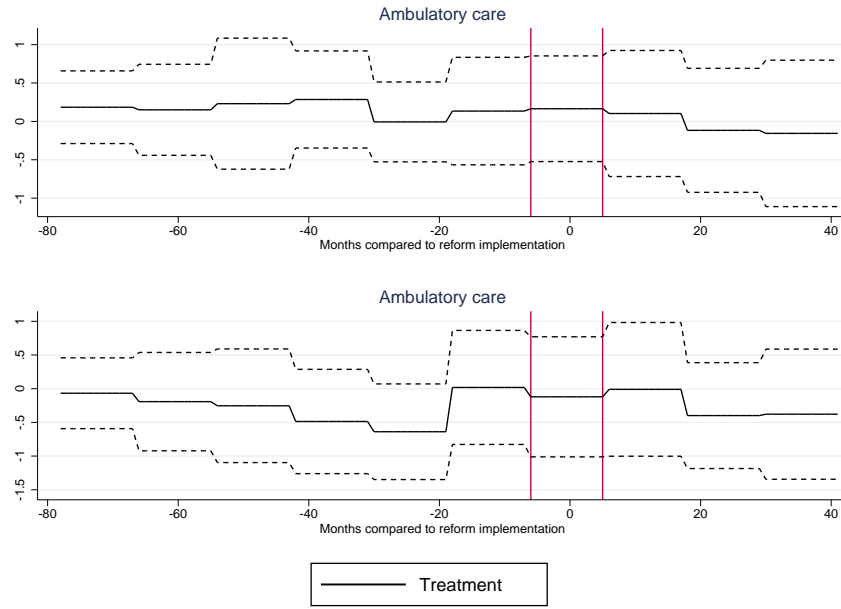
Figure A.5: The lines display the $\beta_{\bar{t}}$ coefficients (solid) and confidence intervals (dashed) from Eq. (3) excluding covariates and municipality-specific linear trends. The first 12-month period is used as reference period and is therefore excluded from the figure. Standard errors are clustered by county. The pre-reform mean (standard deviation) of *ACSC rate* is 17.1 (6.1). **Upper panel:** higher treatment cutoff ($> 6,000$ residents per primary care center). **Lower panel:** lower treatment cutoff($> 5,000$ residents per primary care center).

tion. Using the column (4) estimate yields similar results, as this estimate is close to the one in column (3). The magnitude of the treatment effect is therefore small and very much in line with the baseline specifications.

A.3.2 Subjective measures

When we raise the cutoff for being classified as treated to at least 6,000 patients per primary care center, 10 municipalities switch from treatment to comparison in the subjective measure sample. When we lower the cutoff to at least 5,000 patients per primary care center, 17 change from comparison to treatment. As shown in Table A.4, neither of these changes has much impact on the results. The only notable difference to Table 6 is that the effect for *Recommendation* becomes insignificant when we use the lower cutoff.

Table A.5 contains the estimates when we use the same continuous treatment definition as in

Table A.3: Objective quality: continuous treatment definition

	(1) Baseline	(2) Linear trends	(3) Covariates	(4) Donut
Continuous treatment	-0.0424	-0.0794***	-0.0305	-0.0300*
	(0.0385)	(0.0217)	(0.0267)	(0.0165)
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
Counties	21	21	21	21

Note: The table shows coefficients from estimations with a continuous treatment variable (the municipal population divided by the number of primary care centers in the municipality plus one) interacted with a reform dummy and the *ACSC rate* as the dependent variable. All specifications include municipality, year, quarter, and month-to-reform fixed effects. Column (1) includes no other variables. Column (2) adds municipal linear trends. Column (3) adds both linear trends and covariates. Column (4) contains the same specification as column (3) but The sample covers the period starting 42 months before reform implementation to 42 months after. Standard errors clustered by county in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.3. The specifications are otherwise the same as in Panel B of Table 6 (i.e., including covariates). The estimates have the same sign as in our baseline estimation but are more precise. The estimates are significant for *Phone access*, *Overall impression*, and *Recommendation* ($p < 0.1$, $p < 0.01$, and $p < 0.05$ respectively). The size of the effect is similar though, moving a municipality from the weighted control group mean (4.32) to the weighted treatment mean (8.60) yields percentage point increases of very similar magnitude as in our baseline specification. They are 2.2 percentage points for *Phone access*, -0.5 for *Waiting times*, 2.8 for *Overall impression*, and 1.8 for *Recommendation*. The increased precision may therefore stem from imposing more structure on the relationship between the treatment and the outcome variables, and we prefer the baseline estimates for this reason.

A.4 Unplanned inpatient care

At the municipality and month level, the number of ACSC hospitalisations is rather small, and the variable is relatively noisy. We therefore use another measure, *Unplanned inpatient care*, to

A.10

Table A.4: Subjective quality: sensitivity to treatment cutoff

	(1) Phone access	(2) Waiting times	(3) Overall impression	(4) Recommendation
	Panel A: Higher cutoff			
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
Counties	12	12	12	12
	Panel B: Lower cutoff			
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
Counties	12	12	12	12

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 cutoffs for treatment; in Panel A, the cutoff is at least 6,000 residents per primary care center, in Panel B, the cutoff 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; and *Recommendation*: 0.714. The differences in total observations reflect differences in response rates across survey questions.

Table A.5: Subjective quality: continuous treatment definition

	(1) Phone access	(2) Waiting times	(3) Overall impression	(4) Recommendation
Continuous treatment	0.00510*	-0.00125	0.00641***	0.00416**
	(0.00291)	(0.00195)	(0.00139)	(0.00192)
Observations	88,871	86,609	114,953	114,465
Municipalities	123	123	123	123
Counties	12	12	12	12

Note: Standard errors clustered by municipality in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The table shows coefficients from estimations with a continuous treatment variable (the municipal population divided by the number of primary care centers in the municipality plus one) interacted with a reform dummy. Municipality and survey wave fixed effects and individual covariates are included in all estimations. The differences in total observations reflect differences in response rates across survey questions.

check robustness. This variable measures the number of acute inpatient care episodes per 10,000 residents (see Table A.6 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 though: 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 to the national patient register deteriorated substantially for one county during a 6 month period.

Table A.6: Unplanned inpatient care pre-reform

| | Treatment | | | | Comparison | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
	Mean	SD	Min	Max	Mean	SD	Min	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: Cases of unplanned inpatient episodes per 10,000 residents. Pre-reform = the period 18 to 7 months before a reform. Monthly municipality-level data. Source: National Board of Health and Welfare.

Figure A.6 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 starts well before the implementation of reforms. It therefore seems highly tenuous to attribute the improvements to the reforms. None of the yearly treatment or placebo effects are significantly different from zero, and most are small. The confidence interval (dashed lines) use standard errors clustered by municipality instead of county. 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 shows, the coefficients are not significant anyway.

Table A.7 displays similar estimations as the baseline estimations for ACSC (Table 4) 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, compared with 0.03 for the ACSC rate). However, the esti-

mate is still relatively modest, indicating little effect on objective quality measured by unplanned hospital admissions.
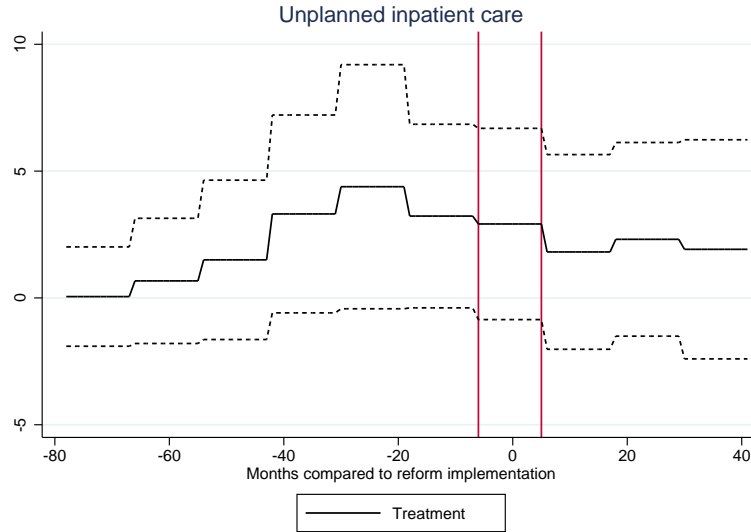

Figure A.6: The lines display the $\beta_{\bar{t}}$ coefficients from regressions using a variant of Eq. 2 on a period from 90 months before a reform is implemented to 42 months after. The first 12-month period is used as reference period and is therefore excluded from the figure. There are six placebo effects before the reforms are implemented (month 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 are 101.8 and 23.3, respectively. Standard errors used to construct confidence intervals are clustered on municipality.

A.5 Heterogeneity over population size

The differences in the results from the weighted and unweighted regressions calls for examination of the influence of very large and very 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 municipalities from the estimation samples. For the objective measure (the ACSC rate), this means that we exclude 30 of the 289 municipalities 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

Table A.7: 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
Counties	21	21	21	21

Note: The table shows coefficients from estimations contrasting the groups with a high and low pre-reform number of patients per primary 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 from 90 months before reform implementation to 42 months after. The joint treatment and comparison group mean and standard deviation in the pre-reform period are 101.8 and 23.3. Standard errors clustered by county in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

yields overall similar results as in the full sample: Figure A.7 and Table A.8 show no indications of treatment having a substantial impact on the ACSC rate.

Moving to the subjective measures, excluding the largest and smallest 5 percent eliminates 14 out of the 123 municipalities in the original sample. The min-max range of population size in 2009 thereby shrinks 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 for this 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.9, 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.

Table A.8: Sensitivity to population size and initial monopoly status; ACSC rate

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
Counties	21	21	21	21

Panel B: Heterogeneity over initial monopoly status

	(1) Baseline	(2) Linear trends	(3) Covariates	(4) Donut
Treatment	-0.489	-0.544*	-0.319	-0.216
	(0.396)	(0.323)	(0.353)	(0.246)
Treatment × OneCenter	0.675	0.652	0.375	0.290
	(0.640)	(0.498)	(0.527)	(0.334)
OneCenter	-0.240	-0.540	-0.467	-0.332
	(0.501)	(0.395)	(0.399)	(0.248)
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
Counties	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 from 90 months before reform implementation to 42 months after. Panel A shows coefficients from estimations contrasting the treatment and comparison groups with the 5 percent largest and smallest municipalities excluded. 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 primary care center before the reform. *OneCenter*=1 in post-reform periods for such municipalities, and *Treatment × OneCenter* captures the heterogeneous effect on municipalities in the treatment group. Standard errors clustered by county in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.9: Sensitivity to population size; subjective measures of primary care quality

	Panel A: No covariates			
	(1)	(2)	(3)	(4)
	Phone access	*Waiting times*	*Overall impression*	*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
Counties	12	12	12	12

	Panel B: Individual level covariates			
	(1)	(2)	(3)	(4)
	Phone access	*Waiting times*	*Overall impression*	*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
Counties	12	12	12	12

Note: The largest and smallest 5 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; and *Recommendation*: 0.714. The differences in total observations reflect differences in response rates across survey questions. Standard errors clustered by municipality in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
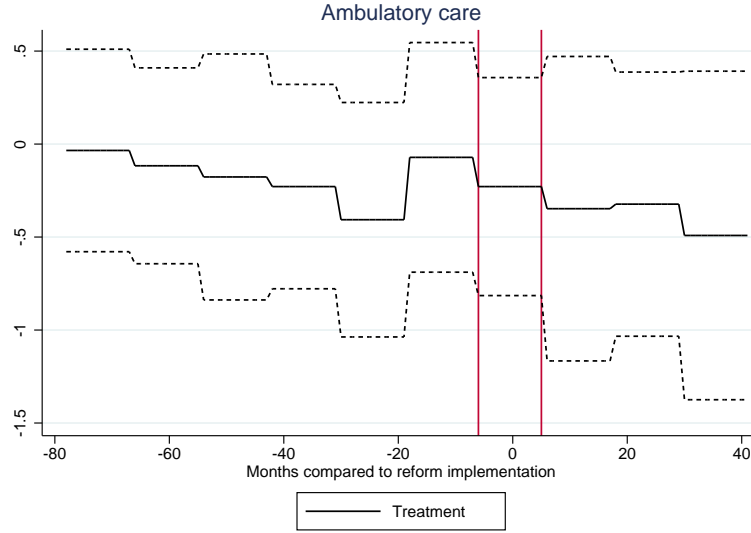
Figure A.7: The lines display the $\beta_{\bar{t}}$ coefficients (solid line) and confidence intervals (dashed lines) from regressions where we have excluded the 5 percent largest and smallest municipalities. We use a variant of Eq. (2) on a period from 90 months before a reform is implemented to 42 months after. The first 12-month period is used as reference period and is therefore excluded from the figure. There are six 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 are 16.8 and 5.5, respectively. The confidence interval is based on standard errors clustered at county level.

A.6   Heterogeneity over initial monopoly status

The pre-reform market structure is another dimension for 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 primary care center to an already competitive market. To test this, we augment the baseline specification with a triple interaction between the treatment group dummy, the post reform dummy and a dummy for pre-reform monopoly municipalities. (To disentangle the pure effect of being a pre-reform monopoly post reform, the model also includes the interaction between the latter two variables, *OneCenter*). Panel B of Table A.8 presents the results. The triple interaction term (*Treatment* × *OneCenter*) is statistically insignificant in all specifications, though it can be noted that the treatment effect is qualitatively different in pre-reform monopolies vs. non-monopolies: Within the set of pre-reform *non*-monopolies, the ACSC rate decreases slightly more in treated municipalities (coefficient on *Treatment*). The small magnitudes are similar to our baseline results. Within the group of pre-reform monopolies, the

A.17

treatment effect estimates are very close to zero (sum of coefficients on *Treatment* and *Treatment* × *OneCenter*) but of positive sign. We conclude that increased competition has not lead to greater quality improvements in previous monopolies than in already competitive markets.

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). The interaction term is also far from significant ($p = 0.375$).

A.7 Patient composition checks of survey data

Table A.10 presents the outcomes of six regressions testing the hypothesis that the treatment and the comparison groups display different changes in patient composition across the three survey waves. Each column uses a different outcome variable, taking the value 1 if the patient has a stable physician contact at the primary care center (column 1); has visited the primary 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).

A.8 Removing the entry channel

In this section, we attempt to examine if the components of reform that were not related to entry – i.e., more available information and lower search and switching costs – by themselves were important. By concentrating on municipalities that were not affected by entry, we eliminate concerns that increased competition may be confounded by increased access to medical staff or reduced travel time to providers.

Table A.10: Composition checks of patient survey data

	(1) Stable contact	(2) Visit ≥ 1	(3) Visit ≥ 2	(4) Visit ≥ 4	(5) Health ≥ good	(6) Health ≥ 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
Counties	12	12	12	12	12	12

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 surveys sent out to population size. Standard errors clustered by municipality in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

To remove the influence of new entry, we concentrate on the subset of municipalities in which the number of care centers did not increase during the period 6 months before–42 months after the reform. Within this subset, we contrast municipalities in which there was only one care center (monopolies) with municipalities in which there were at least two primary care centers (non-monopolies). The monopolies were arguably minimally affected by increased access to information and lower switching costs: there is not much one can do with the information when there is no alternative provider to switch to. We do not know how the two groups were affected in terms of elevated entry threats, although one may think that the threat would remain low in the monopoly group, which includes many small municipalities.

In relation to access, Figure A.8 shows that staff density has developed similarly in monopolies and non-monopolies over the whole period 2005-2013.[1] Thus, for this subset of municipalities, changes to this aspect of access to care should not affect a comparison of objective and subjective care quality in monopolies and non-monopolies.

With no lasting entries, we further know that travel times did not decrease in this subset of municipalities. Travel times could only have increased; i.e., due to exits. However, only in five municipalities were there fewer care centers 42 months after the reform than 6 months before. That

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[1]The caveat that we cannot discriminate between GPs and other physicians applies to this figure as well as Figure 2 in the main text. However, we believe that it is very unlikely that physicians with other specialities switched from secondary to primary care.
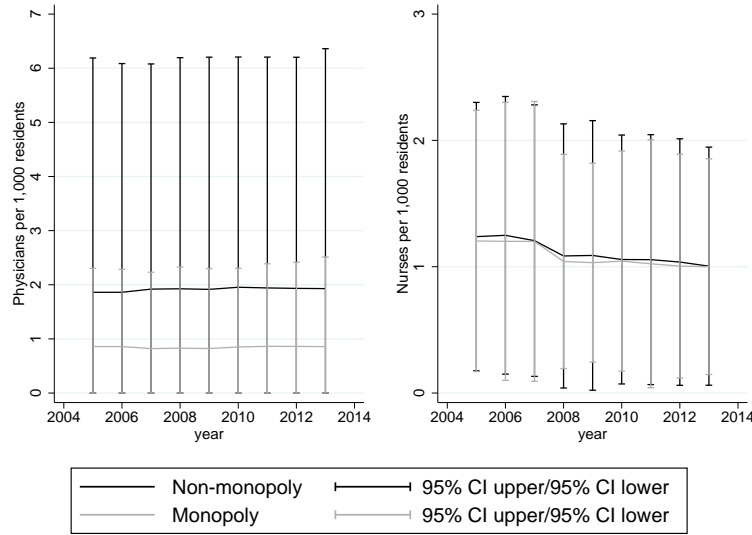
Figure A.8: The lines display the means and confidence intervals for the physician and nurse densities (employed physicians/district nurses per 1,000 residents) by non-monopoly and monopoly group in the subset of municipalities experiencing no entry.

is, the few exits that took place did not last long. The lasting exits occurred in the non-monopoly subset, and only one municipality become a local monopoly due to exit.[2] Thus, exits ought to have a minimal influence on our estimates, but we report results from estimations where municipalities with lasting exit are removed from the sample.

In our analysis of the subset of municipalities with no increase in care centers, we let the non-monopolies play the role of the treatment group, and monopolies the role of the comparison group. Otherwise, the econometric models are specified as our baseline model (see Section 7.3 and 8.3). In the estimations using the *ACSC rate* as the dependent variable, there are 95 monopolies (*NoEntryComp*=0) and 89 non-monopolies (*NoEntryComp*=1). Figure A.9 and Panel A of Table A.11 indicate that the non-monopolies improved slightly more in terms of the *ACSC rate*, but the point estimates are insignificant, small, and the decline in ACSC episodes starts well before the reforms are implemented. Panel B shows that the results are similar when the five non-monopolies with lasting exit are removed from the sample.

Table A.12 contains corresponding results for the subjective outcome measures. In this sample,

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[2]In the one instance of exit among the monopolies, a new care center quickly replaced the old one.

Table A.11: Removing the entry channel, *ACSC rate*

Panel A: Full set of monopolies and non-monopolies

	(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
Counties	21	21	21	21

Panel B: Non-monopolies with lasting exit removed

	(1) Baseline	(2) Trends	(3) Covariates	(4) Donut
NoEntryComp	-0.345	0.182	0.125	0.0473
	(0.328)	(0.241)	(0.216)	(0.192)
Linear trends	No	Yes	Yes	Yes
Covariates	No	No	Yes	Yes
Observations	23,623	23,623	23,623	23,623
Municipalities	179	179	179	179
Counties	21	21	21	21

Note: The estimations use the set of municipalities where there was no increase in the number of care centers during the period 6 months before to 42 months after the reform. *NoEntryComp*=1 in post-reform periods for the municipalities where there were at least two primary care centers in the start of the period; *NoEntryComp*=0 in post-reform periods for the municipalities where there was only one primary care center. Panel A includes the full set of these municipalities, 89 had at least two centers at the start of the period, and 95 were local monopolies. Panel B removes five municipalities from the first group, where the number of care centers was lower at the end of the period compared to the start. All specifications use *ACSC rate* as dependent variable, and include municipality, year, quarter, and month-to-reform fixed effects. The sample covers the period from 90 months before reform implementation to 42 months after. Standard errors clustered by county in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
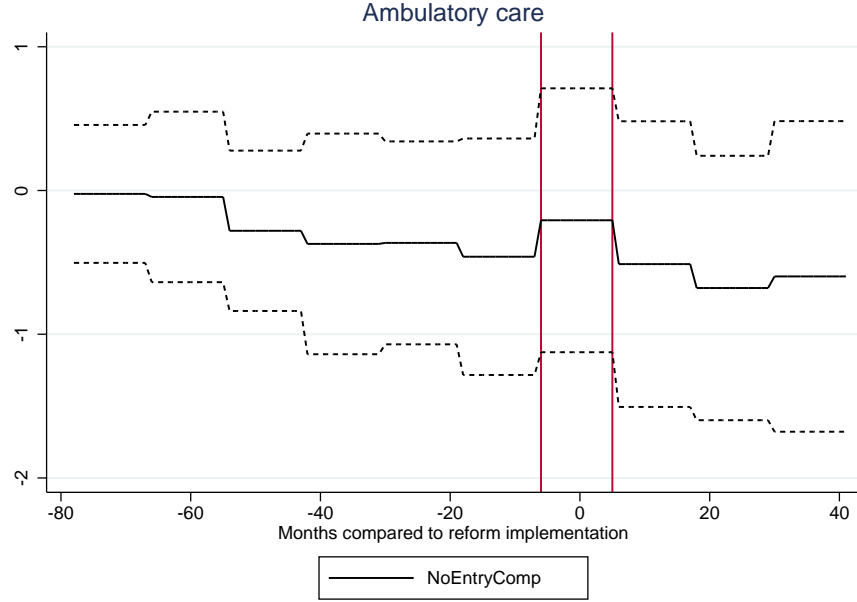
Figure A.9: The lines display the $\beta_{\bar{t}}$ coefficients (solid line) and confidence intervals (dashed lines) from regressions using a variant of Eq. (2) on a period from 90 months before a reform is implemented to 42 months after. The first 12-month period is used as reference period and is therefore excluded from the figure. There are six placebo effects before the reforms are implemented (month compared to reform = 0). Dependent variable is *ACSC rate*. The estimations use the set of municipalities where there was no increase in the number of care centers during the period 6 months before to 42 months after the reform. *NoEntryComp*=1 in post-reform periods for municipalities where there were at least two primary care centers; *NoEntryComp*=0 in post-reform periods for municipalities where there was only one primary care center. Confidence intervals use standard errors clustered by county.

there are 47 monopolies and 41 non-monopolies. The results for the full set in this sample are shown in Panel A; Panel B shows results from estimations excluding the one non-monopoly with a lasting exit in the subjective measure sample. All coefficients on *NoEntryComp* are positive in both Panel A an B and, except for *Waiting times*, statistically significant ($p < 0.05$ or lower). The estimates are larger than our baseline estimations, around 4 percentage points for all three dependent variables (6–8 percent of the means).

It is important to acknowledge that these estimations rely on a selected sample, and may not uncover causal effects. Nevertheless, the results suggest that other channels than the entry channel might have been important. It is also notable that the estimates are at least as large as our baseline estimates in a sample where changes to access do not influence the estimates. If access has an effect on quality, it ought to be positive; thus, these estimates make us less concerned that our

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baseline results are the net effect of large positive access effects and negative competition effects.

A.9   Focusing on the entry channel

In this section, we explore the importance of actual entry. *New entry* is a dummy variable taking the value 1 in the post-reform period for municipalities with at least one new primary care center established from 6 months pre to 42 months post reform.

Using *New Entry* to define treatment, there are 105 municipalities in the treatment group and 184 municipalities in the comparison group in the estimations using the *ACSC rate* as the dependent variable. Figure A.10 is analogous to Figure 3, though with treatment defined by *New entry*. Compared with Figure 3, 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.13 confirms that the relationship between new entry and avoidable hospitalisations is small and statistically insignificant.

We next use our baseline treatment definition (Eq. 1) as an instrumental variable for *New entry*. Column (2) of Table A.13 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 hospitalisations. The estimate is negative (i.e., indicates improved quality) and of larger magnitude than the reduced form, but far from significant.

The same exercises are repeated for the subjective measures. 35 of the 123 municipalities in this sample experienced new entry after, or shortly before, the reform. Panels A–D of Table A.14 present 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 are caused by the different 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

Table A.12: Removing the entry channel, subjective measures

Panel A: Full set of monopolies and non-monopolies

	(1) *Phone access*	(2) *Waiting times*	(3) *Overall impression*	(4) *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
Counties	12	12	12	12

Panel B: Non-monopolies with lasting exit removed

	(1) *Phone access*	(2) *Waiting times*	(3) *Overall impression*	(4) *Recommendation*
NoEntryComp	0.0411**	0.00304	0.0395***	0.0412***
	(0.0201)	(0.0166)	(0.0141)	(0.0156)
Covariates	Yes	Yes	Yes	Yes
Observations	42,339	41,204	55,244	54,971
Municipalities	87	87	87	87
Counties	12	12	12	12

Note: The estimations use the set of municipalities where there was no increase in the number of care centers during the period 6 months before to 42 months after the reform. *NoEntryComp*=1 in post-reform periods for the municipalities where there were at least two primary care centers in the start of the period; *NoEntryComp*=0 in post-reform periods for the municipalities where there was only one primary care center. Panel A includes the full set of these municipalities, 41 had at least two centers at the start of the period, and 47 were local monopolies. Panel B removes 1 municipality from the first group, where the number of care centers was lower at the end of the period compared to the start. Municipality and survey fixed effects are included in all estimations. The sample covers the three years 2009, 2011, and 2013. Each observation is weighted by the inverse of the selection probability (the number of surveys sent out divided by the municipality's population size). The differences in total observations reflect differences in response rates across survey questions. The (joint treatment and comparison) means of the dependent variables in 2009 in the Panel A sample are: *Phone access*: 0.49; *Waiting times*: 0.56; *Overall impression*: 0.65; and *Recommendation*: 0.71. Standard errors clustered by municipality in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

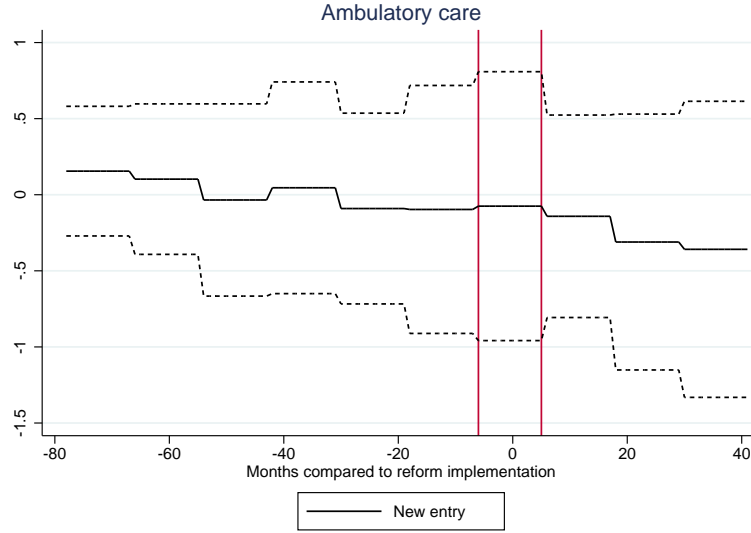Figure A.10: The lines display the $\beta_{\bar{t}}$ coefficients (solid line) and confidence intervals (dashed lines) from DID regressions using a variant of Eq. (2) on a period from 90 months before a reform is implemented and 42 months after. The first 12-month period is used as reference period and is therefore excluded from the figure. There are six placebo effects before the reforms are implemented (month compared to reform = 0). Dependent variable is *ACSC rate*. The mean and standard deviation over the pre-reform period are 17.1 and 6.1, respectively. Confidence intervals use standard errors clustered on counties. *New entry* = 1 in all post-reform periods for municipalities where new care centers were established in the period 6 months before-42 months after the reform.

B) is positive (though insignificant and much smaller in size compared with the other measures). Comparing coefficients with the baseline model shows that subjective quality improvements are even larger when defining treatment by actual entry.

While the results in this section are similar to (or stronger than) the baseline results, it should be stressed that these specifications rely on stronger assumptions than our baseline model. It is overly strong to make causal claims based on a DID estimation using actual entry to define treatment, as potential profits from entry correlate with the potential for quality changes. The slight negative (though insignificant) pre-trend in Figure A.10 indicates that this concern may be warranted. Further, the share of patients with a stable physician contact increased significantly more in the new entry group (results not shown), indicating that the patient casemix may have developed differently in the treatment and comparison group when these are defined by new entry. According to Table 6, this variable has a strong positive relation to high quality ratings. While the IV strategy

Table A.13: 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
Counties	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. *New entry*=1 in all post-reform periods for municipalities where new care centers were established in the period 6 months before-42 months after the reform. Columns (2)–(4): IV estimations using the previous treatment definition (the *Treatment* variable in Table 4) 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 in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

circumvents the problem of endogenous entry, it rests on the assumption that entry would be the only channel for the reform effects. That is a strong assumption, given the increased availability of information, lower switching costs, and stronger potential threats of entry associated with the reforms. Given the estimates in Appendix A.8 , we are reluctant to rule out that these channels were important.

References

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

Swedish Competition Authority, 2012. Val av vårdcentral - förutsättningar för kvalitetskonkurrens i primärvården. Report 2012:2.

Table A.14: Results for subjective measures defining treatment by new entry post-reform.

	(1) DID	(2) Reduced form	(3) First stage	(4) IV
Panel A: *Phone access* as dependent variable				
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
F-statistic, excluded instrument			11.64	
Panel B: *Waiting times* as dependent variable				
New entry	0.0127			-0.0169
	(0.0117)			(0.0313)
Treatment		-0.00658	0.389***	
		(0.0118)	(0.113)	
Counties	12	12	12	12
F-value, excluded instrument			11.79	
Panel C: *Overall impression* as dependent variable				
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
F-value, excluded instrument			13.34	
Panel D: *Recommendation* as dependent variable				
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
F-value, excluded instrument			13.27	

Note: *New entry* = 1 in all post-reform periods for municipalities where new care centers were established in the period 6 months before-42 months after the reform. There are 123 municipalities and 12 counties included in all specifications. All estimations include individual-level covariates and municipality and survey fixed effects. The sample covers the three years, i.e., 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; and *Recommendation*: 0.714. The differences in total observations reflect differences in response rates across survey questions. The differences in response rates also explain the slightly different *F*-values in the first stage regressions. Standard errors clustered by municipality in parentheses. *** p<0.01, ** p<0.05, * p<0.1.