Information, switching costs, and consumer choice: Evidence from two randomized field experiments in Swedish primary health care*

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Abstract

Consumers of services that are financed by a third party, such as publicly financed health care or firm-sponsored health plans, are often allowed to freely choose provider. The rationale is that consumer choice may improve the matching of consumers and providers and spur quality competition. Such improvements are contingent on consumers having access to comparative information about providers and acting on this information when making their choice. However, in the presence of information frictions and switching costs, consumers may have limited ability to find suitable providers. We use two large-scale randomized field experiments in primary health care to examine if the choice of provider is affected when consumers receive comparative information by postal mail and small costs associated with switching are reduced. The first experiment targeted a subset of the general population in the Swedish region Skåne, and the second targeted new residents in the region, who should have less prior information and lower switching costs. In both cases, the propensity to switch provider increased significantly after the intervention. The effects were larger for new residents than for the general population, and were driven by individuals living reasonably close to alternative providers.

Keywords: Consumer choice, Information, Switching costs, Primary health care, Field experiments

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1 Introduction

Consumers of services that are financed by a third party, such as publicly financed education and health care or firm-sponsored health plans, are often allowed to choose from a menu of providers. The rationale for consumer choice is simple: given that consumers have superior knowledge of their preferences and needs, choice should improve the matching of consumers and providers, and strengthen providers' incentives to improve quality. However, the available empirical evidence does not suggest that consumer choice systems in general have lead to substantial quality improvements. Studies of increased patient choice of hospitals have shown mixed effects on health outcomes (e.g. Cooper et al., 2011; Gaynor et al., 2013; Gravelle et al., 2014; Moscelli et al., 2016; Gaynor et al., 2016), and school choice and vouchers have mostly had small or insignificant effects on educational achievement (e.g. Rouse and Barrow, 2009; Fryer, 2016; Epple et al., forthcoming).[1] Studies of health plan choices even challenge the view that consumers are able to choose alternatives in line with their own interests, as many individuals choose strictly dominated plans (Bhargava et al., 2015; Gaynor et al., 2015).

From a scientific as well as from a policy perspective, it is of considerable interest to understand why consumer choice sometimes fails to improve on service quality, and to find ways to improve choice systems. We use two randomized field experiments to examine if frictions on the demand side present obstacles for consumer decision making in the market for primary care, and if such frictions can be reduced. As the first line of care, primary care deals with a diverse variety of health problems. The primary care market thus shares important features with markets in areas such as education, elderly care, hospital services, and health insurance: the product is multi-faceted, not standardized, and consumed either infrequently or not at all before the choice of provider is made, which forces consumers to base their initial choice on limited experience. There is furthermore considerable variation between individuals, as well as over time for the same individual, regarding which features of providers that are valuable in relation to one's needs. In the absence of comparative information, patients' ability to identify high-quality providers may therefore be limited.

The experimental interventions in this study consisted of leaflets with comparative information about a small set of relevant primary care providers, sent out by postal mail. In one treatment arm, individuals also received a form with pre-paid postage fee, which could be used to register a new choice. The interventions thereby addressed two types of demand side frictions that may stop the market from functioning well: information frictions and switching costs. Information frictions prevent consumers from obtaining full information about the quality of different providers,[2] thus weakening the link between free choice and enhanced welfare (Arrow, 1963). In the choice of health care provider, search costs – e.g., time and effort required to find comparative information – apparently give rise to significant informa-

[1]It should be noted that the literature provides few examples of substantial *negative* effects of consumer choice. A recent exception is Abdulkadiroglu et al. (2015), who find relatively large negative short-run effects on test scores of a school voucher program in Louisiana, United States (US).

[2]For recent evidence from health and prescription drug insurance markets that a substantial share of consumer decision making deviate from choices made by a fully informed and rational decision maker, see Abaluck and Gruber (2011, 2016a); Ketcham et al. (2012, 2016); Kling et al. (2012); Bhargava et al. (2015); Handel and Kolstad (2015). See e.g. Thaler and Sunstein (2008) for examples from a diverse set of markets.

tion frictions: across health care settings and countries, only a small minority of consumers actively search for and use comparative quality information (Victoor et al., 2012), despite that such information is often readily available online. This failure to seek information may be a rational response to search costs, as in models of rational inattention (e.g. Matejka and McKay, 2014; Gabaix, 2014), or arise because of more general forms of limited attention (e.g. Bordalo et al., 2013; Caplin, 2016). Another source of frictions is that many consumers have difficulties in understanding health-related information (e.g. Hibbard et al., 2007) or concepts related to health insurance (Loewenstein et al., 2013; Bhargava et al., 2015).

Even if consumers are well-informed, switching costs can decrease market efficiency by stopping consumers from changing to a better provider (Klemperer, 1995). Switching provider is often associated with monetary expenses (e.g., postage fees) or hassle costs (e.g., creating user accounts for online choice systems), which may be significant obstacles for switching (Handel and Kolstad, 2015). A major switching cost is the discontinuation of established relationships, for example with teachers or physicians.[3] By reducing consumers' incentive to try out new providers, switching costs may further undermine consumers' ability to mitigate information frictions.[4]

Our experimental setting is a Swedish region with 1.3 million residents, where free consumer choice has been an integral feature of primary care since 2009. In accordance with the evidence from other markets, studies have failed to find substantial quality improvements due to consumer choice in Swedish primary care (e.g. Fogelberg, 2014; Dietrichson et al., 2016) and few patients compare providers before making their choice (Glenngård et al., 2011; Swedish Agency for Health and Care Services Analysis, 2013; Wahlstedt and Ekman, 2016). Taken together, this evidence suggests that information frictions and switching costs may impede the functioning of the market.

Our randomized field experiments targeted two samples. The first intervention was directed to a sample representative of the general population. Our second intervention targeted new residents of the region, who constitute an interesting special case in terms of information frictions and switching costs: compared to the population at large, new residents face higher search costs, due to their shorter care history and smaller networks in the region, while their switching costs are lower, as they have not had time to build up a relationship with their current provider.

The treatment groups, 10,259 individuals in the population-representative sample and 3,454 in the sample of new residents, received a leaflet designed in collaboration with the regional health care authority. The leaflet contained comparative information on, e.g., accessibility, quality, and available services of an individual's current primary care provider and its three geographically closest competitors. 7,700 of the treated in the population-representative sample, and all treated new residents, received a pre-paid choice form together with the leaflet.

[3]See Hanushek et al. (2007) for a discussion about switching costs in a school choice context, and Starfield et al. (2005) and Hsiao and Boult (2008) for the importance of continuity in the patient-physician relationship in primary care.

[4]Ketcham et al. (2012) suggest that such learning can explain a decline over time in consumers' overspending on prescription drug insurance. Al-Ubaydli and List (2016) review a large set of field experiments in markets and find that behavioral decision making biases are often reduced or disappear when decision makers are sufficiently experienced.

The information items included in the leaflet were accessible on a website run by the Swedish regional health care authorities, subject to a varying degree of search effort on part of individuals. By sending information directly to consumers, the experimental treatment reduced the search costs associated with accessing the website. The leaflets may also have mitigated problems due to cognitive limitations, as the information was presented differently on the leaflets compared to the web. Finally, by including the choice form with pre-paid postage fee, the intervention reduced switching costs – specifically, small monetary and hassle costs associated with switching.

Compared to the control groups (102,599 and 3,452 individuals respectively), who did not receive any information, the interventions led to increased switching rates. In the population-representative sample, the share of individuals switching provider during the follow-up period was 6.5 and 6.3 percent in the treatment groups with and without a pre-paid choice form, versus 5.7 percent in the control group. In relative terms, this corresponds to increases of about 14 and 10 percent. Among new residents, 10.8 percent of the treatment group and 8.8 percent in the control group switched provider during the follow-up, an increase of 23 percent. Because well-defined groups of people either did not receive the leaflet, or lacked the language skills required to understand the information, these intention-to-treat (ITT) estimates address the research question of how information frictions affects choices only to a limited degree. Excluding individuals who died or left the region before the leaflets were sent out and recent immigrants, the relative treatment effects were 16 and 13 percent in the population-representative sample, and 35 percent in the new residents sample.

For the treatment groups that received both the leaflet and the choice form, the treatment effect is always statistically significant ($p < 0.01$), while the effect for the smaller treatment group that did not receive the choice form is only significant in some specifications. As the two estimates are very similar and statistically indistinguishable at the end of follow-up, we believe that the lower statistical power in the arm without choice form explains the failure to reject the null hypothesis in this sample.

In exploratory analyses, we use detailed administrative data to examine why, how, and for whom treatment affected switching decisions. We find that accessibility of care played a prominent role: the effect of the intervention was close to zero for individuals living more than 3 kilometres away from their second closest provider. With the exception of a weaker response among young residents, we find few significant indications of socioeconomic or demographic heterogeneity, though it should be noted that the statistical power is limited in this regard. A notable difference between the treatment arms with and without choice forms is that the choice form arm reacted immediately after the intervention, whereas the increased switching in the other arm was spread over the whole follow-up period. We further find that, conditional on having switched, the intervention increased the propensity to choose one of the other providers presented on the leaflet, rather than a provider not on the leaflet. The propensity to switch to providers not presented on the leaflet did not differ significantly between treatment and control groups. This suggests that the information played a key role in the treatment effect, though the experimental design does not allow us to rule out that the intervention mainly increased consumers' attention to the opportunity to switch provider (Heiss et al., 2016). In further support of the idea that the information *per se* mattered, we show that earlier attention-increasing campaigns in the region, which did not include information about specific providers, have not been followed by unusually large switching rates.

A few similar interventions have previously been studied in the US. Closest to our study, Ericson et al. (2017) analyze a randomized information intervention targeting consumers on the Affordable Care Act (ACA) Marketplace for health insurance, and McCormack et al. (2001) and Farley et al. (2002a,b) use similar interventions to study the effect of mailed out comparative information on health plan choices of new Medicare and Medicaid beneficiaries. None of these studies find significant effects on switching rates.[5] However, Ericson et al. (2017) find a substantial increase in the proportion of consumers that were shopping around on the Marketplace website.

Two field experiments in related markets have found stronger effects of information on switching rates. Kling et al. (2012) find that mailed-out personalized information on Medicare Part D prescription drug plans led to higher switching rates and lower plan costs, in comparison to a control intervention that promoted a website covering the same information. 28 percent switched plans in the treatment group, compared to 17 percent in the control group (a relative increase of about 65 percent). Hastings and Weinstein (2008) use a natural and a randomized field experiment to study the effect of information about school-level proficiency (natural experiment) and test scores (field experiment) on school choice in North Carolina. In both cases, information raised the probability of selecting another school than the default option; from 11 to 16 percent (a 46 percent increase) in the natural experiment and from 31 to 38 percent (a 23 percent increase) in the field experiment. Furthermore, treated individuals on average selected higher-scoring schools, which in turn increased their own test scores.

The relative increases in switching rates found in our study lie in between these two sets of experimental results. It is not surprising that the results differ, given the different choice settings and study populations. In particular, the self-selected population studied in Kling et al. (2012) may have been relatively interested in the drug plan choice, thus overestimating the effect for the Medicare Part D population at large.

Another possible explanation for the differences is the variation in volume and complexity of information materials. Our leaflets were less voluminous than the brochures sent out in McCormack et al. (2001) and Farley et al. (2002a,b), but included more information than the one-dimensional ranking in Hastings and Weinstein (2008) and the leaflets in Kling et al. (2012), which pointed out the cheapest alternative.[6] The information material in Ericson et al. (2017) was similar in complexity to that in Kling et al. Although their intervention did not affect the switching rate, it led to a substantial increase in visits to the choice website, where the complexity quickly expanded as there were up to 60 plans to choose between. Our leaflets avoided such choice overload, as they included information about only a small number of alternative providers.[7]

[5] Similarly, Knutson et al. (1998) and Hibbard et al. (2002) found no significant effects of comparative information on health plan choice in two non-randomized studies of large firms.

[6] That complex information decrease consumers' ability to make informed choices has been shown in a range of hypothetical choice experiments using health care settings (e.g., Damman et al., 2015; Kurtzman and Greene, 2016).

[7] In the context of school district employees' health insurance choices in Oregon, Abaluck and Gruber (2016b) find that a decision support tool had minor impact on employee's forgone savings. This randomized intervention differed in key aspects from those mentioned in the text, most importantly it was not in the

In the trade-off between a simpler information material and a smaller impact on switching, there are good reasons not to neglect the advantages of a more encompassing material. In our case, sending out information about only one quality indicator, or recommending one specific provider, would have been irrelevant or even misleading, given the multifaceted nature of primary care. In markets where quality is a multidimensional construct, and where heterogeneity in consumer preferences motivate providers to specialize, information material need to be more encompassing. Put differently: if providers can be ranked from better to worse based on a single indicator which is relevant for all consumers, then the potential for consumer choice to improve the matching of consumers to providers is small. Consequently, the argument for consumer choice is substantially weakened.

Offering a less simplistic information material is even more important when information campaigns are scaled up to market level. Focusing on a single indicator could strengthen providers' incentives to engage in cream-skimming or teaching-to-the-test behavior (e.g. Holmström and Milgrom, 1991). In the light of such concerns, it is encouraging that we find effects of a relatively encompassing information material. Our finding suggests that similar campaigns can be used to improve the functioning of many types of consumer choice markets.

Our paper further relates to the literature on public release of comparative information – e.g., report cards or rankings published online or in newspapers. According to several literature reviews, public reporting in health care has had little impact on consumers' choices and health outcomes (Fung et al., 2008; Ketelaar et al., 2011; Totten et al., 2012; Mukamel et al., 2014),[8] and the effects of publicly reported information on school choice are mixed (see e.g. Koning and van der Wiel, 2013; Mizala and Urquiola, 2013).[9] Our results suggest that the effort required to access the publicly reported information may be one explanation for its limited impact on choices.

The paper proceeds as follows. Section 2 describes primary health care in Sweden and Skåne. Section 3 details the experimental design and our estimation procedures. Section 4 describes the data and Section 5 presents the results. Section 6 concludes.

2 Primary health care in Sweden and Skåne

Health care in Sweden is a mainly tax-funded system with universal coverage for citizens. Most responsibilities for financing and organization has been decentralized to 21 independent

form of mailed out information. In order to obtain personalized information, consumers had to actively access the decision support tool and to recall their employer's contribution rate.

[8]There are exceptions, see e.g. Pope (2009) who find increased demand for hospitals ranked highly in a newspaper ranking. Patients and physicians often jointly make the decision about hospital though, and it is not clear how much of the effect that is driven by physicians reacting to the ranking.

[9]Andrabi et al. (2014) provide interesting evidence from a field experiment in Pakistan, where treatment was assigned on the market level, and schools and parents were supplied with report cards containing information about school and child performance. This mix of public information about schools and private feedback about children did not affect switching rates significantly, but enrollment and test scores increased, and private school fees decreased.

regions, or county councils, headed by locally elected politicians.[10] The present study is set in Skåne (Scania) a relatively large and densely populated region on the southwestern tip of Sweden with 1.3 million residents.

Primary care is intended to be the first line of care, though patients do not need a referral to seek specialist care. Primary care is typically provided in group practices called primary care centers. A primary care center on average employs about four physicians/general practitioners (GPs), and is additionally staffed with nurses and other specialists such as for example behavioral therapists and gynaecologists (Anell, 2015).

There were 150 primary care centers in Skåne at the beginning of 2015. 86 of the centers were owned and run by the regional health care authority, the others were either private single-firm practices or parts of larger commercial chains. In early 2015, the mean (and median) number of enrolled patients per center was approximately 6,800.

Whether publicly or privately owned, the most important source of revenue for the care centers is public funds distributed by the regional health care authority. In Skåne, the care centers are mainly reimbursed by capitation: care centers receive a fixed sum monthly per enrolled patient. The capitation is risk-adjusted for expected care needs, using the Johns Hopkins Adjusted Clinical Groups (ACG) system, and for socio-economic characteristics associated with higher care need, using the Care Need Index (CNI).[11] Pay-for-performance reimbursement accounts for a minor share of revenues, as does patient fees for visits. The visit fee is regulated by the health care authority. In 2015, the fee was 160 SEK (approximately $20), subject to an annual cap of 1,100 SEK ($135). The fee was 25 percent higher for visits at care centers where the patient was not enrolled.

All residents of Skåne must be enrolled at a primary care center. Residents can freely choose between the primary care centers, which are required to accept new enrollments, and there is no limit on the number or frequency of switches allowed. In 2015, about half of the Scanian population was enrolled at the care center closest to their residential address. The geographically closest care center was the default option when the choice system was introduced in 2009, and is still the default for new residents. After automatically registering the new resident at the closest care center, the region sends a notification letter that also mentions the opportunity to switch. The letter does not contain any information about providers on the market, though.

It is worth noting that individuals enroll at a center, and not with a certain physician. While the regional health care authority emphasizes that continuity in the patient-physician relationship is important (for instance, continuity is a component in the pay-for-performance scheme), patients are not guaranteed to see the same physician on repeat visits.

To guide the choice of provider, the health care authority provides information about the care centers via a website called 1177.se (similar to, e.g., NHS Choices in England). Via this website, patients can obtain information on contact details, opening hours, availability of special clinics and competencies, and patient ratings from a bi-annual patient survey. The website also has an interface for making comparisons of the patient ratings of up to four care centers. Beside the information on 1177.se, many care centers use their own web

[10] The 290 municipalities share responsibility for elderly care and health care in primary and secondary schools.

[11] See e.g. Sundquist et al. (2003) for a description of CNI, and Anell et al. (2016) for effects of risk-adjusted capitation on the distribution of primary care centers.

pages (linked from 1177.se) to describe available services etc.[12] Until 2016, the health care authority in Skåne also published a magazine two-three times per year, which, among other things, contained information about how to switch centers and promoted 1177.se as decision support. The magazine was mailed out to all residents in the region.[13] Over the years, the region has also made occasional advertisements of the right to choose provider. For instance, ads have been published in newspapers, on buses, on the web, and sent by postal mail to the whole population (without information about specific providers).

At the time of our experimental interventions, there were two principal ways to switch care center. One option was to log in to a personalized section at 1177.se, from where it was straightforward to search for and select a care center. Another option was to obtain a choice form, either by printing it from 1177.se (no login required), or by fetching a form at a care center. After filling in and signing the form, it could either be handed in manually or sent by postal mail to the chosen care center. Thus, the only direct monetary cost associated with switching would be the cost for the stamp.

Since the implementation of consumer choice in 2009,[14] about 40 percent of the individuals in our sample have switched providers at least once. Yearly, about 9 percent of the population switch care center. It is not possible to tell directly if the low mobility reflects information friction and switching costs or a mature market in equilibrium. On the one hand, previous surveys indicate that Swedes generally feel that they have made an informed choice of provider. On the other hand, the surveys also show that only a small share of consumers searched for comparative quality information before making their choice, and that the current provider was often the only information source (Glenngård et al., 2011; Swedish Agency for Health and Care Services Analysis, 2013).

3 Experimental design and empirical strategy

We describe the experimental interventions in Section 3.1, the assignment to treatment and control groups in Section 3.2, and our estimation procedure in Section 3.3.

3.1 Experimental interventions

The primary component of the intervention was an information leaflet, which was sent by the regional health care authority by postal mail to the treatment groups. Neither treatment nor control groups were aware that they were participating in an experiment – and in a sense they were not, as the leaflet was a real information campaign from the health authority. In the terminology of Harrison and List (2004), we study a *natural field experiment*.

[12]During the experimental intervention and follow-up periods, a privately funded website also offered broadly the same content as 1177.se (the site discontinued its operation in 2016, due to the significant overlap with content at 1177.se).

[13]In Section 5.3.3, we use the magazine to examine how general reminders affect switching rates.

[14]All Swedish county councils introduced similar reforms in 2007-2010. The counties' systems have two main features in common: primary care centers are not allowed to refuse patients who wants to enroll, and county councils cannot veto the establishment of new centers that fulfil certain pre-specified requirements. See Dietrichson et al. (2016) for more information about the reforms, and effects on supply and care quality.

The leaflets contained comparative information about the primary care center where the individual was currently enrolled and the three geographically closest competitors of this center. An example of a leaflet is available in Appendix A. As a secondary intervention, a subsample of the experimental subjects also received a pre-paid choice form, which may have reduced the monetary and hassle costs of switching: the only effort associated with the use of the attached form was to manually fill in the name of the chosen center and to return the form, which could be done either by postal mail or by handing it in at a care center.

The control groups received nothing. This implies that we cannot separate the effect of increased access to information from the effect of being reminded of the opportunity to freely choose provider (Heiss et al., 2016). Although we foresaw this problem, exogenously given restrictions regarding the sample size per care center forced us to limit the number of treatment arms. Given the widespread knowledge about free choice in Sweden, we believe that the margin for reminder effects is small. In Section 5.3.3, we also show that switching rates have been stable around the time of previous campaigns, which have not included comparative information.

The information leaflet was in the format of an A4 sized paper folded in two (see A for an example). On the front page, there was a short text stating that residents are allowed to freely choose primary care center, that it is important to compare centers to find a suitable one, and that comparative information about the individual's current care center and the three closest alternatives were available on the centerfold of the leaflet. On the end page, there was a description of how to switch care center. It was also noted that the leaflet recipient was not required to make a new choice, and that he or she would remain enrolled at the current center if (s)he did not make a new choice.

The centerfold contained information about several aspects of the four care centers. First, there was information about some general features (address, phone number, opening hours, number of enrolled patients, public/private). Second, there was a set of quality indicators, of which two were taken from a national survey of patients who had visited primary care in 2014 (percentage of respondents that would recommend the care center to others, and perceived waiting time to see a physician)[15], and three indicators collected by the health care authority (percentage of telephone calls that were answered or called back within 2 hours, percentage of patients with 3 or more visits who saw the same physician at least half of the times, and an indicator of whether the care center reached the region's target for adherence to prescription guidelines for elderly). Third, there were indicators for each center's availability of competencies or specialized clinics focusing on certain population or patient groups (elderly individuals, or patients with dementia, asthma, chronic obstructive pulmonary disease, or congestive heart failure), and for the availability of behavioral therapists, gynaecologists, chiropractors, or naprapaths. Fourth and finally, there was an indicator for centers located closely to (or located jointly with) a midwife clinic or a children's health center.

Leading administrators at the health care authority were involved in the decision of what, and how much, information to include on the leaflet. All information was already publicly available, in the sense that it could be reached via 1177.se. However, there was a gradient in how much effort was required to find the information on the webpage. The information about contact details and patient survey scores were presented at the index page of each

[15]Perceived waiting times were measured on a 1-100 scale with 100 = shortest waiting time.

center's webpage, from which the information about available special clinics at the center was only one click away. The information about the three quality indicators measured by the region was not directly accessible from the webpage of each care center, though. To find this information, the individual would have had to use a search engine.

3.2 Assignment to treatment

We used the random number generator in Stata (StataCorp, 2013) to randomly assign individuals from two populations to treatment and control groups. The first population consisted of a representative sample of 11 percent of the region's residents over 18 years of age, drawn randomly from each care center's patient list in the health authority's enrollment register on February 2, 2015. The second population included all individuals (above 18) who entered the enrollment register between February 4 and May 11, 2015 (to avoid treating individuals in both interventions). By and large, the second population thus consisted of individuals that had just moved into the region (from other regions or from abroad). However, the sample also included individuals who had been enrolled at a center outside the region despite continuously living in the region (253 of the 6,906 were registered as inhabitants in the region on December 31th, 2014). While these individuals were newly *registered* residents, rather than new residents, we henceforth use the latter expression to ease the reading.

The full population-representative sample include 112,859 individuals. Out of these, 10,259 were randomly assigned to receive the information leaflet. A randomized sub-sample of these (7,700 individuals) also received the choice form. The randomization was done within each care center's patient list, to ensure that no more than one percent of the patients enrolled at each center would receive the intervention. The reason for this restriction was that the health care authority, for political and legal reasons, wanted to ensure that no single provider would be disproportionally and substantially affected by the intervention. One percent was also deemed as small enough not to substantially affect the market.[16]

One individual chose to opt out from the study after randomization.[17] The sample we use for the intention-to-treat (ITT) analyses of the population-representative sample (PRS) therefore includes 112,858 individuals. Our preferred sample is somewhat smaller, as it excludes individuals who did not receive the information or ought to have had difficulties in understanding it. 137 individuals died or left the region before we extracted address information (for administrative reasons, address data was extracted after the randomization date), and 146 additional individuals were de-registered from the region before the leaflets were mailed out in the last week of February. The mentioned 283 individuals were therefore not in essence part of the information intervention, and are excluded from our preferred sample. The preferred sample also excludes individuals of non-Nordic origin (born outside a Nordic country with two non-Nordic parents) who immigrated to Sweden no earlier than

[16]Given this restriction, we oversampled the treatment that we thought would be most likely to have an effect, i.e., the treatment including the choice form. The restriction is also the reason why we did not include treatment arms including only the choice form or only a reminder about the free choice.

[17]In accordance with the recommendation from the regional ethical board, we gave all individuals the option to not be a part of the study, by announcing the project in two local newspapers in August 2015 (i.e., after the interventions). This is a standard procedure when using register data in Sweden. Note that the advertisements did not mention neither the information campaign nor the experimental set up.

2014, as most of these individuals ought to have faced severe difficulties in understanding the information. In total, our preferred sample in the PRS experiment includes 111,487 individuals (Appendix B.1 shows how the main results change as we sequentially exclude the other subgroups from the ITT sample).

Between Feb 2 (the randomization date of the first experiment) and May 11, 6,906 individuals were newly registered in the region. These formed the population of new residents (NR), of which approximately 50% (3,454) were assigned to treatment. There was only one treatment arm in the NR intervention: information leaflet plus choice form. To avoid spill-over effects within families, this intervention was cluster-randomized by residential address.[18] The number of clusters were 6,059, indicating that most new residents resided in single-person households. The population was extracted from the enrollment register on May 11, the randomization took place on May 25 2015, and the leaflets were mailed out in the second week of June. We have complete information from the health authority's registers for all but one individual, leading to an ITT sample of 6,905 individuals in the NR experiment. For similar reasons as in the first experiment, our preferred sample is smaller: it excludes 102 individuals who died or left the region between randomization and intervention, and almost 3,000 individuals who had recently immigrated to Sweden. Due to lack of birth country data for immigrants arriving in 2015, we are not able to exclude only immigrants of non-Nordic origin for the NR sample.[19] In total, the preferred sample includes 3,812 individuals.

The daily enrollment status was tracked for both samples from the day the leaflets were distributed until early November 2015. This means that the follow-up period was approximately 36 weeks for the population-representative sample and 21 weeks for the new residents sample.

3.3 Estimation

We estimate the main effects of receiving treatment in a regression framework, using a linear probability model (LPM) expressed as:

$$y_i = \alpha + \beta Treatment J_i + \gamma X_i + \varepsilon_i \qquad J = \{W, WO\} \tag{1}$$

where y_i is a dummy variable. Our main dependent variable attains the value one if individual i switched care center at least once during the full follow-up period. In additional analyses of what care centers individuals switched to, we consider two further dependent dummy variables: one indicating individuals who were enrolled at one of the other three care centers on the leaflet, one indicating individuals enrolled at a center not on the leaflet.

For the population-representative sample, $TreatmentW$ indicates the treatment arm with a choice form attached and $TreatmentWO$ indicates the arm without a choice form; as noted, there was only one treatment arm ($TreatmentW$) in the intervention targeting new residents. The vector X_i contains covariates in the form of indicator variables, which we include in our preferred specifications to increase precision. ε_i is a residual term and α is an intercept.

[18]Due to the restrictions of sample size per care center in the first experiment, we could not cluster-randomize the treatments to the PRS. In Appendix B.3, we show that household spill-over effects are unlikely to be a concern for our estimates for PRS.

[19]Birth country data is only available for immigrants registered as residents before Jan 1, 2015. Among immigrants in 2014, we only exclude those of non-Nordic origin, as in the PRS.

Using a linear probability models (LPM), instead of a binary choice model, simplifies interpretation and inclusion of interactions. The linearity assumption is also less restrictive as all right hand side variables are indicator variables. (The main results are also very similar when using a logit model, see Appendix B). Throughout, we perform separate estimations for the two interventions. In the estimations for the population-representative sample (PRS), we weight observations by the inverse of the probability of being drawn, which varies slightly depending on the size of the initial care center's patient list. Assignment to treatment in the PRS was stratified by care center, but the reported estimations do not include strata fixed effects. Neither population weights nor strata fixed effects influence the results (see Appendix B). We use heteroskedasticity-robust standard errors, which in the case of new residents are clustered by home address to account for the cluster-randomization at that level.[20] Randomization inference (e.g. Young, 2016; Athey and Imbens, 2016) on the main specifications yield the same conclusions (see Appendix B.2).

To examine geographic, socioeconomic, and demographic heterogeneity in the response to treatment, we augment the main specification with interaction terms between the treatment indicator(s) and the covariates:

$$y_i = \alpha + \beta TreatmentJ_i + \delta \left(TreatmentJ_i \times X_i\right) + \gamma X_i + \varepsilon_i \tag{2}$$

4 Data

4.1 Data sources

The health care authority in Skåne has a register containing information about the current and past care center enrollments of each resident. Another regional register contains information about contacts with the health care system in both inpatient and outpatient care — visits, hospitalizations, diagnoses etc.[21]

We have access to daily individual-level information from these registers for the two experimental samples. For each individual, the dataset comprises the primary care enrollment history and all health care consumption from 2009 onwards (of course, the length of the times series is shorter for residents who moved in later). The dataset stretches until early November 2015.

To this data, we have matched individual information about demographic (e.g. sex, age, civil status, number of children, foreign background) and socioeconomic (e.g. educational attainment, income) characteristics from official registers held by Statistics Sweden.[22] We also have information about the distances between each individual's home address and all primary care centers in Skåne. The distances are calculated at 1) the dates of the experimental interventions, and 2) the last date of the follow-up period. The distance calculations

[20] Clustering standard errors on the primary care center level yields very similar main results in both experiments (results not shown).

[21] We lack information about visits to private inpatient care providers. These providers account for a very small share of total inpatient care visits and are of little relevance for this study.

[22] Note that all information has been de-individualized, in the sense that real names and personal identification numbers that would enable identification of specific individuals are not available to the researchers.

Table 1: Definitions of covariates

Choice within 1,000/3,000/>3,000m	Individual has ≥2 care centers within 1,000/3,000 or >3,000 meters from home.
Competition within 1,000/3,000/>3,000m	Individual's initial care center has a competitor within 1,000/3,000 or >3,000 m.
Lowest (highest) education tertial	Two thirds of individual's birth cohort has longer (shorter) education (cohort defined by birth decade).
Lowest (highest) income tertial	Gross income in the lowest (highest) tertial of the regional income distribution.
Age below 30	Individual is below 30 years of age.
Age above 65	Individual is above 65 years of age.
Female	Individual is a woman.
Foreign background	Born outside, or both parents born outside, Sweden.
≥5 physician visits	Individual has visited a physician in primary care at least 5 times since 2009.
ActiveChange	Individual has switched care center since 2009.
No background data	Individual is not in the population registers.

Note: All covariates are dummy variables = 1 when the definition above applies and 0 otherwise. Data on choice set, competition, active changes and primary care visits come from the regional health care authority's registers. Data on age, sex, birth country, educational level and income come from Statistics Sweden's registers. Individuals with no background data were registered as residents in Sweden no earlier than January 1, 2015. Gross income includes income from individuals commuting to Denmark.

are based on the region's information about the coordinates of care centers and individuals' addresses. Table 1 shows definitions of the covariates we use in our estimations.

4.2 Summary statistics

Table 2 display descriptive statistics (means and standard deviations) for the covariates in the ITT samples, and shows that the randomizations overall appear to have created balanced samples. Panel A shows these statistics for the control and the two treatment groups in the population-representative sample. Column 7 shows p-values from F-tests of the null hypothesis of equal means in all three groups. For all variables, the differences are small, and only the dummy for having visited primary care more than five times since 2009 is significant at conventional levels. In a regression of the treatment indicator on all covariates, we cannot reject the null that their coefficients are jointly equal to zero ($F = 0.88, p = 0.62$).[23]

Panel B covers the new residents sample. All mean differences between the treatment and control groups are small, but two are significantly different at the ten percent level: the share of individuals belonging to the highest tertial of the income distribution ($p = 0.064$) and the share of females ($p = 0.062$). In a regression of the treatment indicator on all covariates, we cannot reject the null that their coefficients are jointly equal to zero ($F = 0.95, p = 0.52$). The last variable, *Foreign background missing*, indicates the large share of recent immigrants among new residents. Despite the universal coverage of Statistics Sweden's registers, it is not until an individual is registered as a resident that he or she is entered in the population

[23]Note that there are three levels of this outcome variable (control, treatment without choice form, treatment with choice form). If we contrast each treatment group to the control group separately (i.e., exclude the other group from the regression), we still get highly insignificant F-values.

register. In practice, this means that we lack background data in one or more dimensions for people that immigrated to Sweden shortly before the interventions. Notably, the share lacking background data is similar in the treatment and the control group.

Comparing the population-representative sample and the sample of new residents, there is a striking difference in the shares of young and elderly individuals (below 30 or above 65 years of age, respectively). Due to the demographic profile of people moving within the country, the share of young is clearly higher and the share of elderly people lower in the new residents sample. Despite that the share of individuals with foreign background according to the population register is slightly higher in the population-representative sample, the large share of the NR sample lacking background register data mostly comprises people of foreign background.[24]

Finally, we note that the exclusion of individuals to form our preferred sample (*StaySwed*) does not give rise to imbalance between the treatment and control groups. This holds for both the population-representative and the new residents sample.

5 Results

Section 5.1 shows our main results. Section 5.2 explores the heterogeneity of the main results over patient and care center characteristics. Section 5.3 details when and where to people switched, and thereafter discusses the potential reminder effect of the intervention. The tested hypotheses follow our pre-registered analysis plan,[25] except for the discussion on reminders, which is not a direct analysis of the experimental intervention.

5.1 Main results

The ITT estimates for the population-representative sample (PRS) are shown in the first column of Panel A, Table 3. Compared to the 5.7% switching rate of the control group during the 36 week follow-up period, the probability to switch was 0.6-0.8 percentage points higher among treated individuals. The treatment effect is only statistically significant for the treatment arm that received a choice form (*TreatmentW*), but the estimates of the two treatment arms are similar, statistically indistinguishable, and not jointly equal to zero. Given the relatively small size of the *TreatmentWO* arm, we suspect that the insignificance of this variable is due to low statistical power. The results are very similar when we include covariates (column 2).[26]

[24]The group with no background data also includes Swedes who have lived in other countries during the whole period 2008-15, and therefore have not been in the Swedish population register during any of our sample years.

[25]The analysis plan is available at the American Economic Association's registry for randomized controlled trials (www.socialscienceregistry.org) with registration number AEARCTR-0000659, and title "Information and user choice in primary health care markets". The plan includes additional hypotheses, e.g., of examining heterogeneity related to patients' morbidity, which will be addressed in a companion paper.

[26]In specifications with covariates, the reference person is a middle-aged (30-64 year old) man with educational attainment and income in the mid-tertials of the respective distributions, born in Sweden with two Swedish parents, living within 1 km distance to at least two care centers, and initially enrolled at a center that had at least one competitor within a 1 km distance. Since 2009, the reference person has not switched

Table 2: Covariate summary statistics for each experiment

Panel A: Population-representative sample

	Control		*TreatmentWO*		*TreatmentW*		
	Mean	SD	Mean	SD	Mean	SD	p
Choice within 1 km	0.304	0.460	0.300	0.458	0.302	0.459	0.840
Choice within 1-3km	0.285	0.452	0.281	0.449	0.286	0.452	0.856
Choice within >3 km	0.403	0.490	0.412	0.492	0.405	0.491	0.581
Distance missing	0.008	0.089	0.007	0.086	0.007	0.083	0.573
Competition within 1 km	0.502	0.500	0.498	0.500	0.501	0.500	0.923
Competition within 1-3km	0.205	0.403	0.206	0.405	0.204	0.403	0.965
Competition within >3 km	0.289	0.453	0.290	0.454	0.290	0.454	0.991
Lowest education tertial	0.328	0.470	0.329	0.470	0.329	0.470	0.982
Highest education tertial	0.305	0.460	0.299	0.458	0.298	0.458	0.464
Education missing	0.025	0.156	0.023	0.150	0.024	0.153	0.775
Lowest income tertial	0.326	0.469	0.318	0.466	0.330	0.470	0.536
Highest income tertial	0.327	0.469	0.331	0.471	0.319	0.466	0.363
Income missing	0.012	0.107	0.011	0.106	0.011	0.106	0.995
Age below 30	0.203	0.402	0.204	0.403	0.205	0.404	0.914
Age above 65	0.242	0.429	0.243	0.429	0.250	0.433	0.321
Female	0.507	0.500	0.506	0.500	0.514	0.500	0.581
Foreign background	0.262	0.440	0.250	0.433	0.255	0.436	0.237
For. background missing	0.046	0.209	0.050	0.218	0.047	0.212	0.498
≥5 physician visits	0.649	0.477	0.649	0.477	0.665	0.472	0.017
ActiveChange	0.384	0.486	0.380	0.485	0.379	0.485	0.561
StaySwed	0.988	0.109	0.986	0.116	0.988	0.110	0.772
N	102,599		2,559		7,700		

Panel B: New residents

	Control		*Treatment*			
Variable	Mean	SD	Mean	SD	Diff.	p
Choice within 1 km	0.416	0.493	0.412	0.492	0.004	0.754
Choice within 1-3km	0.242	0.428	0.243	0.429	-0.001	0.899
Choice within >3 km	0.236	0.425	0.226	0.419	0.009	0.354
Distance missing	0.107	0.309	0.118	0.323	-0.012	0.120
Competition within 1 km	0.569	0.495	0.565	0.496	0.004	0.713
Competition within 1-3km	0.202	0.401	0.212	0.409	-0.011	0.277
Competition within >3 km	0.213	0.409	0.207	0.405	0.005	0.586
Lowest education tertial	0.142	0.349	0.131	0.338	0.011	0.203
Highest education tertial	0.216	0.411	0.210	0.407	0.006	0.548
Education missing	0.433	0.496	0.444	0.497	-0.011	0.355
Lowest income tertial	0.343	0.475	0.351	0.478	-0.008	0.459
Highest income tertial	0.097	0.296	0.084	0.278	0.013	0.064
Income missing	0.456	0.498	0.464	0.499	-0.008	0.483
Age below 30	0.279	0.449	0.290	0.454	-0.011	0.306
Age above 65	0.513	0.500	0.531	0.499	-0.017	0.149
Female	0.265	0.441	0.246	0.430	0.020	0.062
Foreign background	0.182	0.386	0.178	0.382	0.004	0.675
For. background missing	0.472	0.499	0.490	0.500	-0.018	0.142
StaySwed	0.555	0.497	0.549	0.498	0.006	0.610
N	3452		3454			

Note: p = p-value from test of equal means in treatment and control groups. The statistical tests are calculated on the subsample of individuals with non-missing information on all covariates. *StaySwed* indicates individual still alive and residing in the region at the intervention dates, excluding immigrants of non-Nordic origin who entered the population register after 2013. See Table 1 for definitions of other covariates.

Table 3: Main results

Panel A: Population-representative sample (PRS)

Treatment effect on switching rate after entire follow-up period (36 weeks)

	(1)	(2)	(3)	(4)
TreatmentWO	0.00574	0.00618	0.00740	0.00812*
	(0.00484)	(0.00483)	(0.00490)	(0.00488)
TreatmentW	0.00816***	0.00782***	0.00870***	0.00831***
	(0.00290)	(0.00288)	(0.00291)	(0.00289)
Constant	0.0568***	0.0292***	0.0560***	0.0289***
	(0.000722)	(0.00249)	(0.000722)	(0.00250)
Observations	112,858	112,858	111,487	111,487
R-squared	0.000	0.014	0.000	0.014
p joint W and WO	0.011	0.013	0.004	0.005
p W=WO	0.663	0.767	0.817	0.973
Covariates	No	Yes	No	Yes
Sample	ITT	ITT	*StaySwed*	*StaySwed*

Panel B: New residents (NR)

Treatment effect on switching rate after entire follow-up period (21 weeks)

	(1)	(2)	(3)	(4)
TreatmentW	0.0202***	0.0208***	0.0261***	0.0272***
	(0.00772)	(0.00770)	(0.00993)	(0.00989)
Constant	0.0884***	0.0920***	0.0772***	0.0995***
	(0.00518)	(0.0186)	(0.00640)	(0.0205)
Observations	6,905	6,905	3,812	3,812
R-squared	0.001	0.005	0.002	0.008
Covariates	No	Yes	No	Yes
Sample	ITT	ITT	*StaySwed*	*StaySwed*

Note: The table shows treatment effect estimates from linear probability models. Panel A covers the intervention directed to the population-representative sample (PRS) and Panel B covers the intervention directed to the new residents sample (NR). Columns 1-2 show estimates for the intention-to-treat (ITT) samples (i.e., no exclusions). Columns 3-4 show estimates for our preferred sample *StaySwed*, which excludes out-migrants, deceased individuals, and recent immigrants. In all specifications, the dependent variable is a dummy equal to 1 for individuals who switched care center at least once during the full follow-up period (36 weeks for PRS, 21 weeks for NR). *TreatmentW (WO)* = treatment arm with (without) a choice form attached to the information leaflet. Estimates in even-numbered columns are from specifications controlling for the covariates in Panel A and B of Table 2. *p joint W and WO* = p-value of test of null hypothesis that the coefficients on *TreatmentW* and *TreatmentWO* are jointly equal to zero. *p W=WO* = p-value of test of difference between the estimates on *TreatmentW* and *TreatmentWO*. For PRS, the estimates are weighted by the inverse of the probability of being sampled; such weights are irrelevant for NR as everyone has equal probability of being treated. Robust standard errors in parentheses (clustered by residential address in NR sample); *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

In the third and fourth columns, we use our preferred estimation sample (*StaySwed*), which excludes individuals that were not reached by the intervention or had limited capability to understand or react to it (individuals who died or left the region before the intervention was rolled out, and non-Nordic born individuals who had recently immigrated). This excludes 1,371 individuals, of which 1,088 are immigrants. As expected, the estimates on both *TreatmentW* and *TreatmentWO* are slightly higher than in the ITT sample.[27] The point estimates for the two treatment arms are remarkably similar, they are jointly significant, and both are individually statistically significant in the specification with covariates (p-value of *TreatmentWO* is 0.131 without covariates, 0.096 with covariates). Based on this specification, there is no strong reason to believe that the choice form played a crucial role for the increased propensity to switch.

Panel B shows the main results for the intervention directed to new residents (NR), in which all treated individuals received both the leaflet and the choice form. Despite a shorter follow-up period (21 weeks), the baseline switching rate is higher than in the population-representative sample, around 8-10 percent depending on sample. The new residents reacted stronger to the information intervention. The ITT effect of around 2 percentage points (column 1) implies a more than 23 percent increase in the switching rate compared to the control group, to be compared with 14 percent in the same treatment in the PRS. The difference aligns well with the idea that the new residents initially had lower knowledge about available care centers and their features. In columns 3-4 of Panel B, we show that the treatment effect increases to 2.7 percentage points when we exclude those who did not receive the information or were recent immigrants. This effect corresponds to a relative increase of about 35 percent.

Appendix B shows that our main results are stable to a range of sensitivity checks, such as using a logit specification and including strata fixed effects. The PRS results are also robust to removing sample weights and we find no indications of household spill-overs in this sample (which, in difference to the second intervention, was not clustered by residential address). Further, the conclusions are unchanged when we use randomization-based inference.

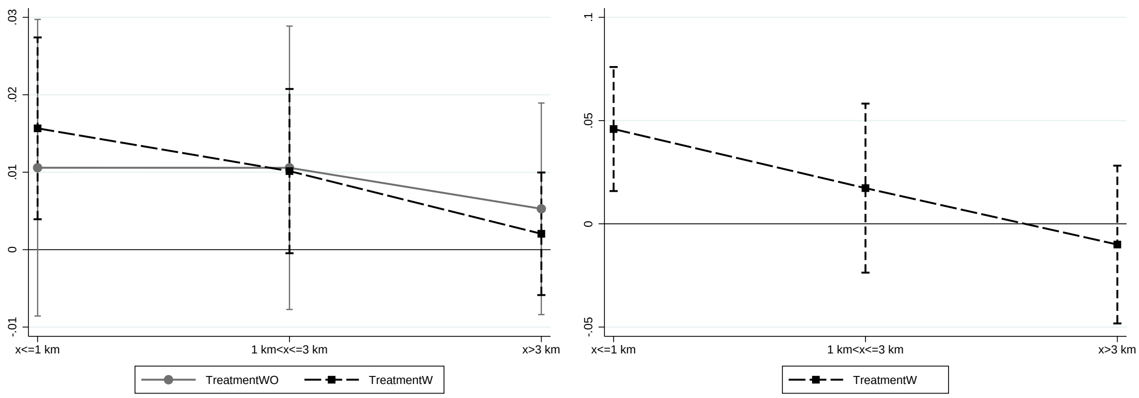
5.2 Heterogeneity

Especially for leaflet recipients residing in rural areas, the three alternative care centers presented on the leaflets might have been located too far away to be attractive options. Figure 1 illustrates heterogeneity in the treatment effect depending on how far the individual would have to travel from his or her residential address to reach the second closest provider.[28] The figure plots treatment effects and 95 percent confidence intervals estimated separately for three categories: individuals living less than 1 kilometre (km), between 1 and 3 km, or

care center once, and has made fewer than 5 visits to primary care. To retain individuals with missing information about covariates in the sample, we use dummy variables to indicate observations with missing values on covariates.

[27] In Appendix B.1, we show the impact of sequentially removing the subgroups.

[28] We obtain similar results when using other choice set definitions: 1) the distance between the individuals current provider and its closest competitor, and 2) the excess distance between the closest and the second closest provider (see Appendix C).



(a) Population-representative sample (b) New residents sample

Figure 1: Heterogenity in treatment effect by distance to the second closest alternative provider. The figure shows the treatment effects and 95% confidence intervals for subgroups defined by the straight-line distance between the individual's home and the second closest alternative primary care provider: less than 1 kilometre ($x \leq 1$ km), between 1 and 3 km (1 km $< x \leq 3$ km), or more than 3 km ($x > 3$ km). *TreatmentWO* (*TreatmentW*) = treatment arm without (with) attached choice form. Estimations on preferred sample (*StaySwed*) including covariates.

more than 3 km away from their second closest care center (straight-line distances).[29] The figure indicates that individuals with no alternative provider close to home did not respond to the intervention ($x > 3$ km). By contrast, for individuals with at least two care centers within 1 km, the relative treatment effect on switching rates was 23 percent (*TreatmentW*) and 15 percent (*TreatmentWO*) in the PRS, and as large as 48 percent in the NR.[30] For the interventions with choice forms, the difference between the two extreme categories are statistically significant. However, it should be noted that for the treatment without choice form, there is no obvious gradient and we lack power to detect significant treatment effects for any category.[31]

To explore socioeconomic and demographic heterogeneity, we estimate a specification in which all covariates are interacted with the treatment indicator(s). Thus we explore heterogeneity related to educational attainment, age, sex, foreign background and medical history. The results are shown in Appendix D and only summarized here. For the new residents, all interaction term are statistically indistinguishable from zero, individually as well as jointly ($p = 0.460$). Also for the population-representative sample, we cannot reject the null hypothesis that all interaction terms are jointly equal to zero ($p = 0.241$). That said, when tested variable-by-variable, the PRS displays an age gradient: the youngest age group reacted less strongly to the intervention, regardless of whether they received a choice form or

[29]The estimations in the figure use the preferred sample (*StaySwed*) and includes covariates, but the patterns are similar for the ITT samples and excluding covariates.

[30]Relative effects for the lowest category calculated by dividing the marginal effects by the unconditional switching rate in the lowest category (7.0 percent in PRS and 9.6 percent in NR).

[31]Appendix C shows the number of observations per category. For the intervention without choice form, there were 755, 714 and 1,041 individuals in each of the three treatment categories. According to the estimated treatment effects, the intervention made only a handful of individuals in each category switch.

not ($p = 0.0159$ in joint significance test of the interaction terms for both treatment arms). No other interactions are significant in covariate-by-covariate joint tests for the two treatment arms, and only one other interaction term is individually significant at the 10 percent level: the negative interaction term between foreign background and the treatment without choice form ($p = 0.091$). Foreign background is in fact one of the only two variables for which the interaction terms of the two treatment arms are statistically distinguishable: for the arm with the choice form, the interaction term is small, positive and insignificant.[32] Though one should not over-interpret this result, which derives from a small number of observations, the possibility that the choice form was crucial for individuals of foreign background aligns well with the observation that the effect of the treatment without choice form increased when we moved from the ITT to the *StaySwed* sample.

Overall, the results do not indicate striking socioeconomic or demographic heterogeneity in response to the treatment. However, it should be stressed that the opportunities to detect such heterogeneity are limited, as the treatment induced switches are relatively few in each subgroup, and our statistical power is consequently lower.

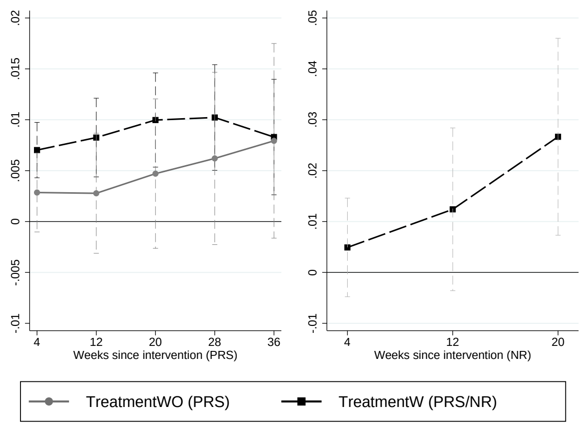
5.3 Mechanisms

To learn more about how and why the intervention affected individuals, Section 5.3.1 studies the timing of switches and how individuals switched. Section 5.3.2 examines if the increased switching rates of the intervention led to increased probability of choosing care centers on the leaflet (rather than other care centers). Section 5.3.3 discusses the potential reminder effect of information interventions. The reported estimations use the subsample of individuals who received and likely understood the information; that is, the *StaySwed* samples in columns 3-4 of Table 3. However, the patterns are similar for the ITT sample.

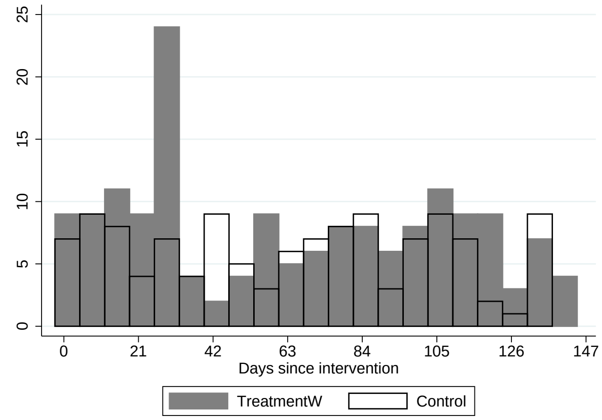
5.3.1 When and how did people switch?

Our detailed data on enrollment periods allow us to track when individuals switched. Figure 2a shows the treatment effect by week since the intervention (the model with covariates is estimated repeatedly using follow-up periods of 4 weeks, 12 weeks, etc). The figure shows that the increased switching rate relative to controls appeared immediately in both the PRS and NR, and grew over time. The accumulation over time is particularly clear for the new residents, suggesting that we might have obtained an even higher treatment effect with a 36-week follow-up period in the NR. On the other hand, in the PRS, the effect for *TreatmentW* did not increase between weeks 20-28, and there was a slight dip between week 28 and 36 (due to more switches in the control group). Nonetheless, the low rate of decline (and the continuing increase for *TreatmentWO*) suggests that the effect of the intervention was to make more people switch, not only to make people switch sooner. It is also notable that conditional on having switched at least once, the probability of switching back to the initial provider was the same for the treatment and control group, as was the probability of making further switches during the follow-up period (results not shown).

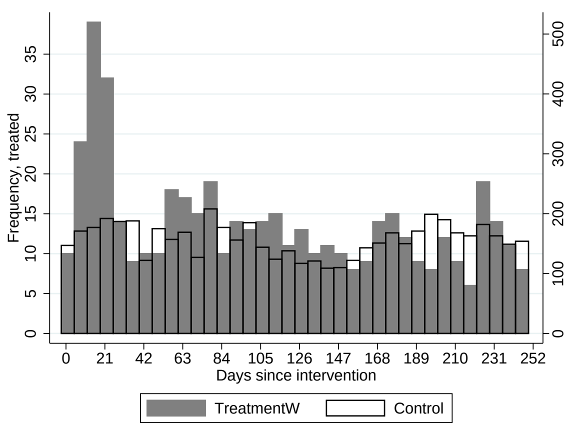
[32] The dummy for having made at least one active change of provider since 2009 is the only other variable where the interaction terms differ significantly between *TreatmentW* and *TreatmentWO*. There is a tendency that previous active changers are more sensitive to *TreatmentWO*.



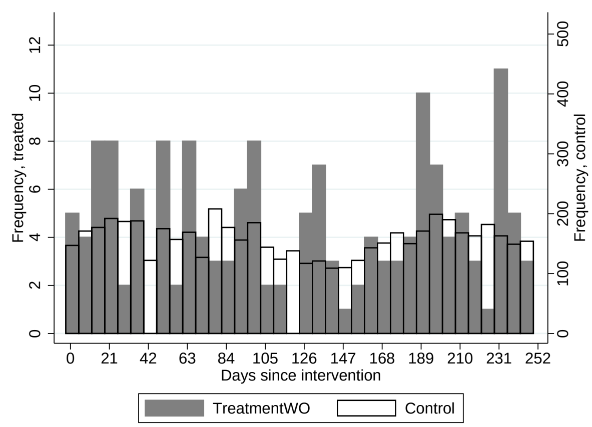
(a) Cumulative treatment effects (PRS & NR)



(b) Weekly n.o. switches (NR)



(c) Weekly n.o. switches (PRS)



(d) Weekly n.o. switches (PRS)

Figure 2: Treatment effects on switching rate over time. Figure (a) shows the cumulative treatment effect with 95 percent confidence interval estimated 4-36 weeks after the interventions; population-representative sample (PRS) in left panel and new residents (NR) in right panel. Estimations use the preferred samples with covariates. Figures (b)-(d): each bar shows the number of registered provider changes (on a weekly basis) plotted against the number of days since the intervention. For individuals who switched more than once, only the first change is counted. In (c)-(d), axis scales are adjusted to make the the bars comparable between the treatment and control groups (which are of different size in the PRS).

Figures 2b-d illustrate the timing of the switches by plotting the weekly number of individuals that made their first switch since the intervention.[33] For the two treatments with choice forms (Fig. 2 b and c), there are marked spikes a few weeks after the intervention. The lag between the interventions and the spikes reflects that the graphs are based on the date of registration of the provider changes. After individuals sent in their choice forms, these were first sorted centrally at the region and then sent to the respective care centers for registration. Thus, the lag between the interventions and the spikes is due to the administration of the sent in choice forms, rather than to a lag in individuals' responses to the intervention.[34]

There is no similar spike for the treatment group without a choice form (Figure 2d). Thus, although the total treatment effect on the switching rate was similar in the two treatment arms, they were not exactly equivalent: the choice form nudged individuals to switch instantly, whereas the treatment without choice form had a gradual impact. There are further some indications that the *TreatmentWO* group chose to switch online more often, though the difference to the other treatment group and the control group is not statistically significant (cf. Appendix E).

5.3.2 Did people switch to other centers on the leaflet?

Having established that the interventions affected the propensity to switch, it is interesting to examine what care centers people switched to. As the treatment reduced the search costs involved in making comparisons only with respect to care centers displayed on the leaflet, one might expect that the intervention made individuals more likely to select these centers. Another reason why centers on the leaflet might have benefited is that the information on the leaflet may have been presented in a more easily-comprehended way than on the website 1177.se. On the other hand, it is possible that individuals were well-informed at the outset about their initial center's closest competitors, and that the intervention spurred people to visit 1177.se to learn about other care centers than those on the leaflet.

In Table 4, we decompose the effect of the treatment into switches to centers included on the leaflet and switches to other centers. The results show that the treatment primarily affected the likelihood to switch to a center on the leaflet. In the first and third columns, the dependent variable is a dummy attaining the value of one for individuals who, when they were last observed during the follow-up period, had switched from their initial provider to one of the other three care centers presented on the leaflet.[35] In the second and fifth column, the dependent variable is a dummy indicating a shift to a center not on the leaflet.

The effect on the likelihood to be enrolled at a center included on the leaflet equals 0.6-0.7 percentage points in the population-representative sample (column 1) and 1.6 percentage points among the new residents (column 3), and is statistically significant in both samples.

[33]The picture is similar if we instead plot the frequencies of *all* (i.e. not only first) switches, indicating that the treatments primarily affected the binary decision of switching provider.

[34]Appendix E shows that the attached choice forms were returned and registered by the postal service very soon after the intervention. Note that the NR leaflets were sent out right before the summer holidays, explaining the longer lag before switches were registered compared to the PRS. Changes taking place online or at care centers are registered instantaneously.

[35]The variable is measured at the end of the follow-up period for individuals still residing in the region at that date. To retain the same estimation sample as before, we use last known registration of individuals who left the region or died during follow-up.

The effect on the likelihood of being enrolled at another center is smaller (0.1-0.2 percentage points in PRS and 1.2 percentage points in NR), and it is insignificant in both samples.

Thus, these estimations show that the intervention primarily increased the propensity to switch to one of the other care centers on the leaflet. In column 3 and 6, we directly examine if the treatment affected the proportion of switchers that choose to enroll at a center included on the leaflet. That is, we exclude individuals who did not switch provider from the samples and estimate the probability of having switched to another center of the leaflet rather than to a center not on the leaflet.[36] Indeed, the estimates in column 3 show that for the PRS, the intervention made centers on the leaflet relatively more popular among switchers. The increase for the new residents is of similar magnitude but insignificant (column 6).[37]

The increase in popularity of centers on the leaflet has some bearing on the issue of whether the treatment effect on switching was only due to a reminder effect, or if the information was important *per se*. The fact that centers on the leaflet became relatively more popular among switchers affected by treatment is hard to explain by a pure reminder effect, and more easily reconciled with the interpretation that the leaflets reduced the search cost for information. However, it cannot be ruled out that the subset of individuals susceptible to a pure reminder effect had different preferences than individuals who would have switched anyway. The next section discusses the scope for reminder effects further.

5.3.3 Reduced information frictions and switching costs, or just a reminder?

It is possible that the advent of receiving a leaflet by itself was sufficient to affect the switching rate to the degree that we observe. Or perhaps the treatment effect was due partly to the information, partly to the reminder. As everyone in the treatment groups received comparative information, we cannot disentangle the information and reminder effects using the experimental data. To gauge the importance of a reminder effect, we instead examine previous reminders of the opportunity to choose provider. The regional health authority has on several occasions mailed out such reminders, but have never before included comparative information about providers. If earlier reminders were followed by unusually high switching rates, then it reasonable to assume that our treatment effects overestimate the importance of improved access to comparative information.

The black solid line in Figure 3 shows weekly switching rates around the publication dates of a magazine distributed by the regional health authority to all residents in 2014.[38] That year, the first two issues of the magazine – published in June and September, respectively – included advertisements of the opportunity to freely choose of provider. The grey dashed line shows the switching rates in the corresponding calendar period in 2013. Only one issue of the magazine was published during this period (in May), and that issue did *not* mention the choice system at all.[39] The graphs show no sign of elevated switching rates in the period

[36]Some individuals switched more than once during the follow-up period. The results are very similar if we instead consider what care center individuals were enrolled at after their first switch.

[37]This may relate to statistical power. Only 344 individuals in the preferred NR sample switched provider; when running the same specification on the ITT sample, in which 680 individuals switched, the estimate is statistically significant (p=0.039) and larger than in column 6 ($\beta = 0.092$).

[38]The switching rates are calculated using the PRS sample. To avoid openings and closures of centers to affect the switching rates, individuals enrolled at newly opened or closed centers are excluded.

[39]Distribution dates in 2014: June 17, September 5, and December 10. Distribution dates in 2013: May

Table 4: Did people switch to other centers on the leaflet or not?

	PRS			NR		
	(1)	(2)	(3)	(4)	(5)	(6)
TreatmentWO	0.00728**	0.000817	0.0788**			
	(0.00315)	(0.00401)	(0.0386)			
TreatmentW	0.00570***	0.00201	0.0440**	0.0161**	0.0118	0.0735
	(0.00181)	(0.00241)	(0.0220)	(0.00750)	(0.00844)	(0.0571)
Constant	0.0109***	0.0174***	0.336***	0.0689***	0.0435***	0.635***
	(0.00150)	(0.00213)	(0.0224)	(0.0156)	(0.0167)	(0.117)
Observations	111,487	111,487	6,326	3,812	3,812	344
R-squared	0.005	0.013	0.028	0.009	0.010	0.066
P joint W and WO	0.001	0.695	0.020			
P W=WO	0.660	0.795	0.424			
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Sample	*StaySwed*	*StaySwed*	*StaySwed*	*StaySwed*	*StaySwed*	*StaySwed*
Subsample	All	All	Switchers	All	All	Switchers
Outcome variable	*Other* on leaflet	*Not* on leaflet	*Other*	*Other*	*Not*	*Other*

Note: Outcome variable in columns 1, 3, 4 and 6: dummy for individuals who, when last observed, were enrolled at one of the other three centers on the leaflet (not the initial). Outcome variable in columns 2 and 5: dummy indicating individuals who, when last observed, were *not* enrolled at any of the four centers on the leaflet. PRS estimates are weighted by the inverse of the probability of being drawn. Robust standard error in parentheses (clustered by center defining the leaflet for NR). *** p<0.01, ** p<0.05, * p<0.1. *P W=WO* is the p-value on test of equality of coefficients on *TreatmentW* and *TreatmentWO*. *P joint W and WO* is the p-value of a joint significance test for *TreatmentW* and *TreatmentWO*.

after the publication of the magazine in 2014. All changes are comparable or smaller than the fluctuations in 2013.

There are other reasons why the reminder effect of our intervention should be small. Survey evidence indicates that the right to choose care provider is well-known in Sweden (Swedish Agency for Health and Care Services Analysis, 2013),[40] indicating that the effect of reminding people that they can choose is small. We would mainly expect noticeable reminder effects on recent immigrants – who are not in our preferred sample, and thus do not drive the effect – and on individuals who have recently considered switching. The prime example of the latter category might be individuals who have recently moved within the region, but have not yet changed provider (due to inertia, for instance). In Appendix F, we show that the baseline effect of the treatment with choice form is not driven by such individuals. That the treatment effect increases over time for some treatment groups also seems inconsistent with a pure reminder effect (cf. Fig. 2a).

Moreover, everyone in the new residents sample received a welcome letter from the health authority recently before our intervention. The letter announced the name of the individual's default provider and mentioned the right to choose care center, but did not contain any more information about providers. If the marginal impact of reminders is decreasing, which

31 and December 4. The December 2013 issue included a notice of free choice. Therefore, the analysis above only considers the May and September 2014 issues.

[40]In this survey, which was based on a nationally representative sample, 91 percent of respondents who had never switched and did not consider to do so were aware of the right to choose (Swedish Agency for Health and Care Services Analysis, 2013).

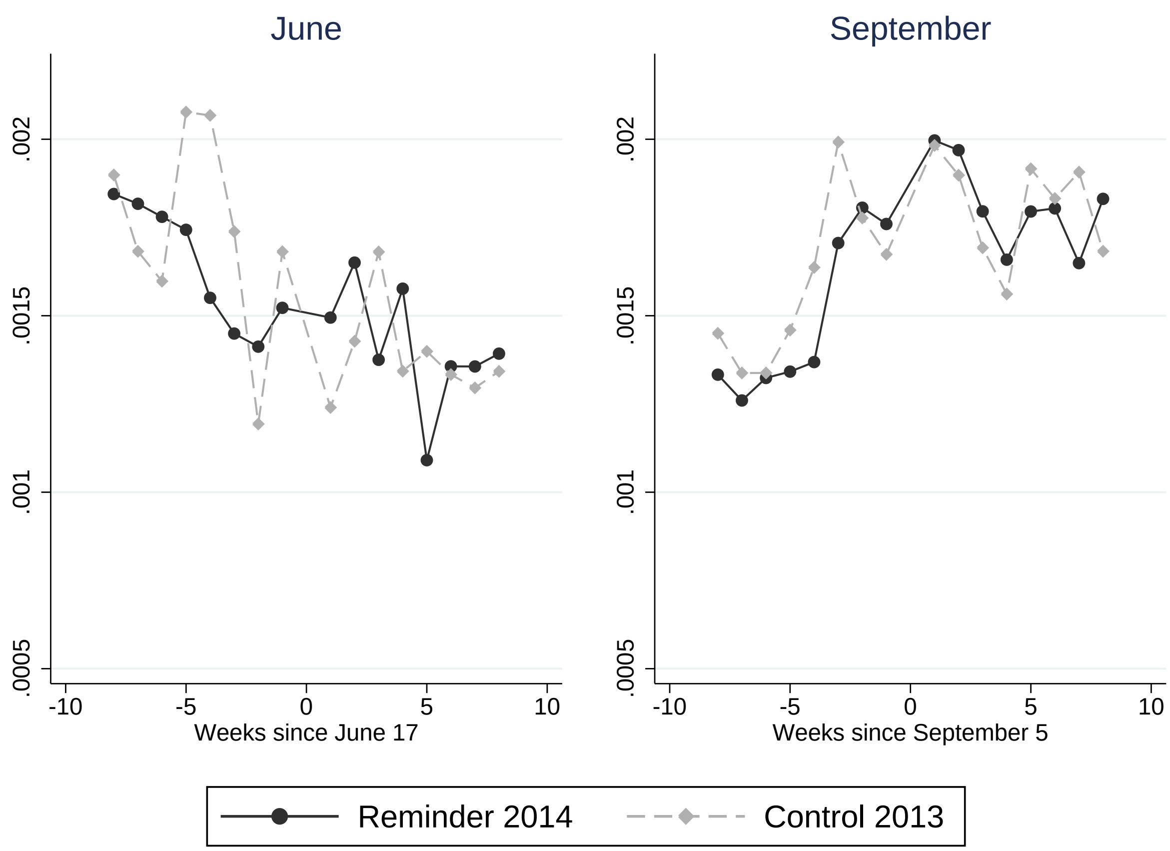


Figure 3: Switching rates around the time of earlier reminders. The black lines show weekly switching rates 8 weeks before and 8 weeks after the regional health authority's magazine was mailed out to the whole population in 2014. The June and September issues of the magazine included advertisements of the free choice of provider. On the horizontal axis, 0 denotes the distribution date of the magazine (June 17 or September 5, respectively). The grey dashed lines show weekly switching rates around the same dates in 2013, when no similar reminders were mailed out. Switching rates are calculated using the historical enrollment data of the population-representative sample.

appears reasonable,[41] the impetus of a second reminder (our intervention) arriving shortly after the welcome letter should be small. As the regional health authority has on several occasions issued reminders to the general population (e.g., the aforementioned magazines and several advertisement campaigns), this argument pertains to the population-representative sample as well.

6 Concluding remarks

We provide the first evidence that frictions on the demand side in the market for primary care can be mitigated, and switching rates increased, by providing individuals with comparative information about providers and eliminating certain small switching costs. A contribution of the study is to show that such frictions can be mitigated even in circumstances when the service is multi-faceted, and thus not well-described by a single indicator. In contrast to previous information interventions that have significantly affected switching rates (Hastings and Weinstein, 2008; Kling et al., 2012), our information material included a richer set of provider characteristics. Our intervention thus acknowledged both the complexity of primary care services and health authorities' imperfect information about individuals' preferences; two features that characterize many consumer choice settings, and are indeed among the core arguments for consumer choice arrangements.

The relative increases of the switching rates in our main analyses range from 10 to 35 percent. The effect is larger for new residents than for the general population. This aligns well with the hypothesis that new residents have less experience with the primary care market, a smaller network that can supply information, and weaker ties to their current provider. The effect is moreover driven by individuals living within a reasonable distance to alternative providers. This result underscores the limited potential for consumer choice to spur care quality improvements in sparsely populated areas. It also suggests that similar interventions might have stronger impact in areas more dense than the current study region.

We find little evidence of socioeconomic or demographic heterogeneity in the treatment effects, although we are reluctant to view this result as conclusive, given our limited statistical power in this regard. One exception is that younger individuals who already resided in the region reacted less than middle-aged or elderly residents. This may seem dissonant with the stronger treatment effect among new residents, of which almost half are below 30 years of age. However, there are plausible reasons, such as lower background knowledge and weaker ties to their current provider, that may explain why new residents reacted stronger to the intervention.

Of the two components in the intervention, the increased information accessibility appears to have been more important than the reduced switching costs, as the choice form mainly affected the timing of switches. This is not to say that switching costs are unimportant. Our intervention did not affect the perhaps most important source of switching costs: discontinuing established relationships with physicians and nurses. Such switching costs may present important remaining obstacles for improving the functioning of the primary care market.

[41] Recall the null effect on switching of the intervention in Ericson et al. (2017), which was the third reminder arriving in a window of two months.

One may ask whether we would have seen the same effects, even if we had not included any comparative information. We present several arguments against substantial reminder effects, but the experimental design does not allow us to completely dismiss this channel. Addressing this experimentally is more challenging than it may seem, due to the possibility of a gradient in the reminder effect. If, for instance, low quality ratings are more salient than high ratings (Heiss et al., 2016), then the strength of the reminder effect depends on the content of one's leaflet. To disentangle the information and reminder effects when the latter depend on quality would require far larger samples than what was attainable in our setting.

A remaining question is whether individuals switched to providers offering higher quality. Because consumers have heterogeneous preferences over primary care services, it is not trivial to examine how the subjective valuations of provider characteristics changed after the interventions. A promising way forward would be to estimate a structural demand model, and use the parameter estimates to examine if the interventions made individuals switch to providers with characteristics that are highly valued by other patients. Such work on the determinants of demand may also shed light on the potential of information interventions to spur quality competition among providers. Our experimental results alone do not answer the question of whether similar interventions scaled up to market level would have induced enough extra mobility to induce competition.

Ultimately, we would like to know whether improved access to information about health care providers affects individuals' health. Such effects, if they exist, may take years to appear, and are therefore outside the scope of this paper.

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A Leaflet example

On the subsequent two pages we show an example of a leaflet from the treatment arm including choice form in the intervention to the population-representative sample. The leaflet was a folded paper in A4 format, with the comparative information about centers printed on the centerfold. On the next page, the left margin shows the leaflet's back page and the right margin shows the front page. The page thereafter shows the centerfold.

The information in the centerfold was the same in the two experiments, with the exception that some quality indicators were updated before the intervention to new residents, and that the information about opening hours during non-office hours was somewhat more detailed.

The layout of the leaflet was precisely the same in all interventions. Right after the example leaflet, we provide English translations of the texts printed on the leaflets in each intervention.

Så här byter du vårdcentral:

- genom att lämna in den bifogade blanketten till den vårdcentral du vill lista dig hos, eller skicka den med posten (porto är betalt)
- genom Region Skånes e-tjänst Mina vårdkontakter

Mer information om Mina vårdkontakter samt möjlighet att skriva ut valblanketter finns på 1177.se:

www.1177.se/Skane/Regler-och-rattigheter/Valja-vardcentral/

På 1177.se kan du också jämföra fler vårdcentraler via tjänsten ”Hitta och jämför vård”.

Om du inte gör något nytt val står du kvar som listad på din nuvarande vårdcentral.

Region Skåne
291 89 Kristianstad
Telefon: 044-309 30 00
Skane.se

Du vet väl om att du kan välja?

Du som bor i Skåne är listad på en vårdcentral. Du kan själv välja vilken vårdcentral du vill gå till. Vårdcentral väljer du genom att lista dig och du kan när som helst byta till en annan vårdcentral.

Du behöver inte vara listad vid den vårdcentral som ligger närmast ditt hem. Du kan till exempel välja en som ligger nära ditt jobb eller en som erbjuder en verksamhet som passar dig och dina behov.

För att du ska kunna hitta den vårdcentral som passar dig bäst är det viktigt att jämföra olika vårdcentraler med varandra. På nästa sida hittar du information om den vårdcentral som du är listad hos idag och de tre vårdcentralerna som ligger närmast denna.

	Berga Läkarhus	Vårdcentralen Stattena	Vårdcentralen Drottninghög	Vårdcentralen Tågaborg
Adress	Rundgången 26 25452 Helsingborg	O D Krooks g. 53 25443 Helsingborg	Blåkullag. 11C 25457 Helsingborg	Tågag. 38 25439 Helsingborg
Telefonnummer	042-15 50 00	042-406 04 00	042-406 02 20	042-406 08 20
Filial	Nej	Nej	Nej	Nej
Ägare	Privat	Region Skåne	Region Skåne	Region Skåne
Antal listade	9 961	10 305	8 598	5 472
Öppettider (besök)	Må-Fr 8-17	Må-Fr 8-17	Må-Fr 8-17	Må-Fr 8-17
Jourtider (besök och telefon)	Lö-Sö 10-15	Må-Fr 17-20 Lö-Sö 10-20	Må-Fr 17-20 Lö-Sö 10-20	Må-Fr 17-20 Lö-Sö 10-20
Jourvårdcentral	Lö: Ödåkra Läkargrupp Sö: på vårdcentralen	Sjukhusområdet i Helsingborg	Sjukhusområdet i Helsingborg	Sjukhusområdet i Helsingborg
<u>Rekommenderas av andra?</u>				
Patientomdöme från 0 till 100*	87	80	69	75
<u>Hur upplevs väntetiden för att få träffa en läkare?</u>				
Patientomdöme från 0 till 100*	75	70	70	66
<u>Är det enkelt att få kontakt via telefon?</u>				
Andel telefonsamtal som besvaras inom 2 timmar	87%	93%	90%	98%
<u>Får du träffa samma läkare?</u>				
Andel patienter som fått träffa samma läkare vid minst hälften av sina besök**	76%	52%	54%	48%
<u>God läkemedelsförskrivning för äldre?</u>				
Uppfyller vårdcentralen Region Skånes mål?	Nej	Nej	Ja	Nej
<u>Vårdcentralen erbjuder även</u>				
Minnesmottagning (demensutredning)	✓	✓	✓	
Äldremottagning				
Astma/KOL-mottagning	✓	✓		
Hjärtsviktsmottagning		✓	✓	
Psykolog				
Gynekolog		✓	✓	
Kiropraktor				
Naprapat	✓			
<u>Inom 100 meter från vårdcentralen finns även:</u>				
Barnavårdcentral	✓	✓	✓	✓
Barnmorskemottagning	✓			

* Patientomdömena kommer från Nationell patientenkät. Omdömena mäts på en skala där 0 är sämsta möjliga utfall och 100 är bästa möjliga. För mer information, se "Hitta och jämför vård" på 1177.se.

** Gäller patienter som gjort tre eller fler besök senaste året.

Translation of leaflet: PRS treatment with choice form

Front page:

Do you know that you have a choice?

As a resident in Skåne, you are listed at a care center. You may choose which care center you want to go to. You choose a healthcare center by listing, and you can switch to another at any time.

You do not have to be listed at the healthcare center closest to your home. For example, you can choose a healthcare center close to your job or one that offers services that suit you and your needs.

In order to find the care center that suits you best, it is important to compare different care centers with each other. On the next page you will find information about the care center you are listed at today and the three care centers closest to it.

Back page:

How to change care center:

- submit the attached form to the care center you wish to register with, or send it by mail (postage is paid).
- use Region Skåne e-service My healthcare contacts.

More information about My Care Contacts is available at 1177.se, where you can also print a choice form:

www.1177.se/Skane

At 1177.se, you also compare more health centers via the "Find and compare care" service. If you do not make a new choice, you will remain listed at your current care center.

Translation of leaflet: PRS treatment without choice form

Front page:

Do you know that you have a choice?

As a resident in Skåne, you are listed at a care center. You may choose which care center you want to go to. You choose a healthcare center by listing, and you can switch to another at any time.

You do not have to be listed at the healthcare center closest to your home. For example, you can choose a healthcare center close to your job or one that offers services that suit you and your needs.

In order to find the care center that suits you best, it is important to compare different care centers with each other. On the next page you will find information about the care center you are listed at today and the three care centers closest to it.

Back page:

How to change care center:

- hand in (directly or by mail) a choice form to the care center you wish to register with.
- use Region Skåne e-service My healthcare contacts.

More information about My Care Contacts is available at 1177.se, where you can also print a choice form:

www.1177.se/Skane

At 1177.se, you also compare more health centers via the "Find and compare care" service. If you do not make a new choice, you will remain listed at your current care center.

Translation of leaflet: New residents

Front page:

Do you know that you have a choice?

As a resident in Skåne, you can choose which care center you want to go to. You choose a healthcare center by listing. If you do not make an active choice, you are automatically listed at the healthcare center closest to your home. When you moved to Skåne you received a letter indicating which care center that is.

You do not have to be listed at the healthcare center closest to your home, and you can switch to another at any time. For example, you can choose a healthcare center close to your job or one that offers services that suit you and your needs.

In order to find the care center that suits you best, it is important to compare different care centers with each other. On the next page you will find information about the care center you are listed at today and the three care centers closest to it.

Back page:
How to change care center:

- submit the attached form to the care center you wish to register with, or send it by mail (postage is paid).
- use Region Skåne e-service My healthcare contacts.

More information about My Care Contacts is available at 1177.se, where you can also print a choice form:

www.1177.se/Skane

At 1177.se, you also compare more health centers via the "Find and compare care" service. If you do not make a new choice, you will remain listed at your current care center.

Translation of leaflet: Centerfold (all treatment arms)

- Address
- Phone number
- Owner
- Number of listed patients
- Regular opening hours (visits)
- Opening hours during non-office hours
- Non-office hour care center
- Recommended by others?
  - Patient rating from 0 to 100*
- Perceptions of waiting time to see a doctor?
  - Patient rating from 0 to 100*
- Is it easy to contact the care center by phone?
  - Share of calls that are answered within 2 hours
- Will you see the same doctor?
  - Share of patients who have seen the same GP on at least half of previous visits
- Appropriate drug prescriptions to elderly?
  - Does the care center fulfil Region Skåne's targets?
- The care center also offers:
  - Memory clinic (dementia investigation)

  - Elderly clinic
  - Asthma/COPD clinic
  - Heart failure clinic
  - Psychologist
  - Gynecologist
  - Chiropractor
  - Naprapath

- Within 100 metres from the care center, there is also:
  - Child health center
  - Midwife clinic

B Robustness of main specification

B.1 Sequential restriction of sample

In the main text, Table 3 shows the results for the ITT sample and the restricted sample *StaySwed*. In Table B.1, we show the impact of sequentially removing each of the subgroups making up the difference between the *StaySwed* and the ITT sample. For the population-representative sample in Panel A, column 1 excludes 137 observations who left the region or died before we even identified their current address. No leaflet was sent to these individuals. In column 2, we additionally exclude the 146 individuals who were de-registered between the address extraction date and the first day of the week when the leaflets were submitted. A leaflet was sent to these individuals, but they ought not have reacted to it as they had either died or left the region. In column 3, we additionally remove 1,088 non-Nordic immigrants, i.e., this is the same estimations as column 4 in the main result Table 3. In the final column, we furthermore exclude 757 observations for whom the database holding company had no information about geographic coordinates. It is possible that these individuals had another postal address registered and thus received the leaflet to that address; therefore, it is conservative to retain them in the sample rather than to view them as untreated. Notably though, the effect in column 4 is larger and more precisely estimated than that in column 3.

For the new residents in Panel B, 102 individuals left the region before the leaflets were submitted; these are excluded from the estimation presented in column 1 of Panel B. In column 2, we additionally exclude 32 individuals whose letters were returned to sender by the postal service. In column 3, we repeat the estimates presented in main text, i.e. where all remaining recent immigrants (non-Nordic in 2014; of any origins in 2015, as no background was information available for these immigrants) are excluded from the sample. In column 4, we additionally remove 420 individuals with missing coordinates.

B.2 Randomization inference

In a recent paper, Young (2016) suggests that regular inference methods may be problematic even in experiments with relatively large sample sizes. As an alternative mode of inference, we estimate our main regression specifications (Table 3) with Stata's *permute* command

Table B.1: Sequential sample limitation

Panel A: Population Representative Sample (PRS)				
Treatment effect on switching rate after 36 weeks				
	(1)	(2)	(3)	(4)
TreatmentWO	0.00636	0.00656	0.00812*	0.00838*
	(0.00483)	(0.00484)	(0.00488)	(0.00490)
TreatmentW	0.00786***	0.00796***	0.00831***	0.00850***
	(0.00289)	(0.00289)	(0.00289)	(0.00291)
Constant	0.0289***	0.0289***	0.0289***	0.0294***
	(0.00250)	(0.00250)	(0.00250)	(0.00250)
Observations	112,721	112,575	111,487	110,730
R-squared	0.014	0.015	0.014	0.014
p W=WO	0.787	0.801	0.973	0.983

Panel B: New Residents (NR)				
Treatment effect on switching rate after 21 weeks				
	(1)	(2)	(3)	(4)
Treatment	0.0209***	0.0201**	0.0272***	0.0234**
	(0.00781)	(0.00782)	(0.00989)	(0.0105)
Constant	0.0943***	0.0922***	0.0995***	0.101***
	(0.0188)	(0.0188)	(0.0205)	(0.0219)
Observations	6,803	6,771	3,812	3,392
R-squared	0.005	0.005	0.008	0.007

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Main specification (including covariates) on following samples: **Panel A**: col 1 excludes 137 obs who left/died after randomization but before extraction of address information. Col. 2 also excludes 146 obs. who left the region before the leaflets were posted. Col. 3 (same as preferred specification in Table 3) also excludes non-Nordic immigrans. Col. 4 excludes individuals with no coordinate information. *p W=WO* = p-value of test of difference between the estimates on *TreatmentW* and *TreatmentWO*. **Panel B**: col 1 excludes 102 individuals who died or left the region before the leaflets were posted. Col. 2 also excludes 32 whose leaflets were returned to sender. Col. 3 is the *StaySwed* sample (preferred specification in Table 3). Col. 4 additionally excludes 420 observations without coordinate information.

using 2,000 replications.[42] After each permutation of the data, Eq. (1) is estimated (with or without covariates included), thus yielding 2,000 treatment effect estimates. A p-value is given by the share of estimated treatment effects that are larger (in absolute value) than the treatment effect estimated on the "real" data. For the population-representative sample, in which there were two treatment arms, we also test the joint significance of *TreatmentW* and *TreatmentWO*. To do this in a simple way, we run the *permute* command on the Frisch-Waugh-Lovell regression (i.e., using residuals from regressions of the dependent variable and each treatment variable, respectively, on the covariates); the test statistic for joint significance is the Wald F-statistic of the FWL regression.

Table B.2 shows results from the randomization tests. Below each treatment effect estimate/F statistic (the same as in the main specifications), three statistics are shown: the estimated p-value and the upper and lower bounds of the 95% confidence interval around the estimated p-value. The randomization inference yield results that are close to the standard inference used in the main text. Notably, the estimated p-value for the treatment without choice form in the preferred sample (*StaySwed*) is very close to the 10% threshold even when covariates are not included ($p = 0.104$, as compared to $p = 0.134$ with standard statistical inference on this specification). For both modes of inference, the p-value is below 0.10 when covariates are included.

B.3 Other sensitivity tests

Table B.3 shows sensitivity tests of the main specification (i.e., estimations including covariates on the *StaySwed* samples). Column 1 shows the preferred specification for the PRS. Compared to the preferred specification for the PRS in columns 1, column 2 shows results from an estimation without sample weights and column 3 shows a specification including strata fixed effects for the individual's initial care center. The results are hardly affected by those specification changes. The final two columns display logit estimates for both experiments. The logit results, which are presented as odds ratios, are similar to the relative effects found in the linear probability model: for instance, the 1.14 odds ratio on *TreatmentWO* in column 4 is comparable to the 14.5 percent increase found in the LPM (to see this, divide the estimate of 0.00812 in column 1 with the unconditional mean of 0.056; see column 3 of main results Table 3), and the odds ratio of 1.4 for the new residents (column 5) is similar to the relative effect of 35 percent in the main results (0.0272/0.0772).

In Table B.4, we explore potential spill-over effects of the PRS intervention, which was not clustered on residential address. The first column shows the baseline results and the following columns show estimations excluding observations that might be affected by spillover, using the following definitions of spillover: Column 1 includes only observations with no duplication in terms of residential address. Column 2 include an observation only if all/non duplications are treated. Column 3 include treated observations only if there are non-treated duplications at same address. Column 4 include only observations if all duplications with the same address are in the same treatment group; and include i) all observations for which all duplications in the same address are in the same treatment group and ii) all observations for which all duplications in the same within address are in the same treatment group. The results are

[42] The syntax used is *permute y "regress y x" _b e(F), reps(2000)*

Table B.2: Randomization inference on main specifications (Table 3)

Panel A: Population Representative Sample (PRS)	(1)	(2)	(3)	(4)
TreatmentWO	0.006	0.006	0.007	0.008
p-value	0.224	0.186	0.104	0.086
Lower 95 CI	0.206	0.169	0.090	0.074
Upper 95 CI	0.243	0.204	0.118	0.099
TreatmentW	0.008	0.008	0.009	0.008
p-value	0.005	0.003	0.002	0.003
Lower 95 CI	0.002	0.001	0.001	0.001
Upper 95 CI	0.009	0.006	0.005	0.007
Joint W and WO	5.023	4.840	6.076	5.958
p-value	0.007	0.006	0.001	0.002
Lower 95 CI	0.004	0.003	0.000	0.001
Upper 95 C	0.012	0.010	0.004	0.005
N	112,858	112,858	111,487	111,487
Sample	ITT	ITT	*StaySwed*	*StaySwed*
Covariates	No	Yes	No	Yes

Panel B: New residents (NR)	(1)	(2)	(3)	(4)
Treatment	0.020	0.021	0.026	0.027
p-value	0.005	0.004	0.005	0.004
Lower 95 CI	0.002	0.002	0.002	0.001
Upper 95 CI	0.009	0.008	0.009	0.007
Observations	6,906	6,906	3,812	3,812
Sample	ITT	ITT	*StaySwed*	*StaySwed*
Covariates	No	Yes	No	Yes

Note: Randomization inference for the preferred specifications. Each of the regressions in Table 3 have been permuted 2,000 times. *p-value* = estimated p-value, i.e., the share of replications when the absolute value of the estimated statistic exceed the estimate of the main specification. *Lower (Upper) 95 CI* = lower (upper) bound of the 95% confidence interval around *p-value*. Columns 1 and 2 show results for the regressions on the intention-to-treat (ITT) sample with and without out covariates, respectively. Columns 3-4 shows results for the regressions on the preferred sample (*StaySwed*), with and without covariates respectively. Panel A shows results for the population-representative sample and Panel B shows results for the new residents sample. *Joint W and WO* is the Wald statistic for the test of joint significance of *TreatmentW* and *TreatmentWO*, the two arms in the intervention to the population-representative sample.

Table B.3: Sensitivity test, PRS and NR

	(1)	(2)	(3)	(4)	(5)
TreatmentWO	0.00812*	0.00813*	0.00818*	1.160*	
	(0.00488)	(0.00488)	(0.00484)	(0.0970)	
	0.0958	0.0956	0.0906	0.0752	
TreatmentW	0.00831***	0.00831***	0.00819***	1.154***	1.403***
	(0.00289)	(0.00289)	(0.00288)	(0.0567)	(0.171)
	0.00409	0.00409	0.00447	0.00355	0.00538
Constant	0.0289***	0.0289***	0.0296***	0.0332***	0.108***
	(0.00250)	(0.00250)	(0.00847)	(0.00168)	(0.0270)
	0	0	0.000468	0	0
Observations	111,487	111,487	111,487	111,487	3,812
R-squared	0.014	0.014	0.025		
$pW = WO$	0.973	0.974	0.999	0.955	

Note:Column (1): Baseline specification for PRS on *StaySwed* sample; (2) Remove sample weights, PRS; (3) Add strata (care center) fixed effects, PRS; (4) Logit, PRS; (5) Logit, NR. Odds ratios reported for logit models. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

consistent with the preferred specification.

Table B.4: Spillover analysis, population-representative sample

	(1)	(2)	(3)	(4)	(5)	(6)
TreatmentWO	0.00812*	0.00745	0.00872*	0.00810*	0.00810	0.00776
	(0.00488)	(0.00516)	(0.00516)	(0.00488)	(0.00515)	(0.00488)
	0.0958	0.149	0.0908	0.0968	0.116	0.112
TreatmentW	0.00831***	0.00864***	0.00913***	0.00828***	0.00774***	0.00829***
	(0.00289)	(0.00310)	(0.00307)	(0.00290)	(0.00291)	(0.00290)
	0.00409	0.00534	0.00295	0.00423	0.00773	0.00423
Constant	0.0289***	0.0288***	0.0290***	0.0290***	0.0293***	0.0289***
	(0.00250)	(0.00266)	(0.00252)	(0.00252)	(0.00251)	(0.00250)
Observations	111,487	97,558	108,968	110,104	110,433	111,438
R-squared	0.014	0.014	0.014	0.014	0.014	0.014
$pW = WO$	0.973	0.841	0.945	0.974	0.951	0.926

Note: Column (1) shows the baseline specification on the *StaySwed* sample. The following columns exclude observations potentially affected by spillover effects according to the following definitions: (2) include only observations with no duplication in terms of residential address; (3) include observation only if all/non duplications are treated; (4) include treated observations only if there are non-treated duplications at same address; (5) include only observations if all duplications with the same address are in the same treatment group; (6) include i) all observations for which all duplications in the same address are in the same treatment group and ii) all observations for which all duplications in the same within address are in the same treatment group. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

C Alternative definitions of proximity to providers

In the main text, we illustrated how the treatment effect depends on the individual's proximity to providers, as defined by the straight-line distance between the individuals residential address and the second closest care center. Individuals were classified in three categories, depending on whether this distance was ≤ 1 km; 1 km$< x \leq 3$ km; $x > 3$ km. Here, we first consider a second definition of proximity, based on distances between care centers; specifically, the straight-line distance between the care center the individually initially was enrolled at and the closest competitor of that center (≤ 1 km; 1 km$< x \leq 3$ km; $x > 3$ km). We also consider a third definition, which uses the *excess* straight-line distance the individual would have to travel from home to reach her second closest (rather than her closest) care center (≤ 1 km; 1 km$< x \leq 3$ km; $x > 3$ km). The tables below show the number of observations per category and treatment group for all three choice set definitions.

Table C.1: Proximity def. 1: distance between individual's home and the 2nd closest care center

	Population-representative				New residents		
Category	*Control*	*TreatmentWO*	*TreatmentW*	Total	*Control*	*Treatment*	Total
$x \leq 1$ km	30,705	755	2,286	33,746	814	797	1,611
1 km$< x \leq 3$ km	28,923	714	2,181	31,818	418	427	845
$x > 3$ km	41,030	1,041	3,095	45,166	480	456	936
Total	100,658	2,510	7,562	110,730	1,712	1,680	3,392

Note: The table shows the number of observations per choice set category for the *StaySwed* samples. Choice set defined by the distance (x) between individual's home and second closest care center.

Table C.2: Proximity def. 2: distance between current care center and its closest competitor

	Population-representative				New residents		
Category	*Control*	*TreatmentWO*	*TreatmentW*	Total	*Control*	*Treatment*	Total
$x \leq 1$ km	50,947	1,263	3,817	56,027	1,126	1,111	2,237
1 km$< x \leq 3$ km	20,778	523	1,558	22,859	376	394	770
$x > 3$ km	29,478	735	2,217	32,430	410	386	796
Total	101,203	2,521	7,592	111,316	1,912	1,891	3,803

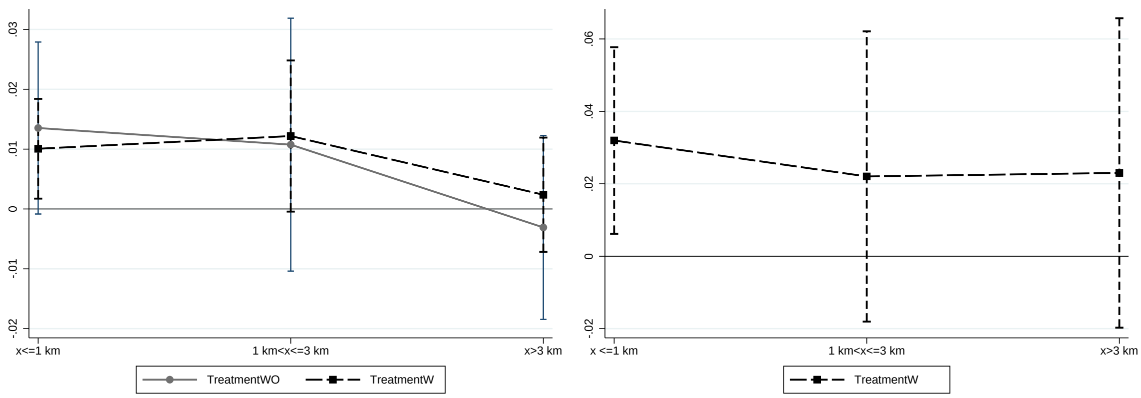
Note: The table shows the number of observations per choice set category for the *StaySwed* samples. Choice set defined by the distance (x) between individual's home and second closest care center.

Figures C.1 and C.2 show the treatment effect gradients using the second and third definitions of proximity to alternative providers. The gradients are less marked than for the first definition, partly because the lowest category absorbs so many observations using these definitions. But the difference between the lowest and highest category is visible, though only statistically significant for the third definition (*TreatmentW*, both experiments). The difference between the lowest and middle categories in Fig. C.2 is also statistically significant for *TreatmentW*.

Table C.3: Proximity def. 3: excess distance from ind. to 2nd rather than closest care center

	Population-representative				New residents		
Category	*Control*	*TreatWO*	*TreatW*	Total	*Control*	*Treatment*	Total
x≤1 kmm	62,850	1,584	4,664	69,098	1,275	1,248	2,523
1 km< x ≤3 km	15,014	362	1,173	16,549	204	201	405
x>3 km	22,794	564	1,725	25,083	233	231	464
Total	100,658	2,510	7,562	110,730	1,712	1,680	3,392

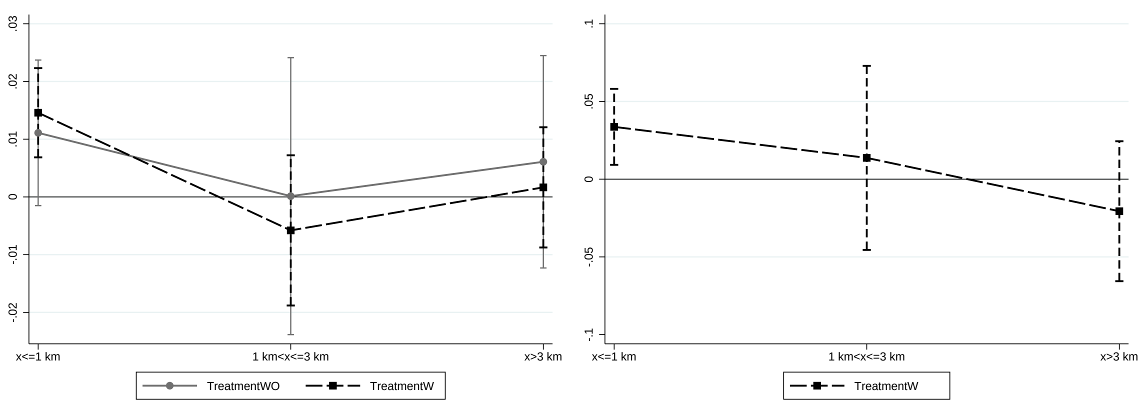
Note: The table shows the number of observations per choice set category for the *StaySwed* samples. Choice set defined by excess distance individual would have to travel from home to reach his/her second closest care center instead of the closest (x).



(a) Population-representative sample

(b) New residents

Figure C.1: Heterogenity over distance between initial center and its closest competitor.



(a) Population-representative sample

(b) New residents

Figure C.2: Heterogenity over individual's excess distance between closest and 2nd closest care center.

D Socioeconomic and demographic heterogeneity

Table D.1 displays the results from specifications exploring socioeconomic and demographic heterogeneity (described in Section 5.2.

Table D.1: Socioeconomic and demographic heterogeneity in treatment effect on switching rate

	PRS			NR
	(1)	(2)	(3)	(4)
TreatmentWO	0.0219			
	(0.0162)			
TreatmentW	0.0272***			0.0432
	(0.00968)			(0.0284)
Lowest education tertial	0.00181			0.00651
	(0.00175)			(0.0178)
*TreatmentWO*Lowest education tertial*	0.00116	0.688	0.604	
	(0.0123)			
*TreatmentW*Lowest education tertial*	-0.00612			0.00685
	(0.00715)			(0.0275)
Highest education tertial	0.00453**			-0.00818
	(0.00182)			(0.0142)
*TreatmentWO*Highest education tertial*	-0.00375	0.946	0.852	
	(0.0123)			
*TreatmentW*Highest education tertial*	-0.00110			-0.0183
	(0.00746)			(0.0221)
Age below 30	0.0342***			-0.0317**
	(0.00220)			(0.0156)
*TreatmentWO*Age below 30*	-0.0306**	0.0159	0.370	
	(0.0137)			
*TreatmentW*Age below 30*	-0.0164*			0.0132
	(0.00856)			(0.0234)
Age above 65	-0.0170***			-0.0416*
	(0.00169)			(0.0224)
*TreatmentWO*Age above 65*	-0.0168	0.319	0.344	
	(0.0119)			
*TreatmentW*Age above 65*	-0.00404			0.0259
	(0.00676)			(0.0370)
Female	0.00671***	0.786	0.494	0.0343***
	(0.00145)			(0.0118)
*TreatmentWO*Female*	-0.00511			
	(0.00974)			
*TreatmentW*Female*	0.00251			-0.0214
	(0.00580)			(0.0182)
Foreign background	0.00812***			-0.0135
	(0.00183)			(0.0145)
*TreatmentWO*Foreign background*	-0.0204*	0.189	0.0732	
	(0.0121)			
*TreatmentW*Foreign background*	0.00479			0.0304
	(0.00768)			(0.0239)

(Table continued on next page)

(Table continued from previous page)	(1)	(2)	(3)	(4)
≥5 physician visits	0.0286***			
	(0.00153)			
TreatmentWO≥5 physician visits*	0.00395	0.532	0.379	
	(0.0102)			
TreatmentW≥5 physician visits*	-0.00632			
	(0.00609)			
Active change	0.0196***			
	(0.00158)			
*TreatmentWO*Active change*	0.0176	0.188	0.0677	
	(0.0109)			
*TreatmentW*Active change*	-0.00509			
	(0.00634)			
Constant	0.0268***			0.0823***
	(0.00231)			(0.0187)
Observations	111,487			3,812
R-squared	0.014			0.020
p joint W and WO	0.00887			
p W = WO	0.776			
p all interaction terms jointly 0	0.241			0.460

Note: The estimates presented in the table derives from a variant of the baseline specification for the probability of switching in the preferred sample (*StaySwed*), in which *i)* each covariate is interacted with the treatment variable(s) and *ii)* the two covariates indicating income tertials are excluded. Results for variables indicating observations with missing values on covariates and distance to/between providers are suppressed for brevity. Column 1 and 4 show the coefficient estimates and standard errors for PRS and NR, respectively. Column 2 shows p-values of joint tests of the hypothesis that the interaction terms for both *TreatmentW* and *TreatmentWO* equal to 0 for each covariate. Column 3 shows p-values of joint tests of equality of interaction terms of *TreatmentW* and *TreatmentWO* for each covariate. p(*TreatmentW TreatmentWO* jointly 0 = p-values of test of *TreatmentW* and *TreatmentWO* jointly equal to 0). p(all interaction terms jointly 0) = p-value of test of all interactions terms jointly equal to 0. Robust standard errors in parentheses (clustered on residential address in NR); *** p<0.01, ** p<0.05, * p<0.1.

Note that we use education as indicator for socioeconomic status, excluding inscome from the estimations for ease of interpretation. Our income measure (gross income) is partly a measure of health, as individuals in bad health states tend to earn lower income. Therefore we believe that education is a more pure measure of socioeconomic status in our context. However, we have also run similar estimations using gross income instead of educational level as measure of socioeconomic status. In these models, the interaction terms are positive for both the lowest and the highest income tertials for the treatment with choice form (*TreatmentW*), indicating a non-monotonic gradient in income. In the NR, the lowest income tertial interaction term is individually significant ($p = 0.081$), but it is not distinguishable from the (insignificant) interaction term on the highest income tertial, and they are jointly indistinguishable from 0. In the PRS, the positive interaction term on the highest tertial is individually significant ($p = 0.046$), but it is not distinguishable from the (individually insignificant) term on the lowest tertial ($p = 0.372$) and they are jointly indistinguishable from zero ($p = 0.133$). In the treatment arm without a choice form, the income gradient is monotonic, with a positive (negative) interaction term for the lowest (highest) tertial; however, none of these terms are significant. In sum, we do not view these results as strong indications of an income gradient.

For the PRS, we have also estimated a version in which the dummy for having visited a GP at least 5 times since 2009 is replaced by a dummy being equal to one for the subset of this group that visited the same GP on at least four out of five occasions. This subset may be particularly reluctant to switch; that is, we expect negative interaction terms. Indeed, the interactions is negative, however they are not significantly different from zero (joint $p = 0.477$, individual $p = 0.236$ for *TreatmetWO*, $p = 0.739$ for *TreatmentW*).

E Switching modes (online or form)

Figure E.1 shows the number of returned choice forms, according to the date when these were registered by the postal service, plotted against the number of weeks since January 1, 2015. A first spike occurred right after the intervention to the population-representative sample (which took place at $x = 8$). A second spike occurred right after the second intervention ($x = 24$). Comparing to Figure 2 (b)-(d), which shows the dates when provider changes was registered by the new provider, we see that the lag in Figure 2 is due to the administrative lag between the point in time when individuals returned the form (Figure E.1) and the point in time when their new care centers had registered the change of provider.

Table E.1 shows how (if) individuals switched provider: either *Active* = via choice form (sent by postal mail or filled in while visiting the selected care center), *Online* = switch via the online interface at 1177.se., or *Initial* = administrative category, e.g., when the region registers new residents for the first time or transfers people whose care center has closed etc. The table only includes the *StaySwed* samples, but the distributions are very similar for the ITT samples. Conditional on having switched, the group without an attached choice form (*TreatmentWO*) were slightly more likely to have registered the switch online, but the differences to the other groups (i.e. *TreatmentW* and control) are not statistically significant.

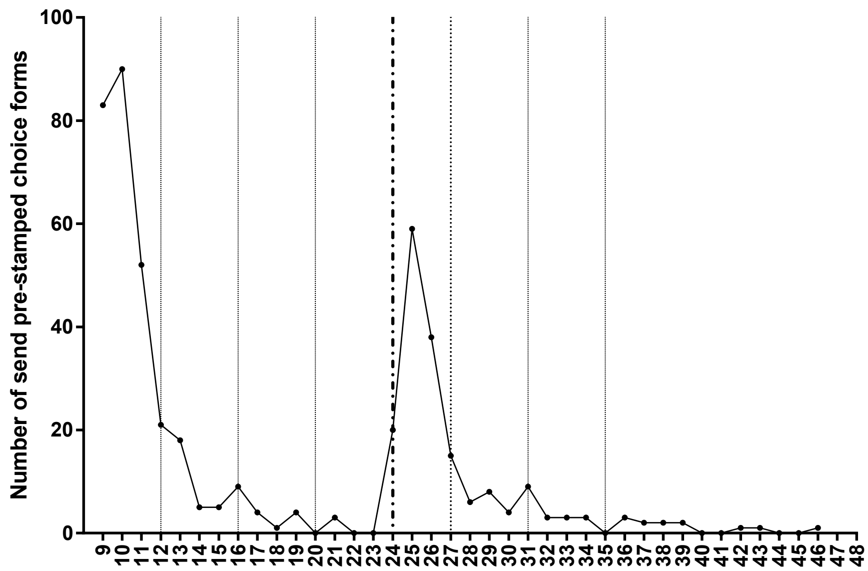


Figure E.1: Number of returned choice forms plotted against the number of weeks since January 1, 2015 (x=0). The first intervention took place in week 8 (x=8).

Table E.1: Type of change (first change after treatment)

Panel A: Population representative sample				
		Control	*TreatmentWO*	*TreatmentW*
Change #		5,674	160	492
% of change		100.00	100.00	100.00
% of all		5.60	6.34	6.47
	Active #	4,470	120	391
	% of change	78.78	75.00	79.47
	% of all	4.41	4.75	5.14
	Online #	1,148	39	94
	% of change	20.23	24.38	19.11
	% of all	1.13	1.55	1.24
	Initial #	56	1	7
	% of change	0.99	0.63	1.42
	% of all	0.06	0.04	0.09
NoChange #		95,683	2,364	7,114
% of change		-	-	-
% of all		94.40	93.66	93.53
Total #		101,357	2,524	7,606
% of change		-	-	-
% of all		100.00	100.00	100.00

Panel B: New Residents				
		Control	*Treatment*	
Change #		148	196	
% of change		100.00	100.00	
% of all		7.72	10.34	
	Active #	101	137	
	% of change	68.24	69.90	
	% of all	5.27	7.23	
	Online #	40	45	
	% of change	27.03	22.96	
	% of all	2.09	2.37	
	Initial #	7	14	
	% of change	4.73	7.14	
	% of all	0.37	0.74	
NoChange #		1,768	1,700	
% of change		-	-	-
% of all		92.28	89.66	
Total #		1,916	1,896	
% of change		-	-	-
% of all		100.00	100.00	

Note: The table shows if and how individuals in the *StaySwed* samples registered their new care center choices. Active = used a choice form, Online = switched online at 1177.se, Initial = administrative change executed by the region.

F Interaction specification for people who moved

In this section, we examine if our main results are driven by individuals who moved around the time of the intervention. As the information campaign was based on the primary care center individuals were enrolled at, and not where they lived, individuals who had or where about to move might not have received relevant information. If such individuals drive the effect to a substantial degree, we would be more inclined to interpret the treatment effects as a reminder effect, and not due to the information content. We consider individuals who moved within the region before the intervention (January 2014–January 2015), and, to increase sample size, individuals who moved (within or out of the region) after the intervention. We assert that the possibility that our intervention affected people's decision to move is negligible. Including a variable partly determined post-treatment in the specification should, in this case, have low risk of biasing the estimations. Very few in the new residents sample moved after the intervention (86 treated individuals). Due to the limited statistical power in that sample, we therefore concentrate on the larger population-representative sample.

The leaflet content was determined by the identity of the care center were the individual was enrolled at the date of randomization. Thus, for individuals who moved before the intervention, the received information was most likely to be irrelevant for those who were not enrolled at any of the four care centers closest to their homes at the date of randomization, as this may indicate that they had not switched providers since they moved. Note though that we cannot rule out that the information might have been relevant; for instance, they may have actively chosen a center far from home but close to their workplace. Among people who moved after the intervention, the information was most likely to be irrelevant for individuals who were enrolled at one of their four closest care centers at the date of randomization, but who had since moved a considerable distance. What constitutes a considerable distances is of course open to dispute, though it can be noted that half of all post-intervention movers moved less than 2 kilometres away and only a quarter moved more than 10 km away from their initial address. We pick 3 km as our cutoff, as the analysis of heterogeneity revealed no strong indications of treatment effects for individuals with no alternative care centers within 3 km from their homes. This cutoff ought to be a conservative choice as well, as some of the centers on the leaflet was clearly within reach even after the move for a substantial share of the group moving farther than 3 km.

Column 1 of Table F.1 shows the results of a specification were a dummy (*Mover*) equal to one for these two subsets of movers (in total 847 individuals) is interacted with the two treatment dummies. Clearly, the effect of the treatment including choice form is hardly affected by the inclusion of the interaction term, and though the estimate for the treatment without choice form is no longer significant, it is only slightly reduced. As before, we cannot reject the hypothesis that the two treatment effects are equal, and they are jointly significant. The interaction terms are imprecisely estimated and point in different directions for the two treatment arms.

In column 2, to increase the sample size of movers further, we take a more inclusive approach and redefine the *Mover* dummy to include everyone who moved (2,059 individuals), regardless of the distance they moved (and the potential relevance of the information). The qualitative results from this specification are similar to those reported in column 1. In this case, the baseline effect of the treatment including choice form is more affected than the treatment effect without a form, though again the estimates are not statistically distinguishable (and they are jointly significant). Neither interaction term is significant, though both are positive. We conclude that the treatment effects were not substantially driven by people who had moved.

Table F.1: Interaction specifications; people who changed address (*movers*)

	(1) Restricted	(2) Unrestricted
TreatmentWO	0.00654	0.00731
	(0.00458)	(0.00473)
TreatmentW	0.00875***	0.00663**
	(0.00277)	(0.00278)
Mover	0.123***	0.0737***
	(0.00415)	(0.00243)
TreatmentWO × *Mover*	0.0167	0.00106
	(0.0276)	(0.0155)
TreatmentW × *Mover*	-0.00138	0.00820
	(0.0161)	(0.00941)
Constant	0.0183***	0.0134***
	(0.00248)	(0.00249)
Observations	111,487	111,487
R-squared	0.035	0.029
p joint W and WO	0.003	0.020
p joint Mover and W	0.642	0.0991
p joint Mover and WO	0.395	0.570
p W = WO	0.675	0.900

Note: The table shows a version of our baseline specification (including covariates) on the preferred sample (*StaySwed*) in the population-representative sample. The outcome variable is a dummy variable equal to 1 if the individual switched care center at least once during the 36-week follow-up period. In column 1, the dummy *Mover* captures individuals who moved either the year before or after the intervention and who presumably received no valuable information by the leaflet (pre-intervention movers whose provider at the time of randomization was not among the four closest providers to their home, and post-intervention movers who were enrolled at one of the four closest at the randomization date but had since moved at least 3 km). In column 2, *mover* indicates everyone that moved during the same period, regardless of where they were enrolled at the randomization date and how far they moved. In column 1 (2), 220 (525) *mover* received *TreatmentWO* and 627 (1,534) received *TreatmentW*. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. *p joint W and WO* = p-value of test of both treatment effects equal to zero. *p W=WO* = p-value of test of equality of *TreatmentWO* and *TreatmentW*. *p mover W(O)* = p-value of marginal treatment effect on movers.