

Anell, Anders; Dietrichson, Jens; Ellegård, Lina Maria; Kjellsson, Gustav

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# Information, Switching Costs, and Consumer Choice: Evidence from Two Randomized Field Experiments in Swedish Primary Health Care

Anders Anell  
Jens Dietrichson  
Lina Maria Ellegård  
Gustav Kjellsson

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

By ANDERS ANELL, JENS DIETRICHSON, LINA MARIA ELLEGÅRD, AND  
GUSTAV KJELLSSON\*

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*Consumer choice of services that are financed by a third party may improve the matching of consumers and providers, and spur competition over quality dimensions relevant to consumers. However, in markets characterized by information frictions and switching costs, the gains from choice may fail to materialize. We use two large-scale randomized field experiments in primary health care to examine if leaflets with comparative information and pre-paid choice forms sent to consumers by postal mail affect choices. The results demonstrate that there are demand side frictions in the primary care market and indicate how these frictions can be mitigated.*

*JEL: D89, I11*

*Keywords: Consumer choice, Information, Switching costs, Primary care, Sweden*

\* Anell: Department of Business Administration, Lund University, Box 7080, SE-22007 Lund, Sweden (e-mail: anders.anell@fek.lu.se); Dietrichson: VIVE - The Danish Center for Social Science Research, Herluf Trolles Gade 11, DK-1052 Copenhagen, Denmark (e-mail: jsd@vive.dk); Ellegård: Department of Economics, Lund University, Box 7082, SE-22007 Lund, Sweden (e-mail: linamaria.ellegard@nek.lu.se); Kjellsson: Department of Economics, University of Gothenburg, Box 640, SE-40530 Gothenburg, Sweden (gustav.kjellsson@economics.gu.se). Acknowledgements: This study would not have been possible without close collaboration with staff at Region Skåne. We are in particular grateful to Magnus Käregård and Carina Nordqvist Falk, who were involved and had final say in every step of the development and implementation of the intervention. We also thank Alexander Dozet, Per Fehland and Liv Remitz for

## I. 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 the providers' incentives to improve quality. However, the available empirical evidence does not suggest that consumer choice systems in general have led 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, Moreno-Serra and Propper, 2013; Gravelle et al., 2014; Moscelli, Gravelle and Siciliani, 2016; Gaynor, Propper and Seiler, 2016), and school choice and vouchers have mostly had small or insignificant effects on educational achievement (e.g., Rouse and Barrow, 2009; Fryer, 2017; Epple, Romano and Urquiola, 2017).<sup>1</sup> Studies of health plan choices challenge the view that consumers are able to choose alternatives in line with their own interests, as many individuals choose strictly dominated plans (Gaynor, Ho and Town, 2015; Bhargava, Loewenstein and Sydnor, 2017).

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 the quality of services, and to find ways to improve choice systems. Using data from two randomized field experiments, this paper provides the first experimental evidence

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<sup>1</sup>It should be noted that the literature provides few examples of substantial negative effects of consumer choice. A recent exception is Abdulkadiroğlu, Pathak and Walters (2018), who find relatively large negative short-run effects on test scores of a school voucher program in Louisiana, United States (US).

that information frictions and switching costs prevail in the market for primary care, and indicates how these frictions can be reduced.

Information frictions prevent consumers from obtaining full information about the quality of different providers,<sup>2</sup> thereby weakening the link between free choice and enhanced welfare (Arrow, 1963). In the choice of health care provider, search costs – e.g., the time and effort required to find comparative information – apparently give rise to significant information 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 such information often being readily available online. This failure to seek information may be a rational response to search costs, as in models of rational inattention (e.g., Sims, 2003; Gabaix, 2014; Matějka and McKay, 2014). Related forms of bounded rationality, such as limited attention (e.g., Bordalo, Gennaioli and Shleifer, 2013; Caplin, 2016) and status quo bias (Samuelson and Zeckhauser, 1988), may also make consumers inadequately informed and cause inertia. Another source of frictions is that many consumers do not understand health-related information (e.g., Hibbard et al., 2007), or concepts related to health insurance (Bhargava, Loewenstein and Sydnor, 2017).

Even if consumers are well-informed, switching costs can decrease market efficiency by stopping consumers from changing to a better-matched provider (Klemperer, 1995). Switching providers 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.<sup>3</sup> By reducing the consumers' incentive to

<sup>2</sup>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); Ketcham, Kuminoff and Powers (2016); Kling et al. (2012); Handel and Kolstad (2015); Bhargava, Loewenstein and Sydnor (2017).

<sup>3</sup>See Hanushek et al. (2007) for a discussion about switching costs in a school choice context, and Starfield, Shi and Macinko (2005) and Hsiao and Boulton (2008) for the importance of continuity in the patient-physician relationship in primary care.

try out new providers, switching costs may further undermine consumers' ability to learn about provider quality.<sup>4</sup>

Our experimental setting is a Swedish region with 1.3 million residents, where consumer choice between in total about 150 providers has been an integral feature of primary care since 2009. Primary care, which deals with a multitude of health problems, 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 either consumed infrequently or not at all before the choice of provider is made. There is furthermore considerable variation between individuals, as well as over time for the same individual, regarding which characteristics of providers that are valuable in relation to one's needs. These features of the primary care market may limit the possibilities for consumers to identify high-quality providers in the absence of comparative information. 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, Ellegård and Kjellsson, 2016) and few individuals compare providers before making their choice (Glenngård, Anell and Beckman, 2011; Swedish Agency for Health and Care Services Analysis, 2013; Wahlstedt and Ekman, 2016).

Our first experimental intervention was directed to a sample representative of the general population. The second intervention targeted new residents, who constitute an interesting special case. Compared to the population at large, new residents face higher search costs, due to their shorter care history and smaller networks in the region. At the same time, their switching costs are lower, as they have not had the 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 by postal mail. The leaflet contained

<sup>4</sup>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 (2017) 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.

comparative information on, e.g., accessibility, quality, and available services of an individual’s current primary care provider and its three geographically closest competitors. By sending information directly to consumers, the experimental treatment reduced search costs and may also have improved understanding, as the information was presented differently on the leaflets compared to information available online. 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. The small monetary and hassle costs associated with switching were therefore reduced.<sup>5</sup>

In the population-representative sample, switching rates were about 14 and 10 percent higher in the treatment groups with and without a pre-paid choice form than in the control group. Among new residents, the switching rate was 26 percent higher in the treatment group compared to controls. For the treatment groups that received the leaflet together with a choice form, the treatment effect is statistically significant ( $p < 0.01$ ) in all specifications for both populations. For the smaller treatment group that did not receive a choice form, the effect on the switching rate is slightly smaller, imprecisely measured and statistically insignificant at conventional levels; however, the two treatments are not significantly different from each other.

The main estimates include individuals living in areas where choice is highly restricted because there are few alternative centers nearby. Examining heterogeneity across several definitions of rural-urban markets, we find that effects are substantially larger in urban markets and always significant for all treatments, while the effects are generally small and insignificant in rural markets. In further exploratory analyses, we use detailed administrative data to examine why, how, and for whom treatment affected switching decisions. We find few strong

<sup>5</sup>Our experiments were not designed to discriminate between behavioral hypotheses why people do and, in particular, do not switch. A fully informed and rational individual would only switch because of reduced switching costs, so individuals switching due to the treatment that did not affect switching costs suffer from some form of bounded rationality. Not switching is on the other hand compatible with behavioral assumptions ranging from individuals being fully informed and rational to complete ignorance.

indications of socioeconomic or demographic heterogeneity – though it should be noted that the statistical power is limited in this regard. Overall, the results suggest that the information mattered. Individuals in all treatment groups were significantly more likely than controls to switch to centers they were provided information about. We also find that the treatment without a choice form significantly affected individuals’ choice in the direction of better rated centers, whereas the evidence is weaker in this regard for the treatment with a choice form.

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. (2002*a,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.<sup>6</sup> However, Ericson et al. (2017) find a substantial increase in the proportion of consumers browsing 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 mail-distributed personalized information on Medicare Part D prescription drug plans led to 65 percent higher switching rates, and lower plan costs, in comparison to a control intervention that promoted a website covering the same information. Hastings and Weinstein (2008) use a natural and a randomized field experiment to study the effect of information about school-level proficiency (natural) and test scores (randomized) on school choice in North Carolina. In both cases, information raised the probability of selecting another school than the default option: by 46 percent in the natural experiment and by 23 percent in the randomized experiment.<sup>7</sup>

<sup>6</sup>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.

<sup>7</sup>In the context of school district employees’ health insurance choices in Oregon, Abaluck and Gruber (2016*b*) find that a decision support tool had a minor impact on employee’s forgone savings. This randomized intervention differed in key aspects from those mentioned in the text. Instead of receiving personalized information by mail, consumers had to actively access the decision support tool.



Apart from the choice setting, our study differs from previous studies in several respects, such as the study population and the type of information treatment. Our study population did not self-select into the study, as in Kling et al. (2012), and our first intervention targeted a more general segment of the population than any previous study. The leaflets we sent out were less voluminous than the brochures sent out in McCormack et al. (2001) and Farley et al. (2002*a,b*).<sup>8</sup> They 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, and the information material in Ericson et al. (2017), which contained an encouragement to shop around and (in one treatment) mentioned the potential savings of switching to the plan with the lowest premium.

The multidimensional nature of primary care implies that a narrow focus on only one quality indicator might be irrelevant or even misleading. In markets where quality is a multidimensional construct, and where heterogeneity in consumer preferences motivate providers to specialize, information material needs to be more encompassing. Offering less simplistic information material is even more important if information campaigns are scaled up to market level, as a too narrow focus may strengthen providers' incentives to engage in cream-skimming or teaching-to-the-test behavior (e.g., Holmström and Milgrom, 1991). It is therefore encouraging that we find effects of a relatively encompassing information material: the result suggests that similar campaigns can be used to improve the functioning of many types of consumer choice markets.

The paper proceeds as follows. Section II describes primary health care in Sweden and Skåne. Section III details the experimental design and our estimation procedures. Section IV describes the data and Section V presents the results. Section VI concludes.

<sup>8</sup>A range of hypothetical choice experiments using health care settings have shown that complex information decrease consumers' ability to make informed choices (e.g., Kurtzman and Greene, 2016).

## II. Primary Health Care in Sweden and Skåne

Sweden has a mainly tax-funded health care system with universal coverage for citizens. 21 independent regions, headed by locally elected politicians, are responsible for the financing and organization of health care. The present study is set in Skåne (Scania), the third largest Swedish region with 1.3 million residents.

The role of primary care is to supply basic medical treatments, preventive care, and rehabilitation. Primary care is typically provided in group practices called primary care centers (PCC). A PCC on average employs about four physicians/general practitioners (GPs), and is also staffed with nurses and, e.g., behavioral therapists or physiotherapists (Anell, 2015). In the beginning of 2015, the year of our interventions, there were 150 PCCs in Skåne, 86 of which were publicly owned and operated and the others private for-profit firms. The mean (and median) number of patients per PCC was approximately 6,800.

The PCCs are mainly reimbursed by a fixed sum per enrolled patient. Patients also pay a visit fee which is regulated by the health care authority. In 2015, the basic fee was SEK 160 (\$20) up to an annual cap of SEK 1,100 (\$135). There was a 25 percent surcharge for visits at other providers than the PCC where one was enrolled.

Since the choice system was introduced in May 2009, all residents are enrolled at a PCC. Residents can freely choose between all PCCs and may switch as often as they like. The PCCs are not allowed to close their lists for new patients.<sup>9</sup> New residents are automatically assigned to their closest PCC and are sent a notification including the name of this center. The free choice of provider is mentioned in the letter, but it does not contain any information about alternative PCCs.

Since the introduction of the choice system, the region has occasionally advertised the right to choose provider via ads in newspapers, on the web, in the

<sup>9</sup>All Swedish regions introduced similar choice systems in 2007-2010. See Anell (2011, 2015) for more information on the reforms.

public transport system, and by postal mail to the whole population. On a few occasions, the right to choose was also highlighted in a magazine sent to all residents by postal mail.<sup>10</sup> Notably though, our intervention is the only occasion on which the region has distributed comparative information about specific PCCs directly to residents. This is not to say that active residents cannot get access to information: a website operated jointly by the Swedish health care authorities, 1177.se, provides information on contact details, opening hours, availability of special competencies, and ratings from a patient survey. There is also an interface comparing up to four PCCs with respect to patient ratings. Many providers describe their services further on their own web pages (linked from 1177.se).

The right to choose provider is well-known in both Sweden and Skåne. Many Swedes also think that they have made an informed choice, although few have searched for comparative information before choosing; often, the current provider has been the only source of information (Glenngård, Anell and Beckman, 2011; Swedish Agency for Health and Care Services Analysis, 2013; Wahlstedt and Ekman, 2016). 11% of respondents to a population-representative survey conducted in 2013 had considered switching but not yet switched; almost half of them stated that lack of information about alternative providers was a reason why they had not yet switched (Swedish Agency for Health and Care Services Analysis, 2013).

To switch providers, one option is to log in to a personalized section of 1177.se, where it is straightforward to search for and select a care center. Another option is to fill in a choice form (available at all PCCs or at the public section of 1177.se), which may be handed in to the chosen PCC directly or by postal mail. Thus, the only direct monetary cost associated with switching would be the cost of the stamp.

<sup>10</sup>In Online Appendix I, we use the magazine to examine if reminders about the free choice per se affect the switching rate.

### III. Experimental design and empirical strategy

#### A. *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.

The leaflets contained comparative information about the PCC where the individual was currently enrolled and the three geographically closest competitors of this PCC. 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 individual only had to fill in the name of the chosen PCC and to return the form, either by postal mail or by handing it in at a PCC.

The control groups received nothing, which implies that we cannot separate the effect of increased access to information from the effect of being reminded about the free choice of provider. We foresaw this problem, but were forced to limit the number of treatment arms because the health authority did not want to treat more than one percent of each PCC’s patients.<sup>11</sup> In practice, we believe that the margin for pure reminder effects is small, given that the right to choose primary care provider is well-known in Sweden. In Online Appendix I, we show that switching rates have been stable around the time of previous information campaigns not including comparative information about providers.

The information leaflet was in the format of an A4 sized paper folded in two. An example of a leaflet is available in Appendix A. On the front page, there was a short text stating that residents are allowed to freely choose PCC, that it is important to compare centers to find a suitable one, and that the centerfold included comparative information about the individual’s current PCC and the

<sup>11</sup>This was to ensure that no single provider would be substantially or disproportionately affected by the intervention.

three alternative centers closest to the current one. The end page included a description of how to switch PCC and a disclaimer stating that the leaflet recipient would remain enrolled at the current one if (s)he did not make a new choice.

The centerfold contained four sets of items describing the four PCCs. The items are described in detail in Appendix A and only summarized here. 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 (willingness to recommend the PCC to others; perceived waiting time to see a physician), and three indicators were collected by the health care authority (telephone response rate; patient-physician continuity; compliance with prescription guidelines for elderly). Third, there were indicators for each PCC's availability of specialized clinics catering to elderly individuals or to certain patient groups (dementia, asthma, chronic obstructive pulmonary disease or congestive heart failure), and indicators for the availability of behavioral therapists, gynecologists, chiropractors, or naprapaths. Fourth and finally, there were indicators for PCCs located nearby a midwife clinic or a children's health center.

Because there were 150 PCCs, there were also 150 unique leaflets. In Appendix A, we show that there was considerable variation in terms of most items on the leaflets, in the region as a whole as well as within a given leaflet. Leading administrators at the health care authority were involved in the decision of what, and how much, information to include on the leaflets. All information was publicly available, though some of it was more easily accessible. Contact details and patient survey scores were presented at each PCC's index page at 1177.se, from which the information about available special clinics at the center was typically only one click away. To find information about the three quality indicators measured by the region, the individual would have had to use a search engine.

### B. 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 population-representative sample of 11 percent of the region’s residents over 18 years of age, drawn randomly from each PCC’s patient list on February 2, 2015. The second population included all individuals (above 18) who entered the enrollment register between February 4 and May 11, 2015; by setting the first date to February 4, we avoided treating individuals in both interventions. By and large, the second population was constituted by individuals who had just moved into the region (from other regions or from abroad).<sup>12</sup>

The full population-representative sample (PRS) included 112,859 individuals, of which 10,259 were randomly (within each PCC) assigned to receive the information leaflet. A randomized sub-sample (7,700 individuals) also received the choice form. Due to the 1 percent constraint, we assigned a disproportionate number of individuals to the treatment arm we *ex ante* believed would have a stronger effect, to ensure that at least one arm would not be underpowered. One individual chose to opt out from the study after randomization.<sup>13</sup> 137 individuals died or left the region before we extracted address information (for administrative reasons, address data was extracted after the randomization date) and an additional 146 individuals were de-registered from the region before the leaflets were mailed out in the last week of February. These groups are not in essence part of the information intervention, and are excluded from our estimation sample. The sample used for our analyses of the population-representative sample (PRS) therefore includes 112,575 individuals.

<sup>12</sup>It also included individuals who were already living in the region but had been enrolled at a PCC in another region for a while (e.g., 253 of the individuals who entered the enrollment register in Feb-May 2015 were residents in the region on December 31, 2014). Results are robust to excluding these individuals, see Appendix B.

<sup>13</sup>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 either the information campaign or the experimental set up.

Of the 6,906 individuals constituting the population of new residents (NR), 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.<sup>14</sup> 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. In addition, 102 individuals died or left the region between randomization and intervention leading to an estimation sample of 6,803 individuals in the NR experiment. In Appendix B, we show our main results for both PRS and NR are robust to retaining individuals who died or left the region in the estimation sample.

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 36 weeks for the population-representative sample and 21 weeks for the new residents sample.

### C. Estimation

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

$$(1) \quad y_i = \alpha + \beta \text{TreatArm}_i + \gamma X_i + \varepsilon_i \quad \text{TreatArm} = \{\text{info}, \text{info\&form}\}$$

where  $y_i$  is a dummy variable. Our main dependent variable attains the value one if individual  $i$  switched PCC at least once during the full follow-up period.

<sup>14</sup>Due to the restrictions of sample size per PCC in the first experiment, we could not cluster-randomize the treatments to the PRS. In Appendix B, we show that household spill-over effects are unlikely to be a concern for our estimates for PRS.

In additional analyses, we examine the probability of being listed at others PCCs (not) on the leaflet.<sup>15</sup> In Appendix C, we investigate switching patterns further using additional outcomes variables: the probability of switching back to the initial provider, the probability of being enrolled at a different PCC than the initial at the end of the follow-up period, and the number of switches.

For the population-representative sample, *info&form* indicates the treatment arm with a choice form attached and *info* indicates the arm without a choice form; as noted, there was only one treatment arm (*info&form*) 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 account for any potential imbalances between the treated and controls and to increase precision (see Table 1 for definitions).  $\varepsilon_i$  is a residual term and  $\alpha$  is an intercept.

Using a linear probability model (LPM), instead of a non-linear choice model, simplifies interpretation and inclusion of interactions. The linearity assumption is not 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 PCC's patient list. Assignment to treatment in the PRS was stratified by PCC, and we include strata fixed effects in our baseline estimations as recommended by Bruhn and McKenzie (2009). 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. Randomization inference (e.g., Young, 2016; Athey and Imbens, 2017) on the main specifications yields the same conclusions (see Appendix B).

<sup>15</sup>For the control group these are the PCCs that would have been on their leaflet, had they been assigned to a treatment group.



To examine heterogeneity of the treatment effects we use LPM models where the treatment dummies are interacted with indicators for rural areas and demographic and socioeconomic characteristics (see Appendix E).

To evaluate if individuals switch to PCCs that were better rated, we follow e.g., Kling, Liebman and Katz (2007) and Finkelstein et al. (2012) by calculating average standardized treatment effects over leaflet items for which a higher rating is unambiguously better (i.e., all items mentioned in III.A except address, public/private and number of patients). To this end, we first estimate  $J$  seemingly unrelated regressions (similar to equation 1) in which the outcome variable  $y_{ij}$  is defined as the difference in item  $j$  between the PCCs where individual  $i$  was enrolled before and at the end of the follow-up period, respectively.<sup>16</sup> We then calculate the average (over items) standardized treatment effects for each treatment indicator  $k$  as:

$$(2) \quad \tau_k = \sum_{j \in J} \frac{1}{J} \frac{\beta_{kj}}{\sigma_j}$$

where  $\sigma_j$  denotes the the standard deviation of outcome variable  $y_j$  in the control group, and  $\beta_{kj}$  is the coefficient on the treatment indicator  $k$  from regression  $j$ . Average standardized treatment effects are calculated for three sets of leaflet items: all items, items related to quality, and items related to available special clinics.

The tested main hypotheses in Section V.A follow our pre-registered analysis plan.<sup>17</sup> The analysis of heterogeneity and whether individuals switched to better rated PCCs were sketched in the analysis plan but the exact specifications were

<sup>16</sup>Five PCCs lacks data on one or more information items. For individuals that are enrolled at such a PCC either at the start or end of the follow-up period, the outcome variable is coded as zero. In Online Appendix H, we show that results are very similar if we instead calculate the outcome variable using the median value of all other PCCs in case of missing information on an item.

<sup>17</sup>The analysis plan is available at the American Economic Association’s registry for randomized controlled trials ([www.socialscienceregistry.org](http://www.socialscienceregistry.org)) with registration number AEARCTR-0000659, and title “Information and user choice in primary health care markets”. The plan includes tests of additional exploratory hypotheses, e.g., of examining heterogeneity related to patients’ morbidity, which will be addressed in a companion paper.

not pre-specified, as we did not have full information of the variables included in the data at the time of registration.

## IV. Data

### A. Data sources

From registers held by the health care authority in Skåne, we have daily information on PCC enrollment and health care contacts at the individual level. The dataset comprises the primary care enrollment history and all health care consumption in Skåne from 2009 to early November 2015 for all individuals in the two experimental samples. The data also include the straight line distances between each individual’s home address and all PCCs in Skåne (at both the start and the end of the follow-up period). To this data, we have matched individual

TABLE 1—DEFINITIONS OF COVARIATES

<i>Rural PCC</i>	Enrolled at a PCC located in a town with up to 15,000 residents.
<i>Choice within 1 (3) &gt;3 km</i>	Individual has $\geq 2$ PCCs within 1 (3) >3 km from home.
<i>Lowest (highest) education tertial</i>	Two thirds of individual’s birth cohort has longer (shorter) education (cohort defined by birth decade).
<i>Lowest (highest) income tertial</i>	Gross income in the lowest (highest) tertial of the regional income distribution.
<i>Female</i>	Individual is a woman.
<i>Age &gt; 30 (&lt; 75)</i>	Individual is <30 ( $\geq 75$ ) years of age.
<i>Age 30-45 (60-75)</i>	Individual is 30-45 (60-74) years of age.
<i>Foreign background</i>	Born outside, or both parents born outside, Sweden.
<i>Child in household</i>	Individual has $\geq 1$ child (< 18 years old) living in the household.
<i>Enrolled at closest PCC</i>	Individual was enrolled at the closest PCC at the time of the intervention.
<i>Pre-intervention mover</i>	Individual moved, and changed closest PCC, between Dec 31 2013 and the intervention.
<i>PCC visits</i>	Number of PCC visits since 2009. Dummies for $\leq 4$ ; 5-14; 15-30; and > 30 visits.
<i>PCC switches</i>	Number of (non-administrative) PCC switches since 2009. Dummies for 0, 1, 2, and $\geq 3$ switches.
<i>Recent switch</i>	Individual has switched PCC at least once in the 36 week period before the intervention.

*Note:* All covariates are dummy variables = 1 when the definition above applies and 0 otherwise. Data on choice set, rurality, switches 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.

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. Table 1 shows definitions of the covariates we use in our estimations. We also include indicators for missing observations.

### B. Summary statistics

The descriptive statistics in Tables 2 and 3 show that the randomizations appear to have created balanced samples. The rightmost column shows  $p$ -values from F-tests of the null hypothesis of equal means in all groups. There are a few significant differences in the PRS, but all differences are small. In a regression of the treatment indicator on all covariates, we cannot reject the null that their coefficients are jointly equal to zero ( $F = 1.05, p = 0.388$ ).<sup>18</sup>

The NR sample (Table 3) is also reasonably balanced. There are statistically significant differences (at a 5% or 10% level) in terms of sex, the share above 60 years, and the share in the highest income tercile; however, in a regression of the treatment indicator on all covariates, we cannot reject the null that the coefficients are jointly equal to zero ( $F = 1.09, p = 0.342$ ).

Notably, we lack all background data for a large share of individuals in the NR sample: about 34% in both the treatment and the control group. Many new residents had recently immigrated to Sweden and were thus not in the official registers at the last point in time for which we have background data (December 31, 2014).<sup>19</sup> A substantial share of these individuals are likely refugees from Middle Eastern countries (Statistics Sweden, 2016). It is further notable that among new residents *with* background data, the share of young (elderly) individuals is much higher (lower) compared to the PRS, reflecting the demographic profile of people

<sup>18</sup>Note that there are three levels of this outcome variable (control, info, info & 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.

<sup>19</sup>For Swedes returning to Sweden after having lived abroad, we have information about at least some background data, e.g., country of birth. Thus, no background data implies being born outside Sweden.

TABLE 2—COVARIATE SUMMARY STATISTICS: PRS

	<i>Control</i>		<i>info</i>		<i>info&amp;form</i>		<i>p</i>
	Mean	std. dev.	Mean	std. dev.	Mean	std. dev.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Rural PCC</i>	0.380	0.485	0.381	0.486	0.381	0.486	0.994
<i>Choice within 1 km</i>	0.304	0.460	0.301	0.459	0.302	0.459	0.879
<i>Choice within 1-3 km</i>	0.286	0.452	0.281	0.450	0.286	0.452	0.879
<i>Choice within &gt;3 km</i>	0.403	0.491	0.412	0.492	0.405	0.491	0.597
<i>Distance missing</i>	0.007	0.083	0.005	0.074	0.006	0.076	0.347
<i>Lowest education tertial</i>	0.328	0.470	0.330	0.470	0.329	0.470	0.971
<i>Highest education tertial</i>	0.304	0.460	0.300	0.458	0.298	0.458	0.490
<i>Education missing</i>	0.025	0.156	0.023	0.150	0.024	0.153	0.787
<i>Lowest income tertial</i>	0.326	0.469	0.316	0.465	0.329	0.470	0.462
<i>Highest income tertial</i>	0.327	0.469	0.333	0.471	0.320	0.467	0.367
<i>Income missing</i>	0.011	0.107	0.011	0.106	0.011	0.106	0.998
<i>Female</i>	0.508	0.500	0.506	0.500	0.514	0.500	0.522
<i>Sex missing</i>	0.000	0.016	0.000	0.020	0.000	0.011	0.726
<i>Age &lt;30</i>	0.203	0.402	0.202	0.402	0.205	0.403	0.944
<i>Age 30-45</i>	0.247	0.431	0.249	0.433	0.242	0.428	0.515
<i>Age &gt;60</i>	0.299	0.458	0.304	0.460	0.309	0.462	0.176
<i>Age missing</i>	0.001	0.024	0.000	0.020	0.000	0.020	0.771
<i>Foreign background</i>	0.251	0.433	0.238	0.426	0.244	0.430	0.160
<i>For background missing</i>	0.011	0.104	0.013	0.111	0.011	0.105	0.731
<i>Child missing</i>	0.014	0.117	0.013	0.111	0.014	0.119	0.802
<i>Child in household</i>	0.333	0.471	0.351	0.477	0.336	0.472	0.139
<i>Enrolled at closest PCC</i>	0.501	0.500	0.498	0.500	0.502	0.500	0.957
<i>Pre-intervention mover</i>	0.130	0.336	0.144	0.351	0.128	0.334	0.105
<i>PCC switches 0</i>	0.616	0.486	0.621	0.485	0.621	0.485	0.558
<i>PCC switches 1</i>	0.265	0.441	0.266	0.442	0.255	0.436	0.169
<i>PCC switches 2</i>	0.085	0.278	0.082	0.275	0.087	0.281	0.766
<i>PCC switches &gt;2</i>	0.035	0.183	0.031	0.173	0.037	0.189	0.298
<i>Recent switch</i>	0.068	0.251	0.076	0.265	0.069	0.253	0.244
<i>PCC visits &lt;5</i>	0.507	0.500	0.503	0.500	0.487	0.500	0.004
<i>PCC visits 5-14</i>	0.364	0.481	0.369	0.483	0.379	0.485	0.031
<i>PCC visits 15-30</i>	0.104	0.305	0.103	0.304	0.107	0.309	0.732
<i>PCC visits &gt;30</i>	0.025	0.157	0.025	0.155	0.027	0.164	0.472
<i>N</i>	102,351		2,549		7,675		

Note:  $p$  = p-value from test of equal means in treatment and control group. See Table 1 for definitions of the covariates.

who moved within the country.

## V. Results

### A. Main results

The estimates for the population-representative sample (PRS) are shown in the first two columns in Table 4. Compared to the 5.7% switching rate of the control group during the 36 week follow-up period, the probability to switch was 0.61 and

TABLE 3—COVARIATE SUMMARY STATISTICS: NR

	<i>Control</i>		<i>info&amp;form</i>		diff	<i>p</i>
	Mean	std. dev.	Mean	std. dev.		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Rural PCC</i>	0.294	0.456	0.279	0.449	-0.015	0.167
<i>Choice within 1 km</i>	0.422	0.494	0.417	0.493	-0.005	0.698
<i>Choice within 1-3 km</i>	0.245	0.430	0.247	0.431	0.002	0.871
<i>Choice within &gt;3 km</i>	0.239	0.427	0.230	0.421	-0.010	0.343
<i>Distance missing</i>	0.094	0.292	0.107	0.309	0.013	0.082
<i>Lowest education tertial</i>	0.140	0.347	0.131	0.338	-0.009	0.285
<i>Highest education tertial</i>	0.215	0.411	0.209	0.406	-0.006	0.555
<i>Education missing</i>	0.434	0.496	0.444	0.497	0.010	0.392
<i>Lowest income tertial</i>	0.340	0.474	0.351	0.477	0.011	0.334
<i>Highest income tertial</i>	0.098	0.297	0.085	0.278	-0.013	0.054
<i>Income missing</i>	0.458	0.498	0.464	0.499	0.006	0.595
<i>Female</i>	0.266	0.442	0.247	0.431	-0.019	0.074
<i>Sex missing</i>	0.466	0.499	0.484	0.500	0.017	0.154
<i>Age &lt;30</i>	0.279	0.449	0.291	0.454	0.012	0.278
<i>Age 30-45</i>	0.128	0.334	0.116	0.320	-0.012	0.119
<i>Age &gt;60</i>	0.046	0.210	0.034	0.182	-0.012	0.013
<i>Age missing</i>	0.485	0.500	0.504	0.500	0.019	0.112
<i>Foreign background</i>	0.179	0.383	0.176	0.381	-0.003	0.737
<i>For background missing</i>	0.471	0.499	0.487	0.500	0.016	0.193
<i>Child missing</i>	0.497	0.500	0.511	0.500	0.013	0.270
<i>Child in household</i>	0.147	0.355	0.144	0.351	-0.004	0.647
<i>Enrolled at closest PCC</i>	0.654	0.476	0.665	0.472	0.010	0.370
<i>N</i>	3,397		3,406			

Note:  $p$  =  $p$ -value from test of equal means in treatment and control group. See Table 1 for definitions of the covariates.

0.83 percentage points higher among the individuals in the treatment arms *info* and *info&form*, respectively. These effects correspond to relative increases of 10.6 to 14.5 percent. The results are very similar when we include covariates in column (2), yielding (relative) treatment effects equal to 0.58 (10.2%) and 0.82 (14.3%).<sup>20</sup> Although the treatment effect is larger and only statistically significant for the treatment arm that received a choice form (*info&form*,  $p < 0.01$ ), the estimates of the two treatment arms are similar, and not significantly different from one another. Given the relatively small sample size of the *info* arm, we suspect that

<sup>20</sup>In specifications with covariates, the reference person is a middle-aged (45-60 year old) man with educational attainment and income in the mid-tercials of the respective distributions, born in Sweden with two Swedish parents, having no children, living within 1 km distance to at least two PCCs, and not initially enrolled at the closest PCC (and for the NR the initial PCC is located in a non-rural area). The reference person in PRS has not moved prior to the intervention, has been enrolled at the same PCC since 2009, and has made fewer than 5 visits to primary care since then. To retain individuals with missing information about covariates in the sample, we use dummy variables to indicate observations with missing values on covariates.

the insignificance of this variable is due to low statistical power.

TABLE 4—MAIN RESULTS

	<i>Treatment effect on switching rate after 36/21 weeks</i>			
	PRS		NR	
	(1)	(2)	(3)	(4)
<i>info</i>	0.00605 (0.00476)	0.00577 (0.00473)		
<i>info&amp;form</i>	0.00825 (0.00275)	0.00816 (0.00265)	0.0203 (0.00783)	0.0233 (0.00768)
<i>Constant</i>	0.0569 (0.000221)	0.0529 (0.00346)	0.0898 (0.00526)	0.169 (0.0230)
<i>N</i>	112,575	112,575	6,803	6,803
<i>R</i> <sup>2</sup>	0.000	0.024	0.001	0.031
<i>p info=info&amp;form</i>	0.684	0.654		
Number of Strata	150	150		
Covariates	No	Yes	No	Yes

*Note:* The table shows treatment effect estimates from linear probability models. Column (1) and (2) covers the intervention directed to the population-representative sample (PRS) and columns (3) and (4) covers the intervention directed to the new residents sample (NR). In all specifications, the dependent variable is a dummy equal to 1 for individuals who switched PCC at least once during the full follow-up period (36 weeks for PRS, 21 weeks for NR). Estimates in even-numbered columns are from specifications controlling for the covariates in Table 2 and 3.  $p \text{ info=info\&form}$  = p-value of test of difference between the estimates. 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)

Column (3) and (4) show the results with and without covariates 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 9 percent. The new residents also reacted stronger to the information intervention. The treatment effect of 2.0 (2.3 with covariates,  $p < 0.01$ ) percentage points implies a 22.6 (26.0) percent increase in the switching rate compared to the control group. Thus, the treatment effect among new residents is larger in both absolute and relative terms compared to the population-representative sample. This finding aligns well with the idea that the new residents initially had less knowledge about available PCCs and their features, as well as lower switching costs.

Appendix B shows that our main results are robust to a range of sensitivity

checks, such as using a logit specification and excluding (including) PCC fixed effects in the specification for the PRS (NR). The PRS results are also robust to removing sample weights and we find no indications of household spill-overs affecting our results in this sample (in contrast to the NR sample, the assignment of treatment was not clustered by residential address in the PRS). Further, the conclusions in both experiments are unchanged when we use randomization-based inference. Finally, Appendix C shows that the estimated treatment effect on the probability of being listed at another PCC than the initial one at the end of the follow-up period is only slightly smaller than the estimates in Table 4 for all three treatment groups.

### B. Rural/urban heterogeneity

The main treatment effects include individuals who reside in rural areas, for whom the alternative PCCs on the leaflets may be located too far away to be attractive options. With few available options, individuals in rural areas may also be better informed about the market. Because the region had to treat all PCCs equally (for legal and political reasons), individuals enrolled at rural PCCs were included in the interventions.

Table 5 displays heterogeneity with respect to the rurality of the PCC the individual was initially enrolled at. In column (1), towns with at most 15,000 residents, corresponding to towns with at most two PCCs,<sup>21</sup> are defined as *rural*. In the PRS (Panel A), the treatment effect among individuals initially enrolled at urban centers is about twice as large as the main effect and it is significant for both treatment arms ( $p < 0.05$  or lower). The differences between individuals enrolled at urban and rural centers are statistically significant. The treatment effects are large and significant for urban individuals, but small and insignificant for rural. The pattern is similar for the new residents (Panel B), though the interaction term is not significant. Column (2) to (4) show estimations where

<sup>21</sup>The only exception is Tomelilla, a town with 8,000 residents but three PCCs. One of these has only 600 patients and is the smallest one in the region.

rural PCCs are instead defined as having at most one competitor within a radius of 1, 3, or 5 kilometers. The results confirm the findings in column (1). Thus, both treatment arms had large effects in urban areas.<sup>22</sup>

TABLE 5—HETEROGENEITY: RURAL VS URBAN

Panel A: Population-representative sample (PRS)				
<i>Treatment effect on switching rate after 36 weeks</i>				
	(1)	(2)	(3)	(4)
<i>info</i>	0.0138 (0.00646)	0.0289 (0.0120)	0.0153 (0.00664)	0.0127 (0.00622)
<i>info</i> × <i>rural</i>	-0.0211 (0.00876)	-0.0307 (0.0129)	-0.0231 (0.00878)	-0.0198 (0.00888)
<i>info</i> & <i>form</i>	0.0128 (0.00334)	0.0108 (0.00466)	0.0135 (0.00323)	0.0132 (0.00320)
<i>info</i> & <i>form</i> × <i>rural</i>	-0.0122 (0.00534)	-0.00356 (0.00565)	-0.0129 (0.00541)	-0.0143 (0.00546)
<i>N</i>	112,575	112,575	112,575	112,575
<i>R</i> <sup>2</sup>	0.024	0.024	0.024	0.024
<i>info</i> + <i>info</i> × <i>rural</i>	-0.0073	-0.0018	-0.0078	-0.0071
<i>p</i>	0.217	0.693	0.175	0.264
<i>info</i> & <i>form</i> + <i>info</i> & <i>form</i> × <i>rural</i>	0.0006	0.0073	0.0006	-0.0011
<i>p</i>	0.887	0.023	0.896	0.795
town/radii	town	1km	3km	5km
Panel B: New Residents (NR)				
<i>Treatment effect on switching rate after 21 weeks</i>				
	(1)	(2)	(3)	(4)
<i>info</i> & <i>form</i>	0.0304 (0.00895)	0.0373 (0.0140)	0.0282 (0.00914)	0.0277 (0.00885)
<i>info</i> & <i>form</i> × <i>rural</i>	-0.0253 (0.0165)	-0.0202 (0.0167)	-0.0152 (0.0163)	-0.0172 (0.0168)
<i>N</i>	6,905	6,905	6,905	6,905
<i>R</i> <sup>2</sup>	0.032	0.032	0.032	0.032
<i>info</i> & <i>form</i> + <i>info</i> & <i>form</i> × <i>rural</i>	0.0051	0.0171	0.0129	0.0105
<i>p</i>	0.713	0.058	0.338	0.463
town/radius	town	1km	3km	5km

*Note:* The table shows estimates of heterogeneity by the rurality/urbanity of the location of the PCC where the individual is enrolled at the time of randomization. In col. 1 rural equals 1 if the PCC is located in a rural town (up to ~15000 residents), which corresponds to PCCs having less than two competitors within the same town. In col. 2 to 4 rural equals if the PCC has less than two competitors within 1,3, or 5 kilometers. Robust standard errors in parentheses (clustered by residential address in NR sample)

<sup>22</sup>In Appendix D we show that the heterogeneity is largely driven by the most urban PCCs and show that the result is similar with other definitions of rurality.



### C. Demographic and socioeconomic heterogeneity

In Appendix E we explore the socioeconomic and demographic heterogeneity (for education attainment, income, age, sex, and foreign background). For brevity we only summarize the results here. Overall, there is no striking socioeconomic or demographic heterogeneity in the response to the treatment, and especially no clear gradients that are consistent over treatments and populations. However, our chances to detect such heterogeneity are limited, as the treatment induced relatively few switches in each subgroup. For the NR, the lack of variation in age and socioeconomic status among individuals with background data further limits statistical power.

In the PRS, we do find a tendency that middle-aged individuals are more responsive than other groups and that middle-income individuals respond less to the treatment including a form. There are further indications that individuals with known foreign background reacted stronger to the treatment with a choice form, but less to the treatment without a form. Relatedly, for the third of the NR sample lacking background data – who had just arrived as immigrants – the response to the *info&form*-treatment is about one third of the response among individuals with background data (although the difference is not statistically significant), while their baseline switching rate is higher. The new immigrants ought to have had problems understanding the information, and may also face lower returns on investments in understanding the market, as they are not guaranteed to stay where they are initially placed. Another potentially important dimension of heterogeneity concerns individuals who have recently moved within the region, and who therefore might want to switch (although not necessarily to one of the PCCs on the leaflet). Appendix G shows that recent movers do not react significantly different from other individuals.

## D. Mechanisms

## WHEN DID PEOPLE SWITCH?

Our detailed data on enrollment periods allow us to track when individuals switched. Figure 1a 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. Thus, it does not seem as though the interventions only made already determined people switch sooner than they otherwise would have done.

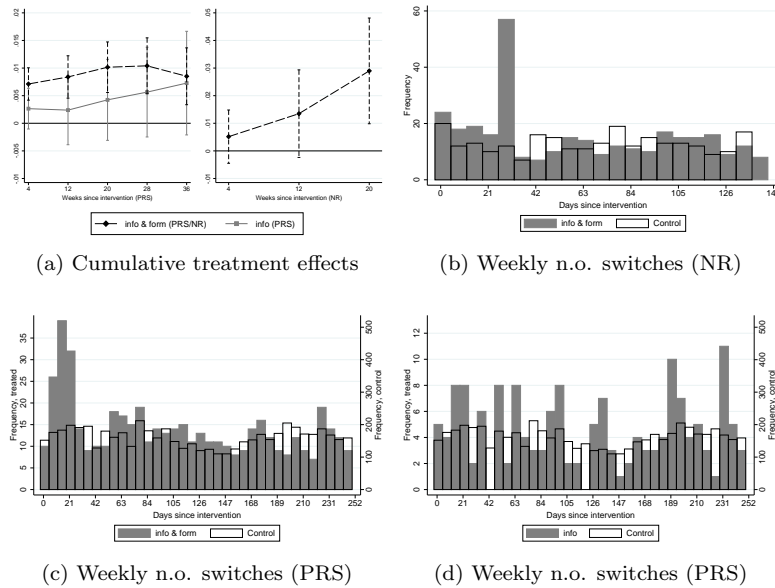


FIGURE 1. TREATMENT EFFECTS ON SWITCHING RATE OVER TIME.

*Note:* Figure (a) shows the cumulative treatment effect with 95 percent confidence interval estimated 4-36 weeks after the interventions; population-representative sample (PRS) in the left panel and new residents (NR) in the 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 bars comparable between the treatment and control groups (which are of different size in the PRS).

Figures 1b-d illustrate the timing of the switches by plotting the weekly num-

ber of individuals that made their first switch since the intervention.<sup>23</sup> There are marked spikes a few weeks after the intervention for the treatments with choice forms (Fig. 1 b and c),<sup>24</sup> but not for the treatment without a choice form (Figure 1d). Thus, although both treatment arms had a similar effect on the switching rate, including the choice form nudged individuals to switch instantly whereas the information-only treatment had a gradual impact.

#### DID PEOPLE SWITCH TO OTHER CENTERS ON THE LEAFLET?

Having established that the interventions affected the propensity to switch, we now examine to what PCCs people switched. As the interventions reduced the search costs involved in making comparisons only with respect to PCCs displayed on the leaflet, one might expect that the intervention made individuals more likely to select these centers. The information on the leaflet may also have been presented in a more easily-comprehended way than on the website 1177.se.

In Table 6, we decompose the treatment effects into switches to PCCs on the leaflet and switches to other PCCs. 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, had switched from their initial provider to one of the other three PCCs on the leaflet.<sup>25</sup> In the second and fifth column, the dependent variable is a dummy indicating a switch to a PCC not on the leaflet.

The effect on the probability of switching to a PCC on the leaflet is 0.5-0.6 percentage points in the PRS (column (1)) and 1.5 percentage points for the new residents (column (4)), and is statistically significant for all treatments ( $p < 0.05$

<sup>23</sup>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.

<sup>24</sup>The lag between the interventions and the spikes reflects that switches were registered with a lag. Appendix F shows that the attached choice forms were returned and registered by the postal service very soon after the intervention.

<sup>25</sup>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 the follow-up period. Some individuals switched more than once during the follow-up period. The results are very similar, if we instead consider what PCC individuals were enrolled at after their first switch.

or lower). The effect on the probability of switching to a PCC *not* on the leaflet is smaller (−0.15 to 0.15 percentage points in PRS and 1.1 percentage points in NR), and insignificant. Thus, these estimations show that the interventions primarily increased the propensity to switch to one of the other PCCs on the leaflet. In column (3) and (6), we directly examine if the treatment affected the proportion of switchers enrolling at a PCC on the leaflet. That is, we include only individuals who did switch provider. Indeed, the intervention made PCCs on the leaflet relatively more popular among switchers (in both PRS and NR).

TABLE 6—SWITCHES TO OTHER PCCs ON THE LEAFLET

<i>Treatment effect on switching rate after entire follow-up period (36/21 weeks)</i>						
	PRS			NR		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>info</i>	0.00632 (0.00293)	-0.00153 (0.00414)	0.0931 (0.0333)			
<i>info&amp;form</i>	0.00550 (0.00187)	0.00176 (0.00222)	0.0406 (0.0212)	0.0147 (0.00559)	0.0106 (0.00672)	0.0720 (0.0365)
<i>Constant</i>	0.0196 (0.00189)	0.0336 (0.00340)	0.349 (0.0210)	0.0975 (0.0196)	0.156 (0.0193)	0.403 (0.115)
<i>N</i>	112,575	112,575	6,479	6,803	6,803	680
<i>R</i> <sup>2</sup>	0.007	0.030	0.046	0.011	0.071	0.168
<i>p</i>	0.807	0.466	0.163	.	.	.
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All	All	Switchers	All	All	Switchers
Outcome	<i>Other</i> <i>on leaflet</i>	<i>Not</i> <i>on leaflet</i>	<i>Other</i> <i>on leaflet</i>	<i>Other</i> <i>on leaflet</i>	<i>Not</i> <i>on leaflet</i>	<i>Other</i> <i>on leaflet</i>

*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 PCCs 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 PCCs on the leaflet. PRS estimates are weighted by the inverse of the probability of being drawn. *p* is the p-value on test of equality of coefficients on *info&form* and *info*. Robust standard error in parentheses (clustered by PCC defining the leaflet for NR).

## DID PEOPLE SWITCH TO BETTER RATED CENTERS?

To examine if the interventions induced people to choose better rated PCCs, we estimate average standardized treatment effects as described in Section III.C). Table 7 shows the results for the PRS in Panel A and NR in Panel B. For the PRS, the effect for all items in column (1) is positive and significant for the *info*-treatment (around 0.014 standard deviations,  $p < 0.01$ ), and positive but

smaller and not significant for the *info&form*-treatment (0.005 standard deviations). Columns (2) and (3) show that both the items related to the quality of care and the items describing special clinics and services contribute to the positive coefficient, though the coefficients for the quality items are larger. In contrast, the overall effect in the NR sample, presented in column (1), is negative and marginally significant (-0.011 standard deviations,  $p < 0.1$ ). The coefficients in column (3) and (5) indicate that both quality and special clinics items contribute to the negative effect. An explanation for this finding may be found in the composition of the NR sample. In particular, the counter-intuitive negative result is driven by the individuals with missing background information, who recently immigrated and thus might not have understood the information. Column (2) of Table 7 shows that the standardized coefficient for all items is small and insignificant when excluding the new immigrants from the sample. Columns (4) and (6) reveals that the quality related items have a positive estimate, whereas the special clinics items has a negative estimate (both are insignificant). A potential explanation for the latter effect is that most new residents are rather young, and may not yet have developed the type of health issues that require the services of the special clinics.

In sum, we find evidence that the *info* treatment made individuals choose better rated PCCs, whereas the evidence is weaker for the *info&form*. The magnitudes of the effects may seem small, but recall that the definition of the outcome variables makes the effects zero for individuals who did not switch or stay because of the interventions. In Online Appendix H, we show that the effects are about an order of magnitude larger when the sample is restricted to those who switched during the follow-up period. As these restricted samples do not capture the effects of individuals who decided to stay enrolled at their initial PCC due to the intervention, we prefer the specifications reported here. Averaging standardized treatment effects increases statistical power, but may hide variation in how individuals value different provider characteristics. In our case, the underlying regression estimates

(se Online Appendix H) suggest that the averages are composed by small positive effects on many items rather than a mix of opposing effects.

TABLE 7—STANDARDIZED TREATMENT EFFECTS

	Panel A: Population-representative sample (PRS)					
	All		Quality		Clinics	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>info</i>	0.0142		0.0216		0.00970	
	(0.00536)		(0.0111)		(0.00744)	
<i>info&amp;form</i>	0.00460		0.00907		0.00193	
	(0.00302)		(0.00697)		(0.00442)	
<i>N</i>	112,575		112,575		112,575	
	Panel B: New Residents (NR)					
	All		Quality		Clinics	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>info&amp;form</i>	-0.0105	-0.00256	-0.0159	0.0182	-0.00719	-0.0150
	(0.00610)	(0.00744)	(0.0150)	(0.0188)	(0.00966)	(0.0115)
<i>N</i>	6,803	4,477	6,803	4,477	6,803	4,477
<i>No background</i>	incl	excl	incl	excl	incl	excl

*Note:* Standardized treatment effects averaged over three collections of items on leaflets: all items, quality items (recommend, waiting time, phone access, opening hours, continuity, drug guidelines), special clinics (heart failure, asthma, elderly, dementia, chiropractor, naprapath, behavioural therapist, gynaecologist, midwife clinic, child health center). The standardized TEs are calculated from estimates of seemingly unrelated regressions (separate for each experiment) in which each dependent variable (=one per leaflet item) indicates the difference between the individual’s current provider and the provider at the time of the intervention. Zero differences assumed for individuals listed at providers with missing information.

## VI. Concluding remarks

We provide the first evidence that sending individuals comparative information about providers and eliminating certain small switching costs mitigates demand side frictions in the market for primary care. More generally, our study contributes by showing that such frictions can be mitigated even in markets for multi-faceted services. In contrast to previous information interventions that have significantly increased switching rates (Hastings and Weinstein, 2008; Kling et al., 2012), our information material included a multi-dimensional 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 that are among the core arguments for consumer choice arrangements. The relative increases of

the switching rates in our main analyses range from 10-15 percent in the general population to around 26 percent among new residents. The stronger response among new residents may reflect that they have less experience of the regional primary care market, a smaller social network that can supply information, and weaker ties to their current provider.

One may ask whether we would have obtained similar treatment effects if the leaflets had not included any comparative information about providers, but only a reminder about the free choice. Our experimental design does not allow us to completely dismiss this channel. However, one indication that the information mattered is that individuals who only received the information leaflet were more likely to be enrolled at better rated PCCs at the end of follow-up than the control group. Interestingly, individuals who also received a choice form did on average switch to PCCs with similar characteristics (i.e., ratings and number of special clinics) as their initial one. One potential explanation might be that this treatment arm affected individuals who were not satisfied with their current provider but had even more negative priors about the alternative providers. The lower switching costs due to the choice form might have been sufficient to push them to try a new provider.

A remaining question is if similar interventions scaled up to market level would have induced enough extra mobility to improve competition over quality. The ultimate value of patient choice lies in its ability to improve individuals' health and well-being. As such effects may take years to appear, we leave for future work to examine if improved access to information about care providers helps the choice system deliver on this promise.

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Online Appendix to INFORMATION, SWITCHING  
COSTS, AND CONSUMER CHOICE: EVIDENCE FROM  
TWO RANDOMIZED FIELD EXPERIMENTS IN  
SWEDISH PRIMARY HEALTH CARE

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## A Leaflet example

The subsequent two pages include an example of a leaflet from the treatment arm with a choice form in the intervention to the population-representative sample. The leaflet was a folded paper in A4 format, with the comparative information about four primary care centers (PCCs) 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. Thereafter, Table A.1 and Figure A.1 show that there was considerable variation in terms of most items on the leaflets, in the region as a whole as well as within a given leaflet.



## 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](http://1177.se):

[www.1177.se/Skane/Regler-och-rattigheter/Valja-vardcentral/](http://www.1177.se/Skane/Regler-och-rattigheter/Valja-vardcentral/)

På [1177.se](http://1177.se) kan du också jämföra fler vårdcentraler via tjänsten ”Hitta och jämför vård”.

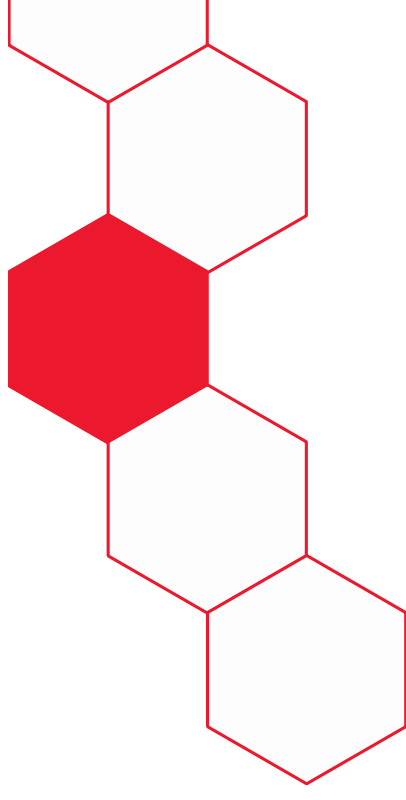
Om du inte gör något nytt val står du kvar som listad på din nuvarande vårdcentral.

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

#### **You know that you have a choice?**

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

You do not have to be enrolled at the care center closest to your home. For example, you can choose a care 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 enrolled 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 at which you wish to enroll, or send it by mail (postage is paid).
- use Region Skåne e-service My Care Contacts.

More information about My Care Contacts is available at [1177.se](http://1177.se), where you can also print a choice form:

[www.1177.se/Skane](http://www.1177.se/Skane)

At [1177.se](http://1177.se), you also compare more care centers via the "Find and compare care" service. If you do not make a new choice, you will remain enrolled at your current care center.

## Translation of leaflet: PRS treatment without choice form

### *Front page:*

#### **You know that you have a choice?**

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

You do not have to be enrolled at the care center closest to your home. For example, you can choose a care 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 enrolled 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 at which you wish to enroll.
- use Region Skåne e-service My Care Contacts.

More information about My Care Contacts is available at [1177.se](http://1177.se), where you can also print a choice form:

[www.1177.se/Skane](http://www.1177.se/Skane)

At [1177.se](http://1177.se), you also compare more care centers via the "Find and compare care" service. If you do not make a new choice, you will remain enrolled at your current care center.

## Translation of leaflet: New residents

### *Front page:*

#### **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 by enrolling at a care center. If you do not make an active choice, you are automatically enrolled at the care 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 enrolled at the care center closest to your home, and you can switch to another at any time. For example, you can choose a care 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 enrolled 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 at which you wish to enroll, or send it by mail (postage is paid).
- use Region Skåne e-service My Care Contacts.

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

[www.1177.se/Skane](http://www.1177.se/Skane)

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

### **Translation of leaflet: Centerfold (all treatment arms)**

- Address
- Phone number
- Owner
- Number of enrolled 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 meters from the care center, there is also:
    - Child health center
    - Midwife clinic

\* Patient rating from the National Patient Survey. The ratings are measured on a scale where 0 represents the worst possible rating and 100 is the best possible rating. For more information, see "Find and compare care" att 1177.se

\*\* Concerns patients with at least three visits during the last year.

TABLE A.1: Variation in dichotomous leaflet items across PCCs

Item	(1) N	(3) Item=1	(4) Variation
<i>DrugGuidelinesOld</i>	147	51	46
<i>DementiaClinic</i>	149	103	40
<i>AsthmaClinic</i>	149	49	67
<i>HeartfailureClinic</i>	149	19	42
<i>BehavioralTherapist</i>	149	76	53
<i>Gynaecologist</i>	149	33	35
<i>Chiropractor</i>	149	13	35
<i>Naprapath</i>	149	12	35
<i>ChildHealthCenter</i>	149	120	29
<i>MidwifeClinic</i>	149	47	75

*Notes.* Column (1): Item on leaflet. Column (2): N.o. PCCs for which information about this item was available (it is missing for recently established care centers). Column (3): N.o. PCCs that complied with the region's prescription guidelines for elderly (*DrugGuidelinesOld*), was located within 100 meters from other primary care services (*ChildHealthCenter*, *MidwifeClinic*), or offered the service (all other items). Column (4): N.o. unique leaflets on which the leftmost PCC (i.e., the initial provider) differed from at least one other provider on the leaflet.

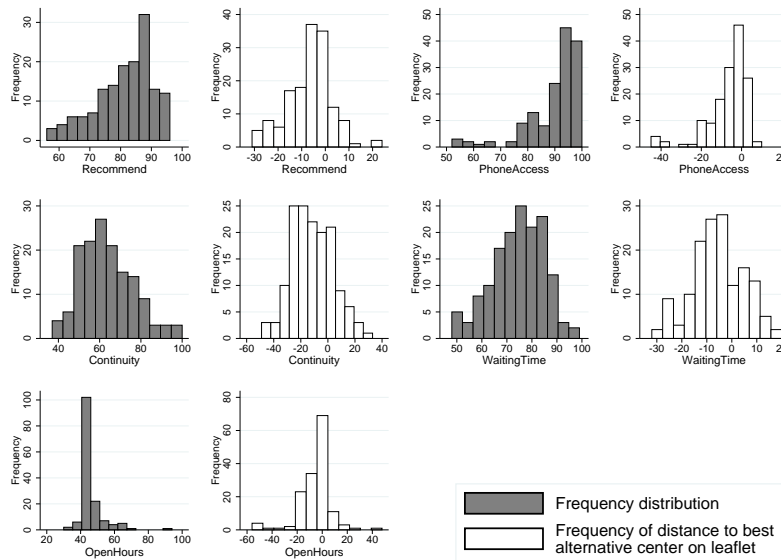


FIGURE A.1: Variation in Continuous Leaflet Items Across PCCs

Gray bars show the frequency distribution for all PCCs in the region. White bars show the frequency distribution of differences between each PCC and the highest-scoring alternative provider on its leaflet. *Enrolled patients* = number of enrolled patients according to the region’s enrollment register. *Recommend* = patient survey respondents’ willingness to recommend their current care provider on a 0-100 scale, with larger values indicating higher willingness to recommend. *WaitingTime* = patient survey respondents’ satisfaction with the time they had to wait to get an appointment on a 0-100 scale, with larger values indicating higher satisfaction. *PhoneAccess* = proportion of mock phone calls from the region that were answered or called back within 2 hours. *Continuity* = proportion of patients with at least 3 visits at the PCC who saw the same physician at least half of the times. Information on *Recommend*, *WaitingTime* and *PhoneAccess* (*Continuity*) was missing for one (two) recently established PCC(s).

## B Robustness of main specification

### B.1 Sequential restriction of sample

In the main text, Table 4 shows the results for a sample restricted to individuals with valid address information and who were still registered in the region the first day of the week when the leaflets were submitted. In Table B.1, we show the impact of sequentially removing the subgroups that are excluded as well as other subgroups that may have been less affected by the intervention. For the population-representative sample in Panel A, column (1) includes all individuals that were drawn from the population.<sup>1</sup> Column (2) excludes the 137 individuals who died or left the region after the randomization but before the extraction of address information. Column (3) additionally excludes 146 individuals who were de-registered between the address extraction date and the first day of the week when the leaflets were submitted. (These estimates corresponds to the main results in Table 4). Although leaflets were sent to these individuals, they ought not have reacted to it as they had either died or left the region.

In the fourth 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. In the fifth column, we only include individuals who also are observed, and have a postal address, in the end of the follow-up period. Notably, the effects of both *info* and *info&form* increase, and become more similar in magnitude, as we restrict the sample.

For the new residents (Panel B), column (1) shows results for the total population (excluding only the one individual with insufficient information from the registers). 102 individuals left the region before the leaflets were submitted; these are excluded from the estimation presented in column (2). In section III.B we mention 253 individuals being residents in the

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<sup>1</sup>These estimates still exclude the one individual who opted out from the study.

TABLE B.1: Sequential restriction of estimation sample

Panel A: Population-representative sample (PRS)					
<i>Treatment effect on switching rate after 36 weeks</i>					
	(1)	(2)	(3)	(4)	(5)
<i>info</i>	0.00569 (0.00472)	0.00571 (0.00473)	0.00577 (0.00473)	0.00601 (0.00476)	0.00602 (0.00464)
<i>info</i> & <i>form</i>	0.00803 (0.00263)	0.00809 (0.00264)	0.00816 (0.00265)	0.00833 (0.00266)	0.00842 (0.00269)
<i>Constant</i>	0.0526 (0.00346)	0.0527 (0.00346)	0.0529 (0.00346)	0.0532 (0.00347)	0.0538 (0.00351)
<i>N</i>	112,858	112,721	112,575	111,810	108,987
<i>R</i> <sup>2</sup>	0.024	0.024	0.024	0.024	0.025
<i>p info=info</i> & <i>form</i>	0.659	0.654	0.654	0.665	0.653

Panel B: New Residents (NR)					
<i>Treatment effect on switching rate after 21 weeks</i>					
	(1)	(2)	(3)	(4)	(5)
<i>info</i> & <i>form</i>	0.0231 (0.00757)	0.0233 (0.00768)	0.0219 (0.00777)	0.0229 (0.00813)	0.0240 (0.00820)
<i>Constant</i>	0.167 (0.0225)	0.169 (0.0230)	0.161 (0.0237)	0.173 (0.0244)	0.172 (0.0248)
<i>N</i>	6,905	6,803	6,557	6,121	5,901
<i>R</i> <sup>2</sup>	0.032	0.031	0.031	0.034	0.033

*Notes.* Eq. 1 (including covariates) estimated on restricted samples. **Panel A:** col. 1 includes all individuals drawn from the population, col. 2 excludes 137 individuals who left/died after randomization but before extraction of address information. Col. 3 excludes additional 146 individuals who left the region before the leaflets were posted (and corresponds to the main result in Table 4). Col. 4 further excludes individuals with no coordinate information. Col. 5 also excludes individuals who are not observed, or lack coordinate information, at the end of the follow up period. *p info*&*form=info* = p-value of test of difference between the estimates on *info*&*form* and *info*. **Panel B:** col. 1 includes all individuals for which we have complete data from the local registers (excludes one observation). Col 2. excludes 102 individuals from the initial total sample, who died or left the region before the leaflets were posted. These estimates corresponds to the main result in Table 4. Col. 3 also excludes 246 individuals, who were residents in the region already on December 31 2014 (these are retained in the sample in column 4 and 5). Col 4 to 5 sequentially excludes the same type of individuals as in Panel A. Robust standard errors in parentheses (clustered by residential address in NR sample).



region on December 31 2014 (seven of these are already excluded in the main estimations). These are removed in column (3), but retained in the sample in column 4 and 5. In column (4) to (5), we repeat the restrictions from Panel A (i.e., being in the sample and having information on coordinates at the end of the follow up period).

## B.2 Randomization inference

Young (2016) suggests that conventional regression inference methods may be problematic, even with relatively large sample sizes. Table B.2 shows results using an alternative mode of inference, in which we obtain p-values for the estimated t-statistics of the treatment effects in Table 4 by randomly permuting the treatment status of each individual, estimating the regression and t-statistics, repeating the permutation exercise 2,000 times and then calculating the share of permutations for which the t-statistics are larger (in absolute value) than the corresponding ones in Table 4.

We use Stata's *permute* command to permute the treatment data for the PRS, stratifying treatment by PCC (as in the original randomization for this sample). The *permute* command is run separately for each of the four specifications in Panel A of the main results table, i.e., treatment status is reassigned 4\*2,000 times.

Because the NR experiment was block randomized by residential address, the *permute* command cannot straightforwardly be applied. Instead, we loop the original code for randomization 2,000 times, each time using a different seed for the pseudo-random number generator. In difference to the PRS permutations, we use the same permutations for the regressions with and without covariates, i.e., treatment status is reassigned 2\*2,000 times.

Table B.2 is structured similarly as Table 4, i.e., column 1 and 2 shows the results for the PRS and column 3 and 4 for the NR. The first row of each treatment arm,  $\beta$ , displays the estimated treatment effects of the main specifications, with the corresponding t-statistics right below. Thereafter, three statistics are shown: the permutation inference p-value and the upper and lower bounds of the 95% confidence interval around this p-value.

The randomization inference yields results that are close to the standard inference used in the main text. We therefore conclude that our results are not sensitive to the mode of inference.

TABLE B.2: Randomization inference on main specifications

<i>Treatment effect on switching rate after 36/21 weeks</i>				
	PRS		NR	
	(1)	(2)	(3)	(4)
<i>info</i>				
$\beta$	0.00606	0.00578	.	.
t-statistics	1.306	1.260	.	.
$p$	0.192	0.209	.	.
<i>Lower 95 CI</i>	0.210	0.227	.	.
<i>Upper 95 CI</i>	0.175	0.191	.	.
<i>info&amp;form</i>				
$\beta$	0.00825	0.00816	0.0203	0.0233
t-statistics	3.011	3.014	2.594	3.041
$p$	0.005	0.004	0.006	0.001
<i>Lower 95 CI</i>	0.009	0.008	0.003	0.000
<i>Upper 95 CI</i>	0.002	0.002	0.011	0.003
$N$	112,575	112,575	6,803	6,803
Number of Strata	150	150		
Covariates	No	Yes	No	Yes

*Notes.* Randomization inference for the preferred specifications using 2,000 permutations of treatment.  $\beta$  and *t-statistics* are the estimated coefficient and the t-statistic from the corresponding specification in Table 4. *p-value* = estimated p-value, i.e., the share of permutations for which the absolute value of the estimated t-statistic exceeds that of the corresponding specification in Table 4 as shown above. *Lower (Upper) 95 CI* = lower (upper) bound of the 95% confidence interval around *p-value*. Columns (1) and (2) show permutation results for the population representative sample with and without covariance. Columns (3) and (4) show results for the new residents, with and without covariates.

### B.3 Other sensitivity tests

Table B.3 shows sensitivity tests of the specification including covariates in column 2 and 4 of Table 4). Column (1) shows a specification without PCC (i.e., strata in the PRS intervention) fixed effects (for the NR, this coincides with the preferred specification), and column (3) shows specification including fixed effects for the individual’s initial PCC (for

the PRS, this coincides with the preferred specification). Column (2) shows results from an estimation without sample weights (only relevant for PRS). For PRS, the results are hardly affected by any of these specification changes. For NR, the treatment effect is slightly larger when applying PCC fixed effects. The final column display average partial effects from a standard logit model. These are almost of the exact magnitude as the corresponding coefficients from the linear probability models.

TABLE B.3: Sensitivity test

Panel A: Population-representative sample (PRS)				
<i>Treatment effect on switching rate after 36 weeks</i>				
	(1)	(2)	(3)	(4)
<i>info</i>	0.00577 (0.00473)	0.00578 (0.00473)	0.00587 (0.00479)	0.00564 (0.00443)
<i>info&amp;form</i>	0.00816 (0.00265)	0.00816 (0.00265)	0.00823 (0.00288)	0.00768 (0.00259)
<i>Constant</i>	0.0529 (0.00346)	0.0529 (0.00347)	0.0470 (0.00280)	
<i>N</i>	112,575	112,575	112,575	112,575
<i>R</i> <sup>2</sup>	0.024	0.024	0.025	
<i>p info=info&amp;form</i>	0.654	0.654	0.668	0.684

Panel B: New Residents (NR)				
<i>Treatment effect on switching rate after 21 weeks</i>				
	(1)	(2)	(3)	(4)
<i>info&amp;form</i>	0.0233 (0.00768)		0.0258 (0.00761)	0.0237 (0.00771)
<i>Constant</i>	0.169 (0.0230)		0.175 (0.0240)	
<i>N</i>	6,803		6,803	6,803
<i>R</i> <sup>2</sup>	0.031		0.065	

*Notes.* Column (1): Specification without care center fixed effects (=preferred specification for NR) on total sample. Column (2): No sample weights (only relevant for PRS). column (3): Specification including care center fixed effects (=preferred specification for PRS). Column (4): Average Partial Effects from logit models. Robust standard errors in parentheses (clustered by residential address in NR sample).

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 have been affected by spillover effects. In column (2), the estimation sample only includes observations for

which there was no duplicate in terms of residential address. In column (3), the sample only includes observations for which either all or none of all duplicates were treated. In column (4), treated observations are only included if there are also non-treated observations at the same address. Column (5) includes only observations for which all duplicates are in the same treatment arm. The results are consistent with the preferred specification.

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

<i>Treatment effect on switching rate after entire follow-up period (36/21 weeks)</i>					
	(1)	(2)	(3)	(4)	(5)
<i>info</i>	0.00577 (0.00473)	0.00529 (0.00451)	0.00627 (0.00463)	0.00576 (0.00473)	0.00560 (0.00454)
<i>info&amp;form</i>	0.00816 (0.00265)	0.00817 (0.00299)	0.00849 (0.00294)	0.00814 (0.00265)	0.00775 (0.00260)
<i>Constant</i>	0.0529 (0.00346)	0.0516 (0.00379)	0.0533 (0.00348)	0.0534 (0.00348)	0.0534 (0.00347)
<i>N</i>	112,575	98,526	110,028	111,176	111,502
<i>R</i> <sup>2</sup>	0.024	0.024	0.024	0.024	0.024
<i>p info=info&amp;form</i>	0.654	0.583	0.674	0.654	0.673

*Notes.* column (1) shows the baseline specification on the total sample. The following columns exclude observations potentially affected by spillover effects according to the following definitions; column (2) includes only observations with no duplication in terms of residential address; column (3) includes observation only if all/non duplications are treated; column (4) includes treated observations only if there are non-treated duplications at same address; column (5) includes only observations if all duplications with the same address are in the same treatment group. Robust standard errors in parentheses.

## C Further switching patterns

The main specification concerns whether individuals have switched providers at least once during the follow-up period. Table C presents estimations related to the further switching patterns during the follow-up period. The first two columns show estimations of the probability of switching back to the initial PCC; column (1) includes the total sample while column (2) only includes those who switched at least once. Column (3) shows estimates of the probability of being enrolled at another PCC than the initial one at the end of the follow-up period (or when the individual was last observed).<sup>2</sup> Finally, column (4) shows estimates of the total number of PCC changes during the follow-up period.

Compared to their respective control groups, the probability of switching back to the initial PCC is similar for treated new residents and for PRS individuals in the treatment arm without a choice form. For the PRS treatment arm *with* a choice form, the probability is slightly higher (significant at the 5% level) compared to the control group. Notably, because the group that switched back account for a small proportion of all changes, the ultimate effect of *inform* on the probability of being enrolled at a different PCC than the initial one at the end of the follow-up (column 3) is still of considerable magnitude, highly significant, and similar to the effect on the probability of any change in the main results in Table 4. The final column shows that there are only negligible effects on the number of changes, on top of the effect on any change.

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<sup>2</sup>Similar to the variable used in Table 4, this 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 in the main results, we use the last known registration of individuals who left the region or died during the follow-up period.

TABLE C.1: Further Switching Pattern

Panel A: Population-representative sample (PRS)				
	Switching back		Other provider	Nr. of switches
	(1)	(2)	(3)	(4)
<i>info</i>	0.000368 (0.000844)	-0.00259 (0.0138)	0.00521 (0.00471)	0.00481 (0.00502)
<i>info&amp;form</i>	0.00133 (0.000649)	0.0158 (0.00964)	0.00701 (0.00255)	0.00919 (0.00311)
<i>Constant</i>	0.00121 (0.000486)	0.0236 (0.00827)	0.0519 (0.00348)	0.0544 (0.00386)
<i>N</i>	112,575	6,479	112,575	112,575
<i>R</i> <sup>2</sup>	0.003	0.018	0.023	0.024
<i>p info=info&amp;form</i>	0.395	0.304	0.730	0.441

Panel B: New Residents (NR)				
	Switching back		Other provider	Nr. of switches
	(1)	(2)	(3)	(4)
<i>info&amp;form</i>	-0.000236 (0.00110)	-0.00308 (0.0102)	0.0239 (0.00759)	0.0230 (0.00845)
<i>Constant</i>	0.00122 (0.00299)	-0.00183 (0.0311)	0.166 (0.0229)	0.175 (0.0242)
<i>N</i>	6,803	680	6,803	6,803
<i>R</i> <sup>2</sup>	0.003	0.029	0.032	0.029

*Notes.* The table shows estimations similar to our baseline specification (including covariates), but with alternative outcome variables. In column (1), the outcome variable is a dummy variable equal to 1 if the individual switched back to the initial care center during the follow-up period. Column (2) uses the same outcome variable but condition the sample on having switched during the follow up period. In column (3), the outcome variable is a dummy variable equal to 1 if the individual was enrolled at another care center than the initial one at the end of the follow-up period (or at the last date the individual is observed). In column (4), the outcome variable is the number of times the individuals switched care center during the follow-up period. Robust standard errors in parentheses (clustered by residential address in NR sample)

## D Heterogeneity: Rural vs Urban

In the main text, we illustrated how the treatment effect depends on the location of the individuals' initial provider. The treatment effect is larger among individuals enrolled at urban PCCs. In these estimations, the definition of *urban* and *rural* PCCs is based on either the rurality of the town where the initial PCC is located or the number of PCCs within a radius of 1, 3, and 5 km from the initial one. In this section, we show results using four instead of two categories of the rural/urban dimension.

The results are displayed in Table D.1. In column (1), urban/rural is defined by the type of town where the initial PCC is located. Urban PCCs in category 1 (the reference category) have at least 4 competitors in the town or city where they are located, while category 2-centers have between 2-4 competitors. Analogously, we divide the rural PCCs into two categories: Category 3-centers have at most one competing center,<sup>3</sup> while centers in category 4 have none; i.e., they constitute a local monopoly. In columns (2) to (4) the categories are instead based on the number of competitors a PCC has within a radius of 1, 3, or 5 kilometers. Category 1 is again the most urban (and the reference category) corresponding to having more than 4 competitors within, e.g., 5 kilometers. (As only one PCC has 4 or more competitors within 1 km, PCCs with 3 competitors within 1 km are included in category 1 in column 2.) Category 2-centers have 2-4 competitors, and the rural categories corresponds to having 1 (category 3) and 0 competitors (category 4).

The treatment effects are typically largest in the most urban PCCs (category 1). They might therefore be driving most of the heterogeneity in the main text, although the difference to other urban PCCs (category 2) is not significant in any specification. There is no support for a treatment effect in any of the rural categories (category 3 and 4).

Another way to capture the potential lack of choice in more rural areas is the individual's proximity to the first alternative provider. Table D.2 show estimates of distance based

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<sup>3</sup>The exception is one rural town, Tomelilla, where each center has two competitors but one of the centers is the smallest center in the region.

heterogeneity using three definition of proximity. In column 1 and 4, proximity is defined by the straight line distance between the individual’s initial PCC and the closest competitor of that PCC (similar to the definition of rural PCCs in Table D.1). The proximity variable used in column 2 and 5 is instead based on the distance between the individual’s residence and its second closest PCC. In column 3 and 6, proximity use the *excess* distance to the second PCC compared to the closest. For all definitions we use three categories: within 1 km; within 1 km to 3 km and within more than 3 km.

Column (1) and (4) confirms the results from Table D.1, showing that individuals who were initially enrolled at PCCs with a competitor nearby are more responsive to treatment. The results are however not as strong as the results in Table D.1, which consider the number of nearby competitors and not only the distance to the closest. This suggests that information is more important where there are several – more than two – available PCCs to choose from.

The results in column (2) and (3) for the PRS-sample and (5) and (6) for the NR-sample are similar for *info&form* with significantly larger effects for individuals with a second provider within 1 km compared to individuals not having a second one within 3 km (for which the effects are effectively zero). The pattern is the same for the *info*-treatment, although the differences are not statistically different from zero.



TABLE D.1: Rural vs Urban

Panel A: Population-representative sample (PRS)				
<i>Treatment effect on switching rate after 36 weeks</i>				
	(1)	(2)	(3)	(4)
<i>info</i> ∩ <i>form</i>	0.0175 (0.00796)	0.0370 (0.0205)	0.0209 (0.00966)	0.0170 (0.00803)
<i>info</i> ∩ <i>form</i> × <i>rural cat2</i>	-0.0154 (0.0120)	-0.0113 (0.0252)	-0.0146 (0.0120)	-0.0154 (0.0112)
<i>info</i> ∩ <i>form</i> × <i>rural cat3</i>	-0.0267 (0.0132)	-0.0390 (0.0220)	-0.0314 (0.0138)	-0.0329 (0.0123)
<i>info</i> ∩ <i>form</i> × <i>rural cat4</i>	-0.0240 (0.0107)	-0.0388 (0.0213)	-0.0275 (0.0120)	-0.0185 (0.0116)
<i>info</i> ∩ <i>form</i>	0.0126 (0.00392)	0.0246 (0.00557)	0.0112 (0.00423)	0.0138 (0.00385)
<i>info</i> ∩ <i>form</i> × <i>rural cat2</i>	0.000800 (0.00744)	-0.0190 (0.00768)	0.00613 (0.00657)	-0.00243 (0.00693)
<i>info</i> ∩ <i>form</i> × <i>rural cat3</i>	-0.0140 (0.00803)	-0.0166 (0.00741)	-0.0163 (0.00806)	-0.0216 (0.00744)
<i>info</i> ∩ <i>form</i> × <i>rural cat4</i>	-0.0112 (0.00646)	-0.0177 (0.00693)	-0.00825 (0.00686)	-0.0109 (0.00702)
<i>N</i>	112,575	112,575	112,575	112,575
<i>R</i> <sup>2</sup>	0.024	0.024	0.024	0.024
definition	town	radii	radii	radii
radii		1 km	3 km	5 km

Panel B: New Residents (NR)				
<i>Treatment effect on switching rate after 21 weeks</i>				
	(1)	(2)	(3)	(4)
<i>info</i> ∩ <i>form</i>	0.0344 (0.00952)	0.0272 (0.0219)	0.0291 (0.0102)	0.0341 (0.00965)
<i>info</i> ∩ <i>form</i> × <i>rural cat2</i>	-0.0253 (0.0295)	0.0151 (0.0284)	-0.00124 (0.0235)	-0.0329 (0.0268)
<i>info</i> ∩ <i>form</i> × <i>rural cat3</i>	-0.0631 (0.0266)	-0.0104 (0.0266)	-0.0476 (0.0279)	-0.0497 (0.0248)
<i>info</i> ∩ <i>form</i> × <i>rural cat4</i>	-0.0113 (0.0197)	-0.00987 (0.0247)	-0.000898 (0.0190)	-0.00351 (0.0213)
<i>N</i>	6,803	6,803	6,803	6,728
<i>R</i> <sup>2</sup>	0.033	0.032	0.033	0.034
definition	town	radii	radii	radii
radii		1 km	3 km	5 km

*Notes.* The table shows estimates corresponding to Table 4 in the main text, but the urban and rural centers are further divided into finer categories depending on the number of competitors within the same town or within 1,3, or 5 kilometers. Robust standard errors in parentheses (clustered by residential address in NR sample).

TABLE D.2: Heterogeneity by distance

<i>Treatment effect on switching rate after entire follow-up period (36/21 weeks)</i>						
	PRS			NR		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>info</i>	0.0132 (0.00747)	0.00849 (0.00889)	0.00898 (0.00631)			
<i>info</i> × 1-3km	-0.00793 (0.0114)	0.000946 (0.0114)	-0.0108 (0.0115)			
<i>info</i> × >3km	-0.0199 (0.0103)	-0.00658 (0.0110)	-0.00591 (0.0108)			
<i>info&amp;form</i>	0.00938 (0.00340)	0.0156 (0.00487)	0.0148 (0.00353)	0.0277 (0.0103)	0.0404 (0.0119)	0.0281 (0.00966)
<i>info&amp;form</i> × 1-3km	0.00323 (0.00694)	-0.00504 (0.00706)	-0.0212 (0.00697)	-0.0207 (0.0192)	-0.0192 (0.0195)	0.00149 (0.0247)
<i>info&amp;form</i> × >3km	-0.00646 (0.00639)	-0.0142 (0.00615)	-0.0140 (0.00635)	0.000415 (0.0190)	-0.0475 (0.0198)	-0.0398 (0.0205)
<i>N</i>	112,575	112,575	112,575	6,803	6,803	6,803
<i>R</i> <sup>2</sup>	0.024	0.024	0.024	0.032	0.032	0.034

*Notes.* The table show heterogeneity over various definition of proximity to an alternative center. Col. 1 and 4 are based on the straight-line distance between the individual and its second closest care center (1 km; 1 km < x < 3 km; x > 3 km). Col. 2 and 5 are based on the *excess* straight-line distance to the second center compared to the closest (1 km; 1 km < x < 3 km; x > 3 km). Col. 3 and 6 are based on the straight-line distance between the care center the individual initially was enrolled at and the closest competitor of that center (1 km; 1 km < x < 3 km; x > 3 km).

## E Socioeconomic and demographic heterogeneity

Tables E.1 and E.2 display the results from estimations exploring socioeconomic and demographic heterogeneity. These treatments effects comes from linear regressions including interactions of the treatment variables and the covariate of interest, with the remaining covariates included as controls (the results are similar when estimating one single model in which all interactions of treatment and covariates are included at once).

TABLE E.1: Heterogeneity over socioeconomic status

<i>Treatment effect on switching rate after 36/21 weeks</i>				
	PRS		NR	
	(1)	(2)	(3)	(4)
<i>info</i>	0.00648 (0.00855)	0.00783 (0.00776)		
<i>info</i> × 1 <sup>st</sup> <i>tercile</i>	0.00216 (0.0116)	-0.00706 (0.0128)		
<i>info</i> × 3 <sup>rd</sup> <i>tercile</i>	-0.00224 (0.0142)	0.00599 (0.0109)		
<i>info</i> & <i>form</i>	0.0110 (0.00460)	0.00158 (0.00454)	0.0310 (0.0156)	0.00928 (0.0222)
<i>info</i> & <i>form</i> × 1 <sup>st</sup> <i>tercile</i>	-0.00547 (0.00628)	0.00580 (0.00652)	0.0135 (0.0261)	0.0335 (0.0249)
<i>info</i> & <i>form</i> × 3 <sup>rd</sup> <i>tercile</i>	-0.000903 (0.00688)	0.0146 (0.00686)	-0.0134 (0.0212)	0.00626 (0.0313)
<i>N</i>	112,575	112,575	6,803	6,803
<i>R</i> <sup>2</sup>	0.024	0.024	0.031	0.032
treatment effects <i>info</i>				
1 <sup>st</sup> <i>tercile</i>	0.00863	0.000775		
<i>p</i>	0.273	0.934		
3 <sup>rd</sup> <i>tercile</i>	0.00423	0.0138		
<i>p</i>	0.668	0.078		
treatment effects <i>info</i> & <i>form</i>				
1 <sup>st</sup> <i>tercile</i>	0.00551	0.00738	0.0445	0.0428
<i>p</i>	0.204	0.102	0.037	0.000
3 <sup>rd</sup> <i>tercile</i>	0.0101	0.0162	0.0176	0.0155
<i>p</i>	0.041	0.002	0.232	0.505
Socioeconomic variable	educ	income	educ	income

*Notes.* The table shows estimations on heterogeneity in the treatment effect over socioeconomic status (income and education) for the two samples, PRS in col. 1 and 2 and NR in col. 3 and 4. Col. 1 and 3 (2 and 4) show results for education (income).

Generally, there is no clear socioeconomic gradient or consistent demographic differences across treatments arms, interventions, and indicators. For the population-representative

TABLE E.2: Heterogeneity over age, sex, & foreign background

<i>Treatment effect on switching rate after 36/21 weeks</i>						
	PRS			NR		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>info</i>	0.0115 (0.00578)	0.00846 (0.00682)	0.0189 (0.0107)			
<i>info</i> × <i>For.background</i>	-0.0214 (0.0119)					
<i>info</i> × <i>Female</i>		-0.00521 (0.00931)				
<i>info</i> × <i>Age</i> < 30			-0.0345 (0.0146)			
<i>info</i> × <i>Age</i> 30 – 45			-0.000223 (0.0148)			
<i>info</i> × <i>Age</i> > 60			-0.0187 (0.0134)			
<i>info</i> × <i>X missing</i>	-0.0481 (0.0129)		-0.264 (0.0647)			
<i>info</i> & form	0.00721 (0.00283)	0.00789 (0.00394)	0.0184 (0.00555)	0.0182 (0.0119)	0.0344 (0.0127)	0.0218 (0.0317)
<i>info</i> & form × <i>For.background</i>	0.00217 (0.00671)			0.0269 (0.0221)		
<i>info</i> & form × <i>Female</i>		0.000531 (0.00534)			-0.0144 (0.0178)	
<i>info</i> & form × <i>Age</i> < 30			-0.0184 (0.00871)			0.00728 (0.0338)
<i>info</i> & form × <i>Age</i> 30 – 45			-0.00872 (0.00775)			0.000197 (0.0383)
<i>info</i> & form × <i>Age</i> > 60			-0.0130 (0.00683)			-0.0170 (0.0501)
<i>info</i> & form × <i>X missing</i>	0.0378 (0.0301)	-0.0769 (0.0577)	-0.272 (0.0645)	0.000720 (0.0165)	-0.0154 (0.0171)	-0.00607 (0.0337)
<i>N</i>	112,575	112,575	112,575	6,803	6,803	6,803
<i>R</i> <sup>2</sup>	0.024	0.024	0.005	0.031	0.031	0.002

*Notes.* Col 1 (4) heterogeneity over foreign background, col 2 (5) over sex, and col 3 (6) over age. *missing X* indicates that information is lacking for the variable of interest. Col 7 show difference in treatment effect for individuals with and without any background data available at Statistic Sweden.

sample, there is a tendency that individuals in the middle income tercile are less likely to respond to treatment. In one of the treatment arms (*info*ℰform), the effect is statistically different from the highest income tercile (at a 5% level). Although the estimates indicate the opposite pattern in the new residents sample, we are reluctant to interpret these results as an actual gradient as the NR-distribution is much more compressed and very few new residents would be in the upper tercile of the PRS-income distribution.

Table E.2 displays the results of our heterogeneity analysis with respect to foreign background, sex and age. There is an age gradient in the PRS (column (3)), with middle-age individuals being more responsive to treatment than younger, and to some degree also older, individuals. The differences in the treatment effect between the youngest age group (below 30) and the middle age group (45 to 60 years old, used as the reference group) are statistically significant at a 5% level for both treatment arms in PRS. In the NR sample, more than half of all individuals for which we have information about age belong to the youngest age group. The estimates for the treatments effects of the older age groups are therefore noisy (column (6)).

The differences in treatment effects between the sexes are small in the PRS (column (2)). The difference is also insignificant in NR; however, the treatment effect is only significant among men (column (5)). The heterogeneity with respect to foreign background suggests that the choice form may play a role for this subgroup. In the PRS (column (1)), the point estimate in the choice form treatment arm is slightly larger for individuals with known foreign background, although the interaction terms are not statistically significant. In the new residents sample (column (4)), individuals of foreign background seem more responsive to the treatment, which included a choice form for all treated. By contrast, the treatment effect in the arm without a choice form in PRS, *info*, is zero for individuals with a known foreign background. The treatment effect for individuals with missing information about background (the  $X \times \textit{missing}$ -variables, which most likely consist of a large share of recent immigrants, follows the same pattern as the treatment effects of individuals with known

foreign background in all treatment arms.

TABLE E.3: Heterogeneity, missing background data

<i>Treatment effect on switching rate after 21 weeks</i>	
	NR
	(1)
<i>info&amp;form</i>	0.0268 (0.00932)
<i>info&amp;form</i> × <i>No background data</i>	-0.0193 (0.0168)
<i>No background data</i>	0.0295 (0.0114)
<i>Constant</i>	0.0798 (0.00604)
<i>N</i>	6,803
<i>R</i> <sup>2</sup>	0.002
<i>info&amp;form</i> + <i>info&amp;form</i> × <i>No background data</i>	0.00751
<i>p</i>	0.592
Covariates	NO

*Notes.* Table shows difference in the treatment effects between individuals with and without any background data available in Statistic Sweden’s register (as of 2015). The estimations use no other covariates than specified in the table as we test differences between individuals with and without information on these covariates

A large share (34%) of the new residents lack background information in Statistics Sweden’s registers. As these registers typically have universal coverage, not having any registered information indicates that the individual is a very recent immigrant. A substantial share of this group are therefore likely refugees from Middle Eastern countries, which arrived in large numbers to Sweden in 2015.

Our experiment was not designed to inform newly arrived refugees. They may have problems understanding the supplied information, may be less informed about the system, and in general have more urgent problems than choosing a primary care provider. In addition, refugees are normally not assigned (or may at least have problems finding) a more permanent place of residence during their first months in the country, and are not guaranteed to stay where they initially are placed. Indeed, the subsample that lack all background information from Statistics Sweden is also overrepresented among individuals with missing data on residential

address coordinates, as well as among the individuals who move between the start and the end of the follow up period.

Accordingly, Table E.3 shows that the baseline switching rate in the group completely lacking background data is close to 3 percentage points larger than the 8 percent switching rate in the subsample with some information from Statistics Sweden ( $p < 0.01$ ). At the same time, the treatment effects in the subsample without background data are not significant and the magnitudes are around a third of the effects in the subsample with some background information.

## F Returned choice forms

Figure F.1 shows the number of sent out choice forms that were returned, according to the date they were registered by the postal service. The x-axis shows 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 1 (b)-(d), which shows the dates when provider changes was registered by the new provider, we see that the lag in Figure 1 is due to the administrative lag between the point in time when individuals returned the form (Figure F.1) and the point in time when their new PCC had registered the change of provider.

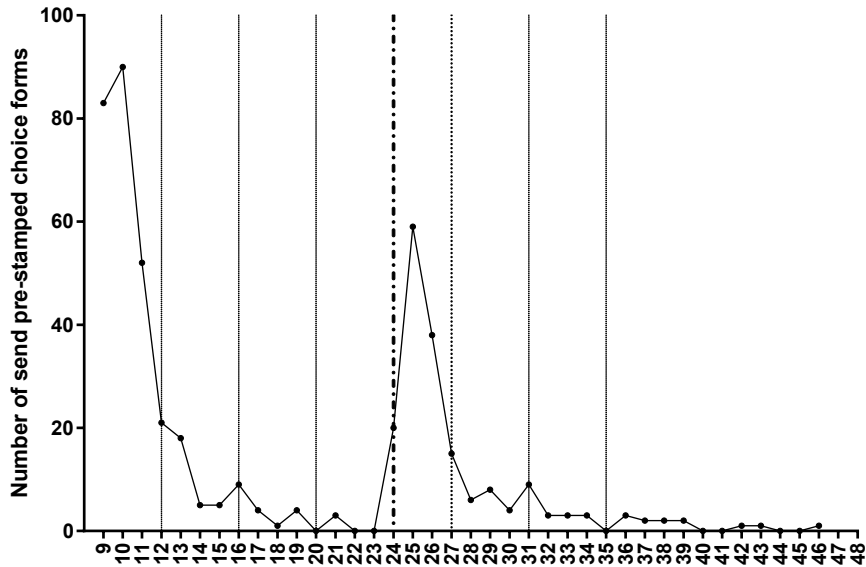


FIGURE F.1: Number of returned choice forms plotted against the number of weeks since January 1, 2015 ( $x=0$ ). The first intervention took place in week 8 ( $x=8$ ).



## G Interaction specification for people who moved

Individuals who moved before (*pre-intervention mover*) or after (*post-intervention mover*) the intervention might have been more responsive to treatment. These individuals might have less information about nearby providers, be more willing to switch (the region does not automatically assign a new provider to individuals moving within the region’s borders), and be less concerned about breaking up established relationships with their earlier provider. Notably, as the leaflet included information about PCCs close to the one where the individual was currently enrolled, irrespective of where they lived, some of the movers might not have received information about providers near their new address. If movers drive the effect to a substantial degree, it is therefore less likely that the information content *per se* drives the treatment effect. In that case, the intervention should better be thought of as a reminder, or as an impetus to search for more information about providers online.

The covariate set of our main estimations include a dummy for being a *pre-intervention mover*. These are individuals that moved to or within the region recently before the intervention (January 2014–February 2015). Moving within the region is defined as changing address *and* having a different closest PCC than before. Column (1) of Table G.1 shows the results of a specification where *pre-intervention mover* is interacted with the two treatment dummies. Thus, the treatment effect for the reference group is the response of individuals who did not move before the intervention. In column (2), we also add a dummy variable for individuals who moved after (but not before) the intervention, and interact this variable with the treatment dummies. We assert that the possibility that our intervention affected people’s decision to move is negligible. Including a variable partly determined by post-treatment in the specification should, in this case, have low risk of biasing the estimations.

Clearly, the effect of the two treatments are hardly affected by the inclusion of the interaction term. The estimate for the treatment with the attached choice form is still highly significant, whereas the estimate for the treatment without the choice form are insignificant with p-values in the similar range as before. As in the main estimations, we cannot reject the

hypothesis that the two treatment effects are equal. The interaction terms are imprecisely estimated, and the treatment effects for *pre/post-intervention mover* are not statistically different from the non-movers. The effect of treatment for *info&form* among individuals who moved before (or after) is, however, larger compared to the reference group, whereas the effect for *info* is smaller.

We do not perform similar estimations for the sample of new residents, as very few of these individuals moved after the intervention, and the *pre-intervention mover* definition makes little sense for the new residents.

TABLE G.1: Interaction specifications; people who changed address

<i>Treatment effect on switching rate after 36/21 weeks</i>		
	PRS	
	(1)	(2)
<i>info</i>	0.00611 (0.00491)	0.00685 (0.00457)
<i>info</i> × <i>pre-intervention mover</i>	-0.00231 (0.0157)	-0.00342 (0.0155)
<i>info</i> × <i>post-intervention mover</i>		-0.0185 (0.0373)
<i>info</i> & <i>form</i>	0.00788 (0.00273)	0.00703 (0.00253)
<i>info</i> & <i>form</i> × <i>pre-intervention mover</i>	0.00215 (0.0102)	0.00297 (0.0102)
<i>info</i> & <i>form</i> × <i>post-intervention mover</i>		0.0143 (0.0268)
<i>Constant</i>	0.0529 (0.00345)	0.0456 (0.00347)
<i>N</i>	112,575	112,575
<i>R</i> <sup>2</sup>	0.024	0.039
<i>p</i> joint		
Covariates	Yes	Yes

*Notes.* The table shows a version of our baseline specification with covariates of 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. *pre-intervention mover* is a dummy for individuals who moved between December 31 2013 and the intervention date (February 22 2015). *post-intervention mover* is a dummy for individuals who moved after, but not before, the intervention. In both cases, moving is defined as having changed address *and* having different closest care centers at the two points in time. 367 (92) pre-intervention (post-intervention) movers received *info* & *form* and 983 (294) received *info*. Robust standard errors in parentheses.

## H Switching and PCC ratings

In Table 7 in the main text we examined if the interventions induced people to choose better rated PCCs using standardized treatment effects for the total samples (as described in Section III.C). As the share that switches centers is relatively small and the definition of the outcome variables makes the effects zero for individuals who did not switch or stay because of the interventions, the magnitudes of the standardized treatment effects are small in these samples. In Table H.1 we condition the estimation samples on having switched during the follow-up period. The restricted samples may give us a better idea of the magnitudes for those affected by treatment, but fails to capture the effects of individuals who stayed enrolled at their initial PCC because they received an intervention.

The sign and significance levels of the standardized treatment effects in Table H.1 are similar to the those presented in the main text, but they are noticeably larger. The positive overall effect for the *info*-treatment in the PRS sample is now 0.19 standard deviations (0.26 for the quality related items and 0.14 for the special clinics items). The effect for the *info&form* is smaller, 0.03 standard deviations (0.09 for the quality related items and -0.015 for the special clinics). The negative overall effect in the NR sample is -0.11, but it is again driven by the subgroup lacking background data. In the subgroup with background data, shown in even columns, is the effect on the quality related items of similar magnitude as the effect for the *info*-treatment in the PRS (0.24). This positive effect is matched by an almost equally large negative effect on special clinics items (-0.22). As we argued in the main text, this sample's relatively young age may make the demand for special clinic services low.

TABLE H.1: Standardized treatment effects for individuals who switched provider

Panel A: Population-representative sample (PRS)						
	All		Quality		Clinics	
	(1)		(2)		(3)	
<i>info</i>	0.168		0.257		0.115	
	(0.0562)		(0.123)		(0.0817)	
<i>info&amp;form</i>	0.0233		0.0888		-0.0160	
	(0.0322)		(0.0769)		(0.0481)	
<i>N</i>	6,479		6,479		6,479	

Panel B: New Residents (NR)						
	All		Quality		Clinics	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>info&amp;form</i>	-0.113	-0.0405	-0.140	0.240	-0.0964	-0.209
	(0.0616)	(0.0820)	(0.150)	(0.194)	(0.0969)	(0.122)
<i>N</i>	680	417	680	417	680	417

*Excl. no background data*

*Notes.* Standardized treatment effects for three collections of items on leaflets: all items, quality items (recommend, waiting time, phone access, opening hours, continuity, drug guidelines), special clinics (heart failure, asthma, elderly, dementia, chiropractor, naprapath, behavioural therapist, gynaecologist, midwife clinic, child health center). The standardized TEs are calculated from estimates of seemingly unrelated regressions (separate for each experiment) in which each dependent variable (=one per leaflet item) indicates the difference between the individual's current provider and the provider at the time of the intervention. Zero differences assumed for individuals listed at providers with missing information. The sample only includes individuals who switched provider at least once during the follow-up. The treatment indicator variable is the only independent variable.

The procedure of summarizing the effects into an index captures the aggregate effect of items that make individually small contributions to the decision of whether to switch or stay. However, it may hide interesting patterns, if there are items that individuals react much stronger to than others. In Table H.2, we present the SURs that underlie the standardized treatment effects in Table 7 (i.e., these estimations include the total sample and not just switchers). The results of the individual SURs show that all quality items except continuity contribute positively to the significant effects for the *info*-treatment in column (1), but only other patients' recommendations and compliance with drug guidelines for the elderly are individually significant (on the 10% level). For *info&form*, all quality items except opening hours contributes positively to the standardized treatment effect, but only waiting time is significant at a 10% level. The coefficients for the special clinic items are of mixed sign for both treatments, and only is significant (closeness to a child health center for the *info&form*-treatment on the 10% level). The overall impression is therefore that the standardized treatment effects reported in the main text better capture the full effects of the interventions.

In NR, there is also a pattern of statistically non-significant effects building up larger standardized treatment effects in both the full sample in column (2) and the sample restricted to the individuals with background data in column (3). Phone access is an exception to this pattern for the full sample, this coefficient is significant ( $p < 0.05$ ). When excluding the subsample lacking background data, which likely consists of a large share of recent immigrants, most coefficients on the quality related items are positive, and all becomes either less negative or more positive. Most special clinics items are negative for both subsamples.

TABLE H.2: SUR

	PRS	NR	
	(1)	(2)	(3)
WaitingTime			
<i>info</i>	0.0692		

*Continued on next page*

	(1)	(2)	(3)
	(0.0624)		
<i>info&amp;form</i>	0.0691 (0.0378)	0.0324 (0.0986)	0.226 (0.119)
<i>Constant</i>	0.180 (0.0932)	-0.314 (0.293)	-0.300 (0.318)
<hr/>			
Recommend			
<i>info</i>	0.0969 (0.0588)		
<i>info&amp;form</i>	0.0456 (0.0369)	0.0302 (0.101)	0.148 (0.123)
<i>Constant</i>	-0.329 (0.0831)	-0.243 (0.287)	-0.242 (0.307)
<hr/>			
PhoneAccess			
<i>info</i>	0.0810 (0.0841)		
<i>info&amp;form</i>	0.00181 (0.0500)	-0.304 (0.137)	-0.0324 (0.165)
<i>Constant</i>	-0.201 (0.0974)	-0.165 (0.410)	-0.271 (0.452)
<hr/>			
Continuity			
<i>info</i>	-0.0471 (0.0803)		
<i>info&amp;form</i>	0.0483 (0.0475)	-0.0196 (0.132)	0.0209 (0.162)
<i>Constant</i>	-0.452 (0.103)	-0.147 (0.459)	-0.0185 (0.495)
<hr/>			
DrugGuidelines for Elderly			
<i>info</i>	0.00448 (0.00239)		
<i>info&amp;form</i>	0.000490 (0.00153)	-0.00336 (0.00451)	-0.00109 (0.00532)
<i>Constant</i>	0.000725 (0.00316)	-0.0180 (0.0145)	-0.0124 (0.0154)
<hr/>			
Opening Hours			
<i>info</i>	0.0735 (0.0475)		
<i>info&amp;form</i>	-0.00193 (0.0301)	-0.0556 (0.0660)	0.0213 (0.0686)
<i>Constant</i>	0.485 (0.0940)	-0.288 (0.183)	-0.173 (0.189)
<hr/>			
Asthma Clinic			
<i>info</i>	-0.00119		

*Continued on next page*

	(1)	(2)	(3)
	(0.00326)		
<i>info&amp;form</i>	-0.00143 (0.00200)	-0.00451 (0.00555)	-0.00560 (0.00660)
<i>Constant</i>	-0.0286 (0.00610)	0.00457 (0.0154)	-0.0119 (0.0165)
<hr/>			
Heartfailure Clinic			
<i>info</i>	-0.000194 (0.00281)		
<i>info&amp;form</i>	-0.00217 (0.00150)	-0.00109 (0.00404)	-0.00583 (0.00475)
<i>Constant</i>	0.0110 (0.00349)	0.000759 (0.0113)	0.00232 (0.0118)
<hr/>			
Elderly Clinic			
<i>info</i>	0.00467 (0.00304)		
<i>info&amp;form</i>	0.000949 (0.00182)	-0.00451 (0.00541)	-0.00910 (0.00642)
<i>Constant</i>	0.0128 (0.00430)	-0.0199 (0.0159)	-0.0199 (0.0168)
<hr/>			
Dementia Clinic			
<i>info</i>	0.00313 (0.00303)		
<i>info&amp;form</i>	0.000887 (0.00196)	-0.00334 (0.00549)	-0.00198 (0.00666)
<i>Constant</i>	-0.0133 (0.00452)	0.0222 (0.0165)	0.0225 (0.0175)
<hr/>			
Gynaecologist			
<i>info</i>	0.00117 (0.00273)		
<i>info&amp;form</i>	-0.000708 (0.00156)	-0.000248 (0.00452)	-0.00758 (0.00545)
<i>Constant</i>	0.0264 (0.00510)	-0.00260 (0.0142)	-0.00106 (0.0151)
<hr/>			
Chiropract_mean			
<i>info</i>	0.000202 (0.00229)		
<i>info&amp;form</i>	0.00000507 (0.00134)	0.0000948 (0.00329)	0.000508 (0.00406)
<i>Constant</i>	-0.0344 (0.00642)	0.0177 (0.0104)	0.0141 (0.0112)
<hr/>			
Chiropractor			
<i>info</i>	0.00131		

*Continued on next page*



	(1)	(2)	(3)
	(0.00202)		
<i>info&amp;form</i>	0.000187 (0.00122)	-0.00120 (0.00327)	-0.00195 (0.00392)
<i>Constant</i>	-0.0409 (0.00651)	0.00261 (0.0115)	-0.00305 (0.0122)
<hr/>			
Behavioral Therapist			
<i>info</i>	0.00216 (0.00309)		
<i>info&amp;form</i>	-0.00161 (0.00195)	0.00196 (0.00537)	0.00121 (0.00618)
<i>Constant</i>	-0.0101 (0.00393)	-0.000998 (0.0144)	0.00566 (0.0154)
<hr/>			
Midwife Clinic			
<i>info</i>	0.00170 (0.00311)		
<i>info&amp;form</i>	0.00128 (0.00192)	-0.000461 (0.00511)	0.000731 (0.00618)
<i>Constant</i>	-0.0204 (0.00545)	0.0211 (0.0168)	0.0183 (0.0181)
<hr/>			
Child Health Center			
<i>info</i>	0.000449 (0.00279)		
<i>info&amp;form</i>	0.00307 (0.00174)	-0.00104 (0.00471)	-0.00232 (0.00550)
<i>Constant</i>	-0.0132 (0.00417)	0.00656 (0.0150)	0.00957 (0.0158)
<i>N</i>	112,575	6,803	4,477
<i>Excl. no background data</i>	No	No	Yes

*Notes.* Seemingly unrelated regressions in which each dependent variable (one per leaflet item) indicates the difference between the individual's current provider and the provider at the time of the intervention. The treatment indicator variable is the only independent variable. Robust standard errors in parentheses (clustered by residential address in NR sample). \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

As five PCCs have missing data on one or more information items, we have to impute outcome variables for 969 (215) individuals in order to use the same sample in all SUR-regressions for PRS (NR). These five PCCs lack information as they were yet to open when the underlying data were measured.<sup>4</sup>

For individuals that are enrolled at a PCC with missing information either at the start or at the end of the follow up period, we cannot compute a start-to-end difference. In the estimations in Table 7 in the main text as well as Table H.2 and Table H.1, we set this difference to zero. In Table H.3, we instead impute the missing data of each center using the median value of all other centers. The outcome variable is then calculated using these imputed values. Overall, both approaches yield similar results.

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<sup>4</sup>Three PCCs started during, or just before, the study period and therefore lacks information on all items, one center lacks information on continuity and drug guidelines for the elderly, and one center lacks information on the drug guidelines only. Only 292 (77) individuals in PRS (NR) are enrolled at a PCC with missing data for all information items.

TABLE H.3: Standardized treatment effects on change in leaflet items

Panel A: Population-representative sample (PRS)						
	All		Quality		Clinics	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>info</i>	0.0146 (0.00529)	0.0263 (0.0113)	0.00753 (0.00750)			
<i>info&amp;form</i>	0.00478 (0.00299)	0.00833 (0.00688)	0.00265 (0.00438)			
<i>N</i>	112,575	112,575	112,575			
Panel B: New Residents (NR)						
	All		Quality		Clinics	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>info&amp;form</i>	-0.0121 (0.00604)	-0.00387 (0.00735)	-0.0183 (0.0149)	0.0162 (0.0187)	-0.00841 (0.00986)	-0.0159 (0.0116)
<i>N</i>	6,803	4,477	6,803	4,477	6,803	4,477

*Incl. missing data*

*Notes.* Standardized treatment effects for three collections of items on leaflets: all items, quality items (recommend, waiting time, phone access, opening hours, continuity, drug guidelines), special clinics (heart failure, asthma, elderly, dementia, chiropractor, naprapath, behavioural therapist, gynaecologist, midwife clinic, child health center). The standardized TEs are calculated from estimates of seemingly unrelated regressions (separate for each experiment) in which each dependent variable (=one per leaflet item) indicates the difference between the individual's current provider and the provider at the time of the intervention. Median/mode value imputed on providers with missing information. The treatment indicator variable is the only independent variable.

## I Just a reminder effect?

That PCCs on the leaflet became relatively more popular among switchers affected by treatment is consistent with the interpretation that the leaflets reduced search costs, and the results in the previous section indicated that at least the *info* treatment made individuals react to the information on the leaflet. However, it is still 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 effects were 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 has never before included comparative information about providers. If earlier reminders were followed by unusually high switching rates, then it is reasonable to assume that our treatment effects overestimate the importance of improved access to comparative information.

The black solid line in Figure I.1 shows weekly switching rates in the PRS sample around the publication dates of a magazine distributed by the regional health authority to all residents in 2014. That year, the first two issues of the magazine – published in June and September – included advertisements of the patient choice policy. 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 patient choice at all.<sup>5</sup> The graphs show no sign of elevated switching rates in the period after the publication of the magazine in 2014. All changes are comparable to or smaller than the fluctuations in 2013.

There are other reasons why the reminder effect of our intervention should be small.

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<sup>5</sup>Distribution dates in 2014: June 17, September 5, and December 10. Distribution dates in 2013: May 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.

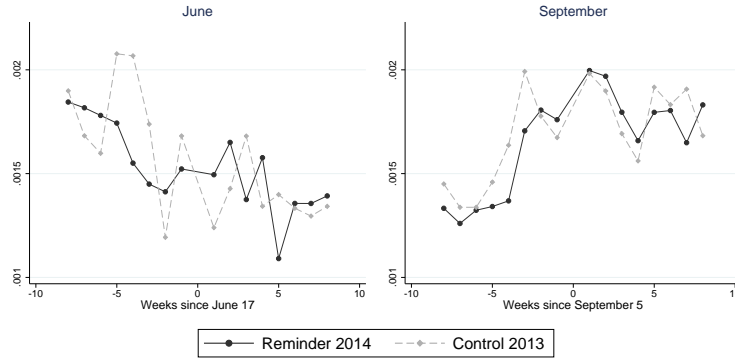


FIGURE I.1: Switching Rates Around Earlier Reminders

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.

Survey evidence indicates that the right to choose care provider is well-known in Sweden (Swedish Agency for Health and Care Services Analysis, 2013),<sup>6</sup> indicating that the effect of reminding people that they can choose is small. We would mainly expect noticeable reminder effects among 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 G, 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. 1a).

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 PCC, but did not contain any more information about providers. If the marginal impact of reminders is decreasing, which appears

<sup>6</sup>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).

reasonable,<sup>7</sup> 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.

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<sup>7</sup>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.

## References

- Ericson, K. M. M., J. Kingsdale, T. Layton, and A. Sacarny (2017). Nudging leads consumers in Colorado to shop but not switch ACA marketplace plans. *Health Affairs* 36(2), 311–319.
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