Journal of Public Economics 196 (2021) 104390

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

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

Anders Anell [a], Jens Dietrichson [b], Lina Maria Ellegård [c], Gustav Kjellsson [d,*]

[a] Department of Business Administration, Lund University, Sweden
[b] VIVE - The Danish Centre of Applied Social Science, Denmark
[c] Department of Economics, Lund University, Sweden
[d] Department of Economics, University of Gothenburg, P.O. Box 640, SE 405 30 Gothenburg, Sweden

ARTICLE INFO

Article history:
Received 7 April 2019
Revised 17 November 2020
Accepted 19 February 2021
Available online 19 March 2021

JEL codes:
D89
I11

Keywords:
Information friction
Switching costs
Consumer choice
Field experiment
Primary care

ABSTRACT

Consumer choice policies may improve the matching of consumers and providers, and may spur competition over quality dimensions relevant to consumers. However, the gains from choice may fail to materialise in markets characterised by information frictions and switching costs. We use two large-scale randomised field experiments in primary health care to examine if individuals reconsider their provider choice when receiving leaflets with comparative information and pre-paid choice forms by postal mail. The first experiment targeted a representative subset of the 1.3 million residents in a Swedish region. The second targeted new residents in the same region, a group expected to have less prior information and lower switching costs than the general population. The propensity to switch providers increased after the interventions in both the population-representative sample (by 0.6–0.8 percentage points, 10–14%) and among new residents (2.3 percentage points, 26%). The results demonstrate that there are demand side frictions in the primary care market. Exploratory analyses indicate that the effects on switching were larger in urban markets and that the interventions had heterogeneous effects on the type of providers chosen, and on health care and drug consumption.

1. Introduction

In health care and education markets, consumers are often allowed to choose from a menu of providers. On the premise that consumers have superior knowledge of their preferences and needs, consumer choice may improve quality by improving the matching of consumers and providers, and by strengthening providers' incentives to compete on quality. However, the empirical literature suggests that the desired quality improvements are not always borne out in practice. Studies of increased patient choice of hospitals have shown mixed effects on health outcomes (e.g., Cooper et al., 2011; Gaynor et al., 2013, 2016; Moscelli et al., 2016, 2018; Skellern, 2017), and the effects are small or absent in primary care (e.g., Dietrichson et al., 2020; Gravelle et al., 2019). In education, school choice and vouchers have mostly had small or insignificant effects on achievement (e.g., Rouse and Barrow, 2009; Fryer, 2017; Epple et al., 2017).[1]

☆ Acknowledgements: We would like to thank the editor, Keith Marzilli Ericson, and two anonymous reviewers for suggestions that improved the paper. This study would not have been possible without close collaboration with staff at Region Skåne. We are in particular grateful to Magnus Kåregård 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 helping us with data, delivery, and design of the information material. We are also thankful to Martin Bøg, Dennis Petrie, Visa Pitkänen, Erik Wengström, Ge Ge, and seminar participants at University of Southern Denmark, University of Gothenburg, SFI Advisory Board conference, 2016 SHEA conference, 2016 NHESG conference, 2016 Swedish national conference in Economics, Research Institute for Industrial Economics, the 8th Swedish Workshop on Competition and Public Procurement Research, KORA, SFI-Lund Workshop in Health Economics, Monash, HELED, 2017 IHEA conference, the 2019 ASHE conference, 2019 EALE conference, IAB-workshop on field experiment in policy evaluation, and the Health Economics Workshop in Stralsund for helpful comments. Financial support from the Swedish Competition Authority (Dnr:316/2013;214/2017) and The Crafoord foundation is gratefully acknowledged.

* Corresponding author.
E-mail addresses: anders.anell@fek.lu.se (A. Anell), jsd@vive.dk (J. Dietrichson), linamaria.ellegard@nek.lu.se (L.M. Ellegård), gustav.kjellsson@economics.gu.se (G. Kjellsson).

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

https://doi.org/10.1016/j.jpubeco.2021.104390
0047-2727/

Understanding why consumer choice may fail to improve the quality of services is crucial to find ways to improve choice systems, and relates to a core question in economics: how individual decision making affects market efficiency. It is well-known that the link between consumer choice and welfare may be weakened by market frictions, such as imperfect information about providers (Arrow, 1963) or transaction costs associated with switching providers (Klemperer, 1995). Using data from two randomised field experiments that manipulated consumers' access to information and switching costs, this paper provides the first experimental evidence that demand side frictions do prevail in the market for primary health care. Furthermore, the experiments provide evidence on how to increase consumer mobility and how information interventions change the matching between consumers and providers.

Studies from diverse settings indicate that consumers rarely search for publicly available comparative quality information before choosing a health care provider (Victoor et al., 2012). This behaviour is consistent with a rational response to search costs, i.e., the time and effort required to find comparative information, as in models of rational inattention (e.g., Sims, 2003; Gabaix, 2014; Matějka and McKay, 2014). Information friction may also be caused by related forms of bounded rationality, such as limited attention (e.g., Bordalo et al., 2013; Caplin, 2016) and status quo bias (Samuelson and Zeckhauser, 1988), or by consumers not understanding health-related information (e.g., Hibbard et al., 2007) or concepts related to health insurance (Loewenstein et al., 2013; Bhargava et al., 2017).[2] Notably though, the observation that consumers do not access publicly available information does not rule out that they are well-informed, as consumers may have access to other sources of information.[3]

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.[4] By reducing the consumers' incentive to try out new providers, switching costs may further undermine consumers' ability to learn about provider quality.[5]

We conducted two field experiments that manipulated consumers' access to information and reduced switching costs. Our first experimental intervention was directed to a representative sample of the general population in the Swedish region Skåne. The second intervention targeted new residents in the same region, who plausibly were less informed about providers and faced lower switching costs, as they had not built up a relationship with their default 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 and sent out by postal mail by the authority. The leaflet contained comparative information on, e.g., accessibility, quality, and available services of an individual's current primary care provider and its three geographically closest competitors. By sending information directly to consumers, the treatments reduced search costs and may also have improved understanding, as the information was presented differently on the leaflets compared to information available online. Furthermore, 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.

In the population-representative sample, switching rates were about 14% (0.8 percentage points) and 10% (0.6 percentage points) 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% (2.3 percentage points) 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 for both samples. For the smaller treatment group that did not receive a choice form, the effect on the switching rate is slightly smaller and statistically insignificant at conventional levels; however, the two treatment effects are not significantly different from each other. That the treatments affected mobility implies that there are demand side frictions in the primary care market.[6]

The main estimation samples include individuals living in areas where choice is highly restricted because there are few alternative providers nearby. Examining heterogeneity across several definitions of rural–urban markets, we find that effects are substantially larger in urban markets and statistically significant in both treatment arms and in both samples. The difference between the treatment arms in the urban areas of the population-representative sample is very close to zero. We find few strong indications of heterogeneity across variables measuring socioeconomic status, demography, and the propensity to use primary care, though the statistical power is limited in these estimations. An exception is that individuals with foreign background reacted to the treatment arm with a choice form, but not to the one without the form.

In further exploratory analyses, we show that the treatment without a choice form significantly affected individuals' choice in the direction of better rated centers, whereas the treatment including a choice form did not. These results indicate that the pre-paid choice form may unintentionally have induced less deliberate choices, either through a reduction of the small hassle costs related to switching or because it was interpreted as switching being desirable or even required. In further analyses, we also find indications of changed patterns of health care and drug consumption, although the signs of the effects are not consistent across treatments and samples. Overall, the results from these analyses show that the interventions changed the matching between consumers and providers, and thus that the impact of the interventions was not only to make individuals register at another provider. As we do not have access to measures of health or well-being, we cannot evaluate whether the matching improved.

To the best of our knowledge, our study is the first to manipulate switching costs in a consumer choice setting, and the first to examine how personalised information interventions affect the choice of health care provider and individuals' consumption of health care and drugs. It is also the first study in this strand of

[2] For recent evidence from health and prescription drug insurance markets suggesting 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, 2016), Ketcham et al. (2012, 2016), Kling et al. (2012), Handel and Kolstad (2015), Bhargava et al. (2017).

[3] Consistent with this, high-quality primary care providers face higher demand in England (Santos et al., 2017) and Norway (Iversen and Lurås, 2011; Biørn and Godager, 2010). Also in hospital markets, demand is typically higher for high quality providers (Chandra et al., 2016; Gutacker et al., 2016).

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

[5] 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 behavioural decision making biases are often reduced or disappear when decision makers are sufficiently experienced.

[6] Our experimental design in the population-representative sample stratified treatment by provider, which helps to isolate the demand-side response. The intervention for new residents was not stratified, but the sample is reasonably too small to have any substantial impact on providers.

research that is set outside the US. Previous interventions in health care markets have focused on choices of health or prescription drug plans, and have not examined the effects on care consumption. McCormack et al. (2001) and Farley et al. (2002a,b), analyse randomised information interventions including mailed out comparative information on health plan choices for new Medicare and Medicaid beneficiaries, and Ericson et al. (2017) analyse a similar intervention targeting consumers on the Affordable Care Act (ACA) Marketplace for health insurance. None of these studies find significant effects on switching.[7] In the context of employees' health insurance choices in Oregon, Abaluck and Gruber (2016) find that access to a decision support tool had no significant effect on plan choices, while restricting employees' choice sets had a minor impact on their forgone savings. Kling et al. (2012) distributed a letter with personalised cost information about Medicare Part D drug plans to a randomised sample of volunteering participants. The letter, which pointed out the cheapest alternative, substantially increased switching rates compared to a control intervention advertising a website covering the same information.

The previously studied interventions include features such as insurance coverage, premiums, and provider networks, aspects that are specific to insurance markets and irrelevant in other common settings such as choices of family physician or school. By studying the choice of a single provider in a setting where prices do not vary – a setting shared by many consumer choice markets – we are able to focus on how information of provider characteristics affects choices.[8] In terms of generality, it deserves to be mentioned that our intervention is the first in the literature aimed at a representative sample of the general population. Notably, the only previous information intervention in a health care context that has had a significant impact on switching targeted volunteering participants (Kling et al., 2012).

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

2. Primary health care in Sweden and Skåne

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 organisation of health care. The present study is set in Skåne, 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., behavioural 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. The others were private for-profit firms. The mean (and median) number of enrolled individuals per PCC was approximately 6,800.

The PCCs are mainly reimbursed by a fixed sum per enrolled individual, risk-adjusted for expected health care consumption and demographic and socioeconomic characteristics (Anell et al., 2018). As the money follows the individual, providers have an incentive to compete on quality to keep the current stock and increase enrolment. Certain aspects of process quality are further incentivised by a tournament-based pay-for-performance scheme, accounting for at most a few percent of revenues (Ellegård et al., 2018). At the same time, every additional service reduces the profit margin, thus generating an incentive to limit service provision. Although public providers are not strictly maximising profits, there is evidence that public as well as private primary care providers in Sweden respond to financial incentives (Dackehag and Ellegård, 2019; Ellegård, 2020).

Patients pay a visit fee which is regulated by the health care authority. In 2015, the basic fee was SEK 160 ($19/£13) up to an annual cap of SEK 1,100 ($133/£92). There was a 25% 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.[9] 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 public transport system, and by postal mail to the whole population. On a few occasions, the right to choose has also been highlighted in a magazine sent to all residents by postal mail.[10] Notably though, apart from our information interventions, the region has never distributed comparative information about specific PCCs directly to residents. This is not to say that active residents would not be able to access information through other channels. For instance, many providers describe their services on their own web pages. Also, a website operated by the Swedish health care authorities, 1177.se, provides information on contact details, opening hours and availability of special competencies. Up until 2019, the website also presented ratings from a patient survey, and included an interface allowing consumers to compare the patient ratings of up to four PCCs. Interestingly though, the traffic to this part of the website was so low that the authorities decided to remove the comparison interface in 2019.[11]

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 et al., 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 into a personalised section of 1177.se, where it is straightforward to search for and select

[7] Although Ericson et al. (2017) find that the provided nudges increased the proportion of consumers browsing around on the Marketplace website, accessing the voluminous information available on the website did not increase the switching rates. Similarly, Knutson et al. (1998) and Hibbard et al. (2002) find no significant effects of comparative information on health plan choice in two non-randomised studies of large firms.

[8] There are related information experiments in school markets on both the individual (e.g. Hastings and Weinstein, 2008) and the market level (Andrabi et al., 2017). Hastings and Weinstein (2008) use natural and randomised field experiments to study the effect of information about school-level proficiency (natural) and test scores (randomised) on school choice in North Carolina. Both interventions decreased the probability of selecting the default option compared to getting no information. Andrabi et al. (2017) supplied both the supply and demand side of Pakistani schooling markets with information. Report cards on school and child performance did not affect switching rates significantly, but increased enrolment and test scores, and decreased school fees.

[9] All Swedish regions introduced similar choice systems in 2007–2010. Residents can freely choose between all PCCs and may switch as often as they like. The PCCs are not allowed refuse new enrolments. For more information on the reforms, see Anell (2011, 2015).

[10] In Online Appendix E, we use the magazine to examine if reminders about the free choice *per se* affect the switching rate.

[11] Personal communication with representative from Inera, the company running 1177.se.

a care center. Another option is to fill out 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.

3. Experimental design and empirical strategy

3.1. Experimental interventions

The primary component of the intervention was an information leaflet, which was sent by the regional health care authority by postal mail to the treatment groups. Neither treatment nor control groups were aware that they were participating in an experiment – and in a sense they were not, as the leaflet was a real information campaign from the health authority.

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. The form may have reduced the monetary and hassle costs of switching: the individual only had to write the name of the chosen PCC and then 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 enrolled individuals. For legal and political reasons, the authorities also required that all PCCs were treated equally.

The information leaflet was in the format of an A4 sized paper folded in two. An example of a leaflet is available in Online 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 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 Online Appendix A and only summarised here. First, there was information about some general features (address, phone number, opening hours, number of enrolled individuals, public/private ownership). 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 special 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 behavioural 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 Online 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.

The leaflet reduced the search costs for individuals that were not already well-informed. It may also have improved understanding, as the information was presented differently on the leaflets compared to the online information. Because each PCC had its own leaflet, treated individuals in the same treatment arm were exposed to different information. Some individuals might have learned that there existed another provider strictly dominating their current PCC; such individuals received an impetus to switch. Others may have been dissatisfied with their current PCC before they received the leaflet, but learnt that there was no better option available, or that the closest acceptable PCC was located too far away; such individuals received an impetus to stay at their current provider. Thus, effects on both switching and staying are possible, and our estimations of the switching rate will capture the net effect.

3.2. Assignment to treatment

We used the random number generator in Stata (StataCorp, 2013) to randomly assign individuals from two populations to treatment and control groups. The first population consisted of a population-representative sample of 11% of the region's residents over 18 years of age, drawn randomly from each PCC's enrolment record on February 2, 2015. The second population included all individuals (above 18) who entered the enrolment 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).[12]

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 randomised subsample (7,700 individuals) also received the choice form. Due to the 1% 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 randomisation.[13] 137 individuals died or left the region before we extracted address information (for administrative reasons, address data was extracted after the randomisation 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 PRS therefore includes 112,575 individuals.

Of the 6,906 individuals constituting the population of new residents (NR), approximately 50% (3,454) were assigned to treat-

[12] 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 enrolment register in Feb-May 2015 were residents in the region on December 31, 2014). The main results are robust to excluding these individuals, see Online Appendix C.

[13] 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.

ment. 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-randomised by residential address.[14] The number of clusters were 6,059, indicating that most new residents resided in single-person households. The population was extracted from the enrolment register on May 11, the randomisation took place on May 25 2015, and the leaflets were mailed out in the second week of June. The NR estimation sample includes of 6,803 individuals, after excluding one individual who lacked complete information in the health authority's registers and 102 individuals who died or left the region between randomisation and intervention. Our main results in both interventions are robust to retaining individuals who died or left the region in the estimation sample (see Online Appendix C).

3.3. Estimation

We estimate the treatment effects in a regression framework based on the following equation:

$$y_i = \alpha + \beta TreatArm_i + \gamma X_i + \varepsilon_i \tag{1}$$

where *TreatArm={info,info&form}* and y_i is either a binary or a continuous outcome variable. For the PRS, *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 (NR). 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. ε_i is a residual term and α is an intercept. To examine heterogeneity of the treatment effects, we augment Eq. (1) with interaction terms between the treatment dummies and indicators of individual characteristics.

For binary outcomes, we use a linear probability model (LPM) to simplify the interpretation and inclusion of interaction terms. 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 Online Appendix C.)

Throughout, we perform separate estimations for the two interventions. In the estimations for the 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 enrolment record. 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 Online Appendix C). We use heteroskedasticity-robust standard errors, which in the case of new residents are clustered by residential address to account for the cluster-randomisation at that level. Randomisation inference (e.g., Athey and Imbens, 2017; Young, 2019) on the main specifications yields the same conclusions (see Online Appendix C).

To evaluate if treated individuals choose PCCs that are better rated, we follow e.g., Kling et al. (2007) and Finkelstein et al. (2012) by calculating average standardised treatment effects (ASTE) over the items on the leaflet for which a higher rating is unambiguously better (see Section 4.2). For each item, we estimate J seemingly unrelated regressions (similar to Eq. 1) of the change over the follow-up period on the treatment indicator(s). We then calculate the average (over items) standardised treatment effects for each treatment indicator k as:

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

where σ_j denotes the standard deviation of outcome variable y_j in the control group, and β_{kj} is the coefficient on the treatment indicator k from regression j. We estimate such average standardised treatment effects for three sets of leaflet items: all items, items related to quality, and items indicating the availability of special clinics.

The tested main hypotheses in Section 5.1.1 follow our pre-registered analysis plan.[15] The analysis of heterogeneity and whether individuals switched to better rated PCCs were sketched in the analysis plan, but the exact specifications were not pre-specified, as we did not have full information of the variables included in the data at the time of registration. These analyses and the follow-up analysis of care and drug consumption are therefore exploratory.

4. Data

4.1. Data sources

From registers held by the health care authority in Skåne, we have daily information on PCC enrolment and health care contacts at the individual level. For all individuals in the two experimental samples, we observe the enrolment spells from 2009 to early November 2015 and care contacts in 2009–2018. We also have data on dispensed doses of prescribed drugs in 2009–2018 from the national Pharmaceutical register, which is held by the National Board of Health and Welfare.

To this data, we have matched individual information about distance to providers (straight line distances between each individual's home address and all PCCs in Skåne at the start and the end of the follow-up period) plus 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. Notably, we lack 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).[16] To become enrolled at a PCC, one must have a residence permit. Therefore, a substantial share of new residents without background data are likely refugees who arrived from Asian and African countries in 2014 and obtained their residence permit in early 2015.[17] As Swedish is not regularly spoken in these countries of origin, the knowledge of Swedish was likely very limited in this group at the time of the intervention. Among new residents *with* background data, it is notable that the share of young (elderly) individuals is much higher (lower) compared to the PRS, reflecting the demographic profile of people who moved within the country (mainly students and newly graduated individuals).

[14] Due to the restrictions of sample size per PCC in the first experiment, we could not cluster-randomise the treatments to the PRS. In Online Appendix C, we show that household spill-over effects are unlikely to be a concern for our estimates for the PRS.

[15] The analysis plan is available at the American Economic Association's registry for randomised controlled trials (www.socialscienceregistry.org) with registration number AEARCTR-0000659 and title "Information and user choice in primary health care markets". We have also registered an updated analysis plan for the analyses of health care and drug consumption (see Section 5.3) after the interventions, but before we received follow-up care data (The updated plan has registration number AEARCTR-0003599). We are yet to gather all data required to follow the updated plan.

[16] 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.

[17] For statistics about residence permits, see migrationsverket.se/Om-Migrationsverket/Statistik/Beviljade-uppehallstillstand-oversikter.html.

4.2. Variables

Our main outcome variable is a binary indicator attaining the value one for individuals who switched providers at least once during our follow-up periods. The follow-up periods stretch from the day the leaflets were distributed (Feb 22 and June 8, respectively) to the day the enrolment data was extracted in early November 2015. The follow-up period for the enrolment status and the characteristics of chosen PCCs is 36 weeks for the population-representative sample and 21 weeks for the new residents sample.

In exploratory analyses, we consider other outcomes related to switching, namely indicators for having switched back to the original provider (at the end of follow-up) or for being enrolled at another provider on/not on the leaflet,[18] and the number of switches (Online Appendix D). We also use shorter follow-up periods to study the timing of switches. To examine whether the interventions affected not only the propensity to register at another PCC, but also the propensity to *contact* said PCC, we define an indicator variable equal to one if the individual had not only switched to a new provider during the enrolment follow-up period, but also contacted this PCC within three years after the intervention.

Switching is only one type of decision that might have been affected by the interventions. The interventions might have informed individuals about the availability of special clinics at their current provider, for instance, thereby affecting care consumption patterns. Also, given the personalised nature of the information leaflets, it should be recognized that not all individuals received a signal that there were better rated providers available. Others may have learned that their PCC was better than they thought, thus in fact getting an impetus to stay at their initial provider. As our main outcome only captures the net effect on switching, it is interesting to further study the interventions by looking at outcomes that indicate changes in the consumer-provider matching.

As mentioned in Section 3.3, we examine if treated individuals enrolled at better-rated PCCs in terms of the leaflet items for which a higher rating is unambiguously better (i.e., all items mentioned in Section 3.1 except contact details, public/private and number of enrolled individuals). Specifically, for each item k, we define an outcome variable as the difference between the value on k for the PCC where the individual was enrolled before the intervention and the PCC where the individual was enrolled at the end of the follow-up period.[19]

We also examine outcome variables measuring the quantity of primary care – the number of contacts with primary care providers, in total and by professional category – and primary care related consumption. The main text includes results for a follow-up period of one year after the interventions, and Online Appendix M includes estimates for a longer follow-up of three years.[20] The primary care contact variables covers all contacts (e.g., telephone contacts, physical visits) with physicians, nurses and/or other care professionals (occupational therapists, physiotherapists, dieticians, behavioral therapists, psychologists, psychotherapists, chiropractors, and naprapaths) at any PCC, not just the one the individual is enrolled at. The two measures of primary care related consumption are the number of dispensed defined daily doses (DDD) of prescribed drugs in outpatient care (including specialist care) and the number of emergency department (ED) visits.

Treatment effects on these measures of care consumption are indicative of *changes* in the matching of consumers and providers. Importantly, though, it is not possible to infer from such treatment effects whether the matching has *improved* or not. For instance, an increase in the number of PCC contacts or drugs dispensed may reflect improvements of access to care, but may equally well reflect that the PCC has failed to remedy ailments as fast as it could have. One may think that a decrease in the number of ED visits would be an unambiguous indicator of quality, as the ED is a substitute for primary care and high quality primary care may prevent the need to visit the ED. In practice though, patients are often referred to the ED from primary care. Therefore, decreases in ED visits may be driven by decreased access to primary care, and thus reflect a worsened matching of patients and PCCs.

Table 1 shows definitions of the covariates we use. In the estimations, we include indicators for missing observations.

4.3. Balance check

To check whether the randomisations created balanced treatment and control groups, we predict the probability of having switched PCC at least once during the pre-intervention period and test whether this probability differs between the treatment and control groups.[21] For the PRS, we estimate a logit model using the full pre-intervention period and all covariates in Table 1 as predictors. As we lack information on the pre-intervention enrolment history of the NR, we instead use predicted probabilities from a model estimated on earlier cohorts of new residents identified using the PRS. For the NRs without background data, we set the probability to the mean of the predicted probability of all NRs.

The results of the regressions with the predicted switching probability as the outcome variable and the treatment indicators as explanatory variables indicate that the treatment and control groups are balanced. The coefficient estimates are small in relation to the baseline switching rate and not significant (0.0063 in the *info* arm and 0.0017 in the *info*&*form* arm compared to a mean of 0.38 in the PRS, $p > 0.1$; 0.0005 compared to 0.21 in NR, $p > 0.1$). Note that the longer pre-intervention period makes the baseline switching rate much larger than in the post-intervention period.

Tables B.1 and B.2 in Online Appendix B show descriptive statistics for the PRS and the NR, respectively. They support the notion that the randomisations created balanced samples. In regressions of the treatment indicator on all covariates, we cannot reject the null that their coefficients are jointly equal to zero. There are a few significant differences for individual covariates, but not more than what could be expected by chance.

5. Results

Section 5.1 reports our estimates of the treatment effects on switching, robustness analyses, and heterogeneity over the type of market (urban/rural) and individual characteristics. Section 5.2 explores the treatment effects on the characteristics of the chosen care centers, and Section 5.3 the effects on health care and drug consumption.

[18] For the control group these are the PCCs that would have been on their leaflet, had they been assigned to a treatment group.

[19] Five PCCs lack data on one or more information items. For individuals that are enrolled at such PCCs, we code the outcome variable for item k as zero. In Online Appendix L, we show that results are very similar if we instead impute the outcome variable using the median value of all other PCCs in case of missing information on an item.

[20] We focus on the one-year estimates in the main text as they are less affected by attenuation bias. Such bias arises over time as some individuals die or move to other markets, and some PCCs close. These kinds of events render the information interventions irrelevant, but treated individuals are still classified as treated. This misclassification risks creating attenuation bias, which would worsen over time. We therefore believe that the one-year estimates are more informative of whether the interventions affected the matching between consumers and PCCs.

[21] We thank an anonymous referee for suggesting this test.

Table 1
Definitions of covariates.

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

5.1. Are there effects on switching?

5.1.1. Main results

The estimates on switching for the PRS are shown in the first two columns in Table 2. Compared to the 5.7% switching rate of the control group during the 36 week follow-up period, the probability of switching was 0.61 and 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% and 14.5%. 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%).[22] Although the treatment effect is only statistically significant for the treatment arm that received a choice form (*info&form*, p = 0.002 with covariates), the estimates of the two treatment arms are similar and not significantly different from one another. Given the smaller sample size of the *info* arm, we suspect that the insignificance of this variable is due to low statistical power.

Column (3) and (4) show the estimates for the intervention directed to the NR, in which all treated individuals received both the leaflet and the choice form (*info&form*). The treatment effect of 2.0 (2.3 with covariates, p = 0.002) percentage points implies a 22.6 (26.0)% increase compared to the switching rate of 9.0% in the control group. Thus, the results of the NR intervention replicates those of the PRS intervention in qualitative terms and the treatment effect is larger in both absolute and relative terms, despite a shorter follow-up period (21 weeks) and a higher baseline switching rate. This finding aligns well with the idea that the new residents initially had less knowledge about available PCCs and their characteristics, as well as lower switching costs.

5.1.2. Robustness and further switching patterns

Online Appendix C shows that our main results are robust to a range of sensitivity checks, such as using a logit or a difference-in-differences specification, or 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 randomisation-based inference.

The main results focus on the probability of making at least one change of PCC. In Online Appendix D, we explore the effects on variables that may indicate to which extent these administrative changes were substantial and lasting. We find positive estimates when we replace the main outcome variable with an indicator for having switched to another PCC *and* contacted said PCC at least once during a three-year period. The treatment increased the probability of switching and contacting the provider by 0.65 and 0.32 percentage points in PRS and 1.1 percentage point in NR.[23] As expected by the definition of the outcome variable, these estimates, as well as the control group means, are smaller than their counterparts in the main results on switching in Table 2.[24] Despite being 0.2–0.3 percentage points smaller in PRS and around 1.2 percentage point smaller in NR, the treatment effects are still of considerable size. The estimate for the *info&form* arm is also still statistically significant in the PRS, but not in NR. Notably, when we focus on the urban markets where the effects on switching are considerably stronger, all estimates are larger and significant while the difference to the effect on switching is of similar size (see the urban/rural heterogeneity in Section 5.1.3).[25] Thus, these findings strongly suggest the effect on switching was not just a bureacratic change as the treatment affected the choice of which care provider to actually consult, in particular for PRS. In Online Appendix D, we further find that the treatment had lasting effects on provider choices: the estimated effects on the probability of being enrolled at another center than the initial one at the end of the follow-up period are only slightly smaller than the estimates in Table 2 for all three treatment groups. This is not to say that no individuals regretted their choice; in particular, the probability of switching back to the initial provider is positive and statistically significant in the *info&form* arm of the PRS.

In relation to the importance of the information *per se*, we note that treated individuals in both arms and samples were significantly more likely to switch to another PCC on the leaflet compared to the control group, whereas no difference is significant regarding switches to other PCCs.[26] In other words, the treatments primarily induced switching to PCCs for which the treatments reduced search costs. Still, we cannot exclude that other mechanisms than the information explain the result; for example, the leaflets may have reminded individuals who had planned to switch providers but forgot or procrastinated (Ericson, 2011, 2017). To examine if reminders without comparative information have similar effects on switching as our interventions, we study switching rates around the publica-

[22] 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.

[23] The estimates equal 0.0065 (p = 0.017) in the *info&form* arm and 0.0032 (p = 0.474) in the *info* arm for PRS, and 0.0113 (p = 0.102) for NR.

[24] The control group mean of this outcome equals 0.052 for PRS and 0.079 for NR compared to the baseline switching rates of 0.057 and 0.090.

[25] In the PRS the estimates for the *info&form* and *info* arm equal 0.0112 (p = 0.075) and 0.0104 (p = 0.005). The estimate in NR equals 0.0143 (p = 0.080).

[26] The significant effect in the *info* arm is of similar size as the main effect, giving support to the suggestion that the lack of significance in Table 2 may be due to the lower power.

Table 2
Main results.

	Treatment effect on switching rate after 36/21 weeks			
	PRS		NR	
	(1)	(2)	(3)	(4)
info	0.00605	0.00577		
	(0.00476)	(0.00473)		
info&form	0.00825***	0.00816***	0.0203***	0.0233***
	(0.00275)	(0.00265)	(0.00783)	(0.00768)
Constant	0.0569***	0.0529***	0.0898***	0.169***
	(0.000221)	(0.00346)	(0.00526)	(0.0230)
N	112,575	112,575	6,803	6,803
R^2	0.000	0.024	0.001	0.031
p info = info&form	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. Columns (1) and (2) cover the intervention directed to the population-representative sample (PRS) and columns (3) and (4) cover 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 mentioned in Table 1 (For exact covariate sets see Table B.1 and B.2). *p info = info&form* shows the *p*-value of test of difference between the estimates of the two treatment arms. For PRS, the estimates are weighted by the inverse of the probability of being sampled; such weights are irrelevant for NR, for whom everyone had an equal probability of being treated. Robust standard errors in parentheses (clustered by residential address in NR sample). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

tion dates of magazines sent out by the regional health authority, which only highlighted the right to choose PCC. We find no signs of elevated switching rates around the time of these previous reminders (see Online Appendix E). Although this result indicates that reminders without comparative information do not increase switching, it does not rule out that the leaflets affected switching through reduced forgetfulness and procrastination.

Fig. 1 indicates that the increased switching rate relative to controls appeared within a month in both the PRS and NR, and that the treatment effects grew over time. The PRS graph (left panel) further illustrates that the *info&form*-treatment arm responded faster to treatment than individuals that did not receive a choice form.[27] The faster response in the *info&form* arm may indicate that these individuals made less deliberate choices. Some evidence in this direction is that the probability of switching back to the intial PCC was significant only in the *info&form* arm of the PRS, as mentioned above. The probability of switching to a PCC on the leaflet was further lower than in the *info* arm, potentially suggesting that individuals in the *info&form* arm used the information to a lower degree.

5.1.3. Heterogeneity: who switched?

This section examines heterogeneity in the treatment effect on switching. We first consider heterogeneity with respect to the market (urban/rural) of the PCC where the individual was initially enrolled. We then turn to heterogeneity with respect to demography, socioeconomic status, and individual characteristics that may relate to the individuals' background knowledge and demand for primary care.

Ex ante, we expected the interventions to have a smaller effect on individuals residing in rural areas. For rural individuals, the alternative PCCs on the leaflets may be located too far away to be attractive options and the search costs associated with obtaining information about relevant providers ought to be lower (because there was a smaller number of relevant PCCs).

Table 3 displays heterogeneity with respect to the rurality of the PCC the individual was initially enrolled at. Panel A confirms that the treatment effects are significantly different in urban and rural areas in the PRS, for four definitions of rurality. In column (1), rural areas are defined as towns with at most 15,000 residents, which corresponds to towns with at most two PCCs.[28] We find very similar treatment effects in urban areas for the *info* and the *info&form* arm (in the PRS). The magnitudes are about twice as large as the main effects and both estimates are statistically significant ($p = 0.034$ or lower). Further, the effects in urban areas are significantly larger than the effects in rural areas, where the effects are not statistically distinguishable from zero. Although the point estimate for the *info* arm in rural areas indicates a more negative effect (-0.7 percentage points) than the close to zero effect in the *info&form* arm in rural areas, the estimates are not significantly different from each other ($p = 0.313$). We find similar results when letting rural PCCs be defined by having at most one competitor within a radius of 1/3/5 km (columns 2–4).[29] Furthermore, the estimates are remarkably similar in all areas when we replace the main outcome variable with an indicator for having switched to another PCC and contacted said PCC at least once during a three-year period (see Online Appendix D).

The results for the new residents follow a comparable pattern (Panel B). The point estimate in urban areas is about 50% larger than the treatment effect in the main specification, and it is considerably larger than the effect in rural areas (although the interaction term is insignificant). As in PRS, the effect in rural areas is only statistically significant at the 10% level when using a quite restrictive definition of rurality (having at most one competitor within a radius of 1 km, column 2). The treatment effect on switching to and contacting another PCC is about half of the corresponding effect in urban areas in column 1 of Table 3 (see Online Appendix D). That is, a smaller proportion of switches was followed by contacts with the new PCC in urban areas of the NR than in the PRS. In rural areas, the effect is very similar.

[27] While our detailed data on enrolment periods allow us to track when individuals' switches were registered, there is a lag between returning the attached choice form and registration due to administration. We are therefore not able to detect instant responses. Online Appendix F depicts the number of registered switches per week and shows that the attached choice forms were returned and registered by the postal service very soon after the intervention.

[28] The only exception is Tomelilla, a town with 8,000 residents but three PCCs, one of which has only 600 individuals enrolled and is the smallest one in the region.

[29] Note that the difference between the treatment arms are only rejected at conventional levels in one specification, and for one subgroup: rural individuals according to the 1 km definition of rurality (column 2, $p = 0.095$). Further note that in Online Appendix G we show results from estimations with less coarse urban–rural categories and interactions with indicators of the distance to the nearest alternative PCC. In both the PRS and NR, these results indicate that the treatment effects are typically largest where there are several – more than two – available PCCs and for the individuals with shorter distances to alternative PCCs.

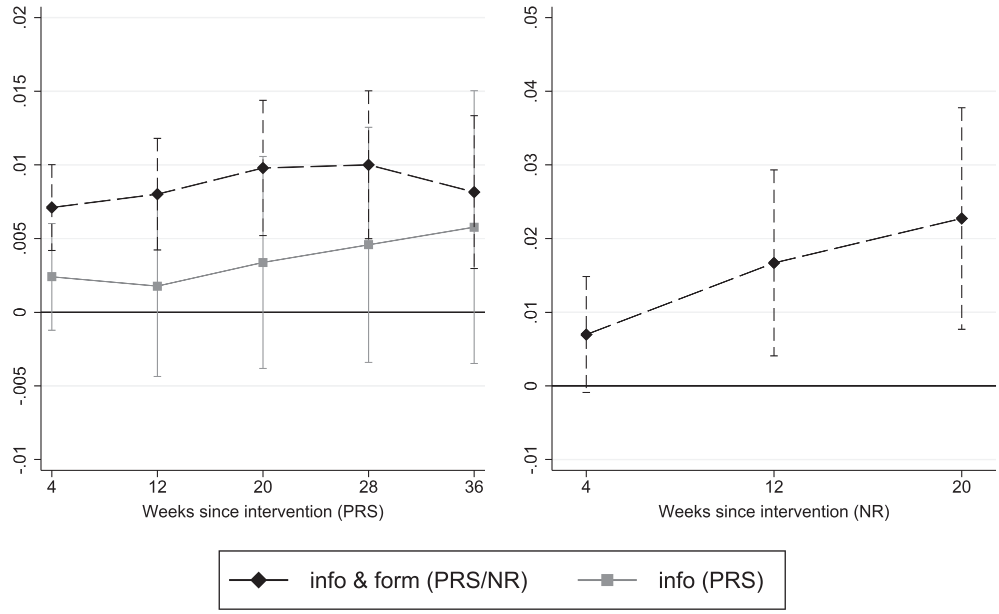

Fig. 1. Treatment effects on switching rate over time. *Note:* The figure shows the cumulative treatment effect with 95 percent confidence interval estimated 4–36 weeks after the interventions; population-representative sample (PRS) in the left panel and new residents (NR) in the right panel. The estimates come from a model with the same covariates as in Table 2 estimated repeatedly using follow-up periods of 4 weeks, 12 weeks, etc.

Table 3
Heterogeneity: Rural vs Urban.

Panel A: Population-representative sample (PRS) *Treatment effect on switching rate after 36 weeks*				
	(1)	(2)	(3)	(4)
info	0.0138**	0.0289**	0.0153**	0.0127**
	(0.00646)	(0.0120)	(0.00664)	(0.00622)
info×rural	−0.0211**	−0.0307**	−0.0231***	−0.0198**
	(0.00876)	(0.0129)	(0.00878)	(0.00888)
info&form	0.0128***	0.0108**	0.0135***	0.0132***
	(0.00334)	(0.00466)	(0.00323)	(0.00320)
info&form×rural	−0.0122**	−0.00356	−0.0129**	−0.0143***
	(0.00534)	(0.00565)	(0.00541)	(0.00546)
N	112,575	112,575	112,575	112,575
R^2	0.024	0.024	0.024	0.024
info + info×rural	−0.0073	−0.0018	−0.0078	−0.0071
p	0.217	0.693	0.175	0.264
info&form + info&form×rural	0.0006	0.0073	0.0006	−0.0011
p	0.887	0.023	0.896	0.795
p info = info&form (rural)	0.313	0.095	0.270	0.489
p info = info&form (urban)	0.878	0.167	0.792	0.952
town/radius	town	1 km	3 km	5 km

Panel B: New Residents (NR) *Treatment effect on switching rate after 21 weeks*				
	(1)	(2)	(3)	(4)
info&form	0.0307***	0.0378***	0.0284***	0.0280***
	(0.00904)	(0.0141)	(0.00924)	(0.00895)
info&form×rural	−0.0259	−0.0206	−0.0154	−0.0175
	(0.0169)	(0.0168)	(0.0166)	(0.0171)
N	6,803	6,803	6,803	6,803
R^2	0.031	0.031	0.031	0.031
info&form +info&form×rural	0.0049	0.0172	0.0130	0.0105
p	0.733	0.061	0.346	0.477
town/radius	town	1 km	3 km	5 km

Note: The table shows estimates of urban–rural heterogeneity on the treatment effects on switching. Rurality is defined based on the location of the PCC where the individual was enrolled at the time of randomisation. In column (1), *rural* equals 1 if the initial PCC was located in a rural town (up to ∼15,000 residents); this captures PCCs with at most one competitor within the same town. In columns (2)–(4), *rural* equals 1 if the PCC had at most one competitor within 1,3, or 5 km. All models includes the same covariates as in Table 2 (the *rural* indicator is itself subsumed by the PCC fixed effects in the PRS estimations). Covariates are omitted from the table for brevity. *info(&form) +info(&form)× rural* provides the treatment effect (and the corresponding p-value) for individuals initially enrolled at rural PCCs. *p info = info&form* shows the p-value of test of the difference between the estimates of the two treatment arms in the urban/rural subsample. Robust standard errors in parentheses (clustered by residential address in NR sample). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

We next summarise our analyses of heterogeneity over demographic and socioeconomic background (full results available in Online Appendix H). The most striking finding is that individuals with foreign background reacted significantly less to the treatment without a choice form compared to individuals with a Swedish background, but at least as much to the treatment with a form (in both PRS and NR). Related, for the third of the NR lacking background data – who recently immigrated – the estimated treatment effect is about one third of that among individuals with background data, although the difference is not statistically significant.

We find no clear gradients for other individual background characteristics. In the PRS, we find a tendency of an inverse u-shaped relationship between the size of the treatment effect and age, and a u-shaped relationship with income.

The information leaflets might have received more attention by individuals who were likely to use primary care in the near future. In Online Appendix I, we predict each individual's probability to contact a primary care physician within the year after the intervention, and interact the treatment indicators with dummies for the three highest quartiles of the distribution of predicted probabilities. [30] We find no strong support for heterogeneous effects in this regard: All interaction terms are statistically insignificant, there are both negative and positive interactions, and the estimates are not monotonically decreasing or increasing in the contact probability for any of the treatments.

Another potentially important dimension of heterogeneity in the treatment effect on switching concerns individuals who had recently moved within the region, and who therefore might have had a higher-than-usual propensity to switch providers (although not necessarily to one of the PCCs on the leaflet). Online Appendix J shows that recent movers in the PRS did not react significantly different from other individuals. (We do not know the history of pre-intervention residential moves for the NR).

We find suggestive evidence that the *info&form* treatment in particular affected the switching propensity of inexperienced individuals when we instead attempt to characterise the switching populations in the two treatment arms in the PRS (see Online Appendix K). We compare the characteristics of the switchers in the two arms by regressing a large number of variables on an indicator for being in the *info&form* arm including only individuals who switched provider at least once in the follow-up period. In general, there are small differences between the arms and only few significant differences. However, among the variables for which the differences are statistically significant, several indicate that switchers in the *info&form* arm had less experience with the regional primary care. They had fewer previous care contacts, had switched less often in the past, and a larger proportion had recently moved to the region (either from other regions or from abroad).

To summarise, the elevated switching rates in the treatment groups demonstrate that there are demand side frictions in the primary care market. It was primarily individuals in urban areas who reacted to the interventions by switching more. The effects of the treatments with and without a choice form were very similar in urban areas, though individuals with foreign background appear to have reacted only to the treatment with a form. Switchers in the *info&form* arm tend to have less primary care experience. Apart from these findings, we find no striking heterogeneity. However, it should be said that our chances to detect heterogeneity are limited, as the treatment induced relatively few switches in some subgroups. For the NR, the lack of variation in age and the large share of individuals lacking background data further limits statistical power.

5.2. Characteristics of chosen care centers

An individual receiving a leaflet might learn that the initial provider was dominated by other nearby providers, or that it was in fact the best available choice. Under all circumstances, we would expect treated individuals to be registered at PCCs with higher ratings at the end of the follow-up period, compared to individuals in the control group. To examine the treatment effects on the characteristics of chosen PCCs, we estimate ASTEs (see Section 3.3). Table 4 shows the results for the PRS in Panel A and for the NR in Panel B and C. Column (1) averages the standardised treatment effect over all items on the leaflet, whereas columns (2) and (3) restrict the averages to items relating to quality measures and availability of special clinics.

For the PRS, the overall ASTE is positive and significant for the *info* arm (0.014 control group standard deviations, σ; $p = 0.008$). The corresponding ASTE is positive but smaller and not significant for the *info&form* arm (0.005σ). The quality items appear to be more important than the special clinic items in both arms. When pursuing the same analysis separately for rural and urban areas (see Online Appendix L), the results are similar in qualitative terms, although the effects are generally larger in urban areas and the significant effects in the *info* arm are only present in urban areas.

The overall ASTE for NR in Panel B is negative (-0.011σ, $p = 0.087$). Both quality and special clinics items contribute to the negative effect. The negative ASTEs are larger in urban areas (see Online Appendix L) and relates to the composition of the NR sample. Specifically, the negative overall estimate is driven by the subgroup of recent immigrants. As previously noted, a large share of these immigrants came from linguistically distant countries, suggesting that they might not have understood the information.

Panel C presents ASTE estimates for a NR sample excluding the recent immigrants. For new residents with Swedish background, the overall ASTE is small and insignificant. However, the overall ASTE turns out to be the net effect of two substantial, though statistically insignificant, effects of opposing signs for quality and special clinics. As most new residents with Swedish background are rather young, they may not yet have developed the type of conditions dealt with at the special clinics.

To summarise, we find that treated individuals in the *info* arm chose significantly better rated PCCs, whereas we find no strong evidence that individuals in the *info&form* arms chose better rated PCCs, neither in the PRS nor in the NR. Recent immigrants in the NR even chose significantly worse rated PCCs. The magnitudes of the effects may seem small, but recall that the definition of the outcome variables implies that they equal zero for the large majority of individuals who did not switch PCC. In Online Appendix L, we show that the coefficients are about an order of magnitude larger when the sample is restricted to those who switched during the follow-up period. Although restricting the sample to switchers yields biased estimates of the treatment effects (as the treatment affected the probability to switch), it is informative with regards to the difference between the treatment arms in the change in ratings per switch. This difference between the *info* and *info&form* arms is statistically significant ($p = 0.022$, see Online Appendix L). So while we do not want to overstate the insignificant differences between the ASTEs of the two arms in Table 4 (lowest $p = 0.115$), there is support for a difference in the change in rating per switch. A plausible explanation of the difference is the tendencies of less

[30] The predictions are based on a logit model using the control group only. In Online Appendix I, we show that the predictive model performs well and the stratification is likely to capture differences in the propensity to seek care. Actual and predicted consumption are very similar and both clearly increase by quartile.

Table 4
Standardised treatment effects.

Panel A: Population-representative sample (PRS)			
	All (1)	Quality (2)	Clinics (3)
info	0.0142***	0.0216*	0.00970
	(0.00536)	(0.0111)	(0.00744)
info&form	0.00460	0.00907	0.00193
	(0.00302)	(0.00697)	(0.00442)
N	112,575	112,575	112,575
Panel B: New Residents (NR)			
	All (1)	Quality (2)	Clinics (3)
info&form	−0.0105*	−0.0159	−0.00719
	(0.00610)	(0.0150)	(0.00966)
N	6,803	6,803	6,803
Panel C: New Residents excl. recent immigrants (NR)			
	All (1)	Quality (2)	Clinics (3)
info&form	−0.00256	0.0182	−0.0150
	(0.00744)	(0.0188)	(0.0115)
N	4,477	4,477	4,477

Note: Standardised treatment effects averaged over three sets of items on the 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 standardised treatment effects are calculated using estimates from 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 enrolled at providers with missing information. All models include the same covariates as in Table 2. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

deliberate choices in the *info&form* arm documented in Section 5.1.2.[31]

As a final note, it should be mentioned that the averaging of standardised treatment effects may hide variation in how individuals value different provider characteristics. In our case, the underlying regression estimates suggest that the averages are mainly composed of many small effects in the same direction, rather than a mix of large and opposing effects (see Online Appendix L).[32]

5.3. Health care and drug consumption

We next examine whether the interventions affected the quantity of health care and drug consumption. We focus on the number of primary care contacts during the first post-intervention year, and comment briefly on additional results regarding other outcome variables and a longer follow-up period (full results are available in the Online Appendix M).

Panel A of Table 5 presents estimates of the treatment effects on the number of primary care contacts. Effects on the total number of contacts appear in column (1) for the PRS and in column (5) for the NR. Effects broken down by professional category – physician, nurse, and others – appear in columns (2)–(4) (PRS) and (6)–(8) (NR). In the PRS, the estimated treatment effect on the total number of primary care contacts is positive and insignificant in the *info* arm, but negative and significant in the *info&form* arm. Within treatment arms, the signs of the estimates do not vary across professional categories. In both arms, the effects on physician contacts are the largest. For the NR, the treatment effect on the total number of contacts is negative but insignificant. The magnitude of this effect is driven by a large and significant negative effect on contacts with *other* care professionals than physicians and nurses.

Recalling the previously detected heterogeneity in switching effects across urban and rural areas, we also examine whether the effects on care consumption also display urban–rural heterogeneity. These results are available in Panel B of Table 5. In the *info* arm, we note a significant increase in the number of physician contacts in urban areas, and a close to zero effect on all measures for individuals in rural areas. The results further reveal that the negative effect on the total number of contacts in the *info&form* arm of the PRS in Panel A is driven by the treated in rural areas. For this subgroup, the treatment effect on primary care consumption is negative for all professional categories, but the overall effect is primarily driven by decreases in contacts with staff that are not physicians. In urban areas, the *info&form* arm displays a close to zero effect on the total number of contacts, which turns out to be the sum of a marginally significant reduction of physician contacts and an increase in contacts with other professions. For NR, the results show that the negative effect on contacts with other professionals in Panel A is driven by the treated in urban areas (perhaps in line with this group being less likely to be enrolled at PCCs with special clinics, where such professionals would be more common). Overall, the results indicate that all treatments changed the patterns of primary care consumption, at least for some subgroups.

In Online Appendix M, we show estimates of the effects on the number of prescribed drugs and ED visits. We find several significant overall and subgroup effects on both measures, which indicate changed health care and drug consumption patterns. We find the following significant overall estimates: the estimates on the number of drug doses (DDD) are negative for the *info&form* arm in the PRS (p = 0.023) and in NR (p = 0.023). The ED visits estimates are negative for the *info&form* arm in the PRS (p = 0.068) and in NR (p = 0.024). The total marginal effects are significant for the following outcomes and subgroups: The DDD estimates are negative for the *info&form* arm in urban areas in the PRS (p = 0.093) and positive in rural areas in the NR (p = 0.004). The ED estimates are negative for the *info&form* arm in rural areas in the PRS (p = 0.020), and positive in urban areas in the NR (p = 0.020). In all instances where the estimate for physician contacts in Table 5 is not close to zero, the estimated treatment effects on the number of prescribed drugs have the same sign as the physician contact estimates (both overall and in the urban–rural subgroups). As a physician consultation is a prerequisite for getting a prescription, this relationship is intuitive. As discussed in Section 4.2, ED visits may be induced by, substitutes to, and prevented by primary care contacts. Thus, the relationship between primary care and ED visits is *ex ante* more

[31] We find less support for two other potential explanations. First, the faster switches in the *info&form* arm gave these individuals more time to actually visit the new PCC, reconsider their choice, and switch back before the end of the follow-up period. By construction, this group would then include a larger share of individuals with a zero difference between the start and end of the follow-up period. However, when we estimate the ASTE using the provider after the first switch, the ASTE is only slightly larger (0.0057σ, p = 0.062). This estimate is around 40% of the ASTE in the *info* arm at the first switch (0.014σ, p = 0.007). Thus, while there may be some merit to this hypothesis, it explains relatively little of the difference. Second, the reduced switching costs may have changed the marginal switcher: a lower cost implies that a smaller quality difference between the current and potential new provider is required to induce a switch. We would then expect higher switching rates and lower quality differences in the *info&form* arm. While the lack of statistical significance on the treatment effect on switching in the *info* arm is consistent with this interpretation, the point estimates for the two arms were very similar and not statistically significant from each other. Further, in urban areas, the *info* and *info&form* arms had almost identical effects on the switching rates, while we still observe a relatively large difference in the ASTE-estimates between the arms (see Online Appendix L).

[32] In Online Appendix L, we report results from an examination of heterogeneity of switching across patient satisfaction with their initial PCC. However, that several indicators seem to contribute to the ASTE-results highlights the difficulties of isolating heterogeneity across any one PCC characteristic. As individual rankings of PCCs depend on unobserved preferences over characteristics, such analyses become difficult to interpret.

Table 5
Treatment effects on primary care contacts by professional category

Panel A: No interaction

	PRS				NR			
	(1) All	(2) Physician	(3) Nurse	(4) Other	(5) All	(6) Physician	(7) Nurse	(8) Other
info	0.116	0.0880	0.0253	0.00215				
	(0.157)	(0.0567)	(0.0810)	(0.0881)				
info&form	−0.189*	−0.0969***	−0.0730	−0.0189	−0.204	−0.00577	−0.00736	−0.190***
	(0.0974)	(0.0311)	(0.0531)	(0.0530)	(0.164)	(0.0601)	(0.0761)	(0.0731)
N	112,573	112,573	112,573	112,573	6,803	6,803	6,803	6,803
Control mean(y)	6.207	2.114	2.845	1.249	3.754	1.362	1.606	0.786
p info = info&form	0.092	0.004	0.299	0.834				
Follow-up length	1 year	1 year	1 year	1 year	1 year	1 year	1 year	1 year

Panel B: Interaction with rural

	PRS				NR			
	(1) All	(2) Physician	(3) Nurse	(4) Other	(5) All	(6) Physician	(7) Nurse	(8) Other
info	0.212	0.165**	0.0905	−0.0433				
	(0.199)	(0.0749)	(0.102)	(0.106)				
info×rural	−0.254	−0.202*	−0.171	0.119				
	(0.323)	(0.114)	(0.167)	(0.187)				
info&form	0.0179	−0.0646*	0.0272	0.0552	−0.269	−0.0313	−0.00211	−0.235***
	(0.123)	(0.0386)	(0.0657)	(0.0668)	(0.193)	(0.0690)	(0.0872)	(0.0899)
info&form×rural	−0.542***	−0.0849	−0.263**	−0.195*	0.227	0.0892	−0.0183	0.156
	(0.201)	(0.0649)	(0.111)	(0.110)	(0.360)	(0.138)	(0.176)	(0.150)
N	112,573	112,573	112,573	112,573	6,803	6,803	6,803	6,803
Control mean(y)	6.207	2.114	2.845	1.249	3.754	1.362	1.606	0.786
p info = info&form	0.396	0.006	0.595	0.422				
p info = info&form, rural	0.100	0.253	0.319	0.217				
p info × rural	0.871	0.665	0.542	0.623				
p info&form × rural	0.001	0.004	0.008	0.109	0.892	0.630	0.893	0.515
Follow-up length	1 year	1 year	1 year	1 year	1 year	1 year	1 year	1 year

Note: The dependent variable counts the number of consultations (in office or via telephone) with physicians (col. 1), nurses (col. 2) and some other professional categories (col. 3; occupational therapists, physiotherapists, dieticians, behavioral therapists, psychologists, psychotherapists, chiropractors, and naprapaths). *p info × rural* = *p*-value of test of treatment effect of *info* treatment for individuals residing in rural areas (etc.). All models include the same covariates as in Table 2. Robust standard error in parentheses (clustered by residential address in NR sample). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

ambiguous. Accordingly, the signs of the estimates differ across treatments and subgroups.

The ASTE results in Section 5.2 for the NR sample indicated that treated individuals within the subgroup of recent immigrants enrolled with worse rated PCCs. It is therefore reassuring that there is no evidence of reduced access to care for this subgroup. All estimates for the care consumption measures are positive, or very close to zero, and insignificant (see Online Appendix M).[33]

In sum, although few consistent patterns emerge across treatments and samples, we find multiple indications that the interventions affected health care and drug consumption during the first post-intervention year. For the outcomes with non-negligible one-year effects, estimates using a three-year follow-up period tend to be of the same sign and smaller in relation to the control group mean (see Online Appendix M).

6. Concluding remarks

Policies to expand consumer choice in health care markets have often failed to deliver substantial quality improvements. Consumers' low mobility and low propensity to search for comparative information suggest that demand side frictions may be a plausible explanation.

We provide the first experimental evidence that there are demand side frictions in the primary health care market and that mobility can be increased by sending out comparative information about providers via postal mail and by eliminating certain small switching costs. We find 10–14% increases in the propensity of switching in the general population, and higher relative increases – 26% – for new residents. The effects on switching are substantially larger and statistically significant in urban markets for all treatments, indicating that similar interventions might have stronger impacts on mobility in areas more dense than our study region. The larger effect in the intervention targeting new residents, who presumably faced higher search costs and lower switching costs than the general population, is further evidence that frictions matter. That we are able to replicate the main result in two interventions directed to different populations suggest that the phenomenon is not specific to the (pre-existing) population of our study region.

Increasing mobility is not an end goal but a potential means to improve the matching between consumers and providers. Exploratory analyses indicate that the interventions had an impact on the characteristics of chosen providers and the health care and drug consumption, i.e., the matching between consumers and providers *changed*. Evaluating if the matching *improved* is admittedly challenging. Our definition of better choices may differ from individual valuations and the advantages of making a "better" choice may not be captured by our outcome measures. Furthermore, we cannot disentangle the effects of information and reduced switching costs from reminder effects, but our results indicate that the information and the choice form mattered. Thus, it seems unlikely that just sending out a reminder would have the same effects. New experi-

[33] The estimated effect on other care professional contacts is negative, but is zero to the third decimal.

ments are needed to uncover more of the mechanisms through which information and switching costs affect the matching, and to determine what information consumers need to make better choices of providers.

Our results nonetheless give several suggestions of how health care authorities can help consumers make more deliberate choices of providers. First, we find elevated switching rates despite that most of the leaflet information was easily accessible online. Thus, although health care authorities, for cost and convenience reasons, may find it attractive to provide information online, this passive information channel is unlikely to eliminate demand side frictions. Indeed, as a response to the low number of visitors, the Swedish health care authorities decided to remove the comparative patient ratings from their website in 2019.

Second, while only receiving comparative information led to enrolment at significantly better rated providers, the combination of information and a pre-paid choice form did not. There are indications that the choice form made individuals switch sooner, make less use of the sent out information, and more likely to switch back to their original provider. The pre-paid choice form may unintentionally have induced less deliberate choices by reducing the small hassle costs related to switching or by signalling that switching was desirable or even required.

Third, our heterogeneity analysis revealed that interventions had similar impacts on the switching rates of individuals with different demographic, socioeconomic, and health characteristics. One exception was that individuals with foreign background – in particular, recent immigrants – reacted differently to the treatments. To affect present inequalities in search and enrolment patterns, similar interventions should be targeted to specific groups; in particular, our results suggest that providing information in commonly spoken foreign languages is important.

As a final remark, we want to underline that our experiments were designed to isolate the demand side effects, and therefore did not provide an opportunity to analyse supply-side responses. Because the payments to providers follow the consumer, mobility-increasing interventions ought to spur competition over the provider characteristics demanded by consumers. As the effects of such competition depend on consumers' preferences, making sure that consumers have the information and support they need to make a deliberate choice of provider would be all the more important in market level campaigns. After all, the ultimate value of allowing consumers to freely choose health care provider lies in the potential to improve their health and well-being.

Appendix A. Supplementary material

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.jpubeco.2021.104390.

References

Abaluck, Jason, Gruber, Jonathan, 2011. Choice Inconsistencies among the Elderly: Evidence from Plan Choice in the Medicare Part D Program. Am. Econ. Rev. 101 (4), 1180–1210.

Abaluck, Jason, Gruber, Jonathan, 2016. Choice Inconsistencies among the Elderly: Evidence from Plan Choice in the Medicare Part D Program: Reply. Am. Econ. Rev. 106 (12), 3962–3987.

Abaluck, Jason, Gruber, Jonathan, 2016b. Improving the Quality of Choices in Health Insurance Markets. NBER Working Paper 22917.

Abdulkadiroğlu, Atila, Pathak, Parag A, Walters, Christopher R, 2018. Free to Choose: Can School Choice Reduce Student Achievement. Am. Econ. J. Appl. Econ. 10 (1), 175–206.

Al-Ubaydli, Omar, List, John A., 2017. Field Experiments in Markets. In: Banerjee, Abhijit V., Duflo, Esther (Eds.), Handbook of Field Experiments, vol. 1. Elsevier, Amsterdam.

Andrabi, Tahir, Das, Jishnu, Khwaja, Asim Ijaz, 2017. Report Cards: The Impact of Providing School and Child Test Scores on Educational Markets. Am. Econ. Rev. 107 (6), 1535–1563.

Anell, Anders, 2011. Choice and Privatisation in Swedish Primary Care. Health Econ. Policy Law 6 (4), 549–569.

Anell, Anders, 2015. The Public-Private Pendulum – Patient Choice and Equity in Sweden. N. Engl. J. Med. 372 (1), 1–4.

Anell, Anders, Dackehag, Margareta, Dietrichson, Jens, 2018. Does risk-adjusted payment influence primary care providers' decision on where to set up practices?. BMC Health Serv. Res. 18, 179. https://doi.org/10.1186/s12913-018-2983-3.

Arrow, Kenneth J., 1963. Uncertainty and the Welfare Economics of Medical Care. Am. Econ. Rev. 53 (5), 941–973.

Athey, Susan, Imbens, Guido, 2017. The Econometrics of Randomized Experiments. In: Banerjee, Abhijit V., Duflo, Esther (Eds.), Handbook of Field Experiments, vol. 1. Elsevier, Amsterdam, pp. 73–140.

Bhargava, Saurabh, Loewenstein, George, Sydnor, Justin, 2017. Choose to Lose: Health Plan Choices From a Menu With Dominated Options. Quart. J. Econ. 132 (3), 1319–1372.

Biørn, Erik, Godager, Geir, 2010. Does quality influence choice of general practitioner? An analysis of matched doctor–patient panel data. Econ. Model. 27 (4), 842–853. URL http://www.sciencedirect.com/science/article/pii/S0264999309001904.

Bordalo, Pedro, Gennaioli, Nicola, Shleifer, Andrei, 2013. Salience and Consumer Choice. J. Polit. Econ. 121 (5), 803–843.

Bruhn, Miriam, McKenzie, David, 2009. In pursuit of Balance: Randomization in Practice in Development Field Experiments. Am. Econ. J. Appl. Econ. 1 (4), 200–232.

Caplin, Andrew, 2016. Measuring and Modeling Attention. Ann. Rev. Econ. 8, 379–403.

Chandra, Amitabh, Finkelstein, Amy, Sacarny, Adam, Syverson, Chad, 2016. Health care exceptionalism? Performance and allocation in the US health care sector. Am. Econ. Rev. 106 (8), 2110–2144.

Cooper, Zack, Gibbons, Stephen, Jones, Simon, McGuire, Alistair, 2011. Does Hospital Competition Save Lives? Evidence from the English NHS Patient Choice Reforms. Econ. J. 121 (554), 228–260.

Dackehag, Margareta, Ellegård, Lina Maria, 2019. Competition, Capitation, and Coding: Do Public Primary Care Providers Respond to Increased Competition?. CESifo Econ. Stud. 65 (4), 402–423. https://doi.org/10.1093/cesifo/ifz002.

Dietrichson, Jens, Ellegård, Lina Maria, Kjellsson, Gustav, 2020. Patient Choice, Entry, and the Quality of Primary Care: Evidence from Swedish Reforms. Health Econ. 29 (6), 716–730. https://doi.org/10.1002/hec.4015.

Ellegård, Lina Maria, 2020. Effects of pay-for-performance on prescription of hypertension drugs among public and private primary care providers in Sweden. Int. J. Health Econ. Manage. https://doi.org/10.1007/s10754-020-09278-y.

Ellegård, Lina Maria, Dietrichson, Jens, Anell, Anders, 2018. Can pay-for-performance to primary care providers stimulate appropriate use of antibiotics?. Health Econ. 27 (1), e39–e54.

Epple, Dennis, Romano, Richard E, Urquiola, Miguel, 2017. School Vouchers: A Survey of the Economics Literature. J. Econ. Lit. 55 (2), 441–492.

Ericson, Keith M. Marzilli, 2011. Forgetting we forget: Overconfidence and memory. J. Eur. Econ. Assoc. 9 (1), 43–60.

Ericson, Keith M., Marzilli, Jon Kingsdale, Layton, Tim, Sacarny, Adam, 2017. Nudging Leads Consumers In Colorado To Shop But Not Switch ACA Marketplace Plans. Health Aff. 36 (2), 311–319.

Ericson, Keith Marzilli, 2017. On the interaction of memory and procrastination: Implications for reminders, deadlines, and empirical estimation. J. Eur. Econ. Assoc. 15 (3), 692–719.

Farley, Donna O., Elliott, Marc N., Short, Pamela Farley, Damiano, Peter, Kanouse, David E., Hays, Ron D., 2002a. Effect of CAHPS Performance Information on Health Plan Choices by Iowa Medicaid Beneficiaries. Med. Care Res. Rev. 59 (3), 319–336.

Farley, Donna O., Short, Pamela Farley, Elliott, Marc N., Kanouse, David E., Brown, Julie A., Hays, Ron D., 2002b. Effect of CAHPS Performance Information on Plan Choices by New Jersey Medicaid Beneficiaries. Health Serv. Res. 37 (4), 985–1007.

Finkelstein, Amy, Taubman, Sarah, Wright, Bill, Bernstein, Mira, Gruber, Jonathan, Newhouse, Joseph P., Allen, Heidi, Baicker, Katherine, Oregon Health Study Group, 2012. The Oregon Health Insurance Experiment: Evidence From the First Year. Quart. J. Econ. 127(3), 1057–1106.

Fryer, Roland G., 2017. The Production of Human Capital in Developed Countries: Evidence from 196 Randomized Field Experiments. In: Banerjee, Abihijit, Duflo, Esther (Eds.), Handbook of Field Experiments, vol. 2. Elsevier, Amsterdam.

Gabaix, Xavier, 2014. A Sparsity-Based Model of Bounded Rationality. Quart. J. Econ. 129 (4), 1661–1710.

Gaynor, M., Moreno-Serra, R., Propper, C., 2013. Death By Market Power: Reform, Competition and Patient Outcomes in the British National Health Service. Am. Econ. J. Econ. Policy 5 (4), 134–166.

Gaynor, Martin, Propper, Carol, Seiler, Stephan, 2016. Free to Choose? Reform, Choice, and Consideration Sets in the English National Health Service. Am. Econ. Rev. 106 (11), 3521–3557.

Glenngård, Anna H., Anell, Anders, Beckman, Anders, 2011. Choice of Primary Care Provider: Results From a Population Survey in Three Swedish Counties. Health Policy 103 (1), 31–37.

Gravelle, H., Liu, D., Propper, C., Santos, R., 2019. Spatial Competition and Quality: Evidence from the English Family Doctor Market. J. Health Econ. 68, 102249.
Gutacker, Nils, Siciliani, Luigi, Moscelli, Giuseppe, Gravelle, Hugh, 2016. Choice of hospital: Which type of quality matters?. J. Health Econ. 50, 230–246.
Handel, Benjamin R., Kolstad, Jonathan, 2015. Health Insurance For Humans: Information Frictions, Plan Choice, and Consumer Welfare. Am. Econ. Rev. 105 (8), 2449–2500.
Hanushek, Eric A., Kain, John F., Rivkin, Steven G., Branch, Gregory F., 2007. Charter School Quality and Parental Decision Making with School Choice. J. Public Econ. 91, 823–848.
Hastings, Justine S., Weinstein, Jeffrey M., 2008. Information, School Choice, and Academic Achievement: Evidence from Two Experiments. Quart. J. Econ. 123 (4), 1373–1414.
Hibbard, Judith H., Berkman, Nancy, McCormack, Lauren A., Jael, Elizabeth, 2002. The Impact of a CAHPS Report on Employee Knowledge, Beliefs, and Decisions. Med. Care Res. Rev. 59 (1), 104–116.
Hibbard, Judith H., Peters, Ellen, Dixon, Anna, Tusler, Martin, 2007. Consumer Competencies and the Use of Comparative Quality Information - It Isn't Just About Literacy. Med. Care Res. Rev. 64 (4), 379–394.
Hsiao, Chun-Ju, Boult, Chad, 2008. Effects of Quality on Outcomes in Primary care: A Review of the Literature. Am. J. Med. Qual. 23 (4), 302–310.
Iversen, T., Lurås, H., 2011. Patient Switching in General Practice. J. Health Econ. 30 (5), 894–903.
Ketcham, Jonathan D., Kuminoff, Nicolai V., Powers, Christopher A., 2016. Choice Inconsistencies among the Elderly: Evidence from Plan Choice in the Medicare Part D Program: Comment. Am. Econ. Rev. 106 (12), 3932–3961.
Ketcham, Jonathan D., Lucarelli, Claudio, Miravete, Eugenio J., Christopher Roebuck, M., 2012. Sinking, Swimming, or Learning to Swim in Medicare Part D. Am. Econ. Rev. 102 (6), 2639–2673.
Klemperer, Paul, 1995. Competition when Consumers have Switching Costs: An Overview with Applications to Industrial Organization, Macroeconomics, and International Trade. Rev. Econ. Stud. 62 (4), 515–539.
Kling, Jeffrey R., Liebman, Jeffrey B., Katz, Lawrence F., 2007. Experimental Analysis of Neighborhood Effects. Econometrica 75 (1), 83–119.
Kling, Jeffrey R., Mullainathan, Sendhil, Shafir, Eldar, Vermeulen, Lee C., Wrobel, Marian V., 2012. Comparison Friction: Experimental Evidence from Medicare Drug Plans. Quart. J. Econ. 127 (1), 199–235.
Knutson, David J., Kind, Elizabeth A., Fowles, Jinnet B., Adlis, Susan, 1998. Impact of Report Cards On Employees: A Natural Experiment. Health Care Financ. Rev. 20 (1), 5–27.
Loewenstein, George, Friedman, Joelle Y., McGill, Barbara, Ahmad, Sarah, Linck, Suzanne, Sinkula, Stacey, Beshears, John, Choi, James J., Kolstad, Jonathan, Laibson, Madrian, Brigitte C., List, John A., Volpp, Kevin G., 2013. Consumers' misunderstanding of health insurance. J. Health Econ. 32 (5), 850–862.
Matějka, Filip, McKay, Alisdair, 2014. Rational Inattention to Discrete Choices: A New Foundation for the Multinomial Logit Model. Am. Econ. Rev. 105 (1), 272–298.
McCormack, Lauren A., Garfinkel, Steven A., Hibbard, Judith H., Norton, Edward C., Bayen, Ute J., 2001. Health Plan Decision Making with New Medicare Information Materials. Health Serv. Res. 36 (3), 531–554.
Moscelli, Giuseppe, Gravelle, Hugh, Siciliani, Luigi, 2016. Market Structure, Patient Choice and Hospital Quality for Elective Patients. CHE Research Paper 139.
Moscelli, Giuseppe, Gravelle, Hugh, Siciliani, Luigi, Santos, Rita, 2018. Heterogeneous Effects of Patient Choice and Hospital Competition on Mortality. Soc. Sci. Med. 216, 50–58.
Rouse, Cecilia Elena, Barrow, Lisa, 2009. School Vouchers and Student Achievement: Recent Evidence and Remaining Questions. Ann. Rev. Econ. 1, 17–42.
Samuelson, William, Zeckhauser, Richard, 1988. Status Quo Bias in Decision Making. J. Risk Uncertainty 1 (1), 7–59.
Santos, Rita, Gravelle, Hugh, Propper, Carol, 2017. Does Quality Affect Patient's Choice of Doctor? Evidence from England. Econ. J. 127 (600), 445–494.
Sims, Christopher A., 2003. Implications of Rational Inattention. J. Monetary Econ. 50 (3), 665–690.
Skellern, Matthew, 2017. The hospital as a multi-product firm: The effect of hospital competition on value-added indicators of clinical quality. CEP Discussion Papers 1484.
Starfield, Barbara, Shi, Leiyu, Macinko, James, 2005. Contribution of Primary Care to Health Systems and Health. Milbank Q. 83 (3), 457–502.
StataCorp, 2013. Stata Statistical Software: Release 13, College Station, TX: StataCorp LP.
Swedish Agency for Health and Care Services Analysis, 2013. Vad vill patienten veta för att välja? Vårdanalys utvärdering av vårdvalsinformation. Report 2013:4.
Victoor, Aafke, Delnoij, Diana M.J., Friele, Roland D., Rademakers, Jany J.D.J.M., 2012. Determinants of Patient Choice of Healthcare Providers: A Scoping Review. BMC Health Serv. Res. 12 (272).
Wahlstedt, Emma, Ekman, Björn, 2016. 'Patient choice, Internet Based Information Sources, and Perceptions of Health Care: Evidence From Sweden Using Survey Data. BMC Health Serv. Res. 16. From 2010 and 2013'.
Young, Alwyn, 2019. Channelling Fisher: Randomization Tests and the Statistical Insignificance of Seemingly Significant Experimental Results. Quart. J. Econ. 134 (2), 557–598.