Increasing price transparency in the Dutch health care market does not affect provider choice*

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Abstract

Price transparency is often viewed as an effective way to encourage price shopping and thereby lower health care expenditure. Using individual claims data for 6 frequent, non-emergency dermatological procedures, we estimate the short-run effect of unexpected publication of prices by a major Dutch health insurer on spending and provider choice. Visits to the price transparency website surged, but spending, the likelihood to visit a new provider, distance traveled, and type of provider visited remained unaffected.

JEL classification: I11, I13.

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1 Introduction

Price transparency is often considered to be an effective way to offset increasing health care expenditure, as providing information about prices combined with cost sharing should drive consumers towards a more cost-conscious choice. Thereby, it also promotes competition between providers (Mehrotra et al., 2017; Hibbard et al., 2012; Volpp, 2016).

This is one of the reasons why many US states have adopted some degree of price transparency legislation (Volpp, 2016) and calls for increased transparency in the Netherlands were supported by the ministry of health (Kleijne, 2016).

Although studies report that only a small fraction of individuals engage in comparing prices (Desai et al., 2016; Chernew et al., 2018), several papers highlight the potential for supply effects (Brown, 2019; Wu et al., 2014). Additional information can also affect the bargaining process (Tu and Lauer, 2009) or cause some providers to adjust prices due to reputational concerns (Christensen et al., 2018).

An increase in transparency can take several forms: from publishing charge prices (Christensen et al., 2018) or median estimated costs (Tu and Lauer, 2009) to equipping employees with privately owned transparency tools (Lieber, 2017; Whaley et al., 2014). In this paper, we study the effect of price transparency on provider choice in a new setting.

For a long time prices negotiated between providers and insurers in the Netherlands were considered private information (Douven et al., 2018). However, in 2016 one of the major insurers in the market, CZ, unexpectedly published a set of prices for procedures below the 885 euro maximum deductible threshold, with main competitors releasing similar information (Kuijper, 2016; De Jong, 2016). In this paper, we use a difference-in-difference approach to estimate the short-run effects that this partial nationwide introduction of a price comparison tool had on health care spending and provider choice. Using a subset of relatively elective and non-emergency dermatological procedures and unique claims data on Dutch health care spending, this paper finds a tightly estimated zero short-run effects on both consumer spending and provider choice. We document that there is a clear potential for savings, ranging from 23% to 25% of the price paid among potential savers. As insurance contracts feature a deductible, many patients would financially benefit from this themselves. Nonetheless, our results suggest that consumers do not exploit these financial opportunities.

This study contributes to a growing literature on the effects of price transparency in health care markets, providing reduced form estimates for the short-run demand effects. Most of the existing literature exploits local, employer-specific transparency tool introduction (eg.: Desai et al., 2016; or Lieber, 2017) and finds varying results from no change to 10-17% decrease in spending conditional on search or as much as 18.7% decrease in spending and changes to the entire market structure when actively approaching consumers (Wu et al., 2014). The so called New Hampshire experiment is a single contrasting example that involved publishing bundled statewide median estimated prices for approximately 30 common medical procedures. While initial studies of this policy indicated no effects (Tu and Lauer, 2009), a 5 year follow-up by Brown (2019) estimated a 5% decrease in the costs for the patients. The following study takes an advantage of a similar large-scale event at a national level, but in contrast to the existing literature it is also able to analyze the effects of publishing exact contracted and ultimately paid prices.

Furthermore, this paper exploits surges in visits on the transparency tool website and the nature of the annual price adjustment in the Netherlands to quantify short-run demand response that can be confidently attributed to consumer behavior. With much of the literature highlighting low usage rates of the transparency tools (eg. Desai et al., 2016; Chernew et al., 2018), this study also investigates the effect of sending a reminder about the transparency tool few months after the initial publication, an event that resulted in over 105 thousand website visits in the first week and a permanent 60% increase in daily visits. Although the treatment group had access to some price information prior to posting of the reminder and hence results should be interpreted with care, we find no evidence for meaningful decrease in spending that would reflect the increase in website visits.

This paper is structured as follows: Section 2 shortly describes the health care system and the events related to the publication of prices. Section 3 introduces the data. Section 4 describes the models we use for quantifying the effects of the events. Section 5 presents results and finally Section 6 provides our preferred explanation for these findings and a proposal how the system could be changed so that price transparency has an effect.

2 Institutional setting

2.1 The Dutch health care system

In 2006 the Dutch health care system underwent a major reform that moved it towards more demand-driven service provision (Enthoven and van de Ven, 2007; Rosenau and Lako, 2008).

Residents in the Netherlands are required to buy a mandatory health insurance from private insurers. The government determines the coverage of the standardized health insurance package. Dutch enrollees have since 2016 an obligatory annual deductible of 385 Euro.

Insurance is provided by private insurers, which are obliged to accept any enrollee (regardless of health risk or pre-existing medical conditions) without price discrimination, with a few minor exceptions. In particular, insurers are allowed to give a rebate of up to 5% on the premium for collective contracts and are allowed to give a rebate for enrollees who choose an extra voluntary deductible of maximal 500 Euro. In addition, insurers are obliged to contract sufficient health care supply to meet demand of their enrollees. The idea behind the system is that insurers can make profits by contracting health care providers and incentivizing them to provide care efficiently. To mitigate risk selection in the insurance market, the Dutch government runs an elaborate system of risk-adjustment in which insurers are compensated for differences between their populations.

Health care providers compete for contracts with insurers. To a large extend (around 70% of hospital revenue), prices of hospitals were liberalized in 2012. In 2005 a case mix system called Diagnosis Treatment Combinations (DTC's) was introduced for the reimbursement of hospital care. According to article 35 of the Healthcare Market Regulation Act hospitals are required to state their invoices in terms of these DTC's, which means that DTC's are comparable between hospitals.

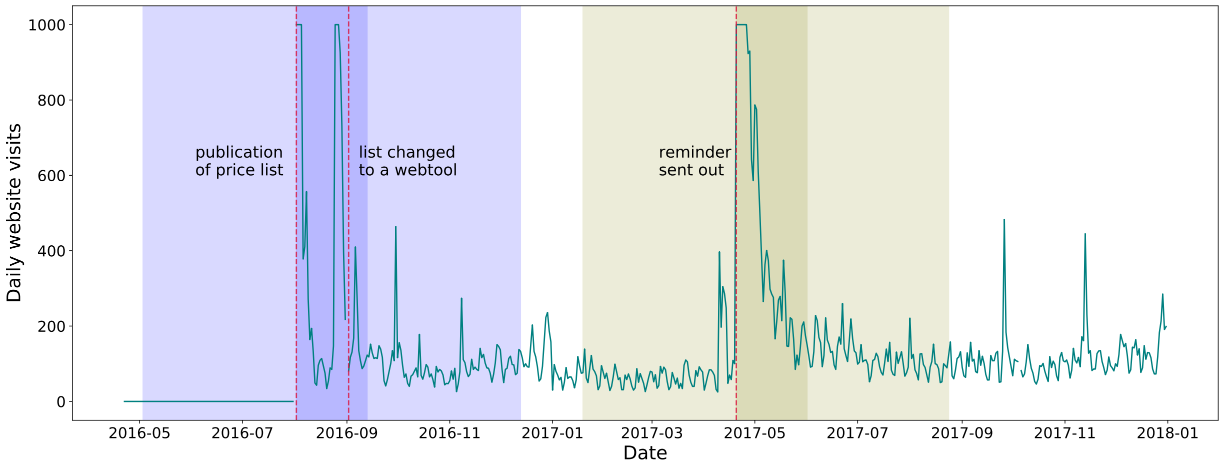
2.2 Information

Motivated by the potential to increase efficiency and contain costs while maintaining quality and accessibility of care (Rosenau and Lako, 2008), the success of a demand-driven health care system such as the one in the Netherlands depends on, among other things, access to information about quality and prices (Enthoven and van de Ven, 2007). In order to ensure competition among market participants as well as promote quality enhancements, consumers should be able to evaluate all the alternatives and make efficient choices (Rosenau and Lako, 2008). Although some degree of information on health care quality is available to consumers in the Netherlands (Beukers et al., 2013), the same could not have been said about hospital prices which up until recently were kept confidential by both insurers and providers (Douven et al., 2018). In light of the fact that these prices are relevant to consumers, because deductible payments directly depend on them, it is surprising that consumers could not easily compare prices between providers.

This situation changed on the 2nd of August 2016 when one of the major insurers in the market, CZ, decided to publish all the contracted Diagnosis Treatment Combinations (DTC) prices below 885 euro (Kuijper, 2016). Soon after, VGZ and Menzis, two other large players in the Dutch market, followed by publishing a similar set of prices (van Bokhorst, 2016; Woldring, 2016). Moreover, another insurance provider, Zilveren Kruis, published more limited data containing only some specific groups of generally described procedures (Skipr, 2016).

Published prices became available through freely accessible search engines located at insurer websites as well as external sources such as Consumer Association (Consumentenbond) website that aggregated prices both across insurers and several transparent providers (De Jong, 2016; search tool is now available as an external module; see Open State Foundation, 2019). With these 4 insurers serving 88.3% of the market in 2017 (NZa, 2017), such publication accounts for a major increase in price transparency.

Figure 1: Daily visits on the CZ website containing the price transparency tool



Note: The figure above plots the amount of daily visits on the CZ price comparison page for the period until 1st of January 2018. The data on prices was initially published in a list format (first dashed vertical line) and replaced a month later by an online tool that allowed for procedure-specific comparison of prices (second dashed vertical line). An email reminder was sent out on 20 April 2017 (third dashed vertical line). In the figure, the maximum number of visits is trimmed at 1000 per day for the clarity of the exposition. Figure 5 in Appendix A zooms in and documents the surge in activity in the first days after the publication of prices and the reminder, respectively. The two shaded areas depict the time periods used in in our main analysis. The darker shades mark the first 6 weeks after the respective event. For some specifications, these data will not be used to account for the fact that it takes time to book an appointment.

Figure 1 presents daily traffic on the website hosting the search tool published by CZ, with maximum daily visits trimmed at 1000 for clarity of the exposition. There was a large traffic increase after the publication of prices, with over 75 thousand visits in first week after the event. Interestingly, there is another spike of activity on the 20th of April 2017, which is when a reminder email about the tool was sent to CZ consumers; this resulted in over 105 thousand visits in the first week after the event.[1] Furthermore, both events resulted in a permanent usage level increase. This is trivially the case for the publication of prices, as usage was zero before the introduction of the tool, but also in case of the email reminder where median daily traffic increased by approximately 65%, from 65 in the period of 12 weeks prior to the event up to 107.5 median daily visits in period between 6th and 18th week after the reminder was sent.

Based on these data, back of the envelope calculations indicate that 98 median daily visits over the entire observed period are equal to approximately 2.43% of daily provider visits made by CZ consumers, suggesting that search rates among consumers are relatively low (similar to Gourevitch et al., 2017; or Mehrotra et al., 2017) despite temporal activity rushes.[2] In addition, it has to be noted that the activity presented resembles visiting the tool's website address and not the actual search activity which may or may not have followed. Furthermore, some consumers may have made several searches during a day. Nonetheless, even such low activity could lead to measurable effects of publishing prices.

[1]See Appendix A for the copy of the reminder sent as well as figures on first weeks post-events.

[2]With approximately 6.9mln procedures in 2017 being recorded (DIS Open Data, 2019) and 21.1% market share of CZ in 2017 (NZa, 2017), this gives an average of 4025 daily visits.

The data on prices reveal that a considerable amount of price information being released. For the 176 unique dermatology DTCs there were over 8800 procedure-provider price pairs available online for CZ by the end of 2016, out of a little above 17 thousand provider-specific prices for that year in total. However, at the same time it is important to keep in mind that due to the two-stage nature of the negotiation process where insurers and hospitals first agree on budget and then negotiate the DTC-specific prices throughout the year (Douven et al., 2018) it could be that some prices were not yet available at the date of publication, despite the publication taking place in the second half of the year.

2.3 Selection of procedures

The aim of our paper is to study the effect of price transparency on patient behavior when patients had enough time to make choices and could benefit from price transparency. We would like to study this for situations in which treatments are fairly simple and standardized, so that quality differences across providers are less important than for other types of care.

With this in mind, we first looked for a type of care that is high volume, not urgent, widely available, and for which the price is relatively low so that cost-sharing matters. For this reason, we focus on dermatology. There are 176 available dermatological procedures. We selected 6 procedures out of those. They are similar to one another, simple and high volume. The account for approximately 66% of all volume within dermatology.

Table 1 contains descriptive statistics for these procedures. There is substantial price dispersion, with the price range often exceeding twice its mean, a disparity that remains large in size across years.

Differences in prices may in principle be completely driven by quality differences across hospitals or differences in the level of competition across geographic areas. This is unlikely for the procedures we chose. To provide empirical evidence for this, Figure 2 shows that negotiated prices often do not seem to follow a systematic pattern. In particular, one would expect that when a hospital has high market power or offers high quality services, then it would generally negotiate high prices relatively to the other hospitals. But then, one should see that many prices for that hospital would be above the average. However, as seen in the upper left and lower right quadrants of the scatter plot in Figure 2, a large fraction of hospitals negotiates a price higher than the average for one procedure and price lower than the average for another, very similar procedure.[3] For instance, in 2016 the Antoni van Leeuwenhoek hospital in Amsterdam charged 143.65 euro for 1-2 outpatient skin cancer checkup visits, while the price for a similar procedure, 1-2 outpatient benign tumor of skin checkup visits, was 80.94 euro. The average price for both procedures

[3]See Appendix B for similar scatterplots between different pairs of products; see Douven et al. (2018) for more detailed analysis of correlation between related product groups.

Table 1: Summary statistics for the selected dermatological procedures

Short Description of the DTC	Year	Provider prices published	Mean Price	Standard Deviation	Minimum Price	Maximum Price	Volume	Percentage of the total
1-2 surgeries	2016	116	451.96	81.38	228.87	737.86	104863	11%
skin cancer or	2017	110	446.25	79.08	231.63	687.27	108622	11%
signs of it	2018	118	438.43	70.58	231.63	696.43	98363	11%
1-2 outpatient visits	2016	114	113.23	21.51	61.42	170.00	249703	27%
skin cancer or	2017	111	112.39	18.06	59.30	185.00	260911	27%
signs of it	2018	120	109.96	16.41	60.19	185.00	240687	27%
1-2 surgeries	2016	118	403.25	65.12	234.84	620.27	39304	4%
benign tumor	2017	113	403.97	66.33	274.50	746.34	37754	4%
of the skin	2018	123	405.82	59.97	275.00	600.00	32972	3%
1-2 outpatient visits	2016	120	114.05	21.50	70.72	225.00	94884	10%
benign tumor	2017	114	111.60	18.63	77.27	185.00	95009	10%
of the skin	2018	123	111.23	17.57	76.92	185.00	85765	9%
1-2 outpatient visits	2016	111	117.59	24.29	70.68	186.67	98842	10%
skin inflammation	2017	105	117.65	20.10	73.29	185.00	100264	10%
or eczema	2018	114	116.71	18.66	76.55	185.00	92213	10%
1-2 outpatient visits	2016	108	123.11	30.69	72.90	222.00	40900	4%
skin conditions	2017	104	120.93	25.09	70.38	211.99	43173	4%
bumps and flakes	2018	115	121.45	22.49	71.44	185.00	39987	4%

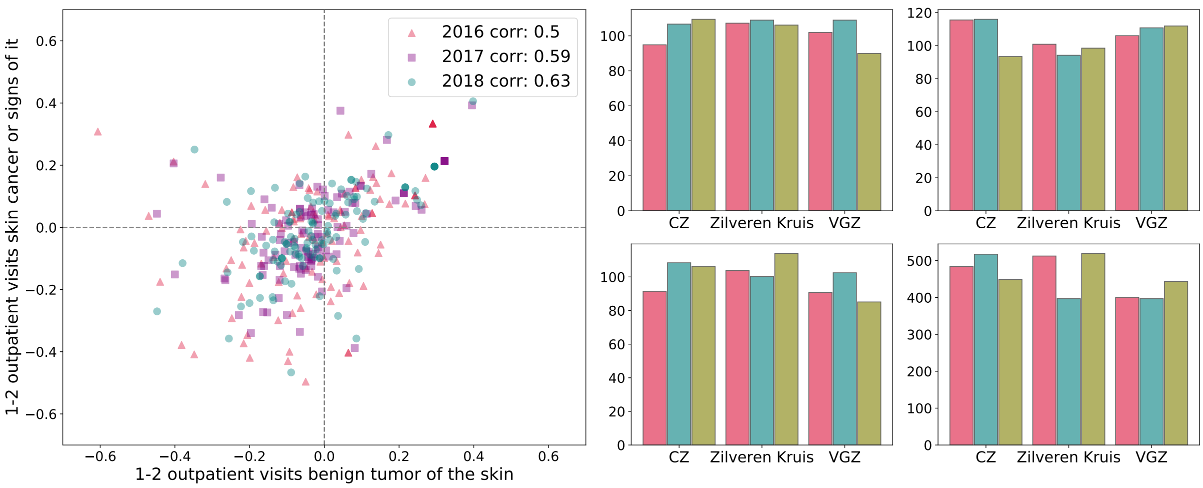
Note: This table presents descriptive statistics for the 6 selected procedures. The table was complied using publicly available price data obtained from the Consumer Association search engine (Open State Foundation, 2019). Volumes were compiled using DIS Open Data (2019). Last column denotes the percentage of the total volume within dermatology DTC subgroup.

was about 113 euro. The bar plots on the right show average prices for each procedure-hospital-insurer combination, for 4 out of the 6 procedures. When analysing it from the side of the insurer, one can see that there is no systematic pattern in prices either.

At this point, one may wonder why there is so much unsystematic price variation. The main reason for this is that the contracts between insurers and hospitals go much beyond specifying prices. For instance, they also specify budgets, information exchange and quality requirements among other things.

From the consumer perspective, this gives rise to an additional challenge. It is possible that a consumer who chooses a provider solely on the basis of low price of some anticipated procedure pays more than average because the procedure coded ex-post is actually more expensive than the alternatives in the area. We expect this to be less of a problem for the 6 procedures we selected, because they are relatively well-defined, though a consumer may still misclassify the DTC, ie. expects skin cancer whereas in fact is diagnosed with a benign tumor of the skin. Nonetheless, although saving opportunities are clearly present in the market due to large price disparities, it remains unclear whether they can be efficiently exploited by the consumers.

Figure 2: Relationship between prices



Note: The scatterplot on the left plots the percentage deviation of the price for one specific DTC (1-2 outpatient visits skin cancer or signs of it; second DTC in Table 1) in one specific hospital and year from the average across hospitals against the percentage deviation of another price (1-2 outpatient visits benign tumor of the skin; fourth DTC in Table 1) from its average. Dots in the upper-left and lower-right quadrants depict hospitals which contracted a price higher than the average for one DTC, and less than the average for another, similar DTC. The bar plots on the right show average contracted prices for 4 out of 6 selected dermatological procedures: 1-2 outpatient visits skin inflammation or eczema (top left), 1-2 outpatient visits benign tumor of the skin (top right), 1-2 outpatient visits skin cancer or signs of it (bottom left), and 1-2 surgeries skin cancer or signs of it (bottom right). Each subplot presents insurer-specific prices for a given DTC contracted with one of the 3 hospitals: Rode Kruis (crimson), Albert Schweitzer (teal) and MC Groep (Zuiderzee Lelystad, Emmeloord, Dronten; olive). The plots show that the price ranking between hospitals is not preserved across insurers and procedures and hence prices negotiated by one insurer are not perfectly informative about prices negotiated by another insurer. See Appendix B for additional scatter plots for other pairs of procedures and the full set of 6 bar plots.

3 Data

Our goal is to estimate the response to the introduction of price comparison tools in the Dutch health care market on patient behavior. For this, we use individual claims data. Data for the entire population were provided by the Dutch Healthcare Authority (NZa). The data cover spending for the years 2015 to 2017 and contain the date of starting the procedure and the exact procedure code (DTC), provider identity, insurer identity and the price paid for the treatments received.[4]

We restrict our sample to the selected group of 6 transparent and widely available procedures from the dermatology specializations described in Section 2. Starting from this, we keep data for two subgroups of patients: CZ customers and customes of another large insurer who, in contrast to several major players in the market, did not publish price data on the selected dermatological DTCs. The patients of the other insurer serve as the control group in our analysis.[5] Since some insurers are part of larger insurer groups, we

[4]This price is always paid by the insurance company. If the treatment falls under the deductible, then the insurance company collects the payment later from the patient. This means that unlike in other settings where patients hand in bills and get reimbursed, we have no missing data on treatments received.

[5]Due to confidentiality issues, neither the identity of this other insurer nor descriptive statistics can be revealed. However, we provide additional evidence that validates the approach taken in the modeling part of the paper.

limit the treatment sample to entities that have the “CZ” name included in the website address that contains the comparison tool.

In order to separate potential demand effects from annual supply adjustments, we perform the analysis locally in time, by using separate within-year subsets of the data.[6] In particular, we select a subset of 12 weeks before and 18 weeks after each relevant event. Such sample selection is not only dictated by a desire to study short run demand response to the specified events, but also to avoid noisy data from the beginning and the end of a calendar year when deductible resets and potential health plan switching is occurring. This selection results in two datasets: May 10 until December 6 in 2016 (with the relevant event occurring on the 2nd of August), and January 26 until August 24 in 2017 (with the relevant event occurring on the 20th of April). Figure 1 marks the relevant subsample periods with blue and green shades, respectively.

We also add several consumer characteristics: 8 age bins, gender, dummy variables for deductible level chosen, any additional health care package, dental plan or collective insurance and the registered location of the patient: postcode, municipality and province. In case of within year change to any of these characteristics, we use the characteristic level that was the most frequent in the year. There are some missing values on these characteristics, but these result in dropping only approximately 0.2-0.3% of the data.

We restrict the samples to only individuals above 18 years old, which covers little over 94% of the sample.[7] Some visits cannot be matched with distance due to missing provider postcode but this does not affect the main results. Some observations are either duplicated or indicate that consumer visited two different providers for the same procedure within the same day. Since this is rather improbable and the observations constitute less than 0.1% of the samples, we exclude them as well. Finally, we drop 9 observations with zero recorded price.

We assume that if a procedure was transparent in a given year, price data for most providers was already available to consumers at date of the relevant event. While this is a plausible assumption for the publication of prices in August, it is slightly less likely for the reminder sentout sample since for many hospitals prices may not yet have been available in late April. Although we expect that a substantial amount of prices is already agreed upon and available for publishing if CZ decided to replace old set of prices with new ones, the estimated effect should be interpreted as the effect of such partial information gain, with an expectation that less information was available at the second event relatively to the first one.

Since prices published by CZ are publicly accessible, it could be argued that individuals

[6]Price adjustments within a year are possible. But this mainly concerns lower-volume treatments. , Across different procedure groups and years there are within-year price changes for 16-17% of all unique prices, but only 1.3-1.9% of actual observations in the dataset. Hence even if they were an effect of price adjustment and not health plans, the fraction of observations affected is negligible.

[7]Children until 18 years old do not have to pay a deductible.

Table 2: Potential savings for the selected procedures

	Publication of prices (August 2, 2016)				Reminder (April 20, 2017)			
	Cheaper and closer	% of the sample	Cheaper and within 10km	% of the sample	Cheaper and closer	% of the sample	Cheaper and within 10km	% of the sample
Treatment	8%	100	9%	100	7%	100	8%	100
	25%	30	23%	35	23%	30	22%	36
Control	8%	100	11%	100	6%	100	8%	100
	23%	33	22%	46	18%	32	17%	45

Note: This table provides information on the potential savings. We distinguish between the treatment and the control group. Numbers are for the pre-event period and for the 6 selected DTCs. For each group, the first row presents results averaged over the entire subsample while the second row only averages over the individuals who can save, with the fraction of population averaged over reported in "% of the sample" column. The column "Cheaper and closer" denotes average percentage savings if individuals would choose a provider that is both cheaper and located closer; "Cheaper and within 10km" denotes average savings if individuals would choose a cheaper provider within the distance of 10km. All percentage values are calculated with reference to the actual amount paid. We use a maximum of 100km distance from the individual postcode to construct the set of alternative providers. Some providers may have several postcodes; since we only observe the choice of the provider and not the location, we assume that the individual visits the closest location among the available ones.

from the control group are able to view them as a proxy for their own prices. Graphs on the right of Figure 2 present prices published in 2016 by 3 transparent hospitals for procedures contracted with two publishing insurers: CZ and VGZ, and Zilveren Kruis who did not publish these particular prices. One can observe that price rankings between hospitals are not preserved among insurers: a hospital that is least expensive for CZ may turn out to be the most expensive for VGZ or Zilveren Kruis. Although this observation cannot be easily generalized to full set of hospitals due to lack of the relevant data, it provides some evidence against the argument that consumers can use price indicators from different insurers as a proxy for their own prices in order to rank hospitals accordingly.

It is important for the validity of the results (details below) that both treatment and control groups face similar saving opportunities before the events (see Appendix C for additional evidence for pre-event similarities between both groups). To assess this, we check whether there are cheaper providers of the same procedures within a reasonable traveling distance and whether there are providers that are both closer and cheaper than the one that was selected. A breakdown of the saving opportunities for the consumers is presented in Table 2. Overall, consumers can save up to 6-11% by choosing a cheaper provider within their choice sets, with savings being substantial even when consumers are to choose a provider that is both cheaper and closer to their location. When aggregating only over individuals who can save from switching, savings can be as high as 25% of the price paid, with a fraction of the population that can save ranging from 30% to even 45% of the subsample. More importantly, potential savings before the policy events are similar for both treatment and control groups: the treatment group faces marginally larger opportunities, which may be a result of larger price dispersion seen in Figure 10, but at the same time a higher fraction of the control sample can save (32-45% compared to 30-36% among the treatment group). Overall, saving opportunities are substantial and available to both groups, which is not surprising given the large dispersion of prices reported earlier. Under the hypothesis that consumers make efficient use of the tools, these opportunities should be exploited by the treatment group once prices become public.

4 Empirical approach

4.1 Effects on spending

Our aim is to estimate the effects of making a price comparison tool available to CZ customers and reminding them of this tool by email. For this, we use claims data over time. There is a control group, which allows us to use a differences-in-differences estimator. For this, we pool observations across procedures and specify

$$\log(p_{ijklmt}) = \beta \cdot (CZ_i * Post_t) + X_i'\alpha + \delta_j + \eta_k + \theta_l + \gamma_t + \kappa_m + \epsilon_{ijklmt}, \tag{1}$$

where p_{ijkmt} is the price paid by the consumer i for procedure j, who is insured with k and receives the treatment on day m of week t, CZ_i indicates that i is a CZ insuree, and $Post_t$ indicates the time periods after the introduction of the price comparison tool or the reminder (we perform separate analyses for the two events). We include a set of controls in vector X_i with coefficient vector α: gender, age, type of health care package, a dummy variable for collective contract and level of the voluntary deductible. We also control for fixed effects of procedure (δ_j), insurer (η_k), province (θ_l), week (γ_t) and day of the week (κ_m).

As usual, β is our parameter of interest. We use a log specification. Therefore, β is the percentage change in the price paid after the introduction of the price comparison tool or after the email reminder was sent out.

Equation (1) is estimated by ordinary least squares, following Brown (2018), Lieber (2017) and Desai et al. (2016). There may be a correlation between individual spending within the same household over time. Since we do not distinguish households and most of the individuals appear only once in the samples, we cannot add fixed effects to account for this in a fashion similar to Lieber (2017). Instead, we cluster the standard errors at the four digit postcode level which should not only account for correlation within households over time, but also subtle location differences such as public connection routes or local GP referral preferences. To minimize the chance that our results are driven by outliers, we follow Lieber (2017) and also estimate a winsorised version of the baseline equation where we trim 5th and 95th quantiles, separately for each procedure in the each sample but jointly for both insurers.

Publication of prices creates a solid baseline for difference in difference design since one group of consumers gained access to negotiated prices while both groups had similar information on prices before the publication. While we also attempt to exploit the second surge in website activity resulting from sending a reminder to CZ consumers, it has to be highlighted that in this particular case treatment group had access to some degree of price information before the event. While new prices were published within 2 weeks prior to the date of the email reminder, CZ tends to keep old prices as a reference due to correlation with newly negotiated ones. So while usage before the email reminder was sent was scarce and information gained was not perfect due to imperfect correlation of prices across years, the results of the second policy estimation should be interpreted with care since the two groups are not exactly the same in the pre-policy period.[8]

One of the potential problems with creating a treatment variable in the specification above is the fact that one should account for the waiting time between signing up for treatment and actually receiving it. Using waiting times data for dermatological procedures in 2016 and 2017, we determine that on average the waiting times are little above 3 weeks, with 90th quantile of waiting times distribution equal to 6 weeks. To avoid a situation where some visits are appointed before the event while others are not, we estimate the baseline equation (1) while only using a subsample that excludes first 6 weeks after the event. This way we can ensure that the transition period does not affect the results, providing a more direct estimate of the level difference. Since procedures obtained on the weekend may not be elective and therefore less susceptible to shopping, we further estimate the baseline equation while excluding observations on procedures obtained on either Saturday or Sunday. We also attempt to remedy potential discrepancies in prices negotiated by the insurers by including large municipality fixed effects that account for local variation in negotiated prices.[9]

Formally, our main identifying assumption is that the error term in (1) is not correlated with the right hand side variables. Importantly, for this to hold, the usual "common trends assumption" needs to hold. In words, this assumption is that the evolution of the outcome over time is the same for the treatment and the control group. In order to assess this, we revisit equation (1) and replace the treatment dummy with weekly insurer fixed effects in order to determine whether parallel trends in spending are present among the two groups of consumers. Specifically, we estimate the model

$$\log(p_{ijklmt}) = \beta_{kt} + \alpha X_i + \delta_j + \eta_k + \theta_l + \kappa_m + \epsilon_{ijklmt}, \tag{2}$$

where β_{kt} denotes weekly insurer fixed effects separately for the treatment and control group. Then, we plot the fixed effects for the two groups over time and compare the

[8] As informed by the insurer, the new prices were published between 5th and 19th of April, with the reminder being sent on the 20th. Unfortunately, it was impossible to track the exact date of the publication; see Appendix A for further discussion of the price publication event.

[9] Fixed effects are only added for municipalities that have at least 100 observations in the pre-policy period; see Appendix C for further information.

evolution in the pre-treatment period between the two groups. It is useful to also estimate fixed effects for the post-treatment period. If evidence in favor of parallel trends has been gathered from data for the pre-treatment period, then one can use the plot for the post-treatment period to get a first idea about the size of the treatment effect and whether it is constant over time. If a treatment effect is present and the effect is constant over time, then one should see that the curve for CZ patients is shifted, but otherwise the evolution is the same. And if the treatment effect is small, then such a plot will indicate this as well.

In addition, we estimate the baseline equation over the period of 12 weeks before the event with an artificially created placebo treatment dummy in the middle of that period. The aim of this exercise is to ensure that any estimate found was not present directly before the event and that no effect is found just by virtue of a large sample size.

Finally, we estimate the baseline equation with an addition of a linear trend for the treatment group over the entire sample period. This addresses the concern that trends are different. This specification can also be used to formally test whether pre-trends are similar if one makes the additional assumption that the treatment effect is constant (which—anticipating the results—is implied by a zero treatment effect).

4.2 Effects on choices

Given that prices are largely fixed within a year, any effect on prices paid by the consumers should also be reflected in the underlying provider choice. Moreover, consumers may not react to price differences but instead learn about new alternatives in the area or browse through insurer website to learn about non-monetary provider characteristics, resulting in altered choices despite no effects on the average prices paid. To investigate that hypothesis further, we estimate parameters of the model

$$newProvider_{ijklmt} = \beta \cdot (CZ_i * Post_t) + X_i'\alpha + \delta_j + \eta_k + \theta_l + \gamma_t + \kappa_m + \epsilon_{ijklmt}, \qquad (3)$$

where the dependent dummy variable $newProvider$ denotes whether an individual visited a provider that is not in the pool of providers visited by the same patient for the DTC within 12 months prior to the current visit date. This is a proxy variable that should provide some indication for novelty in choice. The equation includes similar controls and fixed effects as equation (1) and is estimated with least squares, with standard errors clustered at the postcode level.[10] Since switching behavior is only observed conditional on visiting a provider in the last 12 months, the model is estimated over a subsample of individuals.

In a similar fashion as before, we also estimate equation (3) with additional 6 weeks delay to account for the waiting times. We further estimate another variant of equation

[10] Results are qualitatively similar when we use a logit model.

(3) where we exclude observations related to treatments that were received on the weekend, since such visit may be relatively less elective. Since a one year time span may be considered too long, we estimate a version of the equation (3) where *newProvider* takes a value of 1 if the last provider visited for a DTC within last 12 months is different from the current choice. Finally, we run a placebo test on the pre-policy subsample, add a linear trend to the baseline specification and estimate weekly fixed effects, in each case investigating whether both groups follow similar trends in a similar fashion as in the case of price regressions.

A change in provider choice can also be reflected in the choice of the provider type. Therefore, we also investigate whether consumers are more likely to visit a free-standing facility (ZBC; often specialized clinics that offer outpatient procedures), by estimating the model

$$ZBC_{ijklmt} = \beta \cdot (CZ_i * Post_t) + X_i'\alpha + \delta_j + \eta_k + \theta_l + \kappa_m + \gamma_t + \epsilon_{ijklmt}.$$

These facilities may be generally less frequently visited than hospitals, but access to the transparency tool may increase their salience and result in higher probability of a visit. Finally, the effects of transparency change can have an effect on the distance traveled since consumers may find a cheaper provider located further than the most salient close alternative or become aware of a provider that is located closer to them. More specifically, we specify:

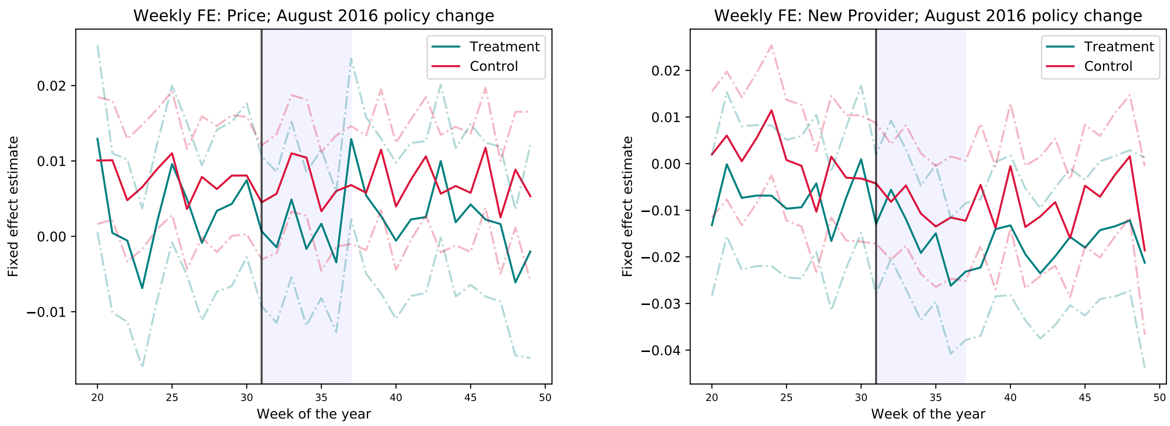
$$distance_{ijklmt} = \beta \cdot (CZ_i * Post_t) + X_i'\alpha + \delta_j + \eta_k + \theta_l + \kappa_m + \gamma_t + \epsilon_{ijklmt}.$$

As before, we estimate the parameters with ordinary least squares, but without clustering the standard errors at the postcode level. Since some providers may have several locations and we only observe the choice of the provider and not the exact location, we assume that the consumer goes to the closest among the available locations of the provider. Although plausible, this is a simplifying assumption since not every location may be offering the dermatological procedures and hence the results should be interpreted with caution.

5 Results

Figure 3 shows the evolution of main outcomes over time. The graph on the left is for the price consumers paid and shows very similar, almost flat trends for both treatment and control group. There is no indication of a treatment effect (see discussion below equation (2)). The graph on the right is for the likelihood to choose a new provider and shows a small downward trend in the first few weeks, yet also here there is no visible difference in the evolution between the groups both pre- and post-treatment and hence

Figure 3: Evolution of main outcomes over time around the time of the publication of prices



Note: The graphs above presents estimates of insurer-week fixed effects. Based on equation (2) and the corresponding equation with outcome variable $newProvider_{ijklmt}$. Solid lines denote the fixed effects, plotted separately for treatment and control groups. Confidence intervals are constructed using clustered standard errors. Weekly fixed effects are normalized with respect to the first week of the control group (first week omitted in the figure). Vertical lines denote the event date.

also no indication of a treatment effect.

The estimation results for model (1) are presented in Table 3. The baseline estimate of a 0.04% decrease in price paid is insignificant and, given low standard errors, points towards a precicely estimated zero effect of the policy. The effect remains similar in magnitude and insignificant when using a winsorized version of the dependent variable. Although the initial publication in a list format or potential booking delays could have affected the results, excluding the first 6 weeks post event results in an insignificant estimate that further changes in sign. Excluding weekend days from the sample or adding large municipality fixed effects does result in slightly higher estimates in terms of absolute values (-0.07% and -0.13%, respectively) that nevertheless remain insignificant. Importantly, the model passes the placebo test and the estimate of a linear trend for the treatment group is not significant at any conventional level, giving supportive evidence for the difference-in-difference approach taken in this paper.

The reduced form evidence for policy effects on consumer choice are displayed in Table 4. While it is possible that patients pay the same prices while choosing different providers, these results are generally in line with the view that price transparency had no effect on either. There is no significant effect of price publication on choosing a provider different than the pool of providers visited for a DTC within last 12 months. The results are further robust to adding a delay, excluding weekend days or reducing the pool of the providers to just the last visited location. Furthermore, there is no significant effect of the event on choosing a ZBC type provider or on the distance traveled to the chosen provider. The overall robust evidence suggests that publication of prices by CZ had no short-run effects on either prices paid or the underlying choice of the company's consumers as compared to

Table 3: Effects of publication of prices on consumer spending

	Base	Winsorized	Delay	NoWeek	Municipal	Placebo	Base^	Municipal^
$CZ_i * Post_t$	-0.0004	-0.0005	0.0004	-0.0007	-0.0013	0.0026	0.0008	-0.0001
	(0.0014)	(0.0012)	(0.0016)	(0.0016)	(0.0013)	(0.0025)	(0.0030)	(0.0028)
Linear Trend							-0.0001	-0.0001
							(0.0002)	(0.0002)
Fixed effects	Yes	Yes	Yes	Yes	Partially	Yes	Yes	Partially
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.923	0.945	0.923	0.927	0.933	0.922	0.923	0.933
Observations	190983	190983	151104	159443	190983	72367	190983	190983

Note: The table presents treatment effect estimates for the log price regressions. "Base" displays results for baseline equation. "Winsorised" estimates the baseline equation while using a winsorised dependent variable, with trimming at 5th and 95th quantiles. "Delay" estimates the baseline equation while excluding the first 6 weeks after the event. "NoWeek" estimates the baseline model while excluding the weekend days from the sample. "Municipal" adds large municipality dummy variables to the baseline model. "Placebo" estimates the baseline model over the pre-policy period, with an inclusion of placebo dummy variable for treatment insurer in the middle of that period. "Base^" and "Municipal^" add linear trends for treatment group over the entire sample. Fixed effects include procedure, insurer, province, week and day of the week. Individual controls include age, gender, type of health care package, collective contract and level of the deductible. All regressions are estimated using ordinary least squares, with standard errors clustered at the 4 digit postcode level.

a control group from another insurer. In each case, the results indicate a tightly estimated zero effect of the event, supporting the conclusions that substantial consumer awareness about the existence and availability of price information does not necessarily result in short-run demand effects.

Figure 4 shows the evolution of the main outcomes around the time of the email reminder. There is a visible difference in trends between treatment and control group. The treatment group has a slightly more negative trend in case of the price regression as seen in the graph on the left of Figure 4, and a more positive pre-trend in case of the new provider regression as seen in the graph on the right of Figure 4 (as the trend is close to zero and the trend for the control group is negative). While these differences do not seem substantial in case of the price regression, they may in both cases invalidate the difference-in-difference approach when we do not take this into account. As discussed before, we remedy this by including an additional linear time trend for the treatment group.

Table 5 presents estimation results for the same set of models as Table 3, now estimated using the posting of the email reminder as the relevant event. The last two columns confirm that there is a significant difference in the time trend (columns 'Base^' and 'Municipal^'), as already indicated in Figure 4. Once we control for this, we find insignificant effects of the email reminder. Finally, results for the effect of reminder sentout on consumer choice are presented in Table 6. Also here, once we control for linear trends, we find insignificant effects of the email reminder.

Table 4: Effects of publication of prices on consumer choice

	Same as previous provider(s)						ZBC provider type		Distance	
	Base	Delay	NoWeek	Last	Placebo	Base^	ZBC	ZBC^	Dist	Dist^
$CZ_i * Post_t$	-0.002	-0.002	-0.002	-0.001	0.005	-0.002	0.003	-0.003	0.1446	0.2023
	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.003)	(0.005)	0.1427	(0.2708)
Linear Trend						0.000		0.000		-0.0039
						(0.000)		(0.000)		(0.0158)
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.054	0.053	0.046	0.031	0.051	0.054	0.094	0.094	0.085	0.085
Observations	159255	125934	128727	159255	59442	159255	190983	190983	190971	190971

Note: The table presents treatment effect estimates for the choice regressions. "Base" presents baseline results for choosing a new provider different than the pool of providers visited within last 12 months. "Delay" estimates the baseline model with exclusion of first 6 weeks post event. "NoWeek" excludes weekend days from the sample. "Last" modifies the dependent variable to take value of 1 only if the last visited provider 12 months prior to the procedure is different than the current one. "Placebo" estimates the baseline model over the pre-policy period, with an inclusion of placebo dummy variable for treatment insurer. "ZBC" estimates the baseline model with ZBC (specialized clinic) dummy as the dependent variable. "Dist" estimates the baseline model with distance in kilometers as the dependent variable. "Base^", "Dist^" and "ZBC^" add linear trends for treatment group over the entire sample. Fixed effects include procedure, insurer, province, week and day of the week. Individual controls include age, gender, type of health care package, collective contract and level of the deductible. All regressions are estimated using ordinary least squares and except for the "Dist" and "Dist^" specifications, standard errors are clustered at the postcode level.

Table 5: Effects of email reminder on consumer spending

	Base	Winsorized	Delay	NoWeek	Municipal	Placebo	Base^	Municipal^
$CZ_i * Post_t$	-0.0005	-0.0005	-0.0017	-0.0006	-0.0020*	-0.0024	0.0044	0.0032
	(0.0014)	(0.0011)	(0.0014)	(0.0015)	(0.0012)	(0.0023)	(0.0028)	(0.0024)
Linear Trend							-0.0003**	-0.0004**
							(0.0002)	(0.0001)
Fixed effects	Yes	Yes	Yes	Yes	Partially	Yes	Yes	Partially
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.936	0.960	0.936	0.938	0.949	0.935	0.936	0.949
Observations	186814	186814	151364	157170	186814	74113	186814	186814

Note: See notes to Table 3.

6 Discussion

This paper finds a tightly estimated zero effect of the publication of prices on health care spending and provider choice in the Netherlands. This result stands in stark contrast to the majority of the literature. At first this is a surprising result, as consumers did visit the website on which they could look up prices (Figure 1) and there were opportunities to save money (Table 2).

One possible explanation is that large shopping opportunities that are in principle available to the consumers may be difficult to exploit because consumers are unable to know about them, even when the information is in principle available. In the descriptive part of this paper we have shown that prices for very similar procedures offered by the same provider are often very different, in unsystematic ways. Combined with a complex system of DTC relations and uncertainty over which particular procedure will be applied consumers may therefore find it challenging, if not hopeless, to efficiently shop among the available providers. In addition, 87.7% of consumers in 2017 faced only a 385 euro annual

Figure 4: Evolution of main outcomes over time around the time of the reminder email

Note: The graphs above present estimates of insurer-week fixed effects. Solid lines denote the fixed effects, plotted separately for the treatment and control groups. Confidence intervals are constructed using clustered standard errors. Weekly fixed effects are normalized with respect to the first week of the control group (omitted in the figure).

Table 6: Effects of email reminder on consumer choice

	Same as previous provider(s)						ZBC provider type		Distance	
	Base	Delay	NoWeek	Last	Placebo	Base^	ZBC	ZBC^	Dist	Dist^
$CZ_i * Post_t$	0.014***	0.013***	0.014***	0.007	0.015***	0.005	-0.008***	-0.002	-0.0495	-0.4080
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.003)	(0.005)	(0.1446)	(0.2870)
Linear Trend						0.001*		-0.000		0.0241
						(0.000)		(0.000)		(0.0161)
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.054	0.054	0.049	0.034	0.053	0.054	0.099	0.099	0.086	0.086
Observations	155997	126170	127065	155997	61545	155997	186814	186814	186754	186754

Note: See notes to Table 4.

deductible (NZa, 2017). Therefore, the incentive for price shopping may be too small to invest into understanding what the different DTCs are so that the prices for the right DTC can be compared.

So, alongside the strong evidence for website usage and consequently a considerable awareness about the availability of the tool and product prices, this may indicate that consumers either checked the website out of interest and did not make use of the information gained or the system is too complex and costly to efficiently shop for the health care products.[11] This conclusion is highly in line with Semigran et al. (2017) who noted that while consumers support the price transparency concept, they face several barriers to efficiently use the tools available.

[11]For data availability reasons, we do not distinguish between consumers who have already crossed the deductible limit (and are therefore not subject to cost-sharing) and those who have not. This, however, does not invalidate the conclusion that access to price information, given estimates with very low standard errors, had no significant overall effect on spending at the population level. Furthermore, we did not find any evidence of an effect on provider choice. This means that it is unlikely that patients used the tool to find expensive providers, took the price as a signal of quality, and switched to those.

One may wonder how this could be changed. Instead of using the negotiated DBC-prices as a basis for the deductible payments, insurers could state fixed prices for deductible payments for procedures that patients are likely to understand, such as for out-patient visits, drugs prescriptions, or surgery.[12] This price could then be multiplied with a provider-specific factor that is related to the average reimbursement the provider receives from the insurance company. If, on average, prices are higher for a provider, then this factor should be bigger than 1, otherwise smaller.

To conclude, our results suggest that price transparency alone does not have an effect on market outcomes in the Dutch settings. Our intuition for this result is that the health care system is too complex from the patient perspective. This suggests that if policy makers would like to enhance competition between providers, then they should simplify the system for the patients. This could be done without changing the deductible level and would tentatively lead to lower out-of-pocket payments, as it would make it easier for patients to price-shop.

[12] Indeed, a few years ago the Dutch Association of Hospitals has proposed to simplify the deductible payments (Van Rooy, 2017) and the health insurer Menzis started to implement this proposal in 2017 (Van Aartsen, 2017). But policy makers did not support this and, to date, no big changes have been made. The reason is that any discussion related to cost-sharing is considered politically sensitive in the Netherlands.

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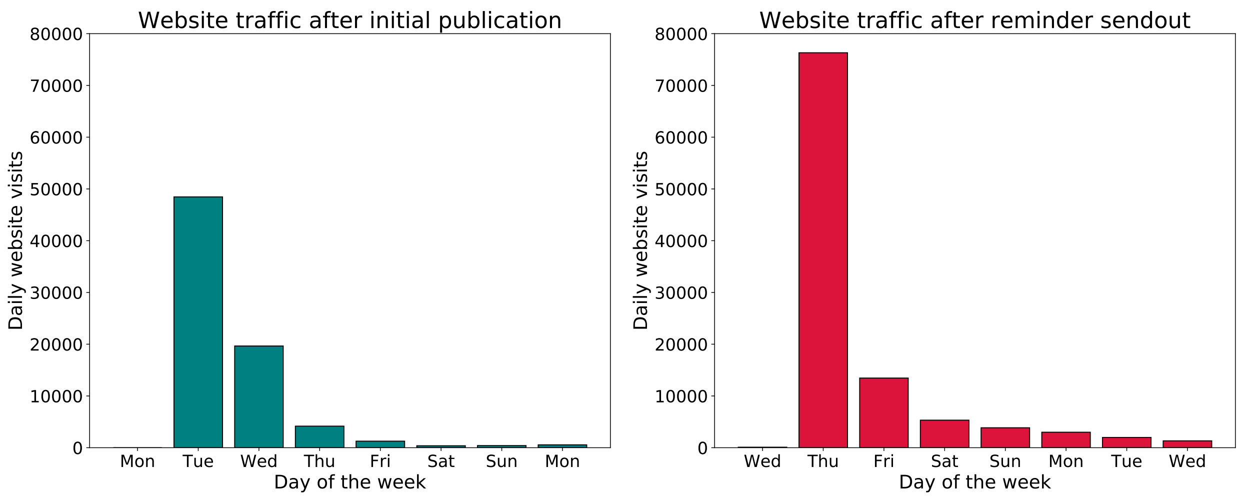
7 Appendices

Appendix A: Published prices

Preceded by a significant social pressure (Open State Foundation, 2015), the publication of price data by CZ received considerable attention. Figure 1 in Section 2 displays the daily traffic on the website that contained the price information published by CZ. Despite the fact that initially prices were published in a list format, there was a substantial amount of traffic on the website that continued well into September and October of the year. Not surprisingly, initial publication resulted in almost 75 thousand visits in the first week after the event, with the majority of that activity happening in the first two days (displayed more clearly on the left of Figure 5). There was also a third (trimmed) spike towards the end of August, but its magnitude was relatively small (around 10 thousand views overall) and there is no explanation for it. It is possible that there were some media reports in this period that related to the publication of prices before it happened, resulting in an increase of the visits on the website.

Figure 5: Website traffic in the respective first week after the events



Note: The figure on the left presents the surge in activity in the first week after the publication of the price list. The figure on the right presents the daily amount of visits in the first week after the reminder email was sent out. Note that each figure starts 1 day before the actual event and that traffic on these days was very low.

There was an even bigger surge in traffic on the website right after the 20th of April 2017, a result of an email about the comparison tool (displayed in Figure 6) being sent out to the CZ consumers. Since the reminder was sent to CZ consumers only, it guarantees that the vast majority of over 105 thousand visits in the first week after that were made by individuals for whom the prices were indeed relevant. In contrast, the initial publication may have gained attention of the consumers of other insurers as well who, perhaps as a result of the media coverage, visited website out of curiosity.

Overall, between 2nd of September 2016 (excluding the list format publication period

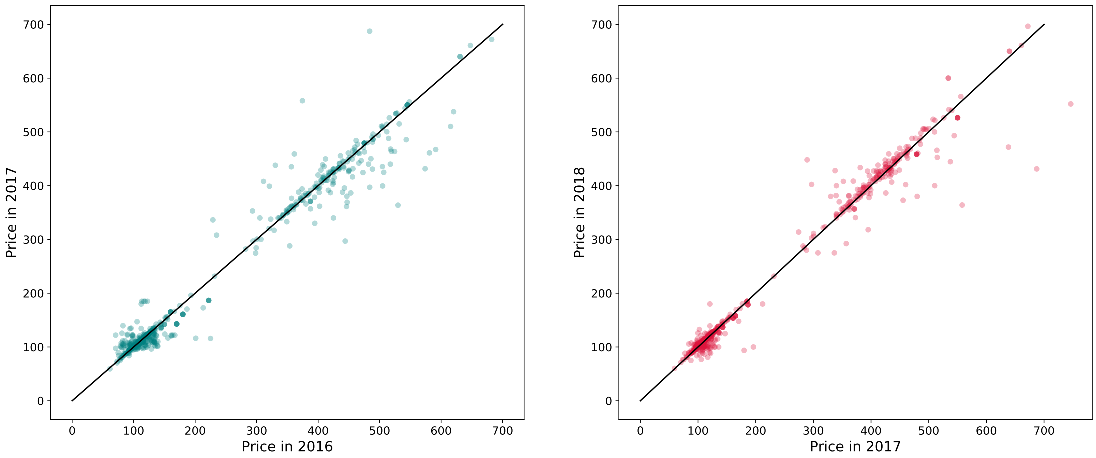
Figure 6: Part of the email sent to CZ consumers informing about the transparency tool

Notes: This figure shows the main part of the reminder email. It reads: "What does your treatment cost? What does a visit to the specialist actually cost? And how much do you pay for an operation for nasal or throat tonsils? Many people do not know what the price of a treatment is. And are surprised by the bill. CZ is happy to give you more insight. With us you can easily view and compare a large number of hospital rates up to 885 euros. This way you know where you stand."

of August) and 4th of October 2017 there were over 153 thousand visits to the website, with almost 84% of them being unique; users spent on average 2 minutes and 14 seconds on the website. Narrowing this down to the study periods, as indicated in Figure 1, the median number of daily visits was 98, with this amount varying from 51 (10th quantile) to 255 (90th quantile). It remains unclear, however, how many of these visits were followed up by any subsequent search activity as the visits are recorded on the search tool URL.

It is often the case that while prices for the current year are not yet available, CZ will keep the old prices on the site for consumer use. A natural question arises whether these are a good indicator of actual prices being paid; Figure 3 highlights the relation of prices negotiated by CZ with providers in years 2016 and 2017. While there is some adjustment across years, overall prices seem to be quite similar indicating that old prices could potentially be used as proxies for the new ones. At the same time, they are not perfectly correlated and therefore such information remains imperfect.

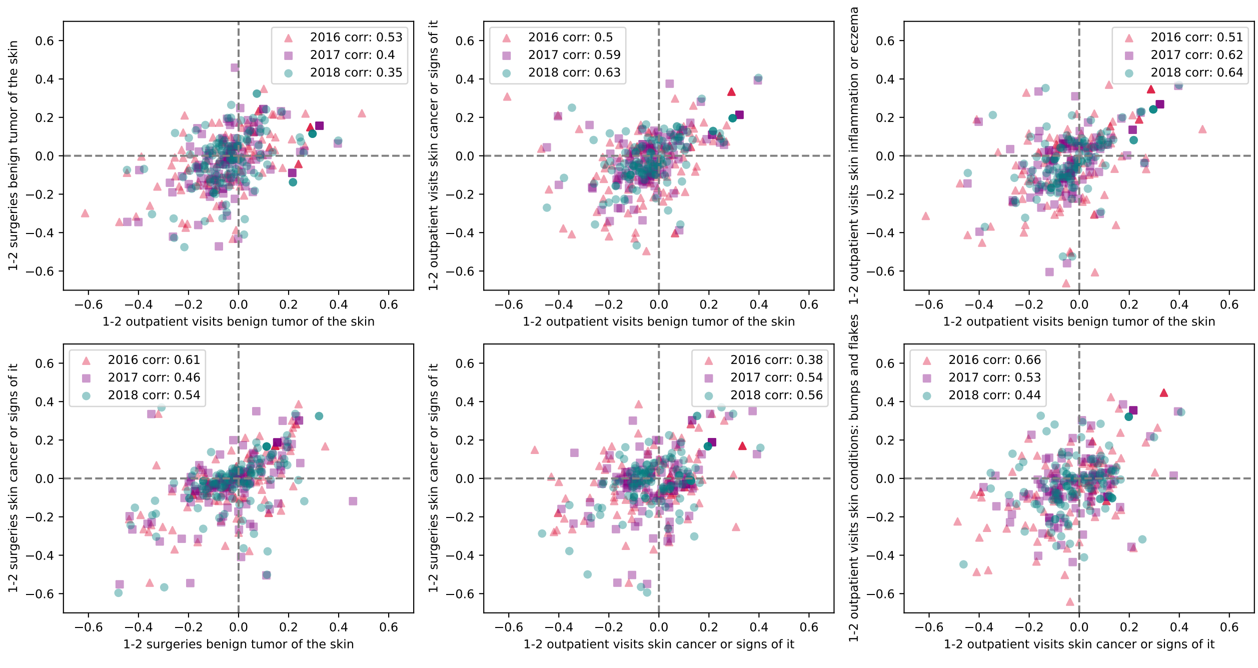
Figure 7: Relationship between prices negotiated by CZ across years



Note: Figures above present scatterplots of prices in 2017 and 2018 against prices in the respective previous year. Each dot is an available provider-procedure pair. While there appears to be some adjustment for several procedure-provider pairs across years, overall the prices are highly correlated between years and prices from 2016 can be used as a good proxy for 2017 prices.

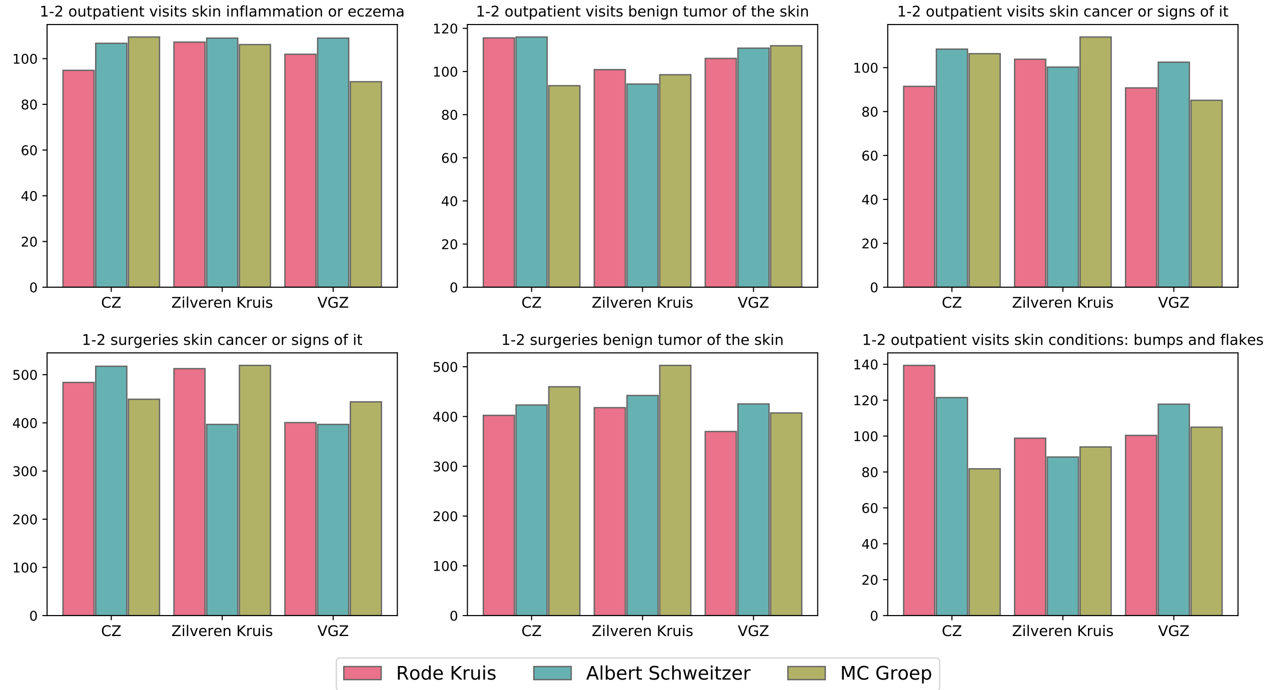
Appendix B: Price dispersion

Figure 8: Relationship between prices for similar DTCs



Note: The scatterplots above plot pairs of relative prices (computed as a percentage difference to the mean across hospitals) of selected pairs of the 6 DTCs that are used in the analysis against one another, like in Figure 2. See main text and notes to Figure 2 for additional details and discussion.

Figure 9: Differences in prices across insurers

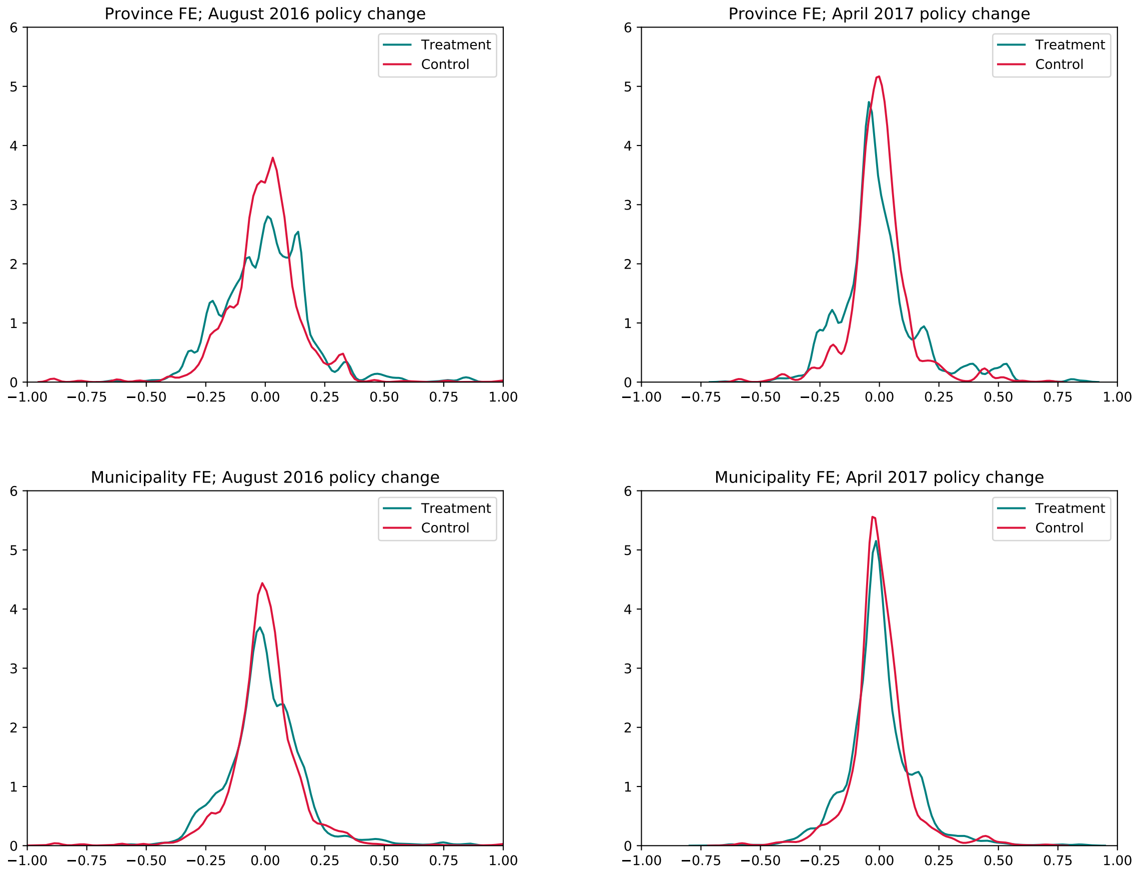


Note: The bar plots show average contracted prices for selected dermatological procedures. Each subplot presents insurer-specific prices for a given DTC contracted with one of 3 transparent hospitals. The figure shows that the price ranking between hospitals is not preserved across insurers and procedures and hence prices negotiated by one insurer are not perfectly informative about prices negotiated by another insurer. See main text and notes to Figure 2 for additional details and discussion.

Appendix C: Fixed effects

As discussed in Section 4, the validity of the empirical approach in this study hinges on the similarity between the treatment and control group. Here, we follow Lieber (2017) and provide evidence on the distribution of the residuals for the pre-periods. The upper row of Figure 10 presents residuals from a regression of the log price paid on province, insurer and procedure fixed effects. The bottom row controls in addition for municipality fixed effects (for those municipalities with at least 100 observations). The distributions are generally similar, although the dispersion is somewhat higher for the treatment group when we only control for province fixed effects. Based on this, we conduct a robustness check in which we also control for these municipality fixed effects. Results are not qualitatively different from the baseline results.

Figure 10: Log price residuals



Note: The figures above plot residuals from regressing log price on province, procedure, insurer and municipality fixed effects, separately for each event and using a subsample before the actual event.