Effects of geographical accessibility on the use of outpatient care services: quasi-experimental evidence from panel count data

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Abstract

In 2010-2012 new outpatient service locations were established in Hungarian micro-regions, which had lacked such capacities before. We exploit this quasi-experiment to estimate the effect of geographical accessibility on outpatient case numbers using both individual-level and semi-aggregate panel data. We find a 24-27 per cent increase of case numbers as a result of the establishments. Our specialty-by-specialty estimates imply that a one-minute reduction of travel time to the nearest outpatient unit increases case numbers e.g. by 0.9 per cent in internal care and 3.1 per cent in rheumatology. The size of the new outpatient capacities has a separate effect, raising the possibility of the presence of supplier-induced demand.

By combining a fixed-effects logit and a fixed-effects truncated Poisson estimator, we decompose the effects into increases in the probability of ever visiting a doctor on the one hand and an increase of the frequency of visits on the other. We find that new visits were dominant in the vast majority of specialties, whereas both margins were important e.g. in rheumatology. Finally, we demonstrate the usefulness of the fixed-effects truncated Poisson estimator in modelling count data by examining its robustness by simulations.

1. INTRODUCTION

Given the profound and inevitable informational asymmetry that is at the heart of the doctor-patient relationship (Arrow, 1963), one perennial issue of health economics is: to what extent do considerations not related to health status affect diagnosis and therapy? These can come from the demand side (e.g. income, relative prices or accessability, i.e. time-related costs of seeking care for patients), as well as from the supply side (e.g. service providers' incentives to provide more or less care than they would give themselves if they were in the shoes of their patients). This latter element also subsumes the hotly debated and highly policy relevant „supplier-induced demand" hypothesis that posits that, under certain monetary incentives (e.g. a fee-for-service environment), doctors might abuse their fiduciary position for their own gain by persuading patients to receive more or different care than what would be optimal according to the state-of-the art of medicine (Peacock and Richardson, 1999).

Since geographical accessability (travel time to the location where care is provided) is clearly one of the possible determinants of the demand for health care (Acton, 1975), it is of great interest to identify its effect on the quantity of use. One empirical strategy to estimate such effects is to focus our attention on the regional variation in the distance to the location of care and the quantity of care given.

Geographical variaton in the quantity of care, however, might also depend on the characteristics of the case. Skinner (2012), informed by economic theory and building on Wennberg et al. (2012), differentiates three groups of care: (1) „effective care" – treatments whose net value is universally high, and where therefore little geographical variation is expected; (2) „preference-sensitive treatments with heterogeneous benefits" – treatments where benefit is heterogeneous, net value is lower and where patient preferences and

physician skills and capacity constraints are more likely to produce differences in utilization rates across otherwise similar patients; and (3) „supply-sensitive care" – treatments where evidence promises negligible or zero effects. In case of types (2) and (3), we can expect and we do observe, cf. Skinner (2012), a lot of geographical variation that is hard to control for.

Several empirical studies suggest that geographical accessibility to health care positively affects the use of health services and health outcomes (Campbell et al., 2000; Erlyana et al., 2011; Haynes and Bentham, 1982; Hyndman et al., 2000; Lavy and Germain, 1994; Pathman et al., 2006). They are mainly based on cross-sectional variation in accessibility (an exception is Avdic, 2014), i.e. they compare the behaviour of population closer to the health service provider to those farther from it, controlling for health care needs (e.g. demographic factors). This procedure has the drawback that there may well remain unobservable factors (e.g. cultural patterns and other determinants that affect the quantity of non-effective care of type (2) and (3) as categorised above) that correlate with accessibility but at the same time influence health service utilization. In this case the effect of accessibility may be under- or overestimated due to the presence of unobserved variables (Wooldridge, 2010). Indeed, some cross-sectional studies yield counterintuitive estimates for the impact of accessibility on the use of health services (Bolduc et al., 1996).

If access to health care improves substantially for a segment of the population, its effect can be directly examined by comparing the pre- and post-treatment behaviour of the affected population while controlling for other factors that may have influenced the change of health care utilization during the period. In this paper we exploit such a natural or quasi-experiment to identify how distance to health care affects the use of outpatient care services.[2]

[2] For a review of natural experiments in health economics see e.g. Jones (2009).

Hungary, an EU member state of 10 million inhabitants with a single-payer health insurance system and virtually universal coverage, expends 7.9% of its GDP on healthcare, 28% of which on outpatient care, our focus of interest here (2011 data, OECD, 2013). In the period we cover, responsibility for providing outpatient care was shared among municipalities, counties, the central government and private providers. The gatekeeping function of family doctors was non-exclusive. The basic benefit package (except for drugs) was (and is) free of out-of-pocket payments for the patients at the point of care, including outpatient care, albeit additional informal gratuity payments are widespread, especially in tertiary care. Most outpatient specialist services are financed by the budget based on fee-for-service points, under a system that scores procedures on the basis of their complexity and resource requirements. The relatively high share of outpatient care in provision and financing is due to the heritage of the Semashko-type model of healthcare provision under Soviet dominance. Central to that original universal model was a multi-tiered system of care with a strict referral system and strongly differentiated network of service providers, with outpatient specialist care one of the distinct tiers of healthcare provision (Gaál et al., 2011; Kornai and Eggleston, 2001).

Between 2010 and 2012 around 430 thousand people gained better access to specialist outpatient care in Hungary when the government created outpatient units in 20 rural micro-regions, which previously lacked capacity. (The investments were funded by the Social Infrastructure Operative Programme [SIOP] 2.1.2. of the European Union.) Locations for the new units were selected based on the applications of municipalities with no or limited local capacity to provide such care, making a case for need and demand. Funding accounted for 500-1000 million forints (2-4 million euros) per unit, generally covering 90-95% of the costs of the establishment of the new units to the municipalities if they complied with a set of administrative requirements (e.g. providing a minimum of services for a minimum of

hours/month, keeping the unit in operation for at least five years). While competition for scarce funds was not an issue (sufficient funds were allocated to be able to subsidize all likely applicants eligible under those rules; in fact the call stipulated that up to 25 municipalities could be awarded funding), there may have been self-selection by eligible municipality based on willingness to apply for funding. We have no administrative data on where the physicians hired to staff the new units were recruited from, but interviews suggested that many commute from nearby urban centres (Hétfa et al., 2013). The newly created units (all still in operation as of 2014) provide comprehensive service for the population of the micro-regions with at least 14 separate specialties at each location, although the number of consultation hours is generally low. As a result, basic specialist outpatient care (which we define as outpatient care in the following four specialties: internal medicine, surgery, obstetrics-gynecology and pediatrics) may now be reached by around 310 thousand more people by car in 20 minutes than before. (At least 1.6 million people – or 16 per cent of the population of Hungary – still live beyond this 20 minute limit.)

At the same time, other parts of Hungary experienced relatively few changes in the management of outpatient care between 2008 and 2012. Hence an appropriate control group of micro-regions can be identified, in which the health care indicators may be compared to those in the micro-regions where new outpatient service locations were established (the „treated" micro-regions). The geographical location of the treated and control micro-regions across the country is shown in Figure 1. The impact of the improvement in accessibility can then be estimated as the difference between the changes in the treated and control groups. We use both individual (micro-level) case statistics, which are analysed in a fixed-effects Poisson regression framework, and semi-aggregate data (measured at the micro-regional level), which are analysed in a fixed-effects linear panel regression setting.

(Figure 1 about here)

The micro-level data enable us to examine the heterogenous impact of the establishment of new outpatient locations on the various age groups and genders, as well as to identify the separate effects of supply-side factors such as the size of the new capacities. Furthermore, by taking into account that patients living in different settlements faced different improvements in travel time, we give a structural interpretation of our results by estimating the effect of a one minute change in travel time by car to the nearest outpatient care provider on health care use. These structural parameters can be used for ex ante evaluation of the impact of future health care investments.

Finally, we decompose the overall effect on case numbers into those happening on the „extensive" and the „intensive" margin, i.e. into „new patients" and the visiting frequency of existing patients. Following the approach of Majo and van Soest (2011), we use the fixed-effects logit model for the extensive margin and the fixed-effects truncated Poisson model for the intensive margin. We demonstrate by Monte Carlo simulations that the fixed-effects truncated Poisson estimator has certain robustness properties: it appears to be consistent not only if the conditional distribution is indeed truncated Poisson but even if it is a mixture of truncated Poisson distributions with the same mixing distribution across periods. This contains the truncated negative binomial distribution as a special case.

The paper is organised as follows. Section 2 introduces the data and Section 3 the econometric estimation methods. Results are presented in section 4 and discussed in section 5. Some details of the data sets and of the properties of the fixed-effects truncated Poisson estimator are given in the Appendices.

2. DATA

In most of our analysis we use a detailed *event-level database* exclusively provided to us for this research project by the National Institute for the Quality and Organizational Development in Healthcare and Medicines (GYEMSZI), which contains administrative information on individual visits to specialist outpatient units.[3] Based on the specialty of the corresponding outpatient unit, we first classify each visit to one of 18 specialty groups (which are shown in Table III and will be referred to as specialties below). Then, for each specialty – and also for all specialties summed up – we construct a *patient-level panel database* that contains the number of outpatient cases *by person* for a 25 per cent random sample of the residents of 20 treated and 21 control micro-regions *for each quarter* between 2008 and 2012. Age, gender of the patient and the postcode of her residence are also recorded.

Due to legal reasons, data on whole Hungary could not be obtained, therefore 21 control micro-regions (out of the 138 rural-type micro-regions, which are outside the most developed Central Hungary and do not coincide with a chief town of a county) were selected with the aim of approximating the observed characteristics of the 20 treated micro-regions as closely as possible. Specifically, the control group was chosen on the basis of the treatment propensity score, estimated from a logit model on the micro-regional level:

$$\Pr(k \in treated) = \Lambda(X_k \delta_0), \quad (1)$$

where k denotes the micro-region, X_k its pre-treatment (year 2008) demographic, socio-economic and health characteristics, and Λ is the logistic function.

[3] The data set went through initial data cleaning and transformation, see online Appendix 2 for details.

The estimated parameters of the logit model are presented in Table A1 in online Appendix 2. The most important explanatory variable of treatment propensity is the number of specialist outpatient consulting hours per 1000 inhabitants before the SIOP 2.1.2. projects. As mentioned earlier, only micro-regions without any substantial outpatient capacity could receive these grants.

The control group was defined by all non-treated micro-regions with a propensity score greater than 0.08. Estimating regressions on a sample pre-filtered on the basis of the propensity score is a usual practice in statistics (see e.g. Angrist and Pischke, 2009) and we roughly followed the advice of Crump et al. (2009) in the pre-filtering procedure.[4] As Table A1 shows the balancing of the explanatory variables between the treated and the control group is satisfactory: the means of the variables are roughly the same and do not differ significantly in the two groups. For instance, the number of weekly specialist outpatient consultation hours per 1000 residents – for which the highest treated-control differences are observed – average to 0.6 in the treated and 1.2 in the control micro-regions, but the latter is still less than one third of the average value of all non-treated micro-regions (3.8), and the t-statistic of the treated – control difference is not significant. Altogether, 430 thousand people live in the treated and 525 thousand people in the control micro-regions.

Out of the newly set-up outpatient services, 14 started to operate in 2011 (the earliest start was September 2010 and the latest was May 2012), hence sufficiently long pre- and post-treatment periods exist in the panel database between 2008 and 2012.

[4] As a rule of thumb, Crump et al. (2009) propose to restrict the sample to units with a propensity score between 0.1 and 0.9. Since in our case the largest propensity score did not differ much across the treated (0.96) and non-treated (0.89) micro-regions, we only restricted the propensity score from below in order to keep all treated micro-regions in the sample.

As a robustness check, we use another, *micro-regional panel data sets* (obtained from the National Health Insurance Fund [OEP]) that contain for each specialty the semi-aggregate number of outpatient cases by month and by micro-region for 56 months (between January 2008 and August 2012) and for 138 rural-type micro-regions (where the micro-regional classification is given by the residence of the patient). The original source of these data is the same event-level database as of the patient-level panel data. It is aggregated to the micro-regional level but covers a much wider population (around 4.8 million people) than the patient-level panel database that could be obtained only for the treated and control micro-regions.

3. METHODS

3.1. Estimating the overall effect

Our main identifying assumption is that the *changes* of case numbers in the treated and control micro-regions would be the same in the absence of treatment – apart from some time-varying characteristics of the micro-regions that we can control for. Since the treated and control micro-regions are observationally similar (see Table A1), this is a reasonable assumption. Moreover, interactions between the treated and control group are likely to be negligible because there is only a small number of treated and control micro-regions with common borders (see Figure 1) and, in any case, the vast majority (around 90 per cent) of the patients of the new specialist units come from their own micro-regions.[5]

[5] Note also that any interaction effect would cause a downward bias on the impact estimates because the case numbers of the *residents* – and not of the specialist units – are compared.

As noted earlier, we examine the panel data sets of different specialties separately. Formally, let N_{it} denote the number of outpatient cases of person *i* in quarter *t* in a particular specialty or in all specialties summed up. Its expected value may depend on some observable (X_{it}) and unobservable (c_i) characteristics of the person and the micro-region. X_{it} includes the treatment dummy or other treatment indicators, which are of primary interest, and c_i allows us to control for all time-invariant patient-level determinants, which may also include micro-regional characteristics. Since N_{it} is a count of events and it is measured in a panel dataset where the distribution of c_i and its relationship with X_{it} may in principle be arbitrary, a fixed-effects Poisson regression framework is appropriate here. In this setting the conditional expectation of N_{it} is modelled as

$$(2) \qquad E(N_{it}|X_{it}, c_i) = \exp(X_{it}\beta + c_i).$$

Wooldridge (1999) proved that the conditional maximum likelihood estimator of this model, constructed by assuming a Poisson distribution and eliminating c_i from the likelihood calculations, has nice robustness properties: it is consistent under the weak assumption that the conditional mean function (2) is well specified. Hence not only an entirely unrestricted relationship may exist between c_i and the observables, the conditional distribution of N_{it} and the dependence across time within a cross-sectional unit can also be arbitrary. (However, the standard errors should be adjusted in the more general cases.[6]) Due to these robustness

[6] In our case, in addition to these adjustments, the standard errors should also be adjusted (by bootstrapping) to take into account the propensity score based pre-filtering. However, since the bootstrap and usual standard errors generally differ only slightly, this adjustment is rarely done in practice (see e.g. Angrist and Pischke, 2009). Without micro-data outside the treated and control micro-regions, we could not perform the bootstrap procedure in the fixed-effects Poisson regression. However, as a robustness check, we replicated the propensity score based pre-filtering and the bootstrap calculations in the fixed-effects panel regression on the semi-aggregate data set (see below) and found that the bootstrap and usual standard errors are almost identical.

properties, the fixed-effects Poisson model is a basic model in the analysis of panel count data.[7]

We use various model specifications to analyse the heterogenous effect of the treatment on different groups of patients and to exploit the heterogeneity of treatment itself. In our *baseline model* $X_{it} = (D_{it}, Z_{it}, Q_{it})$, where D_{it} is the treatment dummy[8], Z_{it} is the vector of time (quarter) dummies, and Q_{it} is the vector of time-varying controls measured at the micro-regional or individual level such as the age group of the patient:

$$(3) \qquad E(N_{it}|D_{it}, Z_{it}, Q_{it}, c_i) = \exp(D_{it}\beta_D + Z_{it}\beta_Z + Q_{it}\beta_Q + c_i).$$

Hence we have a difference-in-differences-type (DiD) specification, where the expectation is modelled on a multiplicative scale. β_D, the parameter of primary interest, gives the approximate percentage impact of the treatment on case numbers, while β_Z controls for the change of case numbers independent of the treatment during the period, and c_i controls for the differences in the initial level of case numbers in various micro-regions and between patients.

In the *interaction model* we include the interaction of D_{it} with age group dummies (A_{it}^{0-17} and A_{it}^{18-59} for the 0-17 and 18-59 years groups, respectively), with gender (F_{it} for females) and with the dummy for living in the chief town (L_{it}) in the conditional expectation equation. Here the parameters of the interaction terms measure the heterogenous effect of the treatment on various groups:

[7] See e.g. Wooldridge (2010) for some applications.

[8] D_{it}=1 if a new outpatient unit operates in the micro-region of patient *i* in period *t* (and D_{it}=0 otherwise). In a variant of the baseline specification – not reported in the paper – the one-year lagged values of D_{it} were also included but the lagged parameter was insignificant suggesting that case numbers were affected by the treatment quite rapidly.

(4) $$E(N_{it}|D_{it}, A_{it}^{0-17}, A_{it}^{18-59}, F_{it}, L_{it}, Z_{it}, Q_{it}, c_i) = \exp(D_{it}\beta_D + D_{it}A_{it}^{0-17}\beta_{DA}^{0-17} + D_{it}A_{it}^{18-59}\beta_{DA}^{18-59} + D_{it}F_{it}\beta_{DF} + D_{it}L_{it}\beta_{DL} + Z_{it}\beta_Z + Q_{it}\beta_Q + c_i).$$

To gain insight into the health economic reasons behind the increase in case numbers, our *capacity model* expands the baseline specification with the size of the new outpatient capacities (per 1000 inhabitants of the micro-region) in the given specialty, denoted by K_{it}. Its parameter measures the effect of the size of the new capacities after controlling for the mere existence of new units:

(5) $$E(N_{it}|D_{it}, K_{it}, Z_{it}, Q_{it}, c_i) = \exp(D_{it}\beta_D + K_{it}\beta_K + Z_{it}\beta_Z + Q_{it}\beta_Q + c_i).$$

Finally, to exploit the heterogeneity in the reduction of travel time, we define M_{it} as the travel time (in minutes) needed to reach the nearest outpatient unit of the given specialty by car from the settlement of person *i* in quarter *t*.[9] In the given period the change of M_{it} is negligible in the control micro-regions compared to that in the treated micro-regions and there is a substantial variability within the treated group as well. The parameter β_M in our *structural model* shows the percentage increase of the number of outpatient cases as a result of a one-minute reduction of travel time to the nearest outpatient unit:

(6) $$E(N_{it}|M_{it}, Z_{it}, Q_{it}, c_i) = \exp(M_{it}\beta_M + Z_{it}\beta_Z + Q_{it}\beta_Q + c_i).$$

As a robustness check of the results from the individual-level data, we also estimate the treatment effect on the *semi-aggregate (micro-regional) panel data* containing all 138 rural-type micro-regions in Hungary. Let m_{kt} denote the aggregate number of outpatient cases in *micro-region k* and *month t*. This is affected by the time-invariant (or slowly varying) characteristics c_k of the micro-region (such as the referring behaviour of the local GP) and by

[9] These data come from a distance matrix of travel times between all settlements in Hungary, provided by GYEMSZI.

observable micro-regional characteristics $X_{kt} = (D_{kt}, Z_{kt}, Q_{kt})$. Here, D_{kt} is again the treatment dummy, Z_{kt} now contains trend and seasonality, and – to control for the heterogeneity of the micro-regions – Q_{kt} includes the local unemployment rate. Our model is then:

$$(7) \qquad \log m_{kt} = D_{kt}\beta_D + Z_{kt}\beta_Z + Q_{kt}\beta_Q + c_k + u_{kt},$$

where u_{kt} is the (time-varying) error term. The model is estimated by fixed effects (*FE linear regression*), which allows the c_k unobserved characteristics to be arbitrarily correlated with D_{kt}. Since the dependent variable is in a logarithmic form, the parameters of the treatment dummies in (3) and (7) are directly comparable.

3.2. A hurdle model for separating the extensive and intensive margins

Micro-level data allow us to separate the extensive margin of adjustment (i.e. the change of the probability of ever visiting an outpatient provider) and the intensive margin (i.e. the change in the frequency of visits). Since the decisions whether to use a health service at all and how much to use it are possibly governed by different forces, hurdle (two-part) models that can separate the two margins have been long in use in health economics (e.g. Bago d'Uva, 2006; Jones, 2009; Pohlmeier and Ulrich, 1995; Winkelmann, 2004). In a hurdle setting, the random event $\{N_{it} > 0\}$ and the random variable conditional on this event $[N_{it}|(N_{it} > 0)]$ are modelled directly. Certain authors prefer zero-inflated or finite mixture models, where the zero and non-zero parts of the distribution come as mixtures of two (or more) latent classes of patients with different underlying intensity to use health services (e.g. Deb and Trivedi, 2002; Jones, 2009). In any case, the extension of such models to panel data is far from straightforward: on the one hand, serious computational difficulties may arise in a

random-effects framework (Cameron and Trivedi, 2013), on the other hand, conditioning out the fixed-effects similarly to the Poisson-case above is rarely possible. In this paper we use a pragmatic approach to model the two hurdles in a fixed-effects setting.

First the *extensive margin* is analysed by a fixed-effects logit model:

(8) $\Pr(N_{it} > 0 | X_{it}, c_i{}^q) = \Lambda(X_{it}\beta^q + c_i{}^q)$,

where Λ is the logistic function, X_{it} now contains the treatment dummy (D_{it}) along with the usual control variables (Z_{it}, Q_{it}), and $c_i{}^q$ denotes the unobserved heterogeneity of person i associated with his / her propensity to visit the doctor at least once a year. The fixed-effects logit estimator is consistent without particular assumptions about the distribution of $c_i{}^q$ or about its relationship with the explanatory variables, hence it is applied very often in econometrics (Wooldridge, 2010).

Second, following and completing the work of Majo and van Soest (2011), we model the *intensive margin* in a fixed-effects truncated Poisson framework, where our inference is based solely on the positive part of our sample. As a starting point, suppose that in a panel data set we observe $(y_{i1}, y_{i2}, \dots, y_{iT})$, which are independent zero-truncated Poisson random variables, conditionally on X_{it} and on the unobserved heterogeneity $c_i{}^s$ (which is now associated with the visiting frequency to the doctor.) That is, we observe the non-zero part of the count data. The probability mass function of y_{it} is given by

(9) $\Pr(y_{it} = k | X_{it}, c_i{}^s) = \frac{\mu_{it}^k / k! * \exp(-\mu_{it})}{1 - \exp(-\mu_{it})} \qquad (k = 1,2, \dots)$,

where

(10) $\mu_{it} = \exp(X_{it}\beta^s + c_i{}^s)$

is the conditional expectation of the corresponding *untruncated* Poisson distribution. Then, as already noted by Majo and van Soest (2011), the distribution of $(y_{i1}, y_{i2}, \dots, y_{iT})$, conditionally on their sum, does not depend on $c_i{}^s$ and hence – similarly to the fixed-effects Poisson regression – a fixed-effects regression framework is applicable in the truncated case, too. Based on the fact that the joint distribution of independent truncated Poisson random variables, conditional on their sum, is truncated multinomial (see e.g. Johnson et al., 1997, p. 32), we derive the conditional (fixed-effects) likelihood and prove the consistency and asymptotic normality of the fixed-effects estimator in the truncated Poisson setting in Appendix 1. Hence, in contrast to other fixed-effects truncated regression models (Wooldridge, 2010), in the truncated Poisson case there is no need to apply various advanced (e.g. semiparametric) estimators because the standard conditional maximum likelihood estimator is consistent.

Appendix 1 proves consistency and asymptotic normality under the assumption that the model is well-specified, i.e. that the joint distribution of $(y_{i1}, y_{i2}, \dots, y_{iT})$ is indeed independent truncated Poisson with the specified conditional expectation. As noted in section 3.1., the usual fixed-effects (untruncated) Poisson estimator is much more robust than this because its consistency requires only a well-specified conditional expectation (while the distribution can be entirely unrestricted). We demonstrate by Monte Carlo simulations in online Appendix 3 that the truncated estimator also has some robustness properties: for $T = 2$ it is consistent even when y_{it} is a mixture of truncated Poisson distributions, with the same mixing

distribution across periods.[10] By choosing the gamma distribution as the mixing law, this contains the truncated negative binomial distribution as a special case.

Returning to our hurdle model to separate the extensive and intensive margins, we may assume that $[N_{it}|(N_{it} > 0)]$ comes from a truncated Poisson (or Poisson-mixture, see above) distribution with untruncated expected value given by equation (10). Then, because of the unknown number of zeros, $\sum_{t=1}^{T} N_{it}$ is *not* a sufficient statistic for ${c_i}^s$, but it is sufficient on the subsample of positive values $\{N_{it} > 0\}$. Hence we may estimate the intensive margin by the fixed-effects truncated Poisson estimator on this subsample. (Thus e.g. if $T = 2$, estimation is carried out on units that take positive values in both periods.)

Alternative approaches would be Deb and Trivedi's (2013) fixed-effects expectation-maximization (EM) framework or the various random-effects estimators of hurdle, zero-inflated or finite mixture models (Cameron and Trivedi, 2013; Jones, 2009). These approaches are, however, computationally intensive and the latter kind of models are not robust to distributional misspecifications in the unobserved heterogeneities, either. Hence, in this paper we opt for the combination of fixed-effects logit and truncated Poisson estimators to provide a flexible way to separate the two margins.

In addition, as a robustness check, we display results from a simple pooled zero-inflated negative binomial model, where the case numbers are modelled as a mixture of a zero distribution and a negative binomial distribution (whose expectation depends on the covariates), and the mixing probability is given by a logit formulation also depending on the covariates (see Wooldridge, 2010). Although – compared to a random-effects or fixed-effects setting – in the pooled version we do not model the clustering of the unobserved

[10] To ensure that equation (10) for the untruncated expectation still holds, the mixing distribution should have a unit expected value.

heterogeneities explicitly, the maximum likelihood parameter estimates of this model are still consistent if the distributions are correctly specified and if the unobserved individual heterogeneities are independent of the covariates. (If balancing is satisfactory between the treated and control micro-regions, the latter assumption seems reasonable. In any case, cluster-robust standard errors should be used in the pooled model.) Note also that the zero-inflated and the hurdle model are not directly comparable because in the zero-inflated formulation N_{it} takes a positive value only if the observation is not in the zero mixture *and* the negative binomial distribution takes a positive value.

Due to the large number of zero visits on the quarterly frequency, we fit the hurdle and zero-inflated models to annual data. To avoid problems with years split by the different commencement of the new units, we use one pre- and one post-treatment year: 2009 and 2012.

4. RESULTS

4.1. Descriptive analysis

By opening the new outpatient units, accessibility to outpatient services dramatically improved in their respective micro-regions. For instance, the average travel time by car for the residents of the treated micro-regions from their settlement to the nearest unit in internal medicine decreased from 19.8 minutes in 2008 to 9.8 minutes in 2012. Meanwhile, the accessibility from the control micro-regions remained unchanged (15.8 minutes on average).

As a result of the new outpatient service locations opening their doors, the patients quickly started to use them. Table I shows that in May-August 2012 around 35-45 per cent of internal

care, surgery and obstetrics-gynaecology cases coming from the micro-regions in which new specialties were set up were treated by new providers, while this ratio was around 60 per cent for patients of the chief towns (where the new units operate). Meanwhile, patient paths in paediatrics did not divert substantially.

(Table I about here)

As a descriptive analysis of the impact of the treatment, Table II displays the standardized number of outpatient cases in the May-August periods of 2010 and 2012 for the treated micro-regions and for those „rural" micro-regions, where non-negligible outpatient capacity existed already before 2010. The data show a dramatic increase in case numbers for the treated micro-regions. While the numbers were well below those of the similar micro-regions in 2010, they increased to roughly that level after the developments. This already suggests that the absence of the supply in a micro-region had a clear negative impact on outpatient care use.

(Table II about here)

4.2. Estimation results

Econometric methods that control for other factors influencing case numbers during the observational period display a similar picture to the descriptive analysis. Table III shows that the new units established under SIOP 2.1.2. increased outpatient case numbers by 24-27 per cent, and this result is robust across the two (individual-level and semi-aggregate) datasets and estimation methods.[11] Looking at separate specialties, rheumatology experienced the largest increase (55 per cent according to the micro-data), which is not surprising, given the

[11] See also Table A2 in online Appendix 2, which displays raw quarterly case numbers by patient between 2010 and 2012: a rapid increase in the treated micro-regions and no change in the control micro-regions can be observed.

large non-financial costs rheumatology patients are facing when they need to travel to an outpatient provider. The establishment of new outpatient locations had a limited impact on paediatrics cases, in line with the descriptive analysis of patient paths presented above.

(Table III about here)

Table III also shows the results of our *capacity model* (equation (5)), where the relative size of the capacities K_{it} appears along with the treatment dummy D_{it}. In some specialties – notably in internal care, traumatology, dermatology, rheumatology, psychiatry, pulmonology, cardiology and ultrasound – the size of the new capacities has a significant and substantial impact (beyond the mere existence of the new unit) on outpatient case numbers. On the other hand, no such effect is present e.g. in surgery, obstetrics-gynaecology or urology. The positive impact of the size of the new capacities suggests the presence of supplier-induced demand but the role of patient-side mechanisms (e.g. through reduced waiting lists) cannot be ruled out, either. On the other hand, if the size of the capacities does not play a role in a particular specialty, that makes the presence of supplier-induced demand unlikely there. Note also that the mere existence of new units (the parameter of D_{it}) is significant in the vast majority of specialties.

According to the *interaction model* (equation (4)), the effect of the treatment on the total number of cases (taking all specialties together) is significantly higher for women than for men (by 3.9 percentage points, i.e. $\beta_{DF} = 0.038$ in equation (4)), and lower by 7.3 percentage points for patients between 18-59 years and by 16.0 percentage points for patients under 18 years than for patients above 60 years.[12] Gender- and age-specific estimates on the specialty

[12] See also Table A2 in online Appendix 2 for the gender- and age-specific average case numbers in the treated and control micro-regions between 2008 and 2012.

level, however, do not paint a consistent picture, so we do not display the detailed results here.

To exploit variability in the reduction of travel times, Table IV shows the estimated percentage impact of a one-minute reduction of travel time for various specialties (calculated from the estimated parameter β_M in our *structural model* (equation (6))). For instance, for internal care, a one-minute reduction of travel time increases the number of cases by 0.9 per cent. The highest values are estimated for rheumatology (3.1 per cent), surgery and dermatology (1.8-2.0 per cent).

(Table IV about here)

4.3. Separating the extensive and intensive margins

Table V displays the estimates of the extensive and intensive margins from both the fixed-effects hurdle and the pooled zero-inflated negative binomial model. Although these models are not equivalent and hence their results slightly differ, both indicate a large and significant positive impact on the extensive margin in the vast majority of specialties. The impact in the median specialty is around 0.25 on the logit scale, which is substantial in per cent increase because the median annual visiting probability was only 0.08 before the treatment in the treated micro-regions. On the other hand, the effects on the intensive margin are rather small and insignificant, with substantial impact measured only in surgery, dermatology, rheumatology and laboratory diagnostics. But even in these specialties the adjustment on the extensive margin seems to exceed that on the intensive margin.

(Table V about here)

5. DISCUSSION

In this paper we considered the opening of new outpatient care locations a quasi-experiment in order to analyse the effects of geographical accessibility on the quantity of care. Our data and estimation methods made it possible to separate the utilization effects of bringing care closer to the patients from the effects of many other determinants of utilization that exhibit geographical variation. The internal validity of our findings (the similarity of our results across two different data sets and estimation methods) seems convincing. As far as external validity is concerned, our findings are hard to compare since the literature consists of cross-sectional analyses rife with inevitable endogeneity problems. Nevertheless our results are roughly comparable with those found e.g. by Erlyana et al. (2011) from cross-sectional survey data in Indonesia.

We obtained significant estimates for how much a one-minute reduction of travel time increases the number of cases across different specialties of care (e.g. 0.9 per cent for internal medicine, but 3.1 per cent for rheumatology). The fact that the visits in rheumatology are the most affected by the need to travel can be explained by the additional pain and effort associated with the movement of rheumatology patients. By combining the fixed-effects logit and the fixed-effects truncated Poisson estimators, we could also decompose the effects of a change in accessibility on case numbers into increases in the probability of ever visiting a doctor on the one hand and an increase of the frequency of visits on the other.

Telling apart impacts on the extensive and intensive margin (the results in Table V) and identifying impacts net of the effect of an increase in the size of outpatient capacity (parameter of the pure treatment dummy in the capacity model of Table III) also goes a long way towards separating an increase in the number of cases that represent an unambiguous

increase in social welfare ("effective care") from cases where the suspicion of the intensification of supply-sensitive care arises. The latter would manifest itself in an increased visit frequency, not in a higher number of previously untreated patients seeing the doctor. While our specifications cannot directly prove the presence of supplier-induced demand, our results can and do point towards it in some specialties, where more outpatient capacity (to be filled by doctors in search of increased funding for more fee-for-service points) partially explained the increase in the number of cases. Our main result, however, is that we could identify significant increases in the number of cases even net of that effect, and on the extensive margin. We argue that these increases unambiguously represent "effective care" as defined in Skinner (2012). In other words, our results do not just show that, thanks to the shortened travel time to the specialist, more people decide to see the doctor with their symptoms, but also that it is more likely to lead not just to higher state and private health costs, but to earlier diagnosis and treatment as well. Thus, if follow-up studies can prove that the effects we found are indeed durable and impact health outcome variables as well, we can deduce that improving access can be a way to geographically target areas with low health status and thereby to reduce health inequality.

In fact, our results, if complemented with estimates of the long run health effects and the fixed and variable costs of establishing such rural outpatient units, could be used by policy makers to carry out an ex-post social cost-benefit analysis of the project, and, with careful validation, our structural parameters could also contribute to ex-ante estimations helping responsible healthcare investment decisions elsewhere as well.

One potential use of our findings could be to consider the time and monetary cost of distance travelled to see the doctor an element of the price of the service and estimate the own-price

elasticity of demand for the frequency of healthcare intervention in a two-part healthcare service demand specification. Such a demand function could shed light on the benefits of more frequent ambulatory healthcare services as perceived by the patient.

Finally, as a methodological advancement, we demonstrated that the combination of fixed-effects logit and truncated Poisson estimators provide an attractive choice for analysing panel count data because they are computationally much less challenging to estimate in large data sets than their competitors and they have certain robustness properties.

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CONFLICTS OF INTEREST

The authors have no conflicts of interest to declare.

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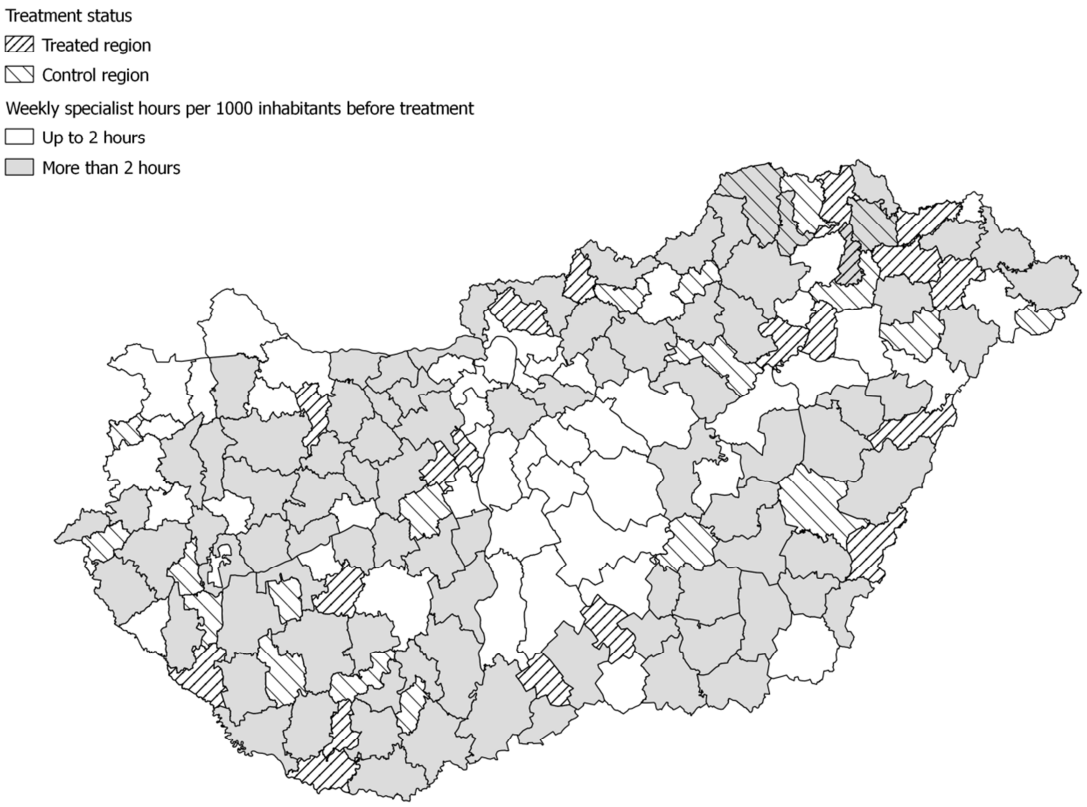
TABLES AND FIGURES

Figure 1: Geographical location of the treated and control micro-regions and the distribution of specialist outpatient consulting hours in Hungary



Source: TSTAR (settlement) data; OEP data on specialist outpatient hours in the four basic specialties combined

Table I: The ratio of cases treated in the micro-region of residence among the cases of the population of the treated micro-regions (per cent)

	Among the whole population of micro-regions			Among the population of the micro-regional chief towns		
	Average	Minimum	Maximum	Average	Minimum	Maximum
Internal care	35.8	13.7	68.9	55.0	18.9	77.8
Surgery	43.8	14.3	65.3	64.4	32.2	83.4
Gynaecology	43.9	17.0	80.1	64.2	22.8	87.5
Paediatrics	11.6	1.2	35.4	18.7	3.0	49.8

Source: own calculations based on semi-aggregate OEP data

Note: The data refer to the period between May 2012 and August 2012, excluding the new unit in Baktalórántháza (which started in May 2012).

Table II: Number of outpatient cases (as a proportion of 100 inhabitants) in the SIOP 2.1.2. micro-regions and similar micro-regions with substantial oupatient capacities before

Number of cases / 100 inhabitants	Age groups (years)			Average	Standardized (for age distribution)	Difference (treated – similar)
	0-17	18-59	60+			
SIOP 2.1.2. micro-regions						
2010	204	354	480	350	355	-21.5%
2012	242	443	706	457	465	1.3%
Similar micro-regions						
2010	258	437	645	452	452	
2012	251	426	712	459	459	

Source: own calculations based on semi-aggregate OEP data

Note: The term „similar micro-region" refers to those micro-regions that had at least 200 hours of specialist monthly outpatient capacity already in 2010 and are outside Central Hungary and not in a chief town of a county. (Hence they are not the same as the control micro-regions.) Outpatient cases are defined after excluding laboratory diagnostics and special one-day services. Annualized rates are shown on the basis of the May–August periods (without adjusting for seasonality). The standardized proportions were calculated based on the age distribution of the similar micro-regions.

Table III: Impact of SIOP 2.1.2. treatment on the number of outpatient cases (baseline and capacity models)

	Model on semi-aggregate data		Models on micro-level data							
	Baseline models				Capacity model				Number of obs. (thousand)	Number of groups (thousand)
	FE linear model (equation (3))		FE Poisson model (equation (7))		FE Poisson model (equation (5))					
	Effect in per cent $\exp(\beta_D) - 1$ (with S.E.)				Parameter of treatment dummy (β_D) (with S.E.)		Parameter of capacities (β_K) (with S.E.)			
All specialties	26.7***	(2.8)	24.2***	(0.8)					3773.3	198.9
Internal care	14.2***	(4.2)	11.7***	(1.3)	0.051**	(0.025)	0.054***	(0.020)	1317.4	68.8
Surgery	39.7***	(5.2)	35.7***	(2.5)	0.268***	(0.036)	0.037	(0.034)	1113.2	57.8
Traumatology	19.0**	(8.0)	10.2***	(2.7)	-0.046	(0.049)	0.307***	(0.095)	966.1	49.9
Gynaecology	22.5***	(4.1)	12.4***	(1.9)	0.085***	(0.031)	0.032	(0.027)	895.3	45.8
Paediatrics	5.3	(4.5)	4.8**	(2.2)	-0.012	(0.036)	0.199*	(0.102)	493.7	27.3
Otolaryngology	32.0***	(6.8)	29.0***	(2.4)	0.204***	(0.043)	0.104	(0.077)	1052.5	54.9
Ophtalmology	34.3***	(3.1)	25.1***	(1.7)	0.218***	(0.029)	0.008	(0.036)	1273.7	65.5
Dermatology	39.1***	(8.8)	34.0***	(2.8)	0.183***	(0.037)	0.227***	(0.064)	885.1	45.7
Neurology	28.1***	(4.1)	23.9***	(2.1)	0.173***	(0.045)	0.088	(0.088)	671.2	35.1
Orthopaedy	40.6***	(4.6)	25.6***	(3.0)	0.196***	(0.045)	0.110	(0.130)	547.3	29.7
Urology	20.2***	(3.8)	15.5***	(2.7)	0.151***	(0.047)	-0.024	(0.143)	478.7	24.9
Rheumatology	88.3***	(16.8)	55.4***	(3.0)	0.192***	(0.037)	0.351***	(0.044)	906.9	46.8
Psychiatry	16.1***	(3.9)	17.7***	(2.9)	0.054	(0.063)	0.163**	(0.071)	384.9	20.0
Pulmonology	-1.3	(5.3)	-4.3***	(0.9)	-0.087***	(0.014)	0.082***	(0.021)	2140.6	109.9
Cardiology	31.0***	(7.7)	19.5***	(2.1)	0.082**	(0.036)	0.187***	(0.063)	625.5	32.6
Lab diagnostics	14.5*	(7.9)	8.9***	(2.5)	0.075***	(0.026)	0.056	(0.083)	1465.9	76.9
X-ray	25.9***	(3.9)	11.0***	(1.1)	0.118***	(0.023)	-0.020	(0.028)	2472.9	129.5
Ultrasound	19.0***	(4.9)	7.7***	(1.3)	-0.070***	(0.027)	0.209***	(0.034)	1654.8	87.4

Source: own calculations based on micro-level (GYEMSZI) and semi-aggregate (OEP) data

***: p<0.01; **: p<0.05; *: p<0.1

Note: All results refer to outpatient cases excluding special one-day services. Robust standard errors are in parentheses.

For baseline models: percentage effects (and not the actual coefficients) are displayed.

FE linear model on semi-aggregate data: fixed-effects linear model on monthly log case numbers by the micro-region of patients (from „rural-type" micro-regions) between January 2008 and August 2012. Controls: linear trend, seasonality, unemployment rate of micro-region, the presence of other minor (SIOP 2.1.3. and RDOP) development in outpatient service. Number of observations: 138 micro-regions x 56 months.

FE Poisson models on micro-level data: fixed-effects Poisson models on quarterly case numbers by patient (living in treated or control micro-regions) between 2008 and 2012. Explanatory variable in the baseline model: treatment dummy (D_{it}). Explanatory variables in the capacity model: D_{it} and the size of the new outpatient capacities (K_{it}). Controls: time (quarter) dummies and age groups. Number of groups (patients) and observations (patient x quarter) are shown, excluding people with zero case number across all periods in a given specialty.

Table IV: Impact of a one-minute reduction of travel time on the number of outpatient cases (percentage changes)

Specialty	β_M in equation (6) (with S.E.)		Number of of obs. (thousand)	Number of groups (thousand)
Internal care	0.953%***	(0.076%)	1316.2	68.8
Surgery	1.951%***	(0.108%)	1112.2	57.7
Traumatology	0.934%***	(0.128%)	965.4	49.9
Gynaecology	1.084%***	(0.106%)	894.3	45.8
Paediatrics	0.450%***	(0.101%)	493.4	27.2
Otolaryngology	1.607%***	(0.109%)	1051.5	54.9
Ophtalmology	1.499%***	(0.087%)	1272.3	65.4
Dermatology	1.813%***	(0.120%)	884.3	45.7
Neurology	1.440%***	(0.103%)	670.6	35.1
Orthopaedy	1.568%***	(0.132%)	546.7	29.7
Urology	1.242%***	(0.123%)	478.2	24.9
Rheumatology	3.111%***	(0.122%)	906.1	46.8
Psychiatry	1.124%***	(0.108%)	384.6	20.0
Pulmonology	-0.348%***	(0.054%)	2138.8	109.9
Cardiology	0.980%***	(0.094%)	624.9	32.6
Lab diagnostics	-0.307%	(0.225%)	1464.2	76.8
X-ray	0.930%***	(0.054%)	2471.2	129.5
Ultrasound	0.778%***	(0.069%)	1653.5	87.4

Source: own calculations based on micro-level (GYEMSZI) data

***: p<0.01; **: p<0.05; *: p<0.1

Note: All results refer to outpatient cases excluding special one-day services. Robust standard errors are in parentheses.

Model: fixed-effects Poisson model on quarterly case numbers by patient (living in treated or control micro-regions) between 2008 and 2012. Percentage impacts of a one-minute reduction are displayed (calculated from the estimated parameter of M_{it}). Controls: time (quarter) dummies and age groups. Number of groups (patients) and observations (patient x quarter) are shown, excluding people with zero case number across all periods in a given specialty.

Table V: Impact on the visiting probability and visiting frequency

	FE hurdle model					Pooled zero-inflated negative binomial model				
	Extensive margin			Intensive margin		Extensive margin		Intensive margin		
	FE logit equation (8)			FE truncated Poisson equation (10)		Logit for the probability of the negative binomial part		Log expectation for the negative binomial part		
	parameter of D_{it}		No. of obs. (thousand)	parameter of D_{it}	No. of obs. (thousand)	parameter of D_{it}		parameter of D_{it}		No. of obs. (thousand)
Internal care	0.181***	(0.024)	55.1	0.053**	(0.025) 23.2	0.190***	(0.035)	0.0189	(0.021)	359.7
Surgery	0.285***	(0.026)	49.8	0.108*	(0.054) 7.5	0.160***	(0.040)	0.158***	(0.030)	359.7
Traumatology	0.157***	(0.035)	41.1	-0.097	(0.090) 3.8	0.431***	(0.049)	0.0996***	(0.038)	359.7
Gynaecology	0.211***	(0.029)	41.0	0.029	(0.040) 16.3	0.004	(0.061)	0.0462**	(0.020)	359.7
Paediatrics	-0.265***	(0.042)	20.8	0.019	(0.050) 7.6	0.297***	(0.084)	0.0308	(0.028)	359.7
Otolaryngology	0.355***	(0.027)	45.6	-0.010	(0.060) 6.6	0.516***	(0.081)	-0.0649	(0.048)	359.7
Ophtalmology	0.368***	(0.024)	55.4	0.040	(0.035) 14.1	0.554***	(0.143)	0.0390**	(0.020)	359.7
Dermatology	0.372***	(0.028)	40.2	0.192**	(0.083) 5.7	0.420***	(0.071)	0.164***	(0.040)	359.7
Neurology	0.267***	(0.034)	28.4	0.157***	(0.041) 8.1	0.199***	(0.028)	0.0149	(0.029)	359.7
Orthopaedy	0.268***	(0.037)	24.0	0.075	(0.092) 3.6	0.265***	(0.033)	0.0645	(0.039)	359.7
Urology	0.194***	(0.040)	20.7	0.044	(0.048) 4.2	0.280***	(0.032)	0.0370	(0.036)	359.7
Rheumatology	0.361***	(0.029)	39.9	0.309***	(0.038) 13.0	0.202***	(0.020)	0.100***	(0.023)	359.7
Psychiatry	0.286***	(0.047)	14.7	0.172***	(0.038) 10.5	0.182***	(0.026)	0.0434	(0.033)	359.7
Pulmonology	-0.232***	(0.020)	90.5	0.088**	(0.043) 46.5	-0.158***	(0.049)	-0.0451***	(0.014)	359.7
Cardiology	0.308***	(0.036)	25.9	0.085*	(0.044) 9.1	0.552***	(0.054)	0.00828	(0.054)	359.7
Lab diagnostics	0.169***	(0.031)	66.1	0.108**	(0.047) 19.8	0.524***	(0.116)	0.311***	(0.027)	359.7
X-ray	0.220***	(0.018)	110.7	0.034	(0.033) 37.5	0.312***	(0.070)	0.129***	(0.013)	359.7
Ultrasound	0.159***	(0.022)	72.4	0.063	(0.042) 17.7	0.249***	(0.049)	0.0975***	(0.016)	359.7

Source: own calculations based on micro-level GYEMSZI data

***: p<0.01; **: p<0.05; *: p<0.1

Note: All results refer to outpatient cases excluding special one-day services and the inhabitants of one micro-region (Baktalórántháza), where the new unit started to operate only in May 2012. Robust standard errors are in parentheses.

FE logit model on the probabilities of visiting the doctor in a given year for people living in treated or control micro-regions. Controls: year dummy and age groups. The estimated parameters show the impact of the treatment on $\log(p/(1-p))$, hence tend to overestimate the impact on $\log p$. Number of periods: 2 years (2009 and 2012). Number of observations (patient x year) is shown, covering people with one zero and one non-zero case number in a given specialty.
FE truncated Poisson model on individual zero-teruncated case numbers in a given year for people living in treated or control micro-regions. Controls: year dummy and age groups. The estimated parameter shows the impact of the treatment on the log expected value of a Poisson distribution on the basis of its zero-truncated part. Number of periods: 2 years (2009 and 2012). Number of observations (patient x year) is shown, covering people with positive case numbers in both periods in a given specialty.
Pooled zero-inflated negative binomial model on case numbers for people living in treated or control micro-regions. Controls: year dummy, gender and age groups (0-17, 18-39, 40-59, 60-69, 70+). The estimated parameters show the impact of the treatment on the log-odds of the probability of the non-zero (negative binomial) part and on the log expected value of the negative binomial part. Number of periods: 2 years (2009 and 2012). Number of observations (person x year) is shown.

APPENDIX 1: Derivation of the fixed-effects truncated Poisson estimator

We use the notations of section 3.2. Let $(S_{i1}, S_{i2}, \dots, S_{iT})$ be independent Poisson random variables with $E(S_{it}) = \mu_{it}$, let $\mu_i = \sum_{t=1}^{T} \mu_{it}$ denote the sum of the Poisson-parameters and let

$$p_{it}(\beta^s) = \frac{\exp(X_{it}\beta^s + {c_i}^s)}{\sum_{r=1}^{T} \exp(X_{ir}\beta^s + {c_i}^s)} = \frac{\exp(X_{it}\beta^s)}{\sum_{r=1}^{T} \exp(X_{ir}\beta^s)}.$$

Note that $p_{it}(\beta^s)$ does not depend on the unobserved effect ${c_i}^s$. Then a well-known result (which is also standardly used to derive the existence of the fixed-effects Poisson estimator) states that the distribution of $(S_{i1}, S_{i2}, \dots, S_{iT})$, conditional on $\sum_{t=1}^{T} S_{it} = k$, is multinomial with parameters $\left(k, p_{i1}(\beta^s), \dots, p_{iT}(\beta^s)\right)$ (Johnson et al., 1997, p. 32). The truncated random variables y_{it} in section 3.2. can be obtained as $y_{it} = [S_{it} | (S_{it} > 0)]$ and hence the joint distribution of the truncated versions, $(y_{i1}, y_{i2}, \dots, y_{iT})$, conditional on $\sum_{t=1}^{T} S_{it} = \sum_{t=1}^{T} y_{it} = k$, differs from the multinomial law only because neither marginal can take the value zero. Hence, this conditional distribution is truncated multinomial, as defined by Johnson et al. (1997), p. 72, with all marginals truncated at zero. By calculating the various joint probabilities of the zero marginals, the probability mass function of this distribution is given by

$$\Pr\left(y_{i1} = k_1, y_{i2} = k_2, \dots, y_{iT} = k_T \middle| \sum_{t=1}^{T} y_{it} = k\right) = \frac{k! * \prod_{t=1}^{T} \frac{p_{it}(\beta^s)^{k_t}}{k_t!}}{1 - q_i(\beta^s)}$$

for $1 \leq k_t < k$ $(t = 1, \dots, T)$ integers such that $\sum_{t=1}^{T} k_t = k$, where

$$q_i(\beta^s) = \sum_{m=1}^{T-1} (-1)^{T-m-1} \sum_{\{t_1, \dots, t_m\} \subset \{1,2,\dots,T\}} \left(p_{it_1}(\beta^s) + \cdots + p_{it_m}(\beta^s)\right)^k.$$

(For $T = 2$ we obtain the formula used by Majo and van Soest, 2011). These formulae do not depend on ${c_i}^s$, so $\sum_{t=1}^{T} y_{it}$ is a sufficient statistic for ${c_i}^s$. Therefore, the conditional log-likelihood can be defined for observation *i* (after omitting the terms that do not depend on the parameters) as

$$l_i(b^s) = \sum_{t=1}^{T} y_{it} \log(p_{it}(b^s)) - \log\big(1 - q_i(b^s)\big).$$

Then, the fixed-effects truncated Poisson estimator of β^s is obtained by maximizing

$$L(b^s) = \sum_{i=1}^{n} l_i(b^s)$$

with respect to b^s. Since the truncated Poisson distribution belongs to the exponential family of distributions, the conditional maximum likelihood framework developed by Andersen (1970) can be applied to derive that this fixed-effects estimator is consistent and asymptotically normally distributed.

APPENDIX 2: Data

Table A1: Selection of control micro-regions: variables in the treated, control and non-treated micro-regions and the coefficients of the propensity score model

	Treated	**Control**	**All non-treated**		
	micro-regions				
Explanatory variables in the propensity score model	**Groupwise means (with S.D.)**			**t-stat (treated – control)**	**Logit coef. (with S.E.)**
Cars per 1000 inhabitants	93.40 (23.7)	91.19 (16.48)	111.06 (26.22)	0.34	-0.0356 (0.0401)
Local tax per 1000 inhabitants (1000 HUF)	10.81 (7.18)	10.34 (5.36)	23.13 (18.09)	0.24	-0.0424 (0.0462)
Local unemployment rate	0.090 (0.035)	0.086 (0.030)	0.063 (0.029)	0.33	17.92 (17.71)
Regular cultural events in a year per inhabitants	0.056 (0.039)	0.058 (0.037)	0.078 (0.037)	-0.21	-25.38** (12.04)
Fraction of persons aged 60 or over	0.207 (0.023)	0.212 (0.028)	0.216 (0.023)	-0.64	-26.95 (17.55)
General practitioners per 1000 inhabitants	0.532 (0.089)	0.529 (0.114)	0.503 (0.068)	0.08	2.127 (4.942)
Fraction of high school graduates (%)	22.24 (4.77)	22.34 (4.12)	28.89 (8.01)	-0.07	0.323* (0.183)
Population of the chief town (1000 inhabitants)	6.34 (2.87)	6.91 (4.19)	26.52 (34.55)	-0.50	-0.353 (0.250)
Fraction of inhabitants living in urban areas	0.197 (0.241)	0.204 (0.257)	0.437 (0.269)	-0.09	5.011 (3.324)
Av. dist. to the chief town of the micro-region (in minutes, by car)	23.58 (9.22)	25.51 (8.57)	25.15 (12.21)	-0.69	-0.0620* (0.0319)
Weekly specialist hours in basic outpatient care per 1000 inhabitants in 2008	0.570 (0.894)	1.200 (1.508)	3.789 (4.581)	-1.63	-1.042*** (0.357)
Population (1000 inhabitants)	21.68 (8.83)	23.35 (11.44)	49.79 (42.33)	-0.52	-0.0668 (0.0490)
Pro-government major in the chief town	0.200 (0.411)	0.238 (0.436)	0.270 (0.446)	-0.29	-0.191 (0.927)
Number of micro-regions	20	21	137		157
Distribution of the propensity score					
Mean	0.591	0.366	0.067		
Standard deviation	0.228	0.270	0.170		
Minimum	0.140	0.094	0.000		
Maximum	0.954	0.898	0.898		

Source: own calculations based on TSTAR (settlement) and OEP data for year 2008

*** p<0.01; ** p<0.05; * p<0.1

Sample: Micro-regions outside Central Hungary (including the chief towns of the counties)

Logit dependent variable: dummy variable representing new outpatient service locations under SIOP 2.1.2.

t-statistic: usual t-statistic to test the equality of the averages in the treated and control micro-regions

Table A2: Average quarterly number of outpatient cases by patient in the treated and control micro-regions, 2008-2012

		2008	2009	2010	2011	2012
Overall	Treated	1.126	1.138	1.114	1.280	1.411
	Control	1.221	1.235	1.197	1.219	1.225
Gender: male	Treated	0.928	0.950	0.928	1.049	1.132
	Control	1.003	1.020	0.985	0.999	0.999
Gender: female	Treated	1.307	1.311	1.285	1.493	1.668
	Control	1.421	1.432	1.392	1.420	1.432
Age: 0-17 years	Treated	0.634	0.662	0.654	0.744	0.786
	Control	0.646	0.669	0.667	0.685	0.692
Age: 18-59 years	Treated	1.113	1.107	1.064	1.206	1.316
	Control	1.195	1.204	1.141	1.143	1.134
Age: at least 60 years	Treated	1.617	1.640	1.622	1.882	2.098
	Control	1.798	1.787	1.757	1.799	1.805

Source: own calculations based on micro-level GYEMSZI data

Note: outpatient cases excluding special one-day services.

Event-level data set: The original event-level data set contains administrative information for a 25 per cent random sample of all residents of 20 treated and 21 control micro-regions who showed up at least once in the health care administrative databases during the period. The comparison of the population of the micro-regions to the sample size of our data set suggests that around 10% of people never visited an inpatient or outpatient service between 2008 and 2012 and hence do not show up in our administrative records. However, these missing cross-sectional observations would not contribute to the estimation of the fixed-effects models and do not bias the results (see section 3).

Data cleaning: The origonal data set included many variables, but the additional dimensions played a role only in data cleaning. For instance, the knowledge of the department and the specialty of the case enabled us to exclude the special one-day inpatient services administered within the outpatient system (e.g. infusion treatments) from the analysis and rather concentrate on „traditional" outpatient provision. In the paper all results are calculated without these one-day services.

Treatment of deaths and migration: The fact that a particular person did not have an outpatient record during a quarter does not necessarily imply that he / she was present in the micro-region but did not visit the doctor. However, in the vast majority of records this is a reasonable assumption. Therefore, zero cases in a particular quarter were imputed for a person if there was no contradictory information. We used three types of contradictory information to exempt someone from being considered as not visiting the doctor in the relevant period: 1) periods before the date of birth, 2) periods after the known date of death, 3) periods after switching residence to a postcode outside of the treated and control regions. This rule is not completely correct since we observe a death only if it occured in a hospital and we observe the location of residence only when a person takes up an inpatient or outpatient service. However, as the majority of deaths occur in a hospital and migration is not widespread in the (older) population that uses health services more frequently, this imputation rule seems to be a reasonable method for creating the panel data set. This is also suggested by the fact that the overall results obtained from the semi-aggregate and micro-level data are very similar (see section 4).

APPENDIX 3: Robustness of the fixed-effects truncated Poisson estimator

When investigating the robustness properties of the fixed-effects truncated Poisson estimator, we use a setup similar to our empirical example. To analyse convergence, the number of cross sectional units is chosen as $n = 1000$ or $n = 10000$. There are two periods $(t = 1, 2)$. The vector of explanatory variables, X_{it}, consists of the constant and a dummy variable D_{it}, for which $D_{it} = 1$ if $t = 2$ and $i > n/2$, and $D_{it} = 0$ otherwise.[13] The conditional expectation equation (10) is:[14]

$$\mu_{it} = \exp(-2 + 1 * D_{it} + {c_i}^s).$$

We observe $y_{it} = [S_{it}|(S_{it} > 0)]$. Here, the probability distribution of $(S_{it}|X_{it}, {c_i}^s, \alpha_{it})$ is conditionally Poisson with expectation $\alpha_{it} * \mu_{it}$, where α_{it} is the mixing random variable. We assume that $(\alpha_{it}|X_{it}, {c_i}^s)$ is conditionally independent across *i* and *t* and $E(\alpha_{it}|X_{it}, {c_i}^s) = 1$. (Hence, $E(S_{it}|X_{it}, {c_i}^s) = \mu_{it}$.) We also assume that (S_{i1}, S_{i2}) is independent conditional on $(X_{i1}, X_{i2}, {c_i}^s, \alpha_{i1}, \alpha_{i2})$.

We use a variety of distributions to simulate the mixing variable α_{it}.

a) In the simplest case, α_{it} is independent identically Gamma-distributed with parameter (κ, κ) (this choice ensures that it has a unit expected value).[15] Since the negative binomial distribution is a Gamma-mixture of Poisson distributions, $(S_{it}|X_{it}, {c_i}^s)$ is negative binomial in this case. We choose $\kappa = 0.5$.

[13] This choice mirrors our empirical setting where roughly half of the units are treated in the second period.

[14] The reason behind this choice is that the non-robustness of the fixed-effects truncated Poisson estimator should show up if the truncation has a relatively large role, i.e. if μ_{it} is close to zero (because the usual fixed-effects Poisson estimator is robust to all misspecifications analysed here).

[15] With this parametrization the probability density function is given by $f(x) = \frac{\kappa^\kappa}{\Gamma(\kappa)} x^{\kappa-1} e^{-\kappa x}$ for $x > 0$.

b) In other simulations α_{it} is lognormal with parameters (τ_i, σ_i^2), where – to ensure the unit expected value – the condition $\tau_i + \frac{\sigma_i^2}{2} = 0$ holds. Our parameter choices:

   1) $(\tau_i = -1/2, \sigma_i^2 = 1)$

   2) Or we allow the distribution of α_{it} to depend on (D_{i1}, D_{i2}) by choosing $(\tau_i = -1/2,\ \sigma_i^2 = 1)$ for $i \leq n/2$ and $(\tau_i = -1/8,\ \sigma_i^2 = 1/4)$ for $i > n/2$. (Note that the distribution of α_{it} still does not depend on *t*.)

c) Finally, we allow the distribution of α_{it} to depend on *t* as well by choosing α_{it} as a Gamma random variable with parameter (1,1) for $t = 1$ and with parameter $(1/2, 1/2)$ for $t = 2$. (Note that $E(\alpha_{it}) = 1$ still holds.)

For the simulation of c_i^s, we first note that (just like α_{it}) the value $\exp(c_i^s)$ enters multiplicatively into the expression of the mixed Poisson-parameter $\alpha_{it} * \mu_{it}$. Hence we may use similar mixing distributions for simulating $\exp(c_i^s)$ as for simulating α_{it} above. (There is one major difference: not even the simulated random variable – let alone the distribution – of $\exp(c_i^s)$ depends on *t*.) Thus we use the above choices a/1, b/1, and additionally – to allow a nonzero correlation between c_i^s and D_{it} – a modified choice, b/2':[16]

b) 2') $\exp(c_i^s)$ is lognormal with parameter $(\tau_i = -1/2,\ \sigma_i^2 = 1)$ for $i \leq n/2$ and $(\tau_i = 1/2, \sigma_i^2 = 1)$ for $i > n/2$.

Table A3 shows the summary statistics of the conditional maximum likelihood estimates obtained for 1000 simulated panel data sets for each choice of n, α_{it} and $\exp(c_i^s)$ above. According to the Table, if the underlying distribution is a mixture of Poisson with the same distribution across time but with possibly different distributions across cross sectional units

[16] Note that here we may allow $E(\exp(c_i^s)) \neq 1$.

(choices a., b/1. and b/2. above, i.e. the first nine rows), then the estimated parameters are within a 0.01 wide interval around the true value ($\beta^s = 1$) in large sample sizes ($n = 10000$), and the empirical standard errors of the estimates are very close to the average robust standard errors calculated by the maximum likelihood routine.[17] This suggests that the fixed-effects truncated Poisson estimator is consistent in this setting, and the robust standard errors calculated from likelihood theory approximate the true uncertainty well.

At the same time, Table A3 also shows that the robustness property does not hold in a more general setting: the estimator does not appear to be consistent if the distribution of the mixing variable α_{it} depends on *t* as well (case c., i.e. the last three rows).

Table A3: Simulation results for the fixed-effects truncated Poisson-estimator

		$n = 1000$				$n = 10000$			
α_{it}	$\exp(c_i^s)$	Mean ($\widehat{\beta^s}$)	S.D. ($\widehat{\beta^s}$)	Rob S.E. (ML)	z-stat.	Mean ($\widehat{\beta^s}$)	S.D. ($\widehat{\beta^s}$)	Rob S.E. (ML)	z-stat.
a	a	1.0176	0.2407	0.2391	2.31	1.0082	0.0797	0.0776	3.26
	b / 1	1.0000	0.2489	0.2365	0.00	1.0099	0.0782	0.0782	4.00
	b / 2’	0.9995	0.1912	0.1827	-0.08	0.9974	0.0602	0.0614	-1.38
b / 1	a	1.0091	0.2565	0.2347	1.13	1.0036	0.0792	0.0782	1.42
	b / 1	1.0302	0.2449	0.2304	3.90	1.0070	0.0816	0.0781	2.71
	b / 2’	1.0076	0.1858	0.1763	1.29	0.9949	0.0628	0.0607	-2.58
b / 2	a	1.0208	0.2046	0.1956	3.21	1.0035	0.0620	0.0616	1.76
	b / 1	1.0161	0.2014	0.1969	2.52	1.0041	0.0638	0.0622	2.05
	b / 2’	1.0036	0.1224	0.1214	0.93	0.9996	0.0389	0.0394	-0.33
c	a	1.0569	0.2375	0.2262	7.57	1.0468	0.0734	0.0726	20.16
	b / 1	1.0509	0.2338	0.2261	6.89	1.0431	0.0723	0.0733	18.85
	b / 2’	1.0357	0.1820	0.1709	6.20	1.0429	0.0562	0.0558	24.17

Source: own calculations

Results for 1000 simulations in each case.

Note: z-stat = (Mean-1)/S.D.; Rob. S. E.: average standard errors calculated by the ML routine.

[17] It should be noted, however, that a formal z-test rejects the null hypothesis $\beta^s_{est} = 1$ at a 5% level in six out of the nine cases. For $n = 1000$ the standard errors are larger and the z-test rejects the null hypothesis at the 5% level in four cases out of the nine.