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## quasi-experimental evidence from administrative panel data

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

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## Abstract

Between 2008 and 2012 new outpatient service locations were established in Hungarian micro-regions, which had lacked outpatient capacities before. We exploit this quasi-experiment to estimate the effect of geographical accessibility on outpatient case numbers using both semi-aggregate and individual-level panel data from administrative sources. Based on propensity score matching methods, fixed-effect linear models and fixed-effect Poisson regression techniques, we find a substantial, 24-28 per cent increase of case numbers as a result of the establishments. Our causal estimates imply that a one-minute reduction of travel time to the nearest outpatient care provider increases case numbers e.g. by 0.8 per cent in internal medicine and 2.8 per cent in rheumatology. We also find that the size of the new outpatient capacities has a separate positive effect on case numbers, possibly caused by supplier-induced demand. By combining a fixed effect logit model and a fixed effect truncated Poisson model, 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 the main source of the increase in internal care, surgery and gynaecology, whereas both margins were important in rheumatology. Finally, as a methodological note, we examine the robustness of the fixed effect truncated Poisson estimator to some forms of misspecification by simulation methods.

## 1. INTRODUCTION[4]

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 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

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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. In developed countries the distance to the appropriate health service provider was shown to influence the number of hospital inpatient episodes (Haynes et al., 1999), asthma mortality (Jones and Bentham, 1997), cancer mortality (Campbell et al., 2000), mammography screening rates (Hyndman et al., 2000), general practitioner consultation rates, out-patient attendance rates and inpatient admissions (Haynes and Bentham, 1982). Pathman et. al. (2006) discuss access to general practitioners in the rural South of the US. In developing countries Erlyana et al. (2011) and Lavy and Germain (1994) discuss the same issue. The above studies generally measure the impact of accesibility on health service utilization rates or health outcomes based on cross-sectional variation in accessibility, 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 – even after using a large set of control variables – 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 will be under- or overestimated in a cross-sectional regression 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, see e.g. some specifications in 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 and 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 in Hungary. (For the application of natural experiments in public health see e.g. Craig et al., 2012.)

Hungary, an EU member state of 10 million inhabitants, expends 7.9% of its GDP on healthcare, 28% of which on outpatient care (2011 data, OECD, 2013); it has a single-payer health insurance system with virtually universal coverage.

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 (Gaál et al., 2011; for a historical understanding of the system: 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.) The newly created units provide comprehensive service for the population of the micro-regions with at least 14 separate branches at each location, although the number of consultation hours in the branches is generally low. As a result, basic specialist outpatient care (i.e. outpatient care in the four basic branches – 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 found, 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) and the impact of the improvement in accessibility can be estimated as the difference of the changes in the treated and control groups. During the particular estimation procedure, we use both semi-aggregated data (measured at the micro-regional level) and

individual (micro-level) case statistics. The semi-aggregated data are analysed both in a fixed-effect panel regression setting and with a combination of difference-in difference and propensity score matching techniques, while for the micro-level data the number of doctor visits is analysed with a fixed-effect Poisson regression.

The micro-level data make it possible to examine the heterogenous impact of the establishment of new outpatient locations on the various age groups and genders. 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 unit (one minute) change in journey time 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 aggregate change in case numbers into the „extensive" and „intensive" margins, i.e. into „new patients" and the visiting frequency of existing patients. Following Majo and Soest (2011), we use the fixed effect logit model for the extensive margin and the fixed effect truncated Poisson model for the intensive margin. We demonstrate by Monte Carlo simulation methods that the fixed effect 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 organized 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 estimation results are relegated to Appendix 1, while the robustness properties of the fixed effect truncated Poisson estimator are examined by simulation methods in Appendix 2.

## 2. DATA

Two different data sets are used in the analysis. First, semi-aggregate data were provided by the National Health Insurance Fund (Országos Egészségbiztosítási Pénztár, OEP). The data set contains the number of outpatient cases in the following *joint* classification:

- branch of the case (branches were classified into 29 groups)
- month of the case (between January 2008 and August 2012)
- micro-region of the patient (covering the 138 „rural-type" micro-regions defined as being outside Central Hungary and not in a chief town of a county).[5]

Thus for each branch we have a panel dataset showing the number of outpatient cases in 56 months and 138 micro-regions, of which 20 micro-regions were actually affected („treated") by the establishment of new outpatient service locations in them. 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 we have sufficiently long pre- and post-treatment periods as well. 430 thousand people (or 4.3 per cent of the population of Hungary) live in the treated micro-regions while 4.9 million people (or 48 per cent of the population) in the 138 micro-regions altogether.

The other data set, provided by the National Institute for the Quality and Organizational Development in Healthcare and Medicines (referred to as GYEMSZI, its acronym in Hungarian, below), contains micro-level information about every outpatient case for a 25 percent random sample of all people living in 20 treated and 21 control micro-regions which are most similar to the „treated" micro-regions in their observed characteristics.[6] The control micro-regions were chosen on the basis of the estimated treatment propensity, see section 3 for details. For every outpatient case the following information is provided:

- date of the case

[5] In the actual data set the number of cases were classified according to other dimensions as well such as the location (institution and department) of the provision, but these additional dimensions played a role only in data cleaning. For instance, the knowledge of the department and the branch 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 the „traditional" outpatient provision. In what follows all results are calculated without these one-day services.

[6] The data set for the whole population of Hungary was not available for us due to legal reasons, therefore we had to restrict our attention to a reasonable sample of micro-regions.

- location of the case (department of outpatient care unit)
- branch of the case
- ICD (International Classification of Diseases) code(s) of the case
- age and sex of the patient
- postcode of the address of the patient.

The different cases belonging to one given patient can also be matched through a special relational code; moreover, a separate data set contains individual-level information for all inpatient cases as well.

In this paper we utilize only a portion of the information content of the above data. We create a panel data set, which contains the number of outpatient cases by person and by period (quarter or year) for all recipients of outpatient service between 2008 and 2012.[7] Those who did not show up at least once during this period are not included in the data set.[8] However, these missing cross sectional observations would not contribute to the estimation of the fixed-effect Poisson model and therefore do not bias the results (see section 4).

## 3. METHODS

### 3.1. Methods for analysing the semi-aggregate data

The semi-aggregate panel data can be analysed in a classical impact assessment setting, comparing the evolution of the outpatient case numbers of the treated micro-regions with a counterfactual situation. (The counterfactual is defined as the hypothetical evolution of the

[7] The fact that a particular person did not have an outpatient record during a period does not necessarily imply that he / she was present in the micro-region but did not visit the doctor. However, in the majority of records this is a reasonable assumption. Therefore, zero cases in particular periods 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 birth dates were of course not imputed; 2) periods after a known date of death were not imputed; 3) if a person switched residence to a postcode outside of the treated and control regions, no period after this event was imputed. This rule is not completely correct since we observe a death only if it occured in an institution 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 an institution 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).

[8] The comparison of the micro-regional population data to the number of people in our dataset suggests that around 10% of the population never visited an inpatient or outpatient service between 2008 and 2012.

treated micro-regions in the – hypothetical – no treatment case.) Of course, the case statistics of all non-treated micro-regions is not likely to serve as a valid counterfactual (i.e. the set of all non-treated micro-regions may not form a valid control group) because the new outpatient service locations were targeted to areas with no previous outpatient capacity, which are generally more economically deprived and more „rural" than an average micro-region.[9] In a panel setting the differences of the treated and non-treated micro-regions can be controlled for with fixed effect panel regression techniques or with a combination of difference-in-difference and matching estimators. For the applications of these methods in public health see the review article of Jones and Rice (2011).

Let $y_{it}$ denote the logarithm of the aggregate number of outpatient cases in micro-region *i* and month *t*. This is affected by some observable and unobservable characteristics of the micro-region. The observable characteristics include measures of economic development (such as the unemployment rate) or the gender and age composition of the micro-region, which are together denoted by vector $X_{it}$. The time-invarying (or very slowly varying) unobservable characteristics of micro-region *i*, which also influence outpatient care use (such as the referring behaviour of the local GP) are denoted by $c_i$. The dummy variable $D_{it}$ labels the presence of a new outpatient unit, i.e. it takes 1 if a new outpatient unit operates in micro-region *i* in month *t* (and otherwise it is zero).[10] Finally, $u_{it}$ denotes the effect of other (time-varying) unobservable characteristics.

Then, if we assume that $X_{it}$ influences $y_{it}$ in a linear way we can write the model as

$$(1) \qquad y_{it} = X_{it}\beta + D_{it}\gamma + c_i + u_{it},$$

where the γ parameter shows the impact of the new outpatient service locations established under SIOP 2.1.2. on the number of outpatient cases.[11] The parameters of this model can be consistently estimated by the fixed effect method under weak conditions that allow the $c_i$

[9] The treated micro-regions have an average population of 21.5 thousand people, of whom only 20 per cent live in settlements with more than 7000 inhabitants. To compare, the average non-Central-Hungarian micro-region, which is not in a chief town of a county, has a population of 35.5 thousand.

[10] There were other extensions in outpatient services in the given period (SIOP 2.1.3., ROP), which are also controlled for and included in $X_{it}$.

[11] In some specifications the lagged values of $D_{it}$ were also used but the lag estimates were insignificant so they are not reported.

unobserved characteristics of the micro-regions to be arbitrarily correlated with the treatment indicator $D_{it}$. (See e.g. the monograph by Cameron and Trivedi (2005).)

Alternatively, as a robustness check using the semi-aggregate data we may control nonparametrically for micro-regional characteristics $X_{it}$. We may choose a control group of micro-regions which is observationally similar to the treated group with propensity score matching, and then use the difference-in-differences method to compare the evolution of the case numbers in the two groups during the observational period.

More formally, the propensity score is defined as the treatment probability estimated from a parametric (e.g. logit) model that contains the observed characteristics as explanatory variables, denoted by $\left(X_{iT_0}, X_{iT_1}\right)$ (where $T_0$ is the pre-treatment and $T_1$ is the post-treatment period):

$$\Pr(i \in treated) = \Lambda\left(X_{iT_0}\delta_0 + X_{iT_1}\delta_1\right) \quad (2)$$

and $\Lambda$ is the logistic function. After estimating this model and predicting the treatment probabilities, the control group can be found e.g. with nearest neighbour matching (i.e. by finding for each treated unit the closest non-treated one in terms of the propensity score) or with kernel matching (when for each treated unit the control is a kernel-weighted average of the non-treated units, with greater weight given to units whose propensity score is closer). Then, the difference-in-differences estimator between the treated and the control group can be applied.[12]

In our analysis the most important explanatory variable of treatment propensity was the number of specialist outpatient consulting hours (as a proportion of the population) before the SIOP 2.1.2. projects. As mentioned earlier, only the micro-regions without any substantial outpatient capacity could receive these grants. In the logit model, some other development indicators of the micro-region were significant as well (see Appendix Table A1 for the logit parameters and Table A2 for the distribution of the propensity score in the treated and non-treated sample). After finding the control micro-regions by nearest neighbour matching, a simple balancing test shows that none of the logit explanatory variables have significantly

[12] For an application of propensity score matching in estimating treatment effects see Dehejia and Wahba (1999), while for its combination with difference-in-differences techniques, see Blundell and Costa-Dias (2009). Propensity score matching is also discussed e.g. in Cameron and Trivedi (2005).

differing means at the 5% level in the two – treated and matched control – groups. (See Appendix Table A1 for the groupwise means of the variables.)

In the practical implementation, to use the available data optimally and to avoid problems of seasonality, we define the post-treatment period as May-August 2012 and pre-treatment period as May-August 2010 in the matching difference-in-differences procedure. We present results with nearest neighbour matching but kernel matching estimates do not differ from them substantially.

### 3.2. The fixed-effect Poisson model for analysing the micro-level data

The micro-level data set contains (in each branch) the quarterly number of outpatient cases for each person living in the 20 treated and 21 control micro-regions. The control micro-regions were chosen on the basis of the logit model presented in Table A1. But, unlike in the semi-aggregate analysis where nearest neighbour or kernel matching was used, in the micro-level analysis all non-treated micro-regions with a propensity score greater than 0.08 were included as control micro-regions. Estimating regressions on a sample pre-filtered on the basis of the propensity score is a usual practice in the statistical literature (see e.g. Angrist and Pischke, 2009) and we roughly followed the advice of Crump et al. (2009) in the pre-filtering procedure.[13] The resulting control group contains roughly the same number of inhabitants as the treated group. Due to the presence of multiple matches in the nearest neighbour procedure, the nearest neighbour control group does not coincide exactly with the micro-level control group but the balancing property of the latter seems satisfactory, too (see Appendix Table A1).

Turning to the regression analysis of the pre-filtered micro-level sample, let us denote by $N_{it}$ the number of outpatient cases of person $i$ in quarter $t$ in a particular branch. Its distribution may depend on the treatment indicator $D_{it}$ and other observable ($X_{it}$) and unobservable ($c_i$) characteristics of the person. In the health economics literature the number of doctor visits is traditionally modelled in a Poisson-regression framework or one of its generalizations (see e.g. Cameron et al., 1988; Deb and Trivedi, 2002; Pohlmeier and Ulrich, 1995). The Poisson-

[13] The usual parametric regression methods may work better on a more homogenous sample, which was already pre-filtered on the basis of the propensity score. 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. Appendix Table A2 shows that in our case the largest propensity score did not differ very much across the treated (0.96) and non-treated (0.89) regions. Hence – in order to avoid the loss of any of the 20 treated regions – we only restricted the propensity score from below.

regression has its panel counterpart as well, the fixed-effect Poisson model, where we allow the treatment indicator $D_{it}$ to arbitrarily correlate with the person's unobserved characteristics $c_i$. (That is, the treatment can be endogenous in the regression.)

Using our notations, the fixed-effect Poisson model assumes in its original form – developed by Hausman et al. (1984) – that $N_{it}$, conditional on $(X_{it}, D_{it}, c_i)$, follows a Poisson-distribution with expectation

$$(3) \qquad E(N_{it}|X_{it}, D_{it}, c_i) = \exp(X_{it}\beta + D_{it}\gamma + c_i),$$

and that $N_{it}$ and $N_{is}$ are independent of each other for $t \neq s$, conditional on $(X_{it}, D_{it}, X_{is}, D_{is}, c_i)$. Under these assumptions, Hausman et al. (1984) derived a conditional maximum likelihood estimator, which eliminates $c_i$ from the likelihood calculations. Hence the estimation of the fixed-effect Poisson model is free from the incidental parameter problem often encountered in fixed-effect models, and does not assume any particular relationship (or lack thereof) between $c_i$ and the observables. Furthermore, Wooldridge (1999) proved that this estimator has nice robustness properties: it is consistent even under the weak assumption that the conditional mean function (3) is well specified. The conditional distribution may be entirely unrestricted and the dependence across time within a cross-sectional unit can be arbitrary. (However, in this more general case, the standard errors should be adjusted.)[14]

Due to these robustness properties, the fixed-effect Poisson model is frequently used in the analysis of panel count data (see section 18.7.4. in Wooldridge (2010) for some applications). In our application parameter γ in equation (3) shows the effect of the treatment on the logarithm of expected case number, after controlling for the observable and time-independent unobservable characteristics of the micro-region and the person. Thus, γ in equation (3) is directly comparable with γ in equation (1).[15] Of course, the micro-level model allows the estimation of the parameters with a smaller standard error.

[14] Principally, 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 not always done in practice (see e.g. Angrist and Pischke, 2009). Without micro-data outside the 20 treated and 21 control micro-regions, we could not perform the bootstrap procedure in the fixed-effect Poisson regression. However, to spot potential biases, we replicated the propensity score based pre-filtering and the bootstrap standard error calculations in the fixed-effect panel regression on the semi-aggregate data set and found that the bootstrap and usual standard errors are only very slightly different.

[15] Note that $y_{it}$ in equation (1) denotes the logarithm of the semi-aggregate case numbers.

Apart from this greater precision, the real virtue of the micro-data is that the heterogenous effect of the treatment can be modelled on various groups of patients (e.g. genders or age groups) and the heterogeneity of treatment can also be exploited. For instance, we can take into account that the reduction of travel time to the outpatient service is different across various settlements in the treated micro-regions, or we can investigate whether the size of the new capacities had a differential impact on the case numbers.

To analyse the heterogeneities, we include the interaction of $D_{it}$ and age group dummies, and the interaction of $D_{it}$ and gender in the conditional expectation equation (3). Furthermore, we allow different treatment effects for the chief towns and other settlements in the treated micro-regions. (The new outpatient providers were built in the chief towns.) Thus the equality of treatment effects across age groups and gender can be tested by the statistical significance of interaction terms, and the equality of the effects in the chief towns and other settlements can be tested by the significance of the chief town variable.

To further exploit the heterogeneity in the reduction of travel time, we define $M_{it}$ as the travel time needed to reach the nearest outpatient unit of the given branch by car (in minutes) from the settlement of person *i* in quarter *t*. 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. To exploit this variation and give a structural interpretation of our results, we estimate the following equation by fixed-effect Poisson methods:

$$(4) \qquad E(N_{it} \mid X_{it}, M_{it}, c_i) = \exp(X_{it}\beta + M_{it}\delta + c_i),$$

where parameter $\delta$ 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.

Finally, to gain insight into the health economic reasons of the increase in the case numbers, we complete the dummy variable model (3) with the size of the new outpatient capacities (per 1000 inhabitants of the micro-region), denoted by $C_{it}$:

$$(5) \qquad E(N_{it} \mid X_{it}, D_{it}, C_{it}, c_i) = \exp(X_{it}\beta + D_{it}\gamma + C_{it}\tau + c_i).$$

Here, $\tau > 0$ means that after controlling for the travel time to the outpatient care unit, the new capacities still have a significant effect on the use of the services.

### 3.3. Separating the extensive and intensive margins

The 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). Following Majo and Soest (2011), we use a two-part (hurdle) approach by modelling first the random event $\{N_{it} > 0\}$ and then the variable $N_{it}$ conditional on this event: $y_{it} = [N_{it}|(N_{it} > 0)]$.

First the extensive margin (i.e. the effect on the visiting probability) is analyzed in a fixed effect logit framework:

$$(6) \qquad \Pr(N_{it} > 0|X_{it}, {c_i}^q) = \Lambda\,(X_{it}\beta^q + {c_i}^q),$$

where $\Lambda$ is the logistic function, $X_{it}$ now contains – for the sake of simplicity – the treatment dummy ($D_{it}$) or the travel time to the outpatient provider ($M_{it}$) or the size of the outpatient capacities ($C_{it}$) along with the usual control variables, 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 effect logit estimator has the advantage that it 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 the econometric literature (see e.g. Cameron and Trivedi (2005) or Wooldridge (2010)).

Second, we model the intensive margin (i.e. the change in the frequency of visits) in a fixed effect truncated Poisson framework. As a starting point, suppose that we only observe the positive part of our count data: $y_{it} = [N_{it}|(N_{it} > 0)]$, and $(y_{i1}, y_{i2}, \dots, y_{iT})$ are independent truncated Poisson random variables, conditionally on $X_{it}$ and on the unobserved heterogeneity associated with the visiting frequency to the doctor, ${c_i}^s$. The probability mass function of $y_{it}$ is given by

$$(7) \qquad \Pr(y_{it} = k|X_{it}, {c_i}^s) = \frac{\mu_{it}^k/k! * \exp(-\mu_{it})}{1-\exp(-\mu_{it})} \qquad (k = 1,2, \dots),$$

where

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

is the expected value of the corresponding untruncated Poisson distribution. (Note that $N_{it}$ is not necessarily a Poisson random variable, only its zero-truncated part follows a truncated Poisson distribution.) Then, as already noted by Majo and 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 effect Poisson regression – a fixed effect regression framework is applicable here, too.[16]

More precisely, 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 $p_{it} = \mu_{it}/\mu_i$. (Note that $p_{it}$ 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-effect Poisson estimator) states that the distribution of $(S_{i1}, S_{i2}, \dots, S_{iT})$, conditional on $\sum_{t=1}^{T} S_{it} = n$, is multinomial with parameters $(n, p_{i1}, p_{i2}, \dots, p_{iT})$ (see also Johnson et al. (1997), p. 32). The truncated random variables $y_{it}$ above 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 $\sum_{t=1}^{T} S_{it} = \sum_{t=1}^{T} y_{it} = n$, 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. It is easy to see by calculating the various joint probabilities of the zero marginals that the probability mass function of this distribution is given by

[16] Note that a natural alternative to the two-part (hurdle) modelling strategy would be to apply a zero-inflated regression framework that explicitly uses a mixture distribution of the people who never visit the doctor and of those who visit the doctor regularly with some underlying intensity (although there is a positive probability that a person from the second group does not visit the doctor in a given quarter). The zero-inflated framework has the advantage that it is invariant to time aggregation, e.g. to the choice between examining the quarterly or annual frequencies. However, in contrast to the two-part model, it lacks a proper fixed effect estimator, i.e. its parameters cannot be estimated without specifying the distribution of the unobserved heterogeneities and their relationship with the observed variables. On the other hand, the various random effect estimators of the zero-inflated Poisson model (see e.g. Hall, 2000) require the exact specification of these distributions and relationships, and are not robust to deviations from these assumptions. Therefore, we choose the fixed effect two-part (hurdle) approach in this paper, which enables us to model the two hurdles (and the two unobserved heterogeneities ${c_i}^q$ and ${c_i}^s$) in a completely flexible way.

$$\Pr\left(y_{i1}=n_1, y_{i2}=n_2, \dots, y_{iT}=n_T \middle| \sum_{t=1}^{T} y_{it}=n\right) = \frac{n! * \prod_{t=1}^{T} \frac{p_{it}^{n_t}}{n_t!}}{1-\sum_{m=1}^{T-1}(-1)^{T-m-1} \sum_{\{t_1,\dots,t_m\}\subset\{1,2,\dots,T\}} \left(p_{it_1}+\cdots+p_{it_m}\right)^n}$$

if $1 \le n_t < n$ $(t = 1, \dots, T)$ are integers such that $\sum_{t=1}^{T} n_t = n$.

For instance, for $T = 2$ we obtain the formula used by Majo and Soest (2011):

$$\Pr(y_{i1}=n_1, y_{i2}=n_2 | y_{i1}+y_{i2}=n) = \frac{n! * \frac{p_{i1}^{n_1}}{n_1!} * \frac{p_{i2}^{n_2}}{n_2!}}{1-p_{i1}^n-p_{i2}^n}$$

for $1 \le n_1 \le n-1$ and $n_1 + n_2 = n$. For $T = 3$ we get

$$\Pr(y_{i1}=n_1, y_{i2}=n_2, y_{i3}=n_3 | y_{i1}+y_{i2}+y_{i3}=n) = \frac{n! * \frac{p_{i1}^{n_1}}{n_1!} * \frac{p_{i2}^{n_2}}{n_2!} * \frac{p_{i3}^{n_3}}{n_3!}}{1-(p_{i1}+p_{i2})^n-(p_{i1}+p_{i3})^n-(p_{i2}+p_{i3})^n+p_{i1}^n+p_{i2}^n+p_{i3}^n}.$$

These formulae only depend on $p_{it}$ but not on $\mu_{it}$ (and hence neither on ${c_i}^s$), so $\sum_{t=1}^{T} y_{it}$ is a sufficient statistic for ${c_i}^s$. Therefore, we can define the conditional log-likelihood for observation *i* (after omitting the terms that do not depend on the parameters) as

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

where, using the notation $p_{it}(b^s) = \frac{\exp(X_{it}b^s + {c_i}^s)}{\sum_{r=1}^{T}\exp(X_{ir}b^s + {c_i}^s)} = \frac{\exp(X_{it}b^s)}{\sum_{r=1}^{T}\exp(X_{ir}b^s)}$,

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

Then, the fixed-effect truncated Poisson estimator of $\beta^s$ 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 effect estimator is consistent and asymptotically normally distributed. Hence, in contrast to other fixed effect truncated or censored regression models, in the truncated Poisson case there is no need to apply various advanced (e.g. semiparametric, see e.g. Honoré [1992]) estimators because the standard conditional maximum likelihood estimator is consistent.

Above, consistency and asymptotic normality was shown 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 3.2., the usual fixed effect (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 are not aware of any theoretical results on the robustness properties of the fixed effect truncated Poisson estimator but we demonstrate in Appendix 2 by Monte Carlo simulation methods that the truncated estimator is also consistent in the case when $N_{it}$ is a mixture of truncated Poisson distributions, with the same mixing distribution across periods. By choosing the gamma distribution as the mixing law, this contains the truncated negative binomial distribution as a special case.

Due to the large number of zero visits on the quarterly frequency, we fit the two-part model to annual data.

## 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 in the case of internal care the average travel time by car from these micro-regions to the nearest unit was around 19.8 minutes in 2008, and it decreased to 9.8 minutes by the end of 2012. Meanwhile, the

accessibility from the control micro-regions used in the micro-level analysis 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 1 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 branches 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 1 about here)*

As a first descriptive analysis of the impact of the treatment, Table 2 displays the standardized number of outpatient cases in 2010 and 2012 for the treated micro-regions and for those „rural" micro-regions, where non-negligible outpatient capacity existed already in 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 2 about here)*

### 4.2. Estimation results

Of course, the descriptive analysis above cannot control for other factors influencing the case numbers during the observational period and cannot estimate heterogenous treatment effects, either. Therefore we present the treatment effects estimated by the three econometric methods described in section 3. Table 3 shows that the new units established under SIOP 2.1.2. increased outpatient case numbers by 24-28 per cent, and this result is robust across all estimation methods. Looking at separate branches, rheumatology experienced the largest increase (55-89 per cent, depending on the estimation method), which is not surprising, given the large non-financial costs rheumatology patients are facing when they need to travel to an outpatient provider. The establishment of new outpatient locations did not seem to have an

impact on paediatrics and pulmonology cases, which is in line – for paediatrics – with the descriptive analysis of patient paths presented above.

*(Table 3 about here)*

### 4.3. Heterogeneity

The patient patterns in Table 1 already suggest that case numbers may have increased more in the chief towns than elsewhere in the treated micro-regions. The results of fixed-effect Poisson regressions, which allow different effects for chief towns and other settlements, and also across age groups and genders, show that in all branches the establishment of new outpatient service locations have had a substantially higher impact in chief towns than elsewhere. For instance, looking at all branches together, the SIOP 2.1.2 projects increased case numbers by 57.0 per cent for the chief towns and by only 20.0 per cent for other settlements. This indicates that patients who experienced a greater reduction of travel time reacted more strongly.

On the other hand, treatment effects do not differ strongly across age groups and gender. Looking at all branches taken together, women and older patients seem to react more strongly. The effect of the treatment is significantly higher for women than for men (by 3.9 percentage points), 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. Gender- and age-specific estimates on the branch level, however, do not paint a consistent picture.

To further exploit variability in the reduction of travel times, Table 4 shows the estimated percentage impact of a one-minute reduction of travel time for various branches (calculated from the estimated $\delta$ in equation (4)). For instance, for internal care, a one-minute reduction of travel time increases the number of cases by 0.8 per cent. The highest values are estimated for rheumatology (2.8 per cent), surgery and dermatology (1.9 per cent).

*(Table 4 about here)*

Finally, we present parameters estimated from models where the size of the new outpatient capacities (per thousand inhabitants of the micro-region), $C_{it}$, appears along with the treatment dummy $D_{it}$ in Table 5. In some large branches, notably in internal care,

traumatology, dermatology, rheumatology, pulmonology and cardiology, 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 branch, that makes the presence of supplier-induced demand unlikely there.

*(Table 5 about here)*

### 4.4. Separating the extensive and intensive margin

As noted earlier, the micro-level data allow us to decompose changes of case numbers into adjustment taking place on the extensive and on the intensive margin. Table 6 shows the rough decomposition of changes between 2009 and 2012 for the five most important branches by comparing the change of the visiting probabilities and visiting frequencies, respectively, in the treated and control micro-regions.[17] Since significance cannot be evaluated based on this rough decomposition, Table 7 displays the parameter estimates from the individual level fixed effect logit (for the extensive margin) and from the fixed effect truncated Poisson model (for the intensive margin). In internal care and obstetrics-gynaecology practically the whole adjustment takes place on the extensive margin. More than two third of the adjustment comes from the extensive margin in surgery, while the intensive adjustment has a larger role than the extensive one in rheumatology. The extensive margin adjustment is significant at the 1% level in all examined branches apart from paediatrics, while the intensive margin is significant at this level only for rheumatology. These results are not surprising given the different patient behaviours in various branches.

*(Table 6 about here)*

*(Table 7 about here)*

[17] Year 2009 is used as the base year because some of the new outpatient locations started to operate as early as September 2010. We omit Baktalórántháza because it started to operate in 2012.

## 5. DISCUSSION AND CONCLUSIONS

Our Hungarian micro-level data on the utilization of ambulatory care before and after the establishment of new outpatient care locations in rural micro-regions as well as on comparable micro-regions where at the same time supply did not change gave us a unique opportunity: we could consider opening these new outpatient care locations a quasi-experiment concerning how better geographical accessability affects the quantity of care given. Thus 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 different data sets and estimation methods) seems convincing.

We obtained significant estimates for how much a one-minute reduction of travel time increases the number of cases across different branches of care (e.g. 0.8 per cent for internal medicine, but 2.8 per cent for rheumatology). The fact that the (mostly returning, see below) 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.

Parallel studies for developed countries, cited in the introduction, discuss access to hospitals, not outpatient or primary care, so they do not allow for direct comparison. Our findings, however, are roughly comparable with those found by Erlyana et al. (2011) and Lavy and Germain (1994) for developing countries, based on cross-sectional methods.

By combining the fixed effect logit and the fixed effect truncated Poisson estimators, we could also decompose the effects of a change in accessability 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. (We also applied simulation techniques to show that our fixed effect truncated Poisson estimator is consistent even if the data come from a mixture of truncated Poisson distributions.) Our empirical findings for different branches of care are noteworthy: new visits were the main source of the increase in the aggregate number of visits when care came closer in internal care and gynaecology, whereas more frequent visits were the main cause for the increase in the number of cases treated in the case of rheumatology.

Telling apart impacts on the extensive and intensive margin (the results in Table 6) and identifying impacts net of the effect of an increase in the size of outpatient capacity (parameter of the pure treatment dummy in Table 5) 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. The presence of supplier-induced demand could also be suspected if more outpatient capacity (to be filled by doctors in search of increased funding for more fee-for-service points) would explain the increase of number of cases. While in many branches we find traces of both (see Tables 5 and 6), and therefore the hypothesis that at least some of the observed higher number of patients seeing the doctor as a result of easier access to outpatient care is due to supplier-induced demand cannot be discarded, in most branches of medicine we could identify significant increases in the number of cases even net of both of those effects, increases that, we argue, unambiguously represent "effective care". 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. Indeed, 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 (cf. Pohlmeier and Ulrich, 1995). Such a demand function could shed light on the benefits of more frequent ambulatory healthcare services as perceived by the patient.

Our results could also directly help Hungarian policy makers when planning the locations of further rural ambulatory care units. They could also be used in cost-benefit calculations meant to determine how the option of opening new outpatient care locations in underserved rural areas compares with other types of potential expenditures on health care improvement in terms of health effects. More generally, they can contribute to the understanding of how patient demand in different classes of cases and for different age and gender groups depends on the pecuniary and time-costs of travel to the location of care.

The establishment of new outpatient care locations between 2010 and 2012 may provide more evidence for health policy making in the future. Matched micro-level outpatient-inpatient data may also allow for a detailed investigation of patients' referral patterns; in particular, the interaction between outpatient and inpatient services e.g. by examining the possible decrease of avoidable hospital admissions as a result of the increased number of outpatient visits.

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**TABLES AND FIGURES**

*Table 1: 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 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 |
| Obstetrics-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 2: Number of outpatient cases (as a proportion of 100 inhabitants) in the SIOP 2.1.2. micro-regions and similar micro-regions*

| 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 200 hours of specialist monthly outpatient capacity already in 2010 and are outside Central Hungary and not in a chief town of a county. 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 3: Impact of SIOP 2.1.2. treatment on the number of outpatient cases (in per cent)*

| | Matching DiD | | | FE on semi-aggregate data | | | FE Poisson on microdata | |
|---|---|---|---|---|---|---|---|---|
| | Change of treated (%) | Change of control (%) | Effect (%) | Effect (%) | | S.E. | Effect (%) | S.E. |
| All branches | 30.7 | 2.6 | 28.0 | 26.7 | *** | 2.8 | 24.4 *** | 0.6 |
| Internal care | 13.7 | 2.6 | 10.6 | 14.2 | *** | 4.2 | 12.2 *** | 1.1 |
| Surgery | 38.2 | 0.0 | 38.1 | 39.7 | *** | 5.2 | 35.1 *** | 1.8 |
| Traumatology | 20.3 | -2.1 | 22.9 | 19.0 | ** | 8.0 | 8.8 *** | 2.4 |
| Obstetrics-gynaecology | 24.4 | 4.6 | 19.0 | 22.5 | *** | 4.1 | 16.5 *** | 1.6 |
| Paediatrics | -1.7 | -3.9 | 2.3 | 5.3 | | 4.5 | 0.9 | 1.9 |
| Otolaryngology | 19.9 | -6.5 | 28.2 | 32.0 | *** | 6.8 | 27.0 *** | 1.8 |
| Ophtalmology | 30.0 | 1.9 | 27.5 | 34.3 | *** | 3.1 | 27.3 *** | 1.3 |
| Dermatology | 36.0 | -2.6 | 39.6 | 39.1 | *** | 8.8 | 36.8 *** | 2.0 |
| Neurology | 32.3 | 1.8 | 30.0 | 28.1 | *** | 4.1 | 22.8 *** | 1.6 |
| Orthopaedy | 36.5 | 20.3 | 13.4 | 40.6 | *** | 4.6 | 28.8 *** | 2.3 |
| Urology | 22.2 | 9.8 | 11.3 | 20.2 | *** | 3.8 | 14.2 *** | 2.3 |
| Rheumatology | 83.0 | 0.5 | 82.0 | 88.3 | *** | 16.8 | 55.0 *** | 1.9 |
| Psychiatry | 17.9 | 2.1 | 15.4 | 16.1 | *** | 3.9 | 16.0 *** | 2.5 |
| Pulmonology | -16.1 | -33.4 | 25.9 | -1.3 | | 5.3 | -0.1 | 0.9 |
| Cardiology | 24.4 | 5.2 | 18.2 | 31.0 | *** | 7.7 | 17.5 *** | 1.6 |
| Lab diagnostics | 21.9 | 9.9 | 10.9 | 14.5 | * | 7.9 | 16.1 *** | 2.3 |
| X-ray diagnostics | 34.2 | 5.3 | 27.4 | 25.9 | *** | 3.9 | 10.0 *** | 1.0 |
| Ultrasound diagnostics | 20.3 | 8.3 | 11.1 | 19.0 | *** | 4.9 | 7.5 *** | 1.1 |

Source: own calculations based on semi-aggregate OEP and 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. Percentage effects (and not the actual coefficients) are displayed.

Matching DiD: comparison of changes in the treated and control micro-regions between the May-August periods of 2012 and 2010.

FE on semi-aggregate data: fixed-effect 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 (SIOP 2.1.3. and RDOP) development in outpatient service. Robust standard errors are displayed.

FE Poisson on micro-level data: fixed-effect Poisson model on quarterly case numbers by patient (living in treated or control micro-regions) between 2008 and 2012. Controls: time (quarter) dummies. Robust standard errors are displayed.

*Table 4: Impact of a one-minute reduction of travel time on the number of outpatient cases (percentage changes)*

| | | |
|---|---|---|
| Internal care | 0.772% *** | (0.0711) |
| Surgery | 1.88% *** | (0.104) |
| Traumatology | 0.852% *** | (0.126) |
| Obstetrics-gynaecology | 1.10% *** | (0.102) |
| Paediatrics | 0.236% ** | (0.0933) |
| Otolaryngology | 1.46% *** | (0.103) |
| Ophtalmology | 1.44% *** | (0.0812) |
| Dermatology | 1.88% *** | (0.116) |
| Neurology | 1.27% *** | (0.0938) |
| Orthopaedy | 1.54% *** | (0.124) |
| Urology | 1.11% *** | (0.118) |
| Rheumatology | 2.82% *** | (0.116) |
| Psychiatry | 1.03% *** | (0.0983) |
| Pulmonology | 0.0754% | (0.0497) |
| Cardiology | 0.847% *** | (0.0884) |
| Lab diagnostics | -1.56% *** | (0.210) |
| X-ray diagnostics | 0.797% *** | (0.0508) |
| Ultrasound diagnostics | 0.679% *** | (0.0644) |

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-effect 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 δ parameters in equation (4)). Controls: time (quarter) dummies.

*Table 5: Impact of the size of the new outpatient capacities on the case numbers (parameter estimates)*

| | Parameter of $D_{it}$ (treatment dummy) | | Parameter of $C_{it}$ (size of capacities) | |
|---|---|---|---|---|
| Internal care | 0.0453* | (0.0248) | 0.0559*** | (0.0202) |
| Surgery | 0.265*** | (0.0354) | 0.0379 | (0.0342) |
| Traumatology | -0.0824* | (0.0481) | 0.332*** | (0.0943) |
| Obstetrics-gynaecology | 0.125*** | (0.0307) | 0.0305 | (0.0272) |
| Paediatrics | -0.0521 | (0.0341) | 0.202** | (0.102) |
| Otolaryngology | 0.188*** | (0.0420) | 0.0948 | (0.0772) |
| Ophtalmology | 0.231*** | (0.0282) | 0.0132 | (0.0355) |
| Dermatology | 0.196*** | (0.0358) | 0.223*** | (0.0641) |
| Neurology | 0.155*** | (0.0442) | 0.102 | (0.0876) |
| Orthopaedy | 0.178*** | (0.0435) | 0.155 | (0.130) |
| Urology | 0.134*** | (0.0466) | -0.0151 | (0.143) |
| Rheumatology | 0.189*** | (0.0363) | 0.351*** | (0.0439) |
| Psychiatry | 0.0362 | (0.0629) | 0.165** | (0.0705) |
| Pulmonology | -0.0425*** | (0.0141) | 0.0856*** | (0.0205) |
| Cardiology | 0.0498 | (0.0358) | 0.183*** | (0.0632) |
| Lab diagnostics | 0.269*** | (0.0255) | 0.0558 | (0.0818) |
| X-ray diagnostics | 0.109*** | (0.0223) | -0.0189 | (0.0278) |
| Ultrasound diagnostics | -0.0603** | (0.0262) | 0.205*** | (0.0344) |

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 (equation (5)): fixed-effect Poisson model on quarterly case numbers by patient (living in treated or control micro-regions) between 2008 and 2012. Explanatory variables: treatment dummy ($D_{it}$) and the size of the new outpatient capacities ($C_{it}$). Controls: time (quarter) dummies.

*Table 6: Decomposition of the impact into the extensive and intensive margin (percentage changes)*

| | Overall effect (%) | | | Extensive margin (%) | | | Intensive margin (%) | | |
|---|---|---|---|---|---|---|---|---|---|
| | Treated | Control | Effect | Treated | Control | Effect | Treated | Control | Effect |
| Internal care | 7.4% | -6.3% | 14.6% | 5.7% | -5.1% | 11.4% | 1.6% | -1.2% | 2.9% |
| Surgery | 40.1% | -1.7% | 42.5% | 24.2% | -1.7% | 26.4% | 12.7% | 0.0% | 12.8% |
| Obstetrics-gynaecology | 12.7% | 0.7% | 12.0% | 7.6% | -5.2% | 13.4% | 4.8% | 6.1% | -1.2% |
| Paediatrics | -9.3% | -0.1% | -9.2% | -15.4% | -2.2% | -13.4% | 7.2% | 2.2% | 4.9% |
| Rheumatology | 56.4% | -4.5% | 63.7% | 21.3% | -3.0% | 25.0% | 28.9% | -1.6% | 31.0% |

Source: own calculations based on micro-level GYEMSZI data

Note: comparison of changes in the treated and control micro-regions between 2010 and 2012, excluding Baktalórántháza. The four basic branches and rheumatology are examined in detail (the latter because it experienced the largest increase in case numbers).

*Table 7: Impact on the visiting probability and visiting frequency (parameters of the treatment dummy in the fixed effect logit model and the fixed effect truncated Poisson model)*

| | Fixed effect logit | | Fixed effect truncated Poisson | |
|---|---|---|---|---|
| Internal care | 0.222*** | (0.0181) | 0.0527** | (0.0248) |
| Surgery | 0.311*** | (0.0188) | 0.107* | (0.0549) |
| Obstetrics-gynaecology | 0.199*** | (0.0210) | 0.0270 | (0.0400) |
| Paediatrics | -0.00448 | (0.0301) | 0.0190 | (0.0497) |
| Rheumatology | 0.411*** | (0.0211) | 0.309*** | (0.0381) |

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. The four basic branches and rheumatology are examined in detail (the latter because it experienced the largest increase in case numbers).

Fixed-effect logit model on the probabilities of visiting the doctor in a given year for people living in treated or control micro-regions. Years: between 2008 and 2012. Controls: year dummies. The estimated parameter shows the impact of the treatment on $\log(p/(1-p))$, hence tends to overestimate the impact on $\log p$.

Fixed-effect truncated Poisson model on individual zero-teruncated case numbers in a given year for people living in treated or control micro-regions Years: 2009 and 2012. Control: year dummy. 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.

## APPENDIX 1

*Table A1: New outpatient service locations established under SIOP 2.1.2.: Groupwise means and coefficient estimates of the variables included in the propensity score logit regression*

| Explanatory variable | Treated | Non-treated | Nearest neighbour control | Micro-level control | Logit Coef. (St. error) |
|---|---|---|---|---|---|
| | group of micro-regions | | | | |
| Cars per 1000 inhabitants | 93.52 | 106.74 | 94.24 | 90.55 | -0.0356 (0.0401) |
| Local tax per 1000 inhabitants (HUF) | 10567 | 21498 | 10605 | 10174 | -4.24e-05 (4.62e-05) |
| Local unemployment rate | 0.087 | 0.066 | 0.078 | 0.087 | 17.92 (17.71) |
| Regular cultural events in a year per inhabitants | 0.056 | 0.079 | 0.046 | 0.058 | -25.38** (12.04) |
| Fraction of persons aged 60 or over | 0.206 | 0.217 | 0.193 | 0.209 | -26.95 (17.55) |
| General practitioners per 1000 inhabitants | 0.495 | 0.492 | 0.520 | 0.522 | 2.127 (4.942) |
| Fraction of high school graduates (%) | 22.24 | 26.76 | 21.72 | 22.15 | 0.323* (0.183) |
| Population of the chief town | 6237 | 15827 | 6020 | 7197 | -0.000353 (0.000250) |
| Fraction of inhabitants living in urban areas | 0.187 | 0.394 | 0.184 | 0.219 | 5.011 (3.324) |
| Av. dist. to the chief town of the micro-region (in minutes, by car) | 23.43 | 25.54 | 22.86 | 25.54 | -0.0620* (0.0319) |
| Weekly specialist hours in basic outpatient care per 1000 inhabitants | 0.543 | 3.694 | 0.690 | 1.145 | -1.042*** (0.357) |
| Population | 21576 | 37096 | 23960 | 25129 | -6.68e-05 (4.90e-05) |
| Pro-government major in the chief town | 0.190 | 0.271 | 0.048 | 0.227 | -0.191 (0.927) |
| Constant | | | | | 6.088 (6.300) |
| Number of observations | 20 | 137 | 20 (multiple matches) | 21 | 157 |

Source: Own calculation based on TSTAR and OEP data

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

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

Groups: treated / non-treated / nearest neighbour matched control / micro-level control (i.e. with pscore > 0.08)

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

*Table A2: Propensity score distributions of treated and not treated micro-regions*

| Treatment status | mean | min. | 1st quartile | median | 3rd quartile | max. |
|---|---|---|---|---|---|---|
| Treated | 0.577 | 0.148 | 0.423 | 0.580 | 0.719 | 0.957 |
| Not treated | 0.072 | 0.000 | 0.000 | 0.000 | 0.034 | 0.891 |

Source: Own calculation based on TSTAR and OEP data

## APPENDIX 2: Robustness properties of the fixed effect truncated Poisson estimator

When investigating the robustness properties of the fixed effect truncated Poisson estimator, we use a setup similar to our empirical example. As earlier, let us denote the cross-sectional units by *i* and the time periods by *t* ($t = 1,2$). We observe $y_{it} = [N_{it}|(N_{it} > 0)]$ and the untruncated random variable $N_{it}$ has the same conditional expectation as in equation (8):

$$E(N_{it}|X_{it}, {c_i}^s) = \mu_{it} = \exp(X_{it}\beta^s + {c_i}^s).$$

However, instead of $N_{it}$ being conditionally Poisson distributed, it is now a mixture of Poisson distributions with the same mixing distribution across periods (but with possibly different mixing distributions across cross-sectional units).

More formally, let $(N_{it}|X_{it}, {c_i}^s, \alpha_{it})$ follow a Poisson distribution with parameter $\alpha_{it} * \exp(X_{it}\beta^s + {c_i}^s)$, where $\alpha_{it}$ is the mixing random variable. (We also assume that $(N_{i1}, N_{i2})$ is independent conditional on $(X_{i1}, X_{i2}, {c_i}^s, \alpha_{i1}, \alpha_{i2})$, but this is a relatively harmless assumption if there are only two time periods.) We assume that $(\alpha_{it}|X_{it}, {c_i}^s)$ is conditionally independent across *i* and *t*, its distribution does not depend on *t* (but may depend on *i*) and – to ensure the formula for $E(N_{it}|X_{it}, {c_i}^s)$ – the expected value is one: $E(\alpha_{it}|X_{it}, {c_i}^s) = 1$.

During the simulations we use the following formulation. In order to examine large sample sizes, we choose the number of cross sectional units as $n = 10000$. There are two time periods. The vector of explanatory variables, $X_{it}$, consists of the constant and a dummy variable $D_{it}$. This dummy is zero in the first period and takes the value of one for half of the cross sectional units in the second period, i.e. $D_{it} = 1$ if $t = 2$ and $i > 5000$, while $D_{it} = 0$ otherwise. These choices mirror our empirical setting where roughly half of the units are

treated in the second period and – for instance in internal care – there are about 11000 patients with case numbers positive in both periods.

We use a variety of distributions to simulate the mixing variable $\alpha_{it}$.

a) In the simplest case $\alpha_{it}$ is independent identically Gamma-distributed with parameter $(\kappa, \kappa)$ (this choice ensures that it has a unit expected value).[18] Since it is well-known that the negative binomial distribution is a Gamma-mixture of Poisson distributions, this choice leads to the case when $(N_{it}|X_{it}, {c_i}^s)$ is negative binomial. Our choices for the parameter:
    1) $\kappa = 1$
    2) $\kappa = 0.5$
b) In other simulations we assume that $\alpha_{it}$ is lognormal with parameters $(\tau_i, \sigma_i^2)$, where – to ensure the unit expected value – the condition $\tau_i + \frac{\sigma_i^2}{2} = 0$ should hold.[19] Our parameter choices:
    1) $(\tau_i = -1/2, \sigma_i^2 = 1)$
    2) $(\tau_i = -1/8, \sigma_i^2 = 1/4)$
    3) In other simulations we allow the distribution of $\alpha_{it}$ to depend on $(D_{i1}, D_{i2})$ by choosing $(\tau_i = -1/2,\ \sigma_i^2 = 1)$ for $i \leq 5000$ and $(\tau_i = -1/8,\ \sigma_i^2 = 1/4)$ for $i > 5000$. (Note that the distribution of $\alpha_{it}$ still does not depend on *t*.)

For the simulation of $c_i^s$, we first note that (just like $\alpha_{it}$) the value $\exp(c_i^s)$ enters multiplicatively into the expression of the mixed Poisson-parameter $\alpha_{it} * \exp(X_{it}\beta^s + {c_i}^s)$. Hence we may use similar mixing distributions for simulating $\exp(c_i^s)$ as for simulating $\alpha_{it}$ above. (However, there is one major difference: even the simulated random variable – not just the distribution – of $\exp(c_i^s)$ does not depend on *t*.) Thus we use the above choices a/1, a/2, b/1, b/2 and additionally – to allow a nonzero correlation between $c_i^s$ and $D_{it}$ – a modified choice, b/3':[20]

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

[19] The lognormal distribution has expected value $E(\alpha_{it}) = \exp\left(\tau_i + \frac{\sigma_i^2}{2}\right)$.

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

b) 3') $\exp(c_i^s)$ is lognormal with parameter $(\tau_i = -1/2,\ \sigma_i^2 = 1)$ for $i \leq 5000$ and $(\tau_i = 1/2, \sigma_i^2 = 1)$ for $i > 5000$.

Finally, we choose the conditional expectation equation (8) as $\mu_{it} = \exp(-2 + 1 * D_{it} + {c_i}^s)$. (That is, the parameter to be estimated is $\beta^s = 1$ because the constant is not identified due to the presence of ${c_i}^s$.) The reason behind this choice is that any non-robustness of the fixed effect truncated Poisson estimator is likely to show up if the truncation has a relatively large role, i.e. if $\mu_{it}$ is not far from zero (because we know that the usual fixed effect Poisson estimator is robust to all misspecifications described above).

To investigate the robustness of the fixed effect truncated Poisson estimator, we simulated the panel data set for each choice above 500 times and estimated the parameter by maximum likelihood.[21] For each choice we obtained that the the mean of the 500 estimated parameters did not differ significantly from the true value ($\beta^s = 1$). This suggests that the fixed effect truncated Poisson estimator is consistent for two time periods if the underlying distribution is a mixture of Poisson distributions, with the same distribution across time (but possibly different distributions across cross sectional units).

Note however that the above robustness property does not hold in more general settings. For instance, according to our simulations, the estimator is not consistent if the distribution of the mixing variable $\alpha_{it}$ depends on *t* as well (even if $E(\alpha_{it}) = 1$ still holds). To demonstrate this, we simulated $\alpha_{it}$ as a Gamma random variable with parameter $(1,1)$ for $t = 1$ and with parameter $(1/2, 1/2)$ for $t = 2$ and obtained biased parameter estimates during the simulations.

[21] Technically, the estimation was carried out by an ML routine written in Stata.