WOMEN AND THE POOR ARE MORE SENSITIVE TO HEALTH CARE PRICES – REGRESSION DISCONTINUITY EVIDENCE

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Abstract – We assess price sensitivity in health care by applying a regression discontinuity design based on age differential copayments in Swedish primary care. At age 20, the copayment increases from 0 to 100 Swedish kronor (≈ $12) per visit to a physician in primary care. The analysis is performed using high-quality health care and economic register data of 73,000 individuals aged 18–22. The copayment decreases the average number of visits to physicians by 5–11%, the main estimate at 6%. For women visits are reduced by 8% and for low-income individuals by 11%. Most price sensitive are low-income women.

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I. Introduction

In 2016, the Organization for Economic Co-operation and Development (OECD) countries spent an average of 9% of their GDP on health care, varying from about 6–7% in the Baltic countries to 17% in the US (OECD 2017). Considering the large and increasing share of resources going to health care, policy makers throughout the world are trying to address increasing costs without hampering quality or access to care. Because patients typically do not face the true economic cost of their health care utilization, there is a long-standing concern that moral hazard, i.e. additional health care utilization due to being insured (Zweifel and Manning 2000), implies demand volumes that will not be financially sustainable or efficient in the long-term.

The use of copayments, or cost sharing in general, has been a standard tool to reduce the risk of moral hazard in health care and to increase efficiency and welfare. In tax-financed health care systems such as the UK and the Nordic countries, funding from copayments rather than from taxes also reduces the welfare cost associated with tax collection. For example, in Sweden it is estimated that every tax-generated Swedish krona (SEK) comes at a cost of SEK 1.3 (Birch Sørensen 2010, Dahlby 2008). However, a concern with the use of copayments is that they might reduce compliance with relevant care and thus cause negative health consequences (Baicker and Goldman 2011, Goldman, Joyce, and Karaca-Mandic 2006). There are also concerns about equality with the use of copayments based on discussions and speculation that low-income groups are more responsive to price changes in health care (Baicker and Goldman 2011).

To assess the welfare and equality consequences of copayments, we need evidence on the behavioral responses to copayments and how such responses potentially differ across demographic and socio-economic groups. Some relatively recent reviews have identified more than 50 papers that have estimated the behavioral response to copayments (Kiil and Houlberg 2014, Skriabikova, Pavlova, and Groot 2010). Kiil and Houlberg (2014) conclude that the majority of the reviewed studies report that average health care utilization is reduced by copayments, with estimates of elasticities (available only from a few of the studies) ranging from −0.01 to −0.4. It is challenging to identify the causal effects of copayments because individuals who consume health care in any given time period do not constitute a random sample of the population, and the price a patient faces is typically non-random. A majority of the studies in the above-mentioned reviews are not based on experimental or quasi-experimental approaches, which means that it is questionable whether one can draw causal conclusions from most of these studies.

Studies with empirical designs that have dealt credibly with selection bias and endogeneity include the seminal work on the Rand Health Insurance Experiment (e.g. Manning et al. 1987), indicating a price elasticity of demand at around −0.2 (Keeler and Rolph 1988). More recently, Chandra, Gruber, and McKnight (2014) utilized exogenous variation in copayments faced by low-income groups in Massachusetts and estimated an average price elasticity of −0.16, which is in line with elasticity estimates for higher-income groups from other studies. Based on data from the Oregon Medicaid lottery, a significant increase in utilization was also seen for low-income groups upon receiving Medicaid coverage (Finkelstein et al. 2012). Non-US evidence is sparser, but includes, for example, regression discontinuity (RD) design studies in Japanese and Taiwanese contexts with elasticity estimates of around −0.1 to −0.2 (Fukushima et al. 2016, Shigeoka 2014, Han, Lien, and Yang 2016). Recent evidence using Swedish data indicate small but significant effects for children’s and adolescents’ demand for medical care (Nilsson and Paul 2015). In exception to the above mentioned studies documenting significant but inelastic demand, a study evaluating a price increase reform in the German statutory health insurance found no significant reduction in physician visits (Schreyogg and Grabka 2010). Likewise, two

Swedish studies assessing regional copayment differences and a regional price increase reform found no significant effects in demand for primary care (Jakobsson and Svensson 2016a, b).

In sum, restricting the empirical evidence to studies with a (quasi-)experimental approach results in relatively few studies estimating the behavioral effects of copayments or cost sharing in general. Several of these studies also concern specific sub-samples of the population, such as only low-income populations, and can thus only make indirect comparisons on the price responsiveness between income groups.

In this paper, we add to the literature by assessing the price responsiveness in health care by using high-quality register data on health care utilization linked with data on economic and demographic background. We take advantage of the fact that in Swedish primary health care there is a discontinuity in the copayment at the age of 20, at which age a copayment of SEK 100 per physician visit is introduced (SEK 100 ≈ $12 in 2017). The main RD estimate shows that the introduction of the copayment reduces visits to physician in primary care by 6%, with robustness estimates between 5–11%. Thanks to a rich data set, we are able to explore discrepancies across gender and income groups, something few previous studies have been able to examine directly. We find that women are more price sensitive, with a reduction in utilization by 8% compared to 4% for men. Furthermore, we find substantial differences across income groups, with a reduction in demand by 11% for the lowest income quartile compared to a reduction by only 1% for the highest income quartile. The results are robust to in extensive sensitivity analyses of different econometric specifications as well as in light of falsification tests using "false" age thresholds.

The rest of the paper is structured as follows. In section I, we describe the institutional setting of the study, i.e. the Swedish health care system and Region Västra Götaland, which is the county council with the age-threshold rule that we utilize. Section II describes the register data and the RD design, and we provide details regarding the robustness and falsification tests that we carried out. The main results are presented in section III, and the results of the robustness checks are shown in section IV. Section V gives the concluding discussion of the paper, where we highlight that as one of the few studies where direct comparison of price responsiveness in health care between income groups can be made, we find substantially higher price sensitivity among low-income individuals.

II. Institutional Background

In the Swedish health care system, the role of the government is to set principles and guidelines, whereas the responsibility to finance and provide health care lies with each of the 21 county councils (regions), and to some extent the 290 municipalities, which are specifically responsible for long-term elderly care. Health care costs represent 11% of Sweden's GDP, which is relatively high compared to other OECD countries (OECD 2017). Taxes, which are set within each county council, fund the bulk of health care costs, while patient out-of-pocket payments and state grants are smaller financing sources (SKL 2017). In outpatient primary care, about 40% of physician visits take place with a private provider, and the equivalent number for outpatient specialist care is 25% (RKA 2016). The private providers are tax-funded as well, and they function under the same policies as the public providers, for example, using identical copayments (although there is also a very small purely private market outside of the tax-funded health care system).

An outpatient primary health care center is the first port of call for non-acute physical and mental problems. At the primary care centers you find physicians specialized in general medicine, hereafter called primary care physicians, along with nurses, physical therapists, midwives, psychologists, etc. Patients are assigned to a specific primary care center based on geographic proximity, but they are free to switch and register at another center of their choice within the county council. The number of physicians is relatively high in Sweden, with 4.0

physicians per 1,000 inhabitants compared to the OECD average of 3.3 (OECD 2017). Despite this, the number of physician consultations in outpatient primary and specialized care is substantially lower in Sweden at 2.9 per capita and year compared to the OECD average of 6.6 (OECD 2017). In international comparisons, Swedish health care fares well on most health outcomes such as low levels of infant mortality and long life expectancy, but accessibility and long waiting times are often-debated issues where the health care system comes out poorly.

The cost-sharing policy of the Swedish health care system consists of two features – a copayment and a cap. Patients are charged a fixed copayment when utilizing health services, and if the total out-of-pocket expenditure reaches the cap within a moving 12-month period, the copayment falls immediately to zero for the remainder of the 12-month period. The general principles are the same across the country (and the cap is fixed at a national level), while the specific details, for example, on the level of copayments, are determined within each county council.

Our study relies on data from the Region Västra Götaland, one of the larger county councils in the southwest of Sweden with about 1.6 million inhabitants out of Sweden's total of approximately 10 million inhabitants (Statistics Sweden 2016b) and containing Sweden's second largest city of Gothenburg, a number of smaller towns, and rural communities. Compared to other county councils and to Sweden as a whole, the region is a "mini-Sweden" with respect to demographic and socio-economic composition and to levels of health care utilization. The cost-sharing rates are as follows: the copayment for a visit to a primary care physician is SEK 100 (SEK 300 if visiting a primary care center other than where the patient is listed), the copayment for a visit to an outpatient specialist is SEK 100–300, and the 12-month cap for outpatient services is SEK 1,100. Most important for our study design is that children and adolescents are excused from copayments up to their 20th birthday. The policy remained the same in the county council throughout the period of interest.

III. Data and Methods

A. Data

We have merged the demographic and socioeconomic register data from Statistics Sweden with the regional health care database, which includes data on all outpatient health care visits and is maintained by the county council of Region Västra Götaland. We extracted the data for all individuals born in 1993–1996 and who were resident in the county in 2014–2015. The data cover 73,000 individuals of the ages 18–22, with 159,000 visits to primary care physician over two years. The merging of the registers and the analysis plan were approved by the Regional Ethics Review Board in Gothenburg (reference number 359-16). Table 1 gives descriptive statistics for the sample.

The age of the patient at the point of visit determines whether the individual is charged a copayment. Due to confidentiality considerations, the date of birth made available is given by the quarter of the month, for example, "the first quarter of July 1995". The first quarter relates to day 1–7, the second to day 8–15, the third to day 16–23, and the fourth to day 24–31, with a total of 48 quarters per year. Age at the time of visit follows the same notation, for example, a person born in the first quarter of July 1995 having a consultation in the fourth quarter of July 2014 will be 19 years and three quarters at the time of the visit.

We observe the individuals' physician visits in each quarter of the month starting from the quarter of the month of the individual's 19th birthday and up to the quarter of the month of their 21st birthday. This implies 97 quarters in total and a window width of ±12 months around the age cut-off (threshold). For each specific age, e.g. 19 years and 3 quarters = 19.06 years, we observe the individuals in the sample who pass that age-cell at some point during 2014–2015, and this results in between 35,000 and 38,000 individuals for each age-cell. Table A1 in the online appendix shows a number of background characteristics for the individuals included in a set of selected age-cells. Among the younger individuals in the sample, the share of men is larger, a larger share has at least one parent with tertiary education and more of them live with at least one of their parents. The share born domestically is smaller among the younger individuals in the sample. Median income, measured as the equivalized disposable income of the household[1], is larger for the younger group (because more of them live with their parents). Regressing each of these background covariates on age, shows that they are running smoothly, without discontinuity, across the age threshold (Figure 1).

[Insert Figure 1, Table 1 Here]

B. Regression Discontinuity Design

We exploit the age discontinuity of the copayment scheme to estimate the effect of copayments on visits to primary care physician using an RD design. If no other variables of importance for the outcome change discretely at the age cut-off and there is no manipulation around the cut-off, we can interpret a discontinuous change in physician visits as a causal effect of the copayment (Lee and Lemieux 2010, Angrist and Pischke 2014). In our longitudinal data set, the subsets of individuals in each age-cell are overlapping, so the individuals of age 19 years and 11 months are to a large extent the same individuals as in the subset of age 20 years and 1 month.

[1] Equivalized disposable income for the year 2014 is the sum of the whole household’s disposable income divided by its consumption weight. Disposable income includes wages, business profits, transfers, pensions, unemployment insurance payouts, taxes, profits from capital, etc. The sum can be negative. (Statistics Sweden 2016a).

Using age as the assignment variable in an RD design has the advantage that the individual cannot manipulate their age (see e.g. Bargain and Doorley 2011, Lemieux and Milligan 2008, Han, Lien, and Yang 2016). Despite this, age cannot be considered completely randomly assigned because everyone in the sample turns 20 years of age at some point, possibly creating an anticipation effect, and this marks a distinction from the standard RD design (Lee and Lemieux 2010). In addition, we need to interpret the discontinuity in outcome as the total effect of all factors that discretely change at the age threshold (Lee and Lemieux 2010). There are many ongoing lifestyle changes for young adults that might be related to health care utilization, but none that switch on or off at the 20th birthday; rather, they are continuously changing.

We apply a sharp RD because the treatment, i.e. the copayment, is a deterministic function of age. To describe the underlying specification of the RD design, following Lee and Card (2008), let Y_1 and Y_0 be the potential outcome in physician visits with or without copayment, let T be a binary indicator of treatment, and let X be a running variable of age determining T by a certain threshold x_0, where $T = 1$ if $X \geq x_0$. The aim is to find the treatment effect $E[Y_1 - Y_0|X = x_0]$, i.e. the expected difference in outcome with or without treatment at the threshold. The data do not identify the expected outcome without treatment at or after the threshold $E[Y_0|X \geq x_0]$, so we need to extrapolate. Empirically we collapse the data to a cell-level weighted regression of age-cell means using the number of observations in each age-cell as the weights and using conventional robust standard errors. We estimate the following regression equation:

$$Y_j = \beta_1 Treat_j + h(x_j) + \varepsilon_j, \qquad (1)$$

where the outcome Y_j is the number of visits to primary care physician per capita and year in age-cell j, and $h(x_j)$ is a smooth function of age with the specific characteristic of $h(x_0) = E[Y_0|X = x_0]$. The variable Treat is defined as:

$$Treat = \begin{cases} 0 \; if \; x_j < x_0 \;\; i.e. \; age < 20 \\ 1 \; if \; x_j \geq x_0 \; i.e. \; age \; \geq 20 \end{cases}. \qquad (2)$$

The parameter of interest in equation (1) is β_1, which gives the treatment effect $E[Y_1 - Y_0|X = x_0]$. The cut-off at age 20 is centered to zero, $x_0 = 0$, thus x_j takes values in quarters of months away from the cut-off. The cell-level weighted regression gives coefficient estimates equivalent to standard least squares estimates of a micro-level regression (Lee and Lemieux 2010). The random specification errors, which are the degree to which the true function $h(\bullet)$ deviates from the specified polynomial function, are assumed to be independent of treatment status, which implies that the least squares estimate $\hat{\beta}$ is consistent for β_1. The cell-level weighted regression also deals with the within-group correlation introduced by these specification errors.

Knowing the date of birth and the date of visit by the quarter of the month implies that of those paying a visit to a physician in the monthly quarter of their 20th birthday (age-cell 0), some individuals had their visit just the day before their birthday (free of charge) and some just the day after (charged the copayment). We use age in years as noted by the care provider at the point of visit to determine the approximate number of visits within the quarter of the month of the 20th birthday that were made just before or just after the birthday. We impute an additional age-cell between −1 and 0, at approximate age 19.99, that includes visits by patients who were reported by the care provider to be 19 years, and we keep the visits where patients were reported to be 20 years in age-cell 0 (Table A2 in online appendix). We assume the number of individuals passing age-cells −0.5 and 0 are evenly distributed.

Apart from a linear RD model, we include polynomial functions of the assignment variable to assess the robustness of the treatment effect. However, it should be noted that higher-order polynomials are prone to bias due to noisy estimates and sensitivity to the degree of polynomial (Gelman and Imbens 2014). Following Lemieux and Milligan (2008) and Bargain and Doorley (2011), we consider simple linear, quadratic, and cubic functions and linear and quadratic splines, where we let the slopes differ on either side of the threshold (the assignment variable in interaction with the treatment dummy). We use Akaike's information criterion (AIC) to assess the goodness of fit of the different model specifications (Jacob et al. 2012).

Our main analysis assesses the effect of copayments on visits to primary care physician. In addition, we compare the main results to copayment effects on visits to outpatient specialists. Thanks to the rich data set, we are also able to perform subgroup analyses on gender and income groups, something few previous studies have done credibly. For analysis of heterogeneity based on income, the sample is divided into four groups by quartiles of household income for the year 2014.

C. Robustness Checks

We perform a number of alternative estimations to assess the robustness of the results, and we focus on the sensitivity with regards to (i) the imprecision of age-cell 0, (ii) the bin width of the assignment variable, (iii) the width of the window of observations, (iv) the use of local linear regression, and (v) the use of false cut-offs.

To assess the sensitivity with regards to the imprecision of age-cell 0, we try two alternative regressions. In the first, we assume that equation (2) holds with the original data and that all patients paying visits in age-cell 0 are charged a copayment, and in the second we exclude the observations from the quarter of the month of the 20th birthday, thus creating a slightly larger gap at the cut-off.

Next we consider the bin width of the assignment variable, i.e. the width of the age-cells, as suggested by Lee and Lemieux (2010). The narrower the bin, the closer to the cut-off, but there will also be more variation and potentially unnecessary noise. We increase the bin width for the robustness check, thus creating 26 monthly bins and assessing the copayment effect farther away from the cut-off but with less variability in the outcome variable.

Another way to assess the trade-off between precision and bias is to explore the window of observations that are included (Bargain and Doorley 2011, Lee and Lemieux 2010). A larger window of values will give more precise estimates due to a larger number of observations, but a smaller window will reduce the bias of the treatment effect. To come closer to the threshold we reduce the window width to ±9, ±6, ±3, and ±1.5 months around the cut-off.

The non-parametric local linear regression is used for robustness checks because it puts more emphasis on observations close to the threshold (Imbens and Kalyanaraman 2012, Skovron and Titiunik 2015). We use a triangular kernel and a data-driven approach to choose the bandwidth of the kernel, which is optimized given the polynomial order (Skovron and Titiunik 2015, Gelman and Imbens 2014, Jacob et al. 2012). Putting more weight on observations close to the threshold, the issue of imprecision of age-cell 0 becomes more important, thus we apply the main approach with imputed age-cell −0.5 and use the alternative version where we assume that equation (2) holds. As suggested by Jacob et al. (2012), when using few observations the functional form is likely to be linear, thus the local linear regression is our main approach, but we also apply a local quadratic regression. We check the bandwidth sensitivity by plotting treatment effect estimates and confidence intervals as a function of bandwidth (Jacob et al. 2012). For inference, we use the robust bias-corrected confidence intervals and p-values, as suggested by Calonico, Cattaneo, and Titiunik (2014). Like in the main analysis, we perform the regression using the age-cell means and frequency weights.

A final check of robustness is a set of falsification tests, or "placebo regressions", of the discontinuity (see e.g. Bargain and Doorley 2011, Lemieux and Milligan 2008, Lee and Lemieux 2010). We do this by running linear splines specifications with a window width of ±12 months for all possible cut-offs for ages 19.00 to 19.75 years and for 20.25 to 21.00 years. Additionally, checking the robustness in different specifications for the false cut-offs of 19.5 years and 20.5 years, we run quadratic splines, increase the bin width to full months, and reduce the window width to ±6 months around the false cut-offs.

D. Tests for an anticipation effect

As mentioned above, the inevitable closing date of free-of-charge health care at age 20 could possibly cause an anticipation effect, an intertemporal substitution, of increased utilization of health services among adolescents approaching the threshold. If so, our model estimates the effect of the threshold rather than the pure price effect, which would violate the causal interpretation and imply an overestimation of the copayment effect. The presence of an anticipation effect is tested by eliminating the observations at ±1, ±2, ±3, and ±4 months around the threshold.

Assessing the copayment effect farther away from the cut-off, implies that the samples just below and just above differ slightly, and therefore we take a closer look on the background covariates in these age-cells and include them as control variables in the regression. Due to high correlation between background covariates and age, we perform one regression for each covariate, including the covariate and its interaction with the treatment dummy in a linear splines model specification. Another way to deal with discrepancies between age-cells is to use a more homogeneous subsample where such differences are minimized. We explore the copayment effect in a subsample of our data; restricting the sample to those about 18,000 individuals born between July 1994 and June 1995, and reduce the window width to age-cells 19.5−20.5 which enables to follow this one specific sample over all age-cells.

IV. Main Results

Figure 2 graphically shows the results of the main analysis. We see that below the age-threshold the slope is increasing, with a visible downward jump at age 20 and a slightly negative slope above the threshold. This indicates that the introduction of the copayment at age 20 reduces the number of primary physician visits. We estimate a copayment effect of −0.07 irrespective of model specification (Table 2). The results are highly statistically significant with all p-values < 0.001. Goodness of fit by AIC indicates that the specification with linear splines gives the best model fit. With an average of 1.12 visits to primary care physicians per year, the estimated effect corresponds to a 6.2% reduction in visits due to the introduction of the copayment (Table 3). As a comparison, for visits to outpatient specialists the copayment effect is estimated to be −0.01. Specialist visits are also exposed to the copayment introduction at age 20, but because it is presumed that there is higher severity of disease when visiting a specialist, and considering the more stringent supply rationing in specialized care, it is quite expected that we would find a smaller copayment effect for visits in specialized care compared to visits in primary care.

[Insert Figure 2, Table 2 and 3 Here]

A. Results from the Robustness Checks

The results from the robustness checks are presented in Table 4 and in figures in the online appendix. The two alternative ways to deal with the issue of imprecision in age-cell 0 estimate copayment effects that are similar to the main result, between −0.07 and −0.08 in almost all specifications. Reducing some of the noise by increasing the width of the age-cell bins to full months yields a slightly smaller copayment effect of −0.05 to −0.06 (Figure A1 in the online appendix). Varying the window width of the linear splines specification shows that the size of the copayment effect, estimated between −0.06 to −0.12, is sensitive to the range of values being used, with a larger variance as the number of observations decreases. In Figure A2 in the online appendix, it can be seen how the slopes vary from positive to negative depending on window width.

The local linear regression, with an optimal kernel bandwidth of 13 quarters of the month, estimates a copayment effect of −0.10 irrespective of how we deal with the imprecision in age-cell 0. Moreover, the estimated copayment effect is stable in sign and size with regards to the kernel bandwidth range (Figures A3 and A4 in the online appendix). In the local quadratic regression, however, we estimate insignificant copayment effects of 0.002 and −0.07 depending on how we deal with age-cell 0. This is possibly due to overfitting because the local approach selects a bandwidth range where a stable estimate of the treatment effect is found given polynomial order. The estimated effect in the local quadratic regression is very sensitive to bandwidth range, yielding positive point estimates given a small bandwidth and negative point estimates given a large bandwidth. Taken together, the robustness checks yield results in line with the main analysis, estimating copayment effects between −0.05 and −0.12, corresponding to a 4.5−10.7% reduction in visits to primary care physicians.

In the falsification tests, we estimate effects ranging from −0.01 to +0.03 for the false cut-offs between 19.00 and 19.75 years, and all of them are lower in magnitude than the result from the true cut-off and all but one are positive (Figure A5 in the online appendix). For the false cut-offs between 20.25 and 21.00 years, the estimated effects range from −0.04 to +0.07, with a large variability in the estimates. Assessing the robustness of the false cut-off at 19.5 years gives an estimate ranging from 0.007 to 0.03 depending on model specification, bin width, and window width (Table 5). At the false cut-off of age 20.5 years, the estimated effect ranges from −0.05 to −0.003. We find that even minor changes in model specification lead to a variety of estimates for the false cut-offs, while similar changes do not significantly affect the results at the true threshold. Thus, the findings for the false cut-offs do not remain robust over any possible threshold or through alternative regressions of the specific false cut-offs.

[Insert Table 4 and 5 Here]

B. Exploring the presence of an anticipation effect

Testing the presence of an anticipation effect estimates a copayment effect between −0.05 and −0.07, (Figure 3 and Table 6). Finding a slightly smaller effect ±1 and ±2 months from the cut-off is an indication for the presence of an anticipation effect. Yielding a relatively larger effect at ±3 and ±4 months from the cut-off could be an effect of age-cell samples (e.g. −12 and +12) deviating in background characteristics, creating a higher level of use in age-cell before the threshold and a lower level of use in age-cells after the threshold.

Examining some important background characteristics in the samples before the threshold, the share of men is larger; the share born abroad is larger; the share having parent(s) with tertiary education is larger; the share living with parent(s) is larger; and the median household income is larger (Figure 1, and Table A1 in the online appendix). All these characteristics are associated with a lower level of physician visits compared to their counterparts (Table 1). In regressions including these background covariates the copayment effect at the cut-off is estimated between −0.07 and −0.08 (Table 6). ±1, ±2, ±3 and ±4 months away from the cut-off yields varying result, but more than half of the estimates are the same as our main estimate (−0.07), or larger (in absolute terms).

Using a single sample to be followed from age 19.5 through 20.5, we estimate a copayment effect of −0.08 at the threshold (Figure A6 in the online appendix), and ±1, ±2, and ±3 months away from the threshold a copayment effect between −0.06 and −0.15. Across all tests for an anticipation effect, we find the smallest effect ±2 months away from the cut-off, some estimates even close to zero. In sum, there are indications of an anticipation effect in the months closest to the copayment cut-off, but the anticipation effect is at most a minor part of the total effect at the threshold.

[Insert Figure 3, Table 6 Here]

V. Heterogeneous Price Sensitivity

With respect to income, we find statistically significant differences between income quartiles (Figure 4 and Table 7). The estimated copayment effect by linear splines is −0.14, −0.09, −0.05, and −0.01 for the 1st, 2nd, 3rd and 4th, income quartile respectively. The 1st quartile corresponds to the group with the lowest incomes. The copayment causes reductions in demand by 11.4%, 7.9%, 4.6%, and 1.1% for the 1st, 2nd, 3rd, and 4th income quartiles, respectively (Table 3). Hence, there is a substantial difference in price sensitivity across income groups. With respect to gender, we estimate a notable difference in copayment effect, around −0.11 for women and −0.04 for men (Figure 5 and Table 7). The results vary slightly depending on model specification. The estimated copayment effect corresponds to a reduction in demand by 7.9% and 4.1% for women and men, respectively (Table 3).

Separating groups based on gender and income, we estimate the largest copayment effects (in absolute terms) among women in low-income groups, and the effect among high-income women is close to our main estimate (Table 7). The smallest (or actually positive) copayment effects are found among men in high-income groups. We have also performed subgroup analyses based on some other background characteristics, finding a larger copayment effect among those born abroad, those having parents with lower education and those living without parents, compared to their respective counterparts (Figure A7 in the online appendix).

[Insert Figure 4 and 5, Table 7 Here]

VI. Conclusion and Discussion

Using an RD design, we have evaluated the effect of introduction of a copayment at age 20 on the number of visits to physicians in primary care. Our results show a statistically significant decrease of 6% in visits to primary care physicians caused by the introduction of the copayment, and these results remain robust through various robustness tests yielding results between 5-11%. In subgroup analyses, we find differential effects in the price sensitivity, with an 8% decrease in visits among women, an 11% decrease among low-income individuals (the lowest income quartile), and among low-income women an even more pronounced price sensitivity. With the high-quality data including all income groups used in this study, we can clearly and credibly show that price sensitivity in health care is heterogeneous with respect to income, which has not been directly evaluated in previous studies.

Comparing our results to that of Nilsson and Paul (2015), in which setting the magnitude of the change in copayment was in the same range, we receive very similar results. We can also compare our results to the demand curve from the Rand Health Insurance Experiment. Keeler and Rolph (1988) estimated the quantity of health care services demanded with a 25% coinsurance rate as the percentage of spending relative to 100% quantity demanded with a 0% coinsurance rate. The economic cost of a visit to physician in primary care in Sweden being about SEK 1,500 (SKL 2016), the coinsurance rate in our case is 100 / 1,500 = 6.67%. Our estimate of a 6% reduction in demand due to the copayment thus corresponds to 94% quantity demanded relative to 100% quantity demanded with no copayment. Based on these assumptions, we can simultaneously plot our estimates next to Keeler and Rolph's Rand demand curve, and we see that the estimates here are relatively close to the Rand results (Figure A8 in the online appendix). However, the overall copayment effect estimated here implies a steeper demand curve, i.e. less price sensitivity compared to the Rand estimates. For the sub-group-specific results, we find that the demand curves of men and of high-income individuals are even steeper. The demand curve of women falls on top of the Rand demand curve, and the demand curve of low-income individuals is flatter, i.e. there is greater price sensitivity compared to the Rand estimates.

To understand the reasons behind the behavioral effects of the copayment introduction, we need to consider both the demand and the supply side. The copayment is low relative to average incomes, and for high users the yearly cap on out-of-pocket expenditures is eventually activated, so patient cost sharing is overall a small share of the typical individual's budget. However, in the Swedish context, it is likely that the supply restrictions overrule the demand effects of the price change. Both gatekeeping and waiting times are used as rationing tools, and patients are offered a visit based on medical prioritization. This process is likely to reduce potential overuse and moral hazard behavior in patients. Our results further confirm that price sensitivity varies with respect to the type of health care, as we find the copayment effect on visits to outpatient specialists to be significantly smaller than the effect on visits to primary care physicians. From the demand side, this can be interpreted as patients being less price sensitive regarding more severe health problems that are in need of specialized care. But again, supply-side restrictions are of importance because gatekeeping and waiting times appear on dual levels, the first being the phone triage system into primary care and the second being the subsequent need of referral for specialized care. This emphasizes the importance of understanding price effects in health care with respect to context such as institutional setting and type of health care.

Distributional effects of cost-sharing policies have not been well researched in the previous literature. On the account of the rich, high-quality register data we have used, our study contributes with unique estimates of heterogeneity in copayment effects based on income and gender. Our study population of young adults includes the entire range of the income distribution, enabling direct comparison between income groups. Direct comparison has been impossible for studies considering a subset of the population, for example, only low-income individuals. Our findings that low-income groups are more price sensitive in demand for physician services than high-income groups was expected in line with previous discussions (Baicker and Goldman 2011), and in full agreement with results from Nilsson and Paul (2015). These findings are important because they suggest that the copayment itself contributes to inequalities in health care with respect to income. Knowing that low-income groups on average are in worse health compared to high-income groups, the heterogeneity in copayment effects might also cause increased inequality in health. Thus, discrepancies in the effects of cost sharing might be problematic in the perspective of equality in health care and in health.

Similar to our findings, Olsen and Melberg (2016) found that teenage Norwegian girls had a higher responsiveness to the abolition of copayments for physician visits compared to boys. In contrast to our findings, Cockx and Brasseur (2003) found men to be more price sensitive than women in demand for physician services in Belgium. We note that, as expected, there are large discrepancies between the genders in the rate of visits (see e.g. Osika Friberg et al. 2016). There is, however, no intuitive logic behind why either men or women would be more price sensitive, and it remains to be explored what mechanisms are in action to create such differences. It should be noted that the usual gender-income correlation is not present in our study population because 80% of the adolescents lived with their parents and the household income is in the majority of cases based mainly on parental income. In addition, a larger copayment effect among women is probably not attributable to women's reproductive needs because the vast majority of health care regarding contraceptives and pregnancies in Sweden takes place with midwives, not with physicians.

Considering the limitations in our study, an important discussion is whether we can trust the causal interpretation of the study design. The definitive end of free-of-charge health care at age 20 possibly creates anticipation effects of increased demand as adolescents approach the threshold. An anticipation effect would be likely in the case of preventive care or a general check-up, but this is presumably quite uncommon among the study population due to their young age and good health. Further, anticipation is unlikely in the sense that health-related issues are uncertain and usually cannot be pre-scheduled. In our tests, we find some indications of an anticipation effect, but the evidence is not clear cut. Comparing to the range of estimates from our robustness checks (−0.05 to −0.12), actually most estimates in the tests for an anticipation effect are within that range. However, something is going on ±2 months away from the cut-off but it is difficult to interpret given such variety of estimates. If there was a prominent anticipation effect, why would it show only at ±2 months, but not to the same extent at ±1 and ±3 months away from the cut-off? In conclusion, we cannot rule out that a minor part of the estimated copayment effect is actually the effect of anticipation of the threshold approaching. Regarding policy implications for example a change in copayment, in presence of an anticipation effect we can expect a more short term effect compared to a pure price effect which would imply effects in a more long-run perspective.

Furthermore, we need to consider the presence of other programs that are restricted by the same age threshold (Lee and Lemieux 2010) and to interpret the discontinuity in outcome as the effect of all factors that discretely “switch on” at age 20. Under Swedish regulations, age 20 gives eligibility to buy alcoholic beverages in liquor stores. Carpenter and Dobkin (2009) found a discontinuous increase in mortality rates at age 21, the legal drinking age threshold in the US, implying discrete effects on health due to access to alcohol. With that in mind, it is possible that access to alcohol is also associated with increased health care use, which then would bias our estimates towards zero. However, sudden alcohol-related health problems presumably affect emergency visits, and a sharp rise in visits to primary care physicians seems questionable. Moreover, because the Swedish setting involves two thresholds for alcohol – age 18 gives access to alcohol in restaurants and bars, and age 20 permits buying alcohol in liquor stores – we may assume that the effect at age 20 is minor. As previously mentioned, adolescents experience a number of lifestyle changes around the age of 20, but because they happen continuously they will not affect health care utilization discretely at the 20th birthday.

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Tables

TABLE 1. – DESCRIPTIVE STATISTICS OF INCOMES AND PHYSICIAN VISITS IN DIFFERENT GROUPS

	Obs	Median	Mean	St. Dev.	Min	Max
Household income 2014 (SEK)[a]						
All	72,882	221,295	239,361	339,389	−2,043,339	78,200,000
1st income quartile[b]	18,222	104,127	94,024	49,102	−2,043,339	146,669
2nd income quartile	18,219	186,200	185,300	21,221	146,670	221,293
3rd income quartile	18,221	256,097	256,720	21,273	221,296	295,875
4th income quartile	18,220	355,612	421,413	632,425	295,877	78,200,000
Per capita visits to primary care physicians in 2014						
All	72,882	1	1.12	1.58	0	20
Women	35,089	1	1.41	1.76	0	17
Men	37,793	0	0.86	1.33	0	20
1st income quartile[b]	18,222	1	1.21	1.73	0	20
2nd income quartile	18,219	1	1.12	1.57	0	17
3rd income quartile	18,221	1	1.09	1.52	0	17
4th income quartile	18,220	1	1.06	1.48	0	15
Born in Sweden	65,022	1	1.13	1.58	0	20
Born abroad	7,860	1	1.08	1.59	0	14
Having parents with secondary educ. (or lower)	35,992	1	1.17	1.63	0	20
Having parent(s) with tertiary educ.	35,387	1	1.07	1.51	0	15
Living without parents 2014	14,013	1	1.26	1.74	0	20
Living with parent(s) 2014	57,401	1	1.09	1.53	0	17
Per capita visits to specialists in outpatient care in 2014						
All	72,882	0	0.80	1.83	0	69

Notes:

a. SEK 1,000 ≈ $120 (2017). Household income is the equivalized disposable income in the year 2014 and is the sum of the whole household's disposable income divided by its consumption weight. Disposable income includes wages, business profits, transfers, pensions, unemployment insurance payouts, taxes, profits from capital, etc. The sum can be negative. (Statistics Sweden 2016a).

b. The 1st income quartile corresponds to the group with the lowest incomes.

TABLE 2. – MAIN RESULTS OF THE COPAYMENT EFFECT ON VISITS TO PHYSICIAN IN PRIMARY CARE

	Copayment effect (std. err.)	Goodness-of-fit AIC
Linear	−0.070 (0.0001)	−341.137
Linear splines	−0.071 (0.0001)	−341.669
Quadratic	−0.071 (0.0001)	−340.607
Quadratic splines	−0.072 (0.0001)	−339.394
Cubic	−0.068 (0.0001)	−338.805

Notes: Robust standard errors are in parentheses, all estimates are statistically significant with p-values < 0.001. AIC without frequency weights.

TABLE 3. – COPAYMENT EFFECTS AND REDUCTION IN DEMAND

	Copayment effect	Visits per capita per year	Reduction in demand
Visits to physicians in primary care			
All	−0.07	1.12	−6.2%
Income quartile			
1st (lowest)	−0.14	1.21	−11.4%
2nd	−0.09	1.12	−7.9%
3rd	−0.05	1.09	−4.6%
4th	−0.01	1.06	−1.1%
Gender			
Women	−0.11	1.41	−7.9%
Men	−0.04	0.86	−4.1%
Outpatient specialist visits			
All	−0.01	0.80	−1.3%

TABLE 4. – RESULTS FROM THE ROBUSTNESS CHECKS

Imprecision of age-cell 0	$T = 1\ if\ age \geq 20$	*Excluding age-cell 0*
Linear	−0.073 (0.0001)	−0.076 (0.0001)
Linear splines	−0.075 (0.0001)	−0.077 (0.0001)
Quadratic	−0.074 (0.0001)	−0.077 (0.0001)
Quadratic splines	−0.083 (0.0001)	−0.087 (0.0001)
Cubic	−0.073 (0.0001)	−0.079 (0.0001)

Bin width: Month		Window width (linear splines)	
Linear	−0.061 (0.0001)	±9 months	−0.063 (0.0001)
Linear splines	−0.062 (0.0001)	±6 months	−0.088 (0.0001)
Quadratic	−0.061 (0.0001)	±3 months	−0.121 (0.0002)
Quadratic splines	−0.051 (0.0002)	±1.5 months	−0.062 (0.0003)
Cubic	−0.049 (0.0002)		

Non-parametric local regression	*Main (imputed age-cell −0.5)*		$T = 1\ if\ age \geq 20$	
Linear splines	−0.099	(−0.176; −0.019)	−0.109	(−0.166; −0.046)
Quadratic splines	0.002	(−0.100; 0.141)	−0.068	(−0.128; 0.018)

Notes: Each cell gives the estimated copayment effect with robust standard errors in parentheses. For the non-parametric local regressions, the parentheses show the confidence intervals. All estimates are statistically significant with p-values < 0.001 except the estimates of local quadratic regressions which are non-significant.

TABLE 5. – FALSIFICATION TEST APPLYING CUT-OFFS AT 19.5 YEARS AND 20.5 YEARS

	Cut-Off 19.5	Cut-Off 20.5
Bin width: quarter of month		
Linear splines	0.028 (0.0001)	−0.023 (0.0001)
Quadratic splines	0.032 (0.0001)	−0.036 (0.0001)
Bin width: month		
Linear splines	0.022 (0.0001)	−0.012 (0.0001)
Quadratic splines	0.007 (0.0001)	−0.003 (0.0002)
Window width: ±6 months		
Linear splines	0.008 (0.0001)	−0.051 (0.0001)
Quadratic splines	0.034 (0.0002)	−0.043 (0.0002)

Notes: Each cell gives the estimated copayment effect with robust standard errors in parentheses, all estimates are statistically significant with p-values < 0.001.

TABLE 6. – TESTS OF AN ANTICIPATION EFFECT

	Main sample	Main sample, adjusting for					One sample, born July 1994 − June 1995
		% men	% born in Sweden	% having parent(s) with tertiary educ.	% living with parent(s)	household income	
At the cut-off	−0.071 (0.0001)	−0.080 (0.0001)	−0.074 (0.0001)	−0.078 (0.0001)	−0.069 (0.0001)	−0.065 (0.0001)	–0.080 (0.0002)
Away from the cut-off by ±1 month	−0.063 (0.0001)	−0.069 (0.0001)	−0.068 (0.0001)	−0.078 (0.0002)	−0.088 (0.0003)	−0.090 (0.0002)	–0.060 (0.0002)
±2 months	−0.054 (0.0001)	−0.007 (0.0002)	−0.052 (0.0001)	−0.043 (0.0002)	+0.0002 (0.0005)	+0.030 (0.0005)	–0.066 (0.0002)
±3 months	−0.073 (0.0001)	−0.072 (0.0002)	−0.092 (0.0002)	−0.054 (0.0003)	−0.067 (0.0005)	−0.043 (0.0006)	–0.152 (0.0002)
±4 months	−0.074 (0.0001)	−0.067 (0.0002)	−0.106 (0.0003)	−0.051 (0.0004)	−0.068 (0.0005)	−0.062 (0.0004)	..

Notes: All regressions estimated using a linear splines model specification. Each cell gives the estimated copayment effect with robust standard errors in parentheses, all estimates (except % living with parent(s), ±2 months) are statistically significant with p-values < 0.001. Due to high correlation between background covariates and age, we adjust only for one covariates at a time. In regressions with covariates, we calculate the copayment effect at its standard error at the threshold using the x-value of that age-cell.

TABLE 7. – GENDER AND INCOME DISCREPANCIES IN COPAYMENT EFFECT

	1st income quartile	2nd income quartile	3rd income quartile	4th income quartile
Linear splines	−0.138 (0.0003)	−0.088 (0.0003)	−0.050 (0.0004)	−0.012 (0.0004)
Quadratic splines	−0.139 (0.0005)	−0.072 (0.0005)	−0.042 (0.0006)	−0.038 (0.0005)

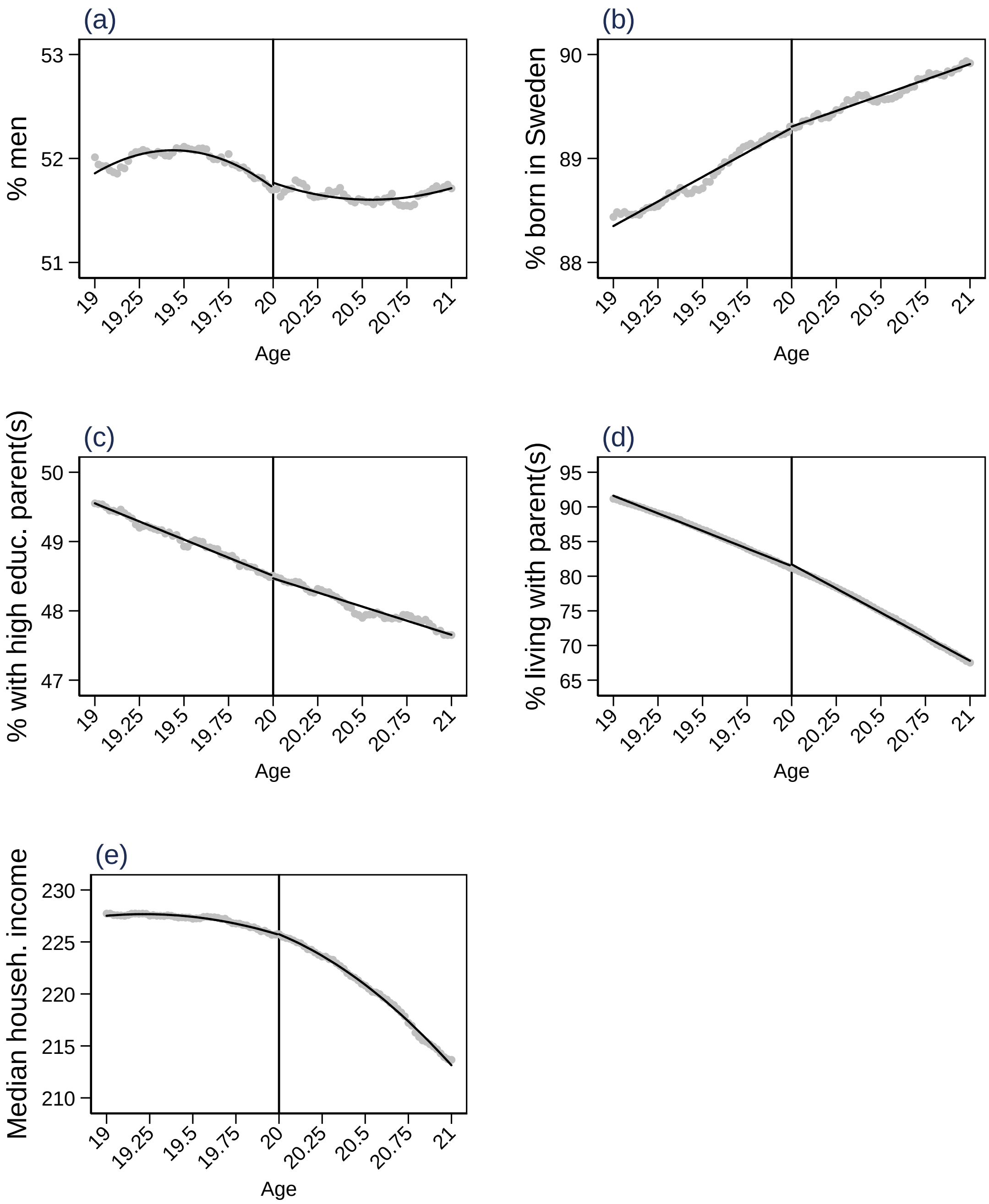
	Women	Men
Linear splines	−0.111 (0.0002)	−0.035 (0.0001)
Quadratic splines	−0.119 (0.0003)	−0.028 (0.0002)

Linear splines	Women	Men
1st income quartile	–0.183 (0.0008)	–0.098 (0.0006)
2nd income quartile	–0.136 (0.0007)	–0.045 (0.0005)
3rd income quartile	–0.065 (0.0008)	–0.039 (0.0005)
4th income quartile	–0.065 (0.0008)	+0.032 (0.0006)

Notes: Each cell gives the estimated copayment effect with robust standard errors in parentheses, all estimates are statistically significant with p-values < 0.001. The 1st income quartile corresponds to the group with the lowest incomes.

Figures

FIGURE 1. – REGRESSION OF BACKGROUND COVARIATES ON AGE-CELLS



Notes: a) percentage of men, b) percentage of individuals born in Sweden, c) percentage having at least one parent with tertiary education, d) percentage living with at least one parent 2014, and e) median household income year 2014.

FIGURE 2. – MAIN RESULTS OF THE ESTIMATED COPAYMENT EFFECT ON VISITS TO PHYSICIAN IN PRIMARY CARE

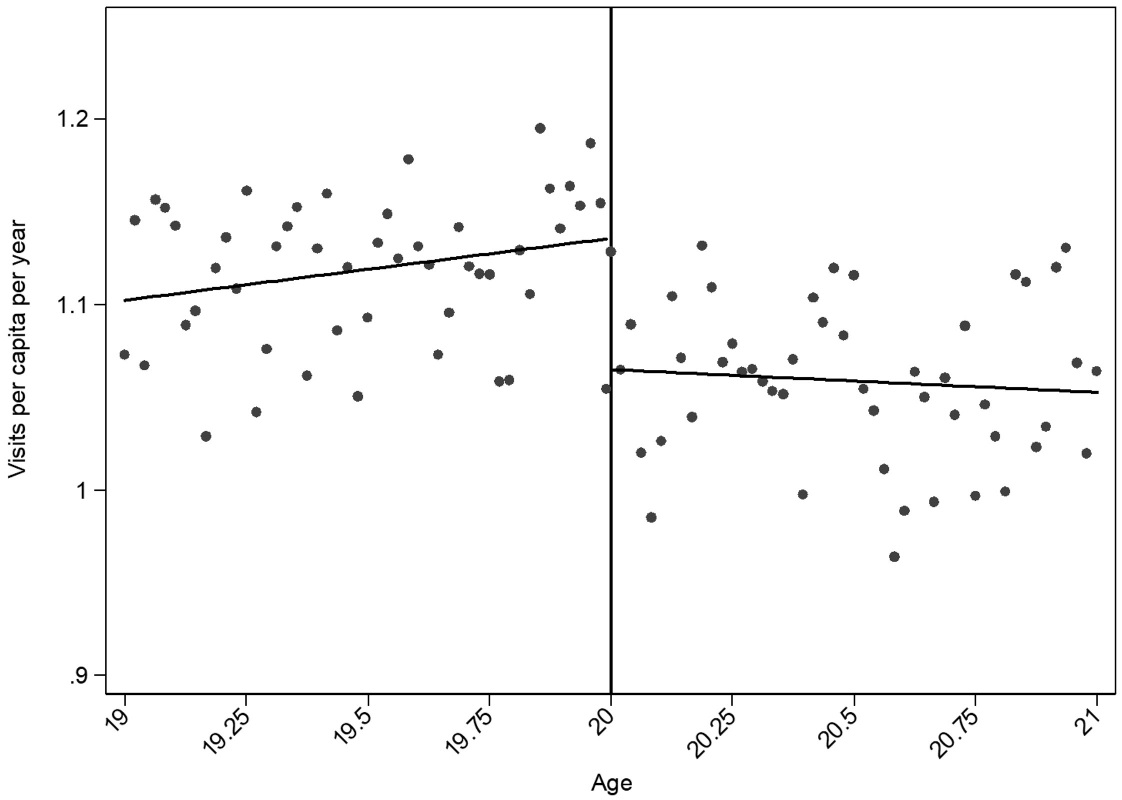
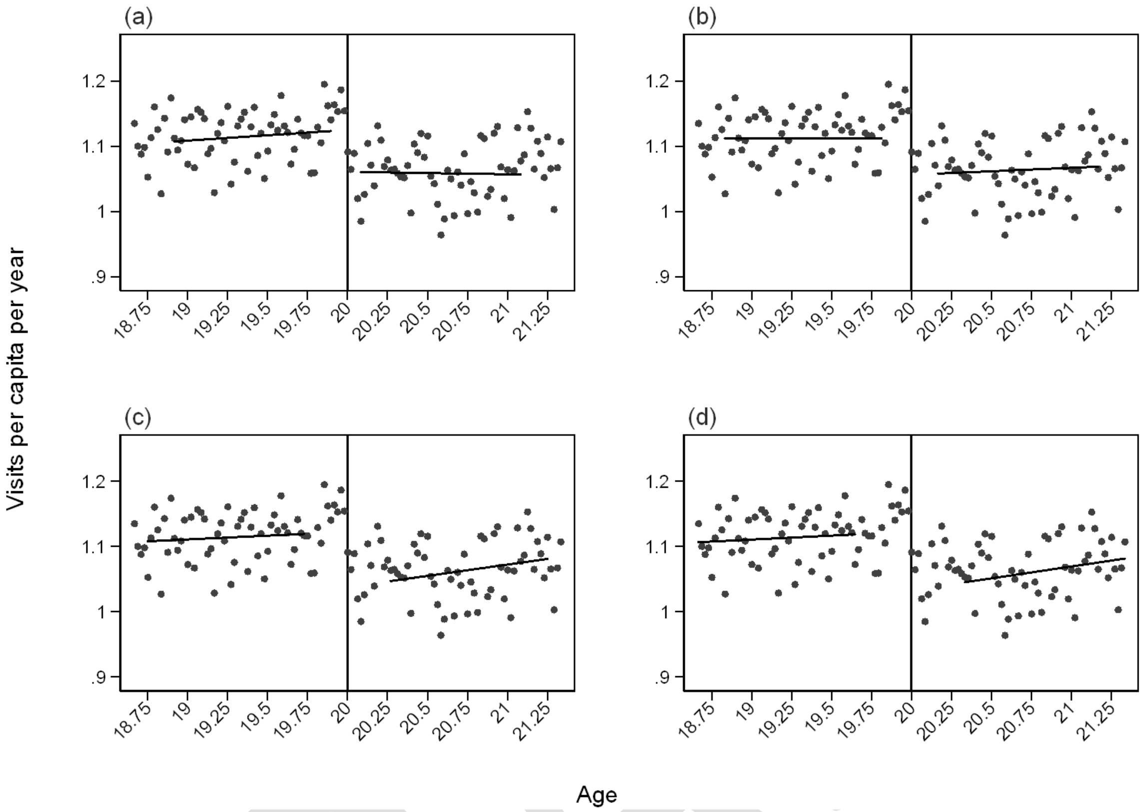
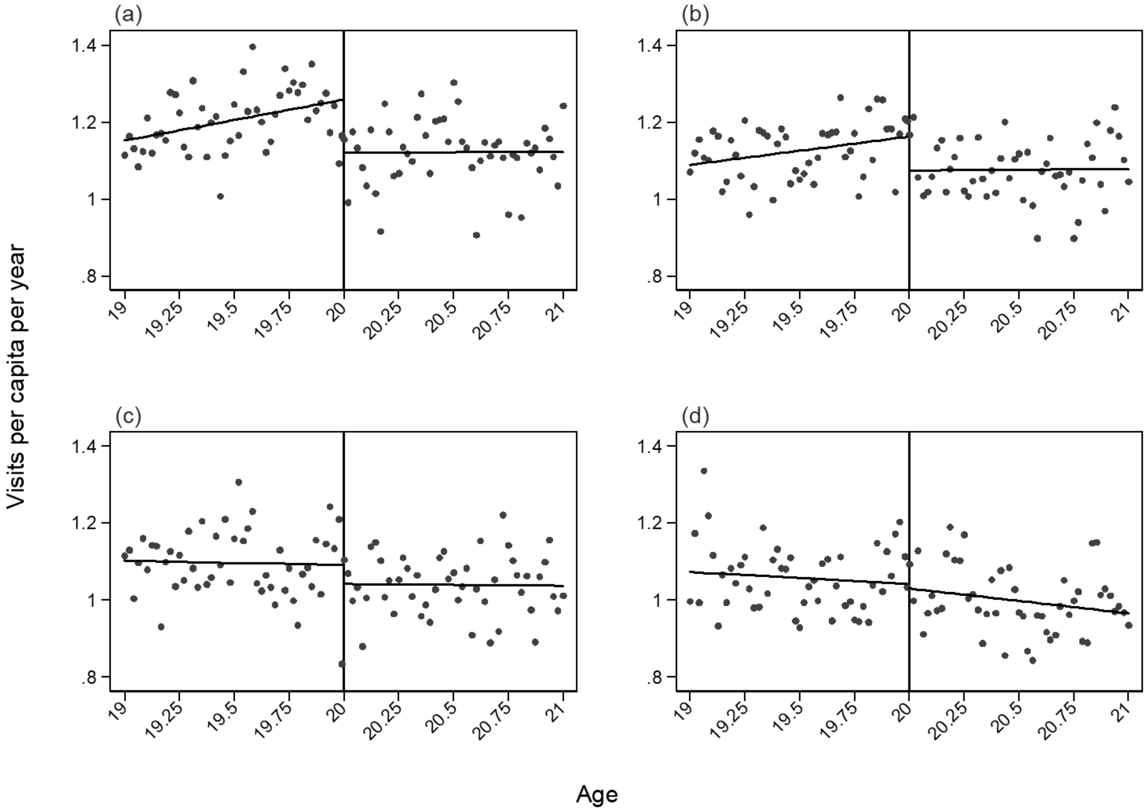


FIGURE 3. – TEST OF ANTICIPATION EFFECT



Notes: Gaps around the threshold of a) ±1 month, b) ±2 months, c) ±3 months, and d) ±4 months.

FIGURE 4. – DISCREPANCIES IN COPAYMENT EFFECTS BETWEEN INCOME GROUPS



Notes: With a) representing 1st income quartile (lowest), b) 2nd income quartile, c) 3rd income quartile, and d) 4th income quartile.

FIGURE 5. – DISCREPANCIES IN COPAYMENT EFFECT BETWEEN WOMEN AND MEN