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The Effect of Copayments on Children's and Adolescents' Demand for Medical Care

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Abstract

We exploit a policy change in Sweden to estimate the effect of copayments on the demand for children's and adolescents' usage of medical care. To this end, we use population-wide registry data including detailed characteristics of individuals and their medical visits. We examine whether the response to prices varies by socioeconomic background or health status. When visits to doctors become free of charge, we find that individuals see a doctor more often. This effect does not represent a substitution away from inpatient care. The size of the response differs by health and income but not by educational attainment.

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1 Introduction

Health insurance plans commonly use patient cost-sharing to address problems of moral hazard. Moral hazard arises when patients seek more medical care or engage in more risky health behaviors when health care consumption is cheaper. While cost-sharing has the potential of reducing unnecessary health care visits or reduce overly risky behaviors, these arrangements may also lead individuals to delay or skip medically necessary treatment, or to substitute away to other, potentially less efficient, forms of treatment.

Policy makers need to ponder these issues when determining the desirable level of cost-sharing in public health insurance plans. While cost-sharing in public health insurance systems is highly debated in general, it is particularly controversial when applied to children and adolescents. This is hardly surprising, since determining the optimal level of cost-sharing is both more difficult and more important in the case of young people. First of all, individuals in childhood or adolescence do typically not decide on their contacts with the medical system themselves. Instead, parents take them to the doctor (or other health care professional) when they deem treatment necessary. It is not clear how parents' preferences for their children's health care compare with their preferences for their own health care. As a consequence, demand price responses for children and youths may be either higher or lower than those for adult's own health care consumption, meaning estimates of adult's price sensitivity will not necessarily apply. Moreover, it is not clear whether parents' demand for their children's health care can be said to be socially optimal or optimal from their children's perspective.

Second, for children and adolescents the effects of forgoing necessary treatment on health and labor market outcomes later in life are likely to be much longer lasting and more severe than for adults.¹ Furthermore, if in particular low-income parents respond to prices of medical care, cost-sharing may lead to worse prospects of those from adverse backgrounds. Since this inequality stems from factors outside of the individual's control, it can be viewed as socially undesirable.

While many studies have estimated the effects of patient cost-sharing on the demand for health care in the population in general (see, in particular, the review by Chandra, Gruber, and McKnight (2010) and the description of the RAND health insurance experiment in Manning et al. (1987)), very little is known about how the demand for children's and adolescent's health care responds to prices. This is probably due to the fact that the study populations in previous studies have been relatively small, making it difficult to estimate effects for different subgroups. Moreover, policy experiments exploited in the literature have typically affected entire populations or families, implying that interactions between parents and children may confound estimates of own-elasticities; for example, if parents are induced to seek preventive care more often, they may bring their kids to see the doctor at the same time.

In this paper, we study the effect of copayments (a per visit fee) on the demand for children's and adolescent's health care by exploiting a copayment exemption for individuals aged 7-19 that was introduced in Swedish Skåne County in 2002. Although copayments in Sweden are generally rather low, it has been documented (Elofsson, Undén, and Krakau 1998) that 22 percent of the (adult) population in Stockholm, Sweden, had forgone seeking care during the past year due to the cost. Before 2002, there was also a general perception among

1. For example, Harris (2001) documents the relationship between child health and adult mortality. Lundborg, Nilsson, and Rooth (2014a) show that health problems in adolescence have large effects on adult labor market outcomes.

pediatricians in Skåne (Hanson, Strömkvist, and Nihlen 2001) that children did not see a doctor as often as they should because of the fees, and that this was especially so among the economically disadvantaged.

Our study not only contributes to the literature by specifically considering the group of children and adolescents, but also by asking whether the price responsiveness to copayments differs by various characteristics including parental socioeconomic status. Due to data constraints, few studies have been able to examine these issues credibly. We ask if effects differ depending on whether the individual has chronic health conditions, and we also ask how the demand for different types of care, such as doctor versus non-doctor visits, and acute versus non-acute visits, is affected.

As we will discuss, the abolition of copayments that we exploit was both unexpected and introduced with short notice. Our analysis employs a difference-in-difference design, where the control group includes either 3-6 years olds (who were always exempted from copayments) or 20-24 year olds (who were never exempted from copayments) and the treatment group includes 7-19 year olds, who were subject to the policy change. It is important to note that patient fees in Sweden are independent of the health care provider and they are not dependent on (own or parental) occupational or labor market status. This lends generalizability to our results since everyone was equally affected. Moreover, since the treatment in our case is determined by age and year, it is not possible to choose treatment status. This lends credibility to our estimates.

We use unique data from Skåne County, covering all medical visits, both inpatient and outpatient, in the years 2001 and 2002. The data include detailed information on type of care as well as the exact date when the medical visit took place. Through the use of personal identifiers, the data have then been linked to administrative registers with information on socioeconomic outcomes.

Our results show that the number of outpatient doctor visits went up by about ten percent when copayments were abolished, but there is little evidence that nurse visits were affected. Moreover, the number of inpatient visits remained constant; suggesting that no substitution from outpatient visits took place. Effects are larger for socioeconomically disadvantaged and for those in worse health.

The rest of the paper is organized as follows. Section 2 provides an overview of previous studies estimating the price sensitivity of health care demand. Section 3 gives information on the institutional setting, on the data we use, and on our econometric approach. In Section 4, we show and discuss our results. Section 5 concludes.

2 Previous work

Estimating the price sensitivity of health care demand has been the subject of numerous studies in health economics, as reviewed in detail by Chandra, Gruber, and McKnight (2007).² Among these studies, the Rand Health Insurance Experiment (HIE) stands out as a widely recognized “gold standard” (Manning et al. 1987; Newhouse and the Insurance Experiment Group 1993; Aron-Dine, Einav, and Finkelstein 2013). Conducted in the 1970s, the experiment randomly assigned families to different insurance plans with different levels of cost-sharing. The results from the HIE were translated into a widely cited overall price elasticity of -0.2 (Keeler and

2. This paper was later published with a more succinct literature review as Chandra, Gruber, and McKnight (2010). For other surveys, see Baicker and Goldman (2011) and Swartz (2010).

Rolph 1988). For our study, three results from the HIE are of special interest: First, the demand for children’s medical care was as price sensitive as the demand for adults’ medical care, albeit less sensitive in the case of inpatient services. Second, sickly patients responded to cost-sharing no less than more healthy patients. Third, low-income families reduced outpatient services more strongly in response to cost-sharing than higher-income families.³ However, potentially different responses by income group in the HIE were attenuated by the presence of income-dependent out-of-pocket caps. Because poor families had lower caps, they were more likely to exceed these caps and enjoy free care for a considerable part of the year. Out-of-pocket caps also exist in the setting that we study. However, they do not depend on income and any differences between income groups are likely to be more pronounced.

The HIE took place over thirty years ago and many structural changes - such as improvements in diagnostic imaging, increasing share of pharmaceuticals in total health care cost, more surgeries performed in inpatient care - have occurred in the medical sector over the decades since then. Moreover, due to differential participation and reporting across experimental arms, the implications of the experiment are relatively uncertain. Aron-Dine, Einav, and Finkelstein (2013) conclude that there is potentially substantial bias in the original estimates based on the HIE. Our paper improves on the HIE by using data that covers the whole population. Moreover, we do not rely on patients reporting visits but use administrative records.

Subsequently to HIE, a number of studies have used policy changes to estimate how health care demand responds to cost-sharing.⁴ Here, we focus on studies that examine the effects of copayments. Cherkin, Grothaus, and Wagner (1989) considered the introduction of a \$5 copayment on office visit rates in a health maintenance organization (HMO) in Washington State in the mid-1980s. Comparing state government employees to federal government employees, who were not affected by the copayments, they found that primary care visits were reduced by 11 percent and specialty care visits by 3 percent. Selby, Fireman, and Swain (1996) examined the effect of the introduction of a \$25 to \$35 copayment for emergency department visits in a California HMO in the early 1990’s. Visits were found to fall by about 15 percent. Chandra, Gruber, and McKnight (2010) studied a policy change affecting a health plan offered to retired public employees in California. In 2002, physician office copayments for HMOs were increased from \$0 to \$10. The authors find that the number of doctor visits fell in response to this fee, and they estimate an elasticity that is somewhat smaller than the one in the HIE. They also document “offset” effects in terms of increased hospital utilization.

For policy purposes, one interesting issue concerns the potential heterogeneity in the response to cost-sharing. Standard economic theory suggests that cost-sharing should be higher for groups that are more sensitive to the price of medical care, since overuse is more of a problem here. Such policies may clash with equity considerations, however, especially if lower-income or disadvantaged individuals are more sensitive to prices.

So far, findings regarding heterogeneity in the response to cost-sharing by socioeconomic

3. This interpretation of the HIE results is not universally accepted. For instance, McGuire (2011) states in his handbook article as regards the HIE: “There was no evidence of differences in demand response by income [...]” (p. 348)

4. Two randomized experiments have also been carried out more recently. Michalopoulos et al. (2011) studied the Accelerated Benefits Demonstration, which provided medical benefits to Social Security Disability Insurance beneficiaries immediately rather than after a 24 months waiting time. Finkelstein et al. (2012) studied the Oregon Health Insurance Experiment which allowed a group of uninsured low-income adults to apply for Medicaid. Both studies showed quite substantial effects on health care utilization for the groups participating.

status are rather mixed. Chandra, Gruber, and McKnight (2007) report similar effects of copayments on retired public employees belonging to different income groups. Trivedi, Moloo, and Mor (2010) in contrast, also studying the elderly, find that copayments in Medicare plans led to larger reductions in outpatient care for enrollees with low income. Focusing on the impact of copayments on emergency department use, Selby, Fireman, and Swain (1996) and Hsu et al. (2006) both document larger declines in visits for patients from low socioeconomic status neighborhoods. In a follow-up paper to their study from 1989, Cherkin, Grothaus, and Wagner (1992) find that their earlier estimates do not vary across income groups. Other work investigates how enrollees in programs specifically targeted at the poor, such as Medicaid, respond to copayments, mostly copayments for drugs (Reeder and Nelson 1985; Stuart and Zacker 1999). More recently, for instance, Chandra, Gruber, and McKnight (2014) exploit exogenous increases in copayments in the Massachusetts Commonwealth Care program designed for non-elderly adults with low income up to 300% of the federal poverty line. They compute the weighted average of copayments for various medical services, including drug prescriptions and office visits, and find an elasticity of total spending of -0.16, quite close to the one from the HIE for a higher income population. Baicker and Goldman (2011) survey the literature and conclude that “evidence to support the contention that low-income groups are more price sensitive is suggestive, but seems less than fully reliable” (p. 58).

It should be noted that each of the above studies suffers from at least one of the following limitations: First, individual income information is typically not available and has to be proxied by regional indicators based on census block group or ZIP code. This induces measurement error. Second, most studies exploit changes in health insurance plans in which either only employed (i.e., higher-income) patients or only low-income patients (e.g. in Medicaid) are enrolled. As a consequence, estimates for high-income and low-income groups are obtained from differential settings, which complicates comparisons. Some studies investigated differential impact by income within the group of employed enrollees, but excluding the unemployed likely yields weaker differences in responsiveness. This paper addresses both of these problems: First, we use administrative data with precise income information on the individual level and second, because public health insurance in Sweden covers the whole population, we can make comparisons across all income groups.

Only a few studies have considered heterogeneous responses by other characteristics, such as health status or race/ethnicity. As for income, our study has the advantage that the sample is not restricted to certain populations and our data is based on individual-level registers. Response heterogeneity by health status is of particular interest because the very sick patients account for a large part of overall health care expenditures. If the more sickly are less price-sensitive than the average patient and if visits by more sickly patients are more costly, then any cost increase due to the abolition of fees will be overestimated when calculated from the average patient’s demand elasticity. In contrast, a stronger sensitivity of the sickly will imply an underestimation of additional cost (Remler and Atherly 2003).

In the presence of an out-of-pocket cap on copayments, one might expect sickly individuals to exceed the number of visits implied by this limit regardless of whether or not copayments are charged. As a consequence, if sickly patients are forward-looking enough, they should respond little to cost-sharing. Finding sickly patients to be as responsive as healthy patients, as in the HIE, therefore implies that “at the margin the sickly exhibit more discretion” (Manning et al. 1987, p. 263). One explanation for this would be that sickly individuals may even over-use medical care, and can reduce the frequency of their visits to a larger extent. In

fact, most other evidence even finds a larger impact on the sickly: Chandra, Gruber, and McKnight (2010) and Trivedi, Moloo, and Mor (2010) both document larger reductions in outpatient visits for elderly with more severe or chronic health problems. Studying the non-elderly population, Cherkin, Grothaus, and Wagner (1989) estimate a larger effect for patients with 10 or more visits in the year before the reform. An exception is Chandra, Gruber, and McKnight (2014), who found somewhat smaller effects for those with chronic conditions such as asthma and diabetes. But all of these studies lack the existence of an out-of-pocket cap, making comparisons with our setting difficult.

As for heterogeneity by race and ethnicity, studies by Trivedi, Rakowski, and Ayanian (2008) and Steinman, Sands, and Covinsky (2001) provided evidence pointing in contrasting directions. However, since their studies were not based on reforms or policy experiments, results are less credible. An exception is Trivedi, Moloo, and Mor (2010), who found that Blacks - unlike Whites - increased their number of outpatient visits in response to cost-sharing. But at the same time, Blacks also make much more use of inpatient care than Whites.

This paper looks at children and adolescents in isolation, whereas other studies break the analysis down into children and adults. Selby, Fireman, and Swain (1996) find a larger decline in visits only for children aged 1-5 years, but not for those aged 6-18 years, the age group we study in this paper. Cherkin, Grothaus, and Wagner (1989) also arrive at similar estimates on average, with girls being more responsive than boys. Overall, and in line with HIE, the literature does not indicate that children have a stronger or weaker response to copayments than adults. If anything, this suggests that our results may have an external validity beyond the age group we are studying in this paper.

3 Data and Method

3.1 Institutional Setting

Skåne County, the southernmost county in Sweden, provides universal and relatively cheap health care to all of its about one million inhabitants. As is typical in Sweden, primary care in Skåne is to a large extent provided by health care centers that offer all types of ambulatory treatment. Rural communities usually have one health care center, while larger cities have several ones. Outpatient care by specialists is provided at hospitals. Most health care is public, but some providers are private. Hospitals, which are almost all public, also supply inpatient care. Here, “inpatient care” refers to all medical contacts that involve at least one overnight stay.

Copayments are charged for several medical services, such as visits to a doctor. The levels of the copayments are determined by the Skåne Regional Council, whose members are directly elected every four years.⁵ Private health insurance is uncommon in Sweden.⁶ As a consequence, everyone that lives in the same county is essentially exposed to the same health care copayment structure.

By the year 2001, all individuals aged seven or above were charged the same copayments

5. The first election to Skåne Regional Council was held in 1998 and the council was then established in the beginning of 1999, following the merger of Malmöhus County and Kristianstad County. Its main responsibilities include health care and public transportation.

6. In the year 2000, only 103 000 Swedes had private health insurance, corresponding to 1.1 percent of the population (Finansdepartementet 2008).

for outpatient care in Skåne.⁷ The copayment amounted to SEK 100 (approximately €10 euros in 2001) for seeing a general practitioner at a health care center as well as for seeing a specialist at a hospital after referral. For visits to a specialist without a referral, visits during out-of-office hours or visits to the emergency department, individuals were charged SEK 200. Visiting a nurse outside primary care or visiting certain other types of health care professionals, such as physical therapists or speech-language pathologists, was charged SEK 80. Nurse visits in primary care were free of charge, which meant that most nurse visits were not charged. So-called medical services, including X-ray examinations were also free of charge. For individuals aged below seven, fees were not charged at all during this time period.⁸

In the end of October 2001, the left-wing opposition in the Skåne Regional Council put forward the proposal to abolish all copayments for individuals aged 19 and below. Unexpectedly, the proposal was accepted, as a result of two members of the right-wing majority accidentally pressing the wrong button (Hanson, Strömkvist, and Nihlen 2001). It came in effect in January 2002. In addition to the abolition of copayments for children and adolescents, the decision involved some minor changes to the fee structure, such as free nurse visits in psychiatric care for individuals above age 18, provision of free contraceptives for individuals aged 20 and below, and abolishing the practice of not charging individuals that had to wait more than 30 minutes for an acute visit (Regionfullmäktige Skåne 2001). During the entire time period we study, there was an out-of-pocket cap on fees implying that no individual had to pay more than SEK 900 a year for outpatient services. This applied to adults' health care, as well as to children's and adolescents' during the years when these were subject to charge. For inpatient care, children and adolescents paid no fees during the time period we study (individuals above 24 were charged).

In Table 1 we show the distribution of fees actually paid by individuals in the sample, before and after the policy change. In accordance with the rules described above, children aged 3 to 6 years virtually never paid any fee throughout the study period. In 2001, the distribution of fees paid by individuals aged 7 to 19 years closely resembles the one for individuals aged 20 years and higher. In 2002, the abolition of fees for individuals aged 7 to 19 years manifests itself in a share of zero fees close to 100 percent for this age group, while their older counterparts continue to pay about the same fees as before.

3.2 Data and Sample

Our dataset contains the universe of contacts with the medical sector in the Swedish county of Skåne between 1999 and 2008. It combines the two "patient administrative register systems" PASiS and PRIVA that are administered by the Regional Council of Skåne.⁹ PASiS contains all publicly provided care, while PRIVA contains all privately provided care. In our empirical analysis, we do not maintain the distinction between public and private care and treat records from both registers equally.

The dataset includes an extensive range of information about each medical contact. We know whether a visit was classified as acute or non-acute, we observe the fee that the patient

7. There were only two exceptions from this rule: Psychiatric care and habilitation for individuals with disabilities were free of charge up until age 18 (Regionfullmäktige Skåne 2000).

8. A number of medical services were generally exempt from fees, including amongst others dialysis treatment and acute treatment of alcohol and drug abuse.(Regionfullmäktige Skåne 2000).

9. Kristensson, Hallberg, and Jakobsson (2007) have used these registers before.

was charged for it and all diagnoses that the patient was given. For outpatient care, we can identify the specific caregiver as either doctor, nurse or other non-doctor. Throughout the study, we focus on real visits to the medical system and ignore contacts via mail, telephone etc. Table 2 reports the average number of visits per month. 3 to 6 year old individuals have 0.27 doctor visits per month, corresponding to approximately 3 visits per year. For older individuals, this number reduces to 2 visits per year. Other outpatient visits are rarer, but exhibit a similar pattern across age groups. Acute visits represent less than half of all visits, with a lower share for doctor than for non-doctor visits.

We merge this health data with another Swedish administrative dataset that contains a variety of socio-economic and demographic variables. This dataset covers all persons born in Sweden between 1940 and 1985, as well as their parents, and all their children. It has been constructed from a number of different registers, such as the Medical Birth Register and - most importantly - the so-called LISA register. The LISA register contains annual information on income by type, as well as data on education, marital status, and many other variables for all individuals aged 16 years and above. We identify a child’s mother and merge her socioeconomic information from the LISA register.¹⁰ We focus on mothers instead of fathers because children are arguably more likely to live with their mothers than their fathers and mothers may be more responsible for a child’s health care utilization.

We will restrict attention to one year before and after the policy change (2001 and 2002). In principle, it is possible to extend this time period in both directions. However, the Skåne health care system has been undergoing continuous transformation since its establishment in 1998, and some changes are potentially unknown to us. By limiting the time period to two years, we thus reduce the risk that our estimates capture something other than the treatment effect of interest.

As the control group in our Difference-in-Differences (DiD) framework, we choose the 3-to-6-year-olds, who were exempt from fees during the whole study period, and the 20-to-24-year-olds, who had to pay throughout. We report results separately using these alternative control groups. Our sample consists of everyone aged 3 to 24 years that lived in Skåne for at least one month in 2001 or 2002. We are left with about 290,000 individuals with about 22 months of observation on average. Table 3 shows characteristics both of the child or adolescent and the mother, some of which tend to differ between the abolition and control groups. We control for all of the characteristics in all regressions.

3.3 Econometric Method

We estimate the following equation in a standard Difference-in-Differences (DiD) framework:

$$(1) \quad Y_{it} = \alpha + \beta Abolition_{it} + \delta_t + \lambda_{it} + \kappa_{it} + X'_{it}\theta + \varepsilon_{it}$$

where Y_{it} is an individual’s i outcome Y in month t , α a constant, and $Abolition_{it}$ an indicator for whether the individual was treated by the abolition of copayments in that month; that is, the individual was between 7 and 19 years old and the year was 2002. β is the effect on the treatment group and δ_t , λ_{it} and κ_{it} are month fixed effects, age (in months) fixed effects and treatment group specific month of the year fixed effects, respectively.¹¹ X_{it} is a vector

10. We assign the adoptive rather than the biological mother, if applicable.

11. Treatment group specific month of the year fixed effects control for the varying degree of seasonal fluctuations across treatment and control groups.

of socioeconomic controls, including sex, birth weight, mother’s education category, mother’s unemployment status, mother’s income category, mother’s marital status, mother’s age and mother’s country of birth and number of children below 20. ε_{it} is an error term. We cluster standard errors at the person level to account for serial correlation within persons over time.¹²

4 Results

4.1 Total Visits

Before beginning our econometric analysis, we plot the number of visits of different types in 2001 and 2002: doctor visits, nurse visits, other outpatient visits, and inpatient visits. We show the raw number of monthly visits for all ages between 3 and 24. Figure 1 suggests a downward trend in the number of doctor visits, as both control groups reduce their number of visits from 2001 to 2002. For the treatment group, however, there is a clear increase in the number of visits. The shift is very similar across age groups.¹³

Figure 2, 3 and 4 show the average numbers of nurse visits, other outpatient visits and inpatient visits. For these types of care, fees were generally charged neither before nor after the reform. There are few visible differences between the years, but there is some indication that 7-19-year-olds increased their number of nurse visits compared to the treatment group.

Turning to our econometric analysis, panel A in Table 4 shows how the average number of monthly visits was affected by the abolition of fees. We distinguish between doctor visits, nurse visits, other outpatient non-doctor visits, and inpatient visits. The table also displays percentage changes and arc-elasticities, calculated based on these estimates.¹⁴

Using the control group of 3-6-year-olds, our findings suggest that the fee abolition increased doctor visits by 0.014 per month. This is roughly a 9 percent increase and it translates to an arc-elasticity of -0.041. Compared to doctor visits, one might expect a much smaller effect on nurse visits since these were most often free of charge both before and after the policy change was implemented. In line with our graphical analysis performed earlier, we find a small absolute effect of for nurse visits, amounting to only 0.003 more visits per month. Interestingly, however, the relative change is found to be almost as large as for doctor visits since the number of nurse visits is much smaller.

Furthermore and as perhaps suggested by Figure 3 there is no significant evidence that other non-doctor visits would have been affected by the reform. There is also no evidence that fees for outpatient services would have had spill-over effects on inpatient visits. This is in line with the findings from the HIE but differs from those of Chandra, Gruber, and McKnight (2010) and Trivedi, Moloo, and Mor (2010) who focused on populations of elderly people.

Changing the control group to individuals between 20 and 24 years of age produces similar

12. We also tried clustering at the mother level to correct for within family correlation in error terms. Results were practically unchanged.

13. The exception is 7-year-olds, who did not increase their number of visits when the reform was introduced. It has to be noted, however, that they do increase their number of visits compared to the control groups and that effects for specific age groups are subject to noise. Using a regression with age-specific treatment effects, it cannot be rejected that the response of 7-year-olds differs from that of other age groups.

14. Regular elasticities cannot be calculated since we are dealing with prices equaling zero. Arc-elasticities are calculated as $\frac{Q_2 - Q_1}{(Q_1 + Q_2)/2} / \frac{P_2 - P_1}{(P_1 + P_2)/2}$, where Q_1 is the pre-reform mean, $Q_2 - Q_1$ corresponds to β in Equation 1 and $P_2 = 0$. Standard errors of arc-elasticities are computed using the delta method.

results. In particular, the effect on doctor visits reduces only slightly to 0.012, suggesting a relative increase by 7 percent. The effect on nurse visits reduces by half and becomes statistically insignificant, but the confidence interval is wide and it cannot be rejected that the effect is as large as the one obtained when using the younger control group. For other non-doctor visits and for inpatient visits, there is again no evidence of an effect of the reform.

Panel B then shows effects along the extensive margin, that is, on the probability of having at least one medical visit of a certain type during the year. Independently of the control group used, results suggest that the probability of having at least one doctor visit increased by between 1 and 2 percentage points when fees were abolished. While this effect is not large, it shows that our results in Panel A are not entirely driven by children and adolescents that in any case would have gone to the doctor at some point during the year. The effect on nurse visits is of the same size as the one on doctor visits when using the control group of individuals aged 3 to 6 but only about half as large when using the older control group. There is no evidence that the probability of seeing other non-doctors or using inpatient care was affected by the reform.

4.2 Dynamics

We next explore the dynamics of the effects on visits when fees were abolished, that is, if effects set in immediately or rather after some time. There are two reasons why it may take time for full effects to set in. First, people have to become knowledgeable about the fee abolition. Since the reform was decided on only a few months in advance, the word probably had not spread to everyone at the time of implementation. Second, since waiting times may be quite long, some individuals visiting health care in the beginning of 2002 probably sought care already before the reform was implemented, and based their decision to seek care on a different fee structure.

In Figure 5 we show the dynamics of treatment effects for doctor visits. We use dummies to indicate each month, and normalize December 2001 to zero.¹⁵ Defining the control group to consist of individuals between 3 and 6 years of age, the figure suggests that the effect of treatment sets in only after about three months. The pattern looks somewhat different when defining the control group to consist of individuals aged 20 to 24. Here, the treatment effect sets in immediately. There is evidence of an increasing trend in the treatment effect however, suggesting that it may have taken some months for the full effect to set in.

Figure 6 shows the dynamics of the treatment effect for nurse visits. Estimates are much less precise than the corresponding ones for doctor visits, but there is some evidence that effects may have set in only after a few months when using the control group of individuals aged 3 to 6. In line with our previous estimates, there is little evidence that nurse visits would have been affected by the reform at all when using the older control group. Also, Figures 7 and 8, which show dynamic effects for other non-doctor visits and inpatient visits, suggest no effects at all independently of the control group.

In Table 5 we report results based on the assumption that the treatment effect is delayed and only sets in after some time. We have thus redefined the abolition variable to equal zero

15. We only consider six months before and six months after the policy change since we would otherwise not be able to identify treatment group specific month of the year fixed effects. We normalize December 2001 to zero, because all estimated effects are relative to the months January-June 2001 and July-December 2002, but we are interested in effects relative to pre-reform months.

in the first three months of 2002. As can be seen in the table, this does not make much of a difference from our main results. For doctor visits, the arc-elasticity increases in absolute value from -0.041 to -0.044 when using the younger control group, and from -0.035 to -0.047 when using the older control group. For nurse visits, there is essentially no difference from the main results and the effect is still insignificant when using the older control group. As before, there is no evidence that other non-doctor visits or inpatient visits would have been affected by the reform. Since all our results are almost unaffected by whether we redefine our reform indicator for the first months of 2002, we follow our first approach in the rest of the paper and keep the reform indicator to equal one throughout 2002.

4.3 Acute vs. Non-Acute Visits

For outpatient doctor and nurse visits, our data allows us to distinguish between acute and non-acute visits, and we run our analysis on these two types of visits separately. An acute visit is defined as a visit where the time interval between the point when the individual initiated a contact with the health care system and the appointment is less than or equal to 24 hours. A non-acute visit is thus a visit for which this time interval is more than 24 hours.

In general, one may expect non-acute visits to be more sensitive to copayments because they are likely to represent less severe health problems. It is also possible, however, that minor health problems that tend to fade away over time would not force individuals to seek non-acute but only acute health care. For these problems, elasticities may also be lower. Calculating elasticities based on HIE, Keeler and Rolph (1988) found no clear evidence that acute visits would be either more or less price sensitive than other visits.

Our results for acute and non-acute visits are shown in Table 6. Both in terms of absolute and relative changes, our findings for doctor visits clearly suggest larger effects on non-acute than on acute visits. Both control groups produce similar results and the number of monthly acute visits is found to increase by about 0.004 when fees are abolished, which corresponds to an arc-elasticity of about -0.03. The number of non-acute doctor visits increases by as much as 0.009, which corresponds to an arc-elasticity between -0.04 and -0.05. The absolute effects are clearly different between acute and non-acute visits, and the relative effects are almost significantly different at the 1 percent level when using the older control group.

For nurse visits, it is more difficult to draw conclusions because results differ quite a lot depending on the control group chosen. Using the younger control group, we find a larger absolute effect for non-acute visits, but the relative effect is the same. Using the older control group on the other hand, the effect is only significant for acute visits.

As before, we also look at the effect on having at least one visit during a year. This produces qualitatively similar conclusions as for the number of visits. In particular, effects are clearly larger for non-acute than for acute visits when considering doctor visits. The abolition of fees led to a one percentage point larger probability of having at least one acute doctor visit, and a two percentage point larger probability of having at least one non-acute doctor visit. For nurse visits, one specification suggests that acute visits increased by one percentage point whereas non-acute visits were unaffected. The other specification suggests the opposite however. Throughout, there is no evidence that the number of acute or non-acute visits to other non-doctors or to inpatient care, or the probability of having at least one visit to these

types of care, would have been affected by the abolition of fees.¹⁶

4.4 Visits by Characteristics

In this subsection, we examine if effects differ by family characteristics such as socioeconomic background. We focus on doctor visits because our analysis so far suggested that they were more strongly affected by the reform than other types of medical services. Here, we only use the younger control group, since parental characteristics are probably less relevant for adult individuals aged 20-24, who might no longer live together with their parents. First, in Table 7, we split the sample by maternal education. Theoretically, education may affect the response to the reform in either direction. On the one hand, higher educated mothers can be assumed to appreciate the value of health care better, especially with respect to children, and therefore react less strongly to financial incentives. Goldman and Smith (2002), for instance, showed that the more educated patients are more likely to adhere to the therapy of chronic conditions. Also, higher educated individuals tend to have higher incomes, which may lead to a smaller sensitivity to copayments since budget restrictions are less important. We return to the income dimension shortly.

On the other hand, higher educated women may also be the ones who are better informed about the policy change through a larger use of newspapers and other media. Put another way, individuals unaware of the reform do probably not respond to it. Higher educated mothers may also have healthier children (Lundborg, Nilsson, and Rooth 2014b), which may lead to a different response to copayments. We return to the issue of differential responses by health in the next subsection.

Table 7 provides no clear evidence that effects would differ depending on the mother's level of education. For the number of visits, effects are somewhat smaller for higher educated, but differences are not significant. The picture is also mixed when looking at the probability of having at least one visit. There is some indication that the effect is smaller if the mother has secondary education rather than elementary education.

In Table 8, we show results after splitting the sample according to family income. Here, the evidence clearly points in the direction of smaller effects for those with higher income. This is in line with the idea that budget constraints are more important when lower-income households decide on their consumption of medical care. In the lowest income quartile, visits increased by 0.023 in response to the reform (an increase by 14 percent), in the second and third by 0.014 (increases by 9 and 8 percent) and in the highest by only 0.008 (5 percent). Low-income households thus responded three times as much to the reform as high-income households, and this difference is almost significant at the 1 percent level. For the probability of having at least one visit, there is no clear pattern and there is in general not enough power to obtain significance.

In Table 9 we ask if effects differ by mother's country of origin. Such differential effects could be due to many factors, such as culture, resources, health, and knowledge. We distinguish between Sweden, EU-15/North America, other European countries, and other non-European countries. There is evidence that those from other non-European countries are more sensitive to fees, especially for the probability of having at least one visit. Despite probably having more limited knowledge about the Swedish society and its policy issues, the probability

16. These results are available upon request.

of having at least one visit to a doctor responds eight times as much in the group of individuals with a mother born in other non-European countries, as in the group of individuals with a mother born in Sweden. When we split the sample by income, our findings remain qualitatively unaltered, indicating that they are not just driven by differences in socioeconomic status (results not shown).

4.5 Visits by Health Status

Our administrative claims data unfortunately does not provide a readily available indicator for health status at a certain point of time. To identify sickly individuals in our sample, we instead look at the presence of chronic conditions based on diagnoses and at high pre-reform health care utilization.¹⁷ We define individuals as suffering from a chronic disease if they were diagnosed with it at least once during the three years preceding the policy change. We study the following conditions that are common among children: “Allergic Rhinitis” (ICD-10-Codes J30-39), “Asthma” (J40-47) and “Mental disorders” (F90-98).¹⁸ Pre-reform utilization is measured as the average number of monthly doctor visits in 2001 - the year before the reform - and ranked among individuals of the same age.¹⁹ The results are presented in Table 10.

Columns 1-4 compare sickly and non-sickly individuals in the total sample. Here, “sickly” refers to having the given condition in panels A-C, and to belonging to the top quintile users of doctor visits in Panel D as opposed to belonging to all others. As expected, individuals classified as sickly see the doctor more often than their non-sickly counterparts. For patients with chronic conditions, the number of monthly visits in 2001 is roughly twice as high. In Panel D, the visits ratio of top-quintile individuals to all others amounts to 7:1. Corresponding to their higher pre-reform utilisation, sickly individuals exhibit a significantly larger absolute increase in the number of visits than non-sick individuals due to the reform. An exception are mental disorders, which show no clear pattern, but note that the number of sickly is much smaller so that standard errors become large. The response by the sickly is also larger in relative terms. The differences are particularly pronounced and sometimes statistically significant when using the older control group.

One might conjecture that the higher response of the sickly is driven by those individuals with a low family income. For this reason, we look at low-income (=bottom quintile) individuals separately in columns 5-6, as opposed to all other individuals in columns 7-8. First note that the 2001 number of visits of both the sickly and non-sickly only varies little with income across all panels. In contrast, treatment effects vary considerably with income. Among low-income families the sickly are only moderately price-sensitive as compared with the non-sickly in relative terms. The opposite is true for higher-income individuals. Here the sickly have sizable responses and the non-sickly show only a small response. Non-sickly individuals in the higher-income group show no response to copayments at all when defining sickly/non-sickly in terms of pre-reform doctor visits.

To summarize the findings in Table 10, first, the sickly respond more to the abolition of

17. We also proxied current health by indicators of adverse health at birth (low birth weight, low APGAR scores), but resulting samples were small and standard errors large, so no conclusions could be drawn.

18. We only include behavioral and emotional disorders since other mental conditions are relatively uncommon in children. As noted, psychiatric care was free for children and youth throughout the time period, so differential effects by these conditions are less expected but may reflect other needs of these individuals.

19. In both cases, we restrict attention to individuals living in Skåne continuously in 2001 and 2002.

copayments than the non-sickly, but only because higher-income non-sickly patients respond very little. This is evidence that sickly patients indeed have discretion over their health care utilization, even among non-poor individuals. However, second, our finding that non-sickly low-income children have a much larger (relative) response than sickly low-income ones suggests that this discretion is limited. An alternative explanation is that the presence of an out-of-pocket cap effectively weakens the price-responsiveness of the chronically ill. Third, large differences in responsiveness by income reported in Table 8 are driven by the non-sickly individuals.

5 Conclusion

In this paper, we exploited a policy change in Sweden to study how copayments for medical visits affect children's and adolescents' usage of health care. The effects of cost-sharing on young patients is particularly interesting because these individuals are typically not yet decision-makers for themselves and forgoing treatment in response to copayments may have larger and longer-lasting health consequences. Yet, there is little previous evidence on if, and how, the health care demand of young individuals responds to cost-sharing.

We find that visits to doctors increased when copayments were abolished and that the shift was similar across age groups. In 2001, the average fee for doctor visits paid by individuals between 7 and 19 years of age was SEK 100 (€10), and its abolition implied a 9 percent increase in overall doctor visits. For nurse visits and other types of visits to outpatient care where copayments were usually not even charged before the policy change, there is no consistent evidence that visits were affected. Moreover and in contrast with some recent evidence based on elderly (Chandra, Gruber, and McKnight 2010; Trivedi, Moloo, and Mor 2010), there is no evidence of substitution with inpatient care.

In addition to establishing the overall response to copayments among children and adolescents, one of our most important contributions is showing that responses vary by family income. Few studies of health care demand have been able to credibly explore differential responses by income. Interestingly, we do not find differential responses by education, suggesting that resources rather than knowledge or social class may explain these findings. Our results by health show that responses vary by interactions of health and income, and that sickly individuals are overall more sensitive to copayments than more healthy ones.

If demand responses largely reflect moral hazard and over-use of health care, economic theory suggests that those with a higher elasticity should pay higher copayments, which in our setting means higher copayments for lower-income families (e.g. McGuire 2011). But higher responsiveness of lower-income families might simply result from tighter budget constraints. Concerns about under-use have therefore motivated proposals to charge lower copayments for poor patients (Rice and Thorpe 1993).

Our study does not provide conclusive evidence on this point. We observe that poor patients have a higher number of visits already before copayments were abolished. But this is not necessarily suggestive of over-use, because it is well-known that the poor on average suffer from worse health. Thus more research is needed to identify the underlying causes of differential responsiveness by income and to inform health policy. Relatedly, an important avenue for future research is to determine if an increased usage of medical care has positive long-run effects in terms of health, and in turn perhaps school results and economic outcomes.

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6 Tables and Figures

Table 1: Changes in fees

	Fee	3-6 Years		7-19 Years		20-24 Years	
		2001	2002	2001	2002	2001	2002
<i>Doctors</i>	0	99.5	99.6	25.5	99.1	30.3	28.5
	50	0.0	0.0	0.2	0.0	0.4	0.3
	60	0.0	0.0	0.0	0.0	0.1	0.0
	80	0.0	0.0	0.3	0.0	0.2	0.3
	100	0.1	0.0	37.6	0.1	35.2	36.3
	200	0.1	0.0	35.1	0.2	32.0	32.3
<i>Nurses</i>	0	99.1	99.1	90.2	98.2	88.7	90.5
	50	0.0	0.0	0.0	0.0	0.0	0.0
	60	0.0	0.0	0.1	0.0	2.8	2.9
	80	0.1	0.0	7.5	0.1	6.5	4.9
	100	0.0	0.0	0.0	0.0	0.0	0.0
	200	0.0	0.0	0.0	0.1	0.1	0.2
<i>Other Non-doctors</i>	0	99.9	99.9	81.4	99.8	63.5	61.5
	50	0.0	0.0	0.0	0.0	0.1	0.0
	60	0.0	0.0	0.1	0.0	0.2	0.2
	80	0.0	0.0	18.0	0.1	34.8	36.1
	100	0.0	0.0	0.1	0.0	0.7	0.9
	200	0.0	0.0	0.0	0.0	0.0	0.0

Notes: Percentages of most frequent fees in 2001 (pre-reform) and 2002 (post-reform) by age group and type of caregiver.

Table 2: Means of monthly visits (by type and caregiver)

	3-6 Years		7-19 Years		20-24 Years	
	2001	2002	2001	2002	2001	2002
<i>Total Visits</i>						
Doctor	0.272	0.268	0.167	0.176	0.191	0.189
Nurses	0.083	0.082	0.042	0.043	0.091	0.090
Inpatient	0.005	0.006	0.005	0.005	0.010	0.011
Other Non-Doctors	0.091	0.092	0.059	0.061	0.072	0.074
Outpatient	0.446	0.441	0.268	0.280	0.353	0.353
<i>Acute Visits</i>						
Doctor	0.120	0.116	0.069	0.070	0.074	0.072
Nurses	0.012	0.011	0.009	0.008	0.013	0.011
Inpatient	0.003	0.004	0.003	0.004	0.008	0.008
Other Non-Doctors	0.001	0.001	0.001	0.001	0.001	0.002
<i>Non-Acute Visits</i>						
Doctor	0.152	0.152	0.097	0.106	0.116	0.117
Nurses	0.071	0.071	0.033	0.034	0.078	0.079
Inpatient	0.002	0.002	0.002	0.002	0.003	0.003
Other Non-Doctors	0.090	0.091	0.058	0.059	0.070	0.072
<i>New Visits</i>						
Doctor	0.115	0.110	0.069	0.069	0.081	0.077
Nurses	0.030	0.030	0.014	0.014	0.026	0.024
Other Non-Doctors	0.009	0.009	0.005	0.005	0.006	0.005
<i>Revisits</i>						
Doctor	0.093	0.091	0.060	0.063	0.063	0.064
Nurses	0.052	0.050	0.027	0.027	0.060	0.061
Other Non-Doctors	0.080	0.080	0.043	0.042	0.033	0.035

Table 3: Descriptives of socioeconomic data (by age group)

	3-6 Years			7-19 Years			20-24 Years		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
Year	2,001.49	0.50	955,128	2,001.50	0.50	3,838,233	2,001.50	0.50	1,459,329
Sex	0.49	0.50	955,081	0.49	0.50	3,838,197	0.50	0.50	1,459,329
Age	4.56	1.12	955,128	12.86	3.67	3,838,233	22.02	1.41	1,459,329
Year of Birth	1,996.48	1.34	955,128	1,988.20	3.73	3,838,233	1,979.03	1.57	1,459,329
Mother's Age	34.70	4.94	955,128	41.95	6.11	3,833,205	50.24	5.06	1,455,022
<i>Mother's Education</i>									
Elementary Schooling	0.10	0.30	953,904	0.14	0.35	3,810,467	0.20	0.40	1,413,157
Secondary Schooling	0.60	0.49	953,904	0.59	0.49	3,810,467	0.53	0.50	1,413,157
University	0.30	0.46	953,904	0.27	0.45	3,810,467	0.27	0.44	1,413,157
<i>Family Income</i>									
1st Quartile	0.23	0.42	953,831	0.25	0.43	3,811,475	0.26	0.44	1,413,387
2nd Quartile	0.31	0.46	953,831	0.23	0.42	3,811,475	0.21	0.41	1,413,387
3rd Quartile	0.27	0.45	953,831	0.26	0.44	3,811,475	0.21	0.40	1,413,387
4th Quartile	0.19	0.39	953,831	0.25	0.43	3,811,475	0.32	0.47	1,413,387
<i>Mother's Country of Birth</i>									
Sweden	0.95	0.23	955,128	0.91	0.28	3,829,418	0.89	0.32	1,444,066
EU-15, North America	0.02	0.15	955,128	0.04	0.19	3,829,418	0.05	0.21	1,444,066
Other Europe	0.02	0.12	955,128	0.03	0.18	3,829,418	0.05	0.21	1,444,066
Other Countries	0.02	0.13	955,128	0.02	0.14	3,829,418	0.02	0.14	1,444,066
<i>Mother's Marital Status</i>									
Married	0.58	0.49	955,128	0.64	0.48	3,815,043	0.67	0.47	1,415,524
Single, Married Before	0.08	0.27	955,128	0.19	0.39	3,815,043	0.25	0.43	1,415,524
Single, Never Married	0.34	0.47	955,128	0.18	0.38	3,815,043	0.08	0.26	1,415,524

Continued on next page

Number of Children Under 20

No Child	0.00	0.00	955,128	0.00	0.00	3,833,205	0.63	0.48	1,455,022
1 Child	0.19	0.39	955,128	0.20	0.40	3,833,205	0.25	0.43	1,455,022
2 Children	0.54	0.50	955,128	0.46	0.50	3,833,205	0.09	0.28	1,455,022
3 Children	0.19	0.39	955,128	0.24	0.43	3,833,205	0.02	0.15	1,455,022
4 or More Children	0.07	0.25	955,128	0.09	0.28	3,833,205	0.01	0.10	1,455,022

Table 4: Total Visits

	Doctor		Nurses		Other Non-Doctors		Inpatient	
	3-19 years (1)	7-24 years (2)	3-19 years (3)	7-24 years (4)	3-19 years (5)	7-24 years (6)	3-19 years (7)	7-24 years (8)
A. Number of Visits								
Abolition	0.0143** (0.0017)	0.0120** (0.0015)	0.0028** (0.0009)	0.0014 (0.0015)	0.0006 (0.0020)	-0.0007 (0.0019)	-0.0005 (0.0003)	0.0002 (0.0004)
% Change	8.61	7.25	6.72	3.28	1.12	-1.22	-10.55	4.48
Arc-elasticity	-0.041	-0.035	-0.032	-0.016	-0.006	0.006	0.056	-0.022
N	4,611,494	5,043,922	4,611,494	5,043,922	4,611,494	5,043,922	4,611,494	5,043,922
B. Any Visit								
Abolition	0.0113** (0.0031)	0.0166** (0.0029)	0.0129** (0.0032)	0.0057* (0.0026)	0.0001 (0.0023)	0.0004 (0.0019)	0.0005 (0.0014)	0.0021 (0.0014)
Mean	0.6401	0.6401	0.2218	0.2218	0.1243	0.1243	0.0336	0.0336
N	390,923	432,451	390,923	432,451	390,923	432,451	390,923	432,451

Notes: Standard errors clustered at the person level in parentheses. Arc-elasticities are computed using the (pre-reform) 2001 mean of monthly visits. We control for age, month, abolition-group specific seasonal effects, sex, mother's education, mother's unemployment status (benefits), family income, mother's marital status, mother's age, mother's country of birth, number of children under 20, and birth weight. ** and * denote significance at the 1 and 5 percent level, respectively.

Table 5: Total Visits - Delayed

	Doctor		Nurses		Other Non-Doctors		Inpatient	
	3-19 years (1)	7-24 years (2)	3-19 years (3)	7-24 years (4)	3-19 years (5)	7-24 years (6)	3-19 years (7)	7-24 years (8)
Abolition	0.0154** (0.0019)	0.0165** (0.0017)	0.0029** (0.0010)	0.0013 (0.0017)	-0.0010 (0.0021)	-0.0000 (0.0021)	-0.0004 (0.0003)	0.0001 (0.0004)
% Change	9.27	9.95	6.97	3.05	-1.78	-0.04	-8.48	1.78
Arc-elasticity	-0.044	-0.047	-0.034	-0.015	0.009	0.000	0.044	-0.009
N	4,611,494	5,043,922	4,611,494	5,043,922	4,611,494	5,043,922	4,611,494	5,043,922

Notes: Standard errors clustered at the person level in parentheses. Arc-elasticities are computed using the (pre-reform) 2001 mean of monthly visits. We control for age, month, abolition-group specific seasonal effects, sex, mother's education, mother's unemployment status (benefits), family income, mother's marital status, mother's age, mother's country of birth, number of children under 20, and birth weight. ** and * denote significance at the 1 and 5 percent level, respectively.

Table 6: Acute/Non-acute Visits

	Doctor								Nurses			
	Acute		Non-Acute		Acute		Non-Acute		Acute		Non-Acute	
	3-19 years (1)	7-24 years (2)	3-19 years (3)	7-24 years (4)	3-19 years (5)	7-24 years (6)	3-19 years (7)	7-24 years (8)	3-19 years (5)	7-24 years (6)	3-19 years (7)	7-24 years (8)
A. Number of Visits												
Abolition	0.0049** (0.0010)	0.0034** (0.0007)	0.0094** (0.0013)	0.0087** (0.0012)	0.0007* (0.0003)	0.0017** (0.0004)	0.0021** (0.0008)	-0.0004 (0.0014)				
% Change	7.12	4.89	9.67	8.93	7.78	20.31	6.44	-1.12				
Arc-elasticity	-0.034	-0.024	-0.046	-0.043	-0.037	-0.092	-0.031	0.006				
p-value	-	-	0.191	0.013	-	-	0.766	0.001				
N	4,611,494	5,043,922	4,611,494	5,043,922	4,611,494	5,043,922	4,611,494	5,043,922				
B. Any Visit												
Abolition	0.0092* (0.0036)	0.0106** (0.0030)	0.0198** (0.0034)	0.0215** (0.0030)	0.0009 (0.0021)	0.0061** (0.0018)	0.0143** (0.0032)	0.0033 (0.0025)				
p-value	-	-	0.033	0.010	-	-	0.000	0.354				
N	390,923	432,451	390,923	432,451	390,923	432,451	390,923	432,451				

Notes: Standard errors clustered at the person level in parentheses. Arc-elasticities are computed using the (pre-reform) 2001 mean of monthly visits. We control for age, month, abolition-group specific seasonal effects, sex, mother's education, mother's unemployment status (benefits), family income, mother's marital status, mother's age, mother's country of birth, number of children under 20, and birth weight. P-values are from t-tests of the comparison with "Acute" for the same age group. ** and * denote significance at the 1 and 5 percent level, respectively.

Table 7: Mother's Education

	Elementary Schooling	Secondary Schooling	University
	3-19 years (1)	3-19 years (2)	3-19 years (3)
A. Number of Visits			
Abolition	0.0189** (0.0056)	0.0151** (0.0023)	0.0111** (0.0031)
2001 Mean	0.1722	0.1687	0.1575
% Change	10.97	8.94	7.07
Arc-elasticity	-0.052	-0.043	-0.034
p-value	-	0.559	0.300
N	603,959	2,733,811	1,273,724
B. Any Visit			
Abolition	0.0257** (0.0094)	0.0065 (0.0040)	0.0165** (0.0058)
p-value	-	0.060	0.404
Mean	0.6417	0.6467	0.6248
N	50,903	231,741	108,279

Notes: Each column reports results from a separate regression. Dependent variable is Total Doctor Visits. Standard errors clustered at the person level in parentheses. Arc-elasticities are computed using the (pre-reform) 2001 mean of monthly visits. We control for age, month, abolition-group specific seasonal effects, sex, mother's unemployment status (benefits), family income, mother's marital status, mother's age, mother's country of birth, number of children under 20, and birth weight. P-values are from t-tests of the comparison with "Elementary Schooling". ** and * denote significance at the 1 and 5 percent level, respectively.

Table 8: Family Income

	<u>1st Quartile</u>	<u>2nd Quartile</u>	<u>3rd Quartile</u>	<u>4th Quartile</u>
	3-19 years	3-19 years	3-19 years	3-19 years
	(1)	(2)	(3)	(4)
A. Number of Visits				
Abolition	0.0231** (0.0039)	0.0144** (0.0035)	0.0136** (0.0037)	0.0080* (0.0040)
2001 Mean	0.1675	0.1689	0.1675	0.1611
% Change	13.77	8.55	8.09	4.96
Arc-elasticity	-0.064	-0.041	-0.039	-0.024
p-value	-	0.094	0.078	0.010
N	1,128,774	1,155,178	1,234,828	1,092,714
B. Any Visit				
Abolition	0.0120 (0.0065)	0.0075 (0.0058)	0.0194** (0.0060)	0.0046 (0.0072)
p-value	-	0.613	0.399	0.448
Mean	0.6382	0.6458	0.6459	0.6304
N	95,527	98,815	104,689	91,892

Notes: Each column reports results from a separate regression. Dependent variable is Total Doctor Visits. Standard errors clustered at the person level in parentheses. Arc-elasticities are computed using the (pre-reform) 2001 mean of monthly visits. We control for age, month, abolition-group specific seasonal effects, sex, mother's education, mother's unemployment status (benefits), mother's marital status, mother's age, mother's country of birth, number of children under 20, and birth weight. P-values are from t-tests of the comparison with "1st Quartile". ** and * denote significance at the 1 and 5 percent level, respectively.

Table 9: Mother's Country of Birth

	Sweden	EU-15, North America	Other Europe	Other Countries
	3-19 years (1)	3-19 years (2)	3-19 years (3)	3-19 years (4)
A. Number of Visits				
Abolition	0.0143** (0.0018)	0.0139 (0.0111)	0.0018 (0.0129)	0.0300* (0.0121)
2001 Mean	0.1675	0.1632	0.1575	0.1275
% Change	8.55	8.54	1.14	23.57
Arc-elasticity	-0.041	-0.041	-0.006	-0.105
p-value	-	0.999	0.388	0.093
N	4,248,194	147,701	127,445	88,154
B. Any Visit				
Abolition	0.0103** (0.0032)	-0.0272 (0.0202)	0.0053 (0.0248)	0.0850** (0.0239)
p-value	-	0.067	0.842	0.002
Mean	0.6442	0.6133	0.6088	0.5471
N	360,386	12,393	10,673	7,471

Notes: Each column reports results from a separate regression. Dependent variable is Total Doctor Visits. Standard errors clustered at the person level in parentheses. Arc-elasticities are computed using the (pre-reform) 2001 mean of monthly visits. We control for age, month, abolition-group specific seasonal effects, sex, mother's education, mother's unemployment status (benefits), family income, mother's marital status, mother's age, number of children under 20, and birth weight. P-values are from t-tests of the comparison with "Sweden". ** and * denote significance at the 1 and 5 percent level, respectively.

Table 10: Health Status

	Overall											
	Sickly		Non-Sickly		Sickly		Non-Sickly		Sickly		Non-Sickly	
	3-19 years (1)	3-19 years (2)	7-24 years (3)	7-24 years (4)	3-19 years (5)	3-19 years (6)	3-19 years (7)	3-19 years (8)				
A. Allergic Rhinitis												
Abolition	0.0342** (0.0110)	0.0093** (0.0017)	0.0548** (0.0133)	0.0109** (0.0016)	0.0125 (0.0301)	0.0198** (0.0038)	0.0411** (0.0113)	0.0061** (0.0020)				
2001 Mean	0.3588	0.1563	0.3588	0.1563	0.3670	0.1588	0.3563	0.1555				
% Change	9.54	5.98	15.27	6.94	3.41	12.47	11.54	3.94				
Arc-elasticity	-0.046	-0.029	-0.071	-0.034	-0.017	-0.059	-0.055	-0.019				
p-value	-	0.268	-	0.025	-	0.307	-	0.023				
N	261,359	4,252,980	243,850	4,551,019	61,646	1,029,488	199,713	3,223,492				
B. Asthma												
Abolition	0.0349** (0.0100)	0.0123** (0.0017)	0.0494** (0.0172)	0.0111** (0.0016)	0.0198 (0.0240)	0.0231** (0.0038)	0.0404** (0.0111)	0.0089** (0.0020)				
2001 Mean	0.3794	0.1577	0.3794	0.1577	0.3861	0.1594	0.3772	0.1571				
% Change	9.20	7.77	13.01	7.07	5.12	14.51	10.70	5.69				
Arc-elasticity	-0.044	-0.037	-0.061	-0.034	-0.025	-0.068	-0.051	-0.028				
p-value	-	0.615	-	0.189	-	0.173	-	0.113				
N	231,210	4,283,129	188,280	4,606,589	57,589	1,033,545	173,621	3,249,584				
C. Mental Disorders												
Abolition	0.0103 (0.0178)	0.0128** (0.0017)	0.0510 (0.0600)	0.0111** (0.0016)	0.0312 (0.0345)	0.0218** (0.0040)	0.0064 (0.0212)	0.0101** (0.0020)				

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2001 Mean	0.3013	0.1651	0.3013	0.1651	0.3243	0.1664	0.2917	0.1647
% Change	3.43	7.76	16.92	6.70	9.62	13.08	2.18	6.11
Arc-elasticity	-0.017	-0.037	-0.078	-0.032	-0.046	-0.061	-0.011	-0.030
p-value	-	0.479	-	0.591	-	0.755	-	0.600
N	88,846	4,425,493	71,267	4,723,602	24,891	1,066,243	63,955	3,359,250
D. Pre Doctor Visits								
Abolition	0.0418** (0.0061)	0.0056** (0.0014)	0.0496** (0.0067)	0.0011 (0.0012)	0.0466** (0.0142)	0.0157** (0.0029)	0.0399** (0.0069)	0.0025 (0.0016)
2001 Mean	0.5871	0.0882	0.5871	0.0882	0.5890	0.0880	0.5865	0.0882
% Change	7.12	6.40	8.45	1.29	7.91	17.85	6.81	2.85
Arc-elasticity	-0.034	-0.031	-0.041	-0.006	-0.038	-0.082	-0.033	-0.014
p-value	-	0.702	-	0.000	-	0.015	-	0.069
N	739,059	3,775,280	763,690	4,031,179	181,319	909,815	557,740	2,865,465

Notes: Each column reports results from a separate regression. Dependent variable is Total Doctor Visits. Standard errors clustered at the person level in parentheses. Arc-elasticities are computed using the (pre-reform) 2001 mean of monthly visits. We control for age, month, abolition-group specific seasonal effects, sex, mother's education, mother's unemployment status (benefits), family income, mother's marital status, mother's age, mother's country of birth, number of children under 20, and birth weight. P-values are from t-tests of the comparison with "Sickly". ** and * denote significance at the 1 and 5 percent level, respectively.

Figure 1: Average number of monthly doctor visits pre-reform (2001) vs. post-reform (2002) by age.

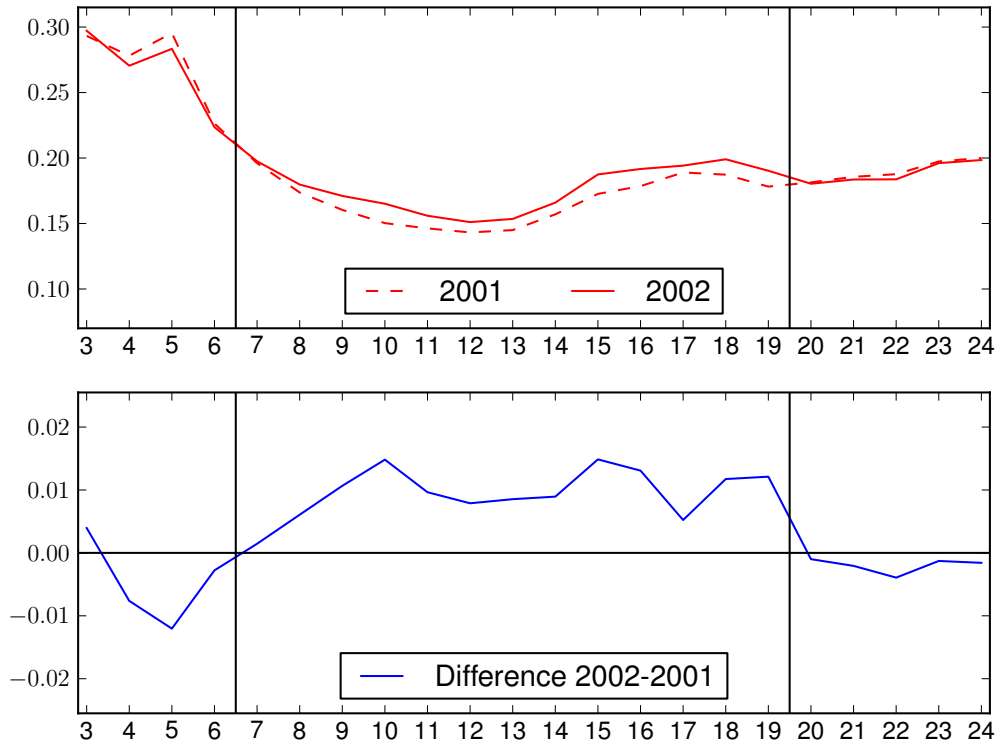


Figure 2: Average number of monthly nurse visits pre-reform (2001) vs. post-reform (2002) by age.

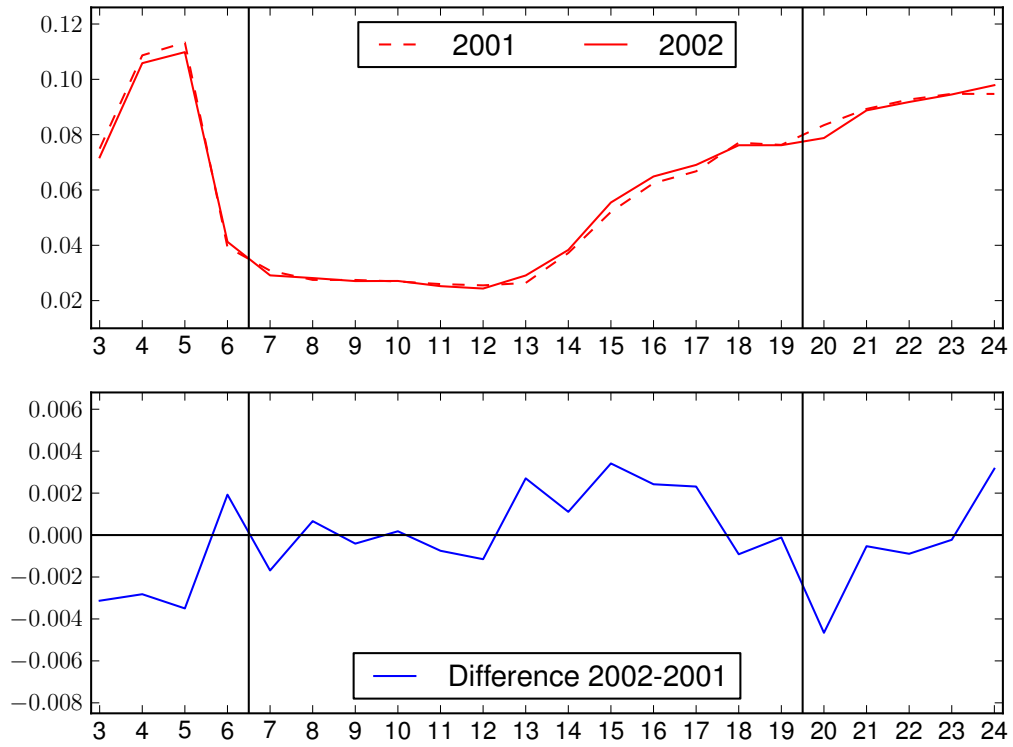


Figure 3: Average number of monthly other visits pre-reform (2001) vs. post-reform (2002) by age.

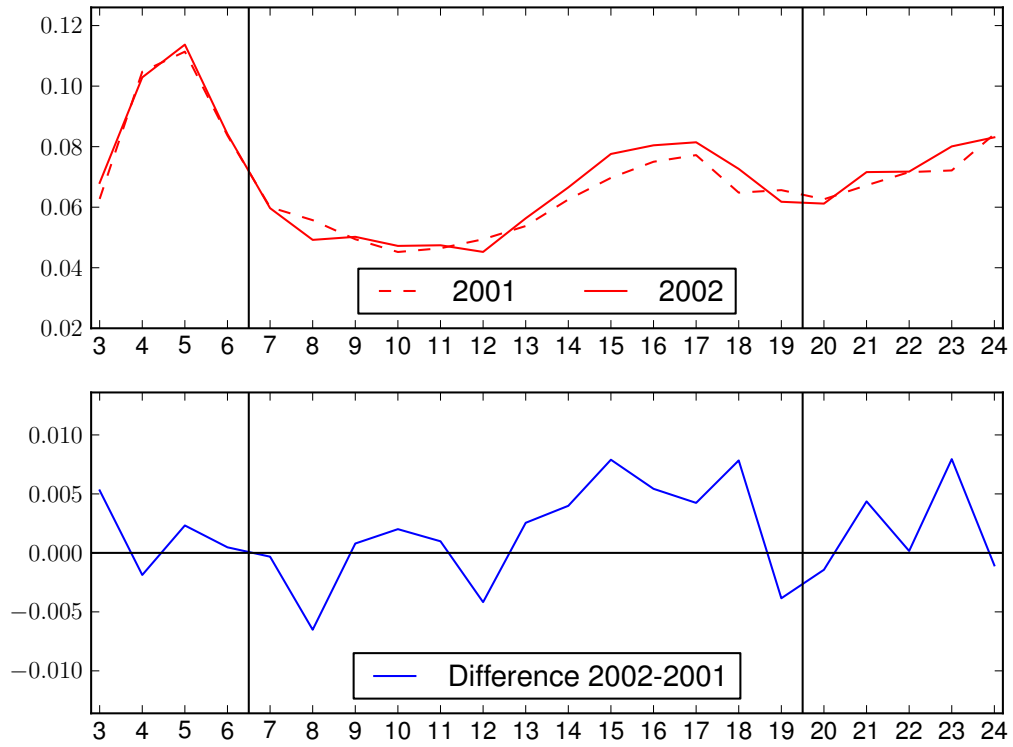


Figure 4: Average number of monthly inpatient visits pre-reform (2001) vs. post-reform (2002) by age.

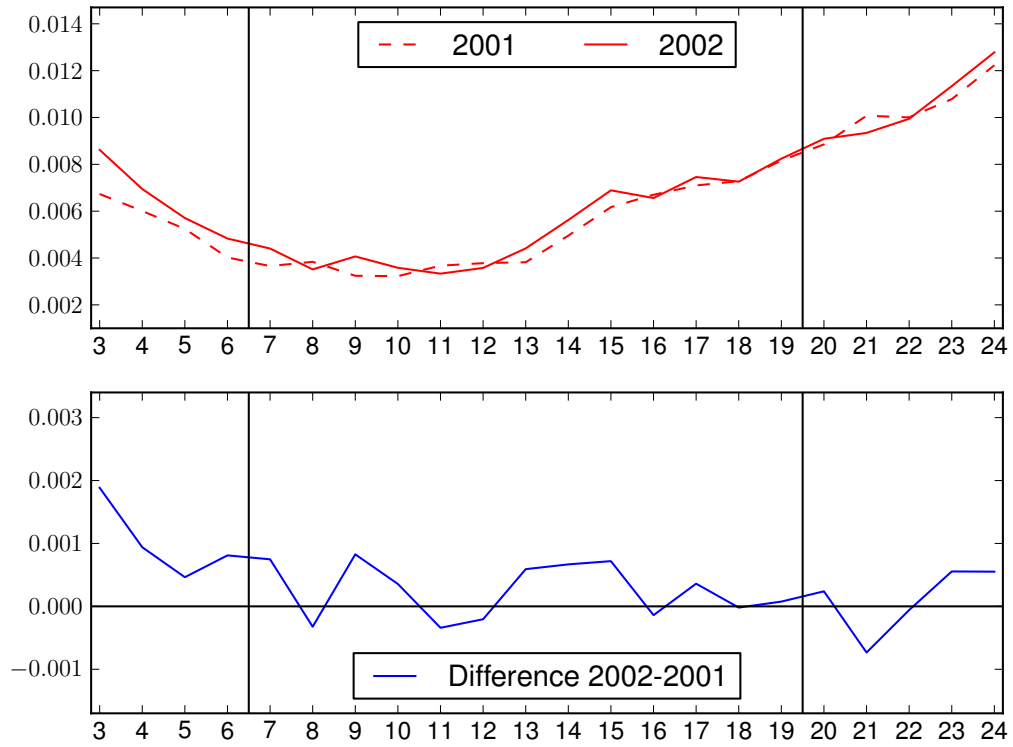
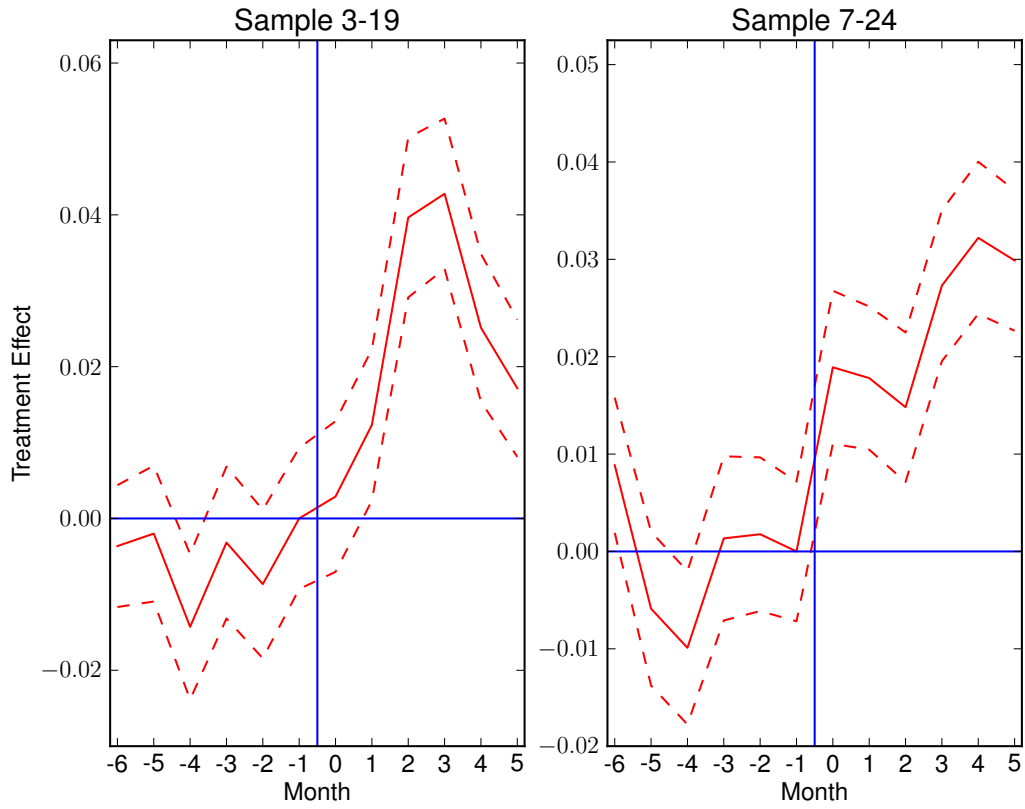
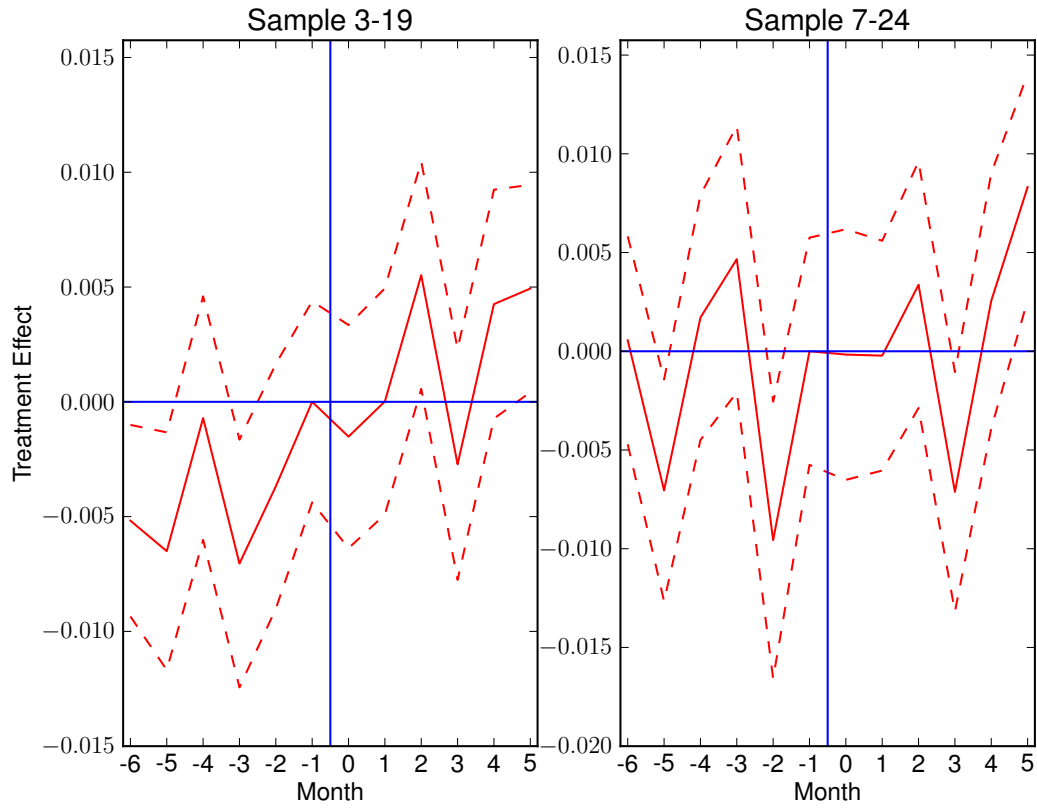


Figure 5: Treatment effects by month for doctor visits



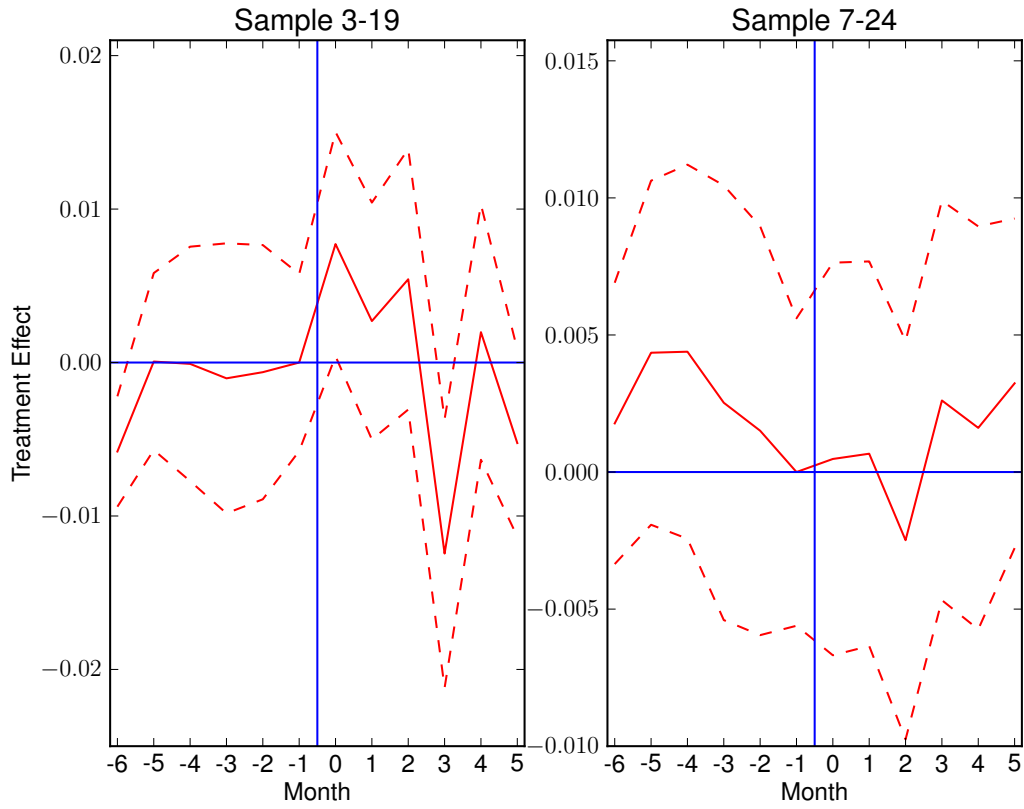
Notes: The treatment effect in the last month prior to the policy change (Dec. 2001) has been normalized to zero. Dashed lines denote 95% confidence bands.

Figure 6: Treatment effects by month for nurse visits



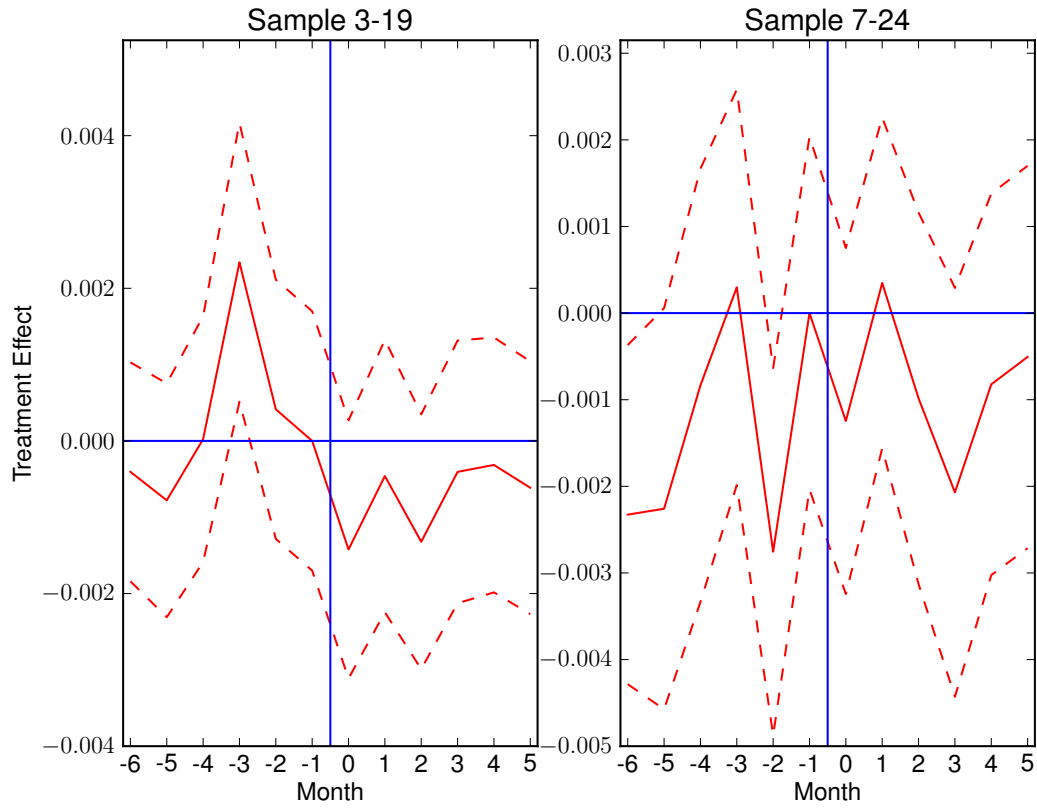
Notes: The treatment effect in the last month prior to the policy change (Dec. 2001) has been normalized to zero. Dashed lines denote 95% confidence bands.

Figure 7: Treatment effects by month for other visits



Notes: The treatment effect in the last month prior to the policy change (Dec. 2001) has been normalized to zero. Dashed lines denote 95% confidence bands.

Figure 8: Treatment effects by month for inpatient visits



Notes: The treatment effect in the last month prior to the policy change (Dec. 2001) has been normalized to zero. Dashed lines denote 95% confidence bands.