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# CONTROLLING HEALTH CARE COSTS THROUGH LIMITED NETWORK INSURANCE PLANS: EVIDENCE FROM MASSACHUSETTS STATE EMPLOYEES

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Controlling Health Care Costs Through Limited Network Insurance Plans: Evidence from Massachusetts State Employees
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## ABSTRACT

Recent years have seen enormous growth in limited network plans that restrict patient choice of provider, particularly through state exchanges under the ACA. Opposition to such plans is based on concerns that restrictions on provider choice will harm patient care. We explore this issue in the context of the Massachusetts GIC, the insurance plan for state employees, which recently introduced a major financial incentive to choose limited network plans for one group of enrollees and not another. We use a quasi-experimental analysis based on the universe of claims data over a three-year period for GIC enrollees. We find that enrollees are very price sensitive in their decision to enroll in limited network plans, with the state's three month "premium holiday" for limited network plans leading 10% of eligible employees to switch to such plans. We find that those who switched spent considerably less on medical care; spending fell by almost 40% for the marginal complier. This reflects both reductions in quantity of services used and prices paid per service. But spending on primary care actually rose for switchers; the reduction in spending came entirely from spending on specialists and on hospital care, including emergency rooms. We find that distance traveled falls for primary care and rises for tertiary care, although there is no evidence of a decrease in the quality of hospitals used by patients. The basic results hold even for the sickest patients, suggesting that limited network plans are saving money by directing care towards primary care and away from downstream spending. We find such savings only for those whose primary care physicians are included in limited network plans, however, suggesting that networks that are particularly restrictive on primary care access may fare less well than those that impose only stronger downstream restrictions.



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As employers and governments look to control runaway health care costs, one place they are turning is to limited network plans. Recognizing that the cost of comparable services can vary widely across providers, insurers are offering plans that exclude the highest cost providers and thereby significantly reduce insurance premiums. These plans often do not vary in their enrollee cost sharing or other plan characteristics, relying only on the restriction to lower cost providers to ensure savings. As a result, they have proven to be increasingly popular, and they appear to be a mainstay of the plan offerings on state and federal exchanges under the Affordable Care Act (ACA). In particular, the explicit tying of ACA insurance subsidies for low income families to the (second) lowest cost plan in the area is likely to induce enormous movement into limited network plans, which are often the least expensive.

But these limited network plans are not without their detractors. Many are concerned that individuals will suffer a disruption in care if they switch to a limited network plan. This could lead to deterioration in the quality of care where the continuity of care is most valuable, such as for those with chronic illness. Recent expansion in limited and tiered network plans (the latter include a broader network but charge differentials for use of more expensive providers) in Massachusetts, for example, was strongly opposed not only by higher cost providers but also by patient advocacy groups.[1] Indeed, this concern prompted tying the ACA subsidies to the *second*-lowest cost plan in an area, to ensure that patients would not be "forced" into networks which did not include their provider. Despite this, ACA critics have recently focused on the dominance of limited network plans on the new exchanges.[2]

Assessing the implications for enrollees of limited network plan enrollment is therefore an important issue for evaluating both the future of employer-sponsored insurance (ESI) and the efficacy of the ACA exchanges. Yet there is virtually no work on the implications of enrollment in a limited network

[1] Weisman and Conaboy (2011)

[2] For example, the CEO of Cedars-Sinai Hospital, Thomas Priselac, recently told *Time* magazine, "We're very concerned with the impact [that a smaller network] has on patients" (Pickert 2014). An article on CNN.com described patients who are "dismayed that their current doctors aren't in the plans or that they can't go to the ones they think are best for them" (Luhby (2014). Similarly, *The Boston Globe* quoted patient Nancy Petro, who said "Now I have to drive 50 miles for blood work when there's a hospital three miles from my house" (Jan 2014).

plan for enrollee well-being. There is an older and much larger literature on the impacts of managed care plans, which include as one of their key aspects network limitations (e.g. Glied, 2000). But this literature was not focused on distinguishing the impacts of network limitations from many of the other differences involved in managing care. There is also a small recent literature on how limited networks impact choice of providers (see Frank et al. for a review), but this literature does not address the impacts on overall spending and utilization patterns.

To address this issue, we turn to the example of the Massachusetts Group Insurance Commission (GIC), the health insurance provider for state employees, which introduced sizeable new incentives for limited network plans as part of their open enrollment for fiscal year 2012. In particular, the state offered a three-month premium holiday for enrollment in limited network plans by state employees. At the same time, the GIC provides insurance for a number of municipalities, to whom this premium holiday was not extended, providing a natural control group. We have obtained from the GIC a complete set of claims data for the 2009 through 2012 period which allow us to assess the implications of this sizeable new incentive for enrollment in limited network plans.

We use these data to answer two sets of questions about limited network plans. First, how responsive are individuals to financial incentives to use such plans? We have sizable variation in financial incentives in our data, with the savings from choosing a limited network plan rising by over $500 per year on average. This allows us to obtain projections for price sensitivity that are highly relevant to employer plans and exchanges. We can also assess *which* enrollees are most price sensitive. Do financial incentives induce only healthy enrollees to join limited network plans, leading to increasing sorting by health across insurers?

We then estimate the implications of limited network enrollment for health care utilization, spending and outcomes, for those enrollees who do decide to switch. We are particularly interested in

assessing the extent to which such switchers change their pattern of physician utilization, and whether this impacts broader health care utilization.

Our findings suggest that switching to a limited network plan is very sensitive to financial incentives; the three month premium holiday offered by the GIC caused 10% of enrollees to switch to limited network plans, with an implied elasticity of switching with respect to the premium savings of 1.3. The healthiest individuals are the likeliest to switch, although the differences by health are not large.

We find that incentives to switch to a limited network plan induced a sizeable reduction in spending for the GIC of 4.2%, implying that the marginal person induced to switch plans by this incentive spent 36% less. Spending falls significantly for most categories of spending and the spending decline appears to be caused by reduction in both the quantities of care received and the prices paid for care. Most importantly, however, there is an *increase* in primary care physician visits and spending that is more than offset by a decrease in specialist visits and spending. Falls in emergency room and hospital spending suggest that any reduction in physician access through network limitations did not cause an increase in use of tertiary care, and there is no evidence of any deterioration along measures of hospital quality. Distance traveled to providers falls for primary care physicians, but rises for specialists and in particular hospitals; there is, however, no evidence that patients are using lower quality hospitals. There is also no evidence of particularly harmful effects for chronically ill patients. But we do find that the savings are concentrated in those individuals who can retain their primary care physician when moving to a limited network plan, suggesting that limits on primary care access may not be as cost-reducing as are downstream limits on other providers. Overall, the findings suggest that the switch to limited network plans reduced spending without harming access to primary care or inducing shifts to more expensive tertiary care.

Our paper proceeds as follows. Section I describes the GIC policy change. Section II lays out the detailed data made available to us by the GIC, and Section III describes our empirical strategy for using

these rich data to identify the impact of limited network plans. Section IV shows the results for plan choice, and Section V presents the results for utilization responses. Section VI considers impacts on patient access as measured by distance traveled and hospital quality, while Section VII considers heterogeneity in our findings. Section VIII concludes.

**Part I: The GIC Policy Change**

The GIC provides health insurance options for all state employees as well as employees of a number of local municipalities who have chosen to buy into the state plan. At the start of our sample period, the GIC insured 81,420 state employees and 109,343 dependents. In addition, there were 23 municipalities purchasing their insurance through the GIC, with 14,232 employees and 19,160 dependents. Municipalities may find the broader negotiating power of the GIC a more attractive alternative to their local purchasing options, although unions often oppose the higher employee contributions and more limited choices associated with GIC enrollment. As a result of these conflicting interests, about 10% of the municipalities in the state were enrolled in the GIC by 2012.

In fiscal year 2011, the year before the limited network incentive plan began, the GIC offered 11 plan options. These plan options are summarized in Table 1. Of these plans, five were classified as broad network plans (mostly Preferred Provider Organizations (PPOs)) that allowed employees a free choice of provider in the counties in which the plans operated. The other six were categorized as narrow network plans with more limited choice of providers. . The cost-sharing features of the broad network and limited network plans are nearly identical, so the only substantive difference across the plans is their network.[3] According to information from GIC insurers, these narrow network plans are

[3] There are minimal differences in cost-sharing across plans; most of the variation is across insurers, not across plan types. For instance, Tufts plans have slightly higher hospital copayments than other plans, but these higher copayments apply to both their broad network plan (Tufts Navigator) and their limited network plan (Tufts Spirit). As a result, average hospital copayments in limited network plans are identical to average hospital copayments in broad network plans. For a specialist (in Tier 1), copays range from $20 to $25, with an average of $24.17 in

established to exclude the most expensive providers while still maintaining sufficient coverage of the plan's service area.

Of course, "narrow network" is a vague term that can have multiple meanings. To provide a richer interpretation of the meaning of a narrow network in the GIC context, we consider empirical measures of network breadth. In particular, we take our full set of data over three years (described further below) and focus on counties in which plans operate. In those counties, we consider all providers for which we observe at least 5 (or 10) in-network claims over the three year period across all insurers. We then calculate, for each insurance plan, the proportion of these providers for which we observe at least 5 (or 10) in-network claims for that particular plan. While this measure undoubtedly incorporates measurement error, it nonetheless provides some relative information about the breadth of each plan's network within the counties in which it operates.

The results of this exercise are presented in Table 2. They suggest that limited network plans, on average, have claims from a smaller fraction of the physicians and hospitals in a county than the broad network plans do. Overall, our measures suggest that broad network plans cover nearly twice as many physicians and about 50% more hospitals than are covered by narrow network plans. While one of the limited network plans, Health New England, appears to have a relatively broad hospital network, it is worth noting that this plan does not operate in eastern Massachusetts, so its network does exclude many hospitals in the state.[4]

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limited network plans and $23.00 in broad network plans. For primary care physicians, copayments range from $15 to $20, with an average of $17.50 in limited network plans and $18.00 in broad network plans.

[4] We have confirmed the results in Table 2 by using data from insurers where available. We have compared the lists of in-network hospitals for limited network plans and found that the results are similar to those that we report in Table 2. For example, Health New England includes 92.3% of hospitals in the claims-based measure and 100% of the hospitals in the list-based measure. Likewise, Tufts Spirit includes 32.9% of hospitals in the claims-based measure and 25.7% in the list-based measure. For physicians, we entered data on all in-network physicians for two limited network plans, Harvard Primary Choice and Health New England. For both plans, we found that for 25% of physicians we did not have enough claims to classify them in our claims data; for the remaining 75% of physicians, 60% (Harvard) or 68% (Health New England) were classified as limited in both our data and the insurers lists, an "effective" match rate of 80-90%.

Prior to the premium holiday, there was an existing financial benefit to choosing limited network plans, reflecting directly the lower cost of those plans to the state. In particular, the state contributed 20% of the cost of insurance plans for active employees hired before July 1st, 2003 (and 25% for those hired after that date), so that a portion of the lower costs of limited network plans were passed on to employees. In 2011, the employee share of the monthly premium for individual coverage ranged from $81.32 for Unicare Community to Choice to $153.36 for Unicare Basic (for workers paying 20% of the premium cost). Sixteen percent of enrollees at the state level chose to enroll in limited network plans.

The financial incentive to switch to a limited network plan varied significantly across employees, for several reasons. First, there were different contribution rules for the local municipalities. For example, while Saugus required that employees contribute only 10% towards most plans, Swampscott required that employees contribute 35% towards most plans. Second, within municipalities, different contribution rules apply to different types of employees. For instance, teachers face different rates than other employees in some municipalities. Third, the state charges different rates to employees with different hiring dates, as described above. Finally, some municipalities provided more generous coverage of lower-cost insurance options. For example, Springfield required that employees contribute 25% towards Unicare Basic, but only 15% towards other plans. These differences in contribution rules generate meaningful differences in the incentive to switch to a limited network plan. For example, the savings from switching from family coverage through Tufts Navigator (a broad network plan) to family coverage through Tufts Spirit (a limited network plan) ranged from $29 per month in the three municipalities that required a 10% employee contribution for both plans, to $160 per month in a municipality that required a 7% higher employee contribution for Tufts Navigator than for Tufts Spirit.

For the fiscal year 2012 open enrollment, the GIC decided to add an extra incentive for state employees to enroll in limited network plans.[5] In particular, state employees who decided to enroll in limited network plans were offered a three month "premium holiday", with no employee contribution required for the first three months of the year. For the affected plans, this premium holiday amounted to a 25% reduction in contributions, ranging from savings of $268 per month for individual coverage from Unicare Community Choice to $764 for family coverage from Harvard Pilgrim Primary Choice. This incentive was not offered by localities which use the GIC system. This policy change induced a major differential incentive for limited network plans for state versus municipal employees.

**Part II: Data**

Our data for this analysis include a complete set of claims and enrollment records for all GIC enrollees for the three fiscal years, spanning the period from July 2009 through June 2012. GIC's insurance contracts run from July through June, so these data allow us to observe three fiscal years. The premium holiday affects fiscal year 2012, which runs from July 2011 to June 2012.

For the purposes of our analysis, we limit the sample to active employees and their dependents who were continuously enrolled over the three years of our sample period. The restriction to continuously-enrolled individuals ensures that the composition of our sample does not change over time. However, this sample restriction precludes the inclusion of eight municipalities that joined GIC during our sample period. Because we cannot identify which employees are teachers, we exclude data from a municipality that has different contribution rates for teachers than for other employees. The resulting sample includes data on 159,732 enrollees, of whom 86% obtained coverage through the state and 14% obtained coverage through one of 21 municipalities in our sample.

[5] At the same time the state imposed mandatory re-enrollment in GIC plans, with an automatic default to limited network plans if individuals didn't re-enroll. But re-enrollment rates were over 99%, so this didn't end up having a very large impact. (Commonwealth of Massachusetts Group Insurance Commission, 2012)

Our (de-identified) data include very limited demographic information such as age and gender, information on enrollment choices, and information on health care utilization and spending over this time period. In the claims data, we identify different types of services and construct annual measures of utilization. Our measures include counts of medical encounters in a year (e.g. office visits, prescription drug purchases, etc.) and total expenditures by all payers for those medical services. While it is possible that out-of-network providers choose not to file claims with the limited network insurers, we do observe claims that indicate that the provider is out-of-network, including some claims for which the insurer pays nothing. While we can't rule out the possibility that we are missing some claims from out-of-network providers, any missing claims are likely to be for low-cost events and would therefore have minimal effect on our overall findings. With three years of claims data for each enrollee, our final data set includes 479,196 annual observations on the 159,732 continuously enrolled individuals in our sample.

Table 3 provides summary statistics on our sample, including information on average annual medical expenditures and utilization. The average member incurs $4,811 in total medical expenses during a year. About 23% of these expenses are incurred in office visits, 18% in inpatient hospitalizations, and 30% in outpatient visits. Prescription drugs account for 19% of the costs. The remainder of the costs includes emergency department visits, labs, and "other" costs, which include home health care, supplies, ambulances, and a variety of other services.

**Part III: Empirical Strategy**

Following the discussion of the GIC policy change above, there are several sources of variation in the cost of limited and broad network plans. At any point in time, there is significant variation across state vs. municipal workers, as well as across state workers by date of hire. Over time, as premiums change, these differences in policy give rise to differential changes in the out-of-pocket premium cost of

limited network plans. And the premium holiday in fiscal year 2012 created a sharp discontinuity in the cost of limited network plans for state employees relative to local employees.

As a result, we pursue two identification strategies in our analysis. The first is a difference-in-differences analysis around the 2012 policy change, comparing state to municipal employees over time. This is a legitimate identification strategy if there are no pre-existing differential trends between these two groups, and if there is no contemporaneous shock to one of these two groups.

To carry out this strategy, we estimate regressions of the form:

(1) $Y_{imt} = \alpha + \beta STATE_m * AFTER_t + \gamma MUNI_m + \tau YEAR_t + \delta X_{imt} + \varepsilon_{imt}$

where i indexes individuals, m indexes municipalities (and state), and t indexes years. STATE is a dummy for obtaining coverage through a state employee, and AFTER is a dummy for fiscal year 2012. MUNI represents a full set of fixed effects for municipality, and YEAR represents a full set of year fixed effects. X is a set of individual controls which includes age, gender, family coverage tier (individual or family), and an indicator for state employees who were hired before July 1st, 2003. The coefficient $\beta$ captures the impact of benefitting from the premium holiday, relative to earlier state workers, and compared to the change over the same time period for municipal workers.

The second identification strategy incorporates the broader price variation that arises from the differential out-of-pocket premium cost of limited network plans across employee types and over time. The advantage of using this approach is that it provides more power to identify the effect of incentives to move to a limited network plan. The disadvantage is that there could be potential endogeneity from several sources in this broader variation. For example, the share that the state and municipalities require their employees to pay for health insurance could be related to underlying insurance demand, or date of employee hire could be correlated with individual insurance demand.

We address these concerns through our second empirical specification:

(2) $Y_{imt} = \alpha + \beta LIMSAV_{mt} + \gamma MUNI_m + \tau YEAR_t + \delta X_{imt} + \varepsilon_{imt}$

where LIMSAV is the savings from switching to a limited network plan for worker i. This measure is computed as the difference in the weighted average of employee contributions to broad network plans and the weighted average of employee contributions to limited network plans, measured as a percentage of the employee contributions to broad network plans. The weights represent the fraction of enrollees in each type of plan who chose each specific plan in a base period, so that more weight is placed on the employee contributions to the more popular plans. Because we weight the employee contributions by baseline enrollment shares, the calculation excludes information about Harvard Primary Choice and Tufts Spirit, which were added as new insurance plan choices in fiscal year 2011. The use of a weighted average across all of the insurance plan options means that this measure does not vary across individuals within a municipality-year. Instead, this measure reflects the sources of variation that were outlined above, including variation across the state and municipalities, across hire date groups (i.e., groups facing the same premium-sharing rules), and over time. The use of a percentage difference in savings means that we are not using variation in the level of savings that arises from differences in premiums across individual and family plans; the dollar value of savings from switching to a limited network plan is always higher for family plans than the dollar value of savings for individual plans, but the percentage savings is always the same.

To address the potential endogeniety of LIMSAV, we include fixed effects for each municipality and controls for the hiring period for state employees to capture those correlates of insurance demand. In practice, since the most significant variation in our sample comes from the premium holiday, our results are similar using either method.

It is very important to be clear on the interpretation of the key coefficient $\beta$. Our estimates of the implications of limited network plans for utilization and outcomes are identified solely by the compliers that switch plans in response to financial incentives. That is, our estimates are *not* a population average estimate of the impact of forcing all enrollees to enroll in a limited network plan.

But current policy conversations center around employee and exchange choice, which consider limited network plans as a choice option, not the mandated default. That is, our estimates provide the relevant estimates of the impacts of offering financial incentives of the range described above on utilization and outcomes.

**Part IV: Enrollee Plan Choice Results**

We begin by examining the effects of financial incentives on the decisions of enrollees to enroll in limited network plans. We estimate the equations above, using as a dependent variable a dummy for enrolling in a limited network plan. We estimate all models as linear probability models, although our results are very similar if we use Probits. Standard errors are clustered at the level of the municipality.

Figure 1 previews our first stage findings. Panel A graphs the savings from choosing a limited network plan by year, separately for municipalities and the state. As the figure shows, municipal employees in 2010 faced larger potential savings from switching to a limited network plan, because the employee share of the premiums was typically higher for them than for state employees. Because limited network plans have lower premiums than broad network plans, a higher employee share of premiums generates more potential savings from choosing a limited network plan. From 2010 to 2011, there was an increase in savings in both groups from choosing a limited network plan. This increase in savings was partially attributable to the fact that broad network plans had relatively large increases in premiums, whereas several of the limited network plans had premium increases that were close to zero. In addition, increases in the employee share of premiums were implemented by the state and by several municipalities.[6] From 2011 to 2012, there was a large rise in the savings from limited network plans for state employees, due to the premium holiday, that was not present for municipal employees.

[6] Under the FY10 Appropriation Act, premium contribution rates for state employees increased by 5 percentage points. These changes were first proposed in June 2009 and implemented in August 2009 (Commonwealth of Massachusetts Group Insurance Commission, 2011). Thus, while the price change was implemented partway

Panel B of the figure shows enrollment rates in limited network plans over time for the state and municipalities. The initial rate of enrollment is higher in municipalities, which is consistent with the higher initial municipality discount for enrolling in such plans. From 2010 to 2011, enrollment in limited network plans rises in both groups by similar magnitudes, once again consistent with the parallel rise in financial incentives over these years. From 2011 to 2012, enrollment in limited network plans in the municipalities is fairly constant, whereas there is an enormous jump in the state plans, mirroring the increase in potential savings for state employees. For state employees, enrollment in limited network plans rises by about 50% between 2011 and 2012.

Table 4 presents these "first stage" results in regression form, confirming what is shown in the figures. We estimate that the premium holiday raised enrollment in limited network plans in the state by over 11 percentage points, relative to the municipalities. Our alternative regression approach, using the full variation over this period, yields an estimate that each 1 percentage point increase in the discount to limited network plans gives rise to a 0.7 percentage point increase in the share of enrollees in limited network plans. The discount rose by 16 percentage points from 2011 to 2012 as a result of the premium holiday, which would predict the same 11 percentage point rise in limited network enrollment. The estimated elasticity of limited network enrollment with respect to its premium discount is 1.28 (or 0.007 * (36.55 / 0.201)).

Table 4 also shows the coefficients on many of the covariates included in these first stage regressions. The coefficients suggest that males are slightly more likely to choose limited network plans than females are. Enrollment in limited network plans peaks between the ages of 30 and 39, and decreases considerably as adults age.

Table 5 explores heterogeneity in price sensitivity, in each case showing the key coefficients from the same specification shown in Table 4, but estimated on only the subsample of interest. We first

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through fiscal year 2010, the primary impact on enrollment choices should have occurred during open enrollment for fiscal year 2011.

consider heterogeneity by underlying health, dividing the population into those who are and are not chronically ill. We identify the chronically ill as individuals with a diagnosis (in an office setting) of hypertension, high cholesterol, diabetes, asthma, arthritis, affective disorders, and gastritis, following Goldman et al. (2004). We find that healthier enrollees are more price sensitive: those who are not chronically ill are about 20% more responsive than those who are chronically ill. This suggests that, when the GIC offered financial incentives for enrollment in limited network plans, they improved the health mix of those plans and worsened the health mix of those remaining in broad network plans, although the differential effect is not large.

Two key determinants of switching could be whether individuals can remain with their insurer, and whether they can keep their doctor. We explore these issues in the remainder of Table 5. We first separate the sample by initial choice of insurer, limiting our sample to those who were enrolled in broad network plans at the beginning of our sample period. We see that there is some heterogeneity in switching by insurer. The first stage is largest for Fallon enrollees, and smallest for Unicare enrollees. There does not appear to be any systematic correlation across companies with the "restrictiveness" of the networks as illustrated earlier.

For 79% of the enrollees in our sample, we can identify their primary care physician based on claims during the first two years of our sample. We identify the primary care physician as the physician with a primary care specialty with whom the enrollee had the most office visits during the first two years of our sample period. We then consider three mutually exclusive groups, among those enrollees who were not yet enrolled in a limited network plan in fiscal year 2011: (a) those whose primary care physician is in the limited network version of the same insurance plan in which they are now enrolled, (b) those whose primary care physician is in a limited network plan, but not that offered by their current insurer, (c) those whose primary care physician is not available through any limited network plan. We

expect enrollment in a limited network plan to be declining across these groups, and indeed that is in the case.

Table 5 shows that that the coefficient on switching (that is identified primarily by the premium holiday) is declining across these groups.[7] The effect for those who can switch without changing insurer or physician is about 60% larger than for those who must switch both insurer and physician. It is interesting to note that over 90% of these enrollees have a primary care physician who participated in at least one limited network plan. This suggests that switching to a limited network plan does not necessarily disrupt primary care relationships for many enrollees.

**Part V: Results for Utilization and Spending**

Having established our "first stage" fact that financial incentives for limited network plans strongly influence choice of such plans, we now turn to estimating the impact on patient outcomes. The regression framework is the same as that used in the previous section, but our dependent variables now relate to health care utilization and outcomes.

These regression coefficients can be interpreted as "reduced form" estimates of the impact of financial incentives on patient outcomes. If we normalize by the "first stage" estimates of the impact of financial incentives on plan choice, we can obtain an implicit instrumental variables estimate of the effect of enrolling in a limited network plan on outcomes. As noted earlier, this IV estimate is valid only for the marginal individuals induced to switch plans by financial incentives, and not for individuals randomly enrolled in a limited network plan.

Since our dependent variables feature both many zeros and a large degree of skewness, we estimate our models by a general linear model (GLM). In the absence of zeroes, we would want to use

[7] Of course, these results suffer from some censoring bias – those whose physician is in a limited network plan may have been most likely to switch before the premium holiday. This would most likely lead us to understate the impact of the financial incentive differential across groups.

ln(spending) as our dependent variable and estimate our models using OLS. However, the presence of observations with zero spending makes this a problematic strategy, so we follow the literature and estimate GLM with a log-link function (Buntin and Zaslavsky, 2004). In this approach, the conditional mean is modeled as:

(3) $\text{Ln E(Spending}|X)_{imt} = \alpha + \beta LIMSAV_{mt} + \gamma MUNI_{m} + \tau YEAR_{t} + \delta X_{imt} + \varepsilon_{imt}$

This approach allows us to avoid selection on the dependent variable and also generates coefficients that are interpretable as percentage changes.[8]

*Total Impact on Spending*

We preview the results for total spending with Figure 2, which shows the trends in total spending for state and municipal employees. For this figure, we use quarterly data on total spending, in order to show more precisely the timing of any changes in total spending. This figure shows that spending trends very closely for state and municipal employees over time before the premium holiday, with state employees having consistently higher total spending (once again consistent with higher enrollment in limited network plans by municipal employees). There is then a very clear relative decline in total spending for state employees, which begins in the first quarter of fiscal year 2012 and becomes even stronger in subsequent quarters. The fact that the spending patterns for state employees, as compared to municipal employees, mirrors the patterns of enrollment in limited network plans seen in Figure 1 is quite suggestive of a causal link.

In Table 6, we formalize this analysis using our regression framework. There are two columns in the table, representing our two identification strategies: the DD strategy is used in the first column,

[8] In contrast to GLM, OLS models $E(\text{ln Spending}|X)_{imt} = \alpha + \beta LIMSAV_{imt} + \gamma MUNI_{imt} + \tau YEAR_{imt} + \delta X_{imt} + \varepsilon_{imt.}$ As a result, OLS generates coefficients that are difficult to translate into statements about E(Spending). The traditional solution has been to use a "smearing" estimator to convert predictions to the unlogged scale (Manning et al., 1987). However, these retransformations are biased in the presence of heteroskedasticity (Buntin and Zaslavsky (2004), Manning and Mullahy (2001)).

while the price variation strategy is used in the second column. In each cell, we present the GLM estimate of the effect on a spending measure. If one wishes to interpret these as structural estimates for the impact of being enrolled in a limited network plan then, as noted above the DD estimates should be multiplied by 1/0.116 = 8.6, and the price variation estimates should be multiplied by 1/0.0070 = 143.

We begin in the first row by modeling total spending. We estimate a marginally significant 4% decline in the level of medical spending. Normalizing by the "first stage" effect, this implies that, for the marginal switcher, there is a 36% decline in total spending when moving to a limited network plan. This is a very sizeable impact. On the other hand, the magnitude of the decline is quite consistent with the difference in total premiums for the two types of plans. Indeed, the mean reduction in out-of-pocket premium for an enrollee to switch from an average broad network plan to an average limited network plan in our sample was 36.55%, as shown in Table 3. Our results suggest that the premium differential for limited network plans is driven not merely by positive selection, but by a substantive difference in costs per capita.[9] The results using full variation, when normalized by the relevant first stage results, generate very similar results: for the marginal switcher, there is a 41% decline in total spending. The fact that our two identification strategies generate such similar results here, and throughout the subsequent results, is reassuring.

We then turn to modeling spending by subcategory of medical utilization. We find a large (albeit only marginally significant) decline in office spending, with the DD estimate implying a roughly 16% decline in office spending for the marginal switcher. We estimate a reduction in hospital spending which is also very large, but statistically insignificant. Unfortunately, the confidence intervals for our

[9] A simple regression of spending on limited network plan enrollment, ignoring the endogeneity of limited network enrollment, yields a cost savings estimate of 30%. It is surprising that the causal impact on spending is larger than the OLS effect, given that the latter includes selection effects. This partly reflects relatively limited selection: the average age of those enrolled is only 3 years younger than those not enrolled, and the odds of chronic illness is only about 10% lower. This may also partly reflect differences between the marginal complier with the policy change and the average enrollee in limited network plans, or differences in the long run effect of enrollment versus first year effects. Nevertheless, the results suggest that most of the savings from limited network plans is from reduced spending by enrollees, not just positive selection.

inpatient hospital spending results are fairly wide, leaving us unable to rule out a large fall or rise in hospital spending.

We do, however, find a large and marginally significant 5% decline in outpatient hospital spending in the DD specification, implying a 43% reduction in outpatient spending for the marginal switcher. Results for outpatient spending are similar in magnitude, and statistically significant, in the specification that uses the full price variation. We see an even larger reduction in lab spending of 8% in the DD specification, implying a 71% reduction in lab spending for the marginal switcher. We have further explored the lab results and found that lab spending associated with office visits doesn't fall significantly; rather, the major change is in lab spending associated with outpatient and emergency room settings.

We find a correspondingly large reduction in emergency room spending. This is striking because it defies the natural hypothesis that when individuals have their physician choices limited they would tend to use more emergency room care. We have further broken emergency room visits by type of visit, and find that about two-thirds of the reduction is from reduced use of the emergency department for treatment of injuries.

We find no meaningful impact on prescription drug spending, but a large impact on "other" spending. When we break down "other" spending into its constituent components, the results imply that there are particularly large reductions in home health care spending and ambulance spending, although neither is significant on its own. It is worth noting that "other" spending constitutes less than 5% of total spending, so declines in this category of spending, while larger in percentage terms, are relatively unimportant in explaining the aggregate decline in spending. Rather, if one takes the point estimates seriously, the changes that appear to be most important in driving the 4.2% decline in total spending include the 5.0% decline in outpatient spending and the (statistically insignificant) 5.6% decline in inpatient spending, which together account for more than half of the overall decline in total spending.

Given the robustness of our findings to the two empirical strategies used in Table 6, for the remainder of the paper we present only the more easily interpretable difference-in-difference results. All results presented below are, not surprisingly, consistent using the fuller measure of incentives.

*Decomposing Spending Impacts*

The large impacts on total spending that are apparent in Figure 2 can be further decomposed to assess whether the savings arise from a reduced quantity of care or lower prices paid for a fixed amount of care. We show the results of this decomposition in Figure 3. In Panel A, we show the changes in costs per service type, weighted by fixed quantities of care for each services. These fixed quantities are based on average utilization for each service category (primary care office visits, specialist office visits, other office visits, inpatient care, outpatient care, ER visits, labs, prescription drugs, and other care) for broad network enrollees in FY 2010. Costs per service are allowed to vary, on average, over time and across state vs. municipal enrollees. Interestingly, average costs per service are similar for state and municipal enrollees at the beginning of the sample period, with costs for state enrollees, if anything, lower than costs for municipal enrollees in FY 2011. But costs per service seem to decline relatively more for state enrollees after the premium holiday, especially towards the end of FY 2012. This figure suggests that the decline in spending among state enrollees is partially attributable to a relative decline in per-service costs.

Panel B of Figure 3 shows the changes in quantities of each type of service, weighted by fixed costs of care. The fixed costs are based on average per-service allowable costs for each type of service for broad network enrollees in FY 2010, while quantities are allowed to vary over time and across state vs. municipal enrollees. State enrollees were clearly using a greater amount of care than municipal enrollees in FY 2010 and FY 2011, but the magnitude of the gap declines visibly beginning at the time of

the premium holiday. Based on these figures, it appears that declines in prices and quantities both play a role in reducing spending for state enrollees after the premium holiday.

Table 7 examines this issue, in a regression framework, for different categories of service. It extends Table 6 by presenting, in addition to our GLM spending results, results for measures of quantity of care and for costs per visit. Specifically, it shows OLS results for a dummy variable for any utilization of that type of service, for number of instances of utilization (visits, stays, tests, drug prescriptions, etc), and for average cost per instance of utilization (conditional on some utilization). The results shown here are all similar if we use Probit models for the any visits outcome, or if we use Poisson count models for the number of visits outcome.

We find that there is no change in the odds of having an office visit, but that the number of visits falls significantly, by -0.15 off a base of 7.36 visits on average. There is no effect on per-visit cost. For inpatient utilization, we once again do not find any results of statistical significance. The point estimates, however, are more consistent with a decline in costs per visit than with a decline in inpatient hospital visits. For outpatient utilization, we do find a reduction in the odds of any outpatient visit, in the number of visits, and in the cost per visit. For emergency room utilization, there are reductions in all measures, but the only significant change is a reduction in the number of visits. For labs, we find a reduction in both the level of lab results and the cost per result, which produces the significant total drop in spending. The results for prescription drug utilization are interesting. As noted earlier, there is no net impact on prescription drug spending. But this appears to reflect a significant reduction in the number of prescriptions (a reduction of 0.4 prescriptions on a base of 11.7 prescriptions), offset by a large (although not significant) rise in the cost per prescription (a rise of $2.08 off a base of $77). We also see a large and statistically significant decline in the quantity of “other” care.

Overall, the results are consistent with the visual impression from the two panels of Figure 3, which suggested that the reduced spending is attributable to reductions in both quantity and price. The

results in Table 7 indicate that the premium holiday is associated with declines in the quantity of care across all categories of service, with statistically significant effects on office visits, emergency room visits, prescription drugs, and other visits. The results indicate that premium holiday is also associated with declines in the per-visit cost for every category of care, except prescription drugs, with statistically significant results for outpatient visits.

*Type of Physician*

The results for physician office utilization suggest the potential value of a further decomposition by type of physician. Table 8 follows the same format as Table 7, but further decomposes the results for office visits by type of provider.

The first panel decomposes office visits into visits to primary care physicians, specialists, and others, using data on provider specialty for all providers in Massachusetts. The "other provider" category, which accounts for 7% of office visits, includes out-of-state providers as well as other providers that we were unable to match to a specialty. The differences across these groups is striking. We find that that primary care office visits increase, with a statistically significant 3% rise in spending in the reduced form, implying a roughly 28% rise in spending on primary care for those who move into narrow networks.[10] At the same time, visits to specialists fall significantly, with a large 5% decline in spending in the reduced form implying a roughly 45% reduction in specialist spending. For other physicians, there is a significant decline in visits, but an offsetting significant rise in cost per visit, resulting in an insignificant effect on spending. These results suggest that enrollment in a limited network plan is associated with a shift towards primary care and away from specialist care.

[10] In 2013, GIC introduced a "Centered Care" Integrated Risk Bearing Organization Initiative that may have affected the mix of primary and specialist care use. However, that initiative was introduced after our sample period ended, so cannot explain our finding of increased primary care and decreased specialist care use.

The second panel decomposes the office visits instead into visits to physicians that the patient had seen previously, and visits to those that the patient had not seen. Of course, we can only identify whether a patient has previously seen a physician if the encounter appears in our claims data; since we do not observe an entire lifetime of claims data for each individual, we will overstate the number of "new" providers. We know that the proportion of providers that we identify as "new" will be artificially high at the beginning of our claims data, and will decrease mechanically over time. We therefore rely on a full additional year of claims data, extending back to July 2008, to identify "new" and "old" providers, while continuing to focus our analysis on the period beginning in July 2009. The disadvantage of this approach is that our regressions include only individuals who were continuously enrolled for four years (or 84% of our baseline sample). But the advantage is that we decrease the measurement error in our classification of "new" vs. "old" providers. Importantly, there is no reason to expect differential measurement error in our classification of new and old providers for state and municipal employees, so we do not believe that our coefficients are biased by this issue.

There is a sizeable reduction in visits to providers whom the patient had previously seen, as well as a reduction in cost per visit, so that total spending on such physicians falls by 3.4%. For new providers, there is a sizeable rise in the odds of a visit, the number of visits, and cost per visit, so that total spending on new providers rises by 5.6%. Given that the mean spending on old providers is $771, while on new providers it is $304, this is consistent with an overall fall in physician spending. Overall, shifting to limited network plans appears to cause a shift away from traditional providers towards newer (lower cost) options. The new providers chosen by individuals are marginally more expensive than new providers chosen previously, but not enough to offset the cost difference between new and old providers.

**Part VI: Impact on Patient Access**

A full analysis of the impact of limited network plans would include effects on patient outcomes. Unfortunately, our data do not contain health outcome measures. Typically used process measures, such as avoidable hospitalizations, rely on inpatient data where our precision is limited. Our efforts to investigate such variables have found no effects but very wide confidence intervals. We have also tried to assess impacts on patient mortality, and once again our estimates were simply too imprecise. [11]

There is one important outcome that is the current focus of much debate over limited network plans, however: patient access to providers as proxied by distance traveled. A major concern raised about limited network plans is that it will lead patients to have to travel much further to see their providers. We can address this concern with our data by examining the distance between patients and the providers they do see when they join limited network plans. To do so, we use the distance between the centroid of patient and provider zip codes in our data, for every provider-patient pair that we observe.

The results of this analysis are shown in the top panel of Table 9. We find that overall there is no significant impact on distance traveled for an office visit. But we find that this masks important heterogeneity by type of office visit: distance traveled for primary care visits falls by 0.65 miles in our reduced form estimates, or by about 5.6 miles as our implied IV coefficient; this is more than half of the baseline distance traveled for primary care.[12] On the other hand, distance traveled rises for specialists,

[11] While we don't observe mortality directly in our data, we can examine the probability of exit from the full sample and, in particular, exit from a family plan when the rest of the family remains enrolled at ages that are not associated with exits due to college graduation or Medicare enrollment. In the full sample, the premium holiday is associated with a statistically insignificant 0.014 percentage point decline in the probability of such an exit. With a standard error of 0.04 percentage points and a mean exit rate of 0.4 percent, we lack the precision to reject a meaningful increase or decrease in such exits. We also examined this exit rate for a variety of subsamples and found no statistically significant effects.

[12] It is possible to observe a negative impact on primary care distance as a result of compositional change in primary care visits. We observe that limited network enrollment is correlated with an increase in primary care visits and, to the extent that those additional visits are to providers who are relatively short distances away from the patients, it is possible to find that limited network enrollment is associated with a decline in average primary care distance.

although not significantly. We find that those patients who continue to see their old providers are traveling shorter distances to do so, but that those patients who see new providers are traveling farther; the latter effect is fairly sizeable, with an implied IV coefficient of about 7 miles, or about two-thirds of the mean distance traveled to new providers.

We find that patients travel shorter distances for outpatient and ER visits, but that they travel much farther to the hospital. The implied IV coefficient on travel to hospitals suggests that the marginal patient switching to a limited network plan travels almost 40 miles further to the hospital. However, this coefficient has a large confidence interval around it, so we cannot exclude considerably smaller increases in the distance travelled.

A related concern is that limited network plans restrict patient access to high-quality providers. In particular, one might be concerned that lower costs are correlated with lower quality. To assess this concern, we obtained eight measures of hospital quality for Massachusetts hospitals and matched them to the hospitalizations in our sample. Our quality measures include the 30-day mortality rates associated with hospitalizations for heart attacks, heart failure, and pneumonia for each hospital. In addition, our measures included the 30-day readmission rates associated with hospitalization for heart attack, heart failure, pneumonia, hip or knee surgery, and all causes.

In the bottom panel of Table 9, we report results from regressions that are exactly like the regressions in the top panel of Table 9, except that they use quality measures for each enrollee (conditional on hospitalization) as the dependent variable. The results are not suggestive of any impact on quality. The results are uniformly statistically insignificant, and they are equally likely to be negative (suggesting higher quality) as they are to be positive (suggesting lower quality). In addition, the point estimates are very small relative to the means. As a result, we conclude that enrollment in limited network plans is not associated with any change in the quality of accessible inpatient hospital care.

**Part VII: Heterogeneity In Responses**

It is possible that the impacts of limited network plans vary considerably by type of patient. We consider in particular three types of heterogeneity in the analysis.

A particular concern is that moving to a more limited network plan may have significant negative connotations for the most ill patients. Table 10a shows the results separately by chronic illness status. We find that the overall effect on spending is similar for the chronically ill and non-chronically ill, although given the larger first stage for the latter group, this implies somewhat larger effects for the marginal chronically ill individual who was induced to switch by the financial incentives. Most importantly, we do not find any evidence that limited network plans caused difficulties in physician access for the chronically ill. Indeed, we find a strong shift in spending from specialists to primary care physicians, with spending falling considerably for the former and rising for the latter. We also find that for the chronically ill there is a significant reduction in inpatient spending, and no statistically significant impact on emergency room use. Taken together, these results do not indicate any particular cause for concern for the chronically ill from switching to a limited network plan.

The next dimension of heterogeneity that we consider is by whether patients' primary care physicians were included in limited plan networks. As described earlier, we divide the sample into those who could keep both their insurer and their primary care provider, those who could keep their primary care provider but to do so would have to switch to a different insurer, and those who could not keep their primary care provider if they switched to a limited network plan.

The results of this analysis are shown in Table 10b. We find that the largest declines in spending are for those who are able to keep their primary care physician, either by moving to the limited network version of the current insurer's plan or by moving to another limited network plan. For these groups primary care visits rise, and specialist visits fall. For those staying with the same insurer, there is a more modest change in both measures; for those keeping their doctor but switching insurers, there is a much

more sizeable reduction in specialist use. This is an interesting finding which suggests that doctors change their use of specialists depending on the network availability of those specialists.

For patients who can keep their physician, we also see no change in hospitalization rates, but a decline in hospital spending, consistent with the idea that the limited network causes them to choose a lower cost hospital. In addition, there are statistically significant declines in outpatient spending and utilization for these groups.

For those whose primary care physician does not participate in a limited network, in contrast, the impact on spending is positive and statistically insignificant, with little change of significance in most categories of spending. This is despite the fact that there is a significant response to the financial incentives in this group in terms of switching to a limited network plan. These findings suggest that the power of limited network plans to lower costs depends critically on those who retain their primary care physician through the switching process. This further implies that the ability to extrapolate our findings depends critically on how limited network plans limit access to primary care physicians; those plans that have very narrow networks of primary care physicians may be less successful in controlling costs.

Finally, we consider heterogeneity by type of illness: which types of illness are driving these results? We classified all of the spending in our sample in 19 major diagnostic categories based on the primary ICD-9 codes.[13] As shown in Table 11, there are negative impacts on spending for about two-thirds of our diagnosis categories. None of the categories with positive spending impacts are significant. In contrast, we find four categories of spending for which the effects are negative and significant: Neoplasms, Respiratory Diagnoses, Musculoskeletal Diagnoses, and Injuries and Poisonings. Thus, our findings are not driven by just one category of spending, but appear broadly spread across the diagnosis spectrum.

[13] Spending for an encounter could be classified into more than one category if there were multiple diagnoses. This could happen if, for example, an individual was hospitalized with more than one primary diagnosis across the claims generated by the hospitalization. As a result, the sum of spending across all 19 diagnostic categories is greater than total spending.

**Part VIII: Conclusions**

The debate over the impact of the shift to narrow network plans has largely proceeded in an evidence vacuum. This paper attempts to move forward our understanding of how individuals choose such plans and their implication for utilization of health care.

We first find that patients are very price sensitive in their decisions to switch to limited network plans, with a price elasticity above one. There is modest adverse selection associated with such price incentives, as those who are most healthy are the most price sensitive.

We then show that the large premium differential between broad and limited network plans is driven not by selection but by real reductions in spending among those induced to switch plans. This reduction in spending comes from both reductions in prices paid and quantity of care used. The reduction in spending does not appear to come from reduced access to primary care; indeed, use of primary care and spending on such services rises for those switching to limited network plans. Rather, the reduction arises from less use of specialists and hospital care. The fact that primary care use is rising, while emergency room and hospital spending is falling, suggests that the move to limited network plans is not adversely impacting health, although we are unable to demonstrate health effects with any certainty. We find that distance traveled falls for primary care and rises for tertiary care, although there is no evidence of a decrease in the quality of hospitals used by patients.

We also found that the positive effects on primary care and reductions in spending on specialist/hospital care occur for both more and less healthy patients, and that the spending reduction holds for a broad spectrum of illnesses. We do find, however, that the spending reduction is driven primarily by those who are able to keep their primary care physician when moving to a limited network plan. Taken together with our overall findings on primary care, we conclude that the real savings from limited network plans arises from restrictions downstream from the primary care provider.

One natural question that arises from our findings is whether the premium incentives provided by the GIC were fiscally beneficial to the state of Massachusetts. The answer to this question appears to be "Yes." We calculate that the employer premium contributions that were paid for all individual and family plan enrollees in fiscal year 2012 was 1.2% percent lower than it would have been in the absence of the premium holiday. This 1.2% reduction in employer premium costs combines a 2.8% reduction in the employer share of the lower premium in limited network plans arising from the 11.6 percentage point increase in limited network enrollment that we calculated in our first stage and a 1.6% increase in the employer's share of all premiums due to the premium holiday. In future years, if the same group of enrollees who responded to the premium holiday were to remain enrolled in limited network plans, the savings would be expected to be approximately 2.8%. While some of the marginal enrollees might switch back to broad network plans, there is a great deal of inertia in insurance plan enrollment.[14] We have analyzed aggregate enrollment data for the years following the premium holiday, and the patterns are consistent with the idea that the vast majority of the new limited network enrollees who were induced to switch by the premium holiday remained in limited network plans in subsequent years. While 31.2% of all enrollees covered by active state employees were enrolled in limited network plans in FY 2012 (the year that the premium holiday took effect), 30.4% were enrolled in limited network plans in FY 13 and 31.1% were enrolled in FY 14. By comparison, 25.1% of all enrollees covered by active municipal employees were in limited network plans in FY 12, 24.5% of them were enrolled in limited network plans in FY 13, and 26.9% were enrolled in FY 14. This inertia in plan enrollments suggests that the fiscal benefits of the 2012 premium holiday were likely much larger in subsequent years when the premium holiday was no longer in place, since the state benefited from a reduced premium bill with any offsetting increase in the employer share of the premium.

[14] In our sample, only 3% of those who were enrolled in broad network plans in FY 2010 switched to limited network plans for FY 2011; similarly, only 1% of those who were enrolled in limited network plans in FY 2010 switched to a broad network plan in FY 2011.

The most important caveat to our results is that they apply to one particular example, and that we may not be able to extrapolate them to other limited network plans, for example those featured on state exchanges. An important goal for future work should be to extend this analysis to those other examples. This should be feasible given that the tax credits available under the ACA provide distinctly non-linear price differentials across health insurance options, allowing future researchers to assess how those induced into limited network plans on exchanges are faring in terms of health care spending and outcomes.

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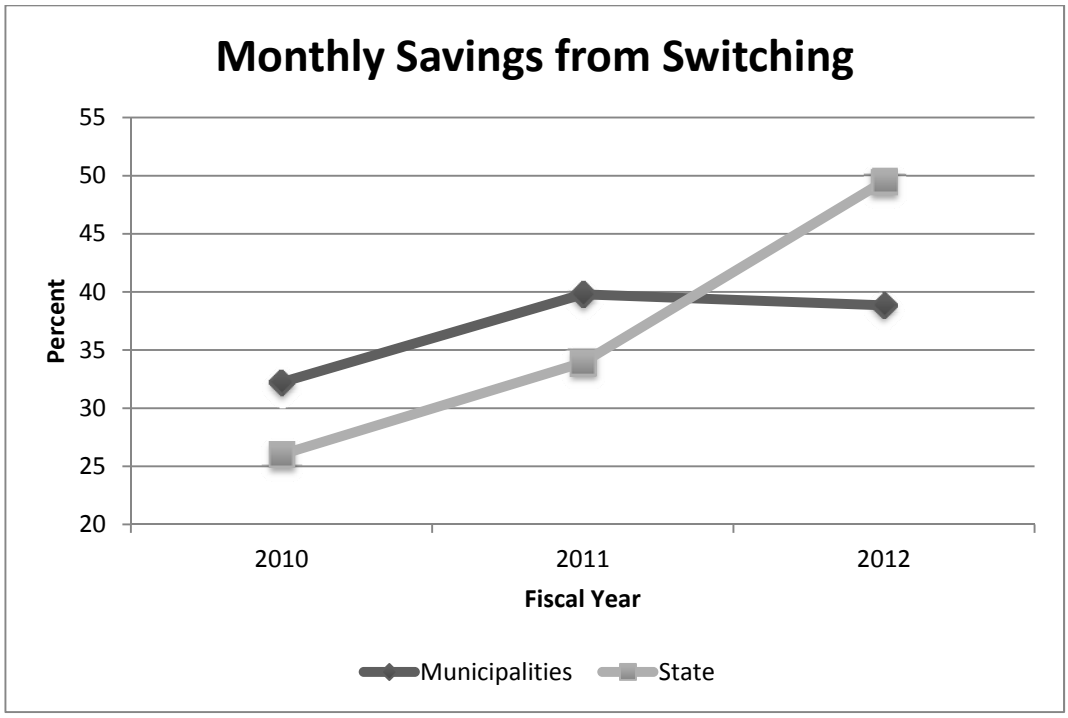
**Figure 1**

**Panel A: The Monthly Savings from Switching to a Limited Network, as a Percentage of Average Broad Network Premium Contribution**




**Panel B: Enrollment in Limited Network Plans, as a Percentage of Total Enrollment**

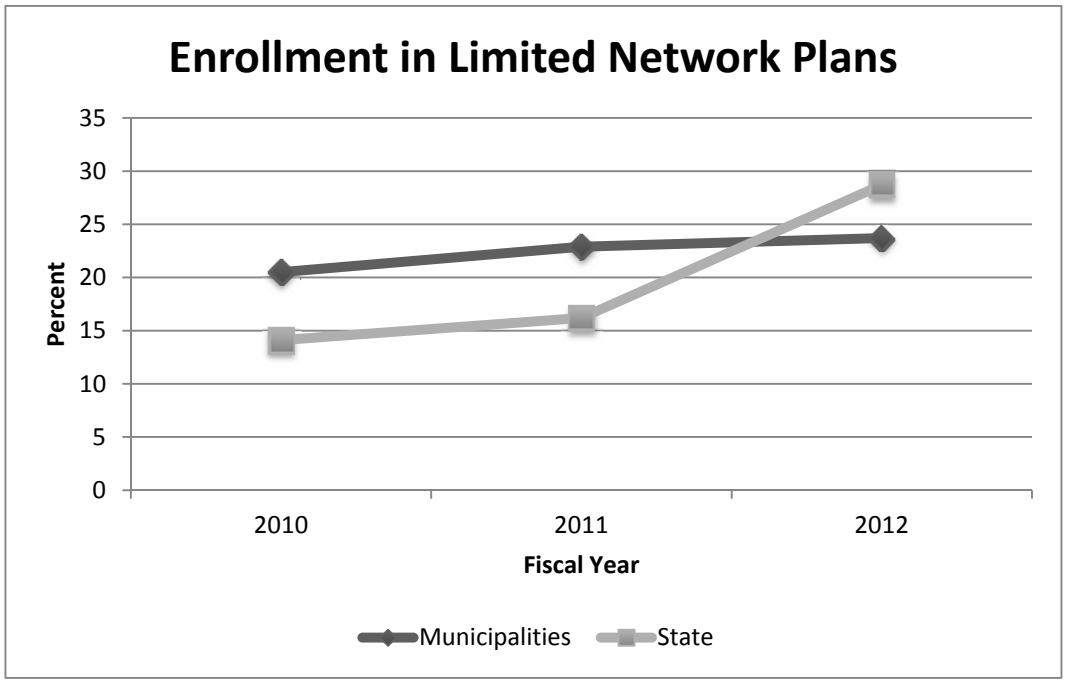


**Figure 2: Total Quarterly Spending per Capita**

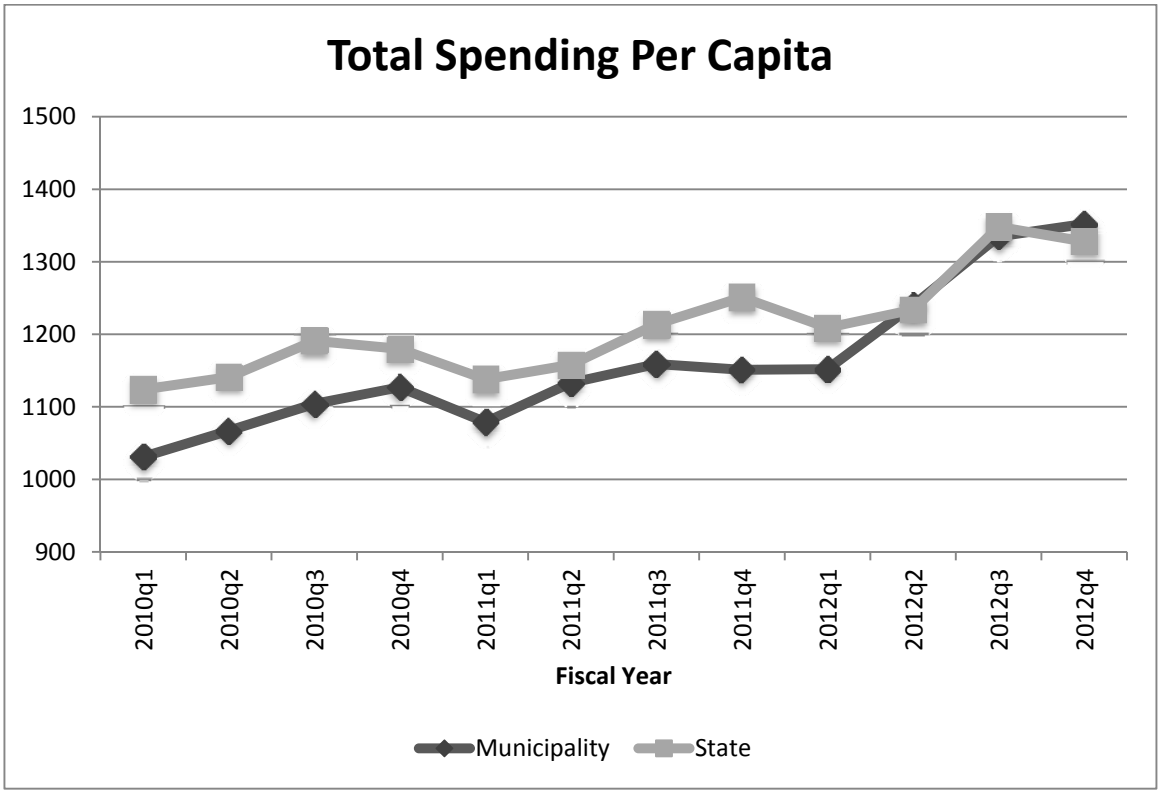
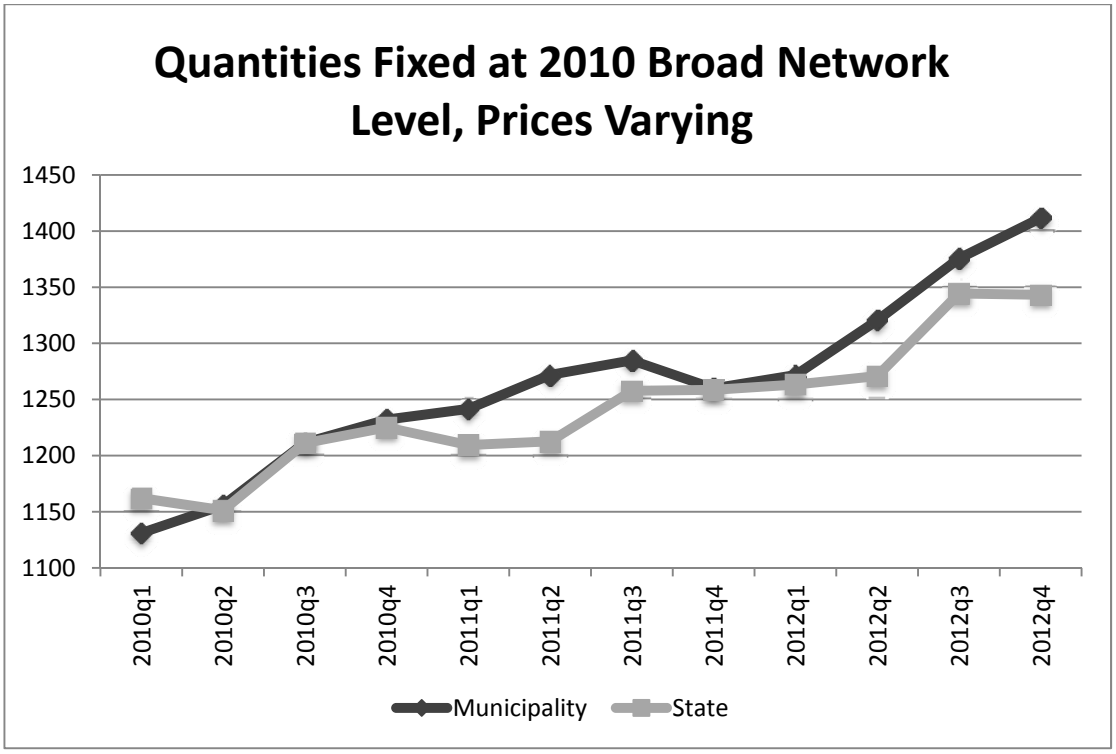


**Figure 3: Decomposition of Changes in Total Quarterly Spending per Capita**

**Panel A**




**Panel B**

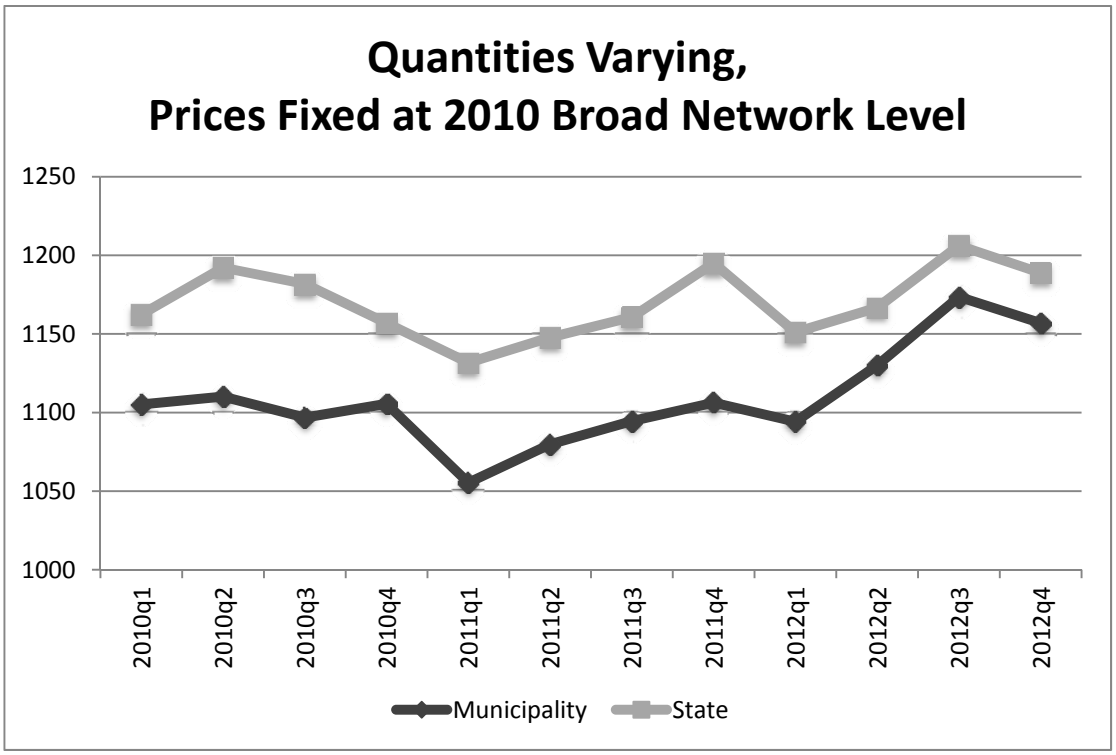


**Table 1: Details of 2010 GIC Plan Options**

| Plan Name | Enrollment in June 2010 | Type of Plan | Limited Network Plan |
|---|---|---|---|
| Fallon Community Health Plan Direct Care | 1% | HMO | Yes |
| Fallon Community Health Plan Select Care | 3% | HMO | No |
| Harvard Pilgrim Independence Plan | 26% | PPO | No |
| Harvard Pilgrim Primary Choice Plan | 0% | HMO | Yes |
| Health New England | 6% | HMO | Yes |
| Neighborhood Health Plan | 1% | HMO | Yes |
| Tufts Health Plan Navigator | 31% | PPO | No |
| Tufts Health Plan Spirit | 0% | HMO-type | Yes |
| UniCare State Indemnity Plan Basic | 17% | Indemnity | No |
| Unicare State Indemnity Plan Community Choice | 6% | PPO-type | Yes |
| Unicare State Indeminity Plan PLUS | 9% | PPO-type | No |

**Table 2: Measures of Network Breadth**

| | Physician | | Hospital | |
|---|---|---|---|---|
| | >5 Claims | >10 Claims | > 5 Claims | >10 Claims |
| **Average across all plans** | | | | |
| Broad | 0.250 | 0.212 | 0.776 | 0.710 |
| Narrow | 0.135 | 0.107 | 0.541 | 0.419 |
| **Harvard Pilgrim** | | | | |
| Broad: Independence | 0.367 | 0.315 | 0.963 | 0.901 |
| Narrow: Primary Choice | 0.110 | 0.077 | 0.570 | 0.418 |
| **Tufts** | | | | |
| Broad: Navigator | 0.351 | 0.312 | 0.827 | 0.815 |
| Narrow: Spirit | 0.054 | 0.034 | 0.329 | 0.158 |
| **Unicare** | | | | |
| Broad: Basic | 0.263 | 0.220 | 0.926 | 0.864 |
| Broad: Plus | 0.199 | 0.160 | 0.802 | 0.728 |
| Narrow: Community Choice | 0.166 | 0.128 | 0.650 | 0.563 |
| **Fallon** | | | | |
| Broad: Select | 0.069 | 0.052 | 0.360 | 0.240 |
| Narrow: Direct | 0.066 | 0.051 | 0.400 | 0.200 |
| **Other Narrow** | | | | |
| Health New England | 0.353 | 0.313 | 0.923 | 0.923 |
| Neighborhood Health Plan | 0.059 | 0.041 | 0.373 | 0.253 |

Notes: This table shows the proportion of providers located in the counties where the insurance plan operates for whom we observe at least 5 (or 10) in-network claims.

**Table 3: Summary Statistics**

| Variable | Mean (Standard Deviation) | |
|---|---|---|
| Enrolled in Limited Network Plan | 0.201 | |
| | (0.400) | |
| Savings from switching to limited network plan (as a % of employee contribution to broad network plan) | 36.55% | |
| | (9.64) | |
| | Spending | Visits |
| Total expenses | $4,811 | - |
| | (15,132) | |
| Office visits | $1,084 | 7.36 |
| | (2,155) | (9.69) |
| Primary Care | $323 | 2.17 |
| | (653) | (2.92) |
| Specialist | $676 | 4.60 |
| | (1799) | (8.31) |
| Other | $85 | 0.55 |
| | (762) | (2.56) |
| Old Provider | $771 | 5.64 |
| | (1,937) | (8.37) |
| New Provider | $304 | 1.43 |
| | (546) | (1.68) |
| Inpatient Hospitalization | $864 | 0.053 |
| | (8,117) | (0.297) |
| Outpatient Hospital | $1,443 | 3.76 |
| | (7,200) | (8.12) |
| Emergency Room | $235 | 0.220 |
| | (995) | (0.635) |
| Lab & X-rays | $69 | 0.550 |
| | (336) | (1.463) |
| Drugs | $900 | 11.69 |
| | (4,417) | (17.03) |
| Other | $210 | 0.70 |
| | (3,324) | (4.38) |
| Number of observations | 479,196 | |

**Table 4 – First Stage Regressions**

| | Difference-in-difference | Full variation |
|---|---|---|
| State employees*Post | 0.1165** | |
| | (0.0036) | |
| Relative Price of Limited Plans | | 0.0070** |
| | | (0.0002) |
| Male | 0.0011** | 0.0011** |
| | (0.0004) | (0.0004) |
| Age 19-29 | -0.0067** | -0.0068** |
| | (0.0010) | (0.0010) |
| Age 30-39 | 0.0236** | 0.0236** |
| | (0.0036) | (0.0036) |
| Age 40-49 | -0.0019 | -0.0019 |
| | (0.0020) | (0.0020) |
| Age 50-59 | -0.0212** | -0.0212** |
| | (0.0037) | (0.0037) |
| Age 60-69 | -0.0546** | -0.0545** |
| | (0.0035) | (0.0035) |
| Age 69+ | -0.0812** | -0.0810** |
| | (0.0069) | (0.0069) |
| Family plan | 0.0006 | 0.0092** |
| | (0.0022) | (0.0022) |
| Number Obs | 479,196 | 479,196 |

Notes: Each column shows coefficients (and standard errors) from a single OLS regression. Other control variables include a full set of municipality and year fixed effects and controls for date of hire. The omitted age category is <19 years old. Standard errors are clustered on municipality. The sample includes all continuously enrolled active employees over the three-year period from fiscal year 2010 to fiscal year 2012; the unit of observation is a person-year.

* denotes significance at the 10% level

** denotes significance at the 5% level

**Table 5 – Heterogeneity in First Stage**

| | Difference-in-difference | Full variation |
|---|---|---|
| Base Estimates | 0.116** | 0.0070** |
| | (0.004) | (0.0002) |
| | By Chronic Illness | |
| No Chronic Illness | 0.104** | 0.0063** |
| (N=132,727) | (0.003) | (0.0002) |
| Chronic Illness | 0.121** | 0.0073** |
| (N=346,469) | (0.004) | (0.0002) |
| | By broad network insurance company in FY 2010 | |
| Fallon | 0.236** | 0.0139** |
| (N=13,695) | (0.012) | (0.0008) |
| Harvard | 0.199** | 0.0117** |
| (N=121,992) | (0.007) | (0.0005) |
| Tufts | 0.109** | 0.0068** |
| (N=169,065) | (0.009) | (0.0005) |
| Unicare | 0.081** | 0.0056** |
| (N=102,381) | (0.006) | (0.0007) |
| | By PCP's availability in a limited network | |
| PCP is in the limited network plan | 0.168** | 0.0100** |
| offered by current insurer (N=187,656) | (0.006) | (0.0003) |
| PCP is in a limited network plan | 0.127** | 0.0077** |
| offered by a different insurer | (0.010) | (0.0006) |
| (N=76,125) | | |
| PCP is not in a limited network plan | 0.101** | 0.0061** |
| (N=43,197) | (0.002) | (0.0002) |

Notes: Each cell shows the coefficient (and standard error) from a single regression. In the first column, the coefficient is on the interaction between "state employee" and post; in the second column, the coefficient is on the relative price of limited network plans. Each row shows results for a different sub-sample. Control variables include gender, age group, enrollment in a family plan, date of hire, and a full set of municipality and year fixed effects. Coefficients are estimated using OLS. Standard errors are clustered on municipality.

* denotes significance at the 10% level

** denotes significance at the 5% level

**Table 6 – Basic Spending Results**

| | Difference-in-difference | Full Variation |
|---|---|---|
| Total Spending | -0.042* | -0.0029** |
| | (0.022) | (0.0013) |
| Office Visits | -0.018* | -0.0012* |
| | (0.010) | (0.0006) |
| Inpatient Hospitalization | -0.056 | -0.0048 |
| | (0.071) | (0.0043) |
| Outpatient Hospital | -0.050* | -0.0033** |
| | (0.025) | (0.0015) |
| Emergency Room | -0.095* | -0.0054* |
| | (0.055) | (0.0032) |
| Lab & X-Ray | -0.083* | -0.0047 |
| | (0.049) | (0.0029) |
| Drugs | 0.003 | 0.0003 |
| | (0.017) | (0.0011) |
| Other | -0.111** | -0.0074** |
| | (0.054) | (0.0036) |
| Number of observations | 479,196 | 479,196 |

Notes: Each cell shows coefficients (and standard errors) from a single regression. In the first column, the coefficient is on the interaction between "state employee" and post; in the second column, the coefficient is on the relative price of limited network plans. Each row shows results for spending on a different type of service. Control variables include gender, age group, enrollment in a family plan, date of hire, and a full set of municipality and year fixed effects. The sample includes all continuously enrolled active employees over the three-year period from fiscal year 2010 to fiscal year 2012; the unit of observation is a person-year. Coefficients are estimated using GLM. Standard errors are clustered on municipality.

* denotes significance at the 10% level

** denotes significance at the 5% level

**Table 7 – Broader Measures of Utilization for DD Model**

| | Total Spending (GLM) | Any Visits (OLS) | Number of Visits (OLS) | Cost Per Visit (OLS) |
|---|---|---|---|---|
| Office Visits | -0.018* | 0.0001 | -0.154* | -0.127 |
| | (0.010) | (0.0026) | (0.083) | (2.087) |
| Inpatient Hospitalization | -0.056 | -0.0005 | -0.0006 | -861.59 |
| | (0.071) | (0.0020) | (0.0027) | (845.44) |
| Outpatient Hospital | -0.050* | -0.0086 | -0.103 | -20.00* |
| | (0.025) | (0.0053) | (0.071) | (11.51) |
| Emergency Room | -0.095* | 0.0026 | -0.0090* | -67.24 |
| | (0.055) | (0.0029) | (0.0046) | (42.15) |
| Lab & X-Ray | -0.083* | -0.0019 | -0.036 | -4.60 |
| | (0.049) | (0.0073) | (0.022) | (4.05) |
| Drugs | 0.003 | 0.0039 | -0.386** | 2.08 |
| | (0.017) | (0.0042) | (0.113) | (1.82) |
| Other | -0.111** | -0.034** | -0.075** | -4.19 |
| | (0.054) | (0.010) | (0.027) | (21.45) |
| Number of observations | 479,196 | 479,196 | 479,196 | Varies |

Notes: Each cell shows the coefficient (and standard error) on the interaction between "state employee" and "post" from a single difference-in-difference regression. Each row shows results for a different service; each column shows a different measure of utilization for that service. Control variables include gender, age group, enrollment in a family plan, date of hire, and a full set of municipality and year fixed effects. The sample includes all continuously enrolled active employees over the three-year period from fiscal year 2010 to fiscal year 2012; the unit of observation is a person-year. Coefficients in the first column are estimated using GLM; results in the other columns are estimated using OLS. Standard errors are clustered on municipality.

* denotes significance at the 10% level

** denotes significance at the 5% level

**Table 8– Results for Office Visit Utilization by Type of Physician**

| | Total Spending (GLM) | Any Visits (OLS) | Number of Visits (OLS) | Cost Per Visit (OLS) |
|---|---|---|---|---|
| | | Primary Care vs. Specialist vs. Other | | |
| Primary Care | 0.030** | -0.002 | 0.040* | 1.95 |
| | (0.015) | (0.005) | (0.023) | (2.09) |
| Specialist | -0.051** | -0.007 | -0.153** | -3.27 |
| | (0.013) | (0.007) | (0.069) | (3.54) |
| Other | -0.014 | -0.0001 | -0.027* | 18.87** |
| | (0.077) | (0.0046) | (0.015) | (6.38) |
| | | Old vs. New Providers | | |
| Old Providers | -0.034** | -0.004 | -0.142** | -2.27 |
| | (0.011) | (0.003) | (0.042) | (1.83) |
| New Providers | 0.056** | 0.016** | 0.051* | 7.13** |
| | (0.013) | (0.007) | (0.028) | (1.40) |
| Number of observations | 479,196 | 479,196 | 479,196 | Varies |

Notes: Each cell shows the coefficient (and standard error) on the interaction between "state employee" and "post" from a single difference-in-difference regression. Each row shows results for a different type of office visit; each column shows a different measure of utilization for that service. Control variables include gender, age group, enrollment in a family plan, date of hire, and a full set of municipality and year fixed effects. The sample includes all continuously enrolled active employees over the three-year period from fiscal year 2010 to fiscal year 2012; the unit of observation is a person-year. Coefficients in the first column are estimated using GLM; results in the other columns are estimated using OLS. Standard errors are clustered on municipality.

* denotes significance at the 10% level

** denotes significance at the 5% level

**Table 9: Impact on Distance Traveled and Hospital Quality**

| Dependent Variable | Mean of Dependent Variable | DD Coefficient |
|---|---|---|
| Measures of Distance Travelled | | |
| Distance to Office Visits | 9.82 | -0.114 |
| | (9.45) | (0.131) |
| Distance to Primary Care Visits | 8.19 | -0.659** |
| | (10.69) | (0.278) |
| Distance to Specialists | 10.53 | 0.038 |
| | (10.11) | (0.183) |
| Distance to Other Office Visits | 9.88 | -0.151 |
| | (15.59) | (0.447) |
| Distance to Old Providers | 9.49 | -0.363** |
| | (10.27) | (0.147) |
| Distance to New Providers | 12.59 | 0.857** |
| | (12.82) | (0.377) |
| Distance to Inpatient Hospitalization | 28.10 | 4.538** |
| | (26.81) | (2.149) |
| Distance to Outpatient Hospital | 14.58 | -1.193** |
| | (13.00) | (0.333) |
| Distance to Emergency Room | 22.23 | -0.774 |
| | (22.43) | (0.485) |
| Measures of Hospital Quality | | |
| 30-Day Mortality Rate, AMI | 13.81 | -0.002 |
| | (1.24) | (0.040) |
| 30-Day Mortality Rate, Heart failure | 10.34 | 0.031 |
| | (1.28) | (0.078) |
| 30-Day Mortality Rate, Pneumonia | 11.04 | 0.062 |
| | (1.50) | (0.112) |
| 30-Day Readmission Rate, AMI | 19.07 | -0.054 |
| | (1.25) | (0.067) |
| 30-Day Readmission Rate, Heart Failure | 23.68 | 0.016 |
| | (1.46) | (0.041) |
| 30-Day Readmission Rate, Pneumonia | 18.24 | -0.044 |
| | (1.27) | (0.050) |
| 30-Day Readmission Rate, Hip or Knee Surgery | 5.51 | 0.026 |
| | (0.68) | (0.018) |
| 30-Day Readmission Rate, All Cause | 16.46 | -0.035 |
| | (1.05) | (0.039) |

Notes: Each cell shows the coefficient (and standard error) on the interaction between "state employee" and "post" from a single difference-in-difference regression. Each row shows results for distance to a different type of provider. Control variables include gender, age group, enrollment in a family plan, date of hire, and a full set of municipality and year fixed effects. The sample includes all continuously enrolled active employees over the three-year period from fiscal year 2010 to fiscal year 2012; the unit of observation is a person-year. Coefficients are estimated using OLS. Standard errors are clustered on municipality.

* denotes significance at the 10% level

** denotes significance at the 5% level

**Table 10a: Heterogeneity in Results by Chronic Illness**

| | Not Chronically Ill | | | Chronically Ill | | |
|---|---|---|---|---|---|---|
| | Spending (GLM) | Visits (OLS) | Distance (OLS) | Spending (GLM) | Visits (OLS) | Distance (OLS) |
| Total Spend | -0.039* | | | -0.043 | | |
| | (0.023) | | | (0.033) | | |
| Office Total | -0.023** | -0.066 | -0.137 | -0.013 | -0.349** | -0.083 |
| | (0.011) | (0.092) | (0.183) | (0.016) | (0.168) | (0.103) |
| Primary | 0.022 | 0.060** | -0.0851** | 0.035* | -0.006 | -0.474 |
| | (0.020) | (0.026) | (0.139) | (0.018) | (0.039) | (0.463) |
| Specialist | -0.053** | -0.085 | 0.210 | -0.045** | -0.303** | -0.095 |
| | (0.015) | (0.071) | (0.244) | (0.021) | (0.143) | (0.133) |
| Other | -0.032 | -0.031 | -3.314** | 0.051 | -0.015 | 1.733** |
| | (0.103) | (0.021) | (0.370) | (0.057) | (0.035) | (0.417) |
| Old | -0.054** | -0.124 | -0.467** | -0.009 | -0.069 | -0.252 |
| | (0.021) | (0.130) | (0.178) | (0.024) | (0.182) | (0.163) |
| New | 0.037 | 0.046 | 0.566* | 0.087** | 0.079** | 1.282** |
| | (0.024) | (0.033) | (0.329) | (0.009) | (0.033) | (0.453) |
| Inpatient | 0.053 | 0.001 | 5.603* | -0.137* | -0.005 | 3.99* |
| | (0.088) | (0.002) | (2.840) | (0.073) | (0.006) | (2.117) |
| Outpatient | -0.045 | -0.109* | -1.072** | -0.051 | -0.088 | -1.291** |
| | (0.038) | (0.057) | (0.253) | (0.034) | (0.143) | (0.457) |
| ER | -0.139** | -0.007 | -2.432** | -0.016 | -0.012 | 1.151 |
| | (0.059) | (0.005) | (0.768) | (0.061) | (0.008) | (0.725) |
| Lab | -0.040 | -0.024* | | -0.159** | -0.064 | |
| | (0.055) | (0.012) | | (0.065) | (0.057) | |
| Drugs | -0.00003 | -0.250** | | 0.007 | -0.710** | |
| | (0.0282) | (0.110) | | (0.029) | (0.242) | |
| Other | -0.151* | -0.049* | | -0.106* | -0.141** | |
| | (0.077) | (0.029) | | (0.055) | (0.044) | |
| N | 346,469 | 346,469 | Varies | 132,727 | 132,727 | Varies |

Notes: Each cell shows the coefficient (and standard error) on the interaction between "state employee" and "post" from a single difference-in-difference regression. Each row shows results for a different service; each column shows a different measure of utilization for that service for one of the two sub-samples. Control variables include gender, age group, enrollment in a family plan, date of hire, and a full set of municipality and year fixed effects. Coefficients in the first and fourth columns are estimated using GLM; results in the other columns are estimated using OLS. Standard errors are clustered on municipality.

* denotes significance at the 10% level

** denotes significance at the 5% level

**Table 10b: Heterogeneity in Results by PCP's availability in limited network plans**

| | Limited network, same insurer | | | Limited network, different insurer | | | No limited network | | |
|---|---|---|---|---|---|---|---|---|---|
| | Spending (GLM) | Visits (OLS) | Distance (OLS) | Spending (GLM) | Visits (OLS) | Distance (OLS) | Spending (GLM) | Visits (OLS) | Distance (OLS) |
| Total Spend | -0.072** | | | -0.130** | | | 0.047 | | |
| | (0.024) | | | (0.055) | | | (0.045) | | |
| Office Total | -0.012 | -0.199 | -0.119 | -0.047** | -0.414** | -0.347 | 0.006 | -0.0001 | -0.025 |
| | (0.015) | (0.116) | (0.156) | (0.019) | (0.192) | (0.301) | (0.053) | (0.207) | (0.569) |
| Primary | 0.032** | 0.072* | -0.637* | 0.046 | 0.068 | 0.225 | 0.053 | -0.018 | 0.789 |
| | (0.010) | (0.038) | (0.338) | (0.036) | (0.061) | (0.311) | (0.065) | (0.075) | (0.658) |
| Specialist | -0.039* | -0.196* | 0.131 | -0.122** | -0.422** | -0.573 | -0.033 | -0.004 | -0.354 |
| | (0.021) | (0.114) | (0.112) | (0.027) | (0.185) | (0.474) | (0.072) | (0.183) | (0.615) |
| Other | -0.204 | -0.066** | -6.548** | 0.168 | -0.025 | -0.820 | -0.041 | 0.064 | -0.409 |
| | (0.159) | (0.029) | (2.370) | (0.149) | (0.169) | (0.717) | (0.225) | (0.066) | (2.101) |
| Old | 0.007 | -0.042 | -0.317 | -0.071** | -0.118* | -0.269 | -0.189* | -0.674* | -0.370 |
| | (0.017) | (0.114) | (0.261) | (0.022) | (0.067) | (0.337) | (0.097) | (0.396) | (0.569) |
| New | 0.086** | 0.081* | 1.011** | 0.055 | 0.129** | -0.023 | 0.059 | 0.067 | -0.105 |
| | (0.025) | (0.046) | (0.342) | (0.086) | (0.053) | (0.456) | (0.069) | (0.107) | (0.480) |
| Inpatient | -0.270** | 0.001 | 2.547 | -0.097 | -0.006 | 6.720 | Insufficient | -0.002 | 7.221 |
| | (0.133) | (0.005) | (2.703) | (0.179) | (0.005) | (4.044) | data | (0.009) | (6.340) |
| Outpatient | -0.095** | -0.164* | -3.152** | -0.202** | -0.271* | 0.920* | 0.171** | 0.451* | -0.301 |
| | (0.036) | (0.086) | (1.229) | (0.086) | (0.146) | (0.481) | (0.085) | (0.219) | (0.712) |
| ER | -0.121 | -0.012 | -0.207 | -0.289** | -0.030 | -0.875 | Insufficient | -0.020 | -0.770 |
| | (0.074) | (0.008) | (0.802) | (0.086) | (0.019) | (1.983) | data | (0.017) | (1.739) |
| Lab | -0.110 | -0.051 | | -0.134 | -0.028 | | -0.019 | -0.011 | |
| | (0.082) | (0.029) | | (0.140) | (0.028) | | (0.120) | (0.052) | |
| Drugs | 0.021 | -0.223 | | -0.002 | -0.602** | | -0.054 | -0.371 | |
| | (0.024) | (0.206) | | (0.056) | (0.276) | | (0.064) | (0.336) | |
| Other | -0.041 | -0.060* | | -0.174 | -0.050 | | -0.190* | -0.141** | |
| | (0.038) | (0.032) | | (0.175) | (0.086) | | (0.104) | (0.055) | |
| N | 187,656 | 187,656 | Varies | 76,125 | 76,125 | Varies | 43,197 | 43,197 | Varies |

Notes: Each cell shows the coefficient (and standard error) on the interaction between "state employee" and "post" from a single difference-in-difference regression. Each row shows results for a different service; each column shows a different measure of utilization for that service for one of three sub-samples. Control variables include gender, age group, enrollment in a family plan, date of hire, and a full set of municipality and year fixed effects. Coefficients in the first, fourth, and seventh columns are estimated using GLM; results in the other columns are estimated using OLS. Standard errors are clustered on municipality.

* denotes significance at the 10% level

** denotes significance at the 5% level

**Table 11: Heterogeneity in Results by Diagnosis**

| Diagnostic Category | Mean of Dependent Variable | Effect on Total Spending |
|---|---|---|
| Infectious and parasitic diseases | $102<br>(2,496) | 0.215<br>(0.181) |
| Neoplasms | $507<br>(6,294) | -0.348**<br>(0.139) |
| Endocrine, nutritional and metabolic diseases & immunity disorders | $298<br>(5,087) | 0.010<br>(0.100) |
| Diseases of the blood and blood-forming organs | $96<br>(3,407) | -0.044<br>(0.128) |
| Mental disorders | $255<br>(2,828) | -0.055<br>(0.046) |
| Diseases of the nervous system | $237<br>(3,501) | -0.208<br>(0.164) |
| Diseases of the sense organs | $139<br>(1,141) | 0.038<br>(0.048) |
| Diseases of the circulatory system | $484<br>(6,048) | -0.036<br>(0.061) |
| Diseases of the respiratory system | $371<br>(5,225) | -0.140*<br>0.077) |
| Diseases of the digestive system | $361<br>(4,246) | 0.053<br>(0.096) |
| Diseases of the genitourinary system | $379<br>(4,726) | 0.013<br>(0.103) |
| Complications of pregnancy, childbirth, and the puerperium | $108<br>(1,527) | Insufficient data |
| Diseases of the skin and subcutaneous tissue | $137<br>(1,840) | -0.022<br>(0.080) |
| Diseases of the musculoskeletal system and connective tissue | $643<br>(4,275) | -0.155**<br>(0.054) |
| Congenital abnormalities | $66<br>(2,432) | 0.065<br>(0.202) |
| Certain conditions originating in the perinatal period | $13<br>(544) | Insufficient data |
| Symptoms, signs, and ill-defined conditions | $893<br>(6,775) | -0.072<br>(0.086) |

| | | |
|---|---|---|
| Injury and poisoning | $393<br>(4,632) | -0.090*<br>(0.048) |
| External causes of injury and supplemental classification | $891<br>(6,557) | -0.052<br>(0.042) |
| N | 479,196 | 479,196 |

Notes: Each cell in the second column shows the coefficient (and standard error) from a single regression; means of the dependent variable are reported in the first column. Each row shows results for spending on a different diagnostic category. Control variables include gender, age group, enrollment in a family plan, date of hire, and a full set of municipality and year fixed effects. The sample includes all continuously enrolled active employees over the three-year period from fiscal year 2010 to fiscal year 2012; the unit of observation is a person-year. Coefficients are estimated using GLM. Standard errors are clustered on municipality.

* denotes significance at the 10% level

** denotes significance at the 5% level